Working (and Not Working) on Weapons

BY KENNETH W. FORD

This is mainly a personal narrative. I offer some opinions and advice at the end.

DECIDING TO WORK ON WEAPONS

My story begins in the spring of 1950, when I was about to turn 24. I had completed two years of graduate study at Princeton and was getting ready for the PhD qualifying exam. John Wheeler, who had agreed to be my dissertation advisor, informed me that he had been granted a leave of absence to join Los Alamos to work on the development of an H-bomb. He said he would be glad to have me accompany him, to divide my time between lab work and dissertation work, but it was a decision that I must make. He didn’t press me.

In early May, Edward Teller came to town. We met on the steps of the Institute for Advanced Study, and he did give me a hard sell.

I considered the matter for scarcely more than a week, talking it over with friends and with the department chair, Allen Shenstone. Shenstone recommended against my going—not because it was weapons work but because, in his experience, too often graduate students who took leaves of absence never came back to finish their work. I was too confident to be moved by that argument. My friends, on the whole, encouraged me to go. John Toll, a student of Wheeler who was further along in his doctoral work and had spent the 1949 fall semester with Wheeler in France, had already agreed to accompany Wheeler to Los Alamos. His reasons may have been similar to the ones that finally motivated me to go—a combination of patriotism and practicality.

I believed that the world would be a safer place if the United States got the H-bomb before the Soviet Union did. At the time, I saw America as a basically moral nation. I could not conceive of this country misusing a powerful weapon, and I shared the general distrust of the USSR. As to practicality, the move to Los Alamos provided an opportunity to work closely with Wheeler, probably more closely than would have been possible in Princeton.

Once the qualifying exam was behind me, I bought a well-used surplus Army Chevrolet Carryall, rounded up a pair of British graduate students to share expenses, and, in late June, set out for Los Alamos. When I arrived, I learned that the Korean War had begun.

MY WEAPONS WORK

As it turned out, I worked full-time on the H-bomb program for two years, one year (1950-51) in Los Alamos and one year (1951-52) at Project Matterhorn in Princeton, which was a satellite of the Los Alamos Lab. (Matterhorn’s other branch, also founded in 1951, was devoted to plasma physics and grew into the Princeton Plasma Physics Laboratory.) Yet I also devoted many nights and weekends to my own research and laid a base on which the actual dissertation was quickly completed in 1952-53. I have been asked if the weapons work delayed my dissertation. The answer is by at most half a year. I could apply many of the theoretical research techniques learned in the weapons program to the pure research. I benefited, too, from close interaction with many outstanding scientists, including Enrico Fermi, Hans Bethe, Stan Ulam, John von Neumann, Robert Richtmyer, and, of course, Wheeler and Teller.

The famous Ulam-Teller idea that made the H-bomb feasible came in the spring of 1951, part way through my first year of work on the project. In the following year, Princeton's Project Matterhorn took on the task of analyzing the "burning" of the thermonuclear fuel (and the additional fission that it caused). The team at Los Alamos designed the triggering A-bomb and analyzed the ignition process. It was my job, aided by John Wheeler and John Toll, to write computer code for the

The MIKE Test

On November 1, 1952, a 10.4 megaton thermonuclear explosion code-named MIKE, ushered in the thermonuclear age. The island of Elugelab in the Eniwetok Atoll, was completely vaporized.

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SEAC computer at the National Bureau of Standards in Washington, DC, and then to debug and run successive versions of the program on the graveyard shift (10:00 pm to 8:00 am, if I remember correctly). This program provided the final prediction of the yield of the first thermonuclear explosion, the so-called Mike shot, at Eniwetok on October 31 or November 1 (depending on which side of the date line you were on). That prediction was 7 megatons. The actual yield was 10 megatons. John Wheeler said later that he thought we may have overlooked an energy-generating effect. My own view is that getting as close as we did was a big success, given the primitive nature of the computer (less powerful than today’s hand-helds) and the extreme simplifications in the physics that was necessary in order to shoe-horn the calculation into a few kilobytes of code.

During this time I had no second thoughts about what I was doing. Much like the earlier Manhattan Project physicists, I became engaged in the challenge, and only wanted to help achieve success. We were joyful when the news of the successful Mike shot reached us in Princeton.

**Deciding to Do No More Weapons Work**

Later in the 1950s, I worked on other secret projects as a consultant to aerospace companies. I went back to Los Alamos occasionally, but always to do pure nuclear physics research.

