Ex.(b)(3) [redacted]

Ex.(b)(3) [redacted]

Weapons Test Operations

By the time of the Test Moratorium in 1958, several nations had tested nuclear weapons. The testing methods varied appreciably, both between nations, and, over that period of time, for United States tests. This section will attempt to outline, briefly, that history up to the moratorium, giving the methods of testing, why those methods were used, and what it was we were trying to accomplish.

The first nuclear weapons test in history by any nation took place in July of 1945, close to Alamogordo, New Mexico. The purpose of the test was to determine whether a spherical assembly system, developed at Los Alamos over the previous couple of years, would actually produce a significant chain reaction that would result in appreciable explosive energy. The device was the so-called Fat Man, [redacted] Ex.(b)(3) The uncertainty in whether the device would work or not was sufficiently great that it was felt worthwhile to use a large fraction of the nation's separated plutonium to test the device, rather than immediately using it in warfare.

Initial estimates of the phenomena to be expected led the Laboratory to fire the device on a tower in order to reduce the fallout and to allow somewhat better observation of the visible phenomena than would have been feasible had it been fired directly on the ground. Estimates had been made by members of the Laboratory as to the phenomena to be expected, that is, blast pressure as a function of distance, light intensities, gamma-ray intensities, neutron intensities, etc. Trinity was the

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*It is the author's memory that at the last Staff Member Meeting before the Trinity shot the various senior members of the Laboratory gave their estimates as to what the yield might be expected to be. The highest number the author remembers hearing was 7 kilotons.

most highly instrumented shot for output phenomenology that the United States was to fire for a large number of years. In restrospect, the measurements having to do with the effects of the detonation were probably appreciably more complete and advanced than those measurements having to do with the detailed working of the device itself. Presumably, this shot could have been fired as an airdrop, but obviously, the detailed measurements required would not have been feasible. The device went at some 20 kilotons, much to the pleasure of the designers. The effects, in particular fallout, were sufficiently noticeable to show that testing in that manner, in a region as highly populated as southern New Mexico, should be regarded with a jaundiced eye in the future.

It is perhaps worthy of note that early plans called for detonation of the Trinity device in a large, cylindrical container, called Jumbo. If the shot were to fail, it was supposed that this would allow recovery of the plutonium. However, after realization of the small likelihood of containment, and the difficulty of recovering the plutonium, which would be mixed with all the other bomb debris, the plan was abandoned. In a sense then, the first test planned by the United States would have been a contained clandestine test in the event of failure.

The Hiroshima and Nagasaki detonations (August 5 and 9, 1945, Greenwich time, respectively) hardly fit into the category of this history since they were not tests but were wartime attacks on a foreign nation. However, from the academic point of view of methods of testing, it is notable that these were airdrops and that there was a small amount of primitive instrumentation to determine that at least the weapons performed. The B-29 drop aircraft was accompanied to the target by two other B-29s, one with some instrumentation on board and the other to take photographs. The Los Alamos scientists did not feel it necessary to test the gun weapon used at Hiroshima (Little Boy) before its combat use since the Trinity test of the implosion device (Fat Man) offered some guarantee of the correctness of the calculations and the detonation mechanism was more predictable. In essence, if the Fat Man went all right (as it did at Trinity), the Little Boy was bound to.

The next United States nuclear weapons tests, Crossroads, in 1946, were really not tests of the nuclear weapons, but tests of the effects of nuclear detonations on ships, specifically on ships in harbors. Hence, the site chosen was chosen for the effects purposes and had nothing to do with weapons diagnostics. Crossroads saw the detonation of two more Christy devices, essentially identical to the Trinity and Nagasaki shots. The weapons diagnostics were therefore designed to show any differences between the Trinity shot and the Crossroads shot. On Crossroads Able fireball measurements were made from land-based cameras, which presumably would allow comparison of the fireball expansion with the Trinity shot. A measurement of neutron flux as a function of distance was made in order to compare with similar measurements at Trinity. On Crossroads Baker only a measurement of the high explosive transit time was made. Radiochemical analysis was made of the debris on both shots.

The first peace time airdrop of a nuclear weapon was Crossroads Able at Bikini Atoll. The measurements suffered from two problems. First of all, the timing of the detonation vs. the measurement timing was off by a number of seconds and caused certain data to be lost and, perhaps more importantly, the detonation took place about 700 yards from the planned zero point which caused certain instrumentation to be mislocated as to field of view and distance from burst. However, the variety and quantity of instruments made these problems not so serious as they might have been. The second Crossroads test, Baker, a detonation 90 feet below the water surface, provided much useful data on an underwater burst and its effects on various types of ships at varying distances. The shots generally showed that Navy vessels were quite resistant to nuclear blast, but the danger from the radioactive water was demonstrated to be quite impressive and was a serious problem to ships.

Ex.(b)(3)

[redacted]

Figure 1.
Trinity tower cab. Fat Man device with N. E. Bradbury facing camera.

Thus, the first new nuclear weapon device test following Trinity came with the Sandstone series at Eniwetok* in 1948. The long period of time from Trinity to Sandstone probably reflected the uncertainty in this country as to the wisdom of further weapon development and certainly reflected the trauma at Los Alamos following the war, during which a large proportion of the senior staff left the Laboratory.

At that time, 1947-1948, there was not yet serious consideration of a permanent proving ground, so Eniwetok was picked on a one-shot basis for Sandstone. It was still not thought feasible to fire nuclear weapons in the continental United States and the Crossroads operation had made the testing organization familiar with atoll operation. Thus, since Sandstone was intended to be largely a ship-based operation, Eniwetok was chosen because of the comparatively good transportation through Kwajalein, the fact that it already had an airstrip and a number of facilities that could be used, and was under the control of the United States. It was necessary to remove

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*Ed. note: The currently favored spelling is Enewetak, but the editors have elected to preserve the author's spelling, which was the officially recognized spelling during the years of atmospheric testing.

the Eniwetok natives. At that time, the natives had great faith in the wisdom of the United States representatives and, furthermore, had no recourse except to accede to the appropriate suggestions. (We had already moved the Bikini natives off in order to conduct the Crossroads operation.)

The use of short towers, a la Trinity, allowed similar diagnostic measurements to be made; the methods of diagnostics were now better understood and were advanced over those of Trinity. (See section on Diagnostics.) In addition to the measurements made by the test organization to diagnose the performance of the devices as well as making certain measurements of the outputs of the devices, there were effects measurements made mainly by military project teams. (See section on Effects Experiments.)

Operation Sandstone led to the concept of the development of Eniwetok as a proving ground, and was the serious beginning of the education of the crews of people who would then conduct fireball measurements, radiochemical measurements, and reaction rate measurements over the next ten years. As such, it began the training of those people who eventually established our readiness capability during the moratorium. Sandstone was, however, a simple operation from the point of view of the diagnostics required, the weapon principles being tested, and the operational problems involved, as compared with later operations. The shots were sufficiently small that the fallout was no serious problem except locally; however, the yields were large enough (49, 18, 36 kt) that firing those shots in the continental United States was thought at that time to be unwise.

Operation Greenhouse, in the early part of 1951, was, with the possible exception of Trinity, the most far-reaching and complex diagnostic operation in the period before the moratorium. In retrospect, it was probably the low computer capability as compared with today and perhaps the lack of the sense of urgency nationally that led to the comparatively long period between Sandstone and Greenhouse. At that time, at Los Alamos, there was a general feeling that a series of a few shots every two years probably adequately matched the design and postshot analysis capability of the Laboratory. Furthermore, the design and construction of the diagnostic systems, especially for George shot, was very time consuming. [redacted] Ex.(b)(3) Another was to furnish the Department of Defense with their first land-based opportunity for serious and detailed effects measurements. Most importantly, however, this series had two shots (George and Item) which were the first attempts to produce and investigate thermonuclear burning. [redacted]

Ex.(b)(3)

However, in late 1950, after the planning and construction and procurement were well along for Operation Greenhouse, the need for accurate yield predictions for Greenhouse led to the decision to launch a series of nuclear experiments on the design of the fission weapons to be used on Greenhouse, since that design now involved theory that went far beyond any past experimentation and, thus, there was some serious question as to the accuracy of the calculations.

Thus, it became necessary to mount a "quick and dirty" operation, called Ranger, to test these principles, Ex.(b)(3) [redacted] There was no way in the short period of time available to construct the proper facilities overseas to test these devices in the manner that had been become normal, that is, firing on short towers with fairly complex instrumentation. Since the device to be tested was in the stockpile stage with respect to high explosive and there was now enough experience in diagnostic techniques, it became clear an airdrop operation could produce the diagnostic results necessary. There was also now enough understanding of fallout to be able to predict that if the devices were fired at sufficient altitude, and the yield kept low enough, the fallout would be at sufficiently low levels that the operations could be safely conducted in the continental United States. A quick survey of possible sites in the United States led to the choice of a portion of the Air Force Gunnery Range, northwest of Las Vegas, known as Frenchman Flat. In short order, a zero point was chosen at Frenchman Flat and an alpha station designed and constructed. Alpha, the exponential rate of growth of the nuclear reaction, was measured using ion chambers on the ground close to the alpha station. The airdrop target was a cross of lights placed appropriately within the alpha detector array to allow the best coverage. Fireball cameras for yield measurements were placed at a quickly constructed control point, some seven miles away from the zero point, and on a nearby hilltop. Radiochemical

