FREEMAN J. DYSON

QUICK IS
BEAUTIFUL

THE FIFTH
TYKOCINER MEMORIAL LECTURE

University of Illinois
at Urbana-Champaign
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BY

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UNIVERSITY OF ILLINOIS
AT URBANA-CHAMPAIGN
LINCOLN HALL THEATER
APRIL 7, 1981
8:00 P.M.
JOSEPH TYKOCINSKI TYKOCINER
1877-1969

Born and educated in Poland, Professor Tykociner worked with Marconi on the first transatlantic wireless communication. His active career as engineer and scientist spanned more than seventy years. The first fifty years of his work were mainly in the physical sciences, beginning as a pioneer in the field of radio. He is probably best known for his invention of sound-on-film, successfully demonstrated in 1922 at the University of Illinois. In all of this work his motives were humanistic; his goals were to improve communications as a tool for education and to improve understanding among peoples. His last score of years was devoted with intensity to the study of the science of research, which he named "Zetetics"; this field encompasses the humanities, arts, and social sciences as well as the physical sciences. Professor Tykociner bequeathed his estate to the University for continuation of this science. His dream of integrating all research and knowledge has led to the establishment of this lecture series, made possible by the Tykociner Memorial Fund.

Professor Tykociner was a member of the Electrical Engineering Faculty at the University of Illinois at Urbana-Champaign from 1921 until he retired in 1948. He remained a very active Professor Emeritus until his death in 1969.
FREEMAN J. DYSON

We are scientists second, human beings first. We become politically involved because knowledge implies responsibility. We fight as best we can for what we believe to be right. Often we fail. —Freeman Dyson, Disturbing the Universe

Freeman J. Dyson, Professor of Physics, the Institute for Advanced Study, Princeton, N.J., was born in Crowthorne, Berkshire, England, in 1923. He attended Winchester College (1936–1941) and the University of Cambridge (1941–1943). From 1943–1945, he served as a civilian in the Operations Research Headquarters, RAF Bomber Command. In 1945, he received his B.A. in Mathematics from Cambridge. He went to Cornell in 1947 with a Commonwealth Fellowship, and became a professor of physics in 1951. In 1953, he moved to the Institute for Advanced Study.

Professor Dyson has studied a remarkably wide range of problems in his scientific career: including quantum electrodynamics, nuclear physics, astrophysics, the TRIGA nuclear test reactor, the Orion spaceship, solar ponds and the environmental impact of additional carbon dioxide releases into the atmosphere.

He is a member of the U.S. National Academy of Sciences. He received the Danny Heineman Prize of the American Institute of Physics; the Lorentz Medal of the Royal Netherlands Academy; the Hughes Medal of the Royal Society (London); the Max Planck Medal of the German Physical Society; the J. Robert Oppenheimer Memorial Prize, Center for Theoretical Studies; and the Harvey Prize by Technion (Israel).

His popular scientific articles for the Scientific American and the New Yorker, and his book, quoted above, have attracted much public attention and establish him as an outstanding commentator on the interplay among science, politics, morality, and the public interest.

He became an American Citizen in 1957.
REMARKS OF WELCOME

by
John E. Cribbet
Chancellor
University of Illinois at Urbana-Champaign
Urbana, Illinois 61801

Welcome to the fifth Tykociner Memorial Lecture. This series, which has brought a group of truly distinguished scholars to our campus over the past several years, was established to honor Professor Joseph Tykociner, a long-time professor of electrical engineering, whose bequest made the series possible. Professor Tykociner was remarkable not only for his imaginative research in a broad range of electrical engineering topics, but also for his deep conviction that technology is the servant of society, that all knowledge is interrelated, and that scholarship must be motivated by the need to serve mankind.

In furtherance of Professor Tykociner's ideals, we have solicited participation in this series by scholars in the humanities, in physical science, and in biology. They have brought us a wealth of thought on the interrelations of these branches of knowledge and their interactions with the needs of society.

Tonight we are extremely fortunate to have with us Professor Freeman Dyson of the Institute for Advanced Study in Princeton. Professor Dyson is an outstanding theoretical physicist, as is attested amply by his membership in his distinguished institution and by his many honors and prizes. He is also known to the well-read lay public for his thoughtful writings on science, society, philosophy, and politics, as illustrated by his books and his articles in the quality magazines. Many of you will be familiar with them.

This evening will be in worthy succession to those previous occasions on which we have heard the thoughts of Isaiah Berlin, Dennis Gabor, Sol Spiegelman, and Leon Cooper.

It is also a fitting tribute to the memory of Professor Tykociner and to the philosophy which he espoused. He would have been
thrilled and excited by the spirit of this lecture series and by the success with which his bequest has been implemented.

Again, I extend a most cordial welcome on behalf of the Campus, and I express the appreciation of all of us for the efforts of the Tykociner Lecture Committee which make this series possible.

