



Trend-spotting: Physics in 1931 and today

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A lot has changed in the past 75 years. Can those changes help us guess where physics will be 75 years from now?

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A physicist who turns 100 this year would have been working on his doctoral thesis in 1931, the year the American Institute of Physics was founded. The student would have been surveying physics and its allied sciences to spot some promising area on which to stake a career. What would the 25-year-old have seen in 1931? And how did the apparently promising areas play out over the next three-quarters of a century? How does the 1931 landscape compare with what a physics student sees today? What important things would the 1931 student have failed to see? What can we learn from these questions as we look over our field today?

A quick way to compare the situations then and now is to look at the *Physical Review*. The most obvious difference is size. Last year's volumes take up about 30 times as much shelf space as did the two 1931 volumes—not to mention that the pages have gotten bigger and the print smaller. To be sure, the 1931 student also had to read *Zeitschrift für Physik* and *Nature*. Even so, the student could have read every important article in the field. Today such breadth is out of the question; dozens of subfields each publish more than the entire physics community did back then. To get an overview of physics nowadays, you must read review journals; scan news stories in *Science*, *Nature*, and *PHYSICS TODAY*; and—that old standby—talk with professors.

The triumph of the quantum

A physics student in 1931 could easily spot some exciting topics. Barely five years had passed since Erwin Schrödinger, Werner Heisenberg, and others gave quantum mechanics a solid mathematical foundation. Now they were struggling to extend and interpret the theory. Albert Einstein and Niels Bohr, the two founding intellects of the revolution, were arguing over the fundamental reality of quantum states. If that argument seemed closer to philosophy than to experimental science, quantum mechanics clearly had the potential to resolve long-standing scientific questions.

Physicists had understood for two decades that atoms were composed of electrons and nuclei, but at that point they had gotten stuck. Now in 1931, Paul Dirac proposed that the electron has an antiparticle, what would come to be called the positron. This was a hint that particles come in families with positive, negative, and perhaps neutral members. Dirac was a year short of his 30th birthday. Younger still, at 26, was Caltech student Carl Anderson, just getting into the cosmic-ray studies that in 1932 would demonstrate the positron's existence.

All kinds of bizarre ideas about particles were in the air. Wolfgang Pauli, for example, was developing the neutrino

hypothesis, although he wasn't quite ready to publish it. On the experimental side in 1931, Ernest Lawrence at the University of California, Berkeley, completed a little prototype cyclotron that accelerated protons to 1 MeV on the way to higher energies that he hoped would break into the nucleus. He had just turned 30; so had Robert Van de Graaff, who was developing another type of particle accelerator.

With such a rich smorgasbord of opportunities, where was a young physicist to begin? Einstein was embarked on a search for a unified field theory, and lesser mortals could at least try to go a step or two beyond quantum mechanics. That was the hope, for example, of 27-year-old Robert Oppenheimer, newly appointed to the Berkeley faculty. But such hopes were frustrated. Dirac's relativistic equations had put a capstone on quantum theory; the edifice was complete. Einstein's unified theory turned out to be mathematically ingen-



Enrico Fermi and Arnold Sommerfeld at a nuclear-physics conference in Rome in 1931, a time when the worldwide physics community was small enough that most forefront physicists were personally acquainted. (Courtesy of AIP Emilio Segrè Visual Archives.)



Accelerator physics then and now. Left: Ernest Lawrence in 1931 with his prototype cyclotron in hand. Right: The Aleph particle detector at CERN's 27-km-circumference LEP collider ring, being disassembled in 2001 to make way for an even larger detector for the Large Hadron Collider, which will begin operation in a year or two.



CERN

ious but physically vacuous, and nobody could do better.

Within the narrower scope of Einstein's general theory of relativity, an opportunity appeared in 1931 when an overlooked work by the Belgian cleric Georges LeMaître appeared in English translation. Only then did theorists read LeMaître's interpretation of the equations of general relativity and see a connection with Edwin Hubble's recent demonstration that distant galaxies appear to recede from us with velocities proportional to their distances. Hubble's discovery could be explained by a continual expansion of spacetime. Einstein immediately abandoned the cosmological constant he had introduced into the equations of general relativity to keep the universe from expanding.

