## Bhangmeter

The optical observations at Trinity in 1945, both by camera and other instruments, showed a double-peaked illumination curve for the light from the bomb. Very early calculations on the fireball expansion phenomenon also indicated that there should be two peaks to the light curve with a minimum of intensity after the first maximum coming at about breakaway, that is, at the time the shock wave breaks away from the expanding front of the fireball. This phenomenon takes place presumably because of the cooling of the fireball front as it expands and because of the formation of nitrogen, oxygen, and hydrogen compounds in the high-temperature shock front before breakaway, and because the opacity of heated air to visible radiation is sufficient to cause absorption of the light from the inner glowing hot gas. As the shock front cools it gradually becomes transparent, allowing visible radiation to escape from the inner hot regions, resulting in an increase in thermal radiation, and producing a minimum in the light curve. The time at which the shock front begins to be transparent is yield dependent.

During Sandstone, as an afterthought, a very simple measurement of the light intensity vs. time was made using a photocell driving the horizontal plates of a cheap oscilloscope. Timing was established by means of a 1,000 cycle per second signal impressed on the vertical plates. The simplicity of the Sandstone measurement technique led to the suggestion, by Fred Reines, after the formation of a permanent test division at Los Alamos, that a simple instrument designed solely to allow a quick observation of the time to the minimum might prove valuable operationally and could conceivably, in the long run, be a dependable method of yield measurement. LASL, therefore, requested that EG&G construct such an instrument and produce several in a portable form. In short order, EG&G designed and constructed a prototype basically consisting of a 930 photocell (blue sensitive surface) and appropriate circuitry to present the signal on a small oscilloscope, which had timing markers on the sweep. Appropriate expanding and compressing circuitry was arranged so that the signal would remain on the oscilloscope face. The scope was then photographed with a Polaroid camera, so that a reading could be obtained within a couple of minutes after detonation. It was common to use four or five such instruments on a detonation. The time to the minimum was then read by several different observers and the numbers averaged out to pick an official value, from which the yield was then estimated.*

Various studies in LASL, EG&G, and the Department of Defense on the theory of the minimum in the light intensity gave somewhat different exponents for the scaling law, usually not one-third. It quickly became apparent, as a result of the more detailed measurements of the light curve by NRL, that the time to the minimum varied with the color of the light observed, but the official bhangmeter continued to use a blue sensitive surface, since that was the surface that had been calibrated. The time to the minimum was affected to a certain extent by the surroundings of the

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*The author always arranged to have a bhangmeter of his own during the operations in order to get his own time to the minimum and woe be to EG&G if their official number was appreciably different than his. An intense afternoon was spent by the entire Group J-7, with its group leader Fred Reines, early in 1950, picking a name for this world-shaking device that was going to produce simple, cheap, and easy yield measurements. At the end of the afternoon, Reines picked a name which we all knew would be misinterpreted for the rest of history. Bhangmeter is not synonymous with bangmeter. Bhang is a variation of Indian hemp, the leaves and seed capsules of which are chewed or smoked, and which then produces the same euphoria as other variations of hashish. The now obvious connotation is that we were off our rockers to think that this thing would ever be particularly useful and anyone else who ever believed it must also have a little something wrong with them.

device when it was fired. A tower shot, with appreciable mass in the tower, might give an answer a little different than an airdrop. A surface shot could give a strong difference because the fireball was expanding in a hemisphere instead of a sphere. In fact, if the shot is at the surface of a perfectly reflecting plane, the surface of the expanding hemisphere follows essentially the same time history as that of an airburst of twice the yield. Since the numbers were very simple to treat, and were available to everyone who happened to be around when the shot was fired, a great number of people had their own calibration curves, which differed enough to lead to great and heated discussion, the difference usually coming about from slightly different interpretations as to the time of the minimum or different yields used for their calibration shots (for example, using fireball yield instead of radiochemical yield, or vice versa) or different personal corrections for the manner of firing. During the period 1950 to 1958, EG&G constructed several more sophisticated versions of the bhangmeter, but they all operated on the same principle. Very late in the game, a few bhangmeters were built with different photosensitive surfaces having different spectral characteristics. The bhangmeter did serve its purpose admirably. By the end of 1958, it was considered to be an instrument that would give the yield (most of the time) to plus or minus 15 percent, and it did have the advantage that it could work off reflected light at an appreciable distance. Thus, by the time of the moratorium, this was a mature tool for the determination of the yield of fairly low-altitude detonations, that is, well into the atmosphere. The author's personal calibration curve, as a result of the experience through 1958, is shown in Figure 7. The bhangmeter could be used on the Dominic airdrops, but obviously was of no value for underground detonations.

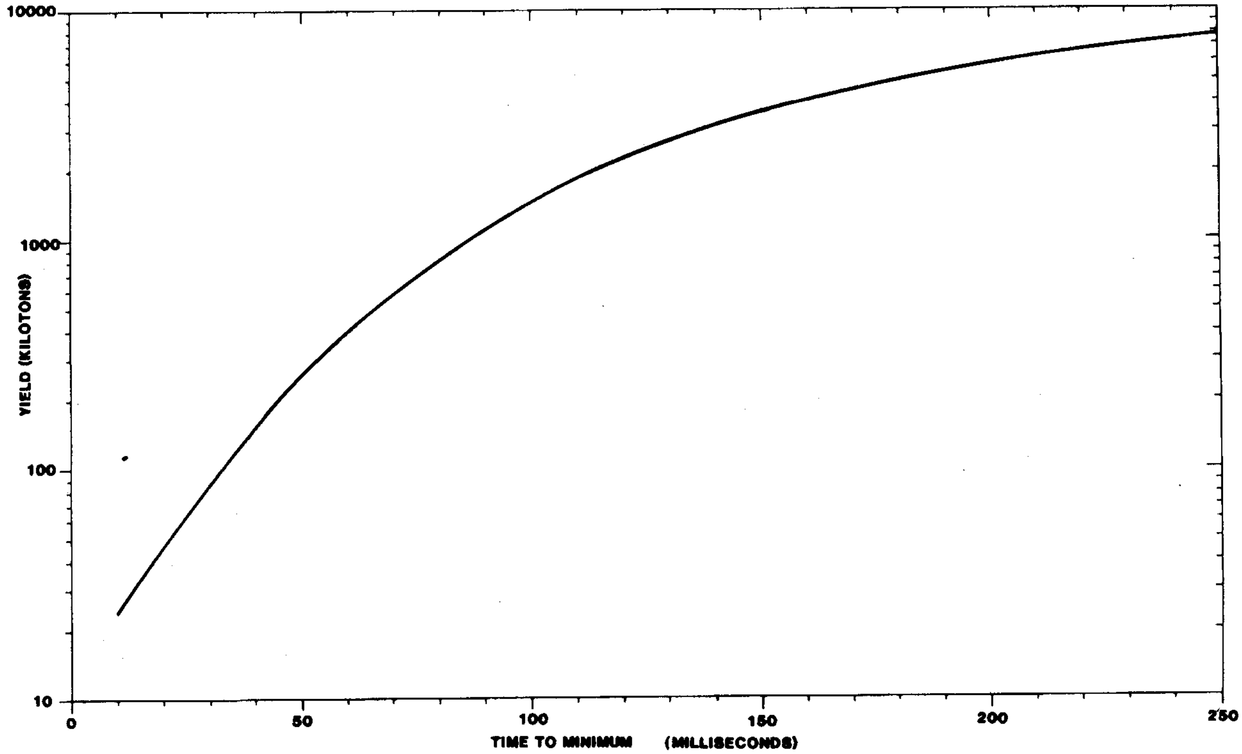

Figure 7.
Author's bhangmeter curve for high yields (1958). Yield vs time to the minimum.