(Professor Dyson will be introduced by Professor John Bardeen.)
INTRODUCTION OF FREEMAN J. DYSON
by
John Bardeen
Professor Emeritus of
Electrical Engineering, Physics, and Center for Advanced Study
University of Illinois at Urbana-Champaign
Urbana, Illinois 61801

It is a great pleasure to have Freeman Dyson with us this evening to give this year's Tykociner Lecture. He is one of the world's leading theoretical physicists. Among his many awards are the Heineman Prize, the most prestigious award for work in mathematical physics, the Hughes Medal of the Royal Society of London, the Lorentz Medal of the Royal Netherlands Academy and the Max Planck Medal of the German Physical Society.

Dyson became interested in mathematics as a boy in England and his remarkable abilities were soon recognized. During World War II he helped analyze the effectiveness, or perhaps better the reasons for the relative ineffectiveness, of bombing German cities in reducing German war production. After the war he continued his schooling at Cambridge University and then migrated to Cornell University where he made outstanding contributions to the formulation of quantum electrodynamics, a field theory, generalizations of which form our present understanding of the structure of matter. His great abilities were recognized by Robert Oppenheimer who persuaded him to come to the Institute for Advanced Study in Princeton in the early fifties, where he has remained since.

Over the years, his activities have covered a broad range, including such questions as to whether intelligent life exists in the universe outside of our small planet. His current interests are in astrophysics and such technical problems as solar energy.

The Tykociner Lectures attempt to cover the culture gap between science and the arts and humanities. Dyson is becoming known outside of scientific circles for his semi-autobiographical writings published in the New Yorker and more completely in a book entitled
Disturbing the Universe in which he attempts to describe to the non-scientist what motivates scientists, what determines their value judgments, what makes them tick.

From his family background Dyson obtained a strong interest in the arts and humanities that have remained with him throughout his life. He obtained a love for music from his father, Sir George Dyson, a teacher, composer and musical director of the London Symphony Orchestra. He obtained a love of poetry, which he makes very effective use of in his writings, from his mother, a lawyer by profession.

In the scientific world we live in, policy decisions often must be based on recommendations of scientists. However, scientists may have differing views that do not depend on differences in regard to scientific questions but on value judgments that are independent of science. In his talk this evening Dyson will describe some experiences, good and bad, on applications of science to technology and try to distill from these some general rules which might be helpful in using scientific discoveries effectively for satisfaction of human needs.

The title of his talk is “Quick is Beautiful.”
I am happy tonight to be honoring Joseph Tykociner, because he was a great inventor and it is time for the pure scientists of America to repay a little of the debt of gratitude which they owe to the inventors. I speak as a pure scientist who has reaped enormous benefits, intellectually and materially, from the social climate created in this country by the generation of inventors to which Tykociner belonged. I detest and abhor the academic snobbery, especially prevalent in places like Princeton, which places pure scientists on a higher cultural level than inventors. High culture and human understanding are sometimes to be found among pure scientists, but they are to be found just as frequently among inventors.

Joseph Tykociner resembled my hero Michael Pupin, another great inventor who came to America from Eastern Europe. Pupin was twenty years older than Tykociner, and was more successful financially, but in all essential respects Pupin and Tykociner were kindred spirits. They shared an intense idealism and a deep faith in the perfectibility of man and in the ennobling influence of science. Pupin wrote a delightful autobiography with the title “From Immigrant to Inventor,” for which I wrote a preface when it was reprinted twenty years ago. I quote now a few sentences from the preface:

“Pupin believed with passionate intensity that the primary aim of science is the pure understanding of nature, and that useful applications must be considered of secondary importance. The prestige and influence which he derived from his inventions he used in an unceasing campaign to improve the standing of fundamental science in America. In this way the paradoxical situation arose, that it was
Pupin the practical inventor who did more than any other man of his
time to convince the American public that great scientific discoveries
are more important than inventions. Pupin’s triumph has been so
complete that now, twenty-five years after his death, pure scientists
have more prestige, more influence and more financial support than
he would have imagined possible. Perhaps, in this apotheosis of the
fundamental researcher, some injustice has been done to the class of
inventors to which Pupin himself belonged. We have reached the
point where a first-rate inventor is rarer than a first-rate scientist.
Inventors are no longer welcomed in most university departments,
and even in industrial laboratories pure research is becoming more
and more the fashionable thing to do. Perhaps the time will soon
come when a group of pure scientists will be compelled to organize a
campaign to prevent the permanent extinction of the inventor.”

These words were written twenty years ago. Perhaps we are see-
ing in recent years some signs of an increasing public awareness of
the importance of invention. Perhaps even the scientific community
is becoming aware that its survival depends in the long run upon
maintaining a healthy balance between pure science and invention.
You people here, by establishing this lecture series in memory of
Tykociner, took a step in the right direction. You made an excellent
start by choosing the inventor Dennis Gabor as your first lecturer. I
hope he was right when he said in his lecture that the heroic age of
invention may be returning. I am proud to be following in his foot-
steps. In my talk tonight I shall try to provide tentative answers to
two questions. Why have our efforts to apply science fruitfully to
human needs in recent decades been so conspicuously unsuccessful?
And what can we do now to make things go better?

