CHAPTER IV

RETURN TO ATMOSPHERIC TESTING--PACIFIC

The story of the return to testing in the Pacific in 1962 is one of many false starts, false directions, frustrations, and uncertainties. These came about because of the balance, or imbalance, of pressures among (1) a President who abhorred the thought of nuclear weapon testing in the atmosphere, who wanted to prevent the proliferation of nuclear weapon systems by the two large nuclear powers, and who felt that the initial step in so doing would be a test ban treaty, complete if possible, but if not, just atmospheric; and yet who also felt that as President he could not let the Russians advance beyond the United States in nuclear technology; a President, therefore, who was determined to give the Russians every chance to come to agreement, (2) a Joint Chiefs of Staff and an Atomic Energy Commission who respected the President's desires, but felt strongly that the Russians were gaining too fast by atmospheric testing, and believed that we could only maintain our lead by also testing in the atmosphere, (3) a State Department who never did know what they thought, (4) a Secretary of Defense who felt as strongly as the President that further nuclear weapon development should be prevented, but who also had to prevent the Russians from outdistancing us, (5) a split President's Science Advisory Committee, (6) a British ally which felt even stronger than the President that there should be no further proliferation, (7) a recalcitrant and "unreasonable" Russian opponent, (8) a technical organization whose morale had to be considered to a certain extent in the decisions made, (9) a set of nuclear weapons laboratory directors with strongly differing opinions on the need for future weapon development, and (10) a JCAE who felt that we should return to atmospheric testing as soon as possible. These conflicting pressures led to a period in which the President was grudgingly, dragging his heels all the way, taking those steps that led eventually to atmospheric testing, but in such a manner that the situation was never clear and was always changing as seen by the field test organization. The President continually kept alive his attempts to achieve a test ban treaty with the Russians and the British, and he finally attained that goal in 1963. It is quite clear that at any time in the interval between the resumption of testing in September of 1961 and the conclusion of the Limited Test Ban Treaty in 1963, President Kennedy would have signed a treaty with the Russians at a moment's notice, had that been possible.

The test organization, however that may be defined, loyally followed these fits and starts, even though occasionally a number of individuals in the system, both in the AEC and the DOD, clearly showed their irritation at this manner of conducting affairs.

Even when the President decided that atmospheric testing was necessary, he insisted that it be as little as possible, as few as possible, and as short as possible.

For some reason that is not quite clear, atmospheric testing was a special horror in Kennedy's mind, even though he himself apparently did not believe that long-range fallout would seriously endanger anyone's health. His actions seem to imply a feeling that underground testing would not seriously affect the international balance of nuclear forces, but that atmospheric testing would lead to sudden and

large changes in our posture vis-a-vis the Russians. He was told that eventually we could probably test as much as a megaton underground.

As a result of these pressures, the AEC and the DOD, in the fall of 1961, while preparing for certain kinds of atmospheric tests, reversed their paths and turned the tests off, turned them on again, and put them in different areas. Even early in 1962 they were trying to decide where to do some portions of the testing and whether or not to do other portions. Still later, during the test operation, these pressures, plus those due to new and deeper thinking on the part of the weapon philosophers, led to continual changes and extensions. There were short periods of stability in which the test organization felt that it had a clear and agreed-upon plan it could carry out. But right up to the end of the operation, that feeling of stability was continually disrupted by the vagaries of nature or politics.

Program Formulation

Achievment of an agreed-upon program for the United States atmospheric series of 1962, eventually called Operation Dominic, was a long and arduous process of compromise between proponents in the AEC and Department of Defense, and opponents in Presidential circles, the Department of Defense, and the AEC. On the sidelines, pressures from Congress and the public affected things slightly.

For once, the Department of Defense got the jump in planning on the Atomic Energy Commission. While Operation Willow planning had officially been stopped during the moratorium, the concepts and reasoning were still clear in the minds of the DASA planners. Toward the end of the moratorium, the growth of AFSWC capability and interest in high-altitude effects specifically related to the antiballistic missile problem and the continued efforts in this field at Rand had led to the growth of a coherent recognition of the associated problems and a growing desire within the Air Force to do something about it. The Army was anxious to continue the development and testing of the Nike-Zeus antiballistic missile system, and the Navy had several stockpiled systems that needed testing. In July 1961 these pressures and the growing likelihood of test resumption led the DDR&E (Harold Brown) to ask Bill McMillan of Rand to lead a group to look at these questions. (Brown had little confidence in the ability of DASA, as then constituted, to handle the problem.) Within two weeks of its formation, he asked the McMillan Committee to consider the necessity for atmospheric effects tests and to recommend a possible program. That committee set about its work with vigor, and by the end of the moratorium had several meetings in coordination with DASA, AFSWC, Rand, the armed services, and certain representatives of the AEC laboratories. By the fall of 1961 they had convinced themselves, and apparently McNamara, of the necessity to conduct several high-altitude detonations.

The Navy, too, had seen the handwriting on the wall and in the last month or so of the moratorium had gone ahead on their own to outline and prepare for tests of the ASROC antisubmarine system and the Polaris fleet ballistic missile (FBM) system. They proposed to conduct the Polaris test on the Atlantic Missile Range, firing into a target area near Ascension Island. By September the Navy had some of the forces in place ready to respond immediately to any Presidential directive on the subject.

The Air Force had begun conceptual planning for an Atlas to be fired from Vandenberg Air Force Base to Kwajalein, to check out the Nike-Zeus antiballistic missile system installed there.

Thus, not long after the moratorium ended in late August the Department of Defense presented to the National Security Council and the President some of the arguments for returning to atmospheric testing, from their point of view. No decision was made.

Once the moratorium was broken and the President had ordered underground testing, the AEC began to move. In the early days of September, during Presidential discussions with Seaborg and McNamara concerning the appropriate US response to the Soviet resumption of testing, both atmospheric and underground testing were considered. McNamara asked Seaborg to determine what the AEC could do on a two-, four-, or six-week schedule to provide three shots of sufficiently high yield that they would be noted off-site and by foreign observers. Seaborg answered on September 5, the day that the President declared the resumption of US nuclear weapon testing, but his answer did not include any plan involving atmospheric testing. However, in preparing that answer Seaborg had had discussions with Luedecke and Betts concerning atmospheric test possibilities, and Betts, in turn, had discussed the question with the Laboratory directors and Kenner Hertford, Manager of ALOO. On September 7 Betts asked the Laboratories for their comments on the advantages and disadvantages of testing in the atmosphere, asking that they reply by September 11 so that a joint AEC-DOD position on atmospheric testing could be prepared for submission to the President in the near future. In his answer on September 8 Bradbury stressed the advantages of atmospheric testing, pointed out that things could be done appreciably more rapidly in that manner, and even went into detail on the possible time scales and possible sites that could be used. However, he did not urge an immediate return to atmospheric testing; quite the contrary:

> LASL has to ignore all the various aspects of propaganda factors one way or another. All the experts are in Washington. In general, we would recommend the following: Let us try out underground testing in Nevada as fast as we can and see what we can do and what troubles we do or do not get into. We may end up saying "fine, fine" or we may come running to Washington after a few months with a cry that we are not getting anywhere. I think we have to give it a good try fast. By the first of the year, we should have a pretty good idea of what the virtues and difficulties are. We will also have a better idea of where we are headed in the international situation. No big warheads really need testing today.

Kenner Hertford gave his opinion that one or two atmospheric tests could be staged quickly at the NTS without undue public reaction. Further correspondence between Betts and the Laboratories in those few days made it clear that Betts was reflecting a Washington feeling of anxiety concerning the slow rate of testing evident in our initial Nevada planning schedules. As it happened, it took ten days to get the first shot off after the directive to return to testing! Thus, the entire system was casting around to see what could be done in a short time.

General McCorkle of AFSWC was busy in the same circles in conjunction with Kenner Hertford, trying to determine what requirements might be put on the Air Force for the Nevada tests and for other longer-range possibilities. At their September 13, 1961, meeting the Nevada Test Site Planning Board concurred in an initial study concerning possible reopening of the Eniwetok Proving Ground and planned a September 21 meeting to develop an integrated approach to Eniwetok test requirements such as balloons, barges, cabling, etc. However it was triggered off, in about mid-September Hertford suggested to McCorkle that they study the possibility of a "quick and dirty" airdrop operation. They worked with Los Alamos and Sandia over the next day or so and by September 19 an initial concept of an airdrop air array operation was in hand. By September 25 Hertford was sufficiently confident of the concept to suggest to Betts that McCorkle be appointed Task Force Commander of an Air Force Task Force to carry out the operation. Betts told him to keep planning.

