Another notable difference between H.R. 3 and the Democrats' bill is a provision in the latter bill that would require mine operators in the East to remain responsible for rehabilitated land for at least 5 years, and in the West for at least 10 years.

The Democrats' bill, and to some extent the Senate bill, have benefited from an unusual liaison with the National Academy of Sciences' study panel on arid land rehabilitation (see page 525). At the panel's request, NAS staff officers have briefed House and Senate staff on the panel's deliberations, a rare departure from the secrecy that usually surrounds NAS studies. The usefulness of this liaison has been limited by the fact that congressional staff have not been allowed to read the report (due to be released this month), but its main public policy implications were spelled out in a 23 July letter to Representative Morris Udall (D–Ariz.), chairman of the Interior subcommittee on environment.

The letter, from Ralph A. Llewellyn, an NAS staff officer, emphasizes in part that:

> We find that most state laws governing surface mining and rehabilitation in the West do not provide for adequate planning, monitoring, enforcing, and financing [of] rehabilitation . . . For these reasons it is apparent that a strong federal "floor" must be provided for regulating rehabilitation. . . . It is also very desirable to provide for substantial public participation in the entire rehabilitation program, from evaluation of pre-plans through the decision regarding success of the results.

In the letter, Llewellyn said the panel also felt strongly that areas of special historic, scenic, archeological, or biological interest should not be mined if the resulting damage could not be undone.

Backers of the Democrats' bill think their measure better reflects such concerns, and some members of the academy panel privately agree. The mining industry and Western utilities, however, seem to regard the bill as an unmitigated disaster. Using identical phrases in separate telegrams to the House Interior subcommittees, the American Mining Congress (which represents the mining industry) and the National Coal Association have declared the Democrats' bill "so unworkable" that it would "virtually prohibit surface mining of coal . . . at the very time the nation is looking to coal as a solution to the energy crisis."

One of the most detailed industry critiques of the bill comes from the Western Systems Coordinating Council (WSCC), representing 42 Western utilities. Among other things, the WSCC objected to provisions that might be used to bar mining from "historic, or fragile, or a natural resource area," and urged instead that such decisions be left to "long-term land use planning" by individual states. The WSCC also objected to a section of the Democrats' measure that would give Indian tribes the same rights and responsibilities as states in regulating mining on reservations, and requiring tribal consent for working both new and existing mines. The council complained that Indians might use this as leverage to make "exorbitant demands" on utilities. A similar provision was deleted from the Senate bill.

Oddly enough, the industry now seems to favor H.R. 3 as the starting point for legislation in the House, even though it fought the bill tooth and nail last year. It may be that industry senses an inevitability about strip-mining legislation, but the choice of H.R. 3 as the "markup" bill would clearly be to its advantage. Interior Committee Democrats would likely respond with a long series of amendments, and the ensuing delays could leave the 93rd Congress closing shop without a strip-mining law on the books.—ROBERT GILLETTE

A Conversation with Eugene Wigner

Eugene Wigner is one of the scientific emigrés who left Europe in the 1920's and 1930's and settled in the United States, providing a crucial leavening of American science. Wigner is now 70 and living in active retirement in Princeton.

His association with the university goes back more than 40 years, spanning an era in which physics and the world changed profoundly. Wigner has been a close observer of and, as a physicist, a major contributor to those changes. His generation reached maturity near the close of the heroic age of physics, dominated by figures such as Bohr and Einstein; Wigner himself played a leading role in establishing the foundations of nuclear physics. He became involved in a fateful scientific decision when, at the start of World War II, he joined with other scientists in conveying the information which persuaded Einstein to write the now famous letter to President Roosevelt which alerted him to the implications of German research in atomic physics. This started the train of events which resulted in the American atom bomb project. During the war, Wigner headed the theoretical physics section at the Metallurgical Laboratory of the University of Chicago, where the first chain reaction was achieved. At the end of the war, he was active in organizing atomic scientists in the successful lobbying effort for civilian rather than military control of atomic energy. After the war, Wigner served for a year as codirector of the Oak Ridge laboratories when serious work on power reactors was begun. He then returned to Princeton, where he continued the research for which he shared the Nobel Prize in 1963. Over the years, a great many of the principal honors and offices that government, universities, and professional societies bestow have come his way.

Wigner did not become a public figure in the way that his Hungarian-born colleagues Edward Teller and the late Leo Szilard did, but this does not mean that he has avoided controversy. Since World War II, American scientists who worked on military projects have tended to identify with one of two general attitudes toward nuclear policy, and especially toward relations with the Soviet Union. The two groups might be labeled conservatives and liberals. The former, generally, have felt that national security depended on the United States maintaining at least comparable nuclear strength, while the latter group put less stress on a strict nuclear balance and more on effective arms control agreements and political detente. Wigner has sided consistently with the conservatives and has expressed his views most vigorously in advocating a stronger civil defense program.

