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FROM TECHNOLOGICAL FIXER TO THINK-TANKER[1]

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INTRODUCTION

I started my scientific career at the University of Chicago, first as a theoretical band spectroscopist concerned with the infrared absorption of CO_2 (the same absorption that causes the greenhouse effect), and then for some half-dozen years as a mathematical biophysicist. Had it not been for World War II, I would probably have spent these 60 years as a neurophysiologist rather than as a philosophically minded nuclear engineer and administrator. I certainly

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would not now be explaining how I got the reputation of being King of the Technological Fixers (as the environmental activist Paul Ehrlich once put it).

Because I was at the University of Chicago and knew some physics and mathematics, I was pressed, in the summer of 1941, into helping Prof. Carl Eckart analyze some experiments on diffusion of neutrons in beryllium that were being conducted by Prof. Sam Allison. At the time I knew nothing about neutrons, but as a biophysicist, I was familiar with the classical theory of diffusion. Eckart asked me to join him half-time for six months—after which time, presumably, a neutron chain reaction would be shown to be impossible, and I could contribute more seriously to the war effort.

Eckart left Chicago in January of 1942; he was replaced by Eugene Wigner—and thus began a friendship and collaboration that has lasted more than 50 years. Eugene, in his lovely autobiography, *The Recollections of Eugene P. Wigner* as told to Andrew Szanton (2), says of me, not that I was a great physicist (which I was not) but that "Weinberg beautifully understood human nature and the cause of human relations; he sized men up quickly and was pleasantly persuasive, a natural diplomat." And of course, I have on many occasions returned Eugene's gracious words, once describing him as perhaps the world's foremost physical scientist and engineer, with his complete mastery of physics and certain branches of mathematics and chemistry, combined with a great aptitude and liking for engineering.

The Metallurgical Laboratory (as the University of Chicago atomic energy project was called) was trying first to establish a nuclear chain reaction based on natural uranium and graphite, and then to design huge chain reactors and the necessary chemical reprocessing plant to produce plutonium. It must be hard for today's generation to realize how bizarrely unreal this goal appeared to someone like me who at the time knew almost nothing about nuclear matters. But I became a believer when, in spring of 1942, Enrico Fermi announced that the multiplication constant in a graphite-uranium "exponential pile" was greater than unity! Nuclear energy was real and had to be taken seriously.

The first chain reaction, in the West Stands Squash Court on December 2, 1942, was a sort of anticlimax. Wigner even talked of skipping the ceremony—but he finally did show up with his famous bottle of Chianti, which he presented to Fermi after the first chain reaction was shut down.

Eight months before Fermi's demonstration, Wigner and his little group of theorists and engineers were designing the Hanford plutonium-producing reactors. I was in charge of the lattice design—that is, figuring out the optimum spacing of the uranium rods. Since uranium was very scarce, the original Hanford reactors contained barely enough uranium to be critical—they were what we now would call "over-moderated." This meant that if the cooling water, which added to the moderation, were removed, the reactors would revert to a more chain-reacting configuration, and in fact might blow up. (This is what happened at Chernobyl.) Although Wigner realized this, he had no choice at the time since uranium was so scarce. Thus the Hanford reactors, as built by DuPont, were unstable against loss of cooling water and were shut down after the war.

Once the Hanford design had been completed in 1943, we had time in 1944 and 1945 at Chicago to think about the future of nuclear energy, and about the future of a world living in the shadow of nuclear weapons. We organized a "New Piles Committee," consisting of the giants—Fermi, Wigner, Leo Szilard, James Franck—and younger people such as Ed Creutz, Philip Morrison, Fred Seitz, Gale Young, and Alvin Weinberg. At these bull sessions, we discussed most of the ideas for future power-producing reactors—in particular the breeder—and the fluid fuel reactors. At the time, we were under the impression that uranium in nature was extremely scarce. Unless the breeder were developed, nuclear energy could not become an important energy source. This notion, which in today's circumstances would be regarded as incorrect, explains why the breeder has always been nuclear energy's holy grail.

It was at one of the meetings of the New Piles Committee that Fermi uttered his famous warning, which I paraphrase: "It is not certain that the public will accept an energy source that produces vast amounts of radioactivity as well as fissile material that might be diverted by terrorists." These words, spoken in Fermi's matter-of-fact manner, have remained vividly in my mind all these years.

We also had time to think about the future of a world with nuclear weapons. Even before Hiroshima, Arthur Compton, our leader, commissioned James Franck to prepare a statement regarding the use of the first bomb and to offer suggestions for the postwar control of nuclear energy. The result was the famous Franck report, which recommended that the bomb be demonstrated before being used on Japan (a position supported by a majority of scientists at Chicago), and that the world should organize some kind of international regime to control proliferation of nuclear weapons. Thus, the two issues that bedevil nuclear energy 50 years later, public acceptance of nuclear energy and proliferation, had been recognized already by the Chicago scientists.

Clinton Laboratories in Tennessee was founded in early 1943. It was a branch of the original Metallurgical Laboratory. As such it too was operated by the University of Chicago, with much of the actual operation being conducted by DuPont, the contractor for Hanford. Clinton was where humans first handled radioactivity on a huge scale—where we learned, under wartime stress, to operate a reactor (the X-10 "pile") at significant power, to perform chemical engineering in intense radiation fields, and to cope with radioactive wastes. Wigner assigned me the task of establishing the lattice spacing for what is now the X-10 historical landmark reactor—but at the time the reactor was anything but a historical landmark: It was here that plutonium was first produced on a gram scale, and where the chemical processes used at Hanford were tested on pilot-plant scale.

THE ORIGIN OF PRESSURIZED WATER

I remained at Chicago during 1944, but I visited Clinton often, particularly to assist in the theoretical analysis of experiments on the chain-reacting properties of lattices containing only uranium and ordinary water. I had become interested in H_2O-moderated chain reactors in 1942 when, under Wigner's direction, we were analyzing theoretically chain-reacting systems moderated by all the feasible moderators—H_2O, D_2O, Be, and C. Bob Christy and I estimated that a lattice of ordinary uranium and H_2O would not chain react, but that the multiplication constant k could be nudged over 1 if the uranium were enriched in ^{235}U from the natural 0.71% to about 0.75%. Bob and I even took out a patent on such a system (as, unbeknownst to us, had Hans Von Halban and Lew Kowarski of Joliot-Curie's group in France); but such patents hardly represented great new flashes of insight. We were working in terra incognita, and everything we touched was new, and therefore patentable.