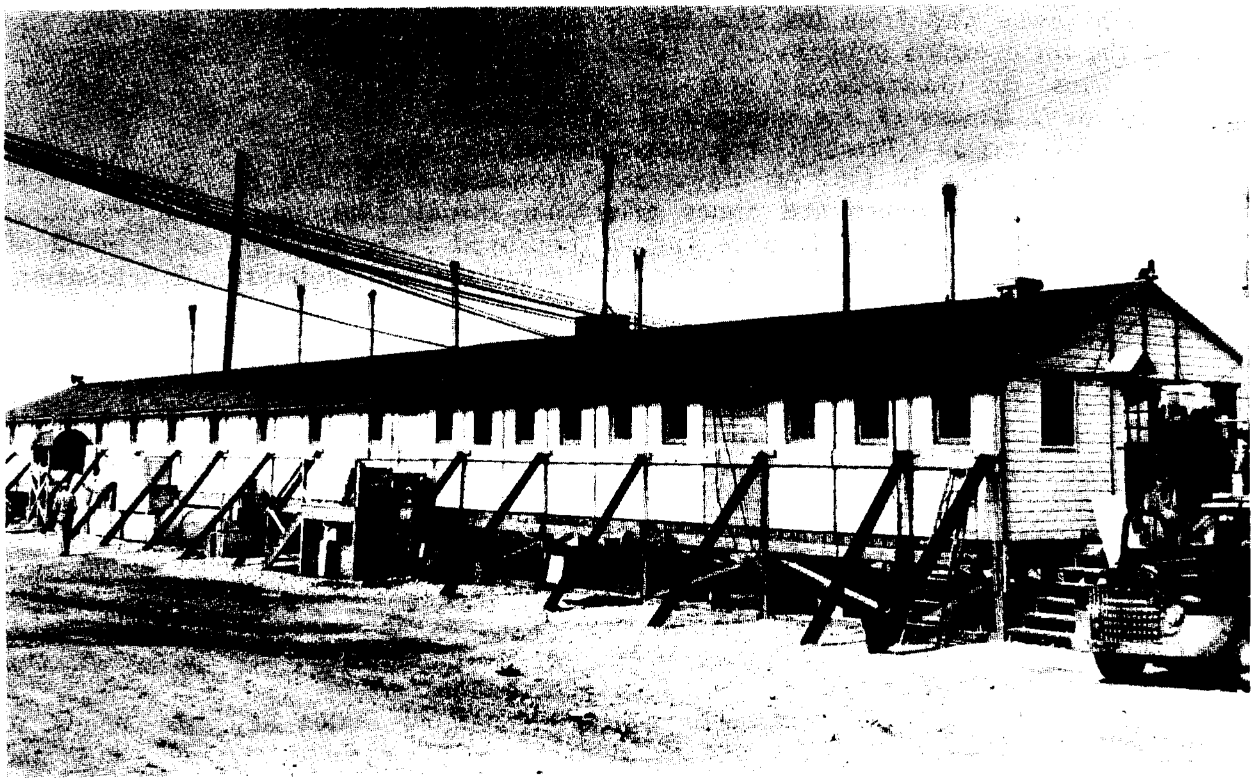

Figure 2.

Ranger control building--Frenchman Flat--Nevada Test Site. Note the shoring found necessary after the first shot to keep the building from collapsing.

sample collector planes were based at Indian Springs. The administrative portion of the operation was kept in Las Vegas, some 70 miles away.

Thus, within a period of some eleven days (January 27 to February 6, 1951), five airdrops were made, allowing the data to be collected that led to the proper decisions for the Greenhouse devices. The time from conception of Operation Ranger to completion was approximately two months and the total operational cost to the AEC was some $3-1/2 million.

Thus, the combination of Operations Ranger and Greenhouse in late 1950 and early 1951 saw the beginnings of the thermonuclear burn weapon and the diagnostic techniques that went with it, the beginning of high-temperature x-ray measurements, the further development of reaction history techniques, and the proving of the operational concept of airdrops for experimental devices that was to be used so much in Nevada in later operations. These operations also tested the ingenuity of, and contributed strongly to the training of, those people who were to carry on in the later 1950s and establish the capability that carried us through the moratorium. Greenhouse also saw the first strong participation of UCRL in the weapons test programs. That Laboratory had, of course, contributed during the war years to all of the facets of the effort that later produced the atomic weapon.

The comparative ease and speed of the Ranger operation, combined with the growth of ideas for new and smaller devices, and the need for such devices as expressed by the military, led to the suggestion that a permanent proving ground for small-yield devices be established in Nevada. The experience of Ranger, in which the alpha blockhouse had been practically buried by fluffed-up dirt around the target area, led to the design of a more permanent installation in Yucca Flat. The firing site was moved from Frenchman Flat to Yucca Flat to get further away from the Las Vegas-Tonopah highway and from the new service town, Mercury. Time for planning also allowed the introduction of many more measurements, mostly output or effects measurements. Thus, the Buster-Jangle operation of the fall of 1951 in Nevada saw the continued development of the methods of conducting an airdrop operation against a fixed ground target. Fireball measurements became more sophisticated, especially with the introduction in 1952 of "Rapatronic" cameras, which took microsecond exposure pictures at predetermined times after the initial explosion. Radiochemical sampling and analysis methods were further improved. The growing Department of Defense need for effects data led to the Jangle surface and underground detonations for effects measurements.

The establishment of the Nevada Proving Ground in 1951 thus allowed a change in testing philosophy. Small devices (up to approximately 60 kt) would be fired in Nevada with the resultant saving in effort and money over the comparatively massive Pacific operations, and the Pacific would be used for tests of those devices that could not be safely fired in Nevada. There were, of course, exceptions to the latter part of this philosophy. A number of small shots were fired in the Pacific in later operations, because the sponsoring Laboratory did not feel it wise to wait for the next Nevada operation. With certain exceptions, a pattern grew of an operation once a year (1952-1958) alternating between the Pacific and Nevada. [redacted]

Ex.(b)(3)

The pleasant state of affairs in which the United States had "the bomb" to itself had ended in 1949, when the Russians tested their first device. While the concept of a thermonuclear device had been extant almost from the beginning of the Manhattan District project, work tended to concentrate on the fission device. The critical decision to accelerate the development of the thermonuclear weapon had been made by President Truman at the end of January 1950 under various political pressures including the strong desire of some to take another quantum step past the Soviets. The Greenhouse series had now demonstrated thermonuclear burn and opened the way to approaching a full-scale thermonuclear device, but it was not until the fall of 1952 that the first thermonuclear device was ready for a full-scale test, which Truman approved just a few days before the Presidential election.

The first full-scale thermonuclear shot was of the Mike device of Operation Ivy on the island of Elugelab on the first day of November 1952 (Eniwetok time). Since the yield was expected to be large, new operational techniques were introduced. Most importantly, the firing control was aboard ship and the entire atoll was evacuated for the detonation. A month or so before the detonation, Edward Teller estimated that there was a remote chance that the yield might be as much as 100 megatons. The recognition of the possible effects of such a large yield led to the early calculations on the possible production of tsunamis (long wavelength ocean waves) and the need to take precautions in case they were produced. (Measurable tsunami waves were observed in Hawaii for most megaton-range Eniwetok Proving Ground shots, and this effect entered seriously into the safety considerations for the 1962 Pacific operation.) The expected yield of Mike also led to the requirement for higher-altitude wind predictions and soundings than previously needed, and the accompanying fallout predictions. (Mike was fired on the planned date, October 31 (CONUS time), which turned out to be the only day for about a month on each side that had adequate firing winds.)

The need to observe the phenomena of thermonuclear device functioning led to a new generation of experiment design on Ivy (which was expanded in Castle). In addition to observing the functioning of the primary in the normal manner, massive experiments were performed to observe [redacted] Ex.(b)(3) The techniques for observing the outputs (gamma rays, neutrons, light, etc.) were also refined. It is interesting to note that after the shot, the yield was uncertain for some time. The fireball measurements were suspect (in some circles) since this was the first large device fired on the ground's surface. The radiochemical results indicated a very low yield (1-2 Mt) initially because of the large natural background of uranium in the crater material. The first moderately correct number (10 Mt) came from the observation of the late-time gamma-ray intensity.

Mike also added to the postshot experience of the task force personnel. The water wave and blast effects were noted and furnished a better basis for preshot planning in later operations. One omission was, however, to have a serious effect later. The fallout from Mike apparently went into a region that was not well instrumented and, hence, no good fallout observations were made at appreciable distances. AEC Headquarters, and in particular Merrill Eisenbud of the Health and Safety Office of the Biology and Medicine Division, had set up their own fallout-monitoring project to try to locate the fallout throughout the Pacific at various distances from the explosion. Although Eisenbud's organization flew extensive missions for three days after the event with various types of equipment, he reported their efforts to be largely unsuccessful in that they never found the main body of the fallout. "Whether it all shot into the stratosphere or not is one of the mysteries of the nuclear age."*

Ivy (1952) also saw the detonation of King (540 kt), [redacted] Ex.(b)(3) It was fired as an

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*Earl H. Voss, *Nuclear Ambush*, Henry Regnery Co., 1963, pages 33 and 34.

airdrop over Runit and added little to the testing capability except to show that such large yields could be safely airdropped. Curiously, history points out that the Russians got a propaganda advantage with the first airdrop of a thermonuclear device in 1955, but it is interesting that the U.S. had the first airdrop of a very high-yield (megaton range) device with the King test in 1952.

The next operation after Buster-Jangle, Tumbler-Snapper, in the spring of 1952 in Nevada, saw the rapid growth of Nevada testing techniques that were then to last, with only one major exception (airdrops), through the period before the moratorium. The experience of Ranger and Buster had quickly shown that while there was a strong advantage operationally and economically in airdrops, there were also a couple of serious disadvantages. The device to be tested had to be constructed to withstand the accelerations experienced during an airdrop and, hence, had to be much closer to the final stockpile construction than would be necessary to simply carry out an experiment. Furthermore, because of the uncertain position of burst owing to the inherent inaccuracies in bombing, it was not feasible to do detailed experiments on the operation of the device and specifically not feasible to do close-in measurements.* Thus, for Tumbler-Snapper, some half of the shots were placed on towers which then allowed detailed measurements of radiation flow, case operation, etc. The tower shots were operationally more difficult to fire because of the increased portion of the radioactivity to be expected in close-in fallout. The tower shots allowed more precise planning and positioning of the instruments and equipments now being fielded by AFSWP,** as a result of the increased interest in effects on the part of the Department of Defense. Thus tanks, jeeps, and pigs, as a function of distance, became a common sight in Nevada. Similarly, as a result of the recognition that we no longer had the sole offensive nuclear force in the world, the question of civil defense was taken up seriously for several years and these experiments in Nevada allowed the responsible organization (Federal Civil Defense Agency) to gain appreciable experience in understanding the effects of nuclear detonations on housing and buildings.

