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Cover: Inside the brightly lit building that once housed the 184-inch cyclotron is Lawrence Berkeley Laboratory’s new Advanced Light Source, a third-generation synchrotron radiation source (see pages 17–27). Photo courtesy of Lawrence Berkeley Laboratory.
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THIS IS A PIVOTAL MOMENT for the field of particle physics. With the recent demise of the Superconducting Super Collider has come a chorus of cries for the largest particle accelerators to be built as truly international projects. And the extensive community of physicists who had been planning experiments for the SSC are now looking hopefully to CERN’s Large Hadron Collider (LHC) as the only real alternative for continuing this research. Meanwhile, at least two panels are hard at work formulating proposals for the future of high energy physics in the United States.

Therefore we are pleased to present here two very timely articles that address some of the important questions confronting this field. George E. Brown, Jr., Chairman of the Science, Space and Technology Committee in the U.S. House of Representatives, recently issued a position paper titled “Big Science and International Cooperation” that is reprinted here in a slightly edited version. Beam Line readers can learn how one of science’s staunchest supporters in Congress, himself a physicist, views the problems of organizing and financing the very large projects that are increasingly needed to advance the frontiers of scientific research.

In the second article, CERN’s new Director General Christopher Llewellyn Smith extends an invitation to physicists around the world to become involved in the LHC experimental program. There he also presents CERN’s conditions for extensive LHC involvement by groups of scientists from countries (such as Japan and the United States) that are not already CERN Member States.

We hear increasing calls, especially from Washington, that scientists need to do a much better job in communicating the value that their work contributes to society. “Third-Generation Synchrotron Radiation Sources,” by former Science reporter Arthur Robinson, provides an excellent example of what can be done in this regard. Even knowledgeable observers will be surprised to learn what an impact storage rings, originally conceived for basic research in high energy physics, are beginning to have in areas that are closely allied with economic competitiveness.

With additional articles from Pedro Waloschek on Rolf Wideröe and Virginia Trimble on nuclear burning in stars, we consider this one of the best issues of the Beam Line we have published to date—and hope you agree. Please let us know what you think of this periodical by answering and returning the attached Reader Survey.

Michael Reider
Rose Donaldson
BIG SCIENCE
and International Cooperation

by GEORGE E. BROWN, JR.

The Chairman of the House Science Committee proposes a few initiatives for internationalizing major science projects.

THIRTY YEARS AGO, few people used the term "big science" let alone mentioned the need for international cooperation on big science projects. Nonetheless the United States pursued a number of major science and technology initiatives such as the Manhattan Project, the Apollo missions to the moon, and the establishment of the national research labs that would by today's standards qualify as big science. These programs all required substantial resources and long-term government commitment; they were models of
scientific teamwork in which creative management aimed to solve daunting scientific and engineering puzzles. The government marshaled the resources for these monumental projects, not for reasons of economic competitiveness or from a commitment to advancing the frontiers of human knowledge, but to demonstrate the superiority of the United States over its enemies.

For almost 50 years our science and technology infrastructure, like most of our society and government, was organized against a common external enemy—the Soviet Union. During the Cold War we fought our battles indirectly, via the size of our armaments, our influence over other countries, the excellence of our scientific and technological endeavors, and even the number of Olympic medals our athletes garnered. The justification for societal investment in science was often based on claims of national security. In many instances, major science initiatives were national exercises that were predominantly driven by international political calculations. When we needed big science to help us cope with international challenges, the government was happy to accept the costs. We sought to prove the superiority of the "American Way" through our achievements. The American people and government were committed to our national motto—"America #1."

With the beginning of the dissolution of the Soviet Union, catchwords like "big" or "mega" science projects began to appear much more frequently in discussions of science policy, as did new justifications for government support of scientific research for reasons of economic competitiveness. Discussion focused on the need for long-term commitment, capital-intensive cost, and the need to internationalize projects at the same time that Congressional support for the current spate of major American projects waned. Today capital-intensive science and technology enterprises are generally viewed by politicians in terms of their cost or the jobs they create. Perhaps because of this shifting rationale, the prognosis for the successful completion of the current group of projects is not bright.
A brief review of a few recent big science projects highlights the current difficulties in completing such projects. After a $2 billion investment, the Superconducting Super Collider was terminated because of escalating cost and the lack of international participation. After a more than $10 billion investment, the Space Station is being redesigned due to excessive cost, and Russian participation is being sought as a means of cost containment and to justify the project as a foreign policy tool. As the Space Station becomes more internationalized, Congressional support may be declining as a result of concerns about jobs and the current budgetary climate. Finally, there is the International Thermonuclear Energy Reactor, which is touted as the most successful large science project to date. However, the inability of the four partners to agree on a location for a single engineering design center required a compromise to establish three separate design centers. And as negotiations begin for siting this reactor, the project could stall or even fail over the partners’ inability to equitably parcel out the perceived benefits of jobs related to the facility’s construction and operation.

The United States has invested significant amounts of money in some of these undertakings only to have them end in termination or redesign. Further, we have gained the reputation for being a difficult and unreliable partner for big science undertakings. Our self-centered focus has blinded us to the possibility of true international collaboration in science and technology. It is obvious that we can’t afford mega-projects by ourselves and that basic research “at the frontier” is becoming increasingly costly. If the United States wishes to continue to be a major research force, we must re-evaluate our motivation and justification for supporting massive science and technology missions. We must develop a new framework to support large enterprises or face the unhappy prospect of becoming a second-string player in research areas requiring large and sustained commitments of resources.

The United States currently is in the midst of a post-Cold War re-evaluation of science and technology. The very notion
of national interest itself is more amorphous and confused than at any time in this century. The appeal of big science projects has been reduced to tenuous claims of economic benefits through technological spin-offs and government-funded jobs. We are striving to reformulate the goals of our science and technology programs within the framework of a new world order we can’t even define.

The broad agreement on national sacrifice in support of the Cold War has collapsed and with it the willingness of the United States to individually bear the costs of big science. As a result, the scientific community must develop a new justification for government support for its mega-projects. If not, in an atmosphere of fiscal constraint, research priorities will be set by politicians bent on extracting economic benefits for their various constituencies. It is obvious that if the United States wants to support big science in the post-Cold War world, we need to do it internationally. We realize that cooperation, not competition, will be one of the guiding tenets of the international paradigm of the post-Cold War era. International cooperation on big science projects can serve as one vehicle to implement this new attitude.

It is necessary to reflect on what is essential for viable, sustained international scientific cooperation on big science projects. One set of principles for future international big science is obvious: a project must be based on shared costs, shared responsibilities and shared recognition. Beyond these basic principles, it is less certain what future big science projects must be built upon. However, four underlying characteristics of the global scientific community may condition our approach to big science.

First, there is a free flow of information in the international research community. International journals and the revolution in electronic communications have allowed researchers to develop a virtual global community that is beyond the geopolitical unity that most politicians and diplomats only talk about building. The density, richness and immediacy of communications carried by electronic networks is likely to grow, further reinforcing the communal nature of scientific
exploration. Just as importantly, this network allows researchers to receive experimental data and results from a science project located on another continent, half a world away.

Second, research tends to progress along the same trajectory within a particular field regardless of the nationality of the scientists. It is not just a coincidence that high energy physicists around the world broadly agree on the need for an accelerator with a higher energy level (although they may not all agree on what energy level is necessary). Today, national research agendas around the world have increasingly common goals.

Third, the results of basic research cannot be captured by any single nation so long as science is viewed as a public good open to all. This suggests that big science (basic research) should not be marketed to policymakers as a solution for economic problems or as a source of commercial spin-offs. Big science does bring important lessons in teamwork and profound achievement, but it will not necessarily translate into a greater market share for Chrysler (or Toyota or Mercedes).

Fourth, the United States and Europe are no longer the sole repositories of scientific and technological excellence. For the past 50 years, one of our most successful foreign policy tools has been the free access of foreign nationals to our system of higher education. Few countries in the world today lack a substantial cadre of scientists or engineers trained in the U.S. or in Europe. There are world-class scientists and engineers throughout the world.

These four qualities of science should inform our approach to building international cooperation in big science. They suggest both reasons why international big science should be pursued and one possible rationale for building support for cooperation among policymakers in the United States. The United States is the de facto global leader in political and economic life. However, policymakers in the U.S. are still struggling with what it means to lead without an enemy and looking for tools that can be most effective in building a sense of global community and cooperation.

Our approach to big science must be as a partnership among equals.
Obviously, any project should be based on scientific and technological merit. Notwithstanding this essential criterion, scientific merit is only a necessary but not a sufficient condition to warrant government support. Big science projects must offer some concrete societal benefit, which could range from increasing the store of human knowledge to finding a cure for malaria. Congress and the American public no longer view science and technology as an enterprise that, if liberally funded will act as a panacea for society's ills. Americans once believed that government support for massive science and technology projects—in both the civilian and defense sectors—provided them with protection from the perceived threat of Soviet domination. Today's science and technology consumers still demand identifiable benefits from their investment.

Within this context, the government can begin to develop an initiative that is not an abdication of U.S. leadership in science and technology. We must acknowledge that science and technology have in many areas advanced to the point where large research undertakings are the next logical step. And we must realize that given the size of the outlay and budgetary limitations among all countries, only an international coalition can support these projects. Our approach to such projects must be as a partnership among equals. Our focus must shift from "America #1" to "America Second to None." This shift in focus is not revolutionary, but evolutionary. It is the result of past experience and the acceptance of current realities.

What I propose is a set of initiatives that will shift our focus from one limited to solely domestic opportunities and resources to one that is framed in terms of international opportunities and resources. In order for the United States to successfully negotiate this change in mindset, I believe we need short-term, medium-term, and long-term action plans.

**Short-Term:** In order to ensure sustained Congressional commitment, all fundamental research projects costing in excess of $50 million should be required to have a full Congressional authorization.

Congressional authorization of big science programs would help ensure the efficient use of funds, allow us to review
international partnering opportunities, and hopefully make us a more reliable partner by establishing multi-year plans. The days when Congress blindly funded a line-item request for a major scientific project are over. For sustained Congressional support, projects must be justified within the new realities. An agency submitting a project should provide a detailed analysis that 1) includes the objectives, timeframe, and cost of the project; 2) divides the project into phases such as feasibility, design, construction, and utilization—with performance standards and milestones to be met at each phase; and 3) analyzes parallel efforts by other countries as well as the prospects for international cooperation and burden sharing during each phase of the project. Authorizing legislation should incorporate these parameters as well as any other milestones that were developed as a part of the Congressional evaluation process. This approach would also give potential international partners the opportunity to understand U.S. expectations and limitations.

**Mid-Term:** Prepare a forecast of the needs for big science projects through the year 2010.

The President’s Science Advisor should commission a report that spells out promising areas of science that require a major focused, sustained commitment in terms of projects (e.g., human genome, global climate change) and/or research machines (e.g., advanced neutron source, high-energy particle accelerator). In addition, this report should assess other countries’ efforts in the field and the current state of and potential for international cooperation. An international overlay to this report would move the process further along the path to burden sharing and avoiding duplication of effort. With this long-term planning framework, we would be able to prioritize projects and apportion and plan funding appropriately. In the past we have approached these projects in an *ad hoc* manner. In the absence of a national framework or goals, big science projects have been viewed on an individual basis.