It is a curious paradox that nowadays the progress of pure science
is being hampered by excessive short-sightedness while the progress
of useful invention is being hampered by excessive long-sightedness.
The politicians who fund pure science are denying support to pro-
posals which do not promise a successful outcome within a year or
two, while the politicians who fund applied research in the Depart-
ment of Energy give the bulk of their support to projects which can-
not possibly come to fruition in less than ten years. Let me begin by
making it clear that I am talking tonight about invention and not
about pure science. I must apologize to those of you who are mainly
interested in fundamental research. I have nothing to say to you. I hope nobody will think I am proposing “Quick is Beautiful” as a good motto for a basic research laboratory. On the contrary. As Lewis Thomas recently said: “For anything very good to come from basic research it simply has to come in the nature of things as a total surprise. It is very hard to program the work far in advance and in fine detail when what you are looking for is really an astonishment.” Basic research needs to be supported by the decade rather than by the year, without any assurance of quick success or quick pay-off. But I assume that many of you have other things to do besides basic research, and it is these other things that I want to talk about. I assume that at least some of you are here for the same reason that I am here, not to think about pure science but to think about ways in which the scientific community may help solve some of the urgent practical problems now facing mankind.
II. A Tale of Two Reactors

We often hear it said that there is a similarity between the human condition of the nuclear physicists some forty years ago and that of the biochemists and biologists today. There are many obvious differences between nuclear physics and microbiology, but the analogies are none the less real. We are now entering a period of intensified biological research and are considering a variety of possible applications of genetic engineering. Perhaps the historical experience of the physicists may have left us with some practical wisdom which might enable the genetic engineering industry to avoid the mistakes which have brought the nuclear power industry into such serious trouble. I will try to give you tonight what little wisdom I have collected in forty years as a part-time applied physicist. Let me come at once to the two questions I am trying to answer. Restated in simpler words, the two questions are: What did the nuclear physicists do wrong? And what can the genetic engineers learn from our misfortunes? I will tell you a few stories about things that I have seen happen during my life as a physicist, and you can then judge for yourselves whether these stories have any relevance to the problems of genetic engineering.

The first story concerns a company called General Atomic which runs a laboratory in La Jolla, California, and manufactures nuclear reactors. The company began as a division of the General Dynamics Corporation in the year 1956. In the summer of that year the company brought together a group of consultants, some expert and some non-expert in the details of reactor engineering, and paid us to sit and think for three months. I was one of the non-expert consultants. The company was then brand new; it had no laboratories, no production facilities, and no products. The consultants could do nothing except think and talk and scribble on blackboards. The company promised to pay one dollar to the inventor, for the patent rights to any reactor which we might invent. I collected my dollar, and so did several other people in the group. In return for this substantial outlay, the company ended the summer with preliminary designs for three new types of reactor with some promise of commercial profitability. One of these designs was chosen for immediate development and went into production with the name TRIGA, standing for
Training, Research and Isotope-production, General Atomic. The first TRIGA was built, tested, licensed and sold within less than three years from the day the consultants assembled in 1956. The company is still producing it and still selling it at a profit. I believe you people have one right here on this campus. The TRIGA is of course not a power-reactor; it is mostly used to produce isotopes for medical research and diagnosis, not to produce electricity.

As a follow-on to the TRIGA, General Atomic decided to develop and market a big power-reactor called HTGR, High-Temperature Gas-Cooled Reactor. The HTGR is theoretically a great reactor. Its high temperature gives it an advantage in thermodynamic efficiency over water-cooled reactors, and its big heat capacity gives it an advantage in safety. It is inherently much less vulnerable to mishandling than the light-water reactors which have monopolized nuclear power production in this country. Unfortunately the HTGR never captured a substantial share of the market. General Atomic sold one each of two versions of the HTGR. The first was a 40 megawatt (electric) version, which produced electricity for a utility company at Peach Bottom, Pennsylvania. It was turned off a few years ago because the utility decided it cost more to run it than 40 megawatts was worth. Peach Bottom was always intended to be a small-scale experiment, a harbinger of bigger and better things to come. The second HTGR sold was eight times more powerful, a 300 megawatt version which is now running at Fort St. Vrain in Colorado. The Fort St. Vrain reactor has a technical problem. When you try to run it at full power, the temperature in the core does not stay steady but wiggles a bit, probably because of some complicated coupling between the thermal expansion of graphite blocks in the core and the flow of the cooling gas through channels in the blocks. The temperature wiggles do not look dangerous, but to be on the safe side the reactor is licensed to run at only seventy percent of full power. At seventy percent power it runs smoothly. Still, you cannot call it an outstanding success of HTGR technology. You cannot expect other utility companies to come rushing with orders for more copies of the Fort St. Vrain reactor until this little problem is fixed.

Now I come to the Spring of last year, 1980. In the meantime General Atomic had been bought and sold twice, sold by General Dynamics to Gulf Oil and sold by Gulf Oil to two other oil compa-
nies, but these upheavals have left the company essentially intact. General Atomic is still in business and still has dreams of selling HTGR reactors. A year ago Harold Agnew, newly appointed president of the company, decided to hold a Class Reunion for the Class of 1956. He invited all the surviving members of the group of consultants who had started the company going with such high hopes in 1956 to come back and have another look at it. Of course we had all in the meantime grown old and dignified, and we were all much too important and too busy to come back for three months and work out some new inventions. The most we could do was to come back for two days and have a good time remembering our lost youth. We did have a good time. And incidentally the General Atomic staff told us about their recent activities and about their plans for the future.