On September 21 Bradbury wrote to Betts suggesting consideration of "quick and dirty" airdrops of stockpile devices Ex.(b)(3) Given proper authority, he felt that with about a week of preparation the Air Force could probably drop them and LASL could obtain minimum diagnostics (bhangmeter and radchem

samples). He also noted that LASL and Sandia were already preparing MK 39 drop cases Ex.(b)(3) The drop cases could probably be ready in about two months.

Thus was born the short-lived Operation Everready, a concept which contributed to the definition of an initial AEC atmospheric test program and to the beginning of the airdrop and air diagnostic capability used later in Dominic.

This groundwork led to presentation of an operational concept at the September 27 meeting of the Planning Board. Ex.(b)(3) Ex.(b)(3) The initial readiness could be achieved using an AFSWC B-52, with minimum diagnostics in the B-52 and radiochemical sampling from a B-57. Ex.(b)(3) . At the same Planning Board meeting it was concluded that in something like two more months, because of the additional diagnostic capability that could be available by then, another three shots could be added. (Also discussed by that group was a concept for a Nike-Zeus high altitude test at Johnston Island, which the Board concluded could be done in something like six months.)

During the next few days Washington picked up this airdrop proposal and, considering the priorities noted in the AEC Laboratory and Department of Defense messages of the last couple of weeks, made their own suggestions. On October 2 Gerry Johnson informed Seaborg that, "It is my opinion that such an operation would provide a means of conducting tests which are urgently needed, could be done very quickly, perhaps in a matter of weeks, and would provide some of the much needed information on devices which could be tested by this means." He recommended that the AEC, in coordination with DASA, quickly develop a test plan based on this concept and specifically suggested that proof tests Ex.(b)(3) be considered for inclusion in such a series. On October 7 Betts, noting that there was a possibility of international pressures causing us to enter another moratorium very soon, requested that plans be developed for firing those three devices under the Everready concept by December 1.

Ex.(b)(3)

In parallel with these actions the Laboratories discussed with DMA their broader aims for a longer-range atmospheric operation, but it was not yet possible to propose a specific list of devices.

The DOD, in the meantime, through the auspices of the McMillan Committee, DASA, AFSWC, and the armed services' representatives, listed those tests that seemed feasible in the moderately near future. Thus Gilpatric, in response to an NSC request and with Seaborg's concurrence, transmitted to the President on September 20 a joint AEC-DOD preliminary test program, and followed it up on October 9 with a more concise statement of needs. In addition to the Nevada program, that letter listed the initial five devices of Operation Everready as follows in Table XXIX.

Ex.(b)(3)

Ex.(b)(3)

[redacted]

The program letter further commented that the first of the improved technology devices could be ready in about six months.

The DOD list included five weapons effects tests and tests of three weapon systems. The weapons effects tests were as follows:

Ex.(b)(3)

[redacted]

The systems tests proposed were two Polaris shots, the first of which could be ready in mid-December 1961; an Atlas-D (to be launched from Vandenberg), which could be ready within two weeks; and the ASROC surface-launched, solid-propellant-rocket, antisubmarine weapon, which also could be ready for full-scale tests within two weeks.

Seaborg followed up the next day (October 10) with a confirming letter in which he estimated the cost of the Everready shots to be perhaps $500,000 to $600,000 per event. Gilpatric requested approval to prepare for such tests at appropriate overseas locations, and Seaborg added that larger-yield proof tests and certain development tests could be carried out in a completely airborne operation within the next few months, but that it would take at least six months and a major operation to open up a Pacific test site for an extended development test program.

With the President's announcement on November 2, 1961, that he had directed the AEC and DOD to prepare for atmospheric nuclear weapons tests, both the AEC and DOD began to develop firmer test proposals. They were aware that the tests would be scrutinized individually, not only by the National Security Council, but by the President himself, and that real need for a test would have to be shown before the President would allow it. Their proposed tests also had to fit within the physical constraints of the possible test sites, which had not yet been chosen, and to a certain extent within the limits on total debris radioactivity release to be allowed. They were aware of the Presidential guidelines which were as follows: Tests will be conducted in the atmosphere only if:

1. the tests will provide information of substantial importance to the national defense,

2. the information needed can be obtained in no other way with reasonable time and effort,

3. atmospheric fallout is to be minimized in all practical ways, and

4. the military need for the tests outweighs the desirability of avoiding all atmospheric fallout.

Furthermore, the proposed tests should be ready within four months, that is, by March 1, 1962, and the tests should be conducted in the shortest possible time with the target of no more than three months duration. The National Security Council had tossed around the idea of limiting the release of radioactivity to that corresponding to 10 megatons of fission, but no hard and fast rule was made.

On November 5 Betts asked the Laboratory directors and Field Office managers to prepare a new set of test proposals for discussion at a meeting in Albuquerque on November 13, 1961. At that meeting Livermore presented 26 proposed shots and Los Alamos 15. Both Laboratories included devices in their listing that, in principle, could be tested underground in Nevada, explaining, however, that the programs would be delayed if they had to wait for the appropriate facilities in Nevada. Betts requested that the Laboratories confirm their proposals by TWX, which both Laboratories did on the 20th. LASL's confirming list was the same as presented in Albuquerque with the addition of two high-altitude shots, one of which might be combined with the proposed Department of Defense program. Except for a 1-megaton shot, the Livermore listing was the same as given in Albuquerque. It is perhaps worthwhile at this point to take a more detailed look at the specific proposals.

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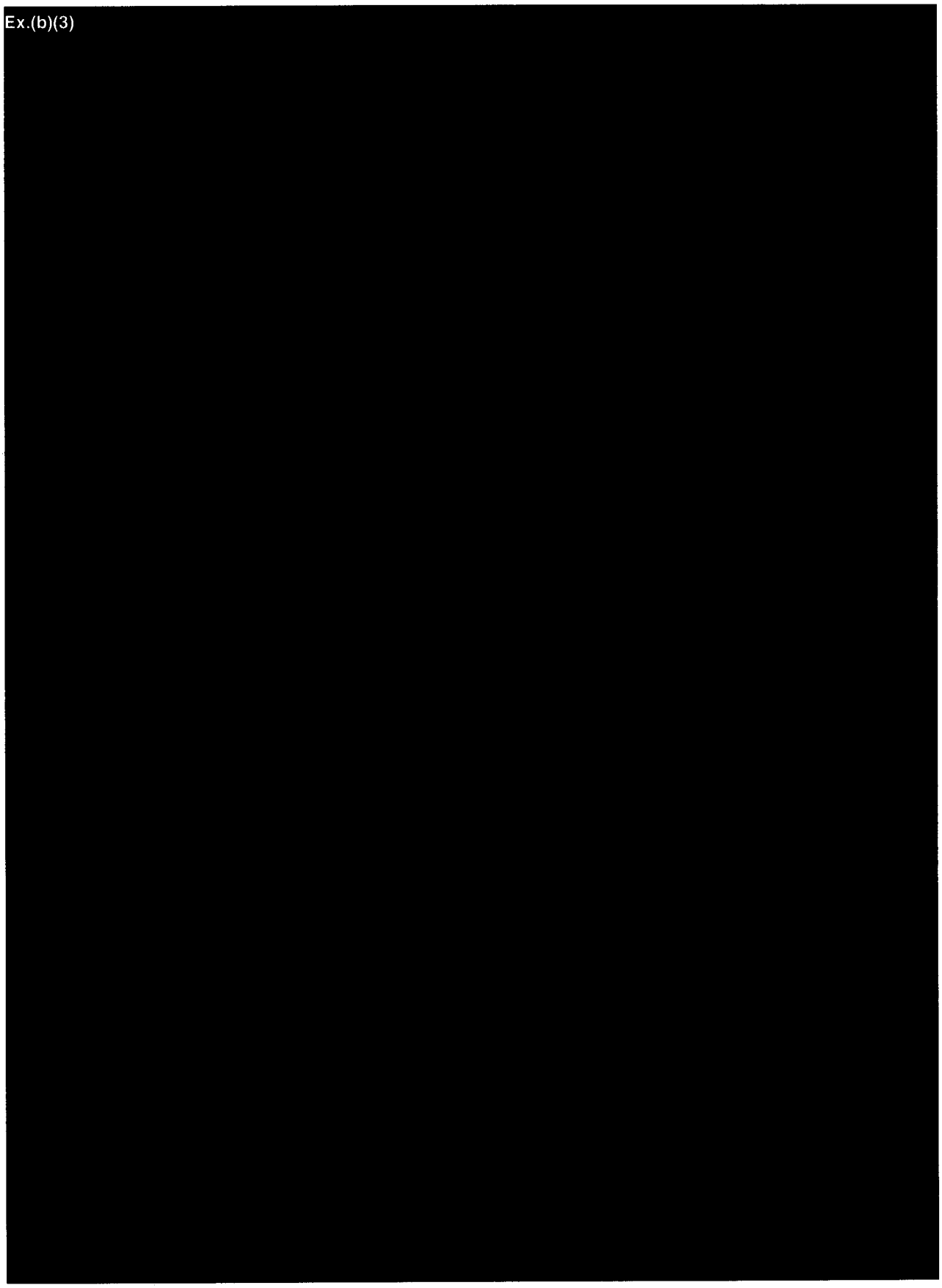