The Clinton experiments on H_2O-moderated lattices were made possible when in 1943, some tens of tons of four-inch long uranium slugs, destined to go into the X-10 pile, became available. Arthur Snell first piled these slugs into a heap to determine how much enrichment was needed to make a bomb out of uranium. (In those earliest days we could not completely rule out the possibility of a chain reaction, and therefore a bomb, in ordinary uranium.) Fortunately, Art discovered that the multiplication constant in an infinite mass of ordinary uranium was only 0.2. Once Snell's experiments were completed, I suggested that the slugs be stacked in a regular lattice, and the interstices be filled with water. We did not expect the system to be chain-reacting, but we hoped that the multiplication would be reasonably close to unity.

The experiments were conducted atop the X-10 pile, the source being the neutrons from the X-10 graphite thermal column. We were astonished, not to say delighted, when we discovered that the best multiplication constant was around unity—fully 0.04 higher than our original prediction.

I remember reporting these results to Fermi and Wigner in Chicago in 1944. They were interested, but since the Hanford reactors were well under way, there was little chance that H_2O moderation would play a part in the war effort. I also wrote a memorandum to Richard Doan, research director of Clinton Laboratories, about the implications of the water experiments. The date was September 18, 1944. Since this was the first mention of pressurized water in the Manhattan project, I reproduce the short note in full.

To: Mr. R. L. Doan
From: A. M. Weinberg

The possibility of using ordinary water as a moderator in a chain reaction pile was not recognized, in spite of the obvious advantages that such a system would possess, until the first measurements on the Clinton lattices were started. It had generally been assumed that a multiplication constant in the neighborhood of .96 could be achieved with water as a moderator and this assumption (which was the only justifiable one at the time) was probably the basis for the fact that the project has been preoccupied with graphite and D_2O systems. Actually, the preliminary experiments at Clinton indicate that a multiplication constant very close to 1 (1.00 ± .01) is obtainable with an arrangement containing only metal rods and ordinary water. The main reason that the multiplication factor is higher than had been estimated at the beginning of the project is that at that time the effect of fast fission had not been adequately taken into account; also the cross-section ratio of uranium to hydrogen is actually higher than had originally been supposed.

The advantages of a system moderated with water are obvious. Such a system would contain within itself a means for cooling. The system would have very high specific production of product since the metal density in an ordinary water system is much higher than in any of the other systems. Such a system, if it works at all, would probably be much more compact and consequently simpler to build than the conventional piles. *Finally, if the coating problem can be adequately solved (by the use, say, of beryllium) it may be possible to run such a system under pressure and obtain high pressure steam which could be used for power production.* (italics added)

A second thread that led to H_2O-moderation began with the discovery, in 1944, that ^{240}Pu was spontaneously fissile. Plutonium produced in the Hanford reactors would therefore be bathed in a background of neutrons from the ^{240}Pu mixed with ^{239}Pu. This meant that the plutonium gun–type weapon would predetonate!

Arthur Compton called a meeting to discuss this turn of events. The meeting was dominated by Wigner; he suggested that Hanford plutonium be converted into fissile ^{233}U (which we thought would not predetonate) in a water-moderated converter. The active lattice in Wigner's converter would consist of Pu-containing aluminum plates, over which water flowed. Surrounding the active core would be an array of thorium slugs which, on absorbing neutrons emitted by the core, would be converted to ^{233}U.

Wigner's converter was never needed, since the ^{239}Pu implosion bomb finally worked. But his was the first serious design of an H_2O-moderated and cooled, seed-blanket reactor. This design was the forerunner of the Materials Testing Reactor, as well as the reactor aboard the U.S.S. *Nautilus* nuclear submarine, and later the land-based Shippingport light-water reactor.

As I was aware of Wigner's plate-type, H_2O cooled converter, and the Clinton exponential experiments on H_2O-natural uranium lattices, it was natural for me to think about using water both as moderator and coolant in a power reactor. The Navy, even before the war ended, was showing interest in nuclear power for submarines; I recall meeting with Ross Gunn, technical director of the Naval Research Laboratory, and Philip Abelson, director of the liquid thermal diffusion isotope separation project, to discuss naval propulsion. By this time (1945), I had made some preliminary estimates of the critical size of a pressurized version of Wigner's plate converter; I found that such a reactor could fit comfortably in a submarine. Indeed, I was sufficiently impressed with the simplicity of a pressurized water system that Forrest Murray and I wrote a report, Mon-P-93, dated April 10, 1946, entitled *High Pressure Water as a Heat Transfer Medium in Nuclear Power Plants.* In this report we described a power version of Wigner's thorium converter. We also mentioned a slightly enriched version of a high-pressure water reactor.

Captain H. G. Rickover (Rick) came to Oak Ridge in 1946. He was accompanied by a half-dozen young naval officers, all bent on learning about nuclear energy and developing a nuclear-powered submarine. The naval contingent attended the School of Reactor Technology, which was directed by Frederick Seitz. It had been set up to convey to industry the newly gained knowledge of nuclear chain reactions. Many of America's most influential nuclear engineers attended the "Clinch College of Nuclear Knowledge," as it was affectionately called.

Rick and I often discussed submarine propulsion. At the time, General Electric's proposal to use liquid sodium as the coolant for a submarine reactor was the main line. I insisted, however, that pressurized water was a better alternative. At first Rick would have none of this: The thermal efficiency of a pressurized water system was too low, he would argue. Lieutenant Commander Eli Roth helped me to persuade Rick that on board a submarine, simplicity—not thermal efficiency—was key.

