Letters

Policy and the First Bombs

In a recent issue of *Science* [**130**, 32 (3 July 1959)] Edward Condon reviews Amrine's book *The Great Decision*, which discusses in part the making of high policy by the United States Government on how to deal with the first atomic bombs. Both author and reviewer emphasize the importance of studying and understanding this piece of history. With this view I am in deep agreement. From earlier experience of my own, and from conversations with contemporary historians who have looked into the problem, it seems clear that the available documentary record is meager. I should not be astonished if the existing record is itself quite meager. Just for this reason it seems to me important not to let serious errors or misrepresentations pass unnoticed.

On one such error, which has to do with the information made "available to our policy makers at the time policy decisions on how to use the bomb were being made," I feel competent to make a comment. Since Condon's review repeats and elaborates a mistake also to be found in Feis' authoritative *Churchill, Roosevelt, Stalin*, I shall do so at some length.

The paragraphs in Condon's review which I wish to discuss are the following:

> . . . Amrine tells us (page 132) that General Groves, in a memorandum to General George Marshall dated 30 December 1944, vastly underestimated the power of the bomb. He estimated the power of the bomb at only 500 tons of TNT, whereas it was actually 20,000 tons when used on Hiroshima. As Amrine says, our military planners "were only given reason to think it was a spectacular improvement in bombs, not another kind of warfare".
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> Now I know that General Groves did not know enough physics to make his own estimate; and I do not believe that anyone at Los Alamos would have made such a low estimate. How then could Groves have erred by a factor of 40? Could it have been intentional, so that the top policy planners would not be aware of the horribly serious nature of the decision they were taking?

I have turned to the memorandum of 30 December 1944 from General Groves to General Marshall, which is published in the first volume of *Foreign Relations of the United States: The Conferences at Malta and Yalta, 1945*. The first paragraph reads:

> It is now reasonably certain that our operation plans should be based on the gun type bomb, which, it is estimated, will produce the equivalent of a ten thousand ton TNT explosion. The first bomb, without previous full scale test which we do not believe will be necessary, should be ready about 1 August 1945. The second one should be ready by the end of the year and succeeding ones at . . . intervals thereafter.

In the first days of August 1945 the final deliveries of components of the gun-type bomb reached the Pacific. This bomb was used against Hiroshima. Its yield is not known very accurately; it was not far from 17,000 tons. The calculation of the yield of nuclear weapons made a priori, and unverified, was not as difficult for this type of weapon as for the implosion bombs; it was nevertheless subject to some uncertainty. The discrepancy between General Groves' prediction of 10,000 tons and the actual value reflects some underestimate in our calculations of the yield, and a corresponding over-design.

General Groves' second paragraph reads:

> Our previous hopes that an implosion (compression) type of bomb might be developed in the late spring have now been dissipated by scientific difficulties which we have not as yet been able to solve. The present effects of these difficulties are that more material will be required and that the material will be less efficiently used. We should have sufficient material for the first implosion type bomb sometime in the latter part of July. This bomb would have an effect which would be equivalent to about 500 tons of TNT. During the remainder of 1945 it is estimated that we can produce . . . additional bombs. The effectiveness of these should increase towards 1000 tons each as development proceeds and, if some of our problems are solved, to as much as 2500 tons.

As I recall it, these estimates were based on extensive conversations between General Groves and senior members of the Los Alamos Laboratory, and reflected the then prevailing opinion, and the results of the most recent experiments and calculations. We had been convinced of our inability to make implosion bombs as we had originally designed them. I believe that it was not until about a decade later that such designs were tested.

By early July, however, we had solved, as General Groves suggested we might, "some of our problems"; and with a more conservative model were prepared to test a first bomb. Even at that time, however, our estimates of the yield were quite uncertain, and for the most part quite low. We established a pool in the laboratory, to record guesses as to the yield of the first bomb. An overwhelming majority made estimates under a few thousand tons; figures in the hundreds of tons were popular. Two visitors, Lee Dubridge and I. I. Rabi, picked the extreme

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values of zero and 18,000 tons. On 16 July, after the test, our first measurement of what had really happened gave 20,000 tons. That morning General Groves communicated this figure to the War Department, for transmission to the Secretary of War and the President, who were in Potsdam, meeting with representatives of the United Kingdom and the U.S.S.R.

I should perhaps add that when a scientific panel met with the Secretary of War's Interim Committee on 31 May 1945, Secretary Stimson and General Marshall were both present. I remember reporting that we expected one model of the bomb to be many times more powerful than the other, but that we were so uncertain of our ground that they might turn out in quite opposite order. I also remember that on that occasion we discussed a possibility, which had been reported to the Government in 1942, of developing bombs in the multi-megaton range by the use of fissionable material, heavy hydrogen, and natural uranium. Here, too, our early designs were to change with time.

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I have seen a copy of the letter which Robert Oppenheimer has written you correcting statements in Amrine's book, which were developed in E. U. Condon's review (which I have not seen).