The loss of data due to the inherent inaccuracy in airdrops led to considerations of still other methods of testing in Nevada that would have some of the aspects of cheapness that the airdrops had. Also, there should be the advantage of a comparatively small amount of local fallout that would not require the great field efforts inherent in tower shots. Thus, Sandia, in conjunction with the two weapons design laboratories, developed the capability of lifting the experimental devices by tethered balloon. This method of emplacement replaced the airdrop system beginning with the Plumbbob operation in 1957 and continuing through Hardtack Phase II in 1958. The balloon system was eminently satisfactory in that it kept the fallout to a minimum, allowed some close-in alpha measurements, was fairly inexpensive, and allowed effects experiments from devices going off at almost militarily optimum altitudes. That method also allowed the gear around the device to be placed in a comparatively haphazard fashion, hence reducing the effort required of the bomb packaging people.

The large crater produced by Mike shot in Operation Ivy made it obvious that the Marshall Islands could not support a long series of high-yield shots fired in that manner, with the inevitable destruction of Marshallese homelands. Thus, the Castle operation in the Marshall Islands in 1954 saw the beginning of a testing technique that was to last through the rest of the operations at the Eniwetok Proving Ground.

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*Except for some telemetry.

**Armed Forces Special Weapons Project.

In that operation many shots were fired on large barges which allowed moderate instrumentation, some careful pointing, and hard wire timing and firing, but which would not produce large craters to do away with the land area, and in some cases would allow reuse, either in that operation or another, of the recording facilities. The barges could be positioned with sufficient accuracy so that collimated systems on shore, in conjunction with appropriate shielding on the barge, allowed observation of specific portions of the device.

The shots of Operation Castle were designed to produce an emergency capability for the United States since the Russians had just tested their first thermonuclear device and, by now, clearly had fission devices in stockpile. The shots were almost all large-yield thermonuclear devices that, in general, produced yields somewhat different than those expected. The Bravo shot, specifically, went to 15 or 16 megatons, as opposed to the predicted 6 megatons and produced fallout that extended to Rongelap and Utirik, where there were native populations, and to Rongerik, where a Task Force weather station was sited. It was probably this large population exposure to radiation, in combination with other things, that led to the beginning of the real pressure to stop atmospheric testing. Castle also saw the reopening of Bikini as a test site. This came about in order to increase the number of acceptable firing days from the point of view of weather and also to give sufficient land surface for further shots to be fired in fixed positions on the land. Again, UCRL contributed heavily to the diagnostics performed on Castle, in addition to firing the first Pacific shot of their own.

At Castle, a hard wire timing and firing system was reinstituted. Hard wire distribution systems were placed around the major portions of both atolls and firing was done from the shore-based control stations. In the case of Eniwetok, the control station was on Parry, and it was on Enyu for Bikini. After the Bravo shot in Operation Castle, it was necessary to go back to a ship-based operation at Bikini because the atoll was too radioactive for safe occupation. However, the shots were still fired from the timing station on Enyu.

Operation Redwing in 1956 and Hardtack Phase I in 1958 at the Eniwetok Proving Ground were then conducted in essentially the same fashion as Castle, as far as development shots were concerned. The diagnostic techniques were refined and changed during that period, but the general philosophy of the method of testing and placement remained the same with minor variations. During Redwing and Hardtack, the Atomic Energy Commission, specifically Libby, insisted that we put an appreciable amount of silica sand in the barges in hopes that such sand would increase the concentration of local fallout and, hence, remove some of the hazards from long-range fallout. He also hoped that more of the strontium would appear as the insoluble silicate, hence reducing the problem of ingestion of long-range fallout. Evidence indicates that this had no particular effect. In a similar vein, during Operation Hardtack, Oak was moved from comparatively deep mooring at Bikini Atoll to a position on the reef at Eniwetok Atoll in order to increase even further the proportion of solids in the radioactive cloud, and to change the strontium compounds formed.

Over this period of time, some shots were fired by other methods, largely for Department of Defense effects purposes. The Navy continued its investigation of the effects on ships of underwater detonations, conducting in 1955 the Wigwam shot at a point in deep water 600 miles off the California coast and continuing variations of that during Hardtack Phase I, with shots in the lagoon or just out of the lagoon at the Eniwetok Proving Ground. The early interest in the effects of high-altitude shots is shown by the HA shot in Teapot in June of 1955, a 3-kt airdrop detonated at 36,620 feet; and the Yucca shot in Hardtack Phase I, a 1.7-kt balloon-lofted detonation at 86,000 feet.

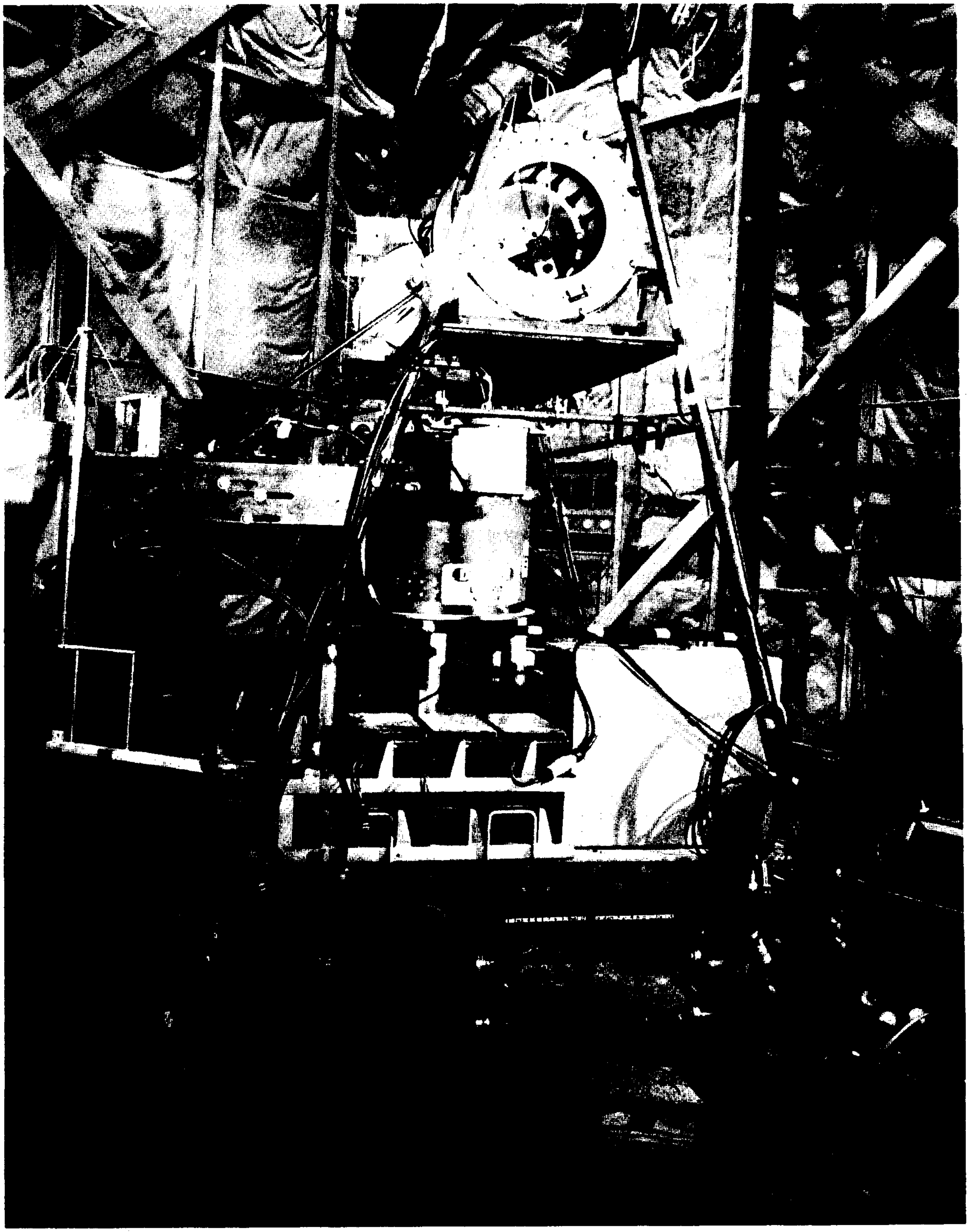

Figure 3.
Typical "balloon cab." The device is under the sign "Live Pit."

More significant, however, from a testing viewpoint, were the Teak and Orange shots of Operation Hardtack in 1958 and the Argus shots. Teak and Orange were conceptually initiated by the AFSWP (and the Air Force) during the period in which the U.S. was considering the Nike-Zeus system as the early ABM system. It was realized that essentially nothing was known about the effects of large-yield, high-altitude detonations. These two shots were originally planned as roughly 5-megaton detonations to take place at approximately 250,000 feet and 125,000 feet (76 and 38 km) and to be fired above Bikini. However, safety studies conducted during the early part of the Hardtack Phase I operation showed that there was appreciable hazard of eyeburn to the Marshallese natives if those detonations took place there. Therefore, late in the Hardtack Phase I operation, the shots were moved to Johnston Island. The launch pad for the Redstone missile was put in and they were detonated at high altitude over Johnston Island two months after the decision to move. This was the beginning of the use of that atoll as a launch site for high-altitude detonations.