Several people laid the technical foundation for this choice of pressurized water. First was Albert Kaufman, a metallurgist at MIT who suggested that zirconium might be useful as a cladding, if the neutron absorption of zirconium were as low as some, but not all, compilations of absorption cross-section indicated. Second was Herbert Pomerance, an experimental spectroscopist at Oak Ridge who discovered that the alleged high neutron absorption of zirconium was caused by a small impurity of hafnium. Third was Sam Untermyer, who at the time was Wigner's technical adviser. Sam was the first to show experimentally that zirconium resisted very hot water. He also invented the canned rotor pump, which has been used widely in pressurized water systems. And finally was Harold Etherington, an engineer from Allis-Chalmers, who headed a team in Oak Ridge that was designing a prototype gas-cooled, BeO-moderated power reactor. The gas-cooled reactor was never built, but Etherington's team drew up the first plans for a pressurized water reactor (PWR). Most of the elements of modern pressurized water reactors—the pressurizer, the arrangement of the fuel plates, the canned rotor pumps, the method of inserting control rods, and so on—were conceived by Etherington's group.

Rick's achievement—the first large power-producing reactors—is surely worthy of great praise—but I have never understood why Rick gave no credit to Harold Etherington and Sam Untermyer for their detailed conceptualization of the first PWR.

Out of *Nautilus* sprang Shippingport, the first large land-based light-water reactor (LWR). Shippingport used Wigner's seed-blanket arrangement, in which a highly enriched seed is surrounded by an unenriched blanket. Since enriched uranium was so expensive at the time, I had never expected a reactor that depended on enriched uranium to be economic. This I now attribute to my lack of understanding of the possibilities of isotope separation by gaseous diffusion, a lack attributable to the wartime compartmentalization between reactor people and isotope separation people. One of the few persons who bridged that gap was Karl F. Cohen. Karl was one of the inventors of the K-25 diffusion cascade. During the war he also spent several months at Chicago working with Wigner's group on D_2O-moderated reactors. Karl therefore understood reactors, and perhaps more importantly, understood that enriched uranium would not always be impossibly expensive. He therefore espoused the use of slightly enriched uranium in central power stations; his espousal of this system has had much to do with the dominance of slightly enriched uranium water lattices for central power.

Critics often ask whether the choice of light water was wise: Would nuclear power have avoided the difficulties it is now experiencing if a different reactor, say heavy-water-moderated or graphite gas-cooled, had been chosen for central power? On balance, I would say no—the probability of mishap in any of these systems is hardly different than in modern LWRs. On the other hand, I must concede that compactness and simplicity, not safety, were the underlying design criteria for the warship *Nautilus;* had safety been the primary design criterion, I suspect we might have hit upon what we now call inherently safe reactors at the beginning of the first nuclear era, rather than 50 years later at the beginning of the second nuclear era.

THE CONVERSION OF CLINTON INTO OAK RIDGE NATIONAL LABORATORY (ORNL)

During this time Eugene Wigner, having taken a liking to the bucolic atmosphere of the wartime Clinton Labs, had sketched out ideas (which he shared with me) for converting the wartime pilot plant into a full-scale laboratory dedicated to the exploitation of the chain reaction. And in 1947, Wigner spent the academic year as Research Director of Clinton Labs.

Wigner's year at Clinton was marked by the design and eventual construction (in collaboration with Argonne National Laboratory) of the first high-powered, light-water-moderated reactor—the Materials Testing Reactor. The MTR stands as one of ORNL's main contributions to reactor technology: It has influenced the design of many of the world's LWRs both as power plants and as research tools.

Wigner left after his year was up, and for more than a year the laboratory had no director. The year 1948 was therefore a time of trouble for Oak Ridge National Laboratory (as Clinton was renamed). Because Wigner had left, the Atomic Energy Commission (AEC) ruled that all reactor work be transferred to Argonne (our sibling since both Clinton and Argonne were offspring of the Chicago Metallurgical Laboratory). No one seemed interested in taking the job of Director of ORNL. At least half a dozen prominent people, including John Dunning, Fred Seitz, and John Manley, were offered the job—but all refused. Finally Union Carbide, the new contractor, asked me to take over—as Director or Research Director. (I later learned that my name was suggested by H. G. MacPherson, the expert on graphite, who had attended the Training School and who worked for Carbide. Mac, during all the years we worked together, never told me that he was the one who suggested my name for Director.) I accepted, but chose the title Associate Director for Research since I felt that I would have fewer purely administrative chores were someone else designated as Director. Nelson Rucker, who had headed Y-12, became Director; and after a year Clarence Larson.

I shan't detail the many exciting, sometimes frustrating, technical developments at ORNL during the 26 years I served, first as Research Director, and then as Director. I shall mention them only by title—Production and Distribution of Isotopes, both radioactive and stable (separated in the Y-12 calutrons); the Fluid Fuel breeder reactor adventure, first with aqueous solutions and then with molten fluoride salts; the abortive attempt to develop a nuclear airplane (out of which, however, sprung the powerful materials research enterprise at ORNL); the Biology Division, which under Alex Hollaender's paternal guidance enjoyed a worldwide reputation; the succession of improved radiochemical processes developed by the chemical technologists, led by Floyd Culler; and the many important discoveries in the physical sciences, some based on our research reactors, some on the growing collection of particle accelerators, some exploiting our access to ultraheavy nuclides, and some, such as Taylor & Datz's pioneering work on crossed-molecular beams, just being done at ORNL because we had a culture of interdisciplinary research. In retrospect, these were good times: People were not burdened with the flood of paperwork that seems to afflict so much of science today.

HOW I BECAME A THINK-TANKER

Instead I shall describe what was never a very large part of ORNL's activities while I was at the Laboratory, but which nevertheless strongly defined what

I did after I left ORNL in 1974, and which has since affected the character of all of the US national laboratories. I refer to our growing fascination with, and attempts to predict, the future—in short, the way futurology has enchanted so many of us.