**Big science projects must offer some concrete societal benefit, which could range from increasing the store of human knowledge to finding a cure for malaria.**
without thought to overall budgetary constraints. A comprehensive report, allowing the review of projects across agency boundaries and scientific disciplines, would be a first step in ordering projects in accordance with overall national goals. This approach complements the Administration's efforts in its Re-inventing Government initiative and its efforts to prioritize and budget federal science and technology programs.

Long-Term: Establish an international panel among the Group of Seven nations to develop international priorities and international funding sources for big-science projects.

Big science projects outlive Administrations, politicians, and—in some instances—countries themselves. We need to begin to develop an international consensus on big-science priorities and a commitment of financial resources. It is primarily the Group of Seven nations who have the majority of resources to fund such projects. However, a portfolio of big science projects should range from undirected basic research to specific global problems that can be addressed through science and technology. One of the failures of international cooperation on these projects in the past has been that the partners have insisted that benefits flow equally to all. In the context of an individual project, this is frequently impossible. We need to assess the benefits of all international projects together. Then we can begin to ensure that, in the long run, benefits of a group of projects flow to all the participants or regions equally. The Group of Seven nations also must recognize the scientific and technological talent and priority-setting contributions that other countries can make to this effort. World-class scientists and engineers should be invited to participate in any international big science project regardless of the ability of their government to play a role in funding the construction of a project.

The benefits of international cooperation can be as intangible as those of basic research, but they are just as important to our future well-being and to the well-being of our planet. International big science may be both an effective policy tool and an important component in building peaceful cooperation.
among nations. By both tapping and enhancing the communal nature of science, we can reinforce bonds of trust and communication and break down barriers between nations. Big science, because it is visible and newsworthy, would provide an object lesson for people around the world in the possibilities of cooperation. Big science projects are not an elixir for the world's ills; they will not stop the tragedy in Bosnia or feed children in Somalia. But a case can be made that more cooperation on science will reduce the chances of a future Bosnia and that big science, if aimed at practical challenges, could contribute to sustainable development. Today, we are faced with a number of problems that are global in scope and require cooperation among many nations: environmental research and action, efforts to clean-up nuclear wastes, the elimination of the AIDS pandemic—all hold out the possibility of becoming international big science.
THE LARGE HADRON COLLIDER

by CHRISTOPHER LLEWELLYN SMITH

The future of physics at CERN revolves around a major upgrade of its world-renowned LEP collider.

THE LARGE HADRON COLLIDER, which will open up the exciting world of physics at TeV energies, will be the centerpiece of the CERN program during the first two decades of the twenty-first century. There is worldwide interest in the LHC, and we hope that the whole project, both the machine and its experiments, will proceed on a partnership basis involving physicists and engineers from different continents.
The CERN Council, CERN’s governing body, is evaluating the LHC project and the Laboratory’s scientific, manpower and budgetary plans for the years 1995–2005. Cancellation of the Superconducting Super Collider has left the LHC as the only realistic machine for studying multi-TeV hadron physics in the foreseeable future. The Council’s decision and discussions concerning participation in the LHC program from outside the Member States therefore make the coming months crucial, not only for CERN but also for the future of world particle physics.

Before looking to the future, I will briefly outline the present political and fiscal status of CERN. During the last decade the number of Members States has grown from 12 to 19, now embracing most of western Europe as well as Poland, Hungary and the Czech and Slovak Republics. Council decisions are based on one vote per country, while contributions to the CERN budget are proportional to the national income of each country, with a ceiling for any one nation currently set at 25 percent of the total and special transitional arrangements for new members.

During the last 20 years, the number of scientific users at CERN has increased five-fold, with more than a quarter now coming from non-Member States. Demands for services and infrastructure have grown accordingly, whereas CERN’s personnel and budget figures have fallen. Staff numbers have been reduced from around 3,600 in 1975 to a little over 2,900 at present, with a further reduction of 550 foreseen for the coming decade. The annual budget, currently around 920 million Swiss francs (or about $640 million at current exchange rates) at 1993 prices, is some 14 percent below its 1974 peak during the construction of the Super Proton Synchrotron (SPS).

In this constrained framework, CERN plans the vigorous exploitation for physics research of the Large Electron Positron Collider (LEP) until the end of the decade, in parallel with a reduction in other programs to free up resources to build the LHC. After a two-year shutdown for installation, LHC commissioning will start in 2002. To provide a modicum of flexibility, especially in view of the gap between the end of LEP physics and the start-up of the LHC, there is a modest budget line for new initiatives from 1997 onwards.

The LEP-I program will comprise running at the Z resonance in 1994, for higher precision measurements of electroweak parameters and beauty quark systems. In 1996 the LEP–II energy will cross the threshold for W pair production, reaching some 90 GeV per beam. This will allow accurate measurements of the W mass, studies of the triple gauge boson vertex (a photon or a Z, plus two Ws), and continued searches for new particles, especially the Higgs boson and supersymmetric particles.

Curtailing CERN’s non-LEP physics program, which currently engages half of the users, will be painful. Our Low Energy Antiproton Ring will become the heavy-ion accumulator for
The ALICE experiment is being designed to observe the results of heavy-ion collisions at LHC. Its detector will, in part, employ well-established technologies for particle detection that can perform reliably in the high particle fluxes expected from these collisions.

The LHC, and the West Area (now used for fixed-target experiments) will be closed and later taken over for testing LHC magnets. Despite these economies, several exciting new projects are already in the pipeline, including collisions of lead ions at 3.7 TeV center-of-mass energy, the search for evidence of neutrino mass in the new muon neutrino beam at the SPS, and the next round of high-precision measurements of CP violation, the tiny asymmetry that may explain the dominance of matter over antimatter in the universe.

As the next century dawns, the LHC will be getting ready to start up with two general-purpose detectors for proton-proton collisions, ATLAS and CMS, a specialized heavy-ion experiment ALICE and a specialized beauty experiment, still to be chosen from COBEX, GAJET and LHB proposals. The LHC machine will exploit pioneering techniques and technologies, with superconducting magnets operating below the 2K temperature of superfluid helium using a "two-in-one" magnet design with the counter-rotating beams housed in a single yoke and cryostat. The LHC design parameters break new ground (see top table on next page), but nevertheless were judged to be "reasonable and realistic" by a recent external review committee composed of 14 experts from all over the world.

LHC design luminosities translate into exceedingly high data rates and put tremendous demands on the detectors and data-capture equipment. A highly successful, technically advanced R&D program, under way now for almost four years, is already showing that it will be possible to build detectors that can cope with these extraordinary experimental conditions.

The LHC machine will access an uncharted energy region that theorists believe contains Higgs bosons, or other phenomena that would explain why most particles are massive while the photon is not. The appearance of supersymmetric particles, also predicted in this energy range, would reveal a beautiful and intimate connection between matter particles and the particles that mediate forces between them. New interactions may be discovered, mediated by heavy vector bosons, indicating a deeper underlying unification of Nature's forces. High-statistics studies of the top quark and of CP violation in decays of B mesons carrying beauty quarks will be possible. Lead-lead collisions in excess of 1,000 TeV total energy will compress nuclear matter to densities and temperatures that prevailed just after the Big Bang, giving us new insights into the birth of the Universe.

The estimated material cost of the LHC machine was deemed to be "accurate" by the external review committee (see bottom table). Following standard CERN accounting practice, the figures cover only expenditures for materials and do not include CERN manpower. The value of existing facilities that will be incorporated in the LHC, namely, the Linac, Booster, Proton Synchrotron, SPS, LEP tunnel and LEP-II cryogenics, as well as site amenities such as buildings and roads, exceeds that...
of the new elements of the LHC, making the project very cost effective.

What really counts, of course, is not the cost of the LHC alone but the total cost of the CERN program as put to Council in December 1993. Normal contributions from Member States fall short of the amount required for the full program as proposed. While part of the gap may be filled by special contributions from some Member States, we are obviously hoping that the rest will be covered by contributions from non-Member States. If this cannot be done, we will have to make painful compromises, most notably that of stretching the LHC project timetable.

The present mood is buoyant. Council gave a very warm reception to the overall plan and resolved to "seek to move during the first half of 1994 to a decision" on the LHC. Early approval of the project would be in the hope and expectation that significant contributions will be forthcoming from non-Member States, and in the knowledge that a fall-back solution exists based on European resources alone, which offers outstanding physics but at a later date with adverse scientific consequences.

The CERN USER community is living proof that the CERN management welcomes inter-regional collaboration, which is of enormous benefit to all concerned and greatly enhances the progress of science. While welcoming world interest in the LHC project, Council has stated that involvement of physicists from outside the Member States "should be on the understanding that usage on a significant scale must involve the provision of resources to suit both CERN and the individual non-Member State concerned." This policy is, regrettably, a departure from the "free access" policy drawn up in 1980 by the International Committee for Future Accelerators. However, this policy was based on the expectation of an approximately balanced cross-use of world facilities, an expectation that is currently no longer fulfilled.

In complying with Council's injunction to seek funds from outside American-based particle physicists doing their research in Europe, at CERN, DESY and the Gran Sasso, currently outnumber European-based particle physicists working in the US by a factor of more than four. This ratio will clearly change, given the interest expressed by European physicists in the SLAC/LBL/LLNL B Factory, as well as by American physicists in the LHC.

### LHC Design Parameters

<table>
<thead>
<tr>
<th>Colliding Beams</th>
<th>Peak Luminosity</th>
</tr>
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<tbody>
<tr>
<td>Proton-Proton</td>
<td>7 TeV per beam</td>
</tr>
<tr>
<td>Lead-Lead</td>
<td>574 TeV per beam</td>
</tr>
<tr>
<td>Proton-Electron*</td>
<td>1.3 to 1.7 TeV center-of-mass</td>
</tr>
<tr>
<td></td>
<td>$10^{34}$</td>
</tr>
<tr>
<td></td>
<td>$10^{27}$</td>
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<td>$2 \times 10^{32}$</td>
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*Option for future development of LHC (protons) and LEP (electrons)*

### LHC Cost Estimates

<table>
<thead>
<tr>
<th>Item</th>
<th>Material Cost [MSfr]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Machine</td>
<td>2230</td>
</tr>
<tr>
<td>Experimental areas</td>
<td>210</td>
</tr>
<tr>
<td>CERN contribution to experiments (about 20%)</td>
<td>220</td>
</tr>
</tbody>
</table>

*Millions of Swiss francs at 1993 prices*
The cascade of accelerator rings that will boost charged particles to the energies required for LEP and LHC.

**LEP/LHC Booster**
**4,PS i LL**
**Proton <^r LEAR**
**PS EPA ,, P LIL**
**iatronLU**
**lion**
**e (positron)**

The CERN Member States, we hope to do much more than simply balance the books on an optimal time scale. The door is open for non-Member States to become genuine partners in the final design, construction and exploitation of the LHC machine and its experiments. We are very pleased that Council appears to be receptive to the idea of offering a voice in LHC decision-making to countries that provide substantial contributions. Such a “globalization” of the LHC project would establish a wonderful precedent for future megascience projects, not only in particle physics but also in other fields.