The theoretical opening was, however, illusory. Attempts to push general relativity into new areas got no further. In 1939 Oppenheimer and a graduate student, Hartland Snyder, did come up with a particularly weird consequence of the theory—what would later be called “black holes.” But at the time, the Oppenheimer-Snyder discovery seemed little more than a mathematical curiosity that couldn't be connected to experiments or observations.

In hindsight, it is clear that one of the best roads forward for the physicist of 1931 was the one Lawrence was taking, along with theorists like Heisenberg, Hideki Yukawa, and others. That road led to the discovery and understanding of new nuclear particles. Physicists in 1931 recognized that the nucleus was a storehouse of enormous energies, but few expected to unlock it soon, if ever. Within 15 years, however, nuclear fission had totally changed international politics and seemed poised to revolutionize the world economy.

For physics, one consequence was a spectacular change in the scale of instrumentation. Nobody looking at Anderson's little cosmic-ray cloud chamber in 1932 could possibly have imagined today's gargantuan neutrino detectors. Similarly, nobody looking at Lawrence's 11-inch cyclotron in 1931 could have imagined the Large Hadron Collider with its 27-kilometer circumference, now nearing completion at CERN.

Such changes in scale brought profound changes in physicists' careers and work styles. In 1931 only a minority of *Physical Review* papers had more than one author, and usually no more than two. Today papers with hundreds of authors are not uncommon.

The giant instruments built in the decades after World War II created a baffling zoo of subnuclear particles. Whereas experimenters in the 1930s strove to “split” the nucleus, nowadays Brookhaven's Relativistic Heavy Ion Collider “melts” the proton into its constituents. The enigmas of the 1950s and 60s have been resolved in our present understanding of quarks and allied families of particles. Every experimental result, thus far, agrees with the predictions of the “standard model” completed in the 1970s. The standard model of particle physics is arguably the major accomplishment of physics in the second half of the 20th century.

That theoretical edifice places us, in 2006, at a quite different stage from where physicists stood in 1931. Linus Pauling was just then publishing *The Nature of the Chemical Bond*, using the new equations of quantum mechanics to explain the rules for covalent chemical bonding. Others were applying the new theory to calculate the positions and strengths of spectral lines with increasing precision. Today we are several decades into an analogous program with the standard model.

Despite the model's unflinching successes, basic questions remain unresolved. The very existence and number of the model's beautifully symmetric families of hadrons and leptons pose an unmet challenge. As quantum theory underlies the numerical regularities of atomic spectra, and as quarks underlie the great variety of hadrons that emerge from high-energy accelerators, so the grand order of the standard model surely requires a deeper explanation we have yet to grasp.

The standard model relies on the old foundation of quantum theory. Another feature of the contemporary scene is the renewed study of the now 80-year-old theory's fundamental characteristics. Lasers and other instruments of surpassing delicacy nowadays pour out a stream of demonstrations of the validity of Heisenberg and Bohr's interpretation of quantum mechanics. Yet those results only confirm Einstein's intuition that quantum physics is too paradoxical ever to be reconciled with ordinary understanding. Meanwhile physicists have also resumed the study of his general theory of relativity after a long period of little progress. Again instrumentation has led

the way, with new kinds of tests in terrestrial laboratories, in space, and in astronomical observations. Like quantum theory, Einstein's equations have passed all the tests so far.

The confirmations of both theories leave us dangling. The standard model is not the long-sought unified field theory. Its quantum basis has never been reconciled with general relativity. Some physicists hope that the role played by quantum mechanics in the 1930s may soon be played by some form of string or brane theory (see the essay by Jim Gates on page 54). Others suspect that string theory is more like the old aether theory of electromagnetism. In the 1890s, leading physicists felt they were on the verge of a great breakthrough with that theory, hoping that kinks in the aether and refinements of Maxwell's equations would soon provide a simple, unified explanation of all phenomena.