Early in 1958, Nick Christofilos of LRL realized that there was a possibility of electrons from high-altitude detonations being trapped in the earth's magnetic field and oscillating back and forth along the field lines, thus artifically producing a shell or shells of high electron densities over much of the earth. Such a phenomenon might have useful military applications. In order to test this theory quickly before the test moratorium went into effect, the Department of Defense arranged the very secret Argus series, which was conducted by a Naval task force (Task Force 88) in August and September of 1958 in the South Atlantic. This resulted in three 1.7-kt detonations at altitudes ranging from about 100 to just over 400 nautical miles. The

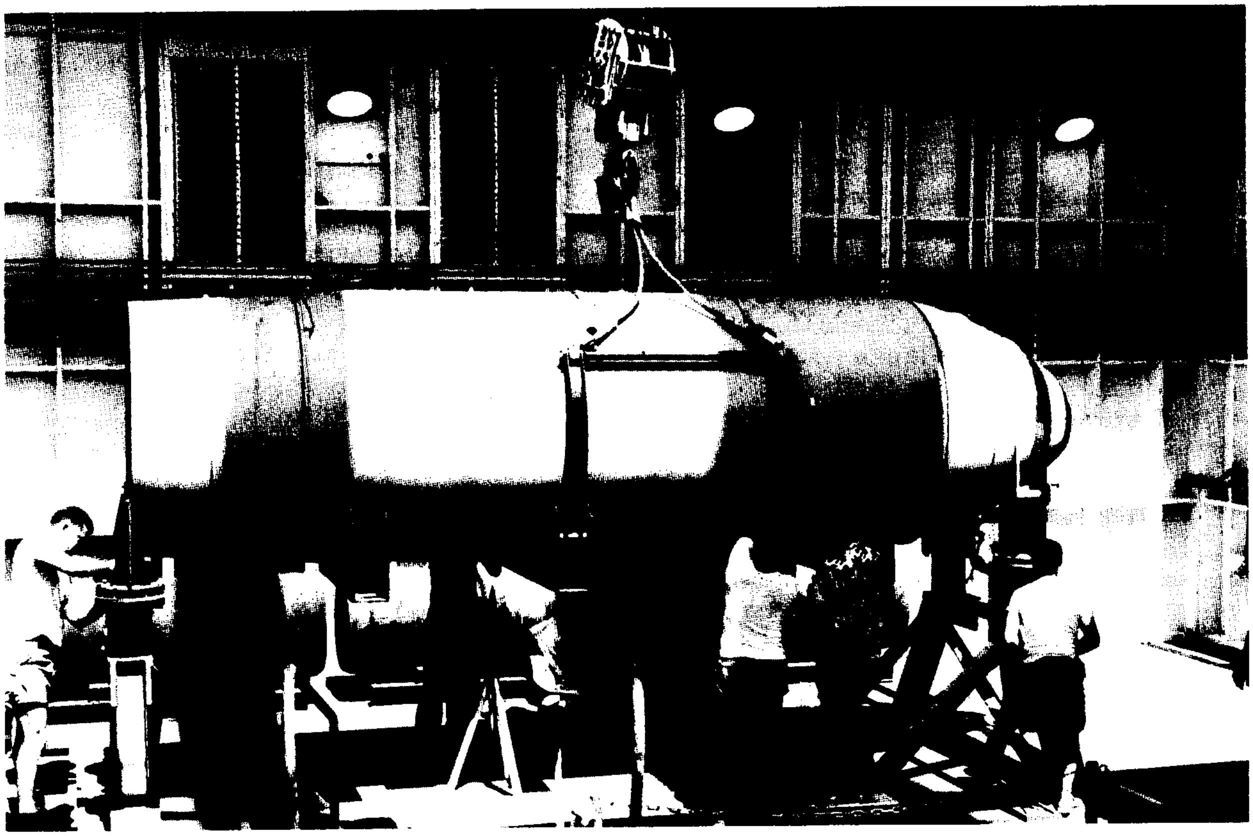

Figure 4.
Operation Castle device being lowered into place on a barge.

devices were carried aloft from a surface ship by a Lockheed X-17a three-stage rocket, making these the only ship-launched, high-altitude, rocketborne nuclear detonations that the U.S. has ever performed. In addition to observations and measurements made from land, ships, and aircraft, instruments carried by sounding rockets and one of the first U.S. earth satellites (Explorer IV) provided useful Argus data.

During the period of 1956-1958, the concept of doing nuclear weapon testing underground received more and more attention, especially by Edward Teller, as a possible solution to some of the test ban debates. Firing underground would presumably allow continued weapons testing without the concomitant fallout problem that was, in some circles, regarded as one of the major difficulties with nuclear weapons testing, or conversely, one of the major reasons for stopping such testing. Thus, in 1957, the Livermore Laboratory conducted the 1.8-kt Rainier shot in a tunnel 900 feet below Rainier Mesa at the Nevada Test Site to investigate the conditions of containment of underground shots. Containment of that detonation was a success, with no tunnel venting. Livermore also conducted, during Hardtack Phase II, four other tunnel shots with moderate success. These shots were fired in tunnels mined into the Rainier Mesa. Over the same period of time, Los Alamos had conducted a couple of very small-yield safety shots in vertical drill holes in Yucca Flat. Ex.(b)(3) Those two shots were stemmed by a concrete plug (precast) just above the device on the bottom of the hole and another very small plug at the top of the hole. From their early underground detonations, Livermore apparently learned that the tunnel was convenient, instrumentation could be placed at various angles around the device, the device could be worked on in place, and the stemming did not seem to be awfully difficult. Los Alamos seems to have learned that drilling holes was cheaper than mining, but that stemming could be a serious problem.

Yet another type of nuclear test operation requiring deployment to the field was initiated and continued through these years. Such tests came to be known as one-point or safety tests. The first of these was done at the NTS at the beginning of November 1955. Three tests at this time and a fourth in January of 1956 were all given the title "56 Project--NTS." A test organization was set up with the AEC Test Manager at NTS having overall responsibility and the tests being carried out by a LASL team headed by their own test director, since these were LASL devices. The different devices containing their normal high explosive Ex.(b)(3) were detonated at a single point by a standard detonator properly situated Ex.(b)(3) Further, to ensure a "worst case" situation, the nuclear fuel quantity was at least as great as the maximum that would occur in production. A neutron source provided neutrons sufficient to assure initiation of a reaction Ex.(b)(3) The objective was to demonstrate that each of the devices was "safe" for this mode of detonation, leading to a nuclear reaction no greater than the equivalent of a few pounds of high explosive. These tests did demonstrate the "one-point safety" of three different LASL devices.

Now that the one-point safety tests became a normal part of the various test series, the next question of weapons safety, that of plutonium contamination, came to the fore. The health hazard from inhalation of plutonium (an alpha emitter) is quite

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*1 shake = 10^{-8} seconds.

serious and the possibility of exposure to this from the various stages of handling of weapons containing plutonium had to be thoroughly evaluated. This resulted in giving Sandia Corporation the responsibility for the "TG 57 program" to perform tests and measurements to further understand the plutonium scattering and contamination characteristics. Thus, a plutonium weapon was single-point detonated in late April 1957 and, through numerous means, Sandia and their support contractors gained further understanding of the patterns of plutonium scattering, fixation, decontamination, and other data.

Several other single-point safety tests were carried out by LASL and Livermore, respectively, in "Project 58--NTS" and "Project 58A--NTS" in late 1957 and early 1958. Other safety tests were incorporated in larger series of various weapons tests.

During the last part of Hardtack Phase II in Nevada, when the test organization was frantic for emplacement positions and was firing several devices a day, a few devices were being detonated in so-called "Gravel Gerties." These were simply small buildings on the surface of the ground with a great amount of dirt piled over them as emplacement positions for very small-yield safety shots, the concept being that the dirt would scavenge the radioactive material and bring it down very close to the zero point and thus prevent off-site fallout. This apparently did help to some extent.

Thus, by late 1958, at the beginning of the moratorium, the test organization had learned to test bombs using a number of different placement methods: airdrops, balloons, towers, barges, and surface shots; and had developed what seemed to it the best methods of so doing, consistent with the characteristics of the test sites it was using. It also had some experience with rocket launching of devices. [redacted] Ex.(b)(3) The test organization had developed some capability for underground testing, but had not pursued that technique far enough to be confident of either the economics or the containment features, or far enough to be confident that all of the necessary diagnostics could be done in a satisfactory fashion. It had conducted a fair number of underwater shots for Navy purposes and was moderately confident of those methods. It could not claim to really understand fallout, but had models to predict the fallout pattern sufficient for operational purposes and knew what kind of weather information was needed. However, it is again to be pointed out that practically all of this experience had to do with surface shots and tower shots. The cloud formation from a venting underground shot is obviously a somewhat different beast. Most importantly, the test organization had by then a great deal of experience in the safety precautions to be taken when nuclear detonations are to take place and had a cadre of people experienced in this field.

Table II lists the operations of 1945 through 1958. The "operational period" normally began about a month before the first planned shot date and ended perhaps a week after the last detonation.

AEC Device Diagnostic Standard Measurements

As time went on in the period of 1946 to 1958, the device designs produced by Los Alamos and Livermore grew in complexity, sizes decreased so the time constants changed and, hence, the requirement for more and more detailed diagnostics grew. We will, in this section, comment mainly on the type of basic diagnostics that had to be performed in the early period after the moratorium to make it useful to fire the shots at all. We will also comment to a certain extent on the more complex measurements that had been developed during the period of 1946 to 1958, simply to illustrate

TABLE II
NUCLEAR WEAPON TEST OPERATIONS[a]

Operation	Dates	Location
Trinity	7/16/45	Alamogordo, New Mexico
Crossroads	6/30-7/7/46	Bikini Atoll, Marshall Islands
Sandstone	4/14-5/14/48	Eniwetok Atoll, Marshall Islands
Ranger	1/27-2/6/51	Nevada Test Site
Greenhouse	4/7-5/24/51	Eniwetok Atoll, Marshall Islands
Buster-Jangle	10/22-11/29/51	Nevada Test Site
Tumbler-Snapper	4/1-6/5/52	Nevada Test Site
Ivy	10/31-11/15/52	Eniwetok Atoll, Marshall Islands
Upshot-Knothole	3/17-6/4/53	Nevada Test Site
Castle	2/28-5/13/54	Eniwetok Proving Ground
Teapot	2/18-5/15/55	Nevada Test Site
Wigwam	4/14/55	29° N, 126°W
Project 56	11/1/55-1/18/56	Nevada Test Site
Redwing	5/4-7/21/56	Eniwetok Proving Ground
Project 57	4/27/57	Nevada Test Site
Plumbbob	5/28-10/7/57	Nevada Test Site
Project 58--NTS	12/6-9/57	Nevada Test Site
Project 58A--NTS	2/22-3/14/58	Nevada Test Site
Hardtack Phase I	4/28-8/12/58	Eniwetok Proving Ground and Johnston Island
Argus	8/27-9/6/58	South Atlantic
Hardtack Phase II	9/19-10/31/58	Nevada Test Site

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[a] The Hiroshima and Nagasaki detonations of World War II were August 5 and August 9, 1945, respectively (Greenwich Civil Time).

the kinds of things we could not immediately do in 1961 as a result of our very quick return to testing with very little preparation, and perhaps more pertinently, very little experience with the new techniques of testing required.