In Europe, we are eagerly awaiting the final report of the HEPAP subpanel, chaired by Sidney Drell, on the “future vision” for particle physics in the United States. We have heard rumors that it will include a recommendation that the US should become fully involved in the LHC project. We hope that these rumors are true, and that the necessary funding will be obtained from Congress. We are encouraged by the recent launch of the US LHC Collaborators Group, following the February American Physical Society Workshop on physics at large hadron colliders at Fermilab. We look forward to working in partnership with our American colleagues with whom we have collaborated so successfully and enjoyably in the past.
THIRD-GENERATION SYNCHROTRON LIGHT SOURCES

by Arthur L. Robinson

Intense beams of ultraviolet light and X-rays from a new generation of storage rings promise revolutionary advances in science and industry.

A whole new generation of synchrotron radiation sources is coming on line throughout the world, providing a quantum jump in brightness to service a growing demand. Such third-generation light sources are already operating in France, Italy, Taiwan and the United States, while six others are under construction and over a dozen more are in the planning stages.
The Advanced Light Source at Lawrence Berkeley Laboratory is located inside the building that once housed the old 184-inch cyclotron.

The Sincrotrone Trieste, a third-generation source of soft X-rays located in Trieste, Italy.

A few of the reasons for this remarkable spurt in construction of third-generation facilities include:

- They are the world's brightest sources of X-rays for scientific research.
- They promise some of the most intimate looks yet at the structure, composition, and chemical bonding of almost any kind of solid, liquid, or gaseous material, from semiconductors to biological molecules.
- They can contribute to technology development for industries whose worldwide markets total more than $750 billion. Moreover, they are government-funded projects that, from the evidence so far, are on time and on budget. And they work.

To scientists, brightness means more than just a lot of light. Two sources may radiate the same amount of light, but one in which the light comes from a small area and shines forth in a narrow cone is much brighter than, say, a light bulb, which is rather large and radiates light waves more or less equally in all directions. Lasers, whose high-intensity, needle-like beams have in the last two decades transformed optical science in the infrared and visible regions of the spectrum, illustrate well both the properties and virtues of high brightness.

In more technical terms, the measure of the amount of light, called the flux, is the number of photons per second. Brightness is the flux divided by the area of the source and by the solid angle of the radiation cone. Third-generation synchrotron light sources are approximately 100 times brighter than are the best existing...
second-generation synchrotron sources and are a whopping 100 million times brighter than conventional X-ray sources found in the laboratory. Although the requirements for brightness differ from experiment to experiment, a brighter beam is generally far more useful because the light remains concentrated in a smaller beam over longer distances. With high brightness researchers can do experiments faster, record higher resolution spectra, examine tiny samples or parts of samples less than 1 micrometer in size, and follow structural changes on nanosecond time scales.

THE FIRST GENERATION

So, what is synchrotron radiation and why is it so bright? The story begins late last century, when physicists began to understand that electrically charged particles—electrons, in particular—radiate electromagnetic waves when they travel along curved paths. Nowadays, when the curving motion is caused by a magnetic field, scientists call the waves synchrotron radiation or, more colloquially, synchrotron light.

Builders of electron accelerators began to appreciate the power of synchrotron radiation in the 1940s. As the energy of circular electron accelerators grew, physicists feared that this radiation would draw away so much energy from the electrons that the maximum beam energy would be limited. In fact, synchrotron radiation has turned out to be a costly nuisance to builders of accelerators for high-energy physics. So-called relativistic electrons following a curved path at nearly the speed of light emit vast quantities of ultraviolet and X radiation. With each increase in beam energy, the size (and hence cost) of the accelerator also grew to keep the radiated power to a manageable level.

Use of synchrotron radiation for scientific research grew slowly in the beginning. Scientists at the General Electric laboratory in Schenectady, New York, first observed synchrotron radiation in 1947 from a small electron synchrotron with an energy of 70 million electron volts [but see the article on Rolf Wideröe, page 31]. Subsequent work led within a decade to the use of these machines as a new source of ultraviolet and X radiation for experiments. By the early 1960s the potential benefits of synchrotron radiation were obvious enough that significant attempts to harness it occurred around the world—in the United States, Europe, and Japan. Several of its features motivated the hardy souls who undertook these first ventures:

Continuous Spectrum. Synchrotron radiation has a broad, continuous spectrum of wavelengths, with the highest photon flux at ultraviolet or X-ray wavelengths depending on the energy of the accelerator; the higher the energy, the shorter the accessible wavelength. Many experiments require an ability to select the wavelength; others require an ability to vary the wavelength continuously over a wide range.

High Flux. The flux from a synchrotron light source is generally far higher than that from conventional rotating-anode X-ray sources, although at a few fixed wavelengths the fluxes may be comparable. Almost every type of experiment benefits from high flux in the form of greater signal and shorter measurement time. In many cases, the enhanced signal permits experiments that would prove to be impractically long with ordinary X-ray sources.
High Brightness. The synchrotron radiation beam is highly collimated due to the strong forward (i.e., tangential to the curved path) emission of synchrotron light into a narrow cone a few hundredths of a degree wide. This high collimation combined with the small size of the light source (the electron beam) makes for high brightness.

Pulsed Beam. Because the electron beam in an accelerator consists of a succession of bunches with spaces between, the synchrotron light is pulsed rather than continuous. Pulsed light allows researchers to follow the progress of a given process (such as a chemical reaction) by for example using one pulse to initiate a process and subsequent pulses to investigate it.

Scientists set up the first synchrotron-radiation experiments at synchrotrons originally built for high energy physics. As storage rings (in which electrons circulate for hours at a fixed energy) became popular for high energy physics, users of synchrotron radiation found them to be much superior light sources, in part because the constant beam energy meant that the spectrum of radiation did not fluctuate with time, as it does in a synchrotron.

In today's terminology, particle accelerators—including storage rings—operated for high energy physics but also used "parasitically" for synchrotron radiation are called "first-generation" sources. In its youth, the venerable SPEAR storage ring at SLAC served as a first-generation synchrotron light source for the Stanford Synchrotron Radiation Laboratory (SSRL).

THE SECOND GENERATION

Users of synchrotron radiation quickly realized that full utilization of this wondrous new light source required facilities built and operated specifically for their work. In the United States, a 1976 National Academy of Sciences report documented the burgeoning demand for so-called
A Synchrotron Radiation Primer

maximum at about $\epsilon = 4\epsilon_{\text{c}}$, the critical photon energy is also a measure of the maximum useful photon energy (short-wavelength limit) of a synchrotron source.

Undulators and wigglers reside in the straight sections connecting the curved sectors of the storage ring. The magnetic structure of these insertion devices consists of a linear array of north-south dipoles of alternating polarity. The normal vertical orientation of the dipoles causes electrons to undergo a nearly sinusoidal trajectory of period $\lambda_u$ in the horizontal plane. The peak magnetic field $B_u$ on the insertion-device axis depends exponentially on the ratio of the gap between the magnet pole faces to this period. The deflection parameter $K$ is the ratio of the maximum angular deviation of the electron trajectory from the undulator axis to the natural opening angle of the synchrotron-radiation cone. Expressed in terms of insertion-device parameters, $K$ is approximately

$$K = 0.934 \frac{B_u}{\text{Tesla}} \lambda_u \text{ [cm]}$$

Whether an insertion device is an undulator or a wiggler depends on the value of $K$. Undulators generally have $K$ values less than 1 and wigglers have $K$ values greater than 10. In between, insertion devices retain significant undulator properties.

Because of the small deflection angle in an undulator, the radiation emitted by successive undulations adds coherently. Interference gives rise to a sharply peaked radiation spectrum consisting of a fundamental and several harmonics whose photon energies can be selected by changing the magnetic field. The photon energy, $\epsilon_n$, of the $n$th harmonic is

$$\epsilon_n = \frac{0.949nE_0^2 \text{ [GeV]}}{\lambda_u \text{ [cm]} \left(1 + k^2 / 2 + \frac{1}{\gamma^2 \theta^2}\right)}$$

where $\theta$ is the horizontal angle of emission relative to the undulator axis. This relation motivates the construction of higher-energy storage rings for the production of short-wavelength (hard) X-rays. Many undulator properties improve with the number of periods $N$. The opening angle of the undulator radiation cone is narrowed relative to that of a bending magnet source by approximately a factor of the square root of $N$:

$$\theta_n \text{ [mrad]} = \frac{1000}{\gamma} \sqrt{\frac{(1 + k^2 / 2)}{2nN}}$$

The intrinsic, on-axis spectral width (full-width-half-maximum) of the $n$th undulator harmonic $\Delta \epsilon_n / \epsilon_n$ is equal to $0.9/nN$. In practice, the lower bound of the spectral width is determined by the angular divergence of the electron beam and by the inevitable small errors in placement of the magnets in the undulator. When the intrinsic width drops below this limit, increasing the number of periods no longer improves the spectral resolution.

In a storage ring with electron current $I$, the total power $P_{\text{net}}$ radiated by an undulator of length $L$ over all harmonics and angles is

$$P_{\text{net}} \text{ [KW]} = 0.633E_0^2 \text{ [GeV]} B_u^2 \text{ [Tesla]} \times I \text{ [Amp]} \times L \text{ [m]}$$

For many purposes, it is only the central cone of undulator radiation that experimenters use. The spectral flux $\phi_n$ per unit bandwidth BW for the $n$th harmonic (integrated over the cone) is

$$\phi_n \text{ [photons/sec/0.1\%BW]} = 1.43 \times 10^{14} I \text{ [Amp]} NQ_0(K)\text{[cm]}$$

where $Q_0$ is a function of value less than or equal to 1 that increases with $K$ and decreases with $n$. The flux is clearly proportional to the number of periods.

The spectral brightness (also called spectral brilliance) is the flux per unit area of the source and unit solid angle of the radiation cone. In the jargon of statistical mechanics, the brightness is the density of flux in phase space. The average spectral brightness $\beta_n$ in the central cone is approximately

$$\beta_n \text{ [photons/sec/mm}^2\text{mrad}^2\text{/0.1\%BW]} = \frac{\phi_n}{(2\pi)^2 \Sigma_x \Sigma_y \Sigma_z \Sigma_y'}$$

where $\Sigma_x$ and $\Sigma_y$ are the RMS horizontal and vertical source sizes for a beam with a Gaussian density distribution, and $\Sigma_z$ and $\Sigma_y'$ are the RMS horizontal and vertical cone opening angles (angular divergences). Brightness, which in most cases is proportional to $N$, is a conserved quantity that cannot be improved by an optical system. Absorption, scattering, and poor design can degrade the brightness downstream from the source.

For $K$ much less than 1, the only significant photon flux occurs at the fundamental frequency. As $K$ is increased when tuning the undulator to lower photon energies, the number of harmonics with measurable photon flux grows rapidly and soon most of the undulator radiation occurs in the harmonics. By the time $K$ reaches a value of 10, the higher harmonics are so closely spaced that they overlap, giving rise to the broad, continuous spectral curve of a wiggler (although the fundamental mode can still be distinct). Another way of looking at this transition is to say that the light from successive wiggler periods does not add coherently. The lack of interference means that each pole of a wiggler acts like an independent bending-magnet source.

