The future of physics

Many physicists may turn to molecular biophysics, pulsar astronomy and the problems of environmental pollution, just as some turned to radioastronomy and computer technology 25 years ago.

Freeman J. Dyson

What sort of physics should we be doing between now and the end of the century? This is a question I thought about recently when we at Princeton dedicated a new physics building; what would the people now in the building have a chance to do during their working lives?

Here are some of my answers to these questions. I will try to look at the next 30 years of physics, not altogether avoiding speculation but mainly concentrating on practical questions that already face us today. My comments necessarily have a personal and a Princeton flavor, but the principles should apply to anyone, anywhere.

Rather than trying to cover everything that goes on in a physics building, I will deal only with research and not at all with teaching. It goes without saying that the choice of fields of research ought to be strongly biassed in favor of projects that will attract students and allow students to participate actively. The bias is implicit in most of the choices I shall advocate. I have some other kinds of bias, which will become apparent as the talk proceeds.

I shall emphasize experimental work and say little about theory. This is not because I consider theory unimportant. An elegant theory, combining mathematical beauty with physical truth, is the ultimate objective of all our efforts in physics. But if theories are the end-product of science, experiments are the driving force.

A strange bunch at Cambridge

I will begin with an example from the past which shows that foresight over a time-scale of 30 years is sometimes possible and can be enormously fruitful. When I came as a graduate student to the English Cambridge 24 years ago most of my physicist friends were cursing the name of Sir Lawrence Bragg, the director of the Cavendish Laboratory. Bragg had become director in 1938, the year after the death of

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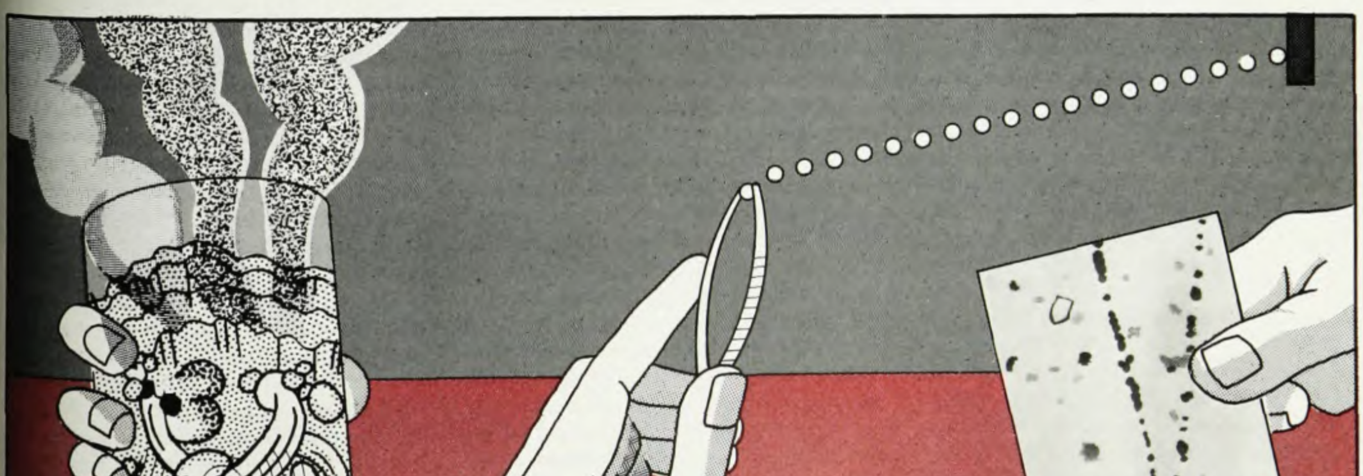

In the next thirty years, physicists may get involved with activated sludge, sequencing large nucleic acids, cosmic-ray work . . .

