Physics in 1981±50

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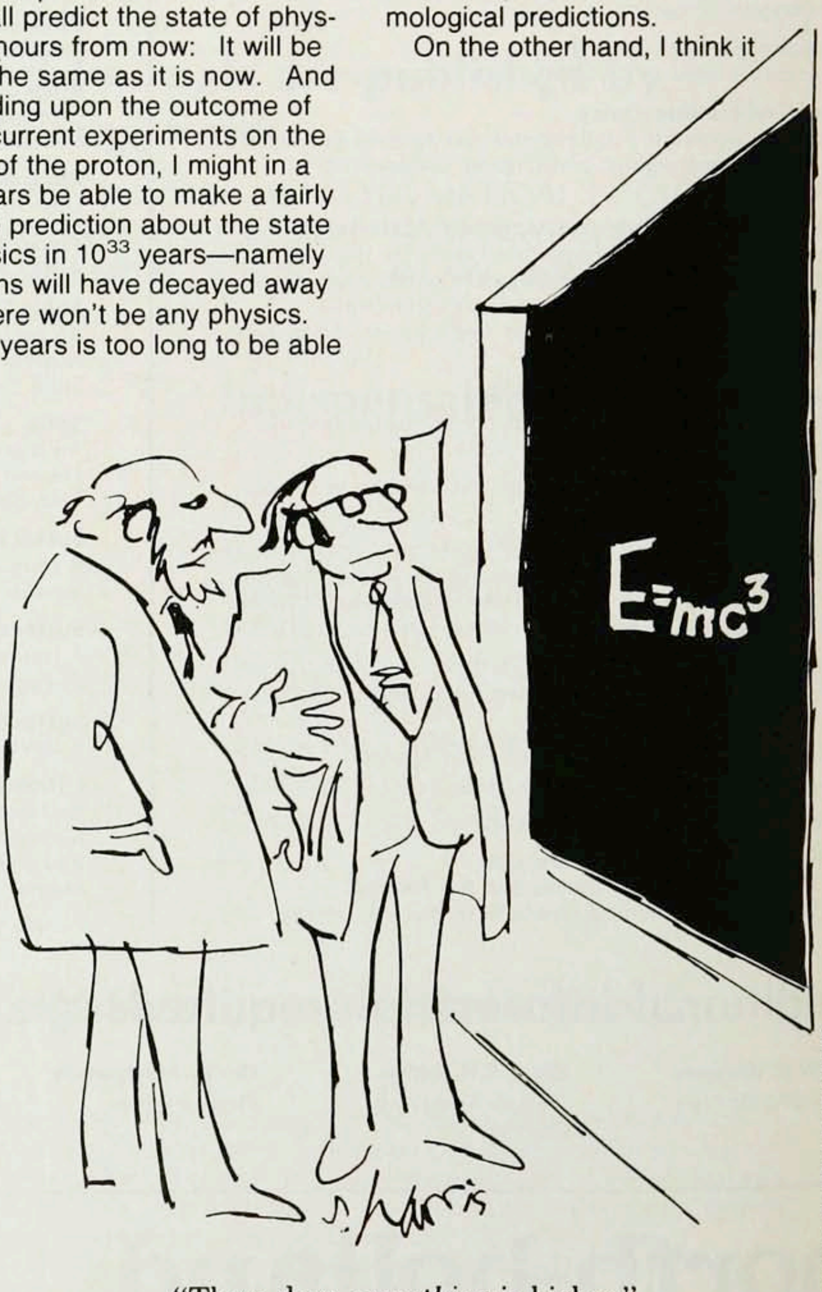
50 years is just the wrong interval of time to predict the future. We could all predict the state of physics 24 hours from now: It will be about the same as it is now. And depending upon the outcome of some current experiments on the decay of the proton, I might in a few years be able to make a fairly reliable prediction about the state of physics in 10^{33} years—namely nucleons will have decayed away and there won't be any physics. But 50 years is too long to be able to make detailed predictions and, of course, too short to make cosmological predictions.

On the other hand, I think it



"These days *everything* is higher."