The main thing which the General Atomic people had to tell us was the result of two safety analyses of their full-scale HTGR power-reactor. By full-scale they mean a thousand megawatts electric, two and a half times the designed power output of Fort St. Vrain. For many years they have concentrated their major effort on the detailed design of a full-scale thousand-megawatt HTGR, a reactor which has not yet been built. In the meantime, two independent safety-analyses of the full-scale HTGR have been done, one by a group of experts in the United States, the other by a group in Germany. Neither group of experts was connected with General Atomic; neither group had any commercial incentive to make the HTGR look good. And both groups came out with similar conclusions: in a certain well-defined sense, the HTGR is roughly a thousand times as safe as a light-water reactor of equal power. The meaning of this statement is the following. The experts analyzed billion-year accidents, caused by combinations of stupidity and bad luck more extreme than anything we saw at Three-Mile Island. A billion-year accident requires so much bad luck that it is supposed to happen only once in a billion years of reactor running time. As you can imagine, a billion-year accident is a hell of a lot worse than Three Mile Island. The reactor core vaporizes, the concrete containment building splits open, the atmosphere happens to have an inversion layer at the worst height and the wind is blowing in the worst direction over a region of high population density. You do not need to believe in the accuracy of the calcu-
lation which says that this disaster happens once in a billion years. All that you need to believe is that it is possible to apply the rules of the accident-analysis game fairly, so that a billion-year accident for a light-water reactor and a billion-year accident for the HTGR are in some real sense equally unlikely. The results of the analyses are then startlingly favorable to the HTGR. The billion-year accident of a light-water reactor kills 3300 people immediately and 45000 people by delayed effects of radiation. The billion-year accident of the HTGR kills zero people immediately and seventy people by delayed effects. The numbers make no claim to accuracy, but the conclusion is qualitatively clear. It is conceivable that a mishandled HTGR may kill people, but it cannot kill them wholesale.

The next question that arises is then, if the HTGR is a thousand times as safe as a light-water reactor, and if public worries about accidents are threatening the very existence of the nuclear power industry, why is there not a crowd of utility executives standing at the door of the General Atomic sales office, waiting to trade in their light-water reactors for a shiny new HTGR? The answer to this question is simple. Even if the utility executives were in a mood to buy new and improved nuclear power-plants, General Atomic would have none ready to sell. The full-scale HTGR has never been built. The components are not in production. The final stages of its engineering development are not complete. If a customer should now come to General Atomic wanting to order a full-scale HTGR, the best that General Atomic could do would be to say: “Well, wait a moment. If you can help us raise a half-billion or so of government money to finish the engineering development, and if we don’t run into any unexpected snags, with luck we could be ready to begin construction in a few years, and if the licensing goes smoothly you should have your reactor on line within ten years after that.” This is not the kind of answer which brings utility executives running to place orders. Nobody in his right mind wants to commit himself to a huge capital investment which will only begin to pay dividends, if all goes well, twelve years later. Only governments can afford to make such investments, and if they are wise they do not make them very often.

So here we see in a nutshell the tragedy of nuclear power. The world crying out for safer power reactors. A company staffed by
capable and dedicated people, with designs for a safer reactor, eager to go ahead with building it. And nothing can be done in less than twelve years. That is why I chose for the title for this talk, "Quick is Beautiful."
III. False Economies of Scale

I told you this story of the two reactors, the TRIGA which was finished and ready to go in three years and the HTGR which cannot be ready in less than twelve years, because I happen to have been personally involved with them. Similar stories could be told about many other industrial products. The nuclear industry is not the only one which has suffered from a hardening of the arteries and lost the ability to react quickly to changing conditions and changing needs. I believe the difference between a three-year and a twelve-year reaction-time is of crucial importance. The rules of the game by which public life is governed, both in the United States and in the world outside, are liable to drastic and unpredictable change within less than ten years. By rules of the game I mean prices, interest-rates, demographic shifts and technological innovations, as well as public moods and government regulations. We have recently seen some spectacular changes in the rules of the game which the automobile industry has to play. We can expect such sudden changes to occur from time to time, but nobody is wise enough to predict when or how. Judging by the experience of the last fifty years, it seems that major changes come roughly once in a decade. In this situation it makes an enormous difference whether we are able to react to change in three years or in twelve. An industry which is able to react in three years will find the game stimulating and enjoyable, and the people who do the work will experience the pleasant sensation of being able to cope. An industry which takes twelve years to react will be perpetually too late, and the people running the industry will experience sensations of paralysis and demoralization. It seems that the critical time for reaction is about five years. If you can react within five years, with a bit of luck you are in good shape. If you take longer than five years, with a bit of bad luck you are in bad trouble.