Wigner's critics suggest that his outlook is frozen in a Cold War mold. One typical exponent of the liberal view says that Wigner fits into the category of "Eastern European anticommunists who feel that Americans are naive about the Russians and we need to be protected from ourselves." According to this view, Wigner sees Soviet-American relations from the standpoint of "a refugee from a small, second-class power" eternally at the mercy of large and menacing neighbors. On civil defense issues Wigner's interpretations are held to be excessively gloomy by the liberals. His fault, they say, is a common one among university scientists who, no matter how brilliant, allow their convictions to diminish the rigor of their analysis when they venture into policy areas.

Some Princeton faculty members feel that, before his retirement 2 years ago, Wigner was treated unfairly by younger, militant faculty and students who attacked him as a hardliner on the Vietnam war. Wigner himself says that he was not conscious of hostility.

In the Princeton debates over American incursion into Laos, observers say that Wigner was not pleading the case as a hawk, but rather was arguing that scientists should continue to advise the government. In fact, the categories of the 1960's do not really fit Wigner. He seems to be a man of priorities.

In person, Wigner is a modest, courteous man, utterly lacking the hauteur of some grandees of science. This is notable in someone who seems entitled to a measure of arrogance, for Wigner's status among his peers as a leading scientific virtuoso of his time, combining brilliance in both theoretical and applied fields, is rivaled by only one or two other physicists. His vigor is attested by the fact that this year he will again serve as visiting professor at Amherst, Louisiana State, and the University of Massachusetts. In describing his manner, one must mention the invincible traces of the Hungarian accent which actually seem to enhance the precision of his speech. And it is tempting to use the cliché about old-world charm. Incidentally, Wigner seems to possess it without the underlying cynicism that often accompanies it. He is extraordinarily generous in his estimates of his colleagues and forebearing in discussing his critics, but this does not mean that he is any less adamant in adhering to his weltanschauung.

Something of the range and quality of Wigner's views are conveyed in the following, moderately edited version of a 2-hour conversation with this reporter in June at his home in Princeton.—JOHN WALSH

Q: You were one of a remarkable group of people born in Budapest who came to the United States in the 1930's, Szilard, von Neumann, and Teller. I wondered what the ingredients were in Hungary when you were a boy. I think you went to the same high school with . . . .

WIGNER: Johnny von Neumann and I did, the others did not, but Johnny von Neumann and I did. We had a wonderful teaching staff and, in particular, a mathematics teacher. He was fantastic. He gave private lessons to von Neumann, he gave me books to read, he discussed them with me, and he took an active interest in his students. He founded a high school mathematical society. I did not realize the wide scope of his influence at that time. And he inspired us.

Q: Did he advise you on where to go from your lycée, your high school?

WIGNER: No, he did not. I went first to the Institute of Technology in Budapest and then to the Institute of Technology in Berlin. I was educated as a chemical engineer. In many cases, that came in very handy. Altogether, as I often say, it is very good if one knows something which not every one of one's colleagues knows because it increases the total knowledge and understanding of your group. This was particularly true when I worked on the so-called uranium project. Of course it was largely an engineering project, but you also had to know nuclear physics. And I was very lucky that I was educated as an engineer but could work as a physicist.

Q: You were in Berlin in the 1920's?

WIGNER: I think I finished in 1924 and then went to Hungary as a chemical engineer.

Q: And had you, when you were in Berlin, met some of the people who later on were in nuclear physics, like Heisenberg?

WIGNER: Heisenberg I only met superficially. I did know Einstein. He gave a seminar on statistical mechanics. And it was a splendid seminar. He could explain things in a visualizable way so that you understood what happened apart from the mathematics, apart from the formal mathematics. Nuclear physics did not exist in those days. But statistical mechanics existed, and I was very much interested in the rate of chemical reactions. That was my doctoral thesis. This, of course, was not entirely an accident: I worked with Polanyi. He is a person with a wonderful mind, a deep insight, and an understanding of more areas than anybody else I know. He started as a physician. He became a physical chemist, then an economist, and now he is a philosopher. And as a philosopher he is even more recognized than in the other fields.

Q: So then in the middle 1920's you returned. . . .

WIGNER: To Budapest, as a chemical engineer. But I subscribed to the *Zeitschrift für Physik* [and read] the articles by Born, Jordan, and Heisenberg. Thus, when I got an offer from Berlin to return as a physicist, I couldn't resist it.

Q: Was this from one of the Kaiser Wilhelm institutes?