## Time Interval

Beginning with the first two-stage device, Mike, in 1952, it became necessary to measure another diagnostically critical number, the time between the primary detonation and the detonation of a secondary. Having developed the primary tools to measure alpha, this was in principle fairly straightforward. But a few words on the subject are, perhaps, in order. Ex.(b)(3) [redacted] In fact, Aamodt and others on Greenhouse had observed the electromagnetic signal from single-stage devices at an appreciable distance from Eniwetok, having stations on Kwajalein, Guam etc. Sandia, in the early Nevada operations after Ranger, put stations to observe both light and electromagnetic signals as far away as Albuquerque. In general, it was observed that the time interval could be measured by the electromagnetic technique up to roughly 500 miles[*] from the detonation over a sea surface. The equipment for so doing was simple, consisting of antennas feeding directly into comparatively fast oscilloscopes, generally with amplifiers. Recognizing this simplicity, time intervals were measured at Castle largely by a single electromagnetic station on Japtan (operated by Rod Ray and John Malik[**]) at Eniwetok, which observed the time intervals both from Eniwetok and Bikini shots. Ex.(b)(3) [redacted] measurement was also made by other techniques. From then on, both in Nevada and at

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[*] Glen Jean, National Bureau of Standards (NBS)--Bikini from Wotje during Castle.

[**] The electromagnetic time interval experiment was actually designed and fielded by Bob England and Ray, but England died a few days before the beginning of Castle in a laboratory accident at Bikini. Thereafter Malik was the project leader.

the Eniwetok Proving Ground, the time interval on two-stage devices was measured by whatever technique, or combination of techniques, seemed to be the easiest at the time. If there were close-in alpha measurements or gamma-ray intensity measurements, then it was simple to observe time interval by observing the gamma signal. If that was not convenient, for example, on the airdrop Cherokee or on some of the barge shots, then Teller light or electromagnetic signals were used. All of these techniques were well developed by 1958. However, it is notable that there was not very much experience, at least in the AEC family, in making these measurements for bombs dropped over water such as we eventually did in Dominic and, hence, the question of reflection of the electromagnetic signal off the water surface had not been seriously considered. [redacted]

Ex.(b)(3)

## AEC Device Diagnostic Nonstandard Measurements

We will now briefly mention some other types of diagnostic measurements that were developed during the period 1950 to 1958, not because they were critical in the return to testing in 1961, but rather to illustrate the kind of information that, in principle, was available but in practice could not be obtained from airdrops, as in Dominic, or initially from underground shots, as in Nevada. Only in recent times have some of these types of measurements been possible in Nevada, and some of them have not yet been reproduced.

### Dinex

Dinex was performed only once, on Greenhouse George. [redacted]

Ex.(b)(3)

Ex.(b)(3) [redacted] Those going directly forward would have the same energy as the incident neutron. The protons were then sent through a collimating system into a magnetic analyzer and those of the desired energy separated out, the desired energy being essentially 14 Mev. The separation of specific energy protons then allowed the observation of a batch of particles, all of which had the same flight time from the burning region to the detector; and, hence, the time smear in the neutron signal due to different flight times was obviated. Protons then impinged directly on an appropriate collecting cup and the resultant signal was sent through cables to fast oscilloscopes at the recording station. The practicalities of the experiment involved such large amounts of materials, both in magnets and in lead to shield the cables, Ex.(b)(3) [redacted] In fact, hunks of melted lead were picked up years afterwards, on Aoman-Biijiri, as a result of that shot. The experiment cost on the order of 10,000,000 in 1951 dollars.

Ganex

To make the same kind of observation as Dinex, but somewhat less expensively, an experiment (first tried on Greenhouse Item) was designed which involved a large iron converter Ex.(b)(3) [redacted] At the converter the neutrons, through the (n,$\gamma$) reaction in iron, produced gamma rays. The observation of those gamma rays at a comparatively remote point, through systems collimated on the converter, allowed an observation of the neutron burn rate. This technique was not used very often after Greenhouse because of the observation that the boost signal could be observed by normal alpha techniques, except in unusual circumstances. Variations of the technique have been used underground in recent years.

Thermonuclear Burn Propagation Rate

On Castle Bravo Ex.(b)(3) [redacted] in 1954, Sterling Colgate and co-workers of UCRL performed a classic experiment in which they measured the burn propagation Ex.(b)(3) Ex.(b)(3) [redacted] In concept this massive experiment was comparatively simple but difficult of execution. The basic experiment consisted of collimators of the appropriate material very close to the device, that is, just outside the point which the case would reach before the secondary exploded. The neutrons from the burning secondary then passed down an array of 2,500-yard long vacuum pipes, approximately six inches in diameter, to fast detectors in a building at the far end.* The detectors converted the neutron signal to an electrical signal, which was then recorded on the oscilloscopes in the next room. Of course, it had to be shown that the cross talk from channel to channel could be kept to satisfactorily low levels. Since the propagation rate was extremely high and the burn rates were high, the most modern detection and recording procedures had to be used. This experiment, which worked very well, and other similar ones in later years, led to a better understanding of the burn propagation through thermonuclear materials, Ex.(b)(3) [redacted]

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*Krause (NRL) had performed a similar measurement on Ivy Mike using a helium-filled tunnel instead of vacuum pipes.

Ex.(b)(3)

[redacted]

Ex.(b)(3)

[redacted]

Ex.(b)(3) [redacted]

From then on, the measurements of radiation flow, or phenomena associated with it, depended in some way upon radiation-driven shock arrival at some point and the observation of that shock arrival time. The shock usually ran into air or some other medium and, hence, produced light. The light was usually observed by means of high-speed streak cameras. Most of the work in this period, 1952 to 1958, was done by Gaelen Felt's group in Los Alamos, with camera design and production being in the hands of Berlyn Brixner and P. Liverman.

The first of these streak camera measurements was on the Mike shot. The measurement was simply an observation of the rate of shock propagation through steel blocks of various thicknesses put on the outside of the steel case of Mike. The steel blocks had light-tight pipes around them leading to mirrors from which the light was transmitted to high-speed cameras at recording stations some miles away. The observations of the shock arrival time at the outer surfaces of the steel blocks allowed a calculation of the driving temperature and radiation arrival time within the case.

A similar observation with framing cameras of a box placed on the front of the Mike device with slanting thin copper strips on it, varying in distance from the case, from perhaps one-quarter inch to three inches, allowed a crude observation of the case velocity which also could be interpreted in terms of the radiation driving temperature.