Ex.(b)(3)

Ex.(b)(3)

Bradbury added, "We believe the country to have been badly mistaken in its belief as to the efficacy of underground testing and that the national policy which followed this opinion might have been quite different had the actual facts been known two years ago. We believe that we should find out by actual experiment what really can be done in space testing before we get trapped into one or another belief regarding it."

In mid-November, President Kennedy appointed a subcommittee of the National Security Council, to be chaired by Seaborg and consisting of Wiesner, Bundy, a State Department representative, and Gerry Johnson from the DOD. In preparation for a meeting of that committee, Betts sent to Seaborg on November 24 the DMA version of a proposed shot program taken from the above listing. DMA had reduced the list to 10 LASL shots and 14 LRL shots, including two high-altitude tests. He noted the possible conflict between the AEC proposed high-altitude tests and the DOD proposals.

On November 21 Gerry Johnson sent Seaborg the DOD proposal for three high-altitude shots; Starfish, Kingfish, and Bluegill. On the next day Gilpatric instructed the Chairman of the JCS to review the Department of Defense proposals for high-altitude shots and pointed out that 12 to 18 months would seem to be required if a meaningful three-event high altitude effects program were to be achieved. Consequently, Gilpatric noted that since only 4 to 5 months lead time were available, the JCS should plan to execute at least one, but no more than two, high-altitude effects tests. The Gilpatric memo apparently contradicted the Johnson memo.

Be that as it may, Seaborg sent to the President on November 29 the resultant National Security Council recommendation. Seaborg's letter noted specifically that the development tests would have to be done in the atmosphere because of the high cost and long development time of a deep space testing capability.

Ex.(b)(3)

Ex.(b)(3) He also noted:

> We have proposed and did plan that yields up to 100 kt would be conducted underground in tunnels; however, our experience with postshot contamination in the tunnels at the Nevada Test Site up to this time gives us some concern that our planning in that aspect was not realistic. It is now estimated that, if feasible at all, tests of this magnitude could be conducted at a rate of not more than a very few per year, even under favorable conditions, in a given tunnel complex. In contrast, large-yield devices can be tested in rapid succession in the atmosphere with relatively little advance preparation of the means of testing.

The letter went on to note that after appreciable discussion on the ground rules to be used and the philosophy behind the choices, the Subcommittee recommended for Los Alamos

Ex.(b)(3)

was noted the intention was to test them underground in Nevada, but "In the event some or all of these cannot be accomplished satisfactorily underground, they may require inclusion in the atmospheric series." It was noted that the Laboratories would like to test the rest of the devices that had been on their list to DMA, but that the NSC was not recommending their inclusion in this operation.

Ex.(b)(3)

These were the forerunners of what became the Tightrope and Checkmate events of Operation Dominic, though these events were at other altitudes.

Seaborg added that the NSC recommended April 1, 1962, as a proposed readiness date for the series. It is fairly clear that this came about because the missile shots could not be ready until perhaps June, and the operation was restricted to a duration of three months.

Seaborg also noted the Los Alamos desire to try out a space testing capability Ex.(b)(3) He proposed that the space testing might be accomplished on Starfish Ex.(b)(3)

Ex.(b)(3)

The President clearly was not satisfied and requested that Seaborg decrease the number of tests by consolidating and substituting among the various proposals. Harold Brown also advised Seaborg that he disagreed with the AEC list, feeling that some six of the shots did not meet the criteria that:

> They can be fully justified on the basis of real (though not necessarily immediate) military importance, and there would be very great difficulty in performing them in other environments.

The President also sought other advice. In early December he discussed his concerns with Hans Bethe, who commented that while he did not consider atmospheric fallout very important, many people did, and as soon as we tested in the atmosphere our propaganda advantage would be lost. He felt that the United States should make a real effort to avoid atmospheric testing or at least restrict it to an absolute

minimum. On December 7, responding to the President's request, Teller pointed out the surprises that we had already seen in the Soviets' progress to date, commented that we had to develop lightweight warheads for our rockets, and that we should work on high yield warheads that could do damage at high altitudes, hence reducing the effectiveness of the Russian missile defense system. He emphasized the importance of acquiring additional ABM information, both from a defensive and offensive viewpoint, and gave his opinion that the further development of clean explosives was very important since the recent tests had put the Soviets into a leading position in that subject. He further commented:

> Since we have not foreseen the present emergency and since we have not planned for it, the atmospheric testing program of the spring of 1962 will fall short of accomplishing the major proportions of the objectives stated above. It is nevertheless essential that we should proceed with an appropriate testing program next spring. The necessarily limited results of such a series will certainly enable us to plan a next series in 1963 in a much more fruitful manner. There is no theoretical way which can replace the hard facts obtained from experience.

Teller went on, "The plan which has been worked out by the Lab Directors and the DDR&E is the result of a careful study with which I agree." In addition, as the DOD had suggested, he urged that there be an ABM test as early as possible utilizing a missile launched from the U.S. to Kwajalein with a Nike-Zeus making the intercept at Kwajalein, and suggested that such a test might be done as early as May or June of 1962. He was optimistic about including in the high-altitude tests of the NSC plan some of the diagnostic apparatus which might be used to develop a space testing capability. He further requested that the President visit the Laboratories and perhaps make a public statement emphasizing that "the development of nuclear explosives can be used to provide us with the strength that ensures peace." Teller also made it clear that he was looking ahead to a test series in 1963 when adequate instrumentation could be available to achieve the most important objectives.

Edward's comment on the value of the operation to be done in 1963 must have struck the President in an odd manner, since he was apparently worried at the time, not so much about the round of Soviet tests that were now almost finished, but about the rounds that might follow in 1962 or 1963, and he could just see both countries continuing the escalation.

On December 19 Conrad Longmire (LASL), in discussions with Panofsky on the subject of atmospheric testing, pointed out that the Russians were apparently doing both systems and effects tests, emphasizing his belief that it was very important for the U.S. to undertake such testing lest the Soviets gain significant advantages, especially in the critically important phenomena of EMP and radar and communications effects.

On January 8, 1962, Betts informed the Laboratories:

> With respect to an item to be added to the Presidential-approved list, a mechanism has been worked out whereby this type of change is accommodated. In brief, the Chairman approves the inclusion of the event and notifies the White House Staff of the added event. The Chairman's notification contains three essential elements as follows: A. the tentative firing date; B. purpose of the experiment; C. the reason why this particular experiment was not included in the basic list approved by the President. Therefore, on each additional event for which you request authority to execute, please advise me as early as possible, repeat, as early as possible, of these three inputs. Once you have provided me in a timely fashion these three inputs, you may assume that authority will be forthcoming prior to your anticipated execution date. Therefore, you may take all steps necessary short of the actual firing prior to your final authorization for the specific shot. These steps may be taken without recourse to DMA.