WIGNER: The offer came from the Kaiser Wilhelm Institute, from a physicist, Weissenberg, who was interested in x-ray spectroscopy, x-ray diffraction, and crystallography. He did very fine work. He gave me a problem which I found easy to solve, but then he said I should make the solution more elegant. And this led me to group theory. There is a German proverb, *Der Dumme hat Glück* ("The stupid one has luck"). And this applied in this case, because soon enough I noticed how important group theory can be in quantum mechanics. Johnny von Neumann helped me very much in finding the proper area of that theory.

Q: Where was he at that time?

WIGNER: He was in Göttingen, but he came to Berlin and I told him I had a group theoretical problem. He said to read the article by Frobenius and Schur.

Q: So this would have been in the late 1920's, and the atmosphere there was intellectually a very lively one?

WIGNER: Yes, by this time I was an assistant at the Technische Hochschule in physics. Richard Becker was my boss.

Q: How would you describe the evolution of something called nuclear physics? What were the events, especially those you were more or less involved in?

WIGNER: The great discovery was the discovery of the neutron, and it

was an entirely experimental discovery. It was Chadwick's, I think. And then Heisenberg wrote a paper. As often with Heisenberg's papers, the details were superseded, but the basic idea to go ahead and look at nuclei as composed of protons and neutrons was sound. I was at that time most interested in solid-state physics. And I had the most wonderful collaborators —it was at Princeton of course. My first graduate student was Fred Seitz. The second one was John Bardeen. The third one was Conyers Herring. I have as much admiration for Herring as for Seitz and Bardeen. They were all fantastic. Herring is not so intent on making discoveries as on understanding and establishing coherence.

Q: Where is he now?

WIGNER: At the Bell Telephone Laboratories.

Q: Let me ask you how the change came from Berlin to Princeton.

WIGNER: One day I received a cable saying, "We invite you for half a year," and it gave a salary which then seemed completely fantastic. It was about seven times my salary in Berlin. I had just finished a book and received a fantastic compensation for it—$500. But the salary in Princeton for 1 month was $600.

Q: Who was instrumental in bringing you here?

WIGNER: I was invited on the advice of Paul Ehrenfest—his name means "strong in honor." He was in Leiden but traveled around a great deal, and he advised Ann Arbor to invite four physicists from Germany and advised Princeton to invite two, namely John von Neumann and me. He knew that we were close friends. He knew that if somebody comes to a new place he feels at times lonesome, and it is a very good thing if he has some old friend around to talk to.

Q: It was a sort of natural move for you then. It was not the atmosphere in Germany. It was before the Nazi takeover in Germany?

WIGNER: Yes, but the invitation was for half a year. I spent half a year in Germany and half a year in Princeton. This also was a very wise arrangement because we maintained contact, both Johnny von Neumann and I, with Berlin. Berlin was a very important center of learning. Schrödinger was there, Einstein was there, von Laue also.

Q: How long did that arrangement last?

WIGNER: About 4 years. It was discontinued because of the Nazis.

Q: So you saw the atmosphere change in Berlin, people began to flee —even in the early days it affected the lives of scientists?

WIGNER: Yes. Very much. As you know, Polanyi left, Schrödinger left. Polanyi had Jewish ancestry—he had to leave. Schrödinger didn't, but he was disgusted.

Q: Had you encountered Szilard at that time?

WIGNER: I met Szilard in Berlin in my student days. He didn't know then what he wanted to do. We first got acquainted when he came to the analytical chemistry laboratory where I had to work for my degree.

Q: So he too had not chosen his field.

WIGNER: No, but he was closer to physics. I think he was more determined to become a physicist eventually.

Q: Was Teller in Berlin at that time?

WIGNER: No, Teller was first in Karlsruhe, and, I think, in Leipzig.

Q: So you knew each other?

WIGNER: We knew each other, but not closely.

Q: Von Neumann came here to Princeton and worked at the university at the same time you did?

WIGNER: First at the university, then when the Institute for Advanced Study was founded, he went to work at the institute.

Q: What kind of a place was Princeton in the 1930's—as a university and as a scientific community?

WIGNER: It was a little behind the times. Quantum mechanics was known to very few. During our time here, first Robertson and then Condon were added to the department, and they were interested. But not as intensely as Johnny von Neumann or I.

Who's Who in the Interview

Here are brief biographical or historical identifications of some of the persons named in this interview in order of mention.