As far as Oak Ridge was concerned, futurology began with the Franck Report, written in Chicago and distributed to the scientists at Oak Ridge. This report visualized a world in which nuclear bombs were commonplace; and it suggested institutions for regulating them, such as what is now the International Atomic Energy Agency (IAEA). Then our New Piles Committee tried to visualize the future of nuclear energy—it was here that the breeder emerged as the central objective of nuclear development, and it was here that Fermi had uttered his prophetic warning. Many of these activities we carried on outside of working hours—for example, when I testified for the first time before Congress in 1945, painting a rosily optimistic picture of nuclear power, but also conceding that nuclear war, in the long run, could be averted only if war itself were extirpated.

The first bona fide futurologist I ever met was Palmer Putnam. Putnam, an engineer who had built a 1250-kW windmill on Grandpa's Knob in Vermont in 1941, had been asked by the AEC to examine the world's energy future, including the possible role of nuclear energy. Putnam gave a series of lectures around 1951 in Oak Ridge on what he described as the views of a Prudent Custodian of the World's Energy System; these lectures appeared as the first of the postwar books on energy futurology—*Energy in the Future* (3). Putnam's approach is still what today's energy futurologists often use: Project the world's population (Putnam's projections were too low); estimate the rate at which per capita energy would grow (he estimated too high); and thereby estimate the world's aggregate energy demand, say in 2000 and 2050. On the supply side, Putnam would make educated guesses for each of the traditional sources—coal, oil, gas, hydropower, and wind—and since his projected demand outran his projected supply by 2000, Putnam insisted that a new energy source was needed, even with the increases in efficiency that he predicted. Putnam placed great faith in the breeder (he even included a picture of Oak Ridge's aqueous homogeneous reactor in his book); and rather less faith in the sun and its children or in fusion, but in his conclusion he urged development of all these inexhaustible resources.

From today's standpoint, Putnam's effort would be regarded as rather naive, since price is hardly mentioned. Today's energy futurology is cast in an econometric mode, with supply and demand depending on price. This, for example, is the spirit of one of the best-known modern energy models, the Edmonds-Reilly world energy econometric model (4), which is more or less the standard energy future assumed in studies of the greenhouse effect.

Perhaps because I was often called on to speak on the future of nuclear energy, I would myself venture into futurology. I recall speaking at the Pittsburgh Analytical Spectroscopists Meeting about the future of the national laboratories (5). Even then, in 1955, I was concerned about what would happen to these institutions when their original raison d'être had disappeared—because nuclear energy's problems had been solved or abandoned, or because the Cold War had ended. I suggested they might redeploy around what I called "geological engineering"—correcting the deterioration of our environment caused by our inexorable growth in population.

Jim Lane at Oak Ridge was one of the first to estimate the costs of power from different nuclear energy systems. Today we could be accused of having been much too optimistic—particularly in the early 1960s, when Phil Hammond preached the doctrine of very big is very cheap—i.e. if the scaling laws for capital cost were extrapolated, in the limit the capital cost per unit of energy would become cheap enough, for example, to desalt the sea. These ideas surfaced in 1967 at the time of the six-day war in the Middle East. In response to a Senate Resolution introduced by Howard Baker, ORNL conducted a study on the possibility of large-scale nuclear desalting in the Middle East. The study was conducted by Americans, Israelis, and Egyptians. Oak Ridge was perhaps the only place in the world at the time where Israelis and Egyptians were jointly engaged in a serious study of how to relieve tensions in the Middle East with a technological fix, cheap nuclear power. (I met with Ambassador Rabin of Israel, now Prime Minister, for an hour in the Knoxville Airport to tell him about our studies—which, since we assumed nuclear power would be very cheap, were generally favorable. He was skeptical—as he put it, we in Oak Ridge, some 5000 miles away from Jerusalem, had lots of "chutzpah" to be figuring out a technological fix to solve the problems of the Middle East. I shot back—No sillier than was Theodore Herzl in drawing up blueprints for the State of Israel in a Viennese cafe.) Nuclear energy turned out to be much more expensive than we had predicted—but with peace breaking out in the Middle East, I would not be too surprised if an ultimate settlement involved some technological approach to deriving water from the sea.

Energy futurology really came of age in the 1970s, when the United States realized that our trade deficit was largely caused by our import of huge amounts of oil. So in 1973 Dixie Lee Ray produced the first of the "modern" energy analyses in a report to President Nixon entitled *The Nation's Energy Future*. The report projected zero oil imports by 1980— ridiculously optimistic, as we judge today, 20 years later. But the report was significant in that it stressed the importance of conservation—an idea that had powerful proponents at ORNL, such as Roger Carlsmith, Bill Fulkerson, Jack Gibbons, Eric Hirst, and Dave Rose.

ORNL was one of the first national laboratories to take demand-side management and energy efficiency seriously. Early in 1970, the National Science

Foundation (NSF), under its Interdisciplinary Research for Problems of Society (IRPOS) and Research Applied to National Needs (RANN) programs, funded a small program at ORNL on energy and the environment. Leading the effort was Dave Rose, on leave from the Massachusetts Institute of Technology (MIT); when he left in September of 1970, Jack Gibbons took over. The ORNL group quickly realized that a direct way to reduce the environmental impacts of energy would be to use less energy. Today, under the leadership of Bill Fulkerson and Roger Carlsmith, some 150 technical people are working on energy efficiency at ORNL. This is now the largest energy project at ORNL, enthusiastically supported by the US Department of Energy (DOE).

This transformation of a substantial part of ORNL from developing supply to reducing demand reflects the change in our government's policies toward energy. When, in 1970, we started to move in this direction, I still was convinced that the merits of nuclear energy would long since have been proven and that wide deployment of nuclear power would obviate the need for strong conservation policies. But I hedged my thinking about nuclear energy by supporting the NSF group's enthusiasm for demand management. If I was wrong about nuclear energy's future, then energy efficiency would surely achieve prominence, and receive support. That now, 25 years later, it has become the largest single energy program at ORNL is astonishing—and possibly a little disappointing to me who still thinks of the early days when I was convinced that nuclear energy would soon become the main source of the world's energy.