In the first decade of the new century, however, the mysterious quantum and the special theory of relativity showed physicists how long and strange would be the road they had still to travel. Not until 1931 did they reach their destination in Dirac's relativistic wave equation. In 2006, the situation of frontier theory is arguably more chaotic. Nobody knows when, or if, a breakthrough will come.

Using quantum physics

While the search inward from the quantum mechanics of atoms to subnuclear particles led to profound puzzles, a search outward into the world of ordinary matter brought extraordinary practical success. Around 1931 Eugene Wigner liked to tell his students at Princeton that if he dropped his glasses, the glass would break but the metal frame would not, and *nobody knew why*. Today we can explain that, as well as the transparency of the glass, the silvery shine of the frame, and almost anything else of practical significance.

Wigner and his students—for example Frederick Seitz, 20 years old in 1931 but only three years from his PhD—and many other physicists glimpsed the prospects for explanation that quantum mechanics opened up, although they could not see how far it would lead. The most obvious path led to the study of solids, or at least simple ones like crystals and superconducting metals. The path was not attractive to everyone. *Schmutzphysik* (dirt physics), Pauli called it, not just for its concern with crystal impurities but also for its connections with applications and hopes for monetary profit. Yet solid-state physics gradually won respect as it worked toward explanations of long-mysterious properties of matter.

One promising route lay through low-temperature experiments, which at that time had reached about 1 K. That was cold enough so that, for example, Willem Keesom and his colleagues could measure shifts between helium I and helium II as they sought an interpretation in terms of phase transitions. No less intriguing were advances in understanding solids at room temperature. Already in 1931 Hans Bethe, a student who turned 25 that year, found a solution of the Ising model for a one-dimensional lattice. That solution would be a key to the more elaborate model that eventually helped explain magnetism and other collective phenomena. It was also in 1931 that theorist Yakov Frenkel proposed excitons, a new sort of "particle" that could exist only as a result of interactions of other particles in an array. The notion of particle-like excitations was a clue to understanding many phenomena, including superconductivity. A still more imposing landmark of 1931 was Heisenberg's introduction of the concept of "holes" in a conductor's electron sea. The idea was soon applied in the comprehensive theory of metals and semiconductors that Alan Wilson was developing with Felix Bloch.

Today the largest field in physics, encompassing more

than a fifth of all the PhDs granted in the last decade and a still higher fraction of physicists' careers, is the study of "condensed matter." The term replaced "solid-state" in the 1960s following successes in the study of fluids. Since then, the rubric "materials science" has been added, pointing toward the proliferation of practical applications. In basic physics, superfluidity research—now reaching into the nanokelvin range—keeps producing surprises. Meanwhile, research on magnetic materials not only has been intellectually satisfying but has brought exponential progress in data storage for computing. Computers themselves, of course, also rely on the manipulation of holes in semiconductors. Computer calculations nowadays increasingly illuminate fluidic and chaotic phenomena and many other things far beyond the reach of calculation 75 years ago.

Everyone knows how applications of solid-state physics have changed industry and daily life. The students of 1931 could not have imagined the consequences, from iPods to "smart" missiles. But they had seen equally revolutionary changes coming from physics even in the short time since they were children, such as the proliferation of radios, airplanes, and electric motors. They understood that the results of their own research, whatever those results might be, should find fabulous applications with far-reaching social consequences.