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does pay to think explicitly about what things we can see coming up in the future, because a lot of our actions are necessarily based on what we think the future will bring. Although I have no confidence whatsoever in my ability to predict the future, I have no greater confidence in anyone else's ability to make such predictions. At least I want to present the following with a clear warning: Don't believe anything that I have to say about the future. To make this warning more explicit, I propose to take the title of the article (Physics in 1981 $\pm$ 50) quite seriously. I am not only going to look at physics in 1931 and 1981 and make predictions about 2031, but I will also, on the basis of where we were in 1931, make predictions about 1981, and a comparison of those predictions with the actual situation will demonstrate the unreliability of 50-year forecasts.

General conditions

Physics does not exist in isolation; it exists in the context of the general life and general conditions of the time. So let us first consider the general state of affairs in certain key areas in 1931, 1981, and some anticipations of how they will look in 2031.

One key area is the economy. The dominant theme in 1931, as we remember all too well, was depression and deflation, and any reasonable person, I think, would have predicted (see table 1) chronic depression and probably continued deflation for 1981. In-

TABLE 1 GENERAL CONDITIONS FOR PHYSICS

	1931	PREDICTED 1981	ACTUAL 1981	PREDICTED 2031
ECONOMY	depression, deflation	chronic depression	double-digit inflation	chronic inflation
COST OF DINNER	\$0.50	\$1.00	\$20	\$50
INTERNATIONAL RELATIONS	war inconceivable	continuing peace	WWII, etc., nuclear weapons	uneasy peace
FEDERAL ANNUAL EXPENDITURE	\$5 billion	\$10-100 billion	\$663 billion	\$2000-20,000 billion
FEDERAL SUPPORT BASIC RESEARCH	\$0 billion	\$0 billion	\$1.5-5 billion	\$15-150 billion
BEGINNING PHYSICS JOBS	0	few	few university, many others	few university, many others
BEGINNING PhD SALARY	\$1500	\$3000	\$18,000	\$170,000
MEMBERS APS	2,570	5,000	31,500	60,000
GEOGRAPHICAL CENTER	Europe	Europe	US	Europe, US, Asia and Middle East

stead we now have double-digit inflation; clearly the prediction would have been incorrect.

So, then, what should we predict about the economy in 2031? Well, you're automatically influenced by where you are at the time. So I have entered a prediction of chronic inflation in the table. But since chronic deflation wasn't right for 1981, maybe this prediction won't turn out right either.

The remaining entries in the table give concrete examples of what these trends in the economy mean. The cost of a good dinner that includes shrimp cocktail (that's always my definition of a really good dinner) was 50 cents in 1931, and with deflation one would have guessed it ought to be 40 cents in 1981 or at least no more than a dollar. When I first made up the table, I put the actual cost of the dinner in 1981 at $10, but after eating out in New York on two successive nights I revised it upwards to $20. Maybe it should be $30.

What will be the price of a dinner in 50 years? If we expect chronic inflation, the figure becomes rather appalling, say a factor of 40. Instead of putting down $800 for the cost of a dinner, I have settled for something rather less ($50).

The next area deals with international affairs. As a 16-year old freshman in college in 1931, it seemed at that time inconceivable that there would be another war. I had read books like H. G. Wells *The Shape of Things to Come* and it had been demonstrated that airplanes could be used to drop bombs. So in 1931 it seemed that if there were ever another war civilization would terminate almost abruptly.

TABLE 2 ENERGY

	1931	PREDICTED 1981	ACTUAL 1981	PREDICTED 2031
FOSSIL	abundant, no problems, no need for conservation or concern for enviornment, low cost	limited but large supplies, no problems, low cost	shortages, OPEC monopoly, enviornmental problems, high cost, need for conservation	coal, synthetic fuel, acid rain, greenhouse CO_2 effect, high cost
SOLAR	hydroelectric, Smithsonian hobby	unlimited hydroelectric, direct solar costly and unncessary	hydroelectric limited, active solar research, water heating	use dependent on cost and economics super-conducting transmission
NUCLEAR FISSION	artificial transformations, nuclear power absurd	none	fission 12% US electricity, problems of safety, siting, waste and acceptance	probable base load
FUSION	not considered	not considered	extensive research and development	probable commercialization, cost and economics

Therefore, the prediction in 1931 (but not in 1933) would have been continuing peace; that is one prediction I would have had complete confidence in. Instead, by 1981 we've had World War II and a number of smaller wars, and nuclear weapons have been developed. Clearly, that prediction would have been totally incorrect.