Ex.(b)(3) [redacted]

Refinements of this philosophy led to the so-called "hot spot" measurements which were conducted in the mid-1950s, largely in Nevada, but also in the Pacific. By observing the light flashes at the bottoms of holes of various depths in the cases, it was possible to observe both the time of arrival of the radiation at a given position along the case and to estimate its temperature by calculational methods similar to those used on Mike. [redacted]

Ex.(b)(3)

In the mid-1950s, it also became very important to the weapons designer to get a better handle than was available theoretically on the opacity of various materials to the propagation of high-temperature radiation. Art Cox designed a variation of the hot spot experiment using so-called "opacity cells" in which radiation was allowed to enter one end of a cylindrical container of, usually, a water solution containing small amounts of the appropriate material. By observing the time that it took radiation to flow through these cells, it was possible to determine the opacity of the material to radiation. Other experiments were conducted using "idealized geometries" to observe the rate of propagation of radiation down channels that could presumably be calculated very well. All of these measurements led to a strong exchange between the experimentalists and theoreticians which assisted in the advance of the understanding of radiation flow. These experimental techniques were well developed by 1958, but there were still many uncertainties in the theoretical understanding of radiation flow and opacity. It was not possible to conduct this type of measurement on the airdrops of Operation Dominic and, initially, not feasible to do them underground in Nevada, although in recent years, variations of some of these experiments have been performed.

### Tenex

During the early planning at LASL in 1949 and early 1950, it was recognized that a measurement of the temperature of the burning thermonuclear region on George and Item would be an important check on the theoretical calculations associated with those devices. The measurement of radiation temperature was attempted by observation of the x-ray spectrum. It was also recognized that the spectrum of the 14-Mev neutrons produced by the DT reaction in the burning region would be broadened due to the very nature of the thermonuclear reaction. That is, the DT reaction takes place because of high thermal motion of deuterons and tritons reacting upon collision. The neutron from the reaction has roughly 14-Mev energy in the center of gravity system of the deuteron and triton, but since that center of gravity is moving with respect to the laboratory system, the neutron will have varying energies in the laboratory system depending upon the motion of the center of gravity. The widening of the spectrum due to this phenomenon is easily calculable for any given burn temperature. Therefore, an observation of the detailed spectrum around 14-Mev would, in principle, allow a determination of the particle temperature. The measurement of the x-ray spectrum, if successful, would give the radiation temperature. It was recognized that it is possible to have a burn in which the radiation temperature and particle temperature are not the same, so both measurements were of interest. Since the neutrons are particles and travel at appreciably less than the velocity of light, even at 14-Mev, and their velocity varies with energy in a well-known fashion, it became clear that an observation of the time of arrival of the neutrons at some point distant from the bomb would allow a detailed measurement of the spectrum near 14-Mev. Experimental criteria were straightforward. The detector had to be at such a distance that the time spread between the arrival of the lowest-energy neutron expected and the arrival of the highest-energy neutron expected was long compared to the burn time of the thermonuclear reaction. A measurement of the spectrum between 12-Mev and 16-Mev would be adequate to determine the temperature, although in actual practice, the spectrum was measured over a somewhat wider band. Ex.(b)(3) Ex.(b)(3) [redacted] Since the measurement would be simply the observation of the current from a detector, it was clear that the detector sensitivity as a function of energy was required and this quantity could be both calculated and measured in the laboratory. There was a little trickiness involved in setting up the oscilloscopes so the sweep would be on at the time of arrival of this band of neutrons. However, by triggering off the rise of the gamma signal (alpha), this problem was solved. Such measurements were conducted by Hall and Waddell for both Greenhouse George and Item and operated satisfactorily. The detector stations were at approximately 200 and 1,000 yards from the device. Ex.(b)(3) Ex.(b)(3) [redacted] Variations of this Tenex technique were used both in Nevada and in the Pacific during most of the operations up through Hardtack. (Variations are now used underground; however, the experiment is in some ways difficult because of the comparatively short distance that the detector can be from the device.) Later theoretical calculations showed that the broadening of the 14-Mev spectrum could come about for reasons other than simply temperature broadening. The deuterium and tritium mass could be moving as a body one way or another, which would only produce a shift in the peak; but if different parts of the burning region were moving in different directions, the spectrum would appear to be broadened. The effect of this additional broadening could be treated theoretically. This diagnostic technique had reached moderate maturity by 1958.

### Pinex

The use of threshold detectors led to a design of another fairly valuable diagnostic tool, but one which produced data that was perhaps more of wonder in the period before 1958 than of actual use to the theoretician, mainly because the computer codes of that time were not sufficiently developed to take account of the phenomenon observed. This measurement was called Pinex and simply consisted of a neutron camera using the high-energy neutrons, that is, 14-Mev neutrons from the thermonuclear burn region, to carry the image. A steel collimator placed some distance from the bomb furnished the pinhole of the neutron pinhole camera. At an approximately equal distance back of the collimator, a plate made of an appropriate threshold detector, initially zirconium, was placed. Upon detonation, the high-energy neutrons from the thermonuclear burn region of a bomb passed through the collimator and pinhole and formed an image on the zirconium plate of the same shape as the burn region and with an intensity related in some way to the burn in that region. Thus, a picture of the integral burn of the booster region, as shown by the 14-Mev neutrons, could be obtained. After exposure, the zirconium plate was recovered, taken back to the laboratory, sliced into very small bits, and their induced radioactivity measured. From that data, a mosaic could be built up to get a picture of the source. Later on, it became obvious that one could simply lay a piece of photographic film on the zirconium and get an image directly from the zirconium activity. Appreciably later, by shielding against the gamma rays, short half-life materials, such as aluminum, were used. Photographic film was placed against the aluminum preshot. High energy neutrons from the explosion induced radioactivity in the aluminum resulting in exposure of the film shortly after the explosion. The film could then be recovered, developed, and would give directly an image of the burn region. This technique, originally developed for tower shots in Nevada, was eventually developed for use both on primaries, and on secondaries on barge shots, even of megaton devices, since the camera could be protected by the water and recovered from the bottom of the lagoon.

By 1958, the technique was well developed and satisfactory for atmospheric detonations. However, again, we had no experience in using the technique underground or on airdrops. Parenthetically, one may note that after several years of underground testing, variations of this technique have become quite important diagnostically. The Laboratories have now developed techniques for either recovering the image recording material from downhole or producing images through the use of fluors in image-transmitting systems which can then retransmit the image uphole. But only in quite recent years have the data obtained by this method been actually useful in a calculational way to the theoretician, because only in recent years has the calculational capability been developed to handle the problem.

## Output Measurements

Another class of measurements are on the borderline between effects measurements and diagnostics measurements. They were useful on both sides of the house. Except for Trinity, the DOD laboratories did not contribute appreciably in these fields until in the mid-1950s when AFSWP began to develop significant in-house competence in the field.