After the November 29, 1961, meeting of the NSC, the Laboratory programs changed very little. There was a continual review on the subject between Betts, Foster, and Bradbury with comparatively minor changes coming about, but there was no further NSC review until after the President had decided to test. [redacted]

Ex.(b)(3)

[redacted] On February 27 Foster replied for himself and Bradbury that their careful review of the proposed devices showed no overlap in the technical objectives and affirmed mutual interest in every experiment listed; therefore, the Laboratories would prefer to keep all six. [redacted]

Ex.(b)(1)

[redacted] Thus, there was a continuing round of conversations on the subject among Harold Brown (DDR&E), JCS, Seaborg, and others. However, when word got around that as part of the Everready practice runs at Tonopah, a dummy TX-43 containing only high explosives had gone off 3,000 feet underneath the drop aircraft instead of 3,000 feet above the ground and that no explanation could be found for the misadventure, the enthusiasm for testing such devices quickly waned in the JCS. With the concurrence of Harold Brown, the Navy and the Air Force kept the Atlas and the Polaris systems tests alive. Apparently, for several months early in 1962, tentative program changes were being made at the level of Seaborg and Brown, changes which evoked continuing admonition to the sponsors that the President had not approved the specific changes and had not agreed to atmospheric testing.

A couple of other program proposals should be noted. Khrushchev had threatened us with a 100-megaton bomb in July 1961. Apparently, in reaction to that, a number of people in the U.S. began to puzzle about such a device. On September 7 Betts asked Foster and Bradbury for detailed estimates for a possible 100-megaton weapon. [redacted]

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[redacted] Foster commented that he had not analyzed these suggestions in detail. The Commission agreed to send a memo to the President which would include the probable effects of a 50-megaton detonation at various altitudes, the time required for U.S. development for such a device, and the possible military uses of a 50-megaton bomb. The Chairman had spoken to the President earlier in the day concerning the 50-megaton atmospheric detonation that had just been announced by the Soviet Union.

Following the Commission meeting, Betts promptly started conversations with the Laboratories on the feasibility of early construction and tests of such a device by the United States, asking for comments by October 27. Sewell pointed out the delivery difficulty for testing if it were to be an airdrop, but Sandia stated on October 26 that a B-52 airdrop was feasible with a parachute they were presently developing. On October 27 the U.N. General Assembly asked the U.S.S.R. to "refrain from carrying out their intention to explode in the atmosphere a 50-megaton bomb," and on the same day Seaborg sent to the President a letter commenting that the General Advisory Committee was convinced that the AEC could, within a short time after a Presidential directive, come up with a single weapon having a yield of about 50 to 100 megatons. The President apparently immediately said to forget it. On October 27 Luedecke wrote "No further action required" on one of the messages on the subject. On October 30 the Soviets exploded their 50-megaton device. It went a little large.

During the moratorium, the last year or so of planning on the part of the AEC and DOD had assumed that any further weapons tests would either be underground or in deep space. The AEC, therefore, as noted earlier, tried to get a shot into the Dominic series to test the deep space concept and gain experience that would be valuable if that method of testing became necessary in the future.

In response to Betts' request on September 25 for suggested actions in atmospheric and high-altitude testing, Sandia prepared a detailed plan which would use a Thor missile launched from Johnston Island to carry test devices to altitude. The Sandia plan estimated that 6 to 10 months would be needed to prepare for detonation of a large yield device at altitudes between 100 and 300 kilometers. The plan listed

the objectives of high-altitude tests in the following way:

> A United States capability for testing nuclear devices outside the earth's sensible atmosphere should be established to provide: (1) A capability of testing large-yield weapons with reduced fallout in the event surface testing is not authorized. (2) A capability of testing large-yield weapons with reduced fallout in the event unacceptable atmospheric contamination is reached by United States and/or Russian surface tests. (3) A capability for vulnerability testing of complete reentry vehicle warhead systems in a vacuum or at appropriate intercept altitudes under controlled conditions. (4) A capability for conducting effects and phenomenology experiments on nuclear devices detonated outside the earth's atmosphere.

The report of the Sandia plan was distributed on October 2, but Don Shuster of Sandia was impatient: he notified the Planning Board members and the Laboratories that Sandia had studied ways to accelerate development of a high-altitude test capability and suggested that the Sandia work on a 300-kilometer Thor system might be applicable. Bradbury replied on October 16, 1961:

> The present NTS underground operation allows somewhat inadequate testing of small devices, but may never allow testing of megaton or larger weapons. Your proposal fits the spirit of the present limitations, i.e., no atmospheric contamination. Therefore, it seems to me that Washington should in the near future allow atmospheric testing or should approve an operation in deep space as you propose.

On October 19 Bradbury urged Betts to consider Shuster's proposal as soon as possible and warned against again going into a moratorium ignorant of different ways of testing. As noted before, Bradbury, in his November 20 message to Betts, proposed that there be a test at an altitude as high as could conveniently be reached in order to assess what really could be done in space. The proposal was discussed at the November 29 meeting of the National Security Council, and it was decided that "Consistent with satisfying DOD requirements, considerations will be given to accommodating the AEC desires to develop a capability for space testing in connection with the proposed 400-kilometer DOD experiment." (This effort led to the Starfish test.) By December 1, 1961, LASL had done enough work to respond to the NSC suggestion. Froman informed Betts that neither of the high-altitude shots, Starfish and Bluegill, would be fully satisfactory for learning how to test warheads in space, and added:

> In particular, 1.5 megatons at 400 kilometers is so large and so close as to cause serious saturation problems in detectors suitable for diagnostics in space testing. We believe that we should check out methods of space testing in order to avoid ignorance in that field such as we had about underground testing. We therefore propose that the AEC sponsor a shot Ex.(b)(1) The best choice of yield requires further calculations and considerations. Incremental costs will be small because we shall instrument and man to do all we can on the 400-kilometer shot in any case. Additional merits of such a test include Argus effects the DOD originally planned and data relevant to Vela Hotel, Vela Sierra, and physics. Effort would also be made to follow fission products and get a handle on fallout from this altitude.

On December 21 LRL offered support for the 1,000-kilometer test. Foster, in a message to Betts, said:

> It is LRL's understanding that at the present time, there are two high-altitude shots planned by the DOD from Johnston Island in the spring of 1962. LRL is planning to piggyback on these shots to develop space testing techniques. Ex.(b)(1)
> purpose. The second one, 1.5 megatons at 400 kilometers, we hope to use to test diagnostic methods. A second shot above 400 kilometers would be desirable for our purposes. Ex.(b)(1)

Within LASL, Hoerlin and Taschek began to press very hard for the experiment. On December 26 they suggested to Bradbury that the shot should be at 1,500 kilometers or higher. In a January 2, 1962, message Bradbury requested that Betts formally advise the Commission of the LASL desire to add to the proposed atmospheric test program an additional high-altitude experimental shot whose primary purpose would be a proof test of weapons diagnostic systems applicable to possible free space testing beyond the region of appreciable geophysical field coupling. His message gave some details of the LASL concept:

> Specifically, the proposed experimental shot would be conducted at Johnston Island using the same type of vehicle, presumably Thor, presently proposed for two DOD high-altitude effects shots. The planned altitude of detonation would be between 1,000 kilometers and 2,000 kilometers (the maximum Thor could reach). The nuclear warhead would be Ex.(b)(3) whose yield would already have been measured in an earlier part of the proposed series. Thus, the diagnostic techniques may be tested against a reasonably typical two-stage thermonuclear weapon of known yield.

Bradbury went on to comment:

> We also recognise that there may be some inevitable delay in determining the response to this proposal at the ultimate level of authority required. Since, however, delay would now preclude the inclusion of the experiment at all and be fatal to its success as well, we are proceeding with technical planning and preparation in the hope that it will eventually be found possible to approve formally this additional experiment.

Bradbury bolstered his case at the JCAE hearings on January 18-19, noting:

> The Laboratory is proposing a very high-altitude shot (Urraca) at 1,000 or 2,000 kilometers using a device of known yield as an experimental test of the diagnostic techniques of in-space testing should this ever become necessary or authorized. Initial studies indicate that this could be a quite powerful and not exceptionally difficult method of weapon testing to be carried out with warheads of reasonable weight and not too great distances from the earth.

Betts continued to avoid the issue. On January 18, 1962, he sent a message to the Laboratories asking for a review of the atmospheric test program and giving his understanding of the situation, but he did not mention the deep space shot. Sandia forced the issue by requesting two reentry vehicles and telemetry antennas from the Air Force, one of which was for the deep space shot now named Urraca. This request brought up the question of funding and led to a January 26 message from the Chief of DASA to DMA, which pointed out that since the shot was not yet approved the AEC should guarantee the funding for such equipment.