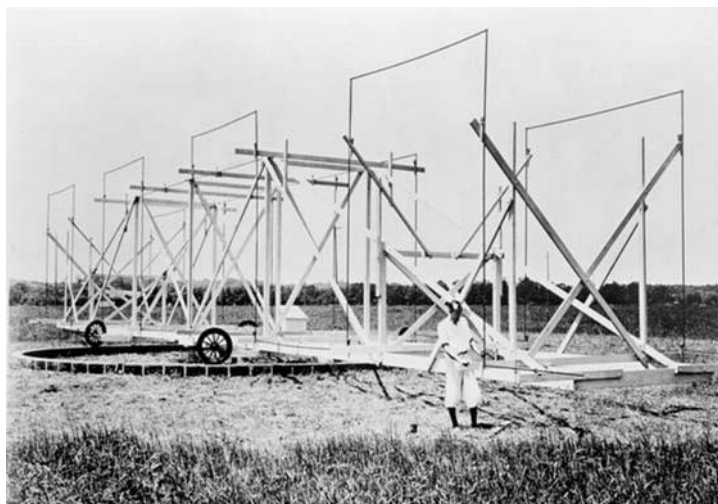
Hyphenated physics

Those two routes forward with the new quantum mechanics—inward into nuclei and outward into solids—were more obvious than some of the other roads that were in fact opening up in 1931. One example is the work of Ernst Ruska, another of the students who turned 25 that year. When he focused beams of electrons to create the first electron microscopes, few could have guessed their value for probing matter. Instrumentation such as positron emission tomography now finds applications not only in physics but in medicine and physiology—even in the investigation of mental states. Manipulating beams is also central to the new nanotechnology, which arranges individual atoms into devices that the most adventurous science-fiction writers of the 1930s never imagined.

To be sure, the best molecular-scale devices in 2006 are rudimentary compared to the intricate machines into which living cells have evolved over the past few billion years. But of all the ways forward that we can see today, I think the clearest is the one leading toward the full control of such machinery. When we get there, the old terms "medical physics" and "biophysics" will have vastly expanded meanings.

The growth of such hyphenated-physics disciplines, only dimly perceived in the 1930s, has been one of the most important trends of the past half-century. The rise of geophysics is particularly instructive. The large data sets necessary for most of the progress in that field were first made available in about 1957 by the global collaboration of the International Geophysical Year and by the advent of satellites. In 1931, for example, a large majority of geologists believed that the theory of continental drift was ruled out by physical limitations on the motions of rock. Today seismic tomography uses computers to study in depth the structure of moving continental plates.

The atmospheric sciences offer especially good examples of that sort of progress. My personal nominee for an outstanding publication of 1931—although it was entirely ignored by other scientists at the time—is a *Physical Review* paper on atmospheric spectroscopy by Edward Hulburt.¹ His innovative calculation supported a hypothesis published three decades earlier: Doubling the carbon dioxide in the atmosphere would bring a significant rise in surface temperature. In 1931, Hulburt was almost alone in finding the idea plausible. Not until the 1950s did others make solid meas-



Radio astronomy then and now. Left: Karl Jansky adjusting his radio-antenna array in Holmdel, New Jersey, around 1931. Right: The Very Large Array of 27 radio antennas near Socorro, New Mexico, each 25 meters in diameter, was completed in 1981. (© 2001 Fred Espenak, www.MrEclipse.com.)



measurements of the absorption of infrared radiation using improved detectors. And theorists used those measurements in computer calculations that were hard to dismiss. Only in the present decade have we gotten data and computer models good enough to convince virtually all experts that greenhouse warming poses a grave risk to the well-being of our civilization.

Astrophysics is yet another hyphenated field in which we can see some key developments of the 1930s only in hindsight. In 1931, for example, Bernhard Schmidt invented an optical system for a telescope that could take high-resolution photographs of a wide area of the sky. Fritz Zwicky was one of the few who saw an opportunity. He turned a Schmidt telescope to a large cluster of galaxies and concluded from their relative motions, as measured by redshift, that the cluster contained a lot of unseen mass. That made little sense to people at the time. Today, the wide-field Sloan Digital Sky Survey is producing huge quantities of data that not only make a convincing case for Zwicky's "dark matter"; they also give evidence of "dark energy" that looks like a manifestation of the cosmological constant that Einstein abandoned too hastily after Hubble's discovery.