### Neutrons

At Trinity, Klema exposed samples of sulfur and gold (shielded and not shielded by cadmium) to the neutron flux from the Trinity device and observed the induced radioactivity. Calibration of the particular sample geometries used on laboratory sources, such as the Omega reactor and the Van de Graaff accelerator, allowed a translation of these data into numerical quantities for the integral neutron flux as a function of distance from that device and an initial attempt to determine the spectrum. These data were very valuable to the early weapons effects philosophers. When Crossroads was planned in 1946, the methods of measuring yield were still somewhat uncertain, and it was felt worthwhile to repeat this simple measurement as one of the many attempts to compare the yield of the Trinity device with that of the supposedly identical follow-on device to be dropped in Crossroads. At that time, there was no particular conviction on the part of the weapons designers that two devices, built the same, would actually operate the same. The uncertainty had to do with the question of when the first chain reaction would actually start, an uncertainty, incidentally, that was to plague designers many times in later designs. The particular counters, sample molds, and calibration sources that were used on Trinity were found, and hence, the identical measurement could be conducted. In addition to the device uncertainty, there was some question as to whether or not the spectrum would change as a function of distance because of the reflecting characteristics of the water surface, as opposed to that of the silica sand of the Trinity site. With appreciable operational difficulty and high adventure on the part of the experimenters, the measurement was repeated on Crossroads Able and indicated that the yields of the two devices were the same within experimental error and that there was no appreciable effect of the water, probably because the Crossroads Able device was fired at moderately high altitude above the water. As a side benefit of the experiment, it was also possible to show that the bomb had missed its intended detonation point by approximately 700 yards.

When the planning for Operation Sandstone came along in 1947, it was again decided to repeat this measurement, probably for no awfully good reason except the enthusiasm of the experimenters involved. However, since the devices were of different construction, with those for Sandstone using smaller high explosive, it was to be expected that the neutron spectrum would differ to some extent. Furthermore, since the Sandstone shots were on towers, it was possible to measure the flux and spectrum with somewhat more accuracy than was possible at Crossroads and also to acquire some data on the variation with yield. For these experiments sample lines were placed both along the land and over the water surface and the results did show some difference in flux and spectrum over the two surfaces, especially in the slow neutron range as detected by gold. The actual neutron intensities as measured above the 3-Mev sulfur threshold were very nearly proportional to the yields of the devices. The fast neutrons as detected by sulfur showed an almost pure exponential drop-off with distance, after the inverse square effect was taken out, which was to be expected, but the slow neutrons showed a pile-up close to the source and extending out perhaps 200 or 300 yards. After that, the slow neutron intensity fell off essentially exponentially following the same curve as the high-energy neutrons, indicating that the far-out slow neutrons had gotten there as fast neutrons. All of this helped the understanding of neutron propagation through the air, which at that time was still under some debate theoretically.

The expectation, after Crossroads, that thermonuclear reactions would someday be attempted, led to further concentration on the part of the experimenters as to how these techniques could be used to further diagnose the devices. The expectation of a thermonuclear burn on Greenhouse made it necessary to attempt to measure the amount of burn. External threshold detectors were an obvious technique. In the period between Crossroads and Greenhouse, a laboratory investigation using the high-energy gamma rays from the betatron led to several possible new detectors, the most outstanding of which, because of its convenient half-life, was zirconium. Zirconium could be used in the field as a (n,2n) detector with the threshold at about 12-Mev. In the laboratory, that threshold could be measured using the $(\gamma,n)$ reaction, the gamma rays coming from the betatron. Obviously, an external measurement with detectors at some distance from the bomb also required information on the attenuation due to air over the distance from the device to the detector and the attenuation from the source inside the device to the outside of the device. [redacted] Ex.(b)(3) These calculations were not simple because they had to be done for the exploded geometry of the high explosive, which was not well known. Determination of the mean free path of the 14-Mev neutrons in air could be made easily by simply having detectors at several distances. These measurements were made at Greenhouse with adequate success. Another technique for measuring the overall burn was, of course, internal detectors which were then collected as part of the cloud and treated radiochemically. These two techniques, that is, both internal and external detectors, were then used through the rest of the period up through 1958 to determine the burn of the primary boost region.

Iodine, with a threshold (n,2n) reaction at roughly 9-1/2 Mev, was used on Sandstone in order to get a background calibration to see if this threshold detector would be satisfactory to observe the high-energy neutrons from the thermonuclear reaction that we could suspect was coming on some later operation. Iodine has a decay half-life of 13 days, which made moderately prompt recovery and counting important. In its use, it was necessary to use both unshielded and lead shielded detectors in order to separate out the activity induced by very high-energy gamma rays of the bomb. Neutrons coming out of the bomb and being captured in the nitrogen of air result in approximately 10-Mev gamma rays of very long mean free path, which had to be dealt with as a background.

In parallel with the above-mentioned effort on Greenhouse, Louis Rosen developed a technique to measure the spectrum of neutrons above, perhaps, 1/4-Mev. This technique involved the use of stations at various distances from the bomb with small-diameter neutron collimators many feet thick, behind which was placed a "neutron camera." The neutron camera consisted of a hydrogenous scatterer, which then emitted protons resulting from (n,p) scattering in the scatterer. The protons were recorded on nuclear emulsion photographic plates and produced tracks of measureable length in the very thick emulsion. Rosen had been using a similar technique in the laboratory and, hence, had done a great deal of work on the track lengths as a function of energy in the nuclear emulsion plates. These cameras were then collected and sent back to the laboratory, where the plates were developed and read by a great team that Rosen had at his command. This was an eminently successful technique for determining the spectrum coming from the bomb in a moderately straight line; but since it was well collimated, it had less value from an effects point of view because it did not, in general, measure the scattered neutrons, i.e., those that were not coming radially from the bomb. Obviously, corrections to obtain the total neutron flux could be made. This method of obtaining the neutron spectrum was comparatively expensive, but was nevertheless used by both weapons laboratories on the appropriate occasion throughout the remainder of the period under discussion. The detailed spectrum obtained was of appreciable value in checking the corresponding neutron output and transport calculations.

One other neutron flux measuring technique deserves to be mentioned, the so-called fission-foil camera. This device, starting with Greenhouse, collected the fission fragments emitted from plates of uranium-238 or -235, shielded or not shielded by lead or cadmium, on a rapidly moving cellophane film. The cellophane film could then be cut up into small strips and their radioactivity measured in a laboratory counter to determine the neutron flux as a function of time after the detonation. Perhaps the major pertinent point that came out of the use of this technique was simply that there was a burst in the slow neutron flux as the shock wave passed the camera.

Obviously, the total neutron output from thermonuclear burn regions could also be obtained from the reaction history experiments. However, in general, the absolute calibration of the detectors and electronics used in those experiments at that time was not sufficiently good to allow an accurate integral measurement.

### Gamma-Ray Flux

The total gamma-ray incident radiation at a distance from nuclear detonations is composed of several parts. One is the prompt radiation from the device itself during its multiplying or immediate disassembly stages. Another is the radiation from the rapidly decaying fission fragments or other activated nuclei as they mix and rise in the fireball, eventually, to form the moderately stable radioactive cloud. There is, on occasion, some contribution at ground level from the stable cloud itself; however, this is usually small because of the great attenuation of the air between the cloud and the ground. Another contribution comes from the capture of neutrons in air and subsequent decay of the resulting nuclei with gamma emission. No appreciable contribution is due to the x-rays from the fireball at distances of interest because of the extremely short mean free path of x-rays in air.