PRESIDENT'S SCIENCE ADVISORY COMMITTEE, SCIENTIFIC INFORMATION, AND THE *ANNUAL REVIEW OF ENERGY*

I served on the President's Science Advisory Committee (PSAC) under George Kistiakowsky in 1959 and 1960, and under Jerry Wiesner until 1962. Kisty's Committee was very much concerned with military matters, about which I knew very little. This, plus my being a government laboratory representative among mostly university people on PSAC, made me very much an outsider.

My main contribution to PSAC was the report *Science, Government, and Information* (SGI) (6), which appeared in 1963 under my chairmanship. This was the first attempt by the highest level of government to address the ever-growing problem of scientific information. On our committee were some of our country's best scientists. In particular, Josh Lederberg's incisive thinking and felicity of style greatly improved the original first draft, which I wrote in Washington during the summer of 1962 while on leave from ORNL. The report attempted to clarify the roles of the technical community, the information community, and the government in what we called the "Information Transfer

Chain." The report had considerable influence among the information community, but on the whole it was ignored by working scientists.

Science, Government, and Information urged the creation of what today would be called Scientific Analysis Information Centers: places where information in a given field would be compacted, and would be disseminated to the appropriate scientific community. Although information centers did not emerge as the central element in information transfer—in part because information has been computerized so astonishingly successfully—quite a few information centers were established in the wake of our report. The Oak Ridge National Laboratory, for example, at one time had about a dozen such centers; and even today some of these centers still flourish.

Science, Government, and Information emphasized the importance of good reviews of the literature:

> *The Technical Community Should Give Higher Status to the Reviewer.* Scholarly reviews, articles, and critical bibliographies also play an important part in easing the information crisis. They serve the special needs of both the established workers in a field and the graduate student entering the field, as well as the general needs of the nonspecialist. Review writing is a task worthy of the deepest minds, able to recast, critically analyze, synthesize, and illuminate large bodies of results. The relation of the reviewer to the existing but widely scattered bits of knowledge resembles the relation of the theorist to available pieces of experimental information. In order to emphasize the growing importance of the reviewer and also the growing difficulties that he faces, scientific and technical societies should reward his work with good pay and with the regard that has been reserved heretofore for the discoverer of experimental information. Those asked to write reviews or to give invited papers reviewing a subject should be selected by the scientific societies with the same care as are recipients of honors or of appointments to the staff of a university.
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> Hand in hand with the increasing recognition of the review author should go an increasing realization by him of his growing responsibilities. He should view his subject dispassionately, paying equal attention to his own contribution and to the contributions of others. He should search for remaining problems and the most fruitful areas of further work as diligently as he emphasizes existing accomplishments. He should also point to areas where further work is necessary (6).

My association with SGI led to my appointment to the Board of Directors of Annual Reviews. Here I worked with that most admirable polymath, William O. Baker, whom I had first met when we were both on PSAC. Bill and I would often talk about the information problem; and it was at this time, around 1970, that energy was becoming a key political issue, as well as a subject for serious analysis. I suggested to Bill, and to the rest of the Annual Reviews Board of Directors, that we establish an *Annual Review of Energy*. Bill was enthusiastic, and he urged me to pursue the idea further.

I had opportunity to discuss the proposed *Annual Review of Energy* in 1972 with Prof. Peter Auer of Cornell at a conference on energy in Puerto Rico. This conference was organized by Peter, and was one of the earliest symposia at which the entire energy problem was reviewed (7).

The Annual Reviews Board of Directors decided to go ahead with the project if we could get support from the National Science Foundation. Here Paul Donovan, who at the time was director of the NSF's Energy Policy Group, was most helpful. He arranged in 1975 for a grant of about $55,000; with this money the *Annual Review of Energy* was launched. We were most fortunate to get Jack Hollander to serve as Editor of *ARE.* Under his imaginative and sophisticated leadership, *ARE* has flourished; in 1991 it became the *Annual Review of Energy and the Environment.* Its circulation is around 1500, and no energy analyst can be without a copy of the latest *AREE.*

THE WHITE HOUSE YEAR: 1974

After leaving ORNL in 1973, I spent the year 1974 in the White House and then in the Federal Energy Administration (FEA) as head of the Office of Energy Research and Development. This was the time of the first oil shock; gas lines were common, and energy was a most important item on the political agenda. This was also the year of Watergate. Many of the Watergate personalities occupied offices in the Old Executive Office Building next to our elegant offices, which had originally been the headquarters for the Navy Department. Name tags outside the offices would disappear regularly as the occupants were implicated in Watergate by the *Washington Post.*

As an amateur in Washington politics, I was struck by the intensity with which the game was played by the many Harvard MBA types. But, although I had been assured both by William Simon and John Sawhill (who successively headed the FEA) that our office would have strong influence on energy research policy, I quickly realized that the line agencies—particularly the Atomic Energy Commission, the Department of Interior, and the Department of Treasury—and the Office of Management and Budget (OMB) really controlled things. To be sure, I chaired an interdepartmental committee of assistant secretaries for energy research from the various departments; but, once everyone realized that our office really had little power to allocate budgets for energy research, the assistant secretaries would delegate lower and lower officials to attend—and our interdepartmental committee quietly died.

So our small staff spent much of its time simply thinking about energy matters. Perhaps our most important contribution was the Solar Energy Research Institute (SERI). With my background as an old nuke, I realized that I ought to bend over backwards in support of non-nuclear energy. How better to establish the government's non-nuclear commitment than to create a national laboratory devoted only to solar energy? Hugh Loweth of the OMB found the idea appealing; the idea was examined by the National Academy of Sciences, and SERI was established a few years later.

One of our regular consultants was Ed Schmidt, an old friend from the days of the nuclear airplane. Ed was a sort of eminence grise of the General Electric company; he seemed to know everything connected with GE's energy business. From his first visit in early 1974, Ed would intone "The first nuclear era is over; let us prepare for the second nuclear era." I too had this feeling—that nuclear power would not survive the growing public disaffection.