On February 6 Bradbury offered to give up the land-based measurement of the angular distribution of prompt radiation from the Ex.(b)(3) if it would help win the argument to get the very high-altitude shot. He commented that we could probably make a stab at getting the neutron spectrum on that shot.

By February 12 Betts had transmitted the LASL request for Urraca to Seaborg and Seaborg had sent it to the President, since on that day, Seaborg noted to McGeorge Bundy, "I have been informed that the DOD has run into problems in funding the high-altitude (1,000 to 2,000 kilometers) shot." However, by way of further confusion, on February 13 Betts authorized the Labs to make complete preparations for Urraca, noting that it had been approved by both AEC and DOD, and, hence, was likely to be approved when it was submitted to the President. There was one more hurdle to jump. Noting that Urraca had been added to the series, CJTF-8 requested an additional Thor since the Department of Defense felt that the five Thors previously requested should be reserved for Starfish and Bluegill and, of course, they still had Kingfish in

their minds. At the Commission meeting on March 7 (after the President had announced his intention to proceed), Gerry Johnson pointed out that there was a Urraca funding problem and asked if the AEC could purchase the $1,700,000 Thor rocket required. After some further negotiation, the AEC agreed to pay for the missile. Thus, Urraca became part of the intended Dominic series.

Another stir in the proposed program arose from LASL discussions with the Army concerning the Nike-Zeus warhead, which was to be the [redacted] Ex.(b)(1) Ex.(b)(3) Thus, in his November 20, 1961, message to Betts outlining the proposed LASL programs, Bradbury also recommended a high-altitude test of the Ex.(b)(3) at an altitude of 125 to 150 kilometers (which was about the intended use altitude of the Nike-Zeus) in order to make detailed output measurements. The shot was discussed by the NSC at their November 29, 1961, meeting during which it was concluded that the Ex.(b)(1) experiment (Bluegill) should partially satisfy the AEC interest in obtaining AICBM effects data at operational altitudes of Nike-Zeus. In studying the problem of how to measure the angular distribution, which was important because it entered into the question of whether the Nike-Zeus warhead had to be stabilized or not, the LASL test division had concluded that a better measurement of the angular distribution could be made using a ground-surface shot with appropriate instrumentation placed around the device. The problem of measurement of the angular distribution at high altitude would be very difficult because of the necessary placement of many detectors at appreciable distances around the bomb. On the other hand, the spectrum could be measured quite well at high altitude by time-of-flight techniques, although LASL also intended to measure it on the ground shot. Thus, on November 30 LASL requested that the Task Force begin searching for an island on which to do the ground test.

Apparently the DOD had not been aware at the beginning of the November 29 NSC meeting that LASL would propose tests of the Ex.(b)(3) warhead at high altitude. Introduction of the LASL proposal at that meeting caused some confusion, possibly because the DOD-proposed Bluegill Ex.(b)(1) kilometers. [redacted]

> In view of the confusion caused at our recent meeting by the sudden injection of a high-altitude effects shot sponsored by LASL, I feel it might be useful to discuss with you the DOD procedures and requirements in this area. As you are no doubt aware, there is a gray area in responsibility for weapons effects measurements between the Department of Defense and the Atomic Energy Commission. However, the division of responsibility has been, of course, to develop weapons and to make those diagnostic measurements that affect the performance and design of the weapon. On the other hand, the DOD responsibility has been to measure those outputs and the effects caused by them that are of military interest.

Brown suggested that LASL may have made the suggestion because of a request to make a specific effects measurement which may have been transmitted directly from the Army to LASL. Brown pointed out that a request from the Army does not constitute a requirement from the Department of Defense, and added that DASA, for the Department of Defense, had the responsibility of assembling and evaluating the services' requirements for weapons effects information. Thus, if any AEC organizations had an interest in a specific measurement, they should discuss that with DASA rather than with a military service. He commented that at the present the DOD did not feel the need for these measurements strongly enough to consider it as justification for a shot. Betts then suggested to Bradbury that the subject of output measurement

responsibility be addressed by LASL. Bradbury's response was in strong disagreement with Harold Brown's letter:

> It has always been previously assumed by LASL and, we believe, by the AEC that it was the responsibility of the AEC to develop weapons and make appropriate diagnostic and experimental measurements toward the subject and, in addition, provide, by calculation or direct measurement, the using agency with definitive information regarding the fundamental output of bombs. Specifically, when information on the x-ray or neutron output of bombs is required by the DOD, it is assumed to be the AEC's responsibility to furnish it.

He went on to quote many examples on both sides of the picture stating, for example, the effects of nuclear weapons on nuclear weapons were an AEC responsibility as was the determination of the actual emanations from a detonating nuclear system. Bradbury commented that LASL now intended to get the spectrum from measurements on the ground; if the ground shot was not practical, we would get what we could in an airdrop or possibly from a ship. He also commented that he did not disagree with the recent verbal statement by DDR&E that no one really needed this information as precisely as many of the subordinate agencies seemed to want it. He denied that LASL had made the proposal at the request of the Army and said, "If he will call off his dogs, we will agree not to go overboard in detailed spectrum diagnostics." The next day, Foster agreed with Bradbury's viewpoint, stating:

> Specifically, my understanding is that the AEC has a responsibility for warhead design and output including blast, x-rays, neutrons, and gamma rays. The 'gray area' is the transmission of these effects. The DOD responsibility is the response of military equipment and personnel to these effects. This position seems adequately covered in Starbird's letter to Dr. Brown, Director of LRL, on November 23, 1960.

Thus, the proposal for a 150-km, Ex.(b)(3) was withdrawn by LASL, and preparations for a surface shot to measure the Ex.(b)(3) continued for the next month and a half. But the pressure led to continued reduction in the project, and Brown was still unhappy about the idea of the AEC making such a measurement.

At the same time, LASL continued its arguments for Urraca, which was also bothering the DOD. Finally, Bradbury gave a bit, commenting on February 6, 1962, that:

> If it would help to strengthen the support for the 1,000-kilometer shot, we could probably say we could make fair neutron output measurements on this shot and thus abandon the need for a ground-based Ex.(b)(3) We would still obviously need an airdrop yield shot of this device. In other words, if something has to give, give on the Baker Island shot, making it into an airdrop, but save the high-altitude shot if at all possible.

Betts quickly took him up on that suggestion and the surface shot was canceled.

One other small flap in planning the series should be noted. On January 24, 1962, Betts informed the Laboratories that the Department of Defense had asked (a) what was the maximum yield warhead possible at the weight of the present Titan II reentry vehicle and (b) could either Laboratory have such a test warhead available for the Dominic series? Further information about the DOD concept was also provided: the Titan II would be the booster, the detonation would occur at an altitude high enough to avoid aerodynamic reentry loads, and consequently, the nose cone was required mainly for warhead protection during launch and exit from the atmosphere. Betts requested that ALOO forward a coordinated Laboratory reply within two days. On January 27 Glen Fowler of Sandia provided the following response:

The commitments which have already been made by the weapons laboratories for the Dominic operation are such that the idea of a special Titan II test on the same time scale is greeted with a somewhat low level of enthusiasm. However, if this program must be undertaken for good reason, there is only one approach that is practical, namely, to use the Mark VI RV/XW-53 warhead combination now being designed for the Titan II. To do this, LASL would have to provide a nuclear system and Sandia would have to provide a complement of warhead hardware as well as a special fusing system for the test. A command-enable timer or a direct command firing system similar to that being built for the Dominic Thor would be required. We estimate that this would add approximately 100 pounds to the standard RV weight of 7,564 pounds. With command firing, detonation altitudes would be limited by reentry ionization problems and could occur down to 200,000 feet. At or above this altitude, neither reentry deceleration nor temperatures would present a problem. Flash blindness consideration, however, would suggest that the detonation altitude should be much higher, like 400,000 feet. We believe that the necessary nonnuclear hardware could be built in time for a July flight, but consider that it would be likely to affect current effort on the XW-53 warhead development to some extent and also would be likely to cause some difficulties with the preparation for the Dominic Thor operations. The exact effect cannot be assessed without a more detailed study. The LASL components probably could be made available for a July flight date with similar program interference problems. All of the above statements apply to the use of the presently planned XW-53/Mark VI RV payload for the Titan II.

Ex.(b)(3)

Considerable design, test, and fabrication effort would be required and there is little doubt that such an effort would cause interference with both weapons development and Dominic effort. In Dominic time scales, the only available launching site appears to be at the Atlantic Missile Range.

