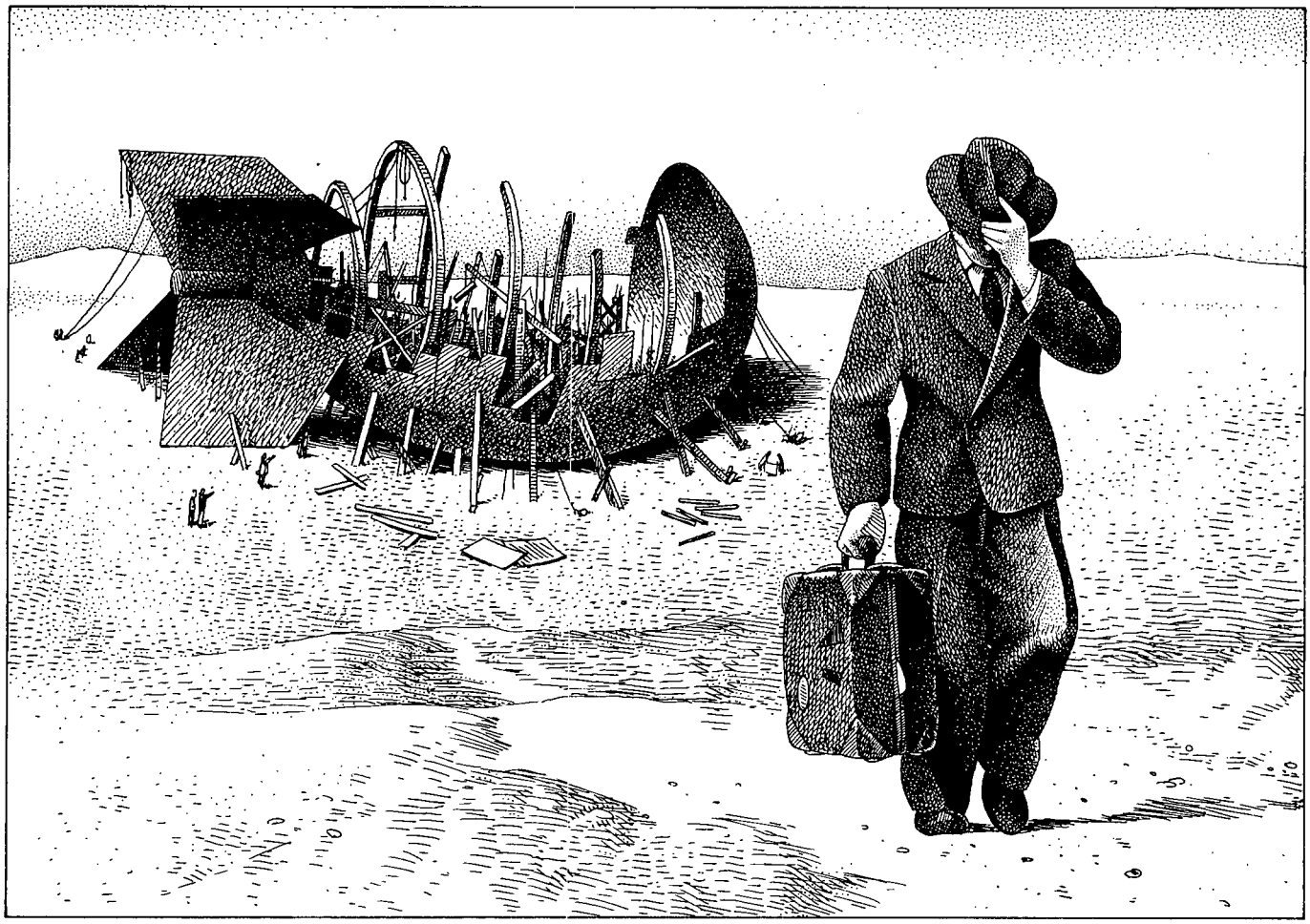

Tom Herzberg, United States

Leaving the bomb project

A nuclear physicist responsible for helping design the atomic bomb tells for the first time why he decided to leave Los Alamos in 1944.

by Joseph Rotblat

WORKING ON THE Manhattan Project was a traumatic experience. It is not often given to one to participate in the birth of a new era. For some the effect has endured throughout their lives; I am one of those.