Viewed along its axis, the wiggler acts as if it were 2$N$ bending-magnet sources in a row. A wiggler therefore generates the same continuous spectrum as a bending magnet, but the wiggler flux increases directly with the number of poles; raising the magnetic field pushes the spectrum toward shorter wavelengths.
"second-generation" synchrotron light sources and helped consolidate the political support for them that resulted in the completion in the early 1980s of the National Synchrotron Light Source (NSLS) at Brookhaven National Laboratory on Long Island and Aladdin at the University of Wisconsin Synchrotron Radiation Center near Madison. Second-generation sources also sprouted during this period, beginning in 1976 in Japan and later on in Europe and the Soviet Union.*

In designing the dedicated synchrotron light sources, accelerator physicists recognized that it was not enough simply to build a high-energy machine and use it as a light source. Two Brookhaven physicists, Renata Chasman and Kenneth Green, made a key contribution when they recognized the importance of high brightness and showed how to achieve it.

The trillion or so electrons circulating in a storage ring do not all follow exactly the same path as they circulate. Instead, they wander around the central orbit, a motion that gives the beam a size defined by the amplitude of the excursions and a divergence defined by the angles they make relative to this orbit. The product of the beam size and the divergence is, in accelerator physics jargon, the emittance. To achieve high brightness from a storage ring, it is necessary to confine the excursions of the electrons—that is, to minimize the emittance.

In accelerators, dipole magnets bend the beam in the horizontal plane, thereby generating the synchrotron radiation that comes out tangentially. Quadrupole magnets provide the necessary focusing. The sequence of dipoles, quadrupoles, and other magnets in a circular machine define its "lattice." Chasman and Green devised what is now called the Chasman-Green lattice or, more technically, the double-bend achromat, in which there are two bending magnets in each curved sector of the storage ring. Both died before they could see the fruits of their work, but their idea, or variants stimulated by it, underlies modern synchrotron light sources of the second and third generations.

That the high brightness of the second-generation synchrotron sources benefits experimenters is amply illustrated at Brookhaven's NSLS. The facility operates two storage rings with different energies, one optimized for ultraviolet and one for X radiation. Streaming away from the two rings, there are many beam lines guiding synchrotron light to almost 100 experimental stations, all of which can collect data simultaneously. The experimental floor is so crowded with equipment that a visitor finds it hard to locate the storage ring itself.

Synchrotron light provides a means for examining the structure of matter—the positions of atoms (atomic structure) and the behavior of the electrons around the atomic nuclei (electronic structure). These two kinds of structure determine all the commonly observed properties of matter, such as strength, chemical reactivity, thermal and electrical conductivity, magnetism and optical transparency or lack of it. Conversely, the ability to control these two kinds of structure allows one to design materials with the particular properties desired.

Experiments at second-generation synchrotron light sources tend to fall into two categories:

- Elastic scattering (such as X-ray diffraction) determines the positions of atoms and molecules. For example, in X-ray diffraction, X-rays leave the sample only in certain directions, so that a film or other detector records a "diffraction pattern" in which the intensities and positions of the spots can be analyzed to obtain atomic positions.

- Spectroscopy provides information primarily about electronic structure, including chemical bonding, but some atomic-structure information is also available. Absorption, emission or fluorescence, and photoelectron spectroscopy are the major, but not the only, spectroscopic techniques.

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*The division of synchrotron light sources into generations oversimplifies the actual situation. In the United States, for example, pioneering smaller storage rings dedicated to synchrotron radiation had been operating at the University of Wisconsin Synchrotron Radiation Center (Tantalus) since 1968 and the National Institute of Standards and Technology (SURF II at the former National Bureau of Standards) since 1974.

Physical shape of the synchrotron radiation beams generated by dipole bending magnets, wiggles and undulators.
Even as the NSLS was under construction, events at SSRL and elsewhere were propelling the world of synchrotron radiation into the third generation. In a key 1979 experiment, SSRL scientists conclusively demonstrated that a special magnet called a multipole wiggler could simultaneously increase the flux of synchrotron radiation and push the spectrum to shorter wavelengths. A year and a half later, in another groundbreaking experiment at SSRL, scientists tested a related type of magnet called an undulator. Both wigglers and undulators are placed in the otherwise empty straight sections between the curved sectors of a storage ring, thereby earning their generic name of insertion devices.

In and of themselves, insertion devices were not new in 1980. Much pioneering work had been done in the Soviet Union, for example. But Klaus Halbach of the Lawrence Berkeley Laboratory (LBL) provided the advance that made insertion devices, and especially undulators, practical sources of synchrotron radiation. Before Halbach, the magnets in insertion devices were electromagnets. For an undulator, however, it is essential to have a large number of poles spaced at intervals of a few centimeters, more closely spaced than is practical for electromagnets, where the electric current generates heat that must be dissipated. Halbach’s idea was to use permanent magnets, which became feasible with the comparatively recent availability of high-field permanent-magnet alloys made largely of samarium (a rare earth element) and cobalt.

The undulator in the 1980 tests at SSRL, which was built at LBL, used this type of magnet. Since then, virtually all insertion devices retrofitted into the straight sections of storage rings around the world (except very high-field wigglers that use superconducting magnets) are likewise based on permanent magnets, with a new alloy primarily of boron, iron, and neodymium now the material of choice.

With the high brightness of undulator radiation a largely new capability—spatial resolution—moves from the status of a demonstration experiment to the realm of practicality. Ordinarily, synchrotron light bathes all or a large part of the sample, so that measurements reflect an average over the illuminated area, but the sharp pencil beams produced by undulators allow researchers to examine small areas in detail. This fine resolution promotes the applicability of synchrotron radiation to real-world materials. Whether semiconductor chips, magnetic storage disks, high-strength alloys, ceramics and plastics, or subcellular structures in biological cells, most materials share the common characteristic of being inhomogeneous, which is to say that the structure, composition, and chemical bonding is not the same throughout.

In fact, the inhomogeneities in most technologically important materials are put there deliberately to produce the desired performance. Ceramics are a case in point. Ceramists introduce additives that collect at the boundaries between ceramic particles in order to increase the strength at these otherwise weak points. Moreover, the size of samples or the features to be examined therein are constantly decreasing. By far the most familiar example is that of microcircuits, which soon may be called nanocircuits, as the size of characteristic features of circuit patterns drops toward 0.1 micrometer (100 nanometers).

The X-undulator beam line on the X-ray ring at Brookhaven’s NSLS illustrates one way to do this—scanning X-ray microscopy. On this beam line, special lenses focus the already highly collimated undulator light to
## Third-Generation Synchrotron Light Sources

Third-generation synchrotron light sources are a truly worldwide phenomenon, although, with the exception of the Brazilian LNLS, all of them are in the northern hemisphere. This table summarizes existing facilities and those presently approved for construction for which published information is available. Other proposed facilities that have not been approved yet are not included here.

Third-generation synchrotron sources are defined by the properties of the storage ring: a beam energy near or above 1 GeV, a low emittance near or below 20 nm-rad, and multiple straight sections specifically designed to accommodate insertion devices. Not all the facilities listed satisfy all these requirements, but as new synchrotron sources, they are listed in the interests of inclusiveness. The table deliberately excludes facilities designed for specialized research, such as development of free-electron lasers, or for commercial purposes, such as X-ray lithography exposure stations for the manufacture of microelectronic circuits.

<table>
<thead>
<tr>
<th>Facility</th>
<th>Circumference (m)</th>
<th>Beam Energy (GeV)</th>
<th>Beam Current (mA)</th>
<th>Emittance (nm rad)</th>
<th>Straight Sections</th>
<th>Current Status</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Hard X-Rays</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>European Synchrotron Radiation Facility (ESRF) Grenoble, France</td>
<td>850</td>
<td>6</td>
<td>≥100</td>
<td>6.2</td>
<td>32</td>
<td>Operating</td>
</tr>
<tr>
<td>Advanced Photon Source (APS) Argonne National Laboratory Argonne, Illinois, USA</td>
<td>1,104</td>
<td>7</td>
<td>≥100</td>
<td>≤10</td>
<td>42</td>
<td>Construction</td>
</tr>
<tr>
<td>Super Photon ring-8 GeV (SPring-8) Harima Science Garden City Hyogo Prefecture, Japan</td>
<td>1,436</td>
<td>8</td>
<td>100</td>
<td>5.6</td>
<td>48</td>
<td>Construction</td>
</tr>
<tr>
<td><strong>Ultraviolet/Soft X-Rays</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Advanced Light Source (ALS) Lawrence Berkeley Laboratory Berkeley, California, USA</td>
<td>197</td>
<td>1.0–1.9</td>
<td>400</td>
<td>3.4</td>
<td>12</td>
<td>Operating</td>
</tr>
<tr>
<td>BESSY II, Berliner Elektronenspeicherring-Gesellschaft für Synchrotronstrahlung Berlin, Germany</td>
<td>240</td>
<td>0.9–1.9</td>
<td>100</td>
<td>6.1</td>
<td>16</td>
<td>Construction</td>
</tr>
<tr>
<td>Elektra, Sincrotrone Trieste Trieste, Italy</td>
<td>259</td>
<td>1.5</td>
<td>400</td>
<td>4.0</td>
<td>12</td>
<td>Operating</td>
</tr>
<tr>
<td>Laboratório Nacional de Luz Sincrotron (LNLS) Campinas, Sao Paulo, Brazil</td>
<td>93</td>
<td>1.15</td>
<td>200–400</td>
<td>7.1</td>
<td>10</td>
<td>Construction</td>
</tr>
<tr>
<td>MAX II, MAX-Laboratory University of Lund, Lund, Sweden</td>
<td>90</td>
<td>1.5</td>
<td>200</td>
<td>8.8</td>
<td>12</td>
<td>Construction</td>
</tr>
<tr>
<td>Pohang Light Source, Pohang University of Science and Technology Pohang, Korea</td>
<td>281</td>
<td>2.0</td>
<td>100</td>
<td>12.1</td>
<td>12</td>
<td>Construction</td>
</tr>
<tr>
<td>SuperACO, Laboratoire pour l’Utilisation du Rayonnement Electromagnétique (LURE) Orsay, France</td>
<td>72</td>
<td>0.8</td>
<td>400</td>
<td>37</td>
<td>8</td>
<td>Operating</td>
</tr>
<tr>
<td>Synchrotron Laboratory of Catalonia Barcelona, Spain</td>
<td>~250</td>
<td>2.5</td>
<td>200–300</td>
<td>15–30</td>
<td>12</td>
<td>Approved</td>
</tr>
<tr>
<td>Synchrotron Radiation Research Center (SRRC) Hsinchu, Taiwan</td>
<td>120</td>
<td>1.3</td>
<td>200</td>
<td>19.2</td>
<td>6</td>
<td>Operating</td>
</tr>
</tbody>
</table>
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spots as small as 35 nanometers in diameter, so that researchers can, for example, look for a feature in an absorption spectrum at each point on the sample surface. If that feature were associated with a particular structure or composition, the experimenter would have a map of its locations and might be able to relate this map to the performance of the material. The key is to be able to focus enough of the light to generate a measurable signal from this tiny area in a reasonable time, so that the experimenter can scan the surface in a few minutes, rather than hours or days. Even the X-1 undulator, which until recently has been the brightest synchrotron X-ray source in its wavelength range, is limited in this capability.

THE THIRD GENERATION

Synchrotron-radiation scientists quickly understood that the second-generation synchrotron sources, which were designed without insertion devices in mind, were not the ideal hosts for these devices, particularly undulators. This realization drove a whole new campaign to build "third-generation" sources. In general, two features characterize the third generation sources. The first is the inclusion of many long straight sections in the storage ring, because the brightness of an undulator increases directly with the length of the device. The second is a refinement of storage-ring design to further reduce the emittance because the brightness is inversely proportional to the emittance squared (see box on pages 20–21). The combination of long undulators with a low-emittance storage ring results in a synchrotron light source that is truly laser-like. However, both wigglers and bend magnets also benefit from the low emittance, and third-generation facilities effectively utilize all three types of sources.