There was no further discussion of the Titan suggestion. The program for Dominic was firm enough to issue the listing and schedule shown in Table XXX.

On March 2 the President publicly announced his decision to resume atmospheric testing, giving the readiness date as April 23, 1962. By mid-March, in response to Betts' request for additional test needs that might have come as a result of tests in Nevada, an Atlas system test and a Polaris system test were included in the schedule.

On April 11, 1962, the Commission met and approved the proposed nuclear test program, specifically including the Atlas and Polaris tests. However, L. K. Olsen, the Commissioner who had not been present at that meeting, noted that in reading over the staff paper he did not find proper justification for the shots, especially the Polaris and Atlas shots, which were justified only by reference to two letters which were not themselves part of the staff paper. On April 12 Seaborg forwarded the list to the President along with a note pointing out that (a) it should be considered a flexible list, (b) that the operation might extend into July, (c) that the Commission had not yet reviewed the operational aspects of the Atlas, and (d) that there was a finite probability of a malfunction which could lead to one or more of the missiles being destroyed in flight, probably burying special nuclear materials deep at sea.

On April 24 Starbird received from Betts a message beginning, "This message constitutes the authority to conduct the atmospheric nuclear test program as follows: On behalf of the Chairman, AEC, I am advising you that the President has approved execution of the atmospheric test program." The program referred to at the beginning of Dominic was as shown in Table XXXI.

Digression on Test Methods

One of the main reasons we were able to go back to testing, both underground and in the atmosphere in 1961 and 1962, was the vast experience of the technical

TABLE XXX
DOMINIC SCHEDULE
February 20, 1962

Ex.(b)(3)	Code Name	Sponsor or Laboratory	Ex.(b)(3)	Position	Date
	Chetco	LRL		GZ-10	04/02
	Adobe	LASL		GZ-10	04/04
	Swanee	LRL		GZ-10	04/06
	Aztec	LASL		GZ-10	04/09
	Questa	LASL		GZ-12	04/11
	Mesilla	LASL		GZ-10	04/13
	Yeso	LASL		GZ-15	04/15
	Yukon	LRL		GZ-10	04/18
	Arkansas	LRL		GZ-12	04/20
	Encino	LASL		GZ-12	04/23
	Swordfish	DOD		Christmas)	04/25
	Rosebud	LASL		GZ-12	05/01
	Truckee	LRL		GZ-10	05/03
	Tanana	LRL		GZ-10	05/05
	Nambe	LASL		GZ-10	05/07
	Petit	LRL		GZ-12	05/09
	Urraca	AEC		Johnston	05/19
	Muskegon	LRL		GZ-10	05/18
	Otowi	LASL		GZ-10	05/21
	Sunset	LASL		GZ-12	05/23
	Bighorn	LRL		GZ-20	05/25
	Bluegill	DOD		Johnston	06/01
	Calaveras	LRL		GZ-20	06/08
	Starfish	DOD		Johnston	06/15

personnel. The point should be made that their training and experience had given them a broad and deep understanding of the problems that had to be solved. The understanding required in development of the techniques that had been used in atmospheric testing was still fresh in their minds and that understanding could be used to develop new techniques in a new, higher altitude medium.

Specifically, the type of testing that we did underground, immediately after the moratorium in the fall of 1961 and the spring of 1962, was not a type of testing that had been done before or with which we had any particular experience. Los Alamos had fired three underground shots before 1961, that is, Jangle, which was buried at 17 feet, and Pascal A and B in Hardtack Phase II, 1958, which were a few hundred feet underground. In these tests the diagnostics were not particularly critical to the experiment. Certainly in the case of the Pascals--the two Pascal shots came roaring out of the holes like a rocket and allowed normal radiochemical sampling with airplanes in the cloud--the actual results were so far different from expectation that the diagnostics were not critical. To illustrate this point in more detail, the basic measurements made on development shots fired in the atmosphere in Nevada were the reaction rate as a function of time, the yield of the bomb as determined by the fireball expansion, and those data that could be derived from radiochemical analysis of samples collected from aircraft fitted with sampling tanks and flown through the debris cloud at fairly late times. The range of intensities to be dealt with in

TABLE XXXI
DOMINIC SCHEDULE

Ex.(b)(3)	Code Name	Sponsor	Ex.(b)(3)	Position	Height Drop (ft)	Ex.(b)(3)
	Adobe	LASL		GZ-10	45,000	
	Aztec	LASL		GZ-10	45,000	
	Arkansas	LRL		GZ-12	35,000	
	Questa	LASL		GZ-12	45,000	
	Yukon	LRL		GZ-10	25,000	
	Frigate Bird	DOD[a]		b	---	
	Mesilla	LASL		GZ-10	45,000	
	Muskegon	LRL		GZ-10	25,000	
	Yeso	LASL		GZ-15	45,000	
	Encino	LASL		GZ-10	45,000	
	Swanee	LRL		GZ-10	35,000	
	Rinconada	LASL		GZ-12	45,000	
	Nambe	LASL		GZ-10	45,000	
	Chetco	LRL		GZ-10	25,000	
	Swordfish	DOD		b	----	
	Sunset	LASL		GZ-12	45,000	
	Tanana	LRL		GZ-10	25,000	
	Bluegill	DOD		c	---	
	Otowi	LASL		GZ-10	45,000	
	Petit	LRL		GZ-12	25,000	
	Starfish	DOD		c	---	
	Bighorn	LRL		GZ-20	45,000	
	Truckee	LRL		GZ-10	25,000	
	Urraca	AEC		c	---	
	Calaveras[c]	LRL		GZ-20	45,000	

[a]Operational Firing
[b]Christmas Danger Area
[c]Johnston Danger Area

Ex.(b)(3)

measuring the reaction history is very great, from essentially as low an intensity as one can measure with very sensitive detectors to the peak of the intensity curve, a range, in some cases, of as much as 10^{30}. In earlier atmospheric testing that range of coverage was accomplished by using detectors of similar sensitivity placed at several distances from the bomb. A common array, for example, might have 10 to 15 detectors (or even more) with the closest detectors in a tower or a balloon cab essentially right up against the bomb and the most remote detectors perhaps 1,000 or 1,500 yards away. Signals from these detectors were then run through very fast cable to recording stations for enough away to survive the blast, say 1,500 to 2,000 yards from the bomb. For the underground tests, because of the containment requirements, the same experimental philosophy could not be used. The close-in detectors could be placed essentially as in the past, as long as the signal cables were placed approximately along a radius from the bomb. Initially the maximum distance of a

detector was limited by the size of the largest usable canister to about 40 ft. This limitation led to the use of insensitive detectors and great amounts of shielding, which, in turn, led to great cable troubles because the cable itself becomes a detector at not very much higher radiation level than the intended detector itself. Thus, the initial underground shots often failed to give records of the high-level gamma data, and it was some time, perhaps a year or so, before new systems were developed to get all the desired data. (Initially scatterers were used; the eventual solution involved precisely the physical effects that caused problems in the early underground tests -- in the so-called Compton diodes invented by L. K. Neher of LASL.)

There was a similar problem with radiochemistry. In atmospheric testing we depended upon "complete mixing" in the cloud, which meant the cloud was allowed to turn over and mix within itself for some time, an hour or so, before sampling was attempted. When sampling was attempted samples were taken from various parts of the cloud in order to ensure they were representative. In underground tests, this process could no longer be used. Whether any sample obtained from the melted and resolidified pool of rock would be representative was a serious question. Initial attempts to allow radioactive gas to flow through small pipes to a gas collection system on the surface were not very successful because of strong fractionation in the piping system, if any sample at all was obtained. Again, the initial results were most unsatisfactory, and it was only after appreciable experience in drilling, sampling, and treating the samples appropriately that satisfactory results were obtained.

Similarly, when the possibility of returning to atmospheric testing arose in the fall of 1961, it became clear almost immediately that this would not be the atmospheric testing of old. Testing at Eniwetok and Bikini from 1954 to 1958 had resulted in the development of well-defined procedures. Diagnostics for most devices fired at those sites in that period consisted of reaction history, yield, and whatever data on the internal workings that could be inferred from radiochemistry. Reaction history was obtained in a manner similar to that described for Nevada, using detectors located at several distances from the device which was placed occasionally on towers or on the ground, but most often on barges, at some distance from a land recording station. The use of barges allowed firing of quite large shots without producing large craters and without permanently disturbing the islands. About one out of eight shots was thoroughly diagnosed, including attempts at real-time determinations of some details of the workings inside the so-called hydrogen bombs. These attempts, in general, involved complex arrays of instrumentation on small islands, with high-speed cables running into detector systems some distance away.