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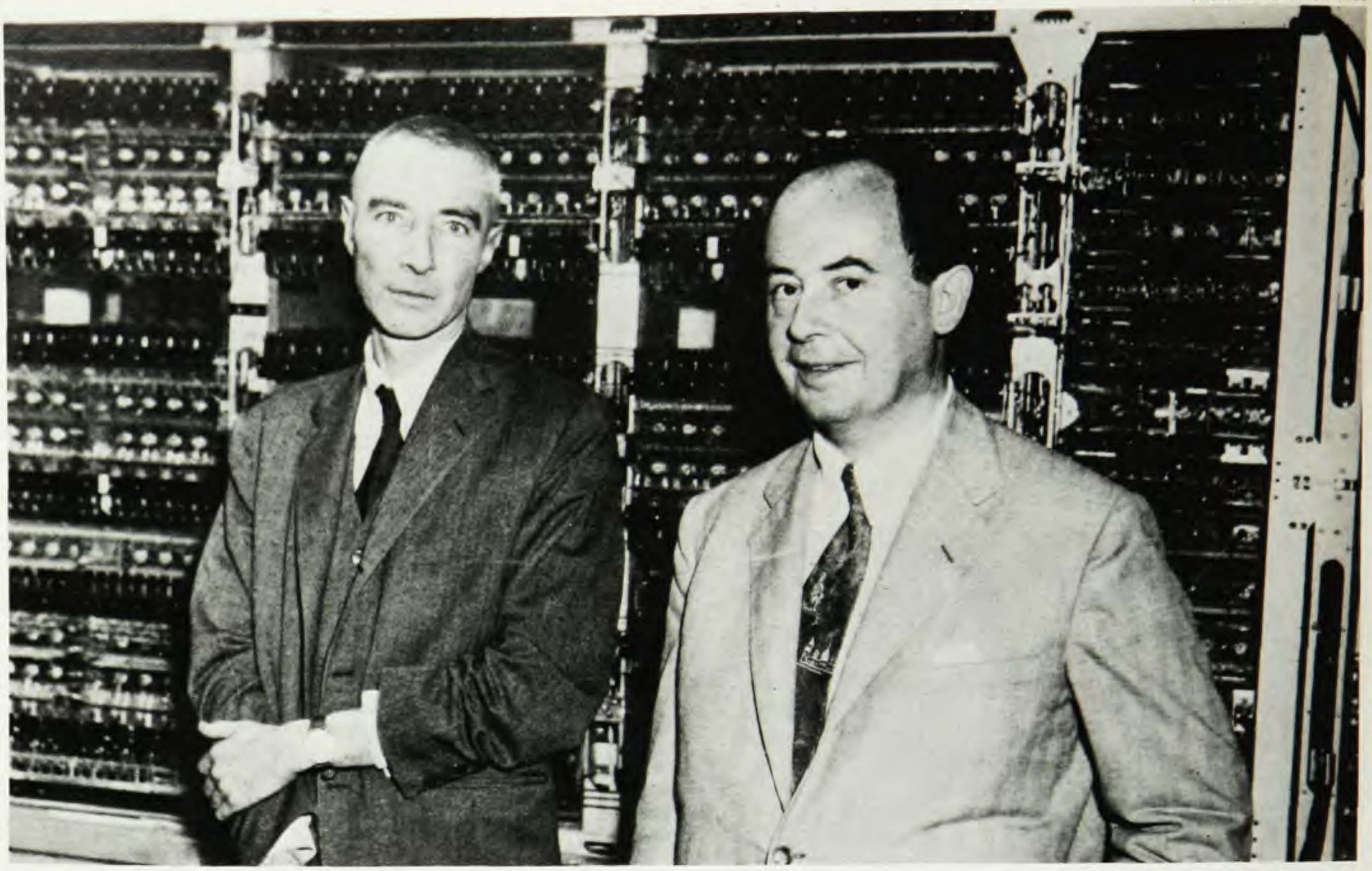


John von Neumann (right) standing with J. Robert Oppenheimer in front of the prototype electronic computer at Princeton. This photo was taken about 1950.