It was also in 1931 that the 26-year-old engineer Karl Jansky began using a crude antenna to study radio telephone interference for Bell Labs. He would later demonstrate that some of the interference he found was radio emission from the Milky Way galaxy. Once Jansky's bosses understood that the galactic emission would not interfere with communications, they assigned him other work. Today astronomers use radio and other waves outside the visible spectrum to probe objects ranging from neutron stars to the cosmic microwave background, a remnant of the Big Bang. These are realms that scientists of the 1930s lacked the observational data to picture in their minds, let alone study.

Another candidate for the most overlooked paper in that period would be one published way back in 1917 but largely forgotten in the 1930s despite its author's reputation: Einstein's prediction of stimulated emission of photons (see the article by Daniel Kleppner in *PHYSICS TODAY*, February 2005, page 30). Today lasers are used everywhere. Why did no one notice that path forward? Because it was too great a conceptual and technical leap from Einstein's curious idea to a working device. The laser was actually approached stepwise through the maser, which could not be built until the development of microwave techniques for radar in the 1940s. So

it's not so much an overlooked path as a technology that would appear only after the rise of other technologies.

I believe we have come here upon a general rule. If physicists in 1931 failed to see some potential paths, it wasn't for lack of creative imagination. What they lacked was data and the instruments and collaborative networks to get the data, and in some cases the computers needed to analyze them. Like many other things important to science today—the mid-ocean rifts, Bose-Einstein condensates, the cosmic microwave background radiation, and so forth—the laser was something that people in 1931 simply had no way of foreseeing.

A community transformed

Physics is not just an intellectual exercise, but also a community of people and their institutions. The first step we should look at in the physicist's career is education. The students of 1931, transported to a physics department of comparable size today, would find many familiar things in the setup of textbooks, courses, examinations, seminars, and thesis mentoring. For better or worse, graduate education in the 21st century retains most of the structures that originated in 19th-century Germany.

Beyond their education, the students of 1931, like those of today, were thinking about jobs. Prospects looked poor as the Great Depression deepened. Most physics students would have expected to get a job in academia, some would have considered jobs in industry, and a few would have sought work in a government institution. But in 1931 none of those prospective employers were hiring anyone. Fortunately for the students, within a few years universities began to expand again, followed by industrial and government labs. The Depression and World War II only temporarily interrupted the exponential rise in the number of physicists, which doubled about every dozen years.²

This growth by orders of magnitude produced a qualitative change. Many physicists in the 1930s, reading everything important in the journals, would shift after a few years from one subfield to another. By the 1960s, it was rare for any-

one to publish in more than one specialty. Meanwhile physicists found they no longer knew personally all the main figures in the field. From a community as close-knit as a village, the physics profession became a sprawling confederation of interlocking subfields, each with its own institutions and customs, with no clear leaders of the whole.

The rise of hyphenated physics compounded the situation by separating the physicists who went into astro- or geo- or biophysics even farther from their colleagues. Yet far more than in the 1930s, physicists today have found that their training can help them in very different undertakings, from Wall Street to sculpture. Angela Merkel, Germany's chancellor, started her career as a physicist. So did Askar Akayev, who returned to the community after he was deposed as leader of Kyrgyzstan.

Of course the exponential rise in numbers could not continue indefinitely. In the late 1970s, the number of physicists in the developed nations reached the limit that society was willing to support. Since then, their numbers have oscillated around that limit, which has been rising gradually with the growth of the economy. (Faster growth continues in some developing countries.)

Because fewer students means fewer professors, the end of exponential growth left us with a different career distribution. The gap has been partly filled by government and government-contract laboratories, which were of little importance in 1931, when they employed only 5% of all American Physical Society members, mainly in the National Bureau of Standards (now NIST). Today they employ about a quarter of APS members.