My main proposal for salvaging nuclear energy was to adopt a "confined siting" policy for the United States. As I saw it then, LWRs, as nonbreeders, would run their course as uranium became expensive. They would be replaced by breeders or near-breeders, which would be the nuclear energy technology of the coming "second nuclear era." Central to this conception was confinement of reactors to no more than 100 sites. These sites would be committed into perpetuity to nuclear power; I thought, perhaps naively, that a limitation of this sort on the places where nuclear power was generated would satisfy many of the concerns of the less extreme nuclear opponents. This proposal for confined sites was taken up by John Sawhill, then head of FEA, in a major speech he made on energy policy; and I still think it is a good idea.

But I realized that this policy would not be adopted in the short run. I therefore asked George Daly, a young economist on our staff, to estimate the economic consequence of abandonment of nuclear energy. George wrote a short memorandum in which he concluded, rather to my chagrin, that the United States economy would be little affected by replacement of nuclear by fossil fuels. But I realized at the time that the economic and environmental consequences of a nuclear moratorium required a larger effort than was represented by George Daly's short memorandum.

One of the main jobs of the FEA was the preparation of the Project Independence "Blueprint." President Nixon, after publication in 1973 of Dixy Lee Ray's report to the President, *The Nation's Energy Future,* had called for the United States to become independent of oil imports by 1980. To formulate governmental policies that would help achieve such an ambitious goal, the FEA tried to estimate how the price of oil would affect supply and demand for imported oil. The instrument for making these projections was a huge econometric-energy model. This was called the Project Independence Blueprint. The Blueprint was possibly the most elaborate attempt at econometric forecasting ever made up to that time; it reputedly cost $20 million. But the whole exercise was flawed, because its predictions of future oil demand and supply depended so strongly on a single number—the price elasticity of demand for liquid fuel. This number was poorly known—and the predictions of the Blueprint were correspondingly shaky. The whole exercise has made me skeptical of very large-scale forecasting. Nevertheless, I have some sympathy with government policymakers who, for lack of anything better, seize upon the results of large-scale models to guide their attempts at formulating policy.

Toward the end of 1974, we summarized what we called "Important Issues" for John Sawhill, who by then was the head of FEA (8). Among these "Important Issues" were the Solar Energy Research Institute, confined siting for nuclear reactors, the development of the electric car, exploitation of western shale, and improved coal-mining technology. That we did not explicitly identify energy conservation as an Important Issue perhaps reflects my strong predilection for supply technologies—despite the good work that had been done at ORNL a few years earlier on conservation. At that time energy policy was pretty much dominated by the Department of Interior and AEC—both supply-oriented agencies. Although we old nukes paid lip service to demand management, our hearts were really in increasing supply, not dampening demand. Nevertheless we had the wit, in presenting our "Important Issues" to FEA, to include an introductory recommendation to develop technologies that use energy efficiently.

This was also the time when the environmental impacts of energy production were coming into prominence. In our introduction we included a recommendation to study long-term climatological effects of energy production: "We should plan to learn enough about climatology over the next 10–15 years to be able to predict with some confidence at what stage man's production of energy will cause serious global effects on climate." Twenty years have passed since that recommendation was made, and we have learned much about climate; yet most of us would claim that although the sign of the CO_2 effect is known, its magnitude remains uncertain.

By the fall of 1974, reorganization of the government agencies responsible for energy research and development was in the wind. I therefore spent a good amount of time talking to Senator Abraham Ribicoff and to Congressman Chet Holifield about the proposals to split the AEC into a Nuclear Regulatory Commission and an Energy Research and Development Administration. I don't know who originally came up with this idea. I do remember that about a year earlier Senator Howard Baker (of Tennessee) told me that the days of the Joint Committee on Atomic Energy and of the Atomic Energy Commission were numbered. And so in October 1974, Congress passed the Energy Reorganization Act, which split AEC into NRC and ERDA.

By this time, I had pretty much had my fill of Washington. Gene and I were married in September of 1974, and around Christmas time, just 347 days after I had set foot in the Old Executive Office Building, I returned to Oak Ridge to serve as Director of the Institute for Energy Analysis.

THE INSTITUTE FOR ENERGY ANALYSIS

I became a full-time energy futurologist at the Institute for Energy Analysis (IEA). The idea for the Institute originated with Bill Baker. At the time, in 1973, Bill was part of the inner circle of the Nixon administration—a sort of scientific wise man who probably had more influence on administration policy than any other scientist. Bill suggested to me that the White House needed systematic, sophisticated analysis of the energy situation and that I ought to form an Institute for Energy Analysis to provide such analyses.

With the help of H. G. MacPherson (Mac), I prepared a prospectus for IEA in 1973. We proposed that IEA be a sort of super think tank—one that would correlate the findings of the many small energy futurology groups that were springing up at the time. IEA would enjoy the advantage of having as its point of contact the very highest level of government.

Bill suggested that I discuss IEA with various people in Washington, in particular John Sawhill, who at the time was a deputy director of OMB for Natural Resources. John assured me that the OMB would support an Institute for Energy Analysis. With this assurance, I visited all the national laboratories, during the fall of 1973, to find a home for the Institute. I finally decided to stay in Oak Ridge, but ORNL was operated by a private corporation, and it seemed unbecoming to place such a "super" think tank under private corporate management. I therefore arranged with Bill Pollard, executive director of Oak Ridge Associated Universities, to establish our Institute for Energy Analysis as a division of ORAU. Except for the year 1974, while I was in Washington (Mac directed IEA during that crucial year of its formation), I spent the next 14 years at IEA—first as Director, and then, upon reaching the age of 70, as a senior fellow.

Ed Schmidt had warned me that think tanks had original ideas only during their first five years. After that they lapse into a kind of bureaucratic somnambulance, during which survival takes precedence over other issues. Our Institute for Energy Analysis survived for 15 years—from 1974 to 1988. Although it never became the "super think tank" envisioned in the prospectus, several important ideas originated at IEA.