For the normal fission device with no boosting and no secondary, in the very early years of testing, two quantities were of prime importance. The first of these was the energy release, or "yield," of the device, which was directly related to the efficiency of burn of the fissionable material. In the early years, specifically around the time of Trinity, Crossroads, and Sandstone, the experimenters let their imagination run riot to imagine experiments that would give them a handle on the yield. At Trinity, a number of esoteric measurements were made. Fermi estimated the yield of the bomb by simply observing the motion of some scraps of paper he dropped from his hand as the blast wave went by. He had calculated the duration of the positive phase with respect to yield, and by simply observing the time it took for the wind to reverse and knowing his distance from the bomb, he could make an estimate of the yield. Measurements of the various outputs were made at Trinity in order to get a handle on the same subject. The neutron flux in various energy regions was measured. The gamma ray output, the integral of the total light, and the light curve were all measured, but without previous experience they could not be particularly trusted as a measurement. They did all establish the range of yield. That is, the experiments could probably determine without much question, with the possible exception of the gamma curve, which was misunderstood, that the yield was somewhere between 10 and 30 kilotons, but they could not pinpoint it.

Radiochemistry/Sampling

The primary method of measuring yield used at Trinity and depended upon more than any other method clear through the 1958 period, is that normally called "radiochemistry." In principle, the concept was very straightforward. After detonation, a portion of the condensed, particulate debris from the detonation was collected and taken to the laboratory. In the laboratory, chemistry was used to separate out of the debris not only the active material that had been used, such as plutonium or uranium, but also certain representative radioactive fission fragments. An observation of the amounts of both of those materials in one sample then allowed a determination of what proportion of the active material had burned. Knowing that and the amount of active material in the bomb, it was possible to then calculate the energy release based on laboratory measurements of the energy developed by one fission. In practice, the system was not quite as simple as this. In the uranium-type devices, there could be some contamination from the uranium in the ground. There was not always uniform mixing in the cloud. There is some small uncertainty as to the energy release per fission, since it is dependent not only on the isotope, but on the energy of the neutron producing the fission. The advent of the thermonuclear bomb increased the complexity of the knowledge necessary to properly interpret the results. The fate of the neutrons produced in the thermonuclear reaction can have a significant effect on the yield.

Samples were collected at Trinity using ground-based air samplers. Samples were also collected of the fallout, but they were not particularly useful. In normal atmospheric testing, the general procedure was to wait for some appreciable length of time (1/2 to 2 hours) until the cloud had mixed (theoretically uniformly) due to its heat-generated turbulent action so that upon sampling, any sample collected would presumably be representative of the whole bomb. The assumption of uniform mixing was not taken on faith. The entire history of the period 1946 through 1958 is one of trying to establish that assumption or, where it seemed to be questionable, to find methods of handling the situation. For example, for most detonations, it was common to collect samples from several portions of the cloud chosen ahead of time by the experienced Laboratory representative in charge of sample collection. The samples were then treated separately to observe any fractionation that might be present, but were lumped together to conclude the results of the detonation.

It was also necessary to let the cloud diffuse and "cool" for a similar period of time (1/2 to 2 hours) in order that the activity would reduce to levels that made it acceptable to send manned aircraft into the cloud. Even at that time, it was quite possible to get into "hot" regions of the cloud or to overstay the appropriate time to such an extent that unacceptable crew doses would be obtained and, therefore, it was a necessary function of the scientific controller to watch penetrations with great care to make sure that no overexposures were experienced. Since on most aircraft the major dose to the pilot could come from the sample collected either in the sampling collectors or in the engines, it was also necessary to prejudge very carefully how much would be collected in order that the aircraft could return to base before the pilot was overdosed. Obviously, appropriate aircraft washdown facilities had to be developed. Several times during this period the sampling tanks had to be redesigned as new aircraft were devoted to this function. Specifically, it was necessary to design tanks to fit the operational speed of the aircraft and still allow the air to pass through the collectors at low enough velocity so the filter papers could handle it; otherwise, mechanical tearing could result. The filters were designed to allow fission particles of all various sizes to be collected with equal efficiency (isohenticity). The design of such collectors was a very large job performed by external contractors with technical guidance from the Laboratory.

Because of the large operational cost of sampling with aircraft, the large dose to the pilots, and the usual shortage of aircraft, several attempts were made during this period to develop quicker or less costly sampling systems. B-17s were converted into drones and, guided from a mother ship, were used in some of the early overseas operations (Crossroads and Sandstone). However, they were very hard to control and a number were lost. They could not reach required altitudes and were very expensive to operate. During Greenhouse a fairly expensive attempt was made to obtain so-called "grab samples." Very large steel devices were constructed, which were placed close to the base of the tower. These devices were designed so that after the first shock wave went by and the device was enclosed in the fireball, a large valve would close, trapping an appropriate amount of the active material inside. Presumably then, at a later time, these "bottles," some of which resembled gun barrels, could be recovered and the sample treated. Unfortunately, the valves in general did not work, no samples were collected, and the method failed. In at least one case the bomb yield was larger than the "bottles" were designed for, and they were destroyed. At Trinity, the soil around the tower, which contained silica, had melted inside the fireball and plated out as a glass on the ground. This glassy material contained enough of the radioactive debris that it was useful as a sample of the bomb. Various attempts were made at Sandstone and Greenhouse to reproduce this by spreading great numbers of broken beer bottles and other silica-containing material around the towers. This, in general, also failed because the materials were blown away. On at least one occasion, it failed because the beer bottles turned out to be plastic. As part of this same trend of thought, a radio-controlled vehicle was obtained for Sandstone to enter the crater early and recover appreciable amounts of the material around the tower. Likewise, this method failed because the recovery system did not work very well and the material at the base of the tower did not contain the required fission debris. A variation of this system had been used at Trinity, in which a manned tank with a remotely controlled bucket on the front of it had been used. The tank got stuck in the crater and caused quite a furor.*

An attempt was made to sample the radioactive cloud at Trinity using filters on B-29s. However, the aircraft did not operate, so no samples were collected.

At Greenhouse (1951) LASL again tried to collect airborne debris by firing 5-inch HVAR rockets with sampling heads developed by China Lake from one island across the shot island to a third island. The rockets were fired a few seconds after nuclear device detonation, and were in general deflected by the shock wave and lost. A few were recovered but the samples were not adequate.

Livermore tackled the rocket sampling problem once more in the late 1950s with the use of small rockets outfitted with a sampler head which was designed to intercept the cloud, take a sample, and then close and parachute to the earth. The collecting heads were built to float and, in some cases, built with small beacons and sea dye so that they could be found in water. This system was tried over several operations in the Pacific and was a forerunner of the "Cleansweep" system tried at Dominic. Since the attempt was to get early samples, these rockets were fired soon after the detonation (five or six minutes), and in some of the early attempts encountered sufficient turbulence in the cloud that they were thrown off course, broken up, or otherwise not recovered. By the time of the moratorium, the rocket sampling system was showing considerable promise, but had not yet been developed into a dependable, operational system.

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*The tank was driven by Sgt. Bill Smith and carried Herb Anderson and Enrico Fermi.

The need to obtain samples from the one-point safety tests of Project 56 led to the design of collectors intended to pick up the large particles of active material that might be expected from very low or zero-yield devices. These collectors consisted of sand-filled wooden boxes, about 4 feet square and 10 feet long, with the long axis radial to the bomb. In principle, the heavy particles would enter the sand and stop. Later "sand sifting" would then reveal the sample. The technique worked when the yield was not appreciably larger than that predicted.

As development efforts changed from fission bombs to large thermonuclear bombs, the pressure grew to obtain a higher-altitude sampling capability. In the case of surface and barge shots, this was necessary simply in order to make sure that representative samples were being obtained. In the case of airdrops, the bottom of the cloud might well rise up to the tropopause or higher and, hence, reach an altitude that could not be reached by some of the earlier sampler aircraft. A list of aircraft used and aircraft characteristics versus time is given in Table III.

TABLE III
AIRCRAFT USED FOR THE COLLECTION OF RADIOCHEMICAL BOMB DEBRIS SAMPLES FOR AEC LABORATORY USE, PREMORATORIUM

Operation	Date	Aircraft
Trinity	1945	None
Crossroads	1946	B-17 Drone, Navy F6F Drone
Sandstone	1948	B-17 Drones
Ranger	1951	T-33
Greenhouse	1951	B-17 Drones, F-80 Drones, B-29
Buster-Jangle	1951	T-33, B-29
Tumbler-Snapper	1952	F-84, B-29, T-33
Ivy	1952	F-84G, B-29, B-36
Upshot-Knothole	1953	F-84G, B-29, B-36
Castle	1954	B-36, F-84G
Teapot	1955	F-84G, B-57A (B-50D Controller)
Redwing	1956	B-57B, F-84G
Plumbbob	1957	B-57B, F-84G, T-33?
58-NTS	1957-58	B-57B
Hardtack	1958	B-57B, B-57D
Hardtack II	1958	B-57B

By the beginning of the moratorium, there was available a quite satisfactory sampling system for normal detonations. The system consisted of the B-57 aircraft in several configurations, with the appropriately designed sampling apparatus. Over this period of time, an Air Force organization, first designated AFOAT-1 and then AFTAC*, had been developing aircraft sampling systems for remote detection of foreign tests. These were first used on the B-29s and later in other aircraft. They eventually developed equipment and instrumentation for use on WC-135As at low altitudes and the U-2 at high altitudes. Their collection systems, however, did not, in

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*AFTAC--Air Force Technical Applications Center.

general take sufficiently large samples to be adequate for the detailed analysis required by the weapons design laboratories, or were limited to long-range bomb cloud sampling by design.

The advent of the thermonuclear bomb, with its large energy release from nonfission fuels, led to the problem of determining the thermonuclear burn. One possible solution to this problem was to collect and analyze the gaseous products of fusion. In the mid-1950s, such gas collection systems were designed by Livermore and AFTAC for use on the sampling aircraft. The samples collected were not of particular value to bomb diagnostics during that period of time. However, these efforts led to an eventual capability for collecting and analyzing such samples, postmoratorium, and have been especially valuable in establishing techniques which are used today with respect to foreign tests.