Now, what about a prediction for 2031? Here I predict a continuation of an uneasy peace. There are two reasons for this prediction. First, I'm basically an optimist. And second, if I'm wrong, what's the point of any of the predictions? Any major war that were to occur would be so drastic that we could forget about all the other predictions. This whole exercise has to based on the implicit assumption that there will be a continuing peace. I can't imagine it being a totally relaxed peace, so I list it as an uneasy peace in the table.

Now we turn to government expenditures. The annual federal expenditure in 1931—the total for all of the federal government—was five billion dollars. Therefore you could have expected with deflation maybe four billion in 1981. But I guess a reasonable prediction might have been ten billion for 1981. You can also justify a higher figure of 100 billion by noting that the five billion figure of 1931 had multiplied by a factor of 20 in the previous 50 years; the total federal budget in 1881 was one quarter of a billion. We see that, for fiscal year 1981, the actual federal budget is 663 billion. If you use the multiplication factor from 1931–1981 to predict for 2031 you end up with 20 000 billion dollars. With the hope that something might happen to curb inflation, I have settled for a range of 2–20 trillion dollars.

Now we come to the federal budget for basic research. In 1931, in units of billions, this figure was almost zero. There was a tiny bit of research at the Bureau of Standards, and there were a few other projects, but these were mostly applied research. There was very little if any federal support for basic research. The general belief at that time was that the federal government would never be involved in basic research. The state governments offered some support, but the idea of the federal government contributing to the support of basic research had not yet been invented. So the prediction for 1981 would have been zero billion dollars as well. I have two figures for the actual budget in 1981. The amount depends on what you call basic research and to what extent you include funds for facilities, instruments, and so on. In any event it's a fairly large sum ($1.5–5 billion). What will it be in the future? The inflation factor is hard to allow for. My guess is that it will continue at the same percentage of the total budget, which would give $15–150 billion.

What about the job situation in physics? For beginning jobs in physics—that is, for the PhD—the situation for 1931 was deplorable. There were no such jobs, or almost none. So the prediction for 1981, given this background (when I was deciding to be a physicist), was that there wouldn't be very many jobs then either. (Looking back I'm not quite sure why under such circumstances I made the decision to go into physics—but there weren't any jobs elsewhere either, so that made it easy.)

You may think that I am exaggerating the employment problems physicists had 50 years ago. After all, everybody remembers nostagically how difficult things were at the time of his youth, and usually exaggerates it. I found a quote that nicely documents what I remembered. In a letter that Linus Pauling wrote to Same Goudsmitt in May, 1933, he remarks "I haven't the faintest idea as to where your student can get a job. Caltech is filled with former National Research fellows hoping for a small stipend. We had only a 10 percent cut in salary a year ago, but we may well have another one."

The present situation is, I think, the following: There are not very many jobs now coming up in the universities—particularly tenured positions—and the prospects for tenured university jobs for the next decade look dismal. There will probably be some improvement, but not at the rate we would like. On the other hand, at the present time (rather happily) there are many other kinds of jobs available—interesting jobs, some involving basic research—in industry, government and national laboratories. My prediction is that things will remain about the same for the next 50 years. Barring a population explosion, there probably will still not be as many tenured university jobs as physicists would like. And at the same time there will still be great need for physicists to tackle many of the serious problems that we have to contend with.

Now let us review the pay physicists get. In 1931 the beginning PhD's salary was around $1500 per year; with deflation you would expect a drop even below this for 1981. But, again being optimistic, I would have guessed instead that this figure would increase in 50 years to $3000. I remember at that time I thought that if ever in my life I received a salary of four or five thousand dollars I would be doing very well.

Today a good many of the beginning jobs actually start at $18 000 and frequently much higher. Can we say what the starting salary will be in 2031? Trying to outguess inflation I have listed a figure that is almost a factor of ten increase ($170 000).