This essay is not an autobiography; it describes only my involvement in the genesis of the atomic bomb. All extraneous personal elements are left out, but their exclusion does not mean that they are unimportant. Our hopes and fears, our resolutions and actions, are influenced by an infinite number of small events interacting with each other all the time. Because of this, each of us may react differently to the same set of conditions. The experience of every Los Alamite is unique.

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AT THE BEGINNING of 1939, when the news reached me of the discovery of fission, I was working in the Radiological Laboratory in Warsaw. Its director was Ludwik Wertenstein, a pupil of Marie Curie and a pioneer in the science of radioactivity in Poland. Our source of radiation consisted of 30 milligrams of radium in solution; every few days we pumped the accumulated radon into a tube filled with beryllium powder. With this minute neutron source we managed to carry out much research, even competing with Enrico Fermi's prestigious team, then in Rome, in the discovery of radionuclides. Our main achievement was the direct evidence of the inelastic scattering of neutrons; my doctoral thesis was on that subject.

In the earlier experiments on inelastic scattering we used gold as the scatterer. By the end of 1938 I had begun to

experiment with uranium, so when I heard of the fission of uranium, it did not take me long to set up an experiment to see whether neutrons are emitted at fission. I soon found that they are—indeed, that more neutrons are emitted than produce fission. From this discovery it was a fairly simple intellectual exercise to envisage a divergent chain reaction with a vast release of energy. The logical sequel was that if this energy were released in a very short time it would result in an explosion of unprecedented power. Many scientists in other countries, doing this type of research, went through a similar thought process, although not necessarily evoking the same reaction.

In my case, my first reflex was to put the whole thing out of my mind, like a person trying to ignore the first symptom of a fatal disease in the hope that it will go away. But the fear gnaws all the same, and my fear was that someone would put the idea into practice. The thought that I myself would do it did not cross my mind, because it was completely alien to me. I was brought up on humanitarian principles. At that time my life was centered on doing "pure" research work, but I always believed that science should be used in the service of mankind. The notion of utilizing my knowledge to produce an awesome weapon of destruction was abhorrent to me.

In my gnawing fear, the "someone" who might put it into practice was precisely defined: German scientists. I had no doubt that the Nazis would not hesitate to use any device, however inhumane, if it gave their doctrine world domination. If so, should one look into the problem to find out whether the fear had a realistic basis? Wrestling with this question was agonizing, and I was therefore glad that another pressing matter gave me an excuse to put it aside.

This other matter was my move to England, where I was to spend a year with Professor James Chadwick in Liverpool, on a grant to work on the cyclotron which was then being completed there. This was my first trip abroad, and the upheaval kept me busy both before the journey in April 1939 and for some time afterward, because I spoke very little English, and it took me a long time to settle down.

Throughout the spring and summer the gnawing went on relentlessly. It intensified with the increasing signs that Germany was getting ready for war. And it became acute when I read an article by S. Flügge in *Naturwissenschaften* mentioning the possibility of nuclear explosives.

Gradually I worked out a rationale for doing research on the feasibility of the bomb. I convinced myself that the only way to stop the Germans from using it against us would be if we too had the bomb and threatened to retaliate. My scenario never envisaged that we should use it, not even against the Germans. We needed the bomb for the sole purpose of making sure that it would not be used by them: the same argument that is now being used by proponents of the deterrence doctrine.

With the wisdom of hindsight, I can see the folly of the deterrent thesis, quite apart from a few other flaws in my rationalization. For one thing, it would not have worked with a psychopath like Hitler. If he had had the bomb, it is very likely that his last order from the bunker in Berlin would have been to destroy London, even if this were to bring terrible retribution to Germany. Indeed, he would have seen this as a heroic way of going down, in a *Götterdämmerung*.

My thinking at the time required that the feasibility of the atom bomb be established, one way or the other, with the utmost urgency. Yet I could not overcome my scruples. I felt the need to talk it over with someone, but my English was too halting to discuss such a sensitive issue with my colleagues in Liverpool.

In August 1939, having gone to Poland on a personal matter, I took the opportunity to visit Wertenstein and put my dilemma before him. The idea of a nuclear weapon had not occurred to him, but when I showed him my rough calculations he could not find anything scientifically wrong with them. On the moral issue, however, he was unwilling to advise me. He himself would never engage in this type of work, but he would not try to influence me. It had to be left to my own conscience.

The war broke out two days after I returned to Liverpool. Within a few weeks Poland was overrun. The stories that Hitler's military strength was all bluff, that his tanks were painted cardboard, turned out to be wishful thinking. The might of Germany stood revealed, and the whole of our civilization was in mortal peril. My scruples were finally overcome.