In the fall of 1961, when we were told to prepare for testing, the political ground rules imposed quickly made it clear that the earlier techniques could not be used, at least not in the earlier form. Even the testing location was not known, although the most likely site seemed to be open ocean somewhere south of Hawaii. The method of bomb emplacement had not been settled; whether by airdrop, which meant very careful packaging of experimental devices to be handled in that manner, or by emplacing the devices on ships that could be destroyed. Furthermore, it was politically clear that we would be testing only devices of fairly large-yield, at least in design, and the smaller tests would be done in Nevada. Therefore, from a diagnostic point of view on the AEC tests, it was accepted very early on that we would attempt to measure only the yield and a very small portion of the reaction history, as well as, once again, obtaining what data we could by inference from radiochemical samples. When preparations for Project Everready began it had been assumed that bhangmeters would be used to measure yield. However, it was also known that there were essentially no bhangmeter calibration data from large bombs fired in the air far above ground level: even the calibrations we had were suspect. Thus, an early requirement

was measurement of the fireball growth rate. We had up to this time fired only two large-yield devices by airdrop: one, the 500 kiloton King shot on Operation Ivy and the other the 3.9 megaton Cherokee shot of Operation Redwing. In both cases, we had island stations at which to place fireball cameras, even though this system did not work on the Cherokee shot because the bomb was dropped out of the field of view of the cameras. A flexible system was required, that is, one with some kind of tracking, if possible, or at least some way to tell the bomb position at the time of detonation. An essential requirement of the fireball method is to know the distance from the camera to the detonation point in order to determine accurately the diameter of the fireball. Because the yield is proportional to the fifth power of the fireball diameter, if a yield measurement accurate to within 5% is desired, the diameter (and hence, the distance) must be accurate to within 1%. Thus, it is immediately clear that if the distance cannot be measured to better than 4 or 5%, then the fireball method of yield determination is not much better than bhangmeters or anything else.

Since this problem was recognized at the inception of the Everready program, plans had been made for installation of fireball cameras in C-130s and development of distance measuring equipment (DME) had begun. However, it was fairly clear to the people responsible for this measurement, primarily Art Cox of LASL, that these were very unreliable systems. This need for accurate distance determination was one of the critical items in the technical judgment that it was necessary to move to Christmas Island. Moving to Christmas Island would provide a stable base for proven radar equipment capable of tracking beacons placed on the drop device as well as in the bomber. From these instruments it was possible to have the cameras pointing at the bomb at the time it went off and measure the distance with sufficient accuracy to provide the desired accuracy in the yield measurement. Once again, this problem was comparatively new to the testers. In earlier operations using either barges or towers, the distance measurement could be inferred from surveying data acquired before the test: for airdrops in Nevada there were sufficient camera stations at well-known points that ordinary triangulation could give the distance with required accuracy. But doing this measurement from floating or airborne platforms in the middle of the South Pacific was an entirely different problem.

For measuring the small piece of the reaction history required for the atmospheric portion of Dominic, there was plenty of experience on previous operations. However, for those operations the measurements were made from roomy ground-based stations, for which Malik, Wouters, Partridge, and Theobald had developed quite satisfactory systems capable of successful measurements from appreciable distances. Installation of these systems in aircraft or on the decks of ships led to design changes imposed by limitations of tracking, antenna space, and electrical power. Thus, the systems that were put into the Everready C-130s to make this measurement were questionable and untried. Consequently, again from an experimental point of view it was preferable to go to Christmas Island and use dependable ground-based systems while learning how to make airborne systems dependable.

There is a fundamental point of AEC nuclear weapons testing that should be made. The experiment is the device; the experiment is something that is being conducted by the developmental portion of the Laboratory. The field forces are making measurements; they are not doing experiments. Thus, they seek high reliability in making those measurements and if they wish to try some new method of making an old measurement, then that new method is usually done in parallel with an old method and done many times until the bugs can be worked out and the new method becomes as reliable as the old one. This philosophy is pertinent to the development of nuclear weapons, but it is a somewhat different philosophy than that which applies when atmospheric effects measurements are to be carried out for atmospheric explosions. For example,

one set of instrumentation, and one site properly instrumented, can be used for detonation-after-detonation for development shots, where the object is to assess the device performance. On the other hand, if one wishes to conduct effects tests, such as the high-altitude shots in Dominic, then it may be necessary to change the basic instrumentation from one test to the next. It is certainly necessary to design the measurement specifically for a particular shot, since in an effects test there should be no question about the device performance. The question is, rather, what are the effects of that explosion on the ambient environment: it is the effects on the environment that are to be measured, not the inner workings of the device.

Early Preparations

The beginnings of Project Everready have been discussed in the program section. However, other moves toward atmospheric testing also took place very early in September 1961. The discussions between President Kennedy, Seaborg, and McNamara in the first days of September clearly raised again the possibility of atmospheric testing. As a result, both the AEC and the Department of Defense began to make arrangements for that possibility should the President wish to move in that direction. In addition to high-level consideration of this possibility, it is also clear that many people in both the AEC and DOD began such moves because of internal convictions or desires to solve specific problems, and, in some cases, just to prevent embarrassment later.

On the AEC side, as noted before, Reeves, as a result of the September 13 Planning Board meeting, had authorized H&N to determine the status of the Eniwetok Proving Ground, and they were encouraged to send a small survey party there as soon as permission could be obtained from Washington.

At the same Planning Board meeting the LASL-proposed high-altitude Nike-Zeus warhead test was mentioned, and Johnston Island became a consideration. By September 29, 1961, Sandia had prepared a report on the possibilities of high-altitude testing which included delivery systems, possible launch sites, modifications of reentry vehicles, fusing and firing systems, diagnostic techniques, and safety. The report recommended the use of a Thor missile launched from Johnston Island, outlined a complete fusing system, including warhead destruct circuits, and suggested a tracking system. The report also included drawings showing the Ex.(b)(3) Ex.(b)(3) mounted in the modified Thor reentry vehicle. Sandia estimated that the first shot could be ready in six months.

The effort that was to become Project Everready led to the development of many tools which were used later in Operation Dominic. As early as September 20, 1961, LASL asked Sandia to prepare some device drop cases, and by September 29 Sandia had developed a plan to provide (a) a universal test vehicle capable of carrying any of the current untested warheads and (b) the necessary instrumentation and support to obtain reasonable diagnostic information. They proposed that airdrops at the Eniwetok Proving Ground be made from a B-47 or B-52 aircraft instrumented with radars and fireball cameras. The Mark 39, Mod 1, Type 3 (Trainer) bomb case would be adapted to the Ex.(b)(3) or other warheads. Appropriate fusing was arranged to preclude ground burst. This system, Sandia said, could be ready for its first test in two months. Other equipment soon appeared. While the first concepts of Project Everready involved the simple drop of a stockpile device off the west coast, discussions in late September quickly solidified into an operational plan that might give the Laboratories at least minimal diagnostics. In this plan the B-52 itself would be equipped with a fireball camera and a bhangmeter to measure yield. Initially one, but later two, C-130 aircraft would be equipped with optical and electromagnetic gear

to measure the interstage times, and later they would also have fireball cameras. Sampling support would be provided by the B-57s now assigned to Air Weather Service. The shots could be located either at Eniwetok atoll or over the open ocean near Johnston Island or Hawaii.

On October 7, 1961, Betts told the Laboratories to be ready to conduct three airdrop tests by December 1, and a mad scramble began in the Laboratories and at AFSWP. The plans group of AFSWP sent McCorkle (AFSWC) an initial concept on October 8:

> The ground rules are that the series would be of relatively short duration with deployment to Hawaii within 10 days after authorization. The concept of operations is for four war-reserve weapons to be dropped from a B-52 aircraft under the control of AFSWP with the detonations to be over the open seas approximately 350 nautical miles southeast of Hilo. Three days of practice missions will precede the first drop which will be done by an airborne Task Force consisting of a drop aircraft, airborne diagnostic instrumentation, photo, command and control, weather reconnaissance, and air-sea rescue aircraft. The Task Group Headquarters will be at Hickam, with all aircraft staged out of the Hawaiian area. The command and control or AOC aircraft will be an RC-121 and a possibility for the diagnostic aircraft is an instrumented C-130 from AFCRL.