In the United States, after the requisite number of studies and workshops for political-base building necessary to obtain federal funding for a large project, the campaign resulted in approval for two third-generation sources—the Advanced Light Source (ALS) at LBL and the Advanced Photon Source (APS) at Argonne National Laboratory. Following the NSLS pattern with separate storage rings for long wavelengths [ultraviolet and so-called soft X-rays] and short wavelengths [so-called hard X-rays], these third-generation facilities specialize in one or the other. The ALS, completed in the spring of 1993 and now operational, uses a 1.5 GeV storage ring to generate ultraviolet and soft X-ray light. The first two undulators installed and working in the ALS replace the X-1 undulator at NSLS as the world's brightest soft X-ray sources for scientific research. The APS, under construction and shooting for a fall 1995 start-up of its 7 GeV storage ring, will be the hard X-ray source, although there is a region of overlap between the two facilities.

The Department of Energy is funding the construction and operation of both the ALS and the APS as national user facilities open to researchers from industrial, university, and federal laboratories. Support for instrumentation comes from a mixture of federal and industrial sources. The ALS cost $99.5 million to build, but when R&D and the cost to bring the facility into operation are included, the total rises to about $150 million. The APS construction cost is now estimated at $467 million, but like the ALS, the total cost to bring the facility into operation will be considerably higher. Moreover, the cost to equip synchrotron light sources with instrumentation for conducting experiments can match or exceed the construction cost.

Synchrotron radiation remains a highly competitive activity. In Europe the chief rival to the ALS is the Sincrotrone Trieste in Italy, which has been completed and is operating. Organized as a private corporation, the Trieste facility nonetheless receives much of its funding through the Italian government. Several additional third-generation sources of soft X-rays based on storage rings with energies from 1.5 to 3 GeV are in various stages from construction to proposal in Brazil, France, Germany, Russia, Sweden, Spain, Switzerland, Ukraine and the United Kingdom.

The counterpart to the APS is the European Synchrotron Research Facility (ESRF). Located in Grenoble, France, the ESRF is a western European project with support for construction and operation coming from 12 countries. Centered around a 6 GeV storage ring, the ESRF is actually the first large third-generation synchrotron light source to finish construction and begin experiments. Asia is keeping up the pace, as well. In Harima Science Garden City west of Osaka, an immense project to build Super Photon ring 8-GeV (SPring-8) is under way under the auspices of the Japan Atomic Energy Research Institute and the Institute of Physical and Chemical Research—both nonprofit research institutes of Japan's Science and Technology Agency. With an 8 GeV storage ring more than 1400 meters in circumference, SPring-8 will be available for research in 1998.

In Korea, the Pohang Light Source at the Pohang University of Science and Technology will begin operating a 2 GeV storage ring later this year for the production of soft X-rays. The Synchrotron Radiation Research Facility in Hsinchu, Taiwan, has already begun producing synchrotron light with a smaller 1.3 GeV storage ring. Soft X-ray sources have also been proposed in Japan and India.

THEY WORK

Two key ingredients in all the third-generation synchrotron light sources are scrupulous attention to engineering design and the ability to manufacture and install massive,
A demonstration microstructure that could be used in a temperature sensor of an automobile engine. It was fabricated at the Advanced Light Source by LBL researchers using deep-etch X-ray lithography.

complex pieces of equipment to exacting specifications. In the ALS undulators, for example, the electron beam size is oval-shaped with a width of about 335 micrometers and a height of about 65 micrometers, or about five human hairs wide and one high. Moreover, to be useful to experimenters, the synchrotron light beams must not move significantly, which means that the electron beam must not move either. A typical requirement is that the beam position be stable to within one-tenth of its size.

To maintain the small beam size and to achieve this degree of stability, the approximately 200 dipole, quadrupole and other magnets around the 197 meter circumference of the ALS storage ring must be precisely positioned to within about 150 micrometers of their ideal locations. And it doesn't do any good to have the magnets in the right place if their power supplies cannot provide comparably stable currents to keep the magnet fields steady. Even then a sophisticated system of about 100 electronic beam-position monitors carefully tracks the beam behavior so that corrections can be made to the magnet currents, thereby adjusting the beam positions.

The undulators themselves have to be built to even higher standards. Incorrect positioning of the magnet poles leads to magnetic-field errors that can severely degrade the brightness of the light. Typically, each pole piece of the magnetic structure must be in its correct vertical position to a precision of better than 20 micrometers over the 5 meter length of the devices. These tolerances must be maintained in a structure that can withstand magnetic forces of up to 40 tons.

Given the complex technology and exacting construction specifications, it is remarkable that third-generation light sources are turning on so quickly. Comparatively speaking, the storage rings in the sources that have reached the operational stage are turning on like light bulbs, with commissioning times measured in months rather than years. The ESRF, for example, reached all its major design goals in less than a year of storage ring commissioning and the ALS is on the same schedule.

In the end, the test of a synchrotron-radiation source is the light delivered to the many hundreds of researchers who use the facility. Moreover, in this age of intense international economic competitiveness, it is certain that government and industrial sponsors of these facilities are counting on them to contribute—either through basic and applied research or by direct involvement in process development and production—to success in the global marketplace. Some examples where such contributions are expected include (figures cited represent estimated worldwide market values):

**Semiconductors.** Manufacturing microchips for computers and electronic equipment is a $100 billion-per-year business. In order to continue the march toward ever more miniaturized devices, development of advanced technology—such as projection X-ray lithography for imprinting tiny circuit patterns on semiconductor chips—is required. Equally important are improved techniques for super-sensitive test-
ing of semiconductor materials to detect trace amounts of damaging impurities that make chips unusable.

Data Storage. Magnetic computer-data storage is another multibillion dollar market (estimated at $50 billion per year), but new magnetic materials capable of reading and writing each bit of information in a smaller area are needed in order to increase storage capacity at a rate matching the insatiable growth in demand.

Polymers. With annual sales in the $500 billion range, polymers represent a huge market for an amazing variety of products from the pedestrian to the most high tech. Petrochemical companies are constantly on the lookout for new materials with improved properties.

Pharmaceuticals. Manufacture of pharmaceuticals is a $150 billion-per-year industry, yet, until recently, development of new drugs was largely a trial-and-error process. With the ability to study the atomic structures of disease-causing agents and their target structures in cells, biologists hope to design pharmaceuticals with just the right molecular structure to block the action of viruses and other causes of disease ranging from the common cold to AIDS.

These examples by no means exhaust the possibilities. Already, the evidence in the short history of the field is that every jump in brightness dramatically expands the range of opportunities. In the coming years, we'll be seeing whether these third-generation synchrotron light sources can indeed deliver on their immense potential.
It was August 1958 when I first heard of Rolf Wideröe. During one of the famous Varenna Physics Schools on Lake Como my friend Bruno Touschek told me of a brilliant Norwegian engineer who in 1943 had brought schnapps and cigarettes after the Gestapo had imprisoned Touschek in Hamburg’s Fuhlsbüttel jail as a result of his fondness for reading foreign magazines. At that time the engineer believed he had had a magnificent idea that would make it possible to build a far more effective atom smasher than had previously been possible. He had applied only well-known laws of physics, but Touschek, as a theoretical physicist, thought that Wideröe’s ideas were not publishable at all as scientific work, for they seemed to him far too trivial and half-baked.

Wideröe, however, would not let go, and he submitted his ideas for patenting. This patent is now regarded as the invention of the storage ring, a type of particle accelerator which today is used throughout the world and finds application in fundamental research as well as for many practical purposes.

Among experts, Wideröe is generally regarded as the “grandfather of all modern particle accelerators”: the inventor, or co-inventor, of many of the most fundamental ideas on this subject, and perhaps even a legitimate candidate for the Nobel Prize. Some of Wideröe’s work did not become well known among physicists until relatively late; after all, patents do not generally appear in scientists’ required reading lists. Many of his ideas were therefore rediscovered by others or developed simultaneously. However, this does not in any way affect the historical facts or the value of Wideröe’s creative and constructive work.

Wideröe himself is an extremely interesting and multifaceted human being. He was born in Oslo, Norway, on July 11, 1902. His early interests included Rutherford’s first disintegration of the nucleus [1918] and the use of high voltage technology to accelerate particles. By 1922, when he was twenty and working as a student of electrical engineering in Karlsruhe, he had already dreamt up the design of a ray transformer, a device that would later become famous as the betatron, an accelerator ring for...
electrons that was widely used to produce X-rays. This is the subject that runs like a thread through his entire life. He then made drawings and calculations in his notebooks, using Maxwell's equations and Einstein's relativistic mechanics. Here he found the famous 2:1 relation between the accelerating and guiding field of a betatron that carries his name.

In 1926, he tried to submit these ideas for a doctoral thesis in Karlsruhe, where it was rejected. Nevertheless, his proposal was understood in the Technical University of Aachen (RWTH), where he actually built such a device, using parts of a standard transformer and a specially made small glass ring, as seen in the bottom left drawing. But this ray transformer refused to function. Electric charges deposited on the inner walls of the tube prevented injection. In principle, Wideröe's machine could accelerate electrons, but the magnetic field was not adequate to keep the particles on stable orbits during the acceleration process.

Wideröe then went on to build a straight-ahead or linear accelerator, following an idea of the Swedish physicist Gustav Ising (see sketch top right). This one did work, and with only 25,000 volts at his disposal, he accelerated atomic nuclei as if 50,000 volts were available. It was the first successful drift tube, the birth of the linear accelerator or linac and the basic principle for the subsequent development of all modern particle accelerators. This earned Wideröe his doctorate in electrical engineering.

In California, Ernest Orlando Lawrence saw Wideröe's thesis, published in the journal Archiv für Elektrotechnik, and from the illustrations deduced the principle with which he went on to invent the cyclotron, and for which he was eventually awarded a Nobel Prize. Lawrence always insisted on citing Wideröe's pioneering work. This perhaps explains why Wideröe is now so relatively well known in the United States. Fortunately, Lawrence did not understand Wideröe's concerns about the lack of stability of circular orbits. Cyclotrons, which can be considered as curved
The Widerøe relay to protect power plants after a short circuit in a high-voltage line.

A recent photograph of Rolf Widerøe with a model of his first linac, exhibited in the German Röntgen Museum in Remscheid.

The Widerøe relay to protect power plants after a short circuit in a high-voltage line.

The 15 MeV betatron (the second in Europe) built by Widerøe in Hamburg in 1943-44. It was first operated in the summer of 1944.

A recent photograph of Rolf Widerøe with a model of his first linac, exhibited in the German Röntgen Museum in Remscheid.

A new photograph of the Widerøe relay, which protected power plants after a short circuit in a high-voltage line.

The 15 MeV betatron (the second in Europe) built by Widerøe in Hamburg in 1943-44. It was first operated in the summer of 1944.

Drift tubes in which the particles are guided by a magnetic field, were essential instruments for the development of nuclear physics.