Straightforward techniques for observing gamma-ray dose had been developed over the years before Trinity for use in laboratory medical installations, etc. These techniques were used in the field at Trinity and Crossroads, where film badges and dosimeters were spread with great profusion over the area around the device. After exposure they were collected, developed, and read in the laboratory in the same fashion as any other film badge. Early-on, various shielding materials were used in conjunction with unshielded film badges and dosimeters to allow correction for the neutron dose to the film badge (the neutrons scattering in the hydrogenous emulsion produced protons which, in their slowing down, cause ionization resulting in darkening of the film). The problem of equilibrium in a hydrogenous mass, such as the human body, was recognized, so that appropriate mock-ups were made to help translate the simple observations into whole body dose. Over the years, better and better process control was established to allow more precise measurements. Early measurements showing the variation of dose with yield were made by Pete Scoville at Sandstone, but after that time, the effort was largely carried out by Ellery Storm of H-Division in LASL, and H. O. Wycott and L. S. Taylor of the National Bureau of Standards, with the assistance and guidance of John Malik. It was quickly observed that the gamma dose was, for a given device design, closely proportional to the fission yield.

More sophisticated measurements were attempted, beginning with Greenhouse, to understand the production, transport, and deposition of gamma rays. At Greenhouse in 1951, the National Bureau of Standards attempted a detailed observation of the gamma rays from the radioactive cloud in the very early stages of fireball expansion and cloud rise by means of a massive station fairly close in, with a great number of collimators pointed in different directions and magnetic analyzing systems at the end of the collimators. Unfortunately, this experiment failed due to blast damage, and was never attempted again in that form. Malik produced a comparatively simple device that allowed observation of the gamma-ray intensity above the ground surface and recorded the data underground, all of this being in a container perhaps one foot in diameter and several feet long. These devices could then be placed at several distances from detonations to observe the time history of a gamma-ray dose. It was the observations of the gamma-ray intensity with this device that allowed Malik to straighten out the initial arguments concerning the yield of the first large thermonuclear device (Mike). Both kinds of measurements were made on a great number of shots through practically all of the operations up to 1958. Parallel laboratory theoretical work combined with the field observations, including photographic evidence as to the position of the cloud and the time of cloud rise, etc., led to a fairly complete understanding of the initial processes and the transport phenomena, etc., that lead to a given dose at a given point in space from a nuclear detonation. Thus, by 1958, this subject was well in hand for normal atmospheric detonations. However, by then the reliability of the fireball technique for yield measurement and radiochemistry for both yield and other data was such that the measurement of gamma rays was no longer actively used to contribute to yield information.

## Thermal Radiation

Outstanding observations of the thermal characteristics of the Trinity shot were made by Julian Mack and others. Very detailed, integrated and time resolved spectral observations were made, along with attempts at the total radiation flux, by various optical means.

Observations were made photographically with high-speed cameras on all operations. It was somewhat difficult to deduce from these observations the actual thermal fluences because of the very complicated calibrations needed for film sensitivity, processing characteristics, optics, etc. These kinds of measurements were used to determine the absolute value of, and the time dependence of, fireball brightness, and in some of the later operations, appropriate filters were used to get some measure of the spectrum versus time.

The major effort after Trinity came when the NRL group under Wayne Hall took on the job, under Los Alamos auspices initially, to document this whole phenomenon. Preston Butler, of NRL, in conjunction with Group J-3 in Los Alamos, began to take spectrum measurements on Sandstone. Harold Stewart took over the job for Greenhouse. The need for measurements of the thermal radiation from nuclear detonations was recognized early on, since thermal radiation was one of the major effects to be expected in warfare as it was contemplated at that time. It was also expected that thermal output could be a good measurement of the yield, once understood. Since the thermal output as a function of time was directly connected to the initial stages of the blast phenomenon, or fireball expansion phenomenon, an understanding of the details of the thermal radiation was to assist in an understanding of fireball expansion, even though the thermal radiation from the shock front is a small portion of the total.

A massive program was therefore initiated for Greenhouse under Harold Stewart and Wayne Hall at NRL. That program included measurements of air attenuation from the bomb, that is, air attenuation over the path from the bomb to the receiver; very detailed high-resolution time integrated spectra; spectrum as a function of time taken on several instruments (both streak and framing cameras through spectrographs); thermopiles to attempt to measure the total thermal radiation; bolometers to measure radiant power as a function of time; and other instrumentation. The so-called blackball was invented. This was a simple device consisting of a hollow copper sphere approximately eight inches in diameter, painted black on the outside, with a maximum reading pressure gauge attached. The sphere was filled with gas (air). Thermal radiation impinging on the black surface heated the copper ball which gradually transferred its heat to the contained gas resulting in an increase in pressure. Therefore, a reading of the maximum pressure was directly related to total thermal radiation received from the bomb. These were very simple instruments that could be mounted at different distances from the detonations, were easily read, and, perhaps more importantly, collected the thermal radiation coming from all directions. The efforts of Stewart's group continued at high level through the whole period before the moratorium, sometimes under the direction of Lou Drummeter or Donald Hansen. Fantastic amounts of information were collected. Other experimenters, Sandia and various groups from the Department of Defense, entered this field of endeavor later on, but their efforts never compared seriously in the straightforward type of measurement with those of the group at NRL. Measurements were made on all the Pacific operations and all the Nevada operations. Hence, a great deal of information was collected on shots of various yields fired in various manners, but it is notable that no appreciable information was collected on high-yield, that is, megaton range, airbursts other than King shot. Coverage in the infrared was minimal.

On Upshot-Knothole (1953) and at later operations, these measurements were extended to include the so-called Chord experiments in which a fixed bright light placed some miles to one side of the detonation could be observed by highly resolving spectral instruments from another station, again placed several miles from the detonation, but in a manner such that the path of light passed fairly close to the detonation at a predetermined distance. The observation of the absorption bands, etc., could give information on those molecules formed in the air due to the gamma ray and neutron flux, or even x-ray flux, before the shock wave or fireball reached the light path. Enough analysis was performed on this great mass of data before the moratorium, mainly by NRL and the group under Herman Hoerlin of Los Alamos, to achieve a fairly complete understanding of the molecular processes taking place during the fireball expansion and of the absorption produced in air ahead of the fireball as well as other phenomena associated with the fireball expansion. These measurements showed, among other things, that the fraction of total yield coming out as a visible part of the spectrum did vary with yield, from about 45 percent at small yield to perhaps 25 percent for megaton shots. Eventually, calibration curves were devised and total thermal provided a moderately accurate measurement of yield, especially in the early operations, that is, the operations in the mid-1950s in Nevada. The measurements showed that the brightness of the fireball peaked at something like 10 to 20 kilotons, decreasing both ways from that to rather great extremes. For example, Ranger A was so cold that it showed line spectra from the components of the bomb. On the other hand, the very large bombs, 10 to 15 megatons, were sufficiently dim that they could *almost* be viewed with the naked eye safely. (However, for self-protection, no one was allowed to do that.) It is perhaps of interest to note that so much data were taken during those years that much of the spectral data still have not been analyzed, and important physical knowledge is still coming out of those data.