Economic and Environmental Impact of a Nuclear Moratorium, 1985–2010

Although IEA was supposed to consider all energy sources, given my background we tended to dwell on the nuclear issue. Thus our first major study, *Economic and Environmental Impacts of a Nuclear Moratorium, 1985–2010* (9), tried to put flesh on the memorandum George Daly had prepared during our year in Washington. The IEA study was funded by ERDA, the money actually being transferred from the much larger CONAES study (Committee on Nuclear and Alternative Energy Systems) of H. Brooks and E. Ginzton.

Again, we found that the effect of a moratorium, beginning in 1985, was surprisingly small. This was traceable to our assumption that, rather than energy demand in 2000 rising to 160 quads per year, as was common wisdom in those days, energy demand would rise to between 101 and 126 quads. Bill Pollard was responsible for what then seemed like a ridiculously low estimate—put forward at that time only by such energy radicals as Dave Freeman in his Ford Foundation study, *A Time to Choose* (10), or Amory Lovins. (As matters have turned out, even our low estimate of 101 quads is too high; by 2000 we probably will use less than 90 quads.)

CO_2 and Energy Policy

Our moratorium study was the first one in which the relation between CO_2 and energy policy was explicitly analyzed. I had been aware of the energy-CO_2 nexus ever since Jerry Olson, an ecologist at ORNL, had begun his studies of the carbon cycle. Even earlier, I became aware of the first modern estimates, by Gilbert N. Plass, of the greenhouse warming caused by CO_2. (Coincidentally, Gilbert and I had been colleagues at the Chicago Metallurgical Laboratory, where we both were part of Wigner's team.) But it was Ralph Rotty who made me fully aware of the CO_2 problem. Ralph joined IEA in 1974. He was both a meteorologist and a mechanical engineer. For several years before he joined IEA he had been tracking the world's output of CO_2, based on United Nations data on energy production. It was Ralph who first called my attention to David Keeling's curves of the inexorable yearly rise of CO_2 of about 1 ppm, a rise modulated by a small seasonal fluctuation.

I suddenly realized that the long-term rise of CO_2 might prove to be the strongest argument possible for preservation of the nuclear option. So, with Keeling curves in hand, I visited whomever I thought might help, both in studying CO_2 and in preserving nuclear energy. I presented my case to Senator Howard Baker, Majority Leader of the Senate; Guy Stever, Director of NSF; Frank Press, President of the National Academy of Sciences; and most important, Bob Seamans, Administrator of ERDA. Bob, after talking with me, established within ERDA an Office of Climate Change, headed by Dave Slade. Much of Dave Slade's money for assessment of CO_2 was given to IEA—and for several years our little think tank was the center for analysis of the CO_2 issue.

Our task was described by the rather ambiguous term "assessment." In point of fact IEA concerned itself with every aspect of CO_2, including the carbon cycle; mitigating strategies, such as planting trees; organizing meetings of the fledgling CO_2 research community; and, with the arrival of Bill Clark, attempts at assessing social impacts of CO_2 accumulation. Everything that was known about the CO_2 problem was summarized in a major volume, *CO_2 Review, 1982,* edited by Bill, and published by Oxford University Press in 1982.

The Second Nuclear Era

Despite the increased motivation for nuclear energy provided by the specter of CO_2, things continued to go badly for nuclear energy. Therefore, IEA sponsored two workshops in 1976 and 1979 at Gatlinburg, Tennessee, at which we tried to hammer out policies for nuclear energy that could be agreed to both by proponents and opponents (11a, 11b). At the first of these meetings, I pointed out that Rasmussen's probabilistic risk assessment implied that there was a good chance of a core melt that would have the direst effects on nuclear energy even though no one would be hurt. Unwittingly, I had predicted the Three Mile Island accident—and the resulting end of the first nuclear era, just as Ed Schmidt had warned me in 1974.

Our response to the ending of the first nuclear era was to convene a gathering of its intellectual leaders to discuss whether we should go back to the drawing board to design new reactor systems that were inherently safe. The group included such luminaries as Manson Benedict (MIT), Karl Cohen (GE), Paul Cohen (Westinghouse), Joe Dietrich (Combustion), Milt Edlund (Babcock & Wilcox), Peter Fortescue (General Atomics), H. G. MacPherson (IEA), Ed Schmidt (GE), and Irv Spiewak (IEA). The group agreed that the design philosophy on which the LWR was based ought to be reexamined.

The Mellon Foundation (of which Bill Baker was Board Chairman) supported us in our major study, *The Second Nuclear Era* (12). The study was managed by Irv Spiewak, and was published in 1985. Our main conclusion was that inherently, or passively, safe reactors were feasible; and that such passively safe reactors might be the technical basis for what we called "the second nuclear era." At the time, our findings were generally dismissed by the industry as being unrealistic, and by the nuclear opponents as being inadequate—but in the decade since the study was published, the idea of passive, or even inherent, safety has become more or less conventional wisdom.

Other Ideas from the IEA

IEA was fortunate in having a group of young, imaginative people who came up with a succession of powerful ideas, the significance of some of which is just being recognized even now.

DANIEL SPRENG AND THE IMPORTANCE OF TIME (13a, 13b) Perhaps the most original idea, to which Daniel Spreng and Pim Van Gool contributed, concerned the role of time in energy policy. Spreng in particular argued that energy, time, and information form a triad; each can be substituted by the other two. For example, in industrial processes, energy is often used wastefully (i.e.

at low second-law efficiency) in order to increase the rate of production (i.e. save time). This was a rather novel idea, since the idea of matching the temperature of a heat source to the end-use temperature—i.e. maximizing second-law efficiency—was receiving much attention at the time. Spreng's insight suggested that the matter was much more complicated, since perfect second-law efficiency implies infinite slowness.

THE REDISCOVERY OF ELECTRICITY Closely related to our realization of the importance of time was our "Rediscovery of Electricity"—a project to which Cal Burwell and Warren Devine contributed (14). Our main point was that electricity, far from being an inferior form of energy as many energy radicals claimed, was in fact, the most versatile of all energy forms. Because of its flexibility, much of the efficiency lost at the point of generation could often be made up at the point of end use. That something like this must be true is suggested by the continuing rise in the fraction of energy that is converted to electricity.