After finishing his dissertation in 1928, Widerøe joined industry where he developed relays, first in Berlin and then in Oslo. These were probably the best relays available at that time for interrupting the current after a short-circuit occurred in a power line. They could also indicate how far away from the relay the short-circuit took place. These so called "Widerøe relays" were later successfully manufactured in Norway and also employed in other countries. Widerøe did not just develop and build these relays, he also sold them for an electricity company and would often deliver and install them himself.

In 1941 Widerøe's interest in accelerators was rekindled when he learned about Donald Kerst's first successful betatron, built in Illinois. Kerst had used Widerøe's 2:1 relation and the beam-stability conditions first developed by Ernest Walton in 1929, then rediscovered and patented by Max Steenbeck in 1933. Widerøe immediately restarted his work on the subject, submitting several patents and publishing a paper in Germany.

In 1943, hoping to free his brother Viggo (a pioneer of Norwegian aviation, founder of today's Widerøe Air Line and an active participant in the resistance) from German imprisonment, he agreed to go to Hamburg to build a ray transformer, or betatron, for the German Luftwaffe. This had been his youthful dream anyway. An unknown physicist had convinced a few experts of the Luftwaffe of the idea of using X-rays against enemy aircraft. Widerøe however, knew nothing of this, and serious physicists eventually persuaded the Luftwaffe to drop this plan.

In Hamburg, a 15 MeV betatron was built and successfully operated in 1944, with the help of the experimental physicists Rudolf Kollath and Gerhard Schumann and of theorist Bruno Touschek (see figures on next page). It eventually ended up (in late 1945) as booty of war in England, where it served to X-ray large steel slabs. Widerøe himself landed in a Norwegian prison as a collaborator. The well-known scientist Odd Dahl and a few other friends managed to persuade the Norwegian authorities of Widerøe's innocence, and he was released after 48 days in prison.

It was in Hamburg in 1943 that Widerøe first wrote down his original ideas about storage rings, in which particles traveling in opposite directions inside circular vacuum pipes were to be made to collide. The energy gained in the acceleration could be much more effectively used...
Anello di Accumulazione, or AdA, the first $e^+e^-$ storage ring ever operated (February 1961). It was built in Frascati, Italy, following a proposal by Bruno Touschek.

than in fixed-target experiments, and the beams stored in the rings might be able to provide the required collision rates. The corresponding German patent was kept secret during the war but retrospectively recognized and published in 1953. This patent noted (besides some ideas that could never be realized) the use of a magnetic field to keep particles of opposite electric charge on counter-rotating orbits. Since at that time only the betatron was known to provide stable orbits, Widerøe proposed a system of betatrons as injectors and storage rings.

In 1956, after a new type of ring accelerator, the synchrotron, had already been devised and tested, groups led by Donald Kerst of Illinois and Gerald O'Neil of Princeton proposed the first realistic storage rings, through the use of two synchrotrons with a common interaction region. These authors probably did not know about Widerøe's patent. Such a system of two electron rings, was actually built at Stanford by Carl Barber, Gerald O'Neil, Bernard Gittelman, Wolfgang Panofsky, and Burton Richter, and was used for an experiment beginning in 1962 to test quantum electrodynamics at short distances.

Even earlier, in February 1961, in the Frascati Laboratories near Rome, Bruno Touschek had managed to operate the first storage ring, using Widerøe's idea for electrons and positrons (Widerøe had proposed protons as positive particles), that collided beams traveling in a single ring. And he certainly knew about Widerøe's patent. Today, storage-ring synchrotrons with colliding beams are the principal instruments that are used to investigate the smallest building blocks of matter.

During his initial studies in Karlsruhe and Aachen, Widerøe had already been concerned about the stability of the orbits of charged particles within accelerator rings. Later on his thinking on this subject resulted in a Norwegian patent (submitted in January 1946), with a good deal of mathematical detail, which contained most of the important ideas required for the construction of a synchrotron. Similar suggestions were made at the same time in other countries, in particular by Edwin
McMillan in the US and Vladimir Veksler in the USSR. These led to the construction of the first large synchrotrons, the accelerators that could reach much higher particle energies than the cyclotron. As Wideröe notes: “The subject was in the air” at that time.

In another early patent (from September 1943), Wideröe proposed a first method for a kind of strong-focussing system for accelerators. Years later, he was one of the first to understand the brilliant ideas of Ernest Courant, Stan Livingston, and Hartland Snyder who devised the true strong focussing scheme in 1952. With Odd Dahl and Frank Goward, Wideröe advised CERN to build the planned Proton Synchrotron (PS) as a strong focussing machine, thus making it possible to reach 28 GeV instead of the originally proposed 10 GeV for about the same cost.

After the war Wideröe built betatrons for the company Brown Boveri (now called Asea, Brown Boveri) in Switzerland. Over the years a total of 78 betatrons were delivered. Some of these served to X-ray large industrial components, but most were used in hospitals for radiation therapy of cancer patients. The energy of the electrons reached first 31 and later on 45 MeV, thus making it possible to irradiate deep tumors with better efficiency [and less damage to normal tissue] than the radiation of normal X-ray tubes or Cobalt-60 devices. After 1956 it was also possible to extract the electron beam from Wideröe’s betatrons and use it for radiation treatments. Wideröe then began to dedicate himself to studying the effects of radiation on the human body. He tried to understand the physical effects of photon and electron ionization in killing cells, and in particular the advantages of using the available higher energies. He called it megavolt therapy.

Wideröe proposed a model and a formula to compute the cell-killing effect of radiation, the two-component theory, which drew great attention. It was extensively tested and applied by Professor Werner Schumacher in the Rudolf Virchow Hospital in Berlin, using two of Wideröe’s betatrons from Brown Boveri. Later on, Wideröe’s theory was extended to three components by...
radiologist Lionel Cohen, who developed very interesting computer programs to calculate doses for the accurate irradiation of patients.

The megavolt therapy started by Wideröe has had great success throughout the world. After 1970, small electron linacs first developed at Stanford could be produced less expensively than betatrons, and they proved to be smaller, more reliable, and more practical.* These linacs follow in principle the original idea of Gustav Ising [see top right sketch on page 30]: A traveling wave accelerates the electrons to an astonishing 20 MeV in only 60 cm of tube.

Wideröe was awarded an honorary doctorate in engineering from the Rheinisch-Westfälisch Technische Hochschule (RWTH) in Aachen in 1962, and in 1964 he received an honorary medical doctorate from Zurich University. These are only two of his many awards and other distinctions. He was a teaching professor at the ETH in Zurich from 1953 to 1973.

When the first large particle accelerators were built by the two research centers CERN in Geneva and DESY in Hamburg, Wideröe was called in as a consultant. His consistently interesting questions, comments, and suggestions can also be found in the proceedings of many an international conference on particle accelerators.

*Editors' Note: See the article by John Ford, "Little Linacs Fight Cancer," in the Spring 1993 issue of the Beam Line.

NOWADAYS ROLF WIDERÖE and his wife Ragnhild live a happy pensioner's life in a lovely house on a hill with a view over the Obersigge Valley and the city of Baden in Switzerland. Every Saturday he welcomes his children and grandchildren for lunch, and every year he celebrates his birthday with friends and relatives in Oslo. He likes to stop over in Hamburg, where he visits old friends, including those at the DESY research institute. And it is with astonishing freshness and enthusiasm that he recounts his life and work.
The fox knows many things, but the hedgehog knows one big thing.

If this is true (and I have met only one hedgehog—briefly—and no foxes at all), then most practicing astronomers, who know that Type II supernovae occur among Population I stars and that surge flares are not the same as flare surges, identify with the foxes, in contrast to our hedgehog-like particle physics colleagues, who know how to superunify.
Nevertheless, my allotted pages in this issue and the next one or two will focus on the biggest astronomical things we know in the waning years of the 20th century, beginning with the source of solar and stellar energy. Since you have surely heard the answer before—fusion of hydrogen to helium, familiarly called hydrogen burning—the real question is how did we come to know it with so much confidence that most astronomers have never really worried about the advertised deficit of solar neutrinos?

The question so phrased demands an answer in historical form. Please remember, though, that a historian of science or a philosopher of epistemology would probably tell the story in a very different way.

FOUNDATIONS AND METHODOLOGY

Where to begin? If we start with the positive whole integers and try to reason forward from there, we will be very late for tea. At the other extreme, if you already accept that the standard picture of stellar structure and evolution is right, then there is nothing left to discuss. Let us somewhat arbitrarily agree to accept as true undergraduate physics up to 1940 or so. The minimum foundation to support the astronomical superstructure then includes conservation of energy, thermodynamics and statistical mechanics, Maxwell’s equations, and special relativity. A little later, you will also be asked to join, at least temporarily, the believers in quantum mechanics and the simpler parts of general relativity.

How to phrase the argument? A very common style of justification for scientific knowledge has two parts. First, show that the suggested mechanism works. Then show that all the plausible (or even implausible) alternatives fail or work less well. For instance, the stripy pattern of positive and negative magnetic anomalies on either side of the mid-Atlantic ridge is ascribed to the operation of sea-floor spreading plus sporadic reversals in the earth’s magnetic field direction [a] because this mechanism will indeed produce that sort of pattern and [b] because nobody has thought of any other very good way. Unattractive alternatives include a previously existing ridge-and-valley structure, filled in by a single lava flow during a period of reversed magnetic polarity, or sequential lava flows during many epochs of alternating magnetic polarity, each of which travels less far than its predecessor.

For the stellar energy case, we will follow the Sherlockian model of first eliminating the impossible, so that, whatever remains, however improbable, must be the truth.

TIME SCALES AND ENERGY SOURCES

The earth and sun have aged with remarkable rapidity since Newton attempted to create a coherent cosmology beginning with a biblical creation event six or so thousand years ago (though not necessarily in 4004 BCE; and even Archbishop Ussher said noon, not 9 a.m.). With only this time scale to account for, the sun could just about live on chemical energy. You can figure it out for yourself. Chemical reactions yield, at best, about 1 eV per atom or 200 kcal per gram. The mass of the sun is \(2 \times 10^{33}\) g and its luminosity \(4 \times 10^{33}\) erg/sec. The rest is arithmetic, and if you have gotten the right answer, the end of the world is at hand.

In fact, no one ever did this calculation seriously or believed that chemical energy was the right answer. By the time conservation of energy was part of the scientist’s tool kit, so also were more powerful sources. The real message of the 10,000 year time scale is that an age determined from written records will inevitably come close to the age of writing.

The first serious questions about the age of the earth were asked by geologists: How long would it take to build up layers of Nile mud to the thickness of the Jura

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1 What is the difference between an astronomer and an astrophysicist? Probably about $10,000 a year. Or whether you are willing to try to explain why you don’t cast horoscopes (astrophysicists hardly ever get asked). Or maybe it is the size of the bulge your tongue makes in your cheek when you write about the solar neutrino problem and the first \(10^{-23}\) seconds of the universe.
Mountains? How long for the earth to cool to its present temperature from a very high one (if you don’t know about internal radioactive heating)? How long for the oceans, fed by rivers carrying solutes, to build up to their present salinity (if you don't know that salt can be precipitated out again)? Something like 10 million years, said the savants of the 18th and early 19th centuries. And the physicists were by then prepared to meet them with conservation of energy and the concept of gravitational potential energy, gradually released by a contracting star. The available time is then of order $GM^2/RL$, and the margin of this page is probably wide enough for you to check that, for a solar radius of $7 \times 10^{10}$ cm, you can have 10 or 20 million years for geology.