I have no way of knowing whether Amrine was misled by statements on the subject made in my book *Churchill, Roosevelt, Stalin*. But I have a sense of responsibility for sharing in the diffusion of this erroneous impression of the anticipated explosive power of the atomic bomb. How I came to overlook the first paragraph of General Groves' memorandum to General Marshall quoted by Oppenheimer I do not understand. I suppose an appropriate penalty—the ultimate proof of my mistake—would be to be blown up by one.

HERBERT FEIS
York, Maine

I would like to correct some of the erroneous impressions a reader would gain from E. U. Condon's "review" of Michael Amrine's book *The Great Decision*, which appeared in a recent issue of *Science*.

This "review" goes far afield from the content of the book, and this despite a confession of little first-hand knowledge. While such frankness may be refreshing, equal frankness would have made Condon add the statement that he is not particularly fond of me.

I would not bother to answer Condon except for the fact that a publication such as *Science* has seen fit to print his "review" without checking his alleged facts. Many are so preposterous as to be obviously incorrect. The conclusions supposedly drawn therefrom present me in an entirely false position.

Condon's charge that I worked "to hobble and frustrate cooperation" with the British simply is not true. Because neither I nor anyone else could even guess at that time how the atomic future might unfold, because I felt that I had no right to give away information developed with United States funds, and because I believed that orderly procedures were essential in such a large and widespread effort, I did not want the previously highly informal communications to continue. These had not been unreasonable before the United States entered upon its major atomic effort. I felt that now specific presidential approval was required for any significant passing of information. I was not alone in this opinion. An executive agreement between President Roosevelt and Prime Minister Churchill, defining British-American relationships in this area, was the first essential to the formalization of the interchange of information.

Yet, it was not until the Quebec Conference of August 1943 that the necessary basic agreement was reached. I was not responsible for the fact that this agreement was not arrived at sooner. As soon as it was signed, proper arrangements were promptly made and the furnishing of information was controlled in accordance with the terms of the Quebec Agreement.

As to Chalk River, Condon ignores the fact that its parent organization was already being established at Montreal before I was placed in charge of the Manhattan project. This laboratory was never a part of the American project. A number of its key personnel were Free French, and France was still under German control. This added to our difficulties. In spite of these complications, the Canadian-British effort always received essential information from us to the extent necessary for the work it was carrying on. The Canadian Government always seemed satisfied with this cooperation. The Royal Commission appointed to investigate Russian espionage in Canada in its report of 27 June 1946 said (page 617):

> From the beginning there was the closest cooperation in scientific research between Canada, the United Kingdom, and, later the United States. While some secrets were not fully shared, as in the case of some details concerning the atomic bomb, the results of continuing research work by scientists in one country were in almost all cases at once communicated to their opposite numbers in the other two.

All available essential data and estimates respecting the bomb were made known to our policy makers when decisions were being made on the use of the bomb. General Marshall, Secretary Stimson, and the President (Roosevelt and later Truman) were kept informed as to the probable effect of the bomb on the conduct of the war. It was my responsibility to keep General Marshall and Secretary Stimson fully informed as to the changes in scientific thinking. including my own, on the probable power of the bomb. And I fulfilled that responsibility.

It is true that I did not know enough physics to insure the accuracy of any estimate, but, I might add, neither did anyone else. We simply did not have in the Manhattan organization a scryer of sufficient ability to do this. The estimates included in my memorandum of 30 December 1944 were based on my latest discussions with, and information from, Oppenheimer and his associates at Los Alamos. I might add that as late as the middle of May 1945 the

responsible heads at Los Alamos felt that the explosive force of the first implosion-type bombs would fall somewhere between 700 and 1500 tons.

Actually my memorandum to General Marshall of 30 December 1944, of which Condon makes such a point, has been completely misrepresented—and this is not the first time, although, with a few omissions (all nonessential for the present discussions), it was published in full with the Yalta papers and is thus readily available to any responsible writer. The first part of this memorandum read as follows (the remainder dealt with informing Admiral Nimitz and the air commander in the Guam area as to the atomic possibilities):

It is now reasonably certain that our operation plans should be based on the gun type bomb, which, it is estimated, will produce the equivalent of a ten thousand ton TNT explosion. The first bomb, without previous full scale test which we do not believe will be necessary, should be ready about 1 August 1945. The second one should be ready by the end of the year and the succeeding ones at . . . intervals thereafter.

Our previous hopes that an implosion (compression) type bomb might be developed in the late spring have now been dissipated by scientific difficulties which we have not as yet been able to solve. The present effects of these difficulties are that more material will be required and that the material will be less efficiently used. We should have sufficient material for the first implosion type bomb sometime in the latter part of July. This bomb would have an effect which would be equivalent to about 500 tons of TNT. During the remainder of 1945 it is estimated that we can produce . . . additional bombs. The effectiveness of these should increase towards 1000 tons each as development proceeds and, if some of our problems are solved, to as much as 2500 tons.

The plan of operations while based on the more certain more powerful gun type bomb also provides for the use of the implosion type bombs when they become available. . . . The time schedule must not be adversely affected by anything other than the difficulties of solving our scientific problems.