Now we come to another category: How many physicists were there and how many will there be? In 1931 there were 2 570 members of the American Physical Society. By 1981, I would have guessed it would have doubled. Instead of that it's gone up to 31 500. And in another 50 years? I don't think it will continue at this rate—that would come to 400 000. Maybe 60 000 would be a reasonable prediction.

Another area considered in the table is where is physics being done? In 1931 it was quite clear that *the* center of physics was Europe. In fact, I was one of the first people I know who took physics seriously and who actually received a PhD in this country rather than getting it abroad (and I'd had a scholarship to do undergraduate work at Cambridge University a couple years earlier). So at the time I would have predicted the center of physics would probably continue in Europe. It's quite clear now—in 1981—that the major center has shifted to the US. But this observation would have been even more valid in 1975 than it is in 1981. There seems to be a shift now in the opposite direction supported by a greater funding for much of basic research in Europe. My guess for 2031 is that the center will be shared among a number of locations—perhaps first Europe and then the US and Asia. Also there's a well known tendency for science to thrive (although not always) where there is lots of money. So we might anticipate in the next 50 years a major resurgence of science in the Middle East.

Energy problems

An area intermediate between the general state of affairs and the field of physics is the energy situation (see table 2). There was no energy problem in 1931; we used almost exclusively fossil fuels and they were abundant and cheap. No one thought there was a need for conservation; no one worried much about the environment; coal was used more extensively than petroleum. So I think that we would have predicted for 1981 that we would be beginning to realize fossil fuels were somewhat limited, but also that we would still have large supplies, no problems and low cost. Instead what we have are intermittent severe shortages. We have an OPEC monopoly. We have environmental problems, high costs, and a clear need for conservation.

What will we have in 2031? I think fossil fuels will still be with us, possibly shifting more to coal. There will probably be synthetic fuels derived from coal, a lot of problems with acid rain and the CO_2 greenhouse effect, and, clearly, problems of high costs.

Not much thinking had been done about other possible sources of energy in 1031 since there wasn't much need or problem. Hydroelectric power was certainly one of *the* promising things in '31; builders were making big dams and with the estimated needs for power it looked as if these plants would cover our needs quite well. The director of the Smithsonian Museum, had a solar energy hobby, which most people thought was a bit ridiculous; that was the solar energy research at the time.

I think the prediction for 1981 would have been that there would be unlimited amounts of fossil and hydroelectric power still available in 1981, and that direct solar power would be both costly and unnecessary.

What is the actual situation? Clearly, hydroelectric power now can supply only a small amount of our demand. There is active solar research going on and in many parts of the country solar energy now provides heat for hot water and for buildings.

In 2031 I think there will be significant uses of solar energy in some places, but how much and how widespread will depend upon cost and economics. An impor-

TABLE 3 PHYSICS

	1931	PREDICTED 1981	ACTUAL 1981	PREDICTED 2031
SCIENCE OF PHYSICS	thriving	tapering off	thriving	thriving
CONDENSED MATTER	mechanical properties, single crystals, super-conductors, little theory	more of the same	extensive applications, transistors, integrated circuits, super-conductors, Josephson junctions, theoretical advances	more of the same
PARTICLES	electron, proton, photon	same	leptons, quarks, gluons, vector bosons, questions of proton stability and neutrino oscillations	?
THEORY OF FORCES	electro-magnetism, relativity, quantum mechanics	unified gravity and electro-magnetism	QED renormalization, quage theories, electroweak, QCD, grand unification, technicolor, supergravity, instability of matter	fully unified theory

tant factor will be our success in developing better and less costly transmission lines. (This is important for the nuclear power as well.) I'm optimistic that eventually the power companies will get around to using superconducting transmission lines, even if they require low temperatures. I feel that power companies have been needlessly timid about becoming involved in cryogenics and have been excessively optimistic that room-temperature superconductors would soon exist.

Now let us consider nuclear fission as an energy source. Of course nuclear fission didn't exist in 1931 and wasn't even dreamed of. Although there were artificial nuclear transformations, the thought of getting power from them was generally considered absurb. For instance, Lord Rutherford, the father of nuclear physics, said in 1933: "Anyone who expects a source of power from the transformation of these atoms is talking moonshine."