Ernest Rutherford. During the brief interregnum the Cavendish had disintegrated with extraordinary speed. Under Rutherford it had been the world center of high-energy physics, "high-energy" in those days meaning anything over a hundred kilovolts. When Bragg took over the wreckage, P. M. S. Blackett and James Chadwick and most of the brilliant younger men who had worked with Rutherford were gone. They had accepted chairs at other universities where they were busy establishing research schools of their own. The leadership in high-energy physics had decisively passed to Berkeley. To the consternation of those who remained in Cambridge, Bragg made no effort to rebuild. He was not seriously interested in plans for a new accelerator. He sat smugly in his office at the Cavendish and said: "We have taught the world very successfully how to do nuclear physics. Now let us teach them how to do something else."

The people Bragg was interested in supporting were a strange bunch, doing things that the high-energy crowd would hardly recognize as physics. There was Martin Ryle, who had come back from the war with truck loads of battered electronic junk and was trying to use this stuff to find radio sources in the sky. There was Max Perutz, who had already spent ten years on an x-ray analysis of the structure of the hemoglobin molecule and remarked quite cheerfully that in another 15 years he would have it. There was a crazy character called Francis Crick who seemed to have lost interest in physics altogether. Like most of my theoretical friends, I decided that I had nothing to learn from this bunch of clowns, and I came to America to be in a place where real physics was still being done.

Seven years later Bragg retired from the Cavendish. By that time it was clear to everybody that when he said he was going to teach the world how to do something else he was making no idle boast. He left Cambridge a center of furious activity and first-class international standing in two fields of research that are probably at least as important as high-energy physics in the overall scheme of things: radio astronomy and molecular biology.[1] Neither of these new sciences had even a name when Bragg was appointed in 1938. By 1953 Ryle's careful mapping of the radio sky was providing a system of reference for astronomers all over the world. The most gigantic and mysterious energy sources in the universe, the radio galaxies and quasars, now usually have names like 3C9 or 3C273, where "C" stands for "Cambridge." Also in 1953 the molecular biologists in Cambridge were not doing so badly. I do not need to describe the way they found the structure of DNA. Anybody who is interested in what it felt like to be a molecular biologist in Cambridge in 1953 can read James Watson's book "The Double Helix."[2] Many people have objected strongly to the way that book was written, but nobody could seriously argue after reading it that the Cambridge of 1953 was suffering from intellectual stagnation.

Unlike Rutherford, Bragg did not leave behind him a disintegrating empire. On the contrary, in the 17 years since he retired, the world standing of Cambridge both in molecular biology and in radio astronomy has been maintained in the face of increasingly intense competition. I have lost count of the number of my old friends in Cambridge who have won Nobel prizes. And two years ago Ryle's radio-astronomy group showed that they are still ahead of the world by discovering the first pulsars. I am happy that Bragg at 80 is in good shape physically and intellectually, and can enjoy this latest triumph of his protégés.

This history of the last 30 years in Cambridge is a little oversimplified. I have perhaps made it too much of a Horatio Alger success story. But I think it has important lessons for us today. What are the lessons? How did

Bragg manage to do so well with what looked in 1938 like a disastrous situation? Broadly speaking, I think he did well by following three rules. The rules are:

- Don't try to revive past glories.
- Don't do things just because they are fashionable.
- Don't be afraid of the scorn of theoreticians.

Besides following these prohibitions, Bragg had also some other more positive advantages. He still lived in the old European system, which gave the director of a laboratory power to do what he liked and to disregard the objections of his colleagues. He was operating, much of the time, in a wartime environment, which removed some of the normal bureaucratic constraints. And, above all, he had a great deal of luck. But luck of this magnitude does not come to a man twice in a lifetime unless he deserves it.

Princeton's record

I think it is fair to say that in Princeton over the last 30 years we have not done as well as Bragg did. Speaking only about my own place, the Institute for Advanced Study, I can say that we score high on Bragg's first rule—not trying to revive past glories. We have not since 1946 had a professor working in the field of general relativity. It seemed to us unreasonable to expect that we could find anybody in this particular field quite as good as Einstein. On the second rule, not doing just the fashionable things, we score middling. We have always had room for a few unfashionable people like Joe Weber, but a distressingly high percentage of our output of paper is in the fashionable part of particle physics and is to me indistinguishable from the paper produced by 20 other institutes of theoretical physics.