Over this same period in time, the capability of the radiochemical technique for weapon diagnostics grew greatly. Initially, the technique was thought of only as the fission yield measurement derived from the ratio of fission fragment production to active material in the sample. This simplicity became disturbed very quickly with the introduction of composite devices that had both uranium-235 and plutonium as fuels, because of the variable partition of fission between the uranium and plutonium materials. However, this introduced little more than a complication in the arithmetic with which the data were treated, although sensitive fission particle analysis helped. It was also recognized early-on that the energy release per fission was dependent upon the energy of the neutron causing the fission. Correct treatment of the incident neutron spectrum was required, and was initially based upon calculations of the neutron energy distribution through the device. Tracer materials, such as rhodium in the plutonium, could act to label different fuels in the radiochemical analysis. [redacted]

Ex.(b)(3)

This measurement then prompted a more detailed correction for the energy spectrum in calculating the yield. Cross calibration between the Los Alamos Scientific Laboratory, AFTAC, the British, and later the Lawrence Livermore Laboratory assured the validity of the primary radiochemical processes and philosophies. [redacted]

Ex.(b)(3)

Since most of the devices contain ordinary uranium, or even depleted uranium, some fission takes place in that material due to the high-energy end of the neutron spectrum and, hence, these materials contribute to the energy release of the device. Correction for this phenomenon was initially made using the theoretical calculations of the neutron distribution. Appreciable difficulty was, however, experienced in the early Eniwetok shots because the natural uranium in the soil mixed with the bomb uranium in the cloud, making it difficult to determine the amount of bomb uranium in the sample. In some of the shots, this was overcome by simply putting barrels of a uranium compound close to the bomb before it was fired. Sufficient uranium of known quantity was therefore in the cloud to mask the background of the natural uranium. Later on, some of the tracers mentioned earlier were placed in the device uranium to obviate this problem.

Ex.(b)(3)

[redacted]

Ex.(b)(3) [redacted] Lithium is present in fairly large amounts in the soil and ocean water of the Eniwetok Proving Ground. Deuterium is present in large amounts in the ocean water. Thus, while it was possible in the late 1950s to obtain samples of the gas from the radioactive cloud, it was difficult to determine what portion of the bomb was present in that sample and, hence, difficult to determine what amount of burn products were in the sample. As mentioned earlier, the radiochemical results on Mike shot, the first large thermonuclear device, were very uncertain because of factors of this sort. Attempts were made to solve this problem by noting the burn of the fissionable materials Ex.(b)(3) [redacted] and from that observation calculating the neutron flux that material was subjected to and in turn calculating the thermonuclear burn that must have produced the flux. Ex.(b)(3) [redacted] However, up to the time of the moratorium, the problem had not been completely solved.

The very large neutron fluxes present in the secondaries of thermonuclear devices resulted in a number of the fission products formed being transformed again because of neutron capture. An understanding of this "burnback" phenomenon was necessary to deduce the correct yield from radiochemical data. This requirement led to the development of detectors of appreciably different cross sections for neutron capture to be placed in the active material. The results from these detectors, combined with intricate arithmetic, usually led to a correction for the burnback in the samples.

Obviously, during this period, the laboratory techniques for handling radiochemical material, for counting and data treatment, including automatic inputs to computers, etc., were continually improved.

In 1957 and 1958, the weapon design laboratories, especially Livermore, began to gain experience on the problems of doing radiochemical analysis of the debris from underground detonations. Several difficulties were apparent. To drill back to the detonation region and obtain a sample was not only expensive but required the development of techniques for drilling and handling the radioactive material without creating a hazard to the operating personnel. The phenomena of underground cavity growth, melting, and resolidification were not well understood. There was no assurance that the samples obtained would be representative of the whole bomb. In fact, it was perfectly clear that in some cases, they would not be representative. This, if properly treated, could be an advantage, but clearly there was a whole new phenomenology to learn. When a bomb is fired underground, a large fraction of the neutrons go out into the dirt around the hole and some of those reflected back in may cause further burn of the fissionable material. Methods of either accounting for this or preventing it, in general, were not developed by the time of the moratorium. The whole question of how to handle thermonuclear burn appeared even more difficult under these conditions, but no solution was attempted before the moratorium. A little about handling large dirt samples was learned as a result of the few underground tests.

Fireball Yield

At the time of Trinity, the general equations for the growth of the fireball as a function of time and of yield had been worked out for an explosion in a simple gas. However, the details of shock-wave expansion in air at very high pressures were uncertain theoretically, and some of the required gas constants of air were not known. Furthermore, any effects of nuclear and thermal radiation on the air before the shock wave hit it were not understood. Most particularly, the calculations did not include the effect of the medium, during early expansion, consisting of bomb fragments, tower fragments, old high explosive, etc., rather than of pure air. Thus, in the period of 1945 through 1949, it was thought feasible to scale the yield by observing the rate of growth of the fireball from one detonation to another, but it was not considered feasible to determine the absolute value of the yield from this measurement. In general, it was thought than any characteristic phenomenon in the expansion, for example, the time of breakaway,[*] would scale as the yield to the 1/3 power. A rough scaling law for fireball growth as a function of yield at this time, derived from the simultaneity solution of the equation of motion, was that the yield was a constant times d^5/t^2, where t is the time taken by the fireball to grow to a diameter d. The solution applies after the fireball has encompassed a mass of air large compared to the mass of the bomb. Thus, in determining the yield, an error in the diameter measurement resulted in five times that error in the yield, and an error in the time resulted in twice that error in the yield.

While photographs of the fireball as a function of time had been taken during Trinity and Crossroads Able, there were problems in later interpretation. The Trinity films were not stored in such a fashion as to make later quantitative measurements completely reliable. Trinity was essentially a ground burst (100 foot tower), but the energy loss to the ground was small. For Crossroads Able, it was difficult even to determine the distance from the camera to the detonation. However, these pictures did allow an initial determination of the constants in the fireball rate of growth equations by comparing the results of such arithmetic with the radiochemical yields determined on Trinity and Crossroads Able. Because of timing signal problems, only streak camera records came out of Crossroads Able.

In order to conduct the Sandstone operation in 1948, the Los Alamos Laboratory set up a temporary task group, under Darol Froman, which allowed some appreciable preplanning and as a result, fireball rate of growth pictures with moderate time resolution were taken during Sandstone, largely under the guidance of Lou Fussel, later of EG&G, and Berlyn Brixner.

However, in 1949, with the formation of a permanent test division at Los Alamos, there began a serious attack, both theoretical and experimental, on this problem. Fred Reines, who was in charge of the experimental portion of the test division work under Al Graves, felt very strongly that it should be possible to make fireball measurements into an absolute yield measurement. He, therefore, set up a section within the division with people such as Fran Porzel and Joe Mullaney to pursue the theoretical aspects of shockwave expansion under these conditions and of the characteristics of air. They, in turn, sought the aid of other experts, particularly Hirshfelder and McGee. At the same time, there was established a relationship with the newly formed company Edgerton, Germeshausen, and Grier (EG&G), which was to expand the capability of detailed fireball measurements, reduce the time uncertainty, etc., throughout the entire period from 1949 to 1958. Porzel worked long and hard on the "analytic solution" to fireball growth and shock formation. While his solution was in fact semiempirical,[**] it was useful, both in this field and also in the basic understanding of blast phenomenology. The characteristics of shock formation and propagation in air at high temperatures, and specifically the constants that go with that, were studied theoretically and calculated to a much higher accuracy using the

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[*] The separation of the shock front from the fireball.

[**] The solution used Fuchs' IBM M problem as a guide.

growing computer facilities at Los Alamos. The effect of the mass of the bomb and tower or local surroundings was clearly noted in the detailed pictures of Greenhouse and later operations. A theory to account for this mass was eventually developed and proven out against the field data. The group in Los Alamos grew over this period so that by 1958 there were several competent people in this field. In the period from its formation to 1958, Livermore also contributed to this effort. However, in general, they took the position that the measurements were well in hand and that their efforts were better spent on other portions of the diagnostics problem.

The knowledge of fireball growth at very early time gained from the streak camera data taken by both the Naval Research Laboratory (NRL) and LASL, assisted appreciably in the understanding of early fireball growth and, hence, all fireball growth.

EG&G, in their partnership with LASL, over this period of time developed experimental techniques that resulted in excellent fireball pictures. They developed the "Rapatronic" camera, which allowed single-shot pictures at preestablished times during fireball growth. Cameras were triggered from the first Teller* light and, hence, could take pictures with something like microsecond accuracy; however, only a few pictures could be taken for any given fireball because each camera only took one exposure. Standard and controlled development techniques were established to assist in determining the edge of the fireball with adequate accuracy and consistency. A storage system was set up in order that the film could be kept under the proper humidity and temperature so that later measurements would be significant. New films were developed with the aid of the manufacturers to better handle the wide ranges of brightness experienced. A team of film readers had been developed and trained at EG&G so that there was consistency in the reading of a given film independent of who read it. The Polaroid process was applied to some cameras to allow early fireball yield determinations. Camera timing was improved so that the inaccuracies in yield, because of timing inaccuracies, could be reduced.

By 1958, the field expertise and the theoretical understanding had reached such a point that the fireball measurement was regarded as "the yield measurement" for thermonuclear bombs and there was a running debate as to whether it matched radiochemistry for normal fission bombs.

Thus, in the latter part of 1958, there was a highly trained crew in EG&G for taking field fireball measurements with a great amount of gear such as cameras, timing systems, etc., for that purpose, and in the Laboratories, a good understanding of fireball phenomenology and the relationship between fireball growth and the energy release of the device. Obviously, this well-established technique for determining yield, and specifically the prime technique for determining the yield of thermonuclear devices, could not be used underground.