The actual situation in 1981 is that about 12 percent of US electrical energy comes from nuclear power. Nevertheless, there are a lot of serious problems and worries about safety, siting, waste disposal and public acceptance. But in spite of these problems my guess is that in 2031 most of the base-load power will come from nuclear fission. But the power companies will not want to depend solely upon any one kind of power, so other energy sources will be used for peaking capacity.

Nuclear fusion was not invented, not considered and not even imagined in 1931; there would have been no way to predict that it would exist in 1981. Now of course we actually have extensive research and development going forward to derive energy from nuclear fusion, and I'm optimistic that the technical problems will be solved. The commercialization of nuclear fusion however will depend on costs, economics and competition with other sources. Fusion also has safety and waste-disposal problems, although they should be less severe than those of nuclear fission.

ACCELERATORS	300 KeV	10 MeV	500 GeV fixed target 62 GeV center of mass	1,000,000 GeV center of mass
ASTROPHYSICS, COSMOLOGY	relativity, expanding universe, optical telescopes	more of same	big bang, 3K radiation, radio and X-ray telescopes, space research and space telescopes, tests of general relativity, quasars, pulsars, black holes and so on.	unexpected discoveries, better theories
ATOMIC AND MOLECULAR	spectroscopy, molecular beams, microwave absorption	same	same plus masers, lasers, NMR, EPR, trapped ions, and so on.	same plus
CLOCK STABILITY	10^{-6}	10^{-9}	10^{-15}	10^{-18}
BIOPHYSICS	effects of radiation	same	X-ray and electron microscopy, DNA, molecular biology, electricity and nerve cells, recombinant DNA, genetic engineering	better understanding of mechanism of the mind and so on.

Physics research

The science of physics as a whole was thriving in 1931 and perhaps thriving so well that one might have expected some tapering off by the year 1981. Instead, physics is still vigorously thriving in 1981. My prediction is that physics in 2031 will also be thriving (see table 3).

Condensed matter. Now we consider the various subfields of physics, starting with, say, condensed matter. In 1931 it wasn't called condensed matter, it wasn't even called solid state. People were just studying things like the mechanical properties of single crystals. There was little theory, and what theory there was we now know was not on a very firm basis.

There would have been no reason to predict anything but more of the same for 1981. But, as we know, there has been a tremendous development of the field, with such applications as transistors, integrated circuits, superconductors, and Josephson junctions, plus enormous theoretical advances in interpreting semiconductors and such phenomena as superconductivity.

My guess for 2031 is that the condensed-matter field will continue as a fruitful research field.

Particle physics. The field of fundamental particles was rather simple in 1931. There were three particles, or two, depending on how you want to count: the electron, the proton and the photon. The neutron had not yet been discovered, but there was a real need for it. It ought to have been discovered just by noting the theoretical difficulty to confine an electron in a nucleus, but nevertheless it hadn't yet been discovered. So the theory of particle and nuclear physics was quite simplistic. All one had to understand were the forces between the basic particles. For 1981 I think we would then have predicted a lot more knowledge about these two particles with the subject being largely exhausted. Higher energies then looked interesting but unattainable.

What actually happened? Well, we now have leptons, quarks, gluons, vector bosons and questions of the proton stability, neutrino mass, neutrino oscillations and so on. Clearly, with such a large change from the prediction all we can put for the future are question marks. You might on the one hand guess that we will have just the same particles but a better understanding. Or you could guess that we will discover sub-units of the quark that theorists are already speculating about.

Theoretical physics. In mechanics and the theory of the forces there were spectacular advances 50 years ago. Electromagnetism, relativity and quantum mechanics are three of the truly great revolutions in the thought of mankind. And they all occurred just before 1931. What would we have predicted for 1981? Well, at that time Einstein was spending most of his time trying to get a unified theory of gravity and electromagnetism. So we could have expected that a unified theory would have been achieved by now. People at that time didn't distinguish between weak and strong forces, and they rarely talked about nuclear forces, so I don't think anyone would have anticipated unification say of weak and electromagnetic forces.