Also mentioned was the possibility of getting C-130s from the Air Photographic and Charting Service. By October 9 Sandia had modified two Mark 39 drop cases and was ready to drop the units from a B-47 at the Tonopah test range. However, on that same day, a meeting at Sandia between AEC and Air Force personnel recommended a B-52 instead, so the drop, which had by then been rescheduled for October 10, was canceled. On October 13 Headquarters Air Force directed Tactical Air Command (TAC) to deliver two C-130 aircraft to Kirtland not later than October 17 and 25. Air crews would be provided by AFSWP. Headquarters also directed Air Defense Command (ADC) to provide RB-57D aircraft for the air sampling mission and arrange that they arrive at Kirtland not later than October 17. The samplers would be maintained and operated by the Air Weather Service. AFSWC was directed to assist in the aircraft modifications and was designated as the Air Force point of contact and control for this program. Within the Laboratories, arrangements were quickly made for LASL to be responsible for one diagnostic aircraft and Livermore the other. EG&G were to arrange for fireball cameras and timing gear as soon as possible and the Laboratories took responsibility for time-interval and other instrumentation. AFSWC took on the job of preparing appropriate DME gear. The equipment initially installed in the 130s was whatever the Laboratories could find in-house, and some of it was primitive.

By mid-October Livermore had decided that they could be ready to drop a Ex.(b)(3) as early as November 15, but equipment had to be installed in the 130s. Although it had been estimated that the installation would take several months, by October 28 it was intended to have the 130s completed by October 31, allowing an initial airborne diagnostic capability to be ready by early November 1961.

The rest of the DOD also began to prepare for atmospheric testing in September. In addition to the September 19 letter transmitted through Seaborg to the NSC (mentioning the possible need for atmospheric testing), as noted before, DASA, on September 20, apparently in reaction to a September 12 memo from Harold Brown, authorized funds to address different types of measurements that might be made with high-altitude tests, including ionization caused by high-altitude detonation of nuclear weapons.

In spite of the feelings that politics would preclude its use, the Eniwetok Proving Ground was not forgotten. At a meeting of AEC, Laboratory, and H&N representatives on September 20 LASL described a quick two-month airdrop operation including ground-based measurements of yield and time interval, and Livermore described its tests, which were to be done from barges. H&N pointed out that (a) the

EPG base camp buildings would need electrical work, but were otherwise in good condition, (b) there were five cargo barges at Eniwetok which could be used as shot barges, and (c) they could obtain sufficient manpower quickly, but the necessary supplies of equipment and material would be a serious problem. Sandia suggested the possible use of balloons for the LASL and Livermore shots.

Bikini, however, would be a more serious problem because essentially all equipment and material had been removed to Kwajalein to support the Nike-Zeus program. The general conclusion seemed to be that a quick operation of a few shots could be done in two or three months, but that a better operation would require six to nine months preparation. This conclusion was confirmed at the Planning Board meeting on September 27, 1961. Subsequently, Sandia prepared a detailed proposal for balloon shots at Eniwetok using aerodynamic balloons (they had not finished the testing of these during the moratorium). The time required to prepare for balloon shots on both atolls was estimated to be six to eight months.

More Political Considerations

James Carr, Acting Secretary of the Interior, in a letter to Chairman Seaborg on November 3, strongly recommended against any further testing of atomic devices in the Trust Territory.

Kennedy kept trying to avoid atmospheric testing. On September 3 he and British Prime Minister Macmillan jointly proposed to Khrushchev a ban on atmospheric testing, to include monitoring by national means. On September 9 Khrushchev refused the proposal, calling it a "dishonest deal" since the U.S. had been preparing for underground testing and knew how to do it. On September 19 Seaborg recommended to the President that the EPG be brought to a three months readiness posture and that a seaborne operation be considered. On September 25 the President, in an address to the U.N. General Assembly, said, among other things, "a nuclear disaster, spread by wind and water and fear, could well engulf the great and the small, the rich and the poor, the committed and the uncommitted alike. Mankind must put an end to war or war will put an end to mankind. Let us call a truce to terror." He called for disarmament, stating that the logical place to begin was a test ban treaty.

By the end of September 1961 awareness of the need for atmospheric testing was growing. September had been a period of recovery from shock and early moves toward preparation for underground testing, but October saw accelerated preparations for atmospheric testing. Schlesinger has noted:*

> The urgencies of security, however, remained at war with the dreams of disarmament. Kennedy had felt that the Soviet atmospheric tests left him no choice but to authorize underground testing of our own. Now, as one explosion in the skies above Siberia followed another through the autumn, it became increasingly difficult to hold the line at underground tests. The Joint Chiefs of Staff, in particular, wanted to resume American tests in the atmosphere as speedily as possible. Early in October, they forwarded a paper calling for atmospheric testing in November. The JCS paper was below their usual level in logic and literacy. When we met to consider it at the State Department, Secretary McNamara, who had obviously not examined it with care before the meeting, quickly perceived its imperfections and abandoned it as a basis for argument. One defense official made an impassioned case for the resumption of atmospheric testing in order to prevent the world from believing that the Communists were gaining so commanding a lead that there was no point in resisting them further. But McGeorge Bundy replied that he was against tests for the sake of psychological warfare and insisted on the principle that we never test

*A. Schlesinger, *A Thousand Days*, page 486.

in the atmosphere unless required by military necessity to do so. Then McNamara made it clear that a serious case for resumption existed in terms of military security, and the meeting ended with the recommendation that the United States take an early occasion to reserve its freedom to test above ground.

Gerry Johnson was quick to support Everready, and in the process triggered off a move that was to have major consequences in the operation. On October 2, after having talked to Kenner Hertford of ALOO and General Donnelly of Field Command, DASA, Johnson told Seaborg that he liked the Everready concept. The subject was discussed at the MLC meeting the next day, with both Betts and General Booth of DASA present. In discussing possible sites, Booth mentioned that DASA was looking into the possible use of Christmas Island, and H&N was looking at Eniwetok revival. Johnson pressed for identification of an alternative to the EPG because of his strong feeling that U.N. political pressures would make its use untenable. On October 10 Commander Holkum of DDR&E briefed Johnson about Christmas Island. Johnson apparently discussed the possibility with Betts in the next day or two and with Hertford, since Hertford later commented to Betts that if the Everready plan were accepted, it could also be adapted for operations from Christmas Island. On October 13 Booth sent Johnson a lengthy report on operational logistics on Christmas Island which had been prepared in 1959 by the Pacific Missile Range. Along with the report, Booth wrote:

It appears that the adaptation of Christmas Island as a U.S. nuclear test site is both operationally and logically feasible. Christmas Island affords sufficient advantages to make it attractive as a base for sampling operations, balloon shots, and offshore detonations with onshore instrumentation.

He also stated his view that the selection of Christmas Island as a nuclear test site was second to Eniwetok, with which he was more familiar, and suggested that a survey of Christmas Island be conducted immediately. Later that same day DMA and DASA staffs decided to survey Christmas Island and suggested that the survey party include representatives from the DOD, the AEC, and the United Kingdom. The relative merits of the different facilities would be made after completion of the survey. Should use of the Christmas Island facilities be desirable, the DOD, AEC, and Department of State would jointly decide (a) how to approach the United Kingdom, (b) the desired U.K. participation in the tests, and (c) what information from the tests would be made available to the U.K.

The subject of test location arose again at the October 17, 1961, meeting of the Commission during Bradbury's discussion of the need for prompt resumption of atmospheric testing. He commented that Christmas Island might be another possible test site and added: "Since it is a British mandate, it would require the cooperation of the United Kingdom." [redacted]

Ex.(b)(1)

Ex.(b)(3)

On October 18 DASA sent to Gerry Johnson a list of suggested personnel to conduct the upcoming survey visit to Christmas Island. The list consisted of eleven military officers from the three services, including one doctor. The subject came up again in the Commission meeting on October 19, which was attended by the members of the MLC. In discussing the possible operations, Johnson explained to the Commission that Christmas Island had better weather conditions than Eniwetok and a larger, fairly level, land mass. He noted that Christmas Island was apparently disputed territory between the U.S. and U.K., but also commented that the U.S. had a big investment at Eniwetok.