NET ENERGY ANALYSIS Net energy analysis (NEA) became the law of the land with the passage of the Non-Nuclear Energy Research & Development Act of 1974. IEA was assigned the task of making sense out of the many conflicting claims concerning NEA. These claims ranged from NEA being as important as economics in choice of energy modality to doubts that there were any useful questions that could be answered by NEA. This was at the time when Prof. Howard Odum had proposed NEA as a new general ecological principle, and Amory Lovins was claiming that nuclear energy should be abandoned because more energy was required to build and operate an expanding nuclear power system than was produced by the system. Among the IEA staff who participated in the NEA studies were Bud Perry, Ralph Rotty, Gregg Marland, Dan Spreng, and Dave Reister. I think IEA's studies on NEA were soberly responsible. A recent book by Dan Spreng (14) pretty much summarized our findings: Yes, there were some useful questions in NEA; but, no, NEA was not a magic talisman that would displace economic analysis, nor was the net energy balance for LWRs negative.

ENERGY-ECONOMETRIC ANALYSIS IEA's "style" tended to be ad hoc and simple. The one exception was the previously mentioned long-range econometric-energy model developed by Jae Edmonds and John Reilly of IEA's Washington office (4). (From the beginning of IEA, Chet Cooper—an economic historian—had led a small staff of social scientists in a Washington branch of IEA.) The Edmonds-Reilly model was created to provide an estimate of how future CO_2 emissions would be affected by various government policies. The model is still used widely by CO_2 analysts.

THE BOMB

I, along with most old nukes, always justified Hiroshima as having shortened the war, and thereby having saved lives. I was therefore pleased that McGeorge Bundy in his *Danger and Survival* (16) presents strong evidence based on Japanese sources that the modern revisionism, which claims that Hiroshima had little to do with ending World War II, is simply wrong. Nevertheless I have always been nagged by the great question, "How can mankind live forever with the bomb?" So in my final years at IEA, I addressed two of the main issues related to the bomb: proliferation, and strategic defenses and arms control.

Like most nukes, I have always argued that the connection between nuclear power and bombs was weak. A country bent on clandestine bomb-making would choose isotope separation (as did Iraq) or a dedicated non-power-producing reactor (such as the Indian or Iraqi research reactors) rather than a power producer. Moreover, given the high cost of reprocessing, the economic incentives to reprocess now are weak. A country bent on reprocessing, at least at this time, must have motives other than short-term economics for such a policy. These ideas were expanded in a study, *The Nuclear Connection,* organized by Marcelo Alonso, Jack Barkenbus, and myself (17). Contributors to the study included many of the best known specialists on proliferation and on reactor technology. In particular, Karl Cohen made a telling case for what has actually happened—that isotope separation or dedicated Pu-producers, not power plants, would be the chosen path for clandestine proliferation.

The other issue was the role of defensive systems in a nuclear-armed world. As early as 1968, I had argued that defensive systems were not destabilizing—if a limit were placed on offensive systems. This was contrary to most conventional wisdom within the arms-control community. Somehow the idea of strict constraints on offense, and few constraints on defense, struck too strongly at the whole idea of Mutually Assured Destruction. I, influenced by the writings of Eugene Wigner, Don Brennan, and Freeman Dyson, argued for the moral superiority of Mutually Assured Survival (MAS). Were there any paths to a world armed with, say, 100 nuclear missiles on each side, but in which both sides had deployed enough strategic defense to neutralize 100 incoming missiles?

Jack Barkenbus and I published two studies (18, 19), in which we outline a plausible path to this MAS world. The basic idea was the Defense-Protected Build-Down—a process in which each side reduces its offensive missiles, but compensates for this loss by deploying defensive weapons sufficient to neutralize an equal number of enemy missiles.

All of this, at the time it was put forward, seemed rather unreal—the Cold War was still going on. Now, the world has changed completely; are any of these ideas of Defense-Protected Build-Down still relevant? I would say that the idea that strategic defense against attack by a few missiles makes technical sense, and is still relevant. A MAS world is therefore not implausible. But in the long run, I come back to my testimony of 50 years ago—that somehow war itself must become obsolete. And, if political scientists such as B. Russett (20) are correct, this means the conversion of the world's 160-odd nation-states to liberal democracies.

THE TRADITION OF NON-USE AND THE SANCTIFICATION OF HIROSHIMA

But to rely on this great conversion to liberal democracy to keep the bomb from being used is unrealistic. Instead, Tom Schelling's lesser aim—the strengthening of the Tradition of Non-Use—is probably more to the point (21). Professor Schelling has pointed out that the bomb has never been used, after Nagasaki, simply because no policymaker can face the unforeseen consequences of its use. Can this Tradition of Non-Use be made permanent?

I have suggested that if the bomb were invested with sufficiently strong and enduring taboos, then these taboos themselves might guarantee the non-use of the bomb far into the future. I have spoken of the "Sanctification of Hiroshima," the conversion of the historical event, Hiroshima, into a religious event—one that people 1000 years from now will view with religious horror, and that will therefore strengthen Schelling's Tradition of Non-Use.

One way to invest Hiroshima with "religious" symbolism is to adorn it with tangible tokens—objects that will remind people of August 6, 1945, into eternity. One such symbol is the Hiroshima Peace Park in Japan. Another is the International Friendship Bell, which has been erected in the center of Oak Ridge, Tennessee as part of the 50th anniversary celebration of the founding of Oak Ridge. (I was chairman of the committee that raised money for the bell.) The bell weighs four tons and is made of bronze. Emblazoned on one panel is a scene of Japan, where the bomb fell; on another, the Appalachians, where ^{235}U in the Hiroshima bomb was made. Four dates are inscribed on the remaining panels: Pearl Harbor, Dec. 7, 1941; V-J Day, Sept. 15, 1945; Hiroshima, August 6, 1945; and Nagasaki, Aug. 9, 1945.

Bronze bells last for 1000 years or more. Should Alvin Weinberg be remembered then, I hope that my association with this unique symbol of permanent peace, much more than my efforts to create a permanent energy source, will be the justification for my immortality.

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