On the third rule, not being afraid of the scorn of the snobs, we score extremely badly. The most original, unfashionable and worthwhile thing that the Institute did since Einstein retired was the design and construction of John von Neumann's prototype electronic computer, the MANIAC. In the ten years after World War II, the group around von Neumann led the world in ideas concerning the development and use of computers. In its way, this was as big a thing as molecular biology or radio astronomy. But the snobs at our Institute could not tolerate having electrical engineers around them who sullied with their dirty hands the purity of our scholarly atmosphere. Von Neumann was like Bragg, strong enough to override the opposition. But when von Neumann tragically died, the snobs took their revenge and got rid of the computing project root and branch.

I always felt that the demise of our computer group was a disaster not only for Princeton but for science as a whole. It meant that there did not exist at that critical period in the 1950's an academic center where computer people of all kinds could get together at the highest intellectual level. The field we abandoned was taken over by IBM. Although IBM is a fine organization in many ways, it can not be expected to provide the atmosphere of intellectual fertility that von Neumann had created here. We had the opportunity to do it, and we threw the opportunity away.

So much for the past. How about the future? I was sorry when our computer project was destroyed, because it was something unique and ahead of its time. I have to confess that I am not equally sorry at the news that the Princeton–Pennsylvania accelerator is to be abandoned next year. It is not that I take any sadistic pleasure in the troubles of my friends. But I believe the loss of the accelerator puts Princeton into a position similar in some respects to that of Cambridge in 1938. The leadership of accelerator physics now passes to Batavia as then it passed to Berkeley. And we have an opportunity to do something different.

There is an excellent tradition in Princeton of not being too specialized. We shall certainly not give up high-energy physics completely just because we are out of the accelerator business. So I begin my prognostications of the future by taking a look at what might be expected to happen in high-energy physics in the next 30 years.

Accelerators and cosmic rays

There are two main ways of doing high-energy physics. The rich man's way is to build accelerators, which give high-intensity beams of particles with accurately controlled energy. The poor man's way is to use the cosmic rays, which descend like the rain from heaven upon rich and poor alike, but have very low intensity and completely uncontrolled energy. I think there is a better-than-even chance that the major discoveries of the next 30 years in high-energy physics will be made with cosmic rays. That is why I venture to say that it may be good for us, scientifically speaking, to be poor.

It can easily happen that I am wrong about the promise of cosmic-ray physics. Going into any field of research is always a gamble. Only in this case I believe that the gamble would be a reasonable one.

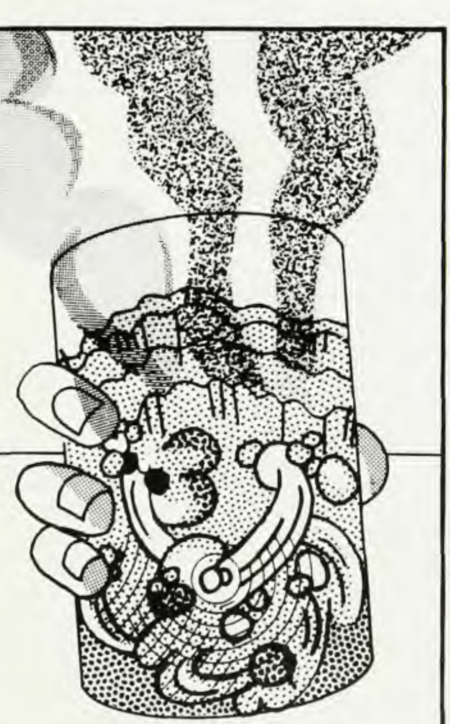
The highest-energy accelerator now operating is at Serpukhov in the Soviet Union and has an energy of 70 GeV. The machine at Batavia will beat this by a factor of at least six when it first starts to operate next year. Roughly speaking, the effect of the huge investment of money and talent at Batavia is to push the energy range of physics up by one power of ten, from the tens of GeV that we shall have in the 1970's.