Let me now go back to the example of the nuclear reactor industry. What happened between 1956 and 1980 to cause such a disastrous slowing-down of the reaction-time? Part of the loss of flexibility can be blamed on government regulations and part can be blamed on the hardening of arteries in individual heads. The people who run the industry are not as young as they were. But regulation
and aging are not the whole story. There are also some identifiable errors of policy which contributed to the slowing-down. In my opinion, the two chief causes of the loss of flexibility of the industry were bandwagon-jumping and false economies of scale.

Bandwagon-jumping is not always bad. Only, before you jump on, you should look carefully to see whether the bandwagon is moving in the direction you want to go. If you are doubtful about the direction, it is a good idea to wait. In the case of the American nuclear power industry, the bandwagon was started by Admiral Rickover, who developed with admirable speed and efficiency a reactor to drive nuclear submarines. Rickover's reactor went into mass production and gave a flying start to the industry. It was a pressurized light-water reactor. So the light-water bandwagon started to roll. When the time came to build power-reactors for the civilian utility market, everybody except General Atomic jumped onto Rickover's bandwagon. Unfortunately they overlooked a well-known fact about submarines. There is not much room to spare in a submarine. Therefore the most important requirement for a submarine reactor is to be compact, to have a lot of power in a small volume. But when you build reactors for civilian utilities, the most important requirement should be safety. Other things being equal, the more compact a reactor is, the more power it generates in a given volume, the more quickly it will melt or vaporize in case of an accident with a loss of coolant. The shorter the time that is available before the reactor melts, the easier it is for somebody pressing the wrong switches to turn an accident into a catastrophe. So compactness and safety are not running in the same direction. The main reason why the HTGR is safer than a light-water reactor is that it is less compact. What is good for submarines is not necessarily good for civilians. But once almost everybody had jumped onto Rickover's bandwagon, it became very difficult for anybody to jump off. By jumping on too soon, the nuclear industry deprived itself of alternative technologies which were leading in different directions. When the public rather suddenly became aware of the deficiencies of light-water reactors, the Rickover bandwagon ground to a halt, but the passengers were then left stuck in the mud with nowhere else to go.

The effect of the bandwagon in immobilizing the nuclear industry was bad enough, but the effect was made much worse by a second factor, the pursuit of false economies of scale. I am not denying the
reality of economies of scale. I am not recommending “Small is Beautiful” as a suitable motto for the petrochemical industry. Up to a point, big plants are usually more economical than small ones. Big nuclear reactors, up to a point, generate cheaper electricity than small ones. But I am saying that the economy of scale is lost or even reversed when the big plant takes too long to build. If a plant takes ten years to build, it is almost certainly too big. The economy of scale is likely to be canceled out by interest charges and by loss of flexibility, and it will often happen that changes in the rules of the game make the big plant obsolete before it even comes on line. So I am saying that you should pursue economies of scale up to the point where each unit takes about five years to bring on line, but no further. Further than that, it is a false economy.

The light-water reactor industry probably made a fundamental mistake in going to thousand-megawatt units. The expected economy of scale seems to have been illusory. Unfortunately, General Atomic felt compelled to make the same mistake with the HTGR. Just to keep up with the Joneses, General Atomic concentrated its efforts on the thousand-megawatt monster which cannot be ready when it is needed.

The market for nuclear power reactors is at the moment non-existent. Nobody knows whether the market will revive in the future. The hopes of the industry rest on the possibility that there will be some new oil crisis or some unpredictable change of political mood which will create a massive new demand for nuclear power. When this happens, the demand will be for reactors which are safe, and flexible, and quick to build. The thousand-megawatt HTGR is safe but not quick. Perhaps General Atomic might finally achieve its rightful share of the market, if it could be ready when the time comes with a HTGR reactor of modest size, thoroughly tested and debugged, and capable of being mass-produced in a hurry.

One of the most beautiful pieces of technology I have ever seen is the factory in Everett north of Seattle where they build Boeing 747’s. The Boeing 747 is not small and neither is the factory. But the factory is wondrous quick. At the time I visited, they were turning out 747’s at the rate of one a week. That is the sort of operation which I have in mind when I say, Quick is Beautiful.

People of my generation who lived through World War II have vivid memories of monumental confusion and incompetence — after
all, the word SNAFU is of World War II vintage — and in spite of all that, we remember that in the end things got done. When Winston Churchill became Prime Minister in 1940, England was desperately short of ships, airplanes, tanks, guns, everything that we needed to fight a war. I saw how bad things were when the little old 22-calibre rifles that my high-school ROTC used for target practice were taken away from us and given to the army. Those rifles probably last saw active service in the Crimea in 1856. In 1940 Winston Churchill spoke on the radio and said, “I am sorry I cannot do anything for you in less than three years. I give an order to build a factory today, and in two years you have nothing, in three years you have a little, in four years you have a lot, in five years you have all you want.” He was right. In five years we had all we wanted and in five years the war was over.