The post-Darwinian generation of biologists and paleontologists wanted more. Evolution from a slime mold to a politician is a very slow process (at least in one direction), for which mere millions of years do not suffice. Contemporary response from the physics community (Lord Kelvin comes to mind, as he so often does when looking for a horrible 19th-century example) appears to have been roughly at the level of “Nyah, nyah; we’re an exact science, and you aren’t.”

In fact, the contraction hypothesis already had a self-consistency problem, though it was a few years into the 20th century before anyone noticed. Certain pulsating stars, of which Delta Cephei is the prototype, are both very bright and very extended. To keep them shining with gravitational potential energy, their radii must decrease by a part in 40,000 per year, and so their densities increase by a part in 13,000 per year. Gravitationally driven pulsation is like free-fall, that is, the period $\sim (G\rho)^{-1/2}$. The implied period change for Delta Cephei itself, 17 seconds per year, seems small. The star has, however, been under continuous observation since 1785 and has not slipped out of phase by even 10% of that amount. The Cepheids, at least, are not contraction powered. There was a brief period of enthusiasm for heating the sun by infalling meteorites etc. Although lots of such accretion could stretch out the future life a good deal, it cannot change the past.

But rescue was on the way. Ernest Rutherford published his first radioactive dating of a rock in 1905, followed closely by Boltwood and Soddy. The physicists' earth was now at least a billion years old, and the sun could hardly be younger. What is more, the proof of the problem also suggested part of the solution, quickly dubbed subatomic energy. First, decay of known radioactive elements would warm the interior of the earth, drawing out the cooling time more or less to whatever
was wanted. Second, these or other similar processes might contribute to energy production in the stars themselves.

For the sun, known radio-nuclides could be ruled out at once. A pure uranium sun, given the measured decay rate, would be a bit less than half as bright as the one we orbit. With 20–20 hindsight, it is a grave temptation to skip immediately to Francis William Aston and his mass spectrograph (which revealed that the mass of a helium atom is less than that of four hydrogens by 0.8%). Honesty, however, compels a brief excursion along yet another blind alley.

While geologists, biologists, and many astronomers were at last content to have $10^{10}$ years for their processes, Sir James H. Jeans wanted still more. He had in mind using stellar encounters to produce the observed eccentricities of binary star orbits and the smooth, relaxed appearance of globular star clusters and elliptical galaxies—over the past $10^{13}$ years. Neither fission nor fusion could possibly suffice to keep his stars alive. Only complete annihilation of protons and electrons had a hope, and, between 1915 and 1929, Jeans formulated a picture of stellar evolution in which half or more of a star's initial mass was radiated away, leaving only just enough for a white dwarf cinder. Rutherford had this picture in mind in 1929 when he reported an age for the earth of $3.4 \times 10^9$ years from the ratio of $^{235}\text{U}$ to $^{207}\text{Pb}$ and interpreted it to mean that the sun must currently be producing uranium. Jeans' belief in the long time scale and a correspondingly copious energy source seems to have survived the events of the next two sections, and he went to his grave in 1946 as, very probably, the last remaining annihilationist.\(^2\)

EDDINGTON, HYDROGEN FUSION, AND BARRIER PENETRATION

Francis William Aston shared Jeans' 1877 birth year and died a year earlier in 1945. The only other thing most of us know about him is that he designed and built the first mass spectrograph, in which positive ions pass through electric and magnetic fields that segregate them into clumps with the same charge-to-mass ratio. His first contribution was to show that most failures of the "small whole numbers" hypothesis for atomic weights (for instance in neon and chlorine) were due to mixes of isotopes. Astronomers, however, remember him for the counterexample—the mass deficit of $^{4}\text{He}$ relative to four $^{1}\text{H}$ atoms, reported in 1920, which is not a result of failing to account for $^{2}\text{H}$ or $^{3}\text{He}$ contamination! The measured deficit plus special relativity promise you about

\(^2\) The reader is encouraged to provide his own matching story about the last creationist.
$7 \times 10^{18}$ erg/gram, if you can somehow make hydrogen fuse to helium.

Early enthusiasts for stellar "subatomic energy," especially helium formation, included Henry Norris Russell (R of the HR diagram, of whom more later), the French physicist Jean Perrin, and Arthur Stanley Eddington. But the greatest of these is Eddington, both because he provided a thorough, book-length discussion and because he seems to have made all the good jokes. Widely quoted is a 1920 remark that "What is possible in the Cavendish Laboratory may not be impossible in the sun." This concerns transmutation of elements in general, and refers not to the first transmutation induced by artificially accelerated particles (that was Cockcroft and Walton, 1932) but to Rutherford's 1919 exposure of nitrogen to alpha particles from radium C, which liberated high velocity protons—$\text{N}^{15}(\alpha,\text{p})\text{O}^{17}$ we would now say, and radium C is $\text{Bi}^{214}$.

The book (which having once learned to call *The Infernal Constitution of the Stars* you will ever after have trouble naming correctly) includes a thorough discussion of the problems as well as the advantages of subatomic, especially fusion, energy. Many of the problems are purely astronomical, having to do with the wide range of rates of energy release vs. stellar mass needed if you want to power both giant stars (like Capella) and dwarfs like the sun, and how you can fold them into a theory of stellar evolution based on gradual exhaustion of fuel. The missing pieces in this part of Eddington's puzzle were [a] the extraordinarily steep temperature dependence of fusion reactions, such that factors of two in $T$ can cause hundred-fold changes in reaction rates, and [b] the difference in interior structure of giants and dwarfs that comes from the former having actually completely exhausted their central fuel supply.

The more fundamental of the problems laid out in sections 204 and 209 of *Infernal Constitution* are those of getting the hydrogen atoms together close enough to fuse and of having enough of them to start with. First the approach problem. Use a bit of margin to persuade yourself that, if you need to get two protons within one fermi or femtometer of each other classically, then
\[ \frac{c^2}{r} = kT \] requires a temperature near \(10^{10}\) K. But Eddington has just spent chapters 1-6 persuading himself and you that stellar central temperatures are a few times \(10^7\) K. The argument is too simple to be much wrong. It starts by observing that stars are in hydrostatic equilibrium (if the sun were not, you would notice over the next 15 minutes or so), and so gravitational forces inward must be balanced by pressure gradients outward, provided by some combination of \(PV = NRT\) and radiation.

"But," he says, "we do not argue with the critic who urges that the stars are not hot enough for this process; we tell him to go and find a hotter place." A. S. Eddington, The Internal Constitution of the Stars

\[ \alpha \]

PAYNE, RUSSELL AND THE CHEMICAL COMPOSITION OF THE STARS

Despite the \(7 \times 10^{18}\) erg/gram available from hydrogen burning, stars will not live long on it unless there is lots of hydrogen to start with. Eddington set the minimum to survive \(10^{10}\) years at 7% for the sun and was most reluctant to accept even that high a value, though his own best estimate of the opaqueness of stellar material to radiation implied up to 20% H in some stars. He was bothered by a funny sort of anthropic consideration, believing that the commonest sorts of stars should have radiation and gas pressure equal at their centers. This is simply not true for stars as we understand them today; but it limited hydrogen content to, at most, a few percent in stars made, he thought, mostly of the same elements as the earth—iron (especially), silicon, oxygen, and such.

Strangely, it is easier to get elemental abundances from the lines in stellar spectra for the less common elements than for the dominant ones, if you insist upon calculating the best model atmosphere you can. The problem is that absorption lines form because the gas is more opaque in the line than in the adjacent continuum. But how opaque is the continuum? That depends on the dominant constituent and what it is doing! Eddington's was iron. Anton Pannekoek is reported to have
tried a 99.4% hydrogen star in 1931—it was nearly transparent.

As long as you stay away from the dominant constituent, you can do a pretty good job by saying that, whatever causes the continuous opacity, it is anyhow the same for all the other, minor, constituents that absorb in a given wavelength region. The situation was not fully clarified until 1938, when Rupert Wildt pointed out that the negative hydrogen ion, H\(^-\), if stable as predicted, would introduce an enormous continuous opacity at optical wavelengths, just where it was needed to make sense out of the lines in coolish stars. A mix with lots of hydrogen and a few heavy atoms to supply extra electrons is every bit as opaque as Eddington's steel stars.

Meanwhile, Cecilia Helena Payne, as part of the first astronomy Ph.D. thesis completed at Harvard (and the first Harvard thesis on any subject by a woman), examined spectral lines in coolish giant stars. Crudely, she assumed that, as you go from one temperature star to another, lines from any element disappear when the number of atoms in the level required to absorb a particular line was equal to a fixed number (whose absolute value doesn't matter). That is, you have to know about ionization and excitation—the Saha and Boltzmann equations—and something about stellar surface temperatures (from their colors) to progress. And you learn only relative abundances.

Many of the elements that appeared to be common from this point of view were also on Eddington's earth-based list of major elements. But the real surprise was hydrogen. Lines visible over the full range of stellar temperatures arise from the first excited state, at 10.6 eV. A level this far above ground can never be
more than sparsely populated in thermal equilibrium. Thus the strong lines must mean that hydrogen atoms greatly outnumber everything else in the stars.

So Dr. Payne concluded in both published versions of her work. You must, however, look hard into her tables and text to find this last key piece in our demonstration that the stars do indeed run on nuclear fusion energy. The more conspicuous passages speak of “anomalous excitation conditions.” Henry Norris Russell is supposed to have been one of those who most strongly discouraged her from publishing her results more aggressively. It is, therefore, more than a little distressing to learn that the paper most often quoted as demonstrating that the sun (and so by implication the other stars) consists mostly of hydrogen is Russell 1929.

Cecilia Payne married Sergei Illarionovich Gaposchkin in 1932 and worked most of the rest of her life on variable stars, not stellar spectroscopy, which she always claimed as her first love. This was not a failure of courage on her part, but unavoidable response to instructions from above, in an era when stars as well as telescopes were made of iron, and some directors’ heads of wood.

SNATCHING DEFEAT FROM THE JAWS OF VICTORY: GAMOW, LANDAU AND NEUTRON CORES

Surely, you say, it is time to meet Hans Bethe and go home. But no, from out of left field comes one last, ill-thrown metaphor. George Gamow struck out first, in his 1937 book: “For still higher densities electrons will probably be absorbed by the nuclei [an inverse β-decay process] and the mixture will tend to a state which can be described very roughly as a gas of neutrons. . . . The question whether most stars at present actually possess such nuclei cannot, however, be answered definitely . . . but there seems to be no reason why they should not. The theory of stellar nuclei gives us another aspect of the question of the creation of the elements and the liberation of energy in stars. . . . As to the liberation of energy one can easily see that the pure gravitational energy liberated in the contractions to such immense densities will already be quite enough to secure the life of the star for a very long period of time.” In answer to two questions you might ask, (a) Yes, the commas are this sparse in the original book and (b) No, Gamow does not anywhere cite Baade and Zwicky on the subject of neutron stars.

Landau stepped up to the plate in early 1938 to show that degenerate neutron gases are more stable than degenerate electron gases for any mass larger than 0.05 M⊙; (if you can get to 10^{14} g/cm^{3} somehow) and to say “When the mass of the body is greater than the critical mass, then in the formation of the ‘neutronic’ phase an enormous amount of energy is liberated, and we see that the conception of a ‘neutronic’ state of matter gives an immediate answer to the question of the sources of stellar energy.” He makes estimates for the sun and Rigel (“Even for such a bright star as β Orionis, we find for the mass of the neutronic core only about 0.1 ☉”) and does not cite Baade and Zwicky either.