We all devoutly hope that Nature has put important new phenomena that we can discover within this one power of ten. If it turns out that she has done so, the effort we put into building the machine will be worthwhile. If there are no basically new things to be found in this particular energy range, the machine will be a monumental flop. I am not taking any bets on which way it will go. But I feel that the long-range prospects for going on with accelerator physics in this style are not good. Even if the most optimistic assumptions about the Batavia machine are realized and we find an exciting new world of phenomena in the hundred-GeV range, we are nevertheless running into a law of steeply diminishing returns. To go a significant step scientifically beyond

Batavia means to go into the thousand-GeV range, and this is likely to be also the thousand-million-dollar range. Unless there are some radical improvements in accelerator technology, it looks as if Batavia will be the end of the line for quite a long time to come.

Contrast this with the situation in cosmic-ray research. Here you have substantial numbers of particles provided free, with energies of the order of 10^{15} eV, thousands of times higher than Batavia can produce. The problem is to build detectors that can sort out what happens at these extreme energies. In the past, because cosmic rays have been the poor man's province, detectors have mostly been primitive and inadequate for doing quantitative high-energy experiments. The accelerators have been able to produce so much more good physics in the last 20 years, only because the accelerator people put huge efforts into building bubble chambers, spark chambers, on-line computers, and other sophisticated detection apparatus. I think the best way to do high-energy physics during the next 30 years will be to look at cosmic rays in the same imaginative and slightly megalomaniac style that has been characteristic of the leading accelerator laboratories. To do cosmic-ray work in this style will be expensive, but nothing like as expensive as building new accelerators. It is the kind of research that can reasonably be undertaken by a university department rather than by a national laboratory.

This assessment of the future of high-energy physics is based on a philosophical point of view very different from the point of view of some of the accelerator propagandists. I have heard some accelerator enthusiasts talk as if they seriously expect, by building one more machine and measuring a few cross sections, to solve all the outstanding riddles of nature. I do not believe that anybody can read God's mind as easily as that. Our experience in high-energy physics so far has taught us that there are new problems and new complexities to be disentangled every time we extend the range of our observations. I would be disappointed, and I would consider that the Creator had been uncharacteristically lacking in imagination, if it turned out that no surprises remained in the vast range of energies beyond the reach of accelerators. My philosophical attitude toward physics is rather close to the attitude that was beautifully described by Giuseppe Cocconi of CERN in a recent article[3] called: "The Role of Complexity in Nature." Like Cocconi, I hope and believe that the universe of high energies will prove to be as inexhaustible as the universe of astronomy and the universe of pure mathematics.



Activated sludge

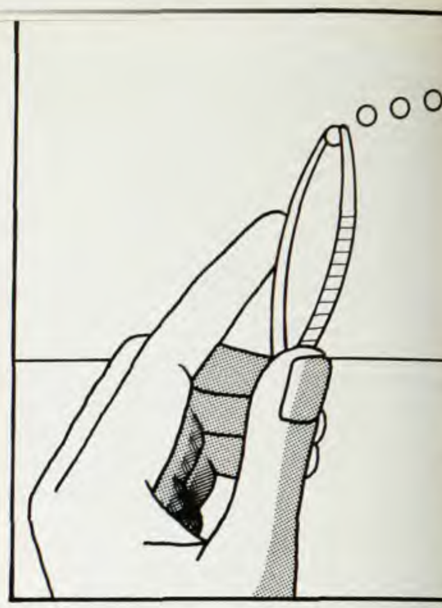
Apart from studying cosmic rays, what else is there for physicists to do? One possibility is to jump onto the antipollution bandwagon. I have even done a little of this myself, sharing an office for a month with a Professor of Sanitary Engineering who taught me all about BOD (Biological Oxygen Demand) and activated sludge. This was great fun, and my office mate was brighter than most of the physicists I know. I would advise any physicist who has a genuine concern for the environment to take the time to find out what the problems are in the field of activated sludge. He will also find out whether he has anything useful to contribute toward solving these problems.