By the time of the moratorium, there were, counting Los Alamos and NRL, some 60 people working in the field on this subject in addition to the DOD and Sandia efforts. Through this long effort there came a great amount of theoretical and experimental knowledge which was used in developing the experimental plan for optical observations of Teak and Orange, the high-altitude shots of 1958, and even more in the theoretical predictions* as to the phenomena to be expected so that the instrumentation could be laid out properly. Thus, in 1957 and 1958, when the high-altitude shots of Hardtack were planned to gather information on the phenomenology of high-altitude detonations, a great amount of instrumentation and expertise was available, and Hansen and Hoerlin were of appreciable assistance in designing not only their own measurements on those shots, but those of other experimenters from other laboratories and from the Department of Defense. Unfortunately, both of those shots had operational difficulties so that very little of the close-in prompt data were obtained from Johnston Island. By this time, both the AEC Laboratories and the Department of Defense had learned to operate some of the optical gear in aircraft, and these were used on Hardtack. In spite of the lack of data, the experience of planning in detail for Hardtack and facing the operational problems gave the experimenters a great deal of experience which was of great value in the Dominic series.

### Blast/Overpressure

The subject of blast is certainly on the borderline between outputs and effects measurements, but, since this point was under continual contention in the late 1950s, there is little reason to straighten it out now, and hence, it will be included here. Initial experiments to study the characteristics of the overpressure or blast as a function of distance from nuclear weapons were made at Trinity in 1945, specifically by Penney (later Sir William Penney and now Lord Penney) and others. Obviously, the basic rules of the propagation of sound through air had been studied for years before the advent of the nuclear weapon. However, not so much was known about the propagation of high-pressure shock waves through air and the theory of the mechanism of the formation of the shock wave in the stage of fireball growth was in very poor shape. Much depended upon the distribution of material throughout the fireball and upon the equation of state of the air in the shockfront of the fireball front as it was growing. The equation of state depends not only on the temperature, which was uncertain, but also on the specific states of the ionic, atomic, and molecular constituents of the gas, which varied with time due to exposure by x-rays, gamma rays, and neutrons, and by the varying recombination rates of many species. Even without the complication introduced by the uncertain atomic and molecular composition

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*An appreciable proportion of the theoretical work was inspired by Hans Bethe, and carried out by Skumanich, Jahoda, and Stone.

of the "air," the interacting phenomena of radiation propagation and high-pressure shock propagation close to the time of breakaway were not well understood.

Some of the early instrumentation used at Trinity and Crossroads was remarkable for its simplicity and ingenuity, and even more remarkable for the consistency of the results produced. For example, Penney exposed sealed beer cans and five-gallon gas cans at several distances from the detonation in order to obtain a measure of the peak overpressure, the concept being that the can would crush to the point at which the internal pressure was equal to the external blast pressure. The cans could then be collected at leisure after the shot, and the volume change measured by simply pouring water in the can, pouring it out into a measuring device, and by very simple calculation deriving the overpressure. Unfortunately, this method had some difficulties. The cans did have some residual strength, requiring a correction at low overpressures; but there was some variation between cans in the crushing pressure required to get to a given volume. The temperature of the air inside could be changed by other phenomena than the shock wave and, hence, affect the volume to which it reduced for a given pressure. For example, the bomb's initial thermal radiation heated the can. The materials of the can did have some inertia, and, therefore, the volume finally achieved was dependent to a certain extent upon the temporal shape of the pressure wave. For instance, an initial very high peak would not be observed. Lastly, as was observed in later operations, the local surroundings of an observation point could produce anomalies that would affect the local overpressure. Because of the tremendous importance of an understanding of blast phenomenology and, in particular, the military need for tables which would give the overpressure as a function of distance and height of burst, etc., a great deal of effort was spent on this subject in the early years.

Greg Hartmann and his co-workers at the Naval Ordnance Laboratory (NOL, now called the Naval Surface Weapons Center) began to develop more detailed methods of observation and put them into effect during the Sandstone operation. Pressure gauges of various kinds were developed with appropriate time resolution to follow the major portion of the shock wave. It quickly became obvious that surface effects adjacent to the pressure-measuring gauge were important, so gauges were mounted on horizontal concrete surfaces or in radial walls. The formation of a permanent testing division at Los Alamos led, in conjunction with NOL, and through the auspices of Reines and Porzel and others, to a greatly expanded blast-measuring program. This led to a massive effort on Greenhouse in which new, improved surface gauges were placed both in ground surface installations and in specially constructed walls radial from the detonation. (The ground surface installations suffered greatly from the heavy rain at Eniwetok.) Efforts were made to take into account the particular characteristics of the air at the time of detonation, the wind direction, etc. In fact, small high-explosive detonations were used just prior to shot to get the sound velocity from the shot point to the detectors.*

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*One of the more exciting incidents of Greenhouse took place at a time when the arming party was in the tower preparing to arm the weapon. A member of the blast team was closing the last switches before evacuating the island and, due to a miswiring, managed to fire a five-pound high explosive on the tower not far below the cab. The arming party leader, Jack Clark, after recovering his equilibrium and allowing people to clean up the personal mess, set off in hot pursuit of the culprit and eventually found him in his little switch station at the other end of the island wondering why his circuits did not seem to be right. After the appropriate chewing out, the man closed the circuit again to show that everything was all right. The monitors immediately showed that it was not all right, and that was the end of the high-explosive part of the experiment on that shot.

At about this time, it was recognized that many other phenomena were affecting the shock wave measurements; in particular, the change in temperature of the air close to the ground due to the thermal burst, dust thrown up into the air from the initial thermal burst, especially in Nevada, etc. AFSWP began to take a larger and larger hand in the measurement of blast phenomenology, as did the Sandia Laboratory. Thus, during the mid-1950s a great spate of experiments were performed by various DOD contractors, Sandia, and Los Alamos to investigate these phenomena. Thermal measurements were made close-in to the tower shots and balloon shots at the NTS to establish the initial thermal pulse on the ground. Measurements of air density and dust loading were made close to the surface by various techniques including photography. Even the range of beta particles in the air as a function of time as the shock wave went by was measured in order to obtain the air density. The Department of Defense actually built a moderate-size lake at their Frenchman Flat site in order to compare the shock wave shape over land and over water for the same detonation. All of these measurements with the concomitant theoretical effort resulted in a fairly detailed understanding of shock wave formation and propagation and the effects of various surfaces on the shock wave shape. Unfortunately, essentially none of this work was performed on megaton bombs fired at altitudes pertinent to wartime use. However, the data were sufficient to establish height of burst curves for the military which, apparently, are still the ones in use. A great deal of the expertise on this subject was lost during the moratorium due to decreased budgets. Further measurements of blast and shock in air could not be made in Nevada on underground shots after the moratorium, and because the interest was on other subjects, very little effort was expended during Dominic on blast.

## Electromagnetic Effects

As was noted previously under diagnostics, electromagnetic effects from nuclear detonations had been observed very early. It promptly became of interest, especially to the military (AFOAT-1, later AFTAC) and others, to document this phenomenon at comparatively long times. Both close-in and long-range measurements were made very early and continued on all of the operations through Hardtack. The interest in this subject stemmed from several concepts. Obviously, the electromagnetic signal might be used as an observational technique to detect a foreign detonation and it was possible, with sufficient unraveling, that the signal could give some diagnostic information about the detonation. With the advent of the planning for intercontinental ballistic missiles, especially the Minuteman with its silo complexes, there was worry that electromagnetic signals would be picked up by the interconnecting circuitry at the missile bases and in some way render the whole launch site ineffective at a very critical time (presumably under attack by a foreign detonation). Of course, there was also strong curiosity about the reasons for the formation and shape of this signal. Close cooperation was maintained during these years between the AEC experimenters (such as Malik, Wouters, Watt, and Partridge) on this subject and their corresponding Department of Defense colleagues, and appreciable contribution to the understanding was made by the British through the JOWOG* meetings on the subject.