The Reaction History

Alpha

Early-on, the term reaction history usually referred simply to the measurement of the exponent in the equation $I = I_0 e^{\alpha t}$ where I_0 could be taken as the flux of neutrons at a given point in the bomb or gamma rays external to the bomb. The equation could also be written as an integral such as $N = N_0 \int^t e^{\alpha t}\, dt$, where N could

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*See later section on Alpha.

be the neutron flux in the bomb, N_0 the initial flux which might be established by the natural background or by an artificial source, and t the time from the beginning of the nuclear reaction. However, in the period of 1945 to 1958, other phenomena were introduced and the term reaction history was used to cover the measurement of all of these quantities. Ex.(b)(3) [redacted] The point of the early reaction history or, as commonly called, alpha measurement, was to determine the rate of increase of the population of neutrons in the device, both resulting from fission and causing further fission, and hence check the "criticality" calculations. Since the neutrons were not immediately available for observation (they were inside the device), and furthermore since they do not travel at the speed of light, by the time they get outside the device, there is some time smearing in the detection of the neutron flux, which depends upon the neutron spectrum. Obviously, the time smearing did not matter as long as the spectrum of neutrons in the device remained constant and alpha was sensibly constant. However, we were not sure of such constancy. There were other difficulties mitigating against the use of neutrons and some which even caused problems in observing gamma rays. Ex.(b)(3) [redacted] the cable carrying an electrical signal from a detector could short out very quickly after the detector received the gamma ray signal. Since the neutrons traveled at a speed appreciably less than the velocity of light, the cable taking an electrical signal from a neutron detector could be shorted from the high-intensity gamma flux before the neutrons could reach the detector. There were several more difficulties recognized before Trinity by the people involved with this measurement, mainly Bruno Rossi and Bob Wilson. The signal was so fast that the recording equipment of the time would possibly not write. The problem of oscilloscope presentation was difficult. When presented in a normal fashion with a linear sweep and the signal vertical, the signal would sweep off the scope before an appropriate measurement could be made. A beam intensifier had to be used to increase the writing speed, but that implied turning on the intensifier at just the right time, and the vacuum tube circuits in those days were not reliable enough to guarantee the time delay. A self-triggering system had to be developed since the observation would only take a microsecond or less on any given oscilloscope, but this was occurring 100 microseconds or more after detonation of the high explosive. Since the time between high-explosive detonation and criticality was uncertain by more than a microsecond, the scopes could not be triggered by the high-explosive signal. The yield of the bomb was expected to be such that a recording station had to be placed an appreciable distance from the bomb, some 1,000 to 2,000 yards, and, hence, the attenuation of the transmission cable then available would be extremely high for signals of the expected rise time. Because of the philosophical difference between a pure exponential and a sine wave, and because of the lack of clearly appropriate exponential signal generators it was not even clear the cable attenuation could be measured or calculated ahead of time. Because of the very rapid growth of gamma-ray intensity, it was clear that unless an extremely fast cable was used and, furthermore, was radial to the bomb, the cable would be shorted before the signal from the detector could get to the recorder. This fact then governed the required output of a detector and made it clear that many amperes of current were necessary. From these criteria, Rossi designed a system involving an ion chamber some six feet long and six inches in diameter as a detector, and transmission cable that was three-inch diameter copper coax, one-inch diameter inner conductor, air dielectric, that was run on catenaries from the tower cab to the ground and then buried in trenches the rest of its way to the recording station. He made a loop some 300 feet in length of this three-inch coax just outside the station to be able to tap off the beginning of the loop to operate the scope intensifiers and then let the signal go the extra 300 feet before being presented on the scope so that the intensifiers would have time to work. He furthermore originated the "Rossi Presentation," which involved a constantly oscillating sine wave with appropriate frequency (190 megacycles/sec) on the vertical plates of an oscilloscope and the signal on the horizontal plates. Thus, no matter how fast the signal, there should be an initial portion that, by the very characteristic of an exponential, moves slowly enough for a few cycles of the oscillator to be presented. Rossi's system worked on Trinity and produced a trace (See Figure 5) that was very fuzzy but, nevertheless, did show the reaction rate such that it could be measured with a probable error of approximately 10 percent. Ex.(b)(3) [redacted]

Ex.(b)(3) [redacted]

Figure 5.
Rossi presentation from Trinity. The ionization chamber output signal is oriented vertically and the fixed frequency oscillator sweeps horizontally in the figure. The three arrows mark the extrema for one cycle of the oscillator. The value of alpha is computed from the signal amplitudes at the extrema relative to an arbitrary baseline.

In parallel with this effort, Bob Wilson developed a unique presentation system that consisted of a charge collection box built into the face of an oscilloscope. By allowing a sweep only in the horizontal direction, starting to one side of his charge collection box, he could arrange a geometry in which the charge collected on the box was related directly to alpha. Unfortunately, because oscilloscopes tend to change their characteristics with time, this method required that a calibration signal be measured approximately minus one second from the detonation. At Trinity, the calibration system failed to operate so only the real measurement was made. Since there was no calibration, it could be interpreted only from calibrations taken many hours before. The result he obtained Ex.(b)(3) was correct, but he could not state the uncertainties. Therefore, in later operations, the designers of new systems followed on from the Rossi system rather than the Wilson system. With the exception of a small amount of work by Clarence Jones during Teapot, to the best of my knowledge, no one pursued Wilson's ingenious path after that.

For Sandstone in 1948 Los Alamos set up a temporary division to conduct the technical portions of the operations. The Laboratory requested and accepted the assistance of competent outside laboratories and, on the subject of reaction history, specifically that of NRL under Wayne Hall and Ernie Krause and of EG&G. Technical liaison and direction on this subject was in the hands of the author and Gus Linenberger. The successful measurement on Trinity having been the Rossi measurement, it was decided to follow that path and make what improvements could be made. No personnel were left in the Laboratory after the great exodus of 1946 who had been deeply involved in the Trinity measurements. The immediate path was to discuss the subject again with Rossi. Those discussions, plus Los Alamos and NRL thoughts on the subject, did not lead to any deep further understanding immediately. NRL could and did make improvements on the oscilloscopes available at that time and produced higher writing speed oscilloscopes. However, no significant advance in the understanding of cable transmission was achieved. Therefore, the basic Sandstone alpha measurements were essentially a repeat of those made at Trinity. Fortunately, two of the Trinity detectors were still on hand and from those, plus drawings, a new stable of identical detectors could be made. The same coaxial system was used that Rossi had on Trinity, that is, three-inch coax, buried after the catenary, some seven feet deep in the coral sand. Also the delay loop and turning philosophy was the same as Rossi had used. The only real difference between Rossi's measurements on Trinity and those conducted on Sandstone was the number of detectors, cables, and oscilloscopes. Sufficient detectors to cover the entire expected range that was detectable were used. The measurements, under the excellent direction of Wayne Hall and especially Ernie Krause, were successful.

However, the large expense and effort involved in this led to a suggestion of another technique by Edward Teller. Edward made the comparatively straightforward observation that gamma rays passing through air produce secondary electrons. Those electrons excite atoms and molecules and those atoms and molecules, in falling back to the ground state, produce light. Since all of the processes followed the initial exponential, and exponentials of the same value added together produced the same exponential, it was obvious that observation of the rate of growth of the light intensity at very early stages gives alpha. Again, it was assumed that the gamma-ray intensity followed the neutron intensity properly. Both Edward and the experimenters to carry this out, namely the author and EG&G, went through the appropriate arithmetic and calculated that the light intensity would be observable. Edgerton, Germeshausen, and Grier were given the job of actually conducting the measurement. The field effort was fairly straightforward. Both photocells and photomultipliers, specifically 930 photocells and 931A photomultipliers, would be used at the focus point of five-foot mirrors, salvaged from Army arc light searchlights, and placed several miles from the detonation as detectors. The signals from these detectors were limited to comparatively small currents so it was necessary to use some electronic trickery to drive the then existing oscilloscopes. Both amplifiers and very high impedance circuitry were used. At the same time that EG&G was developing the equipment to make the measurement, the workers at Los Alamos set out to measure in the Laboratory the actual conversion efficiency from gamma rays to light. In order to do this they used photomultiplier detectors in a steady-state experiment involving a contained air volume in a black enclosure and gamma rays from the Laboratory's approximately 10,000-curie radiolanthanum source. If the conversion efficiency was as calculated, the observation should be straightforward. Basically, one simply varied the air pressure in the container and observed the light output. The output should vary with the air pressure, and any background effects could be separated by observing the light intensity with no air in the container. Unfortunately, the results of this laboratory experiment showed that the conversion efficiency was something of the order of a factor of 1,000 less than the arithmetic had indicated. A meeting just before the field teams disappeared to the Pacific again illustrated Teller's magnificent physical intuition. After a number of hours of argument, Edward simply observed that we the experimenters were ready to make the measurement and were on the way to the field, so why not make it anyway. So they did. The field measurements indicated that the light curve did follow alpha within the observational range. They also showed that a growth rate of higher than one-half generation per shake could not be measured with 931As, but higher alphas were obtainable with the 930 photocells. Later on, the answer to the initial dilemma became clear. While the conversion efficiency from gamma rays to light was a factor of 1,000 down from the initial calculations, the conversion efficiency from neutrons to gammas in the bomb and, hence, the gamma ray output at a given neutron level in the bomb, was a factor of 1,000 more than the calculations had indicated. Thus, the light intensity observed was very close to that shown in the initial arithmetic.

Sandstone also saw the beginning of serious studies of the electromagnetic pulse produced by a nuclear detonation. That pulse caused great trouble in normal observations because it was picked up on signal cables, etc., and distorted the signal that was intended to be measured. The observation of this phenomenon, of course, led to a later method of observing alpha. The existence of such a signal had been predicted by Fermi and noted by Bob Wilson on Trinity.

The establishment of a permanent test division at Los Alamos, J-Division, in 1949, with the appropriate people to study the problem, plus the recognition that thermonuclear reactions, [redacted] Ex.(b)(3) Thus, the NRL group (under Krause) and EG&G, with appropriate funding, developed new high writing-speed oscilloscopes, including the traveling wave oscilloscope, that were eventually to reach writing speeds as high as the velocity of light. They developed very high-power, fast amplifiers in order to look at lower levels in the alpha signal. They studied, but did not particularly improve, cable propagation and they developed new detectors. [redacted] Ex.(b)(3) To do this, NRL and EG&G, in conjunction with Earl Fullman of the LASL, developed photocell-phosphor combinations with high sensitivity and short integrating times. The photocells were especially built photocells some three inches in diameter and 10 inches long, which in practice were then enclosed by a phosphor, on the order of two-inches thick, which was initially naphthalene, but later some of the liquid phosphors. Because of the conservatism of the Los Alamos contingent, it was necessary to use both the photocell systems and the old ion chamber systems until we had determined that the photocells actually would give the same results where appropriate. An initial check was run by Bob Patten on Ranger. The photocell-phosphor combination had received some impetus during Sandstone from a variation on the Teller alpha measurements, in which large, flat plates of naphthalene were put up close to the bomb and the light from those plates observed using 930 photocells with an appropriate mirror focusing system at some distance from the bomb.