These experiences of World War II made an indelible impression on people of my generation. At the bottom of our hearts we still believe you can have anything you want in five years if you need it badly enough and if you are prepared to slog your way through the barriers of confusion and incompetence to get it. Some of us even believe that if tomorrow the oil-exporting countries would do us the favor of imposing a permanent embargo on shipment of oil to this country, the shock to our system would be sufficient to push us into a serious synthetic-fuel program, and we would end up within five years producing so much fuel that we could undersell OPEC in the world market. Such ideas are totally contrary to the accepted wisdom of our economists and politicians. The accepted wisdom says that, no matter what we decide to do about the energy problem, we cannot expect to see any substantial results before the year 2000. The accepted wisdom is no doubt correct, if we continue to play the game by the rules of today. But anybody who lived through World War II knows that the rules can be changed very fast when the necessity arises.

Why is it that our whole economic and political system has tended recently to become so sluggish and inflexible? Why have we become resigned to the idea that nothing substantial can ever be done in less than ten years? Obviously there are many reasons. But I believe the principal reason for this sluggishness is that our whole society has fallen into the same trap as our nuclear industry. Not only in the nuclear industry but in many other industries and public institutions,
we have pursued economies of scale which turned out to be false. One of the most fundamental false economies of scale is the overgrowth of cities. At one time it looked economically attractive to cram millions of people together into huge agglomerations. The budgetary problems of New York City have now made clear to everybody that this was a false economy.

When we turn from sociology to biology, we see the same historical processes at work. So long as no sudden changes in the rules of the game occurred, all through the soft swampy sluggish hundred-million-year summer of the mesozoic era, the dinosaurs pursued their economies of scale, growing big and fat and prosperous, specializing their bodily structures more and more precisely to their chosen ecological niches. Then one day, as we recently learned from the brilliant observations of Luis Alvarez and his colleagues at Berkeley, an asteroid fell from the sky and covered the earth with its debris. The rules of the ecological game were changed overnight, and our ancestors, the small, the quick, the unspecialized, inherited the earth.
IV. Ice-Ponds and Heat-Engines

Let me now tell you a more cheerful story. In Princeton there are two projects in progress, each of them in its own way trying to contribute to a solution of the energy problem. The two efforts stand side by side on the Forrestal Campus of Princeton University. One of them is the TFTR, the Tokamak Fusion Test Reactor, the white hope of the magnetic confinement fusion program, a magnificent piece of engineering, lavishly funded by the Department of Energy. If all goes well, it will cost only $300 million and will be ready to go into operation in a year or two. It will then explore the technology for commercial fusion reactors which will possibly begin producing electricity ten or fifteen years later.

The other project, the one with which I have the honor to be associated, is the Princeton Ice Pond. The 1980 version of the ice pond was a square hole in the ground with a dirt berm around it and a sheet of Griffolyn plastic lining its bottom. Two men with a mechanical digger dug the hole in January 1980. We rented a commercial snow machine and squirted snow over the hole during the cold days and nights of February, until we had something that looked like the Disneyland Matterhorn. Halfway through the snow-making, we found out that we didn’t need that fancy ski-resort snow-machine. We didn’t need skiing-quality snow for our pond. We found out that for our purposes a fireman’s fog-nozzle which you can buy for $300 will do the job well enough. Our Matterhorn stood high and proud for a few weeks. Then the March sun shrank it down a bit, and the April rains reduced it to a pool of slush, filled up to the top of the berm. We covered it over with an insulating layer of plastic and straw, and on top of that we put an air-supported mylar dome to keep the straw dry. In June a hefty hail-storm wrecked the mylar and so we made do with wet straw for the insulation. I say “we,” but you must understand that I am not claiming credit for any of this. The project is run by Rob Socolow and Don Kirkpatrick and Ted Taylor and their students at the Center for Environmental Studies of Princeton University. I am only an unskilled laborer who goes out to help them occasionally. In June we measured the contents of the pond and found that we had about 450 tons of ice with some water underneath it.
All through an exceptionally hot Princeton summer we successfully airconditioned a building by circulating fresh water from the bottom of the pond. We were melting ice at a peak rate of about 7 tons a day; beautiful cool white ice with crevasses into which we could descend and enjoy Alpine scenery under the blazing Princeton sun. When the hot weather came to an end at the beginning of October there were still about 150 tons of ice left. I am sorry that I don’t have any pictures of the ice-pond to show you.

We are now well embarked on the 1981 model Ice-Pond. The 1980 model proved that the idea works, but it was a muddy and messy job, all right for Princeton students but unsuitable for suburban home-owners or business executives to look at out of their picture windows. So the objective of the 1981 experiment is to make the ice-pond elegant. We want also to make it out of more permanent materials, so that it will not need too much maintenance. The ideal is to have a system that works year after year, so that the owners can more-or-less forget about it. Above all, the pond must not give the owners headaches. People have enough headaches already. If ice-ponds cause additional headaches, nobody will want to own them.

The 1981 season began at the beginning of January. We put up a permanent structure, a round frame of light steel girders, and covered it with a heavy-duty plastic dome open at the sides. In a few cold nights we made about a thousand tons of snow under the dome. We now have another Disneyland Matterhorn with its peak about twenty feet above ground level, but this time it is sitting under a plastic roof, well protected from the warm air outside. We have made enough ice to see us through the summer, and most of it will still be there when the hot weather starts.