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*JOWOG--for Joint Working Group, which was established to implement the terms of the 1958 agreement between the government of the United Kingdom of Great Britain and Northern Ireland and the government of the United States of America for cooperation on the uses of atomic energy for mutual defense purposes.

Experiments were performed to measure the field strength as a function of distance from the bomb. An east-west effect was noted on the polarity of certain portions of the signal. The observation of the characteristics of the signal for different types of detonations, that is, airdrop, surface, or tower, and for different types of devices, small yield or large yield, boosted, etc., led to a gradual unraveling of the reasons behind such a signal, a great portion of the work being done by Suydam, Malik, and Wouters. Nevertheless, by the time of the moratorium in 1958, there were still gaps in the understanding of this phenomenon and, unfortunately, just at that time, because of the installation of Minuteman sites, an understanding was becoming more and more important. The AEC Laboratories could and did offer "rule of thumb" precautions to take against upsets of the Minuteman system, but it took the construction of simulators and field experiments during the moratorium to eventually lead to some satisfaction that the sites were safe. Obviously, there is still some uncertainty on this problem.

Various other phenomena were investigated during this period that will not be gone into in detail here. Observations of the ionospheric changes due to high-yield detonations were made by the Department of Defense and contributed to one of the later systems for the detection of foreign nuclear detonations. Observations of the changes in the earth's electric and magnetic fields at moderate distances were made in the Nevada shots, and Fred Reines even considered the use of a nuclear detonation as a source for the observation of neutrinos but eventually decided a reactor was more sensible.

## Effects Experiments

During this period of time, a great number of experiments were conducted by the Department of Defense to determine the effects of weapons outputs on materiel and people. The initial experiments were conducted by the separate Armed Forces and later on by the Armed Forces Special Weapons Project formed on January 1, 1947. The growth of the Civil Defense effort in this country, beginning in 1954 and 1955, led to another set of such experiments emphasizing civilian protection considerations. Some of these were conducted by various health organizations of the AEC Laboratories or AEC Headquarters. Large efforts were expended at Crossroads and Sandstone on military effects. Between 1950 and 1959, some 1,700 separate reports were written on the results of effects experiments conducted in conjunction with nuclear tests. Those reports were written by authors from over 100 experimental organizations, mostly under Department of Defense cognizance. Only a brief overview of the subject can be given here.

The Hiroshima and Nagasaki detonations, while clearly not experiments but the only wartime use that has ever been made of nuclear weapons, furnished in the few years after 1945 a great deal of information on the effects of nuclear weapons, especially on people. The United States at that time occupied Japan and, hence, could carry out postshot investigations with great thoroughness. Unfortunately, while the yield of the Nagasaki "Fat Man" Christy device, the same design as used in Trinity and Crossroads, was fairly well known, the yield of the Hiroshima "Little Boy" device was never determined with sufficient accuracy for evaluation of the Japanese effects data. Many attempts were made in later years to reconstruct the Hiroshima experience, even including the serious suggestion that the device be built again and fired in Nevada. But by then certain detailed documentation necessary to reproduce the device had been lost, if it ever existed. Sir William Penney tried to determine its yield by observing the blast effects on various containers found in the streets of Hiroshima but could never get consistent results. Postshot observations of apparent thermal flux and neutron flux were also used but all proved too inaccurate. Nevertheless, a great deal of information was obtained on the effects of thermal burn, of high-level radiation doses, and of the blast effects on Japanese structures, some of which were of similar construction to American structures.

At Trinity very few true effects measurements in the sense of this section were made. Bill Penney did observe the effect of radiant heating in igniting structural materials. It was intended that B-29 aircraft would be in such a position as to experience effects similar to those that might be expected in the upcoming drops over Japan, but rainy weather delayed the shot, and hence the aircraft were not properly positioned.

As mentioned before, the first postwar operation was solely for effects purposes, and used the then stockpiled MK3A Christy device as the source. Crossroads was set up by the United States Navy to investigate the effects on ships of a nuclear detonation. The Navy was particularly concerned with the problem of a detonation in a harbor and, hence, sought out a lagoon, ending up at Bikini in the Marshall Islands. The Navy had a number of outmoded U.S. military vessels that could be used for this experiment, rather than being scrapped, and also had a few captured Japanese and German vessels.

Two experiments were performed. The first was to determine the effects on ships of an airburst over water, and the second was to look at the effects of an underwater detonation. The airdrop was fired first (20 kt at 520 feet) because it was expected to do less damage than the underwater shot. Hence, it would leave ships for experiments on the later shot. The airdrop, while producing serious effects, did not do quite the damage that had been expected. But the second shot (20 kt at 90 feet depth) was spectacular. Whole ships rose up in the water spout produced, and many of the ships immediately went to Davy Jones' Locker. The radioactive contamination on the ships remaining was sufficiently startling as to color the Navy's thinking on that subject ever after.

The Navy learned a great deal about the effects of airblast and underwater shock on ships as a result of these two detonations. In general, ships suffered serious damage or were sunk at air overpressures greater than 10-12 pounds per square inch, and were damaged above 4 psi. Boilers and deck structures seemed especially vulnerable. Lethal water shock overpressure was in the 3,000- to 4,000-psi range.

Crossroads was also the beginning of the DOD effects efforts in a number of other fields. Biological experiments were conducted using sheep, dogs, etc.* Blast and thermal documentation were carried out. Water waves were measured. Effects on the ionosphere were noted. Radiological observations were made, etc.

During those years, in addition to conducting experiments on AEC-sponsored shots, the Department of Defense sponsored a number of detonations solely for effects measurement purposes. A partial list follows in Table IV.

The effects efforts during the late 1940s and early 1950s were guided by the need to understand the effects of nuclear detonations fired as then militarily deliverable, that is, airbursts, cratering bursts, underwater bursts, and surface bursts. As missile delivery became more feasible, attention turned to the effects of high-altitude and deep space detonations.

The earlier work was devoted to understanding and learning to predict the weapon outputs, and the effects of those outputs on things and people. So the effects community supplemented AEC device output measurements of neutrons, gamma rays,

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*Operationally, it was most interesting to note the placement of these live animals before the shot and somewhat hilarious after the shot, because great numbers of the animals were swimming around the lagoon being chased by their owners.