BY NOVEMBER 1939 my English was good enough for me to give a course of lectures on nuclear physics to the Honors School at Liverpool University, but by then the department's senior research staff had disappeared: they had gone to work on radar and other war projects. I had, therefore, to approach Chadwick directly with an outline of my plan for research on the feasibility of the atom bomb. His response was typically Chadwickian: he just grunted, without letting on whether he had already thought of such a plan. Later I learned that other scientists in the United Kingdom did have the same idea, some of them with similar motivation.

A few days later Chadwick told me to go ahead and gave me two young assistants. One of them presented a problem. He was a Quaker and as such had refused to do war work. He was therefore sent to Liverpool University for academic duties—but was diverted to work with me on the atom bomb! I was not allowed to reveal to him the nature of our research, and I had qualms of conscience about using him in such an unethical way.

The main idea which I put to Chadwick was that for the atom bomb the chain reaction would have to be propagated by fast neutrons; otherwise it would not differ much from a chemical explosive. It was therefore important to measure the fission cross-section for fast neutrons, the energy distribution of fission neutrons, their inelastic scattering, and the proportion of those captured without producing fission. It was also relevant to find out whether stray neutrons might cause a premature start of the reaction,

which meant determining the probability of spontaneous fission of uranium.

We built up a small team of young but devoted physicists and used the cyclotron to tackle some of these problems. Later we were joined by Otto Frisch who measured the fast neutron fission cross-section for uranium-235. I had the idea of using plutonium, but we had no means of making it.

As a result of these investigations, we were able to establish that the atom bomb was feasible from the scientific point of view. However, it also became clear that in order to make the bomb a vast technological effort would be required, far exceeding the manpower and industrial potential of wartime Britain. A top-level decision was reached to collaborate with the Americans. And so I found myself eventually in that "wondrous strange" place, Los Alamos.

IN MARCH 1944 I experienced a disagreeable shock. At that time I was living with the Chadwicks in their house on the Mesa, before moving later to the "Big House," the quarters for single scientists. General Leslie Groves, when visiting Los Alamos, frequently came to the Chadwicks for dinner and relaxed palaver. During one such conversation Groves said that, of course, the real purpose in making the bomb was to subdue the Soviets. (Whatever his exact words, his real meaning was clear.) Although I had no illusions about the Stalin regime—after all, it was his pact with Hitler that enabled the latter to invade Poland—I felt deeply the sense of betrayal of an ally. Remember, this was said at a time when thousands of Russians were dying every day on the Eastern Front, tying down the Germans and giving the Allies time to prepare for the landing on the continent of Europe. Until then I had thought that our work was to prevent a Nazi victory, and now I was told that the weapon we were preparing was intended for use against the people who were making extreme sacrifices for that very aim.

My concern about the purpose of our work gained substance from conversations with Niels Bohr. He used to come to my room at eight in the morning to listen to the BBC news bulletin. Like myself, he could not stand the U.S. bulletins which urged us every few seconds to purchase a certain laxative! I owned a special radio on which I could receive the BBC World Service. Sometimes Bohr stayed on and talked to me about the social and political implications of the discovery of nuclear energy and of his worry about the dire consequences of a nuclear arms race between East and West which he foresaw.

All this, and the growing evidence that the war in Europe would be over before the bomb project was completed, made my participation in it pointless. If it took the Americans such a long time, then my fear of the Germans being first was groundless.

When it became evident, toward the end of 1944, that the Germans had abandoned their bomb project, the whole purpose of my being in Los Alamos ceased to be, and I asked for permission to leave and return to Britain.

WHY DID OTHER scientists not make the same decision? Obviously, one would not expect General Groves to wind up the project as soon as Germany was defeated, but there were many scientists for whom the German factor was the main motivation. Why did they not quit when this factor ceased to be?

I was not allowed to discuss this issue with anybody after I declared my intention to leave Los Alamos, but earlier conversations, as well as much later ones, elicited several reasons.

The most frequent reason given was pure and simple scientific curiosity—the strong urge to find out whether the theoretical calculations and predictions would come true. These scientists felt that only after the test at Alamogordo should they enter into the debate about the use of the bomb.

Others were prepared to put the matter off even longer, persuaded by the argument that many American lives would be saved if the bomb brought a rapid end to the war with Japan. Only when peace was restored would they take a hand in efforts to ensure that the bomb would not be used again.