This summer we shall be air-conditioning two buildings with the ice in the pond. But the main objective this year will be to demonstrate that the job can be done without creating an eye-sore, in a style compatible with the aesthetic standards of real-estate developers and architects.

The ice-pond project is not supported by the Department of Energy. We applied to DOE for funds several times, but the most DOE could do for us was to tell us to apply to Housing and Urban Development, and when we applied to HUD they told us, not un-
expectedly, to go back to DOE. The project finally got started in January 1980 because the Prudential Insurance Company decided we might be a good investment. The Prudential is prepared to spend $300 thousand (not million) to find out whether we are as crazy as we look. The Prudential does not require us to spend three quarters of the money on paper-work.

Now why should the Prudential Insurance Company be interested in supporting a technologically primitive and low-grade project like ours? It happens that the Prudential is investing its surplus cash in the construction of several office buildings in an industrial park near the Forrestal Campus. The possible pay-off for the Prudential is a solar heating and cooling system for one of their office buildings. If our wildest dreams come true, we will be able to supply solar heating and cooling to the building at a capital cost equal to the cost of fuel and electricity used by an equivalent conventional system in about two years. In other words, the Prudential would write off the cost of the solar system in two years and would enjoy free heating and cooling thereafter as long as the system lasted. The Prudential is prepared to gamble $300 thousand on the remote chance that something like that might happen. The government, of course, does not like to gamble.

The key to cheap and reliable solar energy is to have a cheap and massive storage of heat and cold, massive enough so that it can ride over the annual weather-cycle, heat being collected in summer and used in winter, cold being collected in winter and used in summer. The system which we have in mind for the Prudential building would use two ponds for storage, a hot pond containing 100000 tons of hot water, (roughly 2 acres 30 feet deep) and a cold pond containing 10000 tons of ice (roughly ¼ acre 30 feet deep). We started first with the ice-pond experiment because the money came through in January 1980 just in time for the snow-making. It is much easier to make snow in a hurry in winter than to make hot water in a hurry in summer. We hope later on to have an experimental hot pond connected to a large area of cheap plastic air-mattresses collecting solar heat. Then in the following winter we shall find out whether the hot pond stays hot. Unfortunately our students are so busy with the ice-pond that we missed the chance to get started on a hot-water pond in time for this year’s summer.
The beauty of solar-pond technology, to my eyes, lies in the fact that your mistakes do not stay hidden for long. We made a mistake with the mylar dome; in two months the hail ripped it apart and we are ready to try something else. We made another mistake with our heat-engine. An important part of our original plan was to generate home-made electricity with a heat-engine, using the hot and cold ponds as heat-source and heat-sink. The ideal Carnot efficiency of an engine working between 140 degrees Fahrenheit and ice-temperature is twenty per cent, so we thought we could expect to run a real engine making electricity at ten per cent efficiency. We found a supplier in Florida who claimed he could sell us an engine for $12000, putting out ten kilowatts of electricity. 1200 dollars capital cost per kilowatt, and zero cost for fuel, would compete quite well with central-station power. The Department of Energy people told us, in their lordly fashion, that the Florida engine was a pile of junk, but that only made us more determined to prove the Department of Energy wrong. We arranged with the designer in Florida that he would rent us one of his engines for three months. He drove it himself all the way from Florida to Princeton and proudly handed it over. The machine looked good, not like a pile of junk at all. The designer told us how he had run it in Florida using muddy swamp-water full of frogs for the cold end, and the machine handled the frogs smoothly without ever getting choked up. The whole deal looked very good, and we paid our three months' rent with the option to purchase, and handed the machine over to Greg Linteris, one of our undergraduates, to measure its performance carefully. A few weeks later Greg Linteris reported his results. Unfortunately it turned out that the designer of the machine did not understand three-phase circuitry. All his numbers for electric power were too high by a factor of the square root of three. He was better at handling frogs than at handling complex numbers. The actual efficiency of the machine was six per cent and not ten. So we sadly shipped it back to Florida. Round two went to the Department of Energy.

The Prudential Insurance Company has never been enthusiastic about heat-engines. They are happy to buy their electricity from Public Service Electric and Gas, and have no wish to get into the utility business themselves. So it was easy to agree with them to go ahead with the solar heating and cooling experiments and drop the
heat-engine for the time being. If all goes well with the heating and cooling, we can come back to heat-engines later.

That is the story of the Princeton Ice-Pond. I told you the story because it illustrates what I have in mind when I ask for a technology with a quick response. I am not claiming that solar ponds by themselves will solve the energy problem. Still less am I claiming that the little game we are playing in Princeton has demonstrated the existence of an economically viable solar pond technology. What I do claim is that solar ponds are an example of a technology free from the rigidities and the decade-long delays which have made both fission and fusion power unable to respond to urgent need. Solar ponds may or may not turn out to be cheap and effective. If they fail, they will fail quickly and we shall not have spent half a lifetime proving them useless. If they succeed, there is a chance that they could be deployed rapidly on a very large scale. Sites could be surveyed, holes in the ground dug, and plumbing fixtures installed by thousands of local contractors responding to local demand. Plastic liners and pipes and solar collectors could be mass-produced in factories. A whole new industry could grow up in a few years like the industries of World War II. All this is only a dream, or at best a remote possibility. But there is no reason why a new technology has to develop like fission and fusion on a thirty-year time-scale. All it needs in order to go fast is small size of units, simple design, mass production, and a big market. When I go out to the Forrestal Campus and see those two machines, the $300 million TFTR and our little ice-pond, what I see in my mind’s eye is a dinosaur and an early primate. I wonder how long it will be before the next asteroid falls.
V. Genetic Engineering

Finally I have to say something about genetic engineering. I left only a short time for this because I know so little about it. I will speak only in generalities because I am ignorant of details.