An individual physicist, working in close collaboration with engineers and chemists and biologists, may well be able to make important contributions. However, he should not expect that what he does in the environmental field will be mainly physics. If he is any good, he will use his physics only as a cultural background in thinking about problems that are primarily chemical, biological or political in nature. Accordingly I think it would be a mistake for a physics department of a university to become heavily involved as a department in environmental work. Antipollution work is fine for individual physicists as members of interdisciplinary groups, but not for the central activity of a physics department. A department that rushes into environmental work just because it is fashionable is violating the second of Bragg's three rules.

Keep in touch with biology

I should give at least one concrete suggestion for a new direction in which physics may move and flourish during the next 30 years. So here are a few half-baked ideas that I believe may be important for the future. If I were an experimental physicist, this is the line I would be trying to follow.

I take it as self-evident that physics will not flourish in isolation from the rest of science. In particular, physics should keep in close touch with biology, as biology rather than physics is likely to be the central ground of scientific advance during the remainder of our century. Bragg understood this in 1946 when he put his money on Perutz and the x-ray analysis of hemoglobin in preference to a new accelerator. I think it is true now, just as it was in 1946, that a tremendous opportunity exists for making major advances in microbiology by means of physical techniques. An ambitious beginning has



been made by the MAN program at Oak Ridge National Laboratory.[4]

The biochemists have discovered that the large molecules that dominate the basic processes of life have a simple structure. These molecules are of two kinds, proteins and nucleic acids, and both kinds are linear chains. A protein is a long string of units, each unit being one of four nucleotides. Although the protein and nucleic acid chains are twisted and wound up in complicated ways when they are inside living cells, it seems to be true that their properties are uniquely determined by the sequence of units along the chain.

Until now we have had two ways to determine the structure of these molecules. One way is x-ray crystallography, the method Perutz used on hemoglobin and which led Crick and Watson to the double helix. The other way is wet chemistry, cooking the molecules with various reagents until they break into fragments, analyzing the fragments by chromatography, and finally deducing the sequence of the original molecule from the way the various fragments overlap. Both these methods have been used successfully on proteins and on small nucleic acids. Both methods have been brilliantly improved in recent years, so that a protein molecule like hemoglobin, which took Perutz 25 years to crack, can now be sequenced in less than a year.

However, both methods have two basic defects that seem difficult to surmount. First, both methods require macroscopic quantities of molecules in purified form, whereas the majority of biologically important molecules occur in minute traces in a soup of similar molecules from which they can hardly be separated. Second, both methods fail on the large nucleic acids, which are fundamentally the most interesting of all because they are the genetic material.

We have here an intriguing situation for a physicist. On the one hand, there is an enormous harvest of biological discoveries waiting to be reaped by the first man who can sequence an individual protein or nucleic-acid molecule without going through the miseries of chemically purifying a macroscopic sample. On the other hand, the technical problem of sequencing an individual molecule, when the molecule is known to be a linear chain, seems to be of a kind that modern physical methods should be peculiarly well suited to handle. Basically the problem is one of sorting and counting the units out of which the molecule is built, and sorting and counting are precisely the jobs that particle physicists know how to do.

Fishing, sorting and counting

Let me try to explain more specifically how I envisage a possible way in which the problem might be attacked. One would like, for example, to discover the precise sequence of nucleotides in the DNA molecules, which constitute the genetic material in a living cell. One needs first of all to have some way of fishing out one DNA molecule at a time. One needs to separate the molecule from its watery surroundings and to support it in a vacuum without damaging it. One needs to attach the molecule firmly at one place to a solid support, while the rest of the molecule is stretched into a straight line by an electric field and hangs freely in vacuum. One needs to detach the nucleotide units, one by one in sequence, from the loose end of the chain; this is the crucial and no doubt the most difficult step. One needs to ionize the detached units so that they can be steered into a mass spectrometer. Finally there is the easiest part of the whole operation, when the mass spectrometer sorts the ionized units into four channels labeled "adenine," "cytosine," "guanine" and "thymine," and the counters in each channel automatically record the sequence in which the units arrive.