John von Neumann (1903–1957), Hungarian-born mathematician and mathematical physicist who developed game theory and its application to several sciences, particularly economics, and made contributions to other branches of mathematics, logic, and computer theory and design. . . . **Leo Szilard** (1898–1964), Hungarian-born physicist who participated in key discoveries proving the possibility of nuclear fission and controlled chain reaction. Active in World War II atom bomb project. Pacifist and founder of Council for a Livable World. . . . **Edward Teller** (1908–), Hungarian-born physicist, active in research in quantum theory and nuclear physics. Active in World War II atom bomb project. His early studies of thermonuclear reactions led to the development of the hydrogen bomb. Director of Lawrence Livermore lab (1958–1960). . . . **Werner Heisenberg** (1901–), German physicist noted for work on quantum theory, quantum mechanics, and basic formulations for nuclear physics. . . . **Michael Polanyi** (1891–), physical chemist and polymath who taught in Germany, Britain, and the United States and whose writings range across physics, economics, and philosophy. . . . **Frederick Seitz** (1911–), American physicist, contributed to theory of solids and nuclear physics. President of the National Academy of Sciences (1962–1969) and president of Rockefeller University. . . . **John Bardeen** (1908–), American physicist who has made major contributions to solid-state and low-temperature physics. Codeveloper of the transistor and twice Nobel Prize laureate in physics, sharing the prize in 1964 and again in 1972. . . . **Arthur H. Compton** (1892–1962), American physicist, expert in x-rays and cosmic rays, who was director of the Metallurgical Laboratory at Chicago during World War II and key administrator in the atomic bomb project. . . . **Alvin M. Weinberg** (1915–), American physicist, authority on reactor theory and design, worked at the Metallurgical Laboratory and, since 1955, has been director of Oak Ridge National Laboratory. . . . **Béla Kun** (1885–1939), Hungarian Communist who led uprising in Budapest in 1919 and became premier of the short-lived, Communist-dominated government. . . . **Nicholas Horthy** (1868–1957), Hungarian admiral who organized a successful counterrevolution against the Kun regime in 1919, and ruled Hungary as head of a generally reactionary regime until 1944.

Q: Were there ties with Berkeley, and with Columbia and Chicago, in those years?

WIGNER: Very superficial. Occasionally I went to Columbia to discuss matters with [I.I.] Rabi and with Breit. Gregory Breit, whom I admire very much, was at New York University. Rabi was at Columbia.

Q: But they were your colleagues, those who were interested in the same sort of things.

WIGNER: Breit and I became closely acquainted. He spent a year in Princeton at the institute, and we wrote a couple of papers together. One of them was on what many people call the Breit-Wigner formula.

Q: Well, I know that you and others were engaged in the 1930's in working out the theory that carried nuclear physics through the war.

WIGNER: We contributed to it. Heisenberg was the one who started it.

Q: Were you concentrating in the 1930's, or were you interested in a number of things?

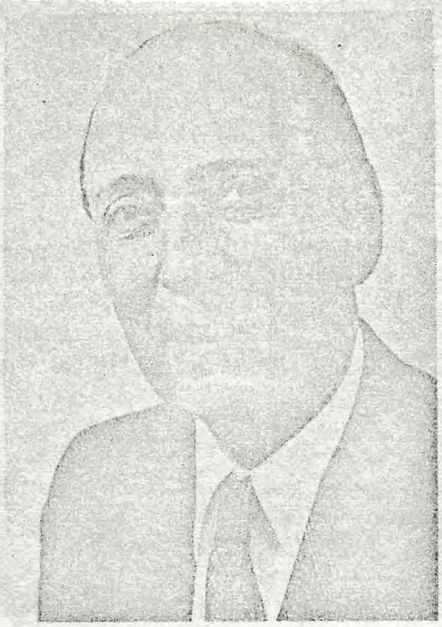
WIGNER: I was most interested in nuclear physics. I wrote a few articles on solid-state physics, but my principal preoccupation became nuclear physics. Again, something came in very importantly. Milton White investigated at Berkeley the proton-proton interaction and found it to be virtually equal to the proton-neutron interaction. This created a very important concept: the isotopic spin. It's not a good name—it should be called isobaric spin. This concept assumed, eventually, great importance. But then, of course, the threat of war grew, and those of us who came from Europe realized how important it was to maintain freedom in at least part of the world. I often fear that most of our colleagues are just as blind now as were our colleagues in the late 1930's—they do not realize the threat to freedom.

Q: Well, certainly the situation of scholars and scientists in the Soviet Union is difficult—there are some parallels, it seems.

WIGNER: Yes, there are some parallels. In fact, I don't need to tell you that Hitler was not somebody I liked—but he did permit emigration. I mentioned that Schrödinger and Polanyi left Germany, and so did many others. The U.S.S.R. does not permit emigration.

Q: Have you ever reflected on why you stayed in Princeton, except for the wartime period?

WIGNER: For 2 years I was at the University of Wisconsin. I must say



Eugene Wigner

that my love of this country originates from the Wisconsin time. I found there the real America, people knowing how to grow potatoes, people knowing the simple life. People not in some clouds up in the sky. And academic people understanding other people not in academic circles. That had an enormous effect on me.