A related change is the strong connection that now exists between physics and the military establishment. That connection has contributed to a mistrust of physics among the public at large. Such mistrust is not new. In 1931, some linked science to the novel horrors of World War I, while others linked it to technologies that brought economic dislocation and ideas that challenged religious faith. Indeed the founders of the American Institute of Physics, while they acted primarily to deal with the Depression's disruption of journal publishing, also asked the new institution to work on improving the public image of physics. Today such work is needed as much as ever.

Public mistrust of physics, now as then, is mostly outweighed by an awareness of its benefits. A student of 1931, transported to a modern laboratory, would probably stare most of all at computers. The computerization of many aspects of the developed economies has been as astounding as electrification was in the decades preceding 1931. The 1920s had seen a corresponding increase in jobs for physicists in industry, and the years since the 1980s have seen an even greater rise. Whereas in 1931 about 12% of APS members worked in industry, today 24% do. For all AIP member societies taken together, the fraction is 27%.^{2,3}

The next 75 years

Can we learn anything about the future by comparing the situation in 1931 with the situation today? Most striking is how different the two are. Some of the trends that have brought that change can be extrapolated forward. In the next 75 years, we can hardly fail to continue to expand the uses of physics and the associated job opportunities. We can also expect continuing improvements in collaborative organization, aided by advances in communication. And we can look for a continuing increase in diversity as women and currently underrepresented minorities join the enterprise in larger numbers. But scientists can't expect the transformative orders-of-magnitude growth seen in the past 75 years. We cannot, after all, account for more than 100% of gross world product! Growth will probably track the changes of national

economies. And what will those be?

Historians will tell you that the one thing you can learn from history is that it's unpredictable. You can't project every linear trend forward. Societies are prone to spurts of exponential growth and, more often than we like to think, collapse. We do have reason to worry about that. Plausible calculations show that humans have already exceeded the planet's carrying capacity, and we are living off resources that cannot be quickly renewed. In 75 years, for example, there is little chance that we will be consuming as much petroleum as we are now, or even as much water. And it is likely that climate changes will be causing grave harm.

It is scarcely possible that the world in 2081 will have 10 billion people each consuming the way the average American—or even the average Russian—does now. Global per capita consumption of physical and biological resources is almost certain to remain well below the current developed-world level. Does that mean that our economies and standards of living must crash? Not necessarily. Against the declining curve of resources, we can match an exponentially rising curve of capabilities. Moore's law of doubling every couple of years is valid not only for processing speed but also for memory capacity, the synthesis of DNA molecules, and more. It predicts a rise so fast that some people think that within a few decades we will have soared to an "omega point" of effective intelligence beyond which our present limited vision cannot see. We are in a race to improve our capabilities faster than we degrade our resources. For winning such a race, nothing is more essential than physics.

Physics is not now at the culmination of a surge of fundamental discovery comparable to the development of quantum mechanics, which students of 1931 rightly foresaw could lead to amazing advances in many areas. Nevertheless, now as then, there are at least two obvious paths forward. On the one hand, we are challenged by deep unknowns in the fundamental nature of matter. On the other hand, we can go much farther in straightforward understanding and manipulation of the immediate material world. On the first path, we can hope for strange insights into both fundamental particles and cosmology, with unforeseeable uses. On the other, we can hardly fail to find more wonders in the physics of condensed matter, and beyond in the realms of nanophysics and biophysics.

What about advances that we can't predict? In the past we have seen many unanticipated discoveries. And most of them—from lasers to dark matter, from medical physics to climate change—depended on new instrumentation (including computers) and extensive observational programs. Today's student should pay special attention to new developments in instrumentation and collaborative organization. We could try to predict what new instruments and programs may come along in the next couple of decades. Beyond that we can only be confident that they will keep coming, each building on what came before. There is every reason to look forward to as many surprising discoveries and extraordinary applications in the next 75 years as we have seen in the past 75.

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3. For recent statistics, see R. Y. Chu, M. Henly, *Society Membership Profile: 2002 Report, US Residents Only*, available at <http://www.aip.org/statistics/trends/reports/profile02.pdf>. ■