The key to this style of analysis is to develop a technique for handling large molecules in a vacuum in such a way that one knows exactly where they are. I do not know how this can be done, but I will not be surprised if somebody learns how to do it within the next ten years. It will most likely be done by a physicist who is broadminded enough to master the chemical idiosyncrasies of nucleotides in addition to the physics of partially wet surfaces.

If the sequencing of individual molecules by physical methods turns out to be possible, either in the way I described or in some other way, the consequences will be startling. It will not only mean that a much wider variety of important molecules will be sequenced. The process will also be an extremely rapid one if it is feasible at all. By either of the two existing methods the sequencing of one protein is a major project, which keeps a team of talented people busy for a year or more. In contrast to this, one can envisage a physical sequencing apparatus that detaches and sorts nucleotide units at a rate of many per second, so that a giant DNA molecule would be completely analysed in an hour.

It is hard to imagine, in a process that handles individual molecules, that there could be any advantage in doing the job slowly. This increase in speed of analysis would cause a real revolution in microbiology. A single laboratory could sequence thousands of big molecules in a year instead of two or three. It would make sense to think of attacking a complete living cell and sequencing all of its protein and nucleic-acid constituents. I cannot pretend to foresee what the biologists will do with all this information. At the very least one should learn something interesting about cancer if one can compare in detail the constituents of a cancer cell with those of a normal cell from the same animal.

But is it "good physics"?

Some of you may object to this style of research, saying that it may be good biology but it isn't physics. That is what many of us were saying about Bragg and Perutz in 1946. I believe we were profoundly mistaken. The idea that physics has to be pure in order to be good, that work on the borderline between physics and biology is beneath the dignity of a true physicist, was wrong in 1946 and is still wrong today. William Spohn's recent article[5] called "Can Mathematics Be Saved?" made some stir in the mathematical world. Spohn's thesis is that the purists who dominate the mathematical establishment have alienated mathematics so completely from the rest of human culture that mathematics itself is in danger of becoming sterile. Much of what he says is equally true if you change the title of his article to "Can Physics be Saved?" and understand "high-energy physics" where he says "modern mathematics." In my opinion, the surest way to save physics from some rather catastrophic stagnation or decline during the next 30 years is to keep young physicists working on the frontiers where physics overlaps other sciences, such as astronomy and biology.

I have described one possible example of such work, the analysis of big molecules by physically pulling them apart. It is easy to imagine other examples. One possibility that has been much discussed among molecular biologists is the development of electron-microscope technology to the point at which the structure of individual molecules becomes directly visible. It might be possible in this way to achieve a nondestructive analysis of large molecules, as versatile and rapid as the destructive analysis that I described.

It would be pointless for me to try to make a complete list of the important things physicists will do in the coming decades. Inevitably the most exciting things will be those I haven't thought of. I myself find that the most exciting part of physics at the present moment lies on the astronomical frontier, where we have just had an unparalleled piece of luck in discovering the pulsars. Pulsars turn out to be laboratories in which the properties of matter and radiation can be studied under conditions millions of times more extreme than we had previously had available to us. We do not yet understand how pulsars work, but there are good reasons to believe that they are the accelerators in which God makes cosmic rays. Besides providing cosmic rays for the particle physicists, the pulsars will, during the next 30 years, provide crucial tests of theory in many parts of physics ranging from superfluidity to general relativity.

I have tried to give here an honest assessment of those tendencies in physics that I find good and bad. I am not gloomy about the future of physics. I agree with Senator Mansfield that a reduction in the level of our financial support would not be a national catastrophe. To my mind there are only two things that would really be disastrous for the future of physics. One is if we would solve all the major unsolved problems. That would indeed be a disaster, but I am not afraid of it happening in the foreseeable future. The other disastrous thing would be if we become so pure and isolated from the practical problems of life that none of the brightest and most dedicated students want any longer to study physics. This second danger does seem to me to be a real one. It will not happen if we stay diversified, if we emphasize work that has important applications outside of physics, and above all if we follow Bragg's third rule: "Do not be afraid of the scorn of theoreticians."

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This article is adapted from a talk given at the dedication of Jadwin and Fine Halls, Princeton University, March 1970.

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