Q: Was that in the late 1930's?

WIGNER: It was in the late 1930's. I think I left for Wisconsin in 1935 and stayed there until 1937. I came back to Princeton.

Q: Has Princeton changed?

WIGNER: Enormously. It is much more connected with the rest of the world than it was at that time. Earlier, it was entirely an ivory tower. It is much less of an ivory tower now. It is not perfect. If you look at the voting record of Princeton, you realize that it differs very significantly from the voting record of the rest of the country.

Q: I suppose that the town and the area have changed terrifically. This was a rather pastoral place when you came here, wasn't it?

WIGNER. And now there are lots of research institutes, semifactories like RCA, Mathematica, and dozens of others. It sort of gives you prestige to have an institute in Princeton.

Q: Have you observed any advantage in having industrial scientists in the area?

WIGNER: When I gave solid-state courses I always had a few people from RCA, but the contact has not been very intense. I don't know how it is in chemistry. The chemists are more industrial minded.

Q: Now could we go back to the period of the war. I've read accounts of the initiative that you and Szilard took in approaching Einstein to write to President Roosevelt, and these accounts indicate that it was the supply of uranium from the Congo that worried you. Is that accurate?

WIGNER: More or less. What worried us most was that Germany might be engaging in uranium research and that the Western powers, the democracies, should then also engage in such research. And Einstein understood it in half a minute. It was really uncanny how he dicated a letter in German with enormous readiness. It is not easy to formulate and phrase things at once in a printable manner. He did. I translated that into English. Szilard and Teller went out, and Einstein signed it. Alexander Sachs took it to Washington. This helped greatly in initiating the uranium project.

Q: Was Szilard at this time the initiator? He was obviously an activist all his life.

WIGNER: In Chicago, they called him "the general" because he told everybody what to do. He meant it well, of course.

Q: And this was characteristic right from the beginning with Szilard?

WIGNER: The idea to approach Einstein and altogether to approach the government originally went against Szilard. He felt that if the government had a hand in the uranium research it would be terribly bureaucratized, and nothing would come out of it. In fact, there was a certain amount of bureaucracy, but without the government help it could not have gotten anywhere.

Q: It really took two stages to move the thing, didn't it?

WIGNER: The next step was a meeting at the Bureau of Standards—a committee meeting at which an Army colonel and a Navy commander participated. And I often tell the story that the colonel said, "Oh gentlemen, this is unnecessary. Wars are won not by weapons, but by the morale of the civilian population." And I did not realize at that time how right he was. Now I do. I replied that if that is so, we should disband the Army and spread that wonderful morale in the civilian population. Then the Navy commander said, "How much money did you say you need?" And we got the money. An anecdote which is both true and perhaps amusing. However, the Army colonel was right. The morale of the civilian population is decisive. But we did not understand it. Never-

theless, it was good to have had the support of the government in the weapons development. Altogether we know that, without radar and without many other developments, the story of the war would have been entirely different.

Q: How were you drawn into the actual mobilization?

WIGNER: Fermi did the principal work at Columbia, and we were in close contact with each other. I decided we should investigate one of the factors, the so-called resonance absorption. Robert R. Wilson, now the head of the National Accelerator Laboratory, and Ed Creutz, who is now associate director of the National Science Foundation, were two experimental physicists whom I persuaded to work on this. They worked together beautifully, with a skill and understanding that I admire. We had constant conferences with Fermi, and one day, just the day after Pearl Harbor, A. H. Compton turned up in Princeton and said, "We want to organize a laboratory."

Q: Literally the day after Pearl Harbor?

WIGNER: Literally the day after Pearl Harbor.

Q: And so you wound up in Chicago?

WIGNER: We moved to Chicago as late as April of 1942, and I was in charge of the theoretical physics section. And again I had unbelievable luck with the collaborators—who were recruited by Compton. One of my collaborators was A. M. Weinberg; the other one, who was technically equally able and farsighted, was Gale Young. I was enormously lucky. We designed the so-called Hanford reactor. We were firmly convinced right from the beginning that the chain reaction would go, and the problem was to get it to go on a scale that would produce significant amounts of plutonium.

Q: Legend has it that you were standing right on top of it when the first chain reaction occurred under the Stagg Field stands, and you handed Fermi a bottle of Chianti. Is that true?

WIGNER: I did not stand on top of the chain reactor, but I did have the Chianti. I always boast that it was more farsighted to know early in 1942 that Chianti would give out than to know that the chain reaction would be established by Fermi. I bought the Chianti in Princeton before moving to Chicago. I had been through another war and I knew such things give out.

Q: So you were sure two things would happen. The design of the Hanford reactor really looked beyond the war to the creation of the power reactor for nuclear energy, didn't it?