Such madness could not long persist. Later that year, in an American Physical Society meeting abstract, Gamow and Teller point out that reaching nuclear density with any reasonable equation of state carries you to 10^{9} K or so and “Under such conditions all kinds of nuclear reactions will proceed at a great rate and will make the total energy production of the star many orders of magnitude greater than the observed radiation. Therefore the core model as well as any other model leading to such high temperatures seems to be ruled out.” Most surprising, they attribute the discredited idea only to Landau 1938, not to Gamow 1937!

Scientific memory, however, is short, and the advent of the solar neutrino problem thirty-some years later prompted S. W. Hawking and others to a brief consideration of an alternative sun partly powered by accretion onto a small central black hole. Unfortunately, the infalling material gets so hot and dense that neutrino production exceeds that from a standard model (Clayton, Newman, and Talbot simply assumed zero neutrinos from the accreting gas). In addition, we end up
living at a very un-Copernican time, just before the black hole accretion runs away and swallows the entire sun.

HOME FREE: HANS BETHE AND THE SOLAR NEUTRINO PROBLEM

At last we have reached 1939, when Hans Bethe wrote down the specific sets of nuclear reactions that we now call the proton-proton chain [with Critchfield] and the CN cycle. Cross sections and Q values [energy released] could be measured in Kellogg and other labs and the measurements extrapolated to stellar conditions of temperature and density. There resulted the great postwar flowering of stellar structure and evolution calculations, beginning with Fred Hoyle and Martin Schwarzschild, that, carried on down to the present time, leads astronomers to say that this is one branch of our subject that is really understood.

From our original Sherlockian perspective, I think it is fair to say that all other energy production mechanisms so far suggested are impossible and that what remains, hydrogen fusion [however improbable?!] can be made to work.

The numbers of scientists also blossomed in the postwar years. Thus, when Raymond Davis first announced that the high-energy neutrinos expected from rare branches of the pp chain were not hitting his C\textsuperscript{12} atoms, only about 10% of the astronomical community even remembered a time when there had been any doubt about the source of solar and stellar energy. Within the brief flurry of speculative responses to the deficit [Bahcall gives an incomplete list of 23 ideas], only about three involve any significant change in solar energy production. Rather, suggestions focus on small changes in temperature distribution or on hiding the neutrinos from the chlorine. Later results reported from Kamioka, SAGE and Gallex have not changed this.

A happy surprise, as seen from the astronomical foxhole, came in the mid-1980s, when the weak interaction community suddenly claimed the solar neutrino deficit as hedgehog territory. In 1985, Bethe himself came out in favor of matter-catalyzed neutrino oscillations [MSW [Mikheyev–Smirnov–Wolfenstein] effect] as the dominant physics, and we astronomers said “Thank you again, Hans!” and happily went back to our Population II stars, Type I supernovae, surge flares, and flare surges, once again [or still] confident that the stars run on energy from hydrogen fusion.
The Classic Papers


ON MARCH 6 BILL ASH, editor of the Beam Line from 1982 to 1985, died after a brief, intense struggle with cancer. SLAC will sorely miss his wit and warmth. One of the poems Bill wrote for the Beam Line is reprinted here as an example. The last article he contributed to the Beam Line, "SLD Prepares for Physics," appeared in the 1990 Fall/Winter issue.

Bill Ash headed the group that built the superconducting final focus for the SLD, having earlier managed the contract with the Japanese firm that supplied the solenoidal magnet coil for this detector. He came to SLAC in 1972 to work on a polarized target for electron scattering experiments in End Station A. In 1976 he joined the group that built and ran the MAC detector at the PEP storage ring. His last project was the leadership of the SLD Vertex Detector Upgrade.

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Storage Ring Blues

*(A tribute, in uncertain meter and rhyme, To the problems of large storage-ring design.)*

There are three vicious demons, an ugly crew, Who are called Eta, Beta, and Delta Nu.

Beta's too big, Delta Nu is too small, And Eta shouldn't matter at all, But it does, you see, and all three conspire To keep luminosity from going much higher.

With PEP's sister, PETRA, things are the same. The ugly triumvirate's a mutual bane.

John Rees, PEP's Aedile, not one to scare, Has traced these three to their very lair.

When the beams cross, they disrupt one another. Push it too hard and they never recover. Delta Nu is the number whose value is set By the size of the current when this limit is met. There are hundreds of billions of charges, of course, And they push on each other with non-linear force. So no one can figure how big it will be. You must build the machine before you can see. Delta Nu, it was hoped, would be point oh six, but it's just point oh two, and there isn't a fix. As Nature would have it, this factor is paired, So the luminosity is down by about three squared.

Next we have Beta, from whose value is found How hard we can focus without losing ground. The more it decreases, the harder we squeeze. We're like to make it as small as we please. But make it small here and it gets bigger there, So this must be done with very great care. At present, in PEP, Beta's higher by two From where it should be when tuning is through.

Momentum changes are very small things, But distort the orbits in all storage rings. If Eta is zero, the effect goes away, And this is the case in all rings today. A little distortion, though not understood, Might just, after all, do PEP some good.

These three factors have made a serious dent: Luminosity is down to just four percent Of the value for L that everyone felt Scaling from small rings, like SPEAR, should have spelt. Playing with Beta and Eta may bring A doubling or trebling to this storage ring. But even without this, there is no distress, for the goal is a guide, not a measure of success.

And PETRA and PEP are running okay, And nothing is standing in either's way Of doing the job they set out to do, Not Eta, not Beta, and not Delta Nu.

—Bill Ash, SLAC Beam Line, 1981
CONTRIBUTORS

U.S. Representative GEORGE E. BROWN, JR., became Chairman of the Science, Space and Technology Committee in January 1991, bringing his scientific and technical training as an industrial physicist as well as extensive experience in local, state and national government.

First elected to the House of Representatives in 1962, Brown has been a member of the Science, Space and Technology Committee since 1965 and the Committee on Agriculture since 1973. He has spent almost three decades working to strengthen America's scientific and technological base and to improve the process by which science and technology can serve national needs. His strong support for institutionalizing long-range planning and investment in science and technology distinguishes Brown's tenure in Congress.

Educated at Oxford, where he earned his bachelor's degree in 1964 and his doctorate in 1967, CHRISTOPHER LLEWELLYN SMITH subsequently was a Royal Society Exchange Fellow at the Physical Institute of the Academy of Sciences in Moscow, a fellow in theoretical studies at CERN, a Research Associate at SLAC, and a staff member at CERN. He joined the physics faculty at Oxford in 1974 and became a full professor and chairman of the department in 1987.

Llewellyn Smith served as expert advisor to the Kendrew Commission review of high-energy physics and the Abragam review of CERN management. Prior to stepping in as CERN's Director General in January 1994, he served as Chairman of its Science Policy Committee.

A native of the San Francisco Bay Area, ARTHUR L. ROBINSON studied materials science at Stanford University as both an undergraduate and graduate student from 1959 through 1968. Following a tour with the U.S. Air Force at Wright-Patterson Air Force Base in Dayton, Ohio, he moved to Washington, DC in 1973 to become a reporter for Science magazine, where he covered developments in physical science and technology, including the rapidly expanding field of synchrotron radiation. Since 1987, Robinson has been a staff scientist with Lawrence Berkeley Laboratory's Advanced Light Source, where he has been able to continue a career combining writing and science.
PEDRO WALOSCHEK began working in particle physics as a student in Buenos Aires in 1951. He spent 1955–56 in Göttingen, Germany studying strange particles and then went to Italy, where he organized a bubble chamber group at Bologna. Later he was affiliated with a group at the University of Bari. In 1968, after two years at CERN, he started developing proportional wire chambers at DESY and as a senior scientist contributed to the design of PLUTO, a detector for the storage ring DORIS. Since 1978 he has written regularly for the general public. Many articles and six books (in German and English) testify to his prodigious activity, which he hopes to continue for many years.

VIRGINIA TRIMBLE divides her time between the Physics Department at the University of California, Irvine, and the Astronomy Department of the University of Maryland, College Park. Her degrees are from UCLA (B.A.), California Institute of Technology (M.S., Ph.D.), and Cambridge University (M.A.). She was the 1986 recipient for the U.S. National Academy of Sciences Award for scientific reviewing and currently serves as editor of Comments on Astrophysics and associate editor of the Astrophysical Journal. She is also vice-president of the International Astronomical Society Commission on Galaxies.
DATES TO REMEMBER


Jun 29–Jul 14  1994 DPF Summer Study on High Energy Physics: Particle and Nuclear Astrophysics and Cosmology in the Next Millennium, Snowmass, Colorado (C. M. Sazama, Fermilab, Box 500, Batavia, IL 60510 or SAZAMA@FNAL.GOV).


Jul 12–22  Enrico Fermi School on Biomedical Applications of Synchrotron Radiation, Varenna, Italy (E. Burattini, INFN, Frascati, I00044, Frascati, Italy, or BURATTINI@IRMLNF).

Jul 18–22  5th International Conference on Synchrotron Radiation Instrumentation [SRI 94], Stony Brook, New York [L. Lever, NSLS–7250, Brookhaven National Laboratory, Upton, NY 11973 or (516) 282-4746 or SRI-94@BNL.GOV].

Jul 20–27  27th International Conference on High Energy Physics [ICHEP] [by invitation], Glasgow, Scotland (Susan Lippmann or Julie Moore, Meetings and Conferences Department, IoP, 47 Belgrave Square, London SW1X 8QX, England or IOPCONF@ULCC.AC.UK).

Aug 2–6  1994 Meeting of the American Physical Society, Division of Particles and Fields [DPF 94], Albuquerque, New Mexico (DPF 94 Coordinator, Department of Physics, University of New Mexico, Albuquerque, NM 87131 or DPF94@UNMB.UNM.EDU).

Aug 8–Aug 19  22nd Annual SLAC Summer Institute on Particle Physics, Stanford, California (Ms. Lilian DePorcel, SLAC, MS 62, Box 4349, Stanford, CA 94309 or SSI@SLAC.STANFORD.EDU).

Aug 29–Sep 3  X-ray Absorption Fine Structure Conference, Berlin, Germany (XAFS Secretariat, Department of Physics, Freie Universität, Arnimallee 14, D–1000 Berlin 33, Germany).

Sep 12–14  European Physical Society Conference on Large Facilities in Physics, Lausanne, Switzerland (European Physical Society, POB 69, 1213 Petit-Lancy 2, France or EPNEWS@CERNVM.CERN.CH).

Sep 26–30  CAM 94 Physics Meeting, Cancun, Mexico (CAM 94 Organizing Committee, Sociedad Mexicana de Fisica, PO Box 70–348, 04511 Mexico, D.F., Mexico or CAM94@FIS.CINVESTAV.MX or FAX +52 +5 +754–68–01).

Oct 18–19  SSRL Users Conference, Stanford Linear Accelerator Center, Stanford, CA (Shirley Robinson, SSRL MS 99, Box 4349, Stanford, CA 94309–0210 or ROBINSON@SSRL01.SLAC.STANFORD.EDU).