Instead what has happened has been a phenomenal change in theory. We now have quantum electrodynamics, which is a remarkable theory capable of more accurate predictions than almost any other theory of fundamental physics. There is the idea of renormalization that enables us to control infinities in QED. There are the gauge theories of the electroweak force and a theory of the strong nuclear forces—quantum chromodynamics. Then, there are the more speculative theories with lovely names, such as "grand unification," "technicolor," and "supergravity;" and finally there is the question of the instability of matter.

So what will the theorists have accomplished 50 years from now? Let me make just one prediction. I am optimistic that we will have a fully unified theory, with all the forces fitting in together, even if not perfectly. We are more justified now in hoping that this will occur than at any time in the past.

Accelerators were just being built in 1931. Not much had been done with them, but they existed up to about 300 kilovolts energy. One might then have guessed that, for 1981, they would have gone up to about 10 MeV. In 1937 Albert Rose and Hans Bethe published a very important article, pointing out for the first time what the problems of stability in an accelerator were. The article, entitled "The Maximum Energy Obtainable from the Cyclotron," concluded that it would be impossible for a cyclotron to accelerate protons to more than 12 MeV [Phys. Rev. 52, 1954 (1937)]. So that a prediction of 10 million volts would not seem too conservative.

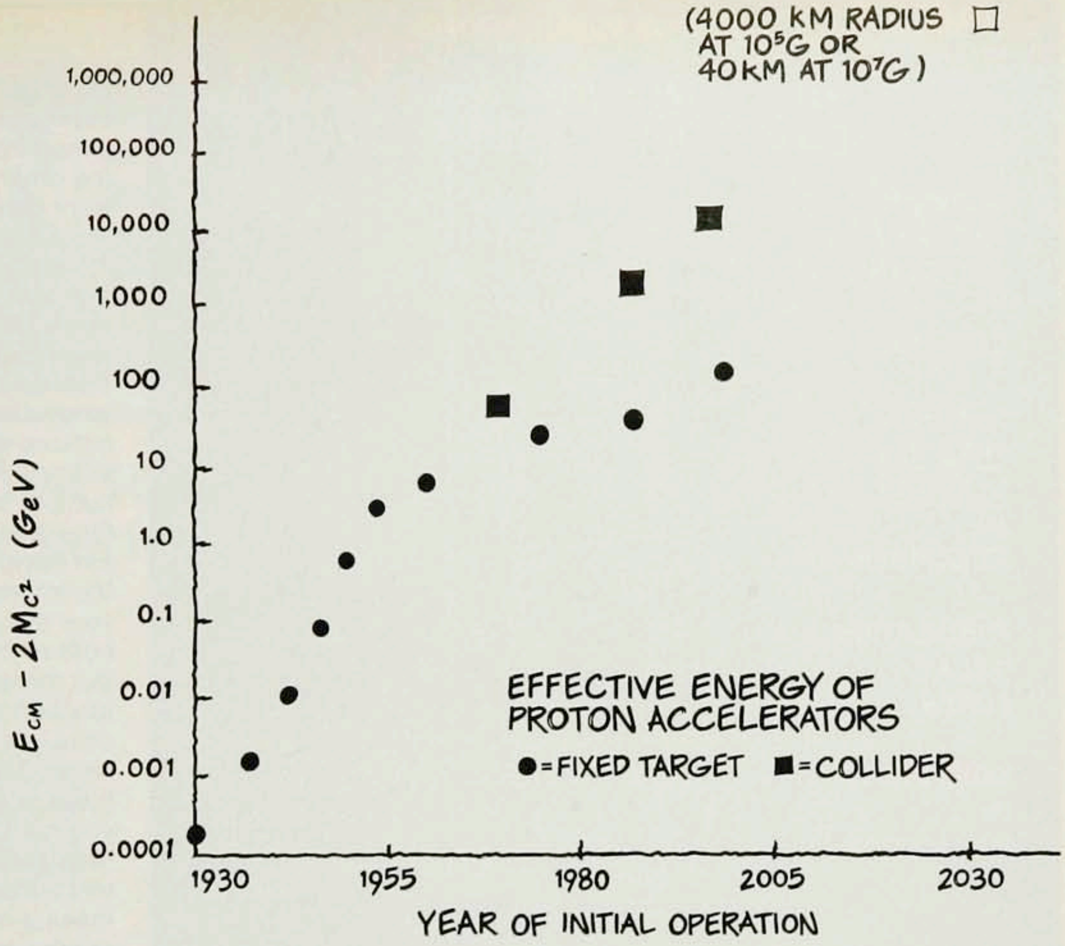
Instead in 1981 we now talk in terms of GeV's rather than MeV's, and there now exist 500 GeV fixed-target machines, 62-GeV in the center-of-mass machines, and coming up in the near future at Fermilab is the highest energy accelerator: the 1 000 GeV (or 1 TeV) Tevatron.