WIGNER: This is a difficult question. We wanted to produce plutonium and we wanted to produce it as easily as possible. As an engineer I knew that it wasn't so easy to put something into production: this requires overcoming 55 little things. The engineers in Chicago wanted to construct a helium-cooled reactor, and I felt—all the theoretical physicists felt—that it was too difficult an engineering job, and it indeed turned out that way. It has turned out that the helium-cooled power reactor, even at present, doesn't seem a very attractive engineering job —even now when we have much more experience.

Q: There is a general question that has interested me. Very often when scientists talk about the Manhattan project they say that the lucky thing was that you had a group of physicists with strong theoretical backgrounds who were able to come in and act as engineers.

WIGNER: We even knew about corrosion.

Q: Right, but here you were, head of the theoretical physics division, yet with an engineering background.

WIGNER: That was very lucky.

Q: Yes, but was that typical or not typical?

WIGNER: Not typical.

Q: Fermi for instance was a physicist pure and simple, and Oppenheimer was of course a physicist. And so you were really an anomaly in this group?

WIGNER: Well at that time I was a physicist. But I remembered engineering. I knew, for instance, that corrosion can become a very important problem. I knew how to calculate water flow. I remember how the DuPont engineers were surprised that we could calculate the water flow through the tubes.

Q: Were you handicapped in that period by the absence of computers?

WIGNER: No, the calculation of a multiplication constant I still do in longhand. You can overestimate the usefulness of computers, in many areas.

Q: How about your transfer from Chicago? Did there come a point when the group at the Metallurgical Lab felt they had done their job and began to disperse?

WIGNER: We did not disperse, and that was Compton's desire. Compton's and Charles Smyth's. They felt it was important for us to keep together and review the detailed DuPont plans, and in the meantime they told us to work on power reactors. There was sort of a trick to keep us together, and a successful trick. When the DuPont drawings came, it was very important—with all respect to DuPont—that we review them, because American chemical engineers had a much more practical training than German chemical engineers, and as a result they were much less familiar with atomic theory. Fermi started every one of his speeches to the engineers with, "The neutron is a tiny particle." It sounds ridiculous now.

Q: What were the circumstances of your moving to Oak Ridge?

WIGNER: It was essentially [because of] Charlie Thomas of the Monsanto Company. He told me that it was very important to have a good strong leadership for the uranium power development. But Oak Ridge at that time was so terribly bureaucratized that I am sorry to say I could not stand it, and we came back to Princeton after a year. The person who took over was Alvin Weinberg, and he slowly, slowly improved things. I would not have had the patience, and I admire him very much.

Q: You certainly knew as much as anyone at that time about reactor design and about reactor theory.

WIGNER: Well, we knew about equally as much, Alvin Weinberg, Gale Young, and I.

Q: And from then on did you maintain. . . .

WIGNER: A very, very close interest. I wrote several papers, I gave a great deal of advice, I went to visit Oak Ridge frequently, and I did not wash my hands of it.

Q: Have you maintained an interest throughout the evolution of the American reactors and the discussion of the breeder, and so forth? Have you tried to keep up, or did your interests diverge in the 1950's?

WIGNER: I have maintained an interest, but the matter has become so detailed that almost nobody can know the whole thing. I know much less about it than many other people, including Alvin Weinberg, and I could mention many others who are devoting 80 percent of their attention to it. Perhaps I devote 10 percent.

Q: After Oak Ridge and the return to Princeton, how did your own work evolve?

WIGNER: I remained interested in nuclear physics very strongly. My work which I like most was done at that time—most people don't like that part of my work. But I like it, and I also worked a good deal on old subjects

like symmetry. I also became interested in what is called the epistemology of quantum mechanics. It is a weakness of old people that their interests spread out, and they know, as the Germans say, "nothing about everything." You know that saying, "A full professor knows nothing about everything, an associate professor knows something about a little, and an assistant professor knows it better." You see I am still partly Hungarian, and I love jokes.

Q: I wondered, was your family a German family?

WIGNER: Hungarian. My grandfather was Austrian more than Hungarian, but he lived in Hungary.

Q: Well, you were there in the last days of the empire, which must have had a special flavor.

WIGNER: Yes, it was a very confusing time.

Q: And were you there during the war?

WIGNER: I was too young to be drafted, but I was there during the Communist regime.

Q: Béla Kun?

WIGNER: Béla Kun, as you say. Which was not as bad as it could have been.

Q: But not very good?

WIGNER: But not very good either. But the period after that was a bitter reaction. It was also bad.

Q: Were you there when Horthy was . . . ?

WIGNER: Yes.

Q: So you saw both kinds of dictatorship?

WIGNER: Yes, both kinds of dictatorship, and I like neither. And that is why I emigrated to Germany.