Still others, while agreeing that the project should have been stopped when the German factor ceased to operate, were not willing to take an individual stand because they feared it would adversely affect their future career.

The groups I have just described—scientists with a social conscience—were a minority in the scientific community. The majority were not bothered by moral scruples; they were quite content to leave it to others to decide how their work would be used. Much the same situation exists now in many countries in relation to work on military projects. But it is the morality issue at a time of war that perplexes and worries me most.

Recently I came across a document released under the Freedom of Information Act. It is a letter, dated May 25, 1943, from Robert Oppenheimer to Enrico Fermi, on the military use of radioactive materials, specifically, the poisoning of food with radioactive strontium. The Smyth Report mentions such use as a possible German threat, but Oppenheimer apparently thought the idea worthy of consideration, and asked Fermi whether he could produce the strontium without letting too many people into the secret. He went on: "I think we should not attempt a plan unless we can poison food sufficient to kill a half a million men." I am sure that in peacetime these same scientists would have viewed such a plan as barbaric; they would not have contemplated it even for a moment. Yet during the war it was considered quite seriously and, I presume, abandoned only because it was technically infeasible.

AFTER I TOLD Chadwick that I wished to leave the project, he came back to me with very disturbing news. When he conveyed my wish to the intelligence chief at Los Alamos, he was shown a thick dossier on me with highly incriminating evidence. It boiled down to my being a spy: I had arranged with a contact in Santa Fe to return to England, and then to be flown to and parachuted onto the part of Poland held by the Soviets, in order to give them

the secrets of the atom bomb. The trouble was that within this load of rubbish was a grain of truth. I did indeed meet and converse with a person during my trips to Santa Fe. It was for a purely altruistic purpose, nothing to do with the project, and I had Chadwick's permission for the visits. Nevertheless, it contravened a security regulation, and it made me vulnerable.

Fortunately for me, in their zeal the vigilant agents had included in their reports details of conversations with dates, which were quite easy to refute and to expose as complete fabrications. The chief of intelligence was rather embarrassed by all this and conceded that the dossier was worthless. Nevertheless, he insisted that I not talk to anybody about my reason for leaving the project. We agreed with Chadwick that the ostensible reason would be a purely personal one: that I was worried about my wife whom I had left in Poland.

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And so, on Christmas Eve 1944, I sailed for the United Kingdom, but not without another incident. Before leaving Los Alamos I packed all my documents—research notes as well as correspondence and other records—in a box made for me by my assistant. En route I stayed for a few days with the Chadwicks in Washington. Chadwick personally helped me to put the box on the train to New York. But when I arrived there a few hours later, the box was missing. Nor, despite valiant efforts, was it ever recovered.

THE WORK ON THE Manhattan Project, as I said at the outset, has had an enduring effect on my life. Indeed, it radically changed my scientific career and the carrying out of my obligations to society.

Work on the atom bomb convinced me that even pure research soon finds applications of one kind or another. If so, I wanted to decide myself how my work should be applied. I chose an aspect of nuclear physics which would definitely be beneficial to humanity: the applications to medicine. Thus I completely changed the direction of my research and spent the rest of my academic career working in a medical college and hospital.

While this gave me personal satisfaction, I was increasingly concerned about the political aspects of the development of nuclear weapons, particularly the hydrogen bomb, about which I knew from Los Alamos. Therefore, I devoted myself both to arousing the scientific community to the danger, and to educating the general public on these issues. I was instrumental in setting up the Atomic Scientists Association in the United Kingdom, and within its framework organized the Atom Train, a travelling exhibition which explained to the public the good and evil aspects of nuclear energy. Through these activities I came to collaborate with Bertrand Russell. This association led to the foundation of the Pugwash Conferences, where I met again with colleagues from the Manhattan Project, who were also concerned about the threat to mankind that has arisen partly from their work.

After 40 years one question keeps nagging me: have we learned enough not to repeat the mistakes we made then? I am not sure even about myself. Not being an absolute pacifist, I cannot guarantee that I would not behave in the same way, should a similar situation arise. Our concepts of morality seem to get thrown overboard once military action starts. It is, therefore, most important not to allow such a situation to develop. Our prime effort must concentrate on the prevention of nuclear war, because in such a war not only morality but the whole fabric of civilization would disappear. Eventually, however, we must aim at eliminating all kinds of war. □