# Physics in 50 Years

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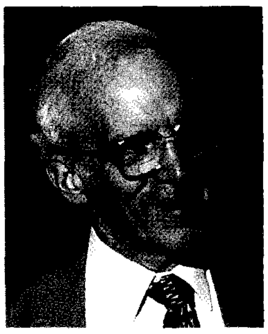

*PHYSICS TODAY invited me to talk about the future of physics at its fiftieth-anniversary celebration. Overcome by the desire to see old friends and the promise of good food and drink, I agreed.*

Let me start by expressing my pleasure in participating in PHYSICS TODAY's fiftieth-anniversary celebration, and expressing my deep appreciation to Charles Harris, Steve Benka and Gloria Lubkin for providing me with this evidently irresistible opportunity to humiliate myself publicly by attempting to say something intelligent about physics in the next fifty years.

As everyone knows, long-term predictions in science are hopeless and even short-term predictions are usually wrong. Fortunately, they are usually wrong the right way, for in physics-unlike the common situation in human affairs-reality frequently exceeds expectations. I documented this phenomenon some years ago in a PHYSICS TODAY "Reference Frame" column ("A Lesson in Humility," December 1991, page 9), showing that if one compares the best forecasts made by a group of responsible scientists with what actually happens, the forecasts are pale compared to the reality. The particular group of responsible scientists was the Physics Survey Committee, headed by William Brinkman, which prepared a report in 1986. Looking back on our omissions five years later, I found the following unpredictable discoveries and overlooked advances: Supernova 1987A, high-temperature superconductivity, atom cooling and laser manipulation, buckyballs, complexity, chaos and nonlinear dynamics, superdeformed nuclei, large-scale structure of the universe and mesoscopic physics.

In my own field of atomic, molecular and optical physics, there was such rapid progress after the Brinkman report that a new committee set out to prepare an up-to-date survey. The result, *Atomic, Molecular and Optical Science* (National Academy Press, 1994), was about as up to date as possible. Nevertheless, it gave little inkling that the most exciting advance in atomic physics for decades was about to take place-Bose-Einstein condensation in a gas. Another missed topic was quantum computation, which was a hot topic within a couple of years. Such omissions are not due to lack of imagination or shortsightedness. If any blame is to be assigned, it must be assigned to Nature for being too generous.

Progress in technology ought to be easier to forecast than discoveries in basic science, but even here the predictions are likely to be askew. One of my favorite childhood books was a 1912 edition of the *Book of Knowledge*. There was a splendid article on the latest technical wonder, the airplane, with a full page devoted to illustrations of the airplanes of the future. They were not mere biplanes. They were triplanes, quadraplanes, and airplanes with up to a dozen wings. And at the 1939 World's Fair, the General Motors Futurama displayed a gorgeous model of teardrop-shaped cars whizzing through pristine cities on highways with fantastically complex intersections and overpasses. The Futurama actually provided a pretty good picture of today's highways, but thanks in large part to automobile emission, the cities are hardly pristine, and the cars, of course, are not whizzing at high speed-much of the time they are crawling bumper to bumper.

In spite of the obvious pitfalls of prediction, there is a long and honorable tradition of physicists mis-forecasting scientific progress. Toward the end of the 19th century, physics was so impressive that some respected physicists thought the job was pretty well finished. Oliver Lodge stated that "The whole subject of electrical radiation seems working itself out splendidly"—just a few years before the ultraviolet catastrophe struck-and A. A. Michelson claimed that the "major laws of physics are pretty well known." That was in 1894 at the University of Chicago, at the dedication of the Ryerson Laboratory. Exactly one decade later, Einstein published his papers on the electrodynamics of moving bodies, the photoelectric effect, Brownian motion and the quantum theory of solids.

However, to be candid, I should point out that if you are *really* smart, you may be able to say something intelligent. Lord Kelvin, for instance, was really smart. In 1900, he presented a lecture at the Royal Institution entitled "Nineteenth Century Clouds over the Dynamical Theory of Heat and Light." He spotted two clouds. Cloud one was the problem of the ether-there seemed to be no way to account for the effects of motion through it. Cloud two was the problem of specific heats: He emphasized that the equipartition theorem gave incorrect values for specific heats of molecules unless one arbitrarily excluded certain motions. He characterized both of these clouds as being pretty dark, and of course he was right. Nevertheless, even Kelvin had no way to foretell the revolution about to take place.

Since it is essentially impossible to predict scientific discoveries, it is tempting to go in the opposite direction and predict things that will not happen. However, this is also most unwise, since it practically guarantees that they will happen. Perhaps you have your own pet list of failed predictions. On my list are Rutherford's claim that anyone who thought nuclear energy would be useful was talking moonshine, and the prediction made to Charles Townes that the maser would never work-this by some respected physicists at Columbia. I recall a talk at an American Physical Society meeting by President Reagan's science adviser, George Keyworth, shortly after the President announced the Strategic Defense Initiative whose technical goal was an impenetrable missile defense. There had been much opposition from the scientific community, and to counter that Keyworth produced a long list of things that experts said could not work but eventually did work, from airplanes to tele-

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vision. The argument appeared to be that because so many experts said SDI was not technically possible, we should be assured that it was technically possible. Unfortunately, we can't be absolutely sure that universal disapproval by experts guarantees success.

So, I will refrain from predicting what will not occur in the future. Further, I won't even hazard a guess about which fields will decline, for my own field, atomic physics, appears to have actually died several times in this century. Fermi instantly abandoned it when he learned about the neutron. Norman Ramsey told me that when he approached I. I. Rabi in 1937 about doing graduate work, Rabi declared that the field of molecular beams was pretty well washed up. That was a few months before Rabi invented magnetic resonance. In the 1960s, when it seemed that spectroscopy was getting pretty routine, laser spectroscopy transformed atomic physics. And just a few years ago, there was no inkling of the tremendous excitement that laser cooling and trapping were about to generate, though last year's Nobel Prize in Physics leaves no doubt as to the scientific interest.

For these reasons, I will not predict that high-energy experimental physics will dwindle because of the time scale and cost of large machines, and the need to work in corporate-size research groups. It would not surprise me if in fifty years there are working groups devoted to the VNL-NLC, the "Very Next to Last Next Linear Collider." More likely, particle research will be done with some totally different approach. In any case, from having read graduate applications at MIT for a few years, I know that there are bright students interested in experimental high-energy physics. As long as bright students are attracted to a field, it will do okay

Having argued that one cannot predict what will happen and what will not happen in physics, I appear to have totally welshed on my assigned subject. However, I believe that there is actually something to say. It follows from the simple observation that physics is built upon experiment. Whenever there is a major advance in experimental technique, new physics comes tumbling out. The recent history of astronomy and astrophysics demonstrates this pretty convincingly.

Within the last fifty years, cosmology has been transformed from metaphysics to hard science, and our vision of astronomical and astrophysical processes has been incredibly expanded. The creation of radio telescopes led to the discovery of quasars, pulsars, gravitational lenses and black holes. The 2.7 K cosmic background was discovered, and fluctuations that illuminate the earliest stages of the universe were detected. Optical telescopes, enhanced a hundredfold by the invention of the CCD camera, revealed large-scale structures in the universe and, more recently, evidence for the acceleration in the expansion of the universe. And with the creation of x-ray telescopes, the mysterious x-ray bursters were discovered. The list is too large to summarize, and it is growing too rapidly. Thanks to these new tools for seeing the cosmos, we now live in a golden age of astrophysics.

This golden age is not merely for astrophysics, for all of science is linked. Rydberg atoms, for example, were first observed by radio astronomers. My interest in them grew directly out of that discovery, and they have been my bread and butter ever since.

So there are a few examples from astrophysics of what can happen when you have new techniques for seeing things. There is no reason to believe that experimental advances in astronomy are about to cease. Gravitational wave astronomy, for instance, seems a good bet to be realized long before PHYSICS TODAY'S centenary.

Because physics is driven by experiment, and new experimental techniques are continually created, there is every reason to be optimistic about our scientific future. This simple thought gives me the courage to make a few predictions.

First, I predict that in fifty years there will be physics. By physics, I mean the science of understanding the physical world in quantitative detail. However, in some areas what is meant by "quantitative detail" today may not be the same fifty years from now. Prior to the creation of quantum mechanics, no thoughtful physicist would accept that a language of mere probabilities could provide a quantitative description. The very first intimation of probabilistic behavior-radioactive decay—was deeply troubling. Today, most physicists regard quantum descriptions as natural and intuitive, and they are not at all troubled by the language of probability.

Lots of people, myself included, expect that physics will have increasingly important things to say about biological processes, but the language of that physics may turn out to be different from the language we know. And if physics comes to deal with large complex systems (by which I mean something neurological, possibly the brain of a fruit fly or something like that), it may well use a language that we could not accept today as being quantitative, just as a turn-of-the-century physicist could not honestly accept a quantum description as being quantitative.

Second, I predict that new experimental techniques will continue to flower, and that whenever we acquire some new tool for looking at the natural world, we will see marvelous things. That thought is expressed most beautifully by a Jesuit paleontologist and philosopher, Pierre Teilhard de Chardin, in his book *The Phenomenon ofMan* (1955). Teilhard was concerned with the development of living forms, the evolution of systems of higher consciousness and ultimately human development. He was certainly not concerned with progress in physics. Nevertheless, what he wrote fits physics exactly: "The history of the living world can be summarized as an elaboration of ever more perfect eyes within a cosmos in which there is always something more to be seen." In physics, the creation of new and more powerful experimental methods is indeed an elaboration of ever more perfect eyes. I predict that we will continue to create new eyes, and that whenever we do, we will not merely see more things, we will make breathtaking discoveries.

And my final prediction is this: If fifty years from now you should happen to look at the centenary issue of PHYSICS TODAY, you *will be amazed!*

*I thank Susan N. Coppersmith and Anthony P. French for historical references.* ■

### *Thinking* About the Future

Notwithstanding the perils and pitfalls of predicting the future of physics, one cannot intelligently plan for the future by merely extrapolating from the past. A celebratory parry is not the forum for addressing this issue seriously, but one might try bringing together a small group of leading physicists with diverse interests to share their views. Such an exercise was carried out by the Princeton University physics de partment in November 1996, in its celebration of Princeton's 250th anniversary. There were great talks on nonequilibrium physics, computation and neurobiology, complexity, high-temperature superconductivity, medical imaging, cosmology, experimental and theoretical gravitation, neutrino oscillations, high-energy colliders, string theory and particle theory. In an introductory talk, Sam Treiman de scribed a similar conference held fifty years earlier. He too stressed that the fruits of science are more abundant than anyone can predict.

The proceedings of the 1996 Princeton conference make excellent reading: *Critical Problems in Physics,* V. L. Fitch, D. R. Marlow, M. A. E. Dementi, eds., Princeton U. I'., Princeton, N.J. (1997).