## TABLE IV
## DOD-SPONSORED EFFECTS SHOTS
## (1946-1958)

| Operation | Shot | Date | Purpose |
|---|---|---|---|
| Crossroads | Able | 06/30/46 | Airblast on ships |
| | Baker | 07/24/46 | Water shock on ships |
| Greenhouse | Easy[a] | 04/20/51 | Structures, blast |
| Buster-Jangle | Jangle S | 11/19/51 | Effects of small-yield |
| | Jangle U | 11/29/51 | Surface and cratering detonations |
| Tumbler-Snapper | TS-1 | 04/01/52 | Terrain Effects |
| | TS-2 | 04/15/52 | Terrain Effects |
| | TS-3[a] | 04/22/52 | Terrain Effects |
| Upshot-Knothole | Encore | 05/08/53 | Terrain Effects |
| Teapot | ESS[a] | 03/23/55 | Underground effects |
| | HA | 04/06/55 | High-altitude (36,620') outputs |
| Wigwam | Wigwam | 05/14/55 | Radioactive/underwater shock phenomena |
| Plumbbob | Priscilla | 06/24/57 | Vulnerability and Effects shot; Ex.(b)(3) |
| Hardtack I | Yucca | 04/28/58 | High-altitude (86,000') effects |
| | Wahoo | 05/16/58 | Underwater effects (500') |
| | Umbrella | 06/08/58 | Underwater effects (150') |
| | Teak | 08/01/58 | High-altitude (252,000') effects |
| | Orange | 08/12/58 | High-altitude (141,000') effects |
| Argus | 3 shots | 08/27-09/06/58 | Deep space Ex.(b)(1) effects |

[a] Cosponsors with AEC.

thermal radiation, and blast, gradually taking over some of the measurements completely. At the same time, they investigated the effects of these outputs on airplanes, tanks, jeeps, clothing, docks, housing, underground shelters, animals, ships, etc.[*] They studied the effects on radio and radar propagation, that is on the ionosphere. Long-range detection schemes based on these phenomena were put into operation. Methods of predicting and detecting radioactive fallout were investigated.

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[*] Perhaps one of the most outstanding effects measurements in Nevada from the point of view of the outsider was the experiment intended to be an observation of the effects of the blast wave from nuclear detonation on blimps. Several operating blimps were brought to Nevada, and appropriate mooring towers established for them at the proper distance from the expected detonation. It was important that the wind be blowing in the right direction since it was intended that the blimps be head-on to the shock wave. After a number of operational difficulties in which one blimp got loose for a while, the experiment was performed. The expectation was that since the surface of the blimp was fairly flexible, the shock wave would pass through the gas inside the blimp just canceling the shock wave pressure outside, and that no particular damage was to be expected. However, as anyone could have told them, but no one did, the velocity of a shock wave is different in helium than it is in air. Specifically, it is faster. Therefore, the shock entered the front end of the blimp as expected, but by the time it had reached the rear end, the shock wave inside the blimp was appreciably ahead of the shock wave outside. So the entire pressure differential was exerted against the rear end of the blimp and blew it right out, with the concomitant effects on the airworthiness of the machine.

Figure 9.
Crossroads Baker just emerging; note ships.

In the late 1950s, because of growing concern with the intercontinental ballistic missile and antiballistic missile systems, appreciable attention was turned to high-altitude detonation effects. Blast and thermal phenomenology were expected to be strongly different than for sea-level detonations. X-rays would become important. Bomb debris itself could get into space, perhaps showing effects in other parts of the world. Radio and radar propagation could be seriously affected. As early as the Fizeau shot (9-14-57), Sandia was investigating the effects of the fireball on [redacted] Ex.(b)(1)

During this time (1946-1958), the efforts to understand the effects of bomb outputs on people (and other animals) were also widespread. Dogs and other animals were exposed to air shock to determine the damage mechanisms. The detailed mechanism of neutron and gamma interactions with cells were studied. Skinburn and eyeburn criteria were determined. The effects of radioactive material on the skin or after ingestion were documented. Of particular note is the work of Lauren Donaldson and his co-workers at the University of Washington who have documented animal and man radiation effects at the Eniwetok Proving Ground from 1946 to the present.

As the result of the massive effort on the part of the effects community, by the time of the moratorium, the effects of low-level or surface nuclear bursts were in general adequately understood. Cratering for small shots at about "optimum" depth had been documented (although the effects for very shallow bursts were still hazy). However, the effects of high-altitude detonations were still very uncertain. On some subjects, the knowledge was still too dim to ask even the right questions.

## Systems Tests and Operational Exercises

The Hiroshima and Nagasaki airdrops of August 5 and 9, 1945, were, of course, the first nuclear weapons systems tests even though performed in wartime. As all the world knows, they were successful in that the mission was completed, the bombers were able to get away from the nuclear detonation safely, and the devices operated properly.

Thus, Crossroads Able in 1946 was the third test of the airdrop capability and did show up a difficulty. The bomb missed the target by some 700 yards. The normal explanation is that it "planed" immediately after leaving the aircraft and, hence, followed the wrong trajectory.* In the period between 1946 and 1958, a great number of devices were delivered by military aircraft. The bombs of the Ranger operation in early 1951 were airdropped from a B-50. A large fraction of the Buster, Tumbler-Snapper, Teapot, and Upshot-Knothole operations were airdrops. The 500-kt King shot of Operation Ivy in 1952 was dropped from a B-36H aircraft, and the roughly 5-megaton Cherokee detonation of Operation Redwing in 1956 was dropped from a B-52B aircraft. In the strictest sense, none of these were systems tests in that the devices were, in general, not yet stockpiled in their operational configuration, but in many cases, the shapes dropped and their weights and aerodynamics were identical to stockpiled devices and only minor modifications were made in the bombing aircraft, usually simply to arrange a radio link to start timers at the moment of bomb release. No serious genuine system difficulties were noticed during this period of time, although many minor things were observed and corrected. There were, of course, normal mechanical aircraft difficulties.** Human error was occasionally experienced.*** At the request of the technical side of the house visual bombing was used almost completely. However, there was radar backup.

On July 19, 1957, the Air Force conducted a test of Ex.(b)(3) air-to-air missile at the Nevada Test Site. Ex.(b)(3) The missile was fired Ex.(b)(3) and detonated at 20,000 feet. The crew Ex.(b)(3) received 4 R, but there was no observable dose to observers on the ground.

Thus, by the time of the moratorium, the Air Force had had a large number of experiences that were essentially systems tests using small bombs in Nevada, had gone through two airdrops in the megaton range in the Pacific, and had conducted one air-to-air missile test.

While the Navy conducted during this period of time a number of effects tests, the most notable being Crossroads in 1946, no genuine Navy systems tests were conducted.

The Army conducted its first and only true systems test in Nevada at Operation Upshot-Knothole. The Grable test of May 25, 1953, was the test of a Mark 9 artillery shell fired from a 280-mm gun. The only notable operational change between the manner in which this shot was conducted and the manner it would presumably be used in the field came about because the scientific advisor at that time, Al Graves, was not convinced that there was no possibility of the shell going off in the gun barrel. The Army, therefore, arranged the simple mechanism of a cable from the triggering

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*Listening on board ship at the time of the drop, the author remembers that the bombardier commented immediately that he had "tossed that one," possibly implying some error on his part.

**For example, at Ranger, Hoyt Vandenberg, who was at the Control Point for one of the shots, noted "The Air Force doesn't seem to be able to get rid of its built-in oil leaks."

***The "pickle barrel" in Nevada was occasionally as large as 1,600 feet in radius. The Cherokee airdrop of Operation Redwing missed by approximately five miles due to human error.

mechanism of the gun over a pulley attached to a lead brick. The lead brick was held on a small platform by a dogging mechanism which was actuated by a DN11 relay from the timing system. This