Q: How do you account for the Hungarians' remarkable record? Hungarians are not very numerous, it's a small country.

WIGNER: Many Hungarians were forced to emigrate, and emigration is in many ways very stimulating. Most of my classmates remained in Hungary, and there you are part of society. In a foreign country you have to excel. And this was very stimulating for us. Johnny von Neumann was different. He was a genius. He was brighter than anybody I met.

Q: And broader?

WIGNER: And probably broader. He was interested principally in mathematics. He worked in theoretical physics. He worked in economics, and he knew large parts of history as well as a professional historian. He spoke well Hungarian, German, English, also French and Italian, and he could read Latin and Greek. There are people who speak more languages than that, but he was a mathematician, not a linguist.

Q: So you think it is partly the stimulus of exile. Have you been back to Budapest?

WIGNER: Not since the war. I am afraid of what I could see.

Q: Have you been back to Berlin?

WIGNER: Yes.

Q: And have you been able to reestablish contact?

WIGNER: Yes.

Q: If I might, I'd like to pursue the matter of your relationship with the government. Obviously the whole relationship changed radically during the war and continued to change. After the war and the Manhattan project experience, what kinds of contact have you had with the government over the years?

WIGNER: I know the science establishment reasonably well. I also have reasonably close contact, in fact very close contact, with the civil defense office. I am very much interested in civil defense. I fear that our neglect of civil defense may become a true disaster.

Q: So your opinion of the early 1960's you still hold?

WIGNER: I still hold.

Q: Certainly civil defense is a subject not even seriously discussed now.

WIGNER: It is not a popular subject.

Q: But you've maintained an interest in it.

WIGNER: Yes, I go to Oak Ridge every month for a week to work on it. There's a group there working on it. I hope effectively, technically effectively.

Q: I remember that you spent a year there in 1963–64 or 1964–65.

WIGNER: I think it was 1963–64.

Q: And your group had produced a detailed proposal?

WIGNER: Yes, but we had no luck in having it implemented. I could tell long stories on that, it's very unfortunate. Other powers are now much stronger in missiles, and, if they evacuate their cities, then the total fatalities which we can cause are about 7½ million, which is ten times less than they can cause us. Of course, it is unpleasant to speak about this subject, but it may be necessary. I am pleased to see that you don't share the feelings of so many of your colleagues who are opposed to the defense of our country. It often exasperates me when I read articles in *Science* that are completely irrational and opposed to any defense measure.

Q: I suppose its generational. I suppose I share a skepticism about the intentions of others.

WIGNER: You just have to read the Russian newspapers. The strategic balance has changed. Draw the consequences.

Q: I wonder if this is a temperamental or experiential attitude. You're right that my younger colleagues tend to think the war is over, or that war is over, and feel that the Chinese and the Russians are not a serious threat. I'd like to believe it, very much, but I find it difficult to. Do you think simply that the fact that you grew up in a society that was under stress and that finally you went through the shaking experience of the war and revolution . . . .

WIGNER: Do you know the history of Hungary after the war?

Q: Not really.

WIGNER: The Yalta agreements said free elections, free government. Free elections were held, the Communists had 5 percent of the vote, the Social Democrats about 14 percent. They were called in by the Communist occupation governor and told, "Unite." Then they had about 10 percent of the deputies in the Parliament. The Small Holder party—the peasants' party—had the majority, about 60 percent as I remember. They formed the government. One day the prime minister left the country to negotiate some loans in Switzerland. During his absence, the secretary of the Small Holder party was arrested. Imre Kovacs was his name. After a week of arrest they produced a confession that he wanted to overthrow the government. After another week they even produced his body. And they installed a Communist government. They offered the prime minister who was still in Switzerland the privilege of permitting his wife and children to join him if he did not return to the country. He accepted that. This is how the Communist government was installed.

Q: You feel that there hasn't been an evolution. I suppose this is part of the explanation of the way Professor Teller feels as well. His experience is much the same as yours.

WIGNER: The same as mine. And most people who have seen this have a similar feeling. The Czechs who came, and the Germans, understand it better. A world under one dictatorship will not have science either. The Russians now support science vigorously because it helps militarily, but

once it is over, scientists become a nuisance. They come up constantly with new ideas. They say Marx talked a great deal of nonsense. A world in which there are several governments, each devoted to the happiness of its own people and with free emigration so that there should be a real competition for the goodwill of the people, that is an attractive world.

Q: This is an important issue. One has to look at Szilard, who went through a similar experience and yet whose attitudes seem to have been, not diametrically opposed, but quite different. How do you account for it?

WIGNER: I heard the explanation that he had the same desires which many of our semiscientists have—namely, to have a political influence and, of course, in a democracy he could not have it. Among people he helped to assume power, he expected much more.