The basic techniques developed for Greenhouse for the measurement of alpha became the standard techniques through the period 1951 through 1958 with comparatively minor variations. New photocells were developed for slightly faster response. Oscilloscope research continued and produced ever more satisfactory oscilloscopes. Trigger circuits were developed that allowed shorter delay loops and perhaps most important, the understanding of cable construction and transmission grew so that it eventually became possible to use somewhat smaller cable (7/8-inch diameter) by correcting for attenuation at the high frequencies.

The sudden necessity to conduct the Ranger operation around Christmas of 1950 and early 1951, before the Greenhouse operation, led to an unexpected, but in the long run, quite profitable variation of the use of the alpha system--the alpha measurements being under the guidance of R. B. Patten. This was the first time since Trinity that LASL had conducted, in the field, all of its own alpha measurements. The basic gear that had been developed for Greenhouse was used along with some old Sandstone equipment. Specifically, the ion chambers developed by Rossi, which were to be used again at Greenhouse, were used as detectors. Since the bombs were to be detonated at something like 1,000 to 1,500 feet, it was recognized that only late-time measurements were possible, but, since these bombs presumably had a constant alpha, that was satisfactory. A single blockhouse was constructed that would contain some 25 to 30 oscilloscopes and four sets of detectors were placed at the ends of a cross centered on the recording building on the order of 500 feet out from the building. Delay loops were wound and installed inside the building. The whole array then became the target for the bomber. It was the philosophy that if he missed the building, he would hit moderately close to one of the detector systems at the end of one of the arms of the cross. This system worked adequately with comparatively minor problems from a technical point of view* and led to the use of similar systems for airdrop and balloon shots at the Nevada Test Site during later operations.

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*However, there were serious operational problems. The shock wave from the detonation hitting the ground caused the ground to fluff so that eventually there was soft sand of the order of eight feet deep around the building through which it was almost impossible to walk and vehicular traffic was not possible. In between shots, it was necessary for the people to get into the building, which had a long entrance tunnel, and they had to go through this fluff which was, of course, quite radioactive because of activation from the neutron flux from the devices. They had to pass through this radioactive region and into the shelter of the building quite rapidly to check out and reset their system. Getting to the detectors became most difficult because of the radioactivity. As the operation went on at approximately one shot per day, the detectors were gradually destroyed and for the last shot could not be replaced without overdosing personnel appreciably. To solve this problem, the field team at Frenchman Flat, namely Jack Clark and Bill Ogle, simply moved the lighted target array to the one set of detectors that was still operating properly, in order to increase the probability of getting a signal. Since the bombers were bombing on the lighted array, it did not occur to the field team that anyone else could possibly care about this movement, so no notice was given to the Air Force or the Test Manager and Scientific Advisor, who for Ranger were in Las Vegas. The last bomb, Ranger F, was dropped successfully and the alpha measurements were achieved successfully. However, some three days after that shot, the reporters, in a normal press briefing, inquired of Al Graves, who was the Scientific Advisor, as to whether the target had been

The attempts to bring Teller alpha into the status of a dependable measurement were continued in Greenhouse to a certain extent but with no real breakthroughs. It did not seem that one could make the photomultipliers respond at higher rates than an alpha of perhaps one generation per shake. The use of ordinary photocells to observe fast alphas, such as those expected with boosts, did not seem feasible because of the requirement for very fast amplifiers, which were not yet available. However, EG&G, Wayne Hall at NRL, and groups at Los Alamos continued to work on this subject. Ex.(b)(3) [redacted] The alpha measurements were therefore solely Teller light measurements, conducted by Lee Aamodt, using 930 photocells and five-foot dishes to collect the light. The measurements were adequate and successful. Between 1954 and 1958, there were continued efforts on the development of Teller alpha. However, in that period, physically smaller single-stage devices were developed. Ex.(b)(3) [redacted] It was recognized that the light signal, which was far down the chain from the neutrons, would not follow the neutron intensity in the device with high accuracy and our capability of calculating back from the light intensity to the neutron density involved so many steps that it could not be done accurately. Therefore, the measurement was not considered a principal diagnostic measurement up through 1958. It was, of course, pursued at that time and later as a possible observational technique to use in observing foreign tests.

Beginning in approximately 1948 (though predicted by Fermi before Trinity), the testing system recognized that in the same fashion as Teller light, the bomb should produce an electromagnetic signal, the early stages of which, for the same reasons, should, in principle, follow the alpha curve. In this period of time, a number of experimenters investigated this phenomenon in great detail and tried to convert the measurement of this phenomenon into an adequate measure of alpha. Ernie Krause, during Greenhouse and Sandstone, devoted some of his oscilloscopes to this effort. Watt, Malik, and Theobald at Los Alamos continued to investigate portions of the problem. Watt, specifically, tried to look at the field inside the high-intensity gamma sphere. Lou Wouters, at UCRL and later at Livermore, conducted both measurements and theoretical investigations of this phenomenon. Bob England and Clyde Cowan at Los Alamos conducted rather large experiments during Buster as did Ralph Partridge on Tumbler-Snapper. England and Partridge were the first to show that the light curve and the electromagnetic curve, at least in the early stages, followed the proper exponential. Unfortunately, these results were not well recognized and it is only in very recent years (1973), due to increased capability, that we have been able to again experimentally arrive at this conclusion. Thus, at the beginning of the moratorium in 1958, the measurement of reaction history by observing the electromagnetic signal was not in a satisfactory state. The measurement of long-time electromagnetic signals out to many microseconds had been conducted largely by

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*(cont)moved. He commented, "No," and a few hours later asked Ogle the reason for the question. The answer was that it had been moved but notice of that fact had not been considered important. Graves was extremely embarrassed and from then on, rejected the philosophy that the Test Manager and Scientific Advisor could be physically separated from the rest of the technical organization in conducting an operation.

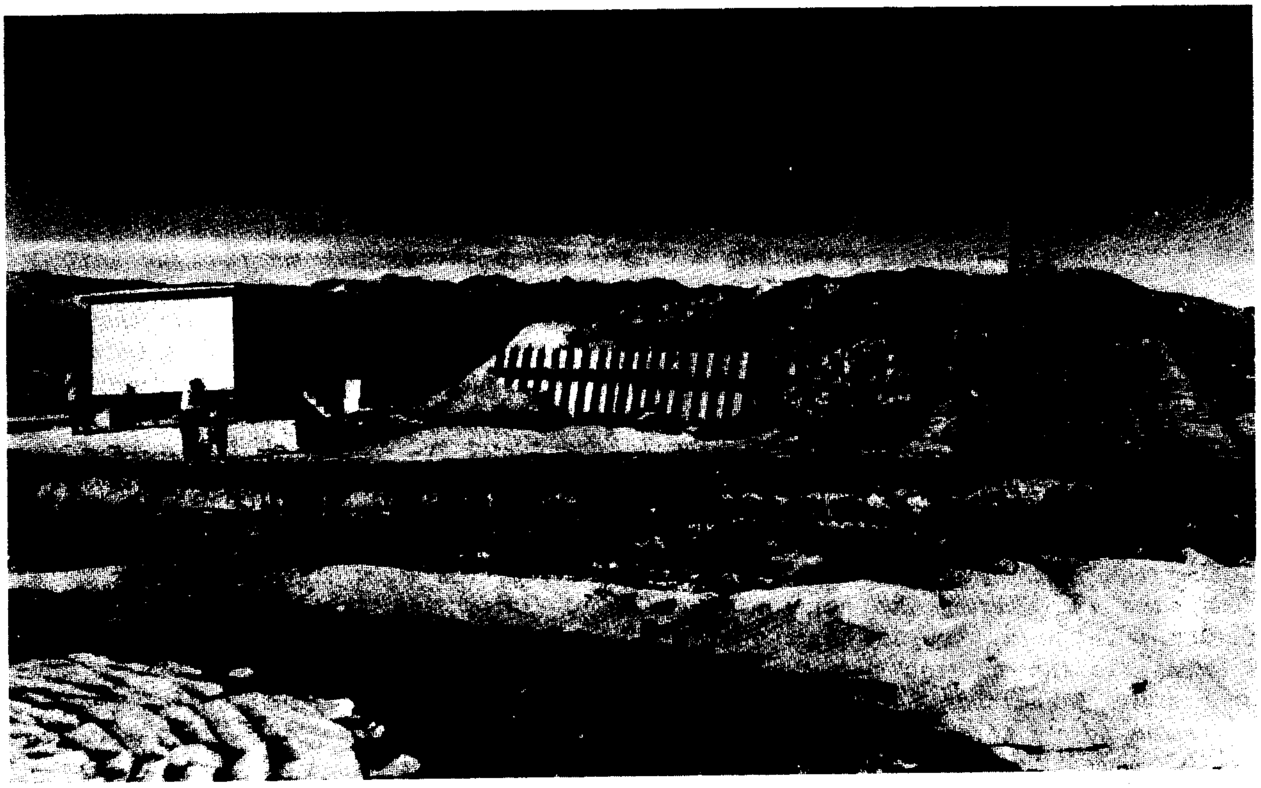

Figure 6.

Ranger alpha recording building (under the pile of dirt). The small building was removed for the detonations.

contractors to the Department of Defense for entirely different purposes and will be discussed in a later section.

As previously mentioned, the advent of the thermonuclear reaction, or boosting, at Greenhouse in 1951 led to the requirement for much faster scope writing rates, better detector response, and better cable transmission characteristics. The developments previously mentioned, largely those by the NRL group under Krause, led to the desired capability. While the reaction rates were also measured by other techniques, the simple technique of following the gamma-ray curve worked satisfactorily on the first boosted device (Greenhouse Item) [redacted]

Ex.(b)(3)

To do this properly, of course, required not only the assumption of the inverse square law, but a knowledge of the attenuation of air for the spectrum of gamma rays produced. This knowledge was obtained both by common timing between detectors at various distances and by other measurements of the gamma-ray intensity as a function of distance.