When I compare the biological world with the world of mechanical industry, I am impressed by the enormous superiority of biological processes in speed, economy and flexibility. A skunk dies in a forest; within a few days an army of ants and beetles and bacteria goes to work, and after a few weeks barely a bone remains. An automobile dies and is taken to a junk-yard; after ten years it is still there. Consider anything that our industrial machines can do, whether it is mining, chemical refining, material processing, building or scavenging; biological processes in the natural world do the same thing more efficiently, more quietly and usually more quickly. That, it seems to me, is the fundamental reason why genetic engineering must in the long run be beneficial and also profitable. It offers us the chance to imitate nature’s speed and flexibility in our industrial operations.

It is difficult to speak of specific examples of things genetic engineering may do for us in the long run. Specific examples always sound too much like stories out of Astounding Science Fiction magazine. Fully aware of this danger, I mention three long-range possibilities which you may or may not take seriously. First, the energy tree, programmed to convert the products of photosynthesis into conveniently harvested liquid fuels instead of into cellulose. Second, the mining worm, a creature like an earthworm, programmed to dig into any kind of clay or metalliferous ore and bring to the surface the desired chemical constituent in purified form. Third, the scavenger turtle with diamond-tipped teeth, a creature programmed to deal in a similar fashion with human refuse and derelict automobiles. None of these creatures performs a task essentially more difficult than the task of the honey-bee with which we are all familiar. But it is a sound instinct which leads us to be distrustful of such specific and grandiose ideas. If we pursue long-range objectives of this kind, we are likely to find ourselves involved in a twenty-year development program with all the inertia and in-
flexibility of a nuclear power program. The whole advantage of biological technology will be lost if we let it become rigid and slow. So I hope we shall make our entry into genetic engineering in a thoroughly pragmatic and opportunistic fashion, choosing projects which lead quickly to short-range objectives, choosing processes which fit conveniently into the framework of existing chemical and pharmaceutical industries. That is the way we went into nuclear engineering with the TRIGA reactor in 1956. And that is the way we should have continued in nuclear engineering if we had been a little wiser.

Above all, we should try to exploit the small scale and fine tuning of biological processes to achieve production facilities which are rapidly responsive to changing needs. Never sacrifice economies of speed to achieve economies of scale. And never let ourselves get stuck with facilities which take ten years to turn on or off. If we follow these simple rules, I believe there is a good chance we will help genetic engineering to fulfill the promise of a cleaner and more liveable world for mankind, the promise which nuclear energy once made but was never able to fulfill.

A last brief word about bombs and genetic dangers. In my discussion of nuclear energy, I spoke only about reactors and not at all about bombs. In my opinion, the biggest mistake by far of the nuclear scientists was their enthusiastic pursuit of bombs. I cannot consider Three Mile Island to be an event in any way comparable with Hiroshima. The question then has to be faced, whether the pursuit of genetic engineering might expose us to dangers comparable with the dangers of nuclear weapons and nuclear war. I have thought a lot about this question and I believe the answer is no, with one essential proviso. The proviso is that the existing laws restricting experimentation on human subjects continue to be enforced. In other words, genetic engineering must stop short of monkeying around irresponsibly with the species Homo Sapiens. So long as Homo Sapiens is left out of it, I do not see how genetic engineering can lead to military abuses significantly worse than the old-fashioned chemical and biological weapons which our government has now wisely abandoned. And I do not see how genetic engineering can lead to accidental disasters in any way comparable with nuclear explosions. But do not take my word for it. Expect the unexpected.
Keep a careful look-out for dangers ahead, and when something on
the horizon looks bad, call a halt, blow the whistle, and try to find
a different way to go. If the worst comes to the worst, there are
other ways to make a living.

I do not think that any of the theoretically possible dangers of
genetic engineering will turn out to be real. I think that the bene-
fits of it will be large and important. So I wish luck and joy to the
young scientists who are now beginning their careers as genetic
engineers. The best luck that I can wish them is to have as much
fun with genetic engineering as we had with the TRIGA reactor
and with the Princeton Ice-Pond. Developing a new technology is
a lot of hard work, but it is also a lot of fun. If they are lucky,
they will find, as Joseph Tykociner found seventy years ago, that
invention is just as creative and just as exciting a way of life as
scientific discovery. When they grow old they may find, as Joseph
Tykociner found, that the life of an inventor also provides ample
room for philosophical reflection and for active concern with the
great problems of human destiny.