My rosy-eyed view of the intrinsic morality of my own country was shattered by the Vietnam adventure. In the late 1960s and early 1970s I participated in anti-war demonstrations and did some anti-war lobbying in Washington. At some point during that time I decided to do no more weapons work—or secret work of any kind. This decision was, in a way, moot, because I was not, at the time, doing any classified work, and was not expecting to be asked to do any. Yet it seemed important to me not only that I make the decision, but that I make it publicly. I chose to “announce” my decision in a talk I was to give in Cloudcroft, NM, at a gathering of scientists opposed to the war—I think it was in the summer of 1968. Into that talk I dropped the remark that I had decided against doing any further secret work or weapons work. I felt that if I didn’t say it publicly, I was always free to change my mind, but that having said it publicly—even to a small audience—I was committed for sure.

I have indeed not changed my mind and have done no classified work since then, although, on a later sabbatical, I did return to Los Alamos, which I have found to be a most agreeable place to live and work. The lab was kind enough to invite me to join the nuclear theory group for unclassified research.

**Perspective**

During World War II, probably no one had any qualms about working on nuclear weapons, and there was general joy when the Alamogordo test was a success. As to using the bomb against Japan, there were some dissenters, but the majority of those who had worked on it did not question its use on a Japanese city.

During the period of the H-bomb development, opinion was more divided. Most physicists who were asked declined the invitation to join that effort. They opted out for a combination of reasons. Some were opposed to the idea of an H-bomb in principle (this group included some very influential physicists such as Robert Oppenheimer, Fermi, and I. I. Rabi). Some simply felt, only five years after the end of World War II, that they had already done their bit. They wanted to focus on teaching and academic research. Some doubted the urgency of the project (despite the Soviet atomic explosion in 1949). Yet some of the best and the brightest did agree to work on the project. Fermi, Bethe, and von Neumann devoted part-time to it. Teller, Wheeler, and Ulam gave it full-time. (Note that five of these six people were immigrants from Europe. They felt, perhaps, a special obligation to heed a call from their new nation.)

Since that time, in the 1970s and beyond, weapons work has become even more controversial. This is especially true of the National Missile Defense system (successor to the Strategic Defense Initiative, or “Star Wars”), which many regard as just an enormous waste of resources, and, moreover, something with a destabilizing effect internationally. Yet, even though there are many who refuse to work on it, the government has no trouble finding an adequate cadre of talented people to devote their skills to this or any other weapons program. Principled dissent is not itself enough to stop a program,

(continued on next page)
KENNETH W. FORD

Kenneth Ford’s early education was in Kentucky and at Phillips Exeter Academy. He performed World War II service in the Navy. He did graduate work in theoretical nuclear physics at Princeton University. He worked with David Bohm and John A. Wheeler. Ken participated in the crash hydrogen bomb program. His first academic appointment was at Indiana University. He spent his Fulbright year at Heisenberg’s Institute and a research year at Los Alamos, 1957-1958. He also taught at Brandeis University. Ken also held administrative positions at the University of California-Irvine and at New Mexico Institute of Mining & Technology. He then held subsequent positions at the University of Maryland and at a biomedical start-up company. He was the American Physical Society (APS) Education Officer prior to becoming Executive Director of the American Institute of Physics from 1987-1993.

Dr. Kenneth Ford is the retired Executive Director and Chief Executive Officer of the American Institute of Physics. He has taught high-school physics and served as science director of the David and Lucile Packard Foundation. His book, written with John A. Wheeler, Geons, Black Holes, and Quantum Foam: A Life in Physics, won the 1999 American Institute of Physics Science Writing Prize. He has since written another book, The Quantum World: Quantum Physics for Everyone, published by the Harvard University Press. Ken was also one of the guest ethics panelists at the 2004 Sigma Pi Sigma Quadrennial Congress, held October 15-16, 2004, in Albuquerque, NM.

Books by Kenneth W. Ford include:

I advise each young scientist to think about the issue, and to think about it even before being asked to join a project. Any decision to work or not to work on a particular weapons program should be an informed personal decision based on rational considerations. A positive decision should not be based on blind patriotism. A negative decision should not be based on blind distrust of a particular party or set of leaders.

And one final piece of advice. The decision to work or not to work on weapons should not be based on salary and benefits and location. This is more important than it sounds. There are surely many people who engage in weapons work because it provides a good living, perhaps in a pleasant place, not to mention freedom from the publish-or-perish threat. I would like to think that the present generation of young scientists are idealistic enough to base their decision about weapons work on their analysis of how that weapon may or may not contribute to a better or safer world.

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Ken Ford wearing Crow Indian tie (a gift from the president of Little Big Horn College, Montana); 1999.

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