For a considerable period prior to this we had fully expected to use the gun-type bomb first and, as we did at Hiroshima, to use it without previous test. Actually, the reading of this memorandum by President Roosevelt was only a minor portion of the conference, and further information was given him later—the next day, I believe—by Secretary Stimson. That the President was fully informed is well illustrated by a statement of Secretary of State Stettinius, who, in his book *Roosevelt and the Russians*, mentioned his meeting with President Roosevelt shortly after this date, and the latter's

telling him on this occasion that if a bomb were dropped on New York City at 42nd Street and Broadway, the resulting explosion would lay New York low.

President Truman was fully aware of the potential power of the bomb from the time that he had his first conference on it with Secretary Stimson and me on 25 April 1945. At this meeting the President was not shown the 30 December memorandum. He did see, among other papers, Secretary Stimson's memorandum of 25 April, the first paragraph of which read as follows:

"Within four months we shall in all probability have completed the most terrible weapon ever known to human history, one bomb of which could destroy a whole city" (Stimson and Bundy, *On Active Service in Peace and War*, vol. 2, p. 635). Could anything have been clearer as to the import of the bomb?

From the very first, the messages to Potsdam were definite and clear as to the magnitude of the Alamogordo explosion. It was described in one of the cables as being visible for over 200 miles and audible for more than 50. Nothing could be further from the truth than the charge that there was any design to minimize the bomb.

I trust that because of the scientific reputation and value of your publication, because unanswered statements, no matter how unfounded, tend to become history, and because of the palpable injustice to me of Condon's "review," you will publish this letter in full.

LESLIE R. GROVES

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In response to the editor's invitation to comment on the letters from J. R. Oppenheimer and General Groves, I would say that I welcome the attention thus focused on Amrine's book. If there are errors in that book, this underscores the need for scholarly historical research on the full story of uranium bomb development.

Groves confirms that he did restrict exchange of information with our British partners—exchange which had been well established before he came on the project. More than a year and a half was lost before Roosevelt and Churchill reestablished that cooperation, in August 1943. How much delay resulted from that unfortunate policy will need to be assessed when the real history is written.

When that history is written one of the least important parts will be that which says whether General Groves is particularly fond of me or vice versa. A complete history will tell of the frantic efforts of one man to retain personal control of atomic energy in America by trying to rush the May-

Johnson bill through Congress; of how he was foiled in this by some of the scientists; and of his embittered efforts to take revenge. Such a history will examine carefully whether these things were related to the doings of the House Committee on Un-American Activities in 1948 and 1949, which deprived this country of the services of so many brilliant young American scientists.

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Radionuclides and Bone Cancer

A serious error exists in J. G. Kaplan's recent letter (*1*). It was stated that the Russians had observed bone cancers developing in dogs about 3 years after the injection of 0.1 microcurie of strontium-90 per kilogram. Actually, the radionuclide used in these Russian studies was not strontium-90, as erroneously reported by Engstrom *et al.* (*2*), but the much more dangerous thorium-228 (*3*).

The injected amounts of strontium-90 which it has been proved cause bone cancer are much higher than 0.1 μc/kg. Finkel (*4*) found that the incidence of osteogenic sarcomas in 90 mice injected with 44 μc of strontium-90 per kilogram was somewhat higher (6 percent) than that in 150 controls (2 percent), but the probability of this being due to chance occurrence was 20 to 30 percent. In a current study in our laboratory, 60 beagles have been injected with from 0.5 to 100 microcuries of strontium-90 per kilogram. Thus far only one dog, injected with 94 μc/kg, has developed a bone tumor. These results do not imply that lesser amounts of strontium-90 are without effect. However, they do illustrate the enormous difficulty in experimentally determining the consequences of very small injections of strontium-90, such as 0.1 μc/kg.

The maximum permissible body burdens of strontium-90 and radium-226 should be set so as to give the same probability (or improbability) of causing undesirable effects. If the ratio of these limits is based on the observed biological effects of strontium-90 and radium-226 in experimental animals, the maximum permissible body burdens, for occupational workers, of 2 microcuries of strontium-90 and 0.1 microcurie of radium-226 correspond fairly well.

In view of Kaplan's opinion of the "obvious impertinence" of physicists in biological questions, it seems strange that he would base the heart of his argument on the theoretical calculations of Rolf Björnerstedt, who is a physicist.

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References

1. J. G. Kaplan, *Science* **130**, 728 (1959).
2. A. Engstrom, R. Björnerstedt, C. J. Clemedson, A. Nelson, *Bone and Radiostrontium* (Wiley, New York, 1957), p. 133.
3. R. Björnerstedt, in a letter (9 July 1959) citing results reported in *Conference on the Remote Consequences of Injuries Caused by the Action of Ionizing Radiation*, F. G. Krotkov *et al.*, Eds. (State Med. Lit. Press, Moscow, 1956).
4. M. P. Finkel, *Science* **128**, 637 (1958).

Meteorology for Non-Science Majors

There is general agreement that all college students should have an experience in science before they receive their diplomas. Very rarely, it seems, is meteorology recommended as one of the courses the student should take to satisfy his science requirement. This is regrettable in view of the fact that meteorology has within it all the ele-