Q: Von Neumann seems to have agreed with you. Teller's analysis seems to have been very much like yours, so Szilard is very much the exception in this group.

WIGNER: He was "the general." Well, we all have our weaknesses.

Q: Do you find an awareness among your colleagues here in Princeton—a political awareness? Or do you find them simply not wanting to think about such things?

WIGNER: Most of them say, "Oh, I am a scientist, leave me alone." Maybe also some are semiscientists and interested in influence and power. Most of them, I believe, think exactly as I do, even if they are not willing to go to bat. I am willing to go to bat and be unpopular, very unpopular.

Q: Do you think that's true?

WIGNER: Not scientifically. I have many more invitations than I can accept. But politically I am unpopular.

Q: Have you, as you've talked to people you've known for a long time, found that the plateau of federal funding in science has damaged the enterprise? Those things that you're really interested in seeing done, do you think they're not being done?

WIGNER: It's difficult to tell. The spirit of science has changed. I don't know whether you've heard about that song which was composed soon after the war, "Take back your billion dollars."*

Q: Was it a parody by scientists?

WIGNER: It expressed a desire by scientists to preserve the old monastic spirit. I forget who wrote that song. I am very poor at singing, otherwise I would sing it for you. And of course the spirit of science has changed significantly. It is true that some scientists owe their most important scientific accomplishments to the fact that they had expensive equipment with which they could get results which could not be obtained otherwise. This was wonderful for science, but the monastic spirit of science had an attraction for those of us who chose science as a monastic occupation. We did not want influence. We also realized we would be thought to be a little crazy. We were not interested in power.

Q: What you've said indicates that there's not a conflict but a tension between, on the one hand, remaining true to the image of monastic science as you've described it, and, on the other hand, the responsibility to point out, for instance in your view of civil defense, that there are shortcomings in American policy. It must be very hard to reconcile.

WIGNER: Yes, I am trying to contribute to a journal which advocates civil defense. And, of course, I work on it as I mentioned.

Q: Have you retained any advisory jobs with either the Defense Department or anybody else?

WIGNER: I don't now have any advisory jobs. I am too old for that.

Q: How about your interests outside physics or outside science? Are those minerals over there?

WIGNER: Well, I was a chemical engineer and learned mineralogy. My daughter, when she wants to give me a present, knows a store where you can buy pretty minerals. And she does give me presents. And they are pretty. My daughter is a very nice girl, as most daughters are.

Q: Right. I have one myself.

WIGNER: How old is she?

Q: Fourteen.

WIGNER: Oh, she will still further improve in time.

Q: Do you read German or Hungarian now?

WIGNER: I read Hungarian poetry. Hungarian poetry, in my opinion, is as good as any poetry. I occasionally read German. But, of course, physics is very difficult to keep up with. It takes an enormous amount of time.

Q: The last thing I'd like to talk about is the enterprise of physics. Do you think that simply the numbers of people who have entered the field and the way physics has developed . . . has it gotten so ramified that there are too many kinds of physics with too many people in it?

WIGNER: I knew physics when I came to Princeton. I don't know physics anymore. It's too large.

Q: And you try to keep up?

WIGNER: Yes, rather to keep up than to write many papers.

Q: Would you say that physics and physicists have changed very much?

WIGNER: The relation of teachers to students hasn't changed very much. The attitude in physics is different. People become much more specialized. Last week I read *Physical Review Abstracts*, and every other abstract I could not understand. Perhaps it is partly the jargon—the technical expressions. It hurts me, and I'm afraid it will hurt physics.

Q: Has the development of civilian uses of atomic energy gone about as you expected?

WIGNER: I thought that civilian atomic power would become economical much before it did.

Q: What do you think caused the delay?

WIGNER: The collaboration of physicists and engineers may not have been as close as it was earlier. And the problems may have been more difficult than we realized. Do not forget that conventional power production also developed; it became less expensive. As far as the problems are concerned, I am convinced they are soluble. Remember the agitation caused by the railroads. They thought people would go mad. Reactors can be made to work with reasonable safety. It is generally forgotten that fossil power also has its dangers—notably, it puts sulfur dioxide into the air. It is so easy to say this or that project is dangerous. Life is dangerous. This does not mean, of course, that we shouldn't make every effort to minimize the dangers.

* Take away your billion dollars,
Take away your tainted gold,
You can keep your damn ten billion volts,
My soul will not be sold.
Take away your army generals; their kiss is death, I'm sure.
Everything I build is mine, and every volt I make is pure.

* From "Take Away Your Billion Dollars," written in 1946, music and lyrics by physicist Arthur Roberts, who worked during World War II at the M.I.T. Radiation Laboratory and is now at the National Accelerator Laboratory, Batavia, Illinois.