To predict the future of accelerators we can seek a slightly firmer basis. Stan Livingston used to plot a curve showing the increase in accelerator energy as a function of time. I've revised his curve (see figure on page 33) to plot the important energy, the energy in the center-of-mass system minus the rest energy of the particle, versus time. This is the most relevant thing to plot and we find it has continually increased with time since 1931.

If there had been no new ideas, this curve would have become flat. But there were lots of new ideas—the alternate gradient principle, separated function magnets, superconducting magnets and, colliding beams. In fact the figure shows that if one plots accelerators and disregards colliding-beam experiments, the curve does indeed flatten out. With colliding beams the curve continues to rise.

How will this curve behave over the next 50 years? We can imagine that it will continue to rise while flattening out until 2031 when it reaches 10 million GeV. But if you think about this figure a little more closely, it has some disconcerting implications. If you assume a maximum magnetic field of 10^5 gauss (which is the most that can be achieved with superconductivity at present), the radius of the accelerator becomes 4000 kilometers; it is hard to believe that this would be feasible.

On the other hand, if superconductors improve enough to give 10^7 gauss then the radius would be 40 kilometers; 10^8 gauss would bring it down still further to 4 kilometers. But this would require a



new idea or at least a much better superconductor. I end with a more conservative prediction—one million GeV in the center of mass instead of the 10 million that would come from the figure.

Cosmology had a major revolution in the 1931 period—relativity—and this led to the need for the notion of an expanding universe. The main astronomical tool at the time was the optical telescope. The prediction for 1981 would have been more of the same—a better cosmological model and bigger telescopes. In actuality much more has been accomplished in the 50 years since than anyone could have envisioned. First, we have developed a good picture—even data about how the universe started: the Big Bang or first explosion. The observation of the 3-K radiation enables us to look back and collect real evidence about that first explosion. The techniques of radio and x-ray telescopes, space research, and space telescopes have enabled us to make many crucial tests of general relativity in recent years. In addition, we have objects undreamed of before: quasars, pulsars, black holes, and so on. In 2031 I would guess that cosmology will still be an exciting field with many unexpected discoveries.

Atomic and molecular physics in 1931 consisted almost exclusively of spectroscopy, a little bit of work with molecular beams, and the very beginning of things like crude experimental microwave absorption—the beginning of microwave spectroscopy. We would probably have expected more of the same for 1981. Instead we have not only more of the same but also masers, lasers, nmr, electron and paramagnetic resonance, trapped ions and so on. My own hunch for 2031 is that all those techniques will continue to evolve but there will also develop some new techniques aimed at higher stability. For example, take a subfield with which I am familiar: clock stability. At present atomic clocks are the most stable clocks we have. In 1931 the best you had was either a tuning fork or a rather crude crystal control with a stability of around a part in a million, or 10^{-6}. At that time you might have guessed that with improvements, better crystals, and so on, we might have 10^{-9} by 1981. Instead, the atomic clocks now give us one part in 10^{15}, with some experiments achieving even slightly better results. So if you were an optimist, you would take the same rate of improvement and say we should have one part in 10^{24} in 2031. I doubt that this will happen. On the other hand, there are some promising techniques, such as laser cooling of trapped ions, that might increase stability by another factor of a thousand over the next 50 years.

Biophysics is the last area of physics to discuss. Very little was going on in 1931—chiefly the studies of effects of radiation on cells. I remember that around that time at the Carnegie Institution in Washington people were irradiating fruit flies and seeing what the effects were. There was no reason to expect anything but more of the same for 1981. Instead there has been a revolution; it's now hard to determine what's biology and what's physics in the field. Biophysics has certainly become dependent on both physics techniques and physicists who have worked in the field. Today we have x-ray and electron microscopy, DNA, molecular biology (a name not even used previously), the study of the role of electricity in nerve cells, and the possibility of recombinant DNA. The prediction I make for 2031 is continued growth in the field, probably leading to a better understanding of nerve cells and possibly even of the biggest biological mystery: how the brain can function as such an excellent computer and how the mind works.

TABLE 4 PHYSICS PUBLICATIONS

	1931	PREDICTED 1981	ACTUAL 1981	PREDICTED 2031
NUMBER PHYS REV ARTICLES IN DECEMBER	22	60	350	1000
NUMBER PHYS REV LETTERS IN DECEMBER	11	30	106	300
PUBLICATION DELAY (DAYS)	21	30	138	60
COMPUTERS IN PUBLICATIONS	none	none	communication, editing, composition, billing, subscription fullfillment, information storage and retrieval	new forms of publication, problems

Physics publications

The last area I want to review is physics publications. Here again we see marked changes (see table 4). I went to the library and randomly selected one month's contributions to *Physical Review* for 1931 and 1981. There were 22 papers in the *Physical Review* for December 1931. In 1931 you might have predicted an increase by a factor of three for 1981, say a total of 60 papers. Instead the actual number of papers in the *Physical Review* in December 1980 was 350, an increase by a factor of about 17. *Physical Review Letters* shows a similar increase: 11 in 1931 versus 106 in 1981.

What should we predict for 2031? Will the publication rate flatten out, diminish, increase? I have opted for a conservatively small increase.

What about delay in publication? In December 1931 the delay for *Letters* was 21 days. You might expect it to have gotten worse as time went on, say, up to 30 days. But the actual average delay for December 1980 was 138 days.

This could be a way to solve the publication problem. If you extrapolate by the same factor of 6, you get a 700-day delay in 2031.

My prediction, however, is that the delays will decrease. Publication is clearly harder to do now; there are so many articles and the refereeing problem is almost overwhelming. But I still expect publishing delay to decrease rather than increase.

The final thing in publishing is the role of computers. That was not at all an issue in 1931, and it wouldn't have been predicted for 1981. Computers clearly have had a major impact already, not in changing the form of the output of publication, but in the mechanism of doing the publishing and in the economics involved. Computerization has spread into communication, editing, composition, billing, subscription fulfillment and even information storage and retrieval. But what is the future going to bring? Perhaps another major change. At one extreme, publications in the present form could cease to exist by 2031. You might carry a little pocket computer on which you would punch in the article you want to see, and then somehow it would be transmitted from AIP headquarters. On the other hand, scientists may still want published journals to carry around with them and think about.

I really don't know what to predict in this area. I think there is a real problem that people don't worry enough about when they discuss information retrieval systems and replacing printed publications with computerized operations. Publications serve a number of different purposes. One is to find out what other people have done. Another is to get the information that you need for your own work. Computers serve both of these purposes very well. But there's another very important function served by journals: to stimulate people to invent new ideas. The stimulation of new ideas is an ill defined process: you read something here and you read something there, and then you suddenly realize you can make a maser. Or you look at various reports of data in particle physics and recognize a simpler interpretation in terms of quarks. This is a very important function served by journals, and so far my impression is that the automated systems often make this function more difficult. When you are using a computer console to dig up information, you're really thinking, not about new ideas about physics, but about what should be the right thing to type in to get the information out. There are three ways we might go from here. We could improve the input-output mechanism to make it more attractive and more stimulating to people, thereby making it more effective than journals for stimulating the development of new ideas. The opposite extreme is to conclude that we aren't going to solve this problem, so maybe we should design a computer program to invent the ideas for us. But if we fail in this also, we may well find that publications of approximately the present form are still needed even with spectacular advances in computerized retrieval of stored information.

I conclude these attempts to look ahead 50 years with the hope that no one will be seriously misled by my predictions. The only prediction to take seriously is that the future will be exciting; it is exciting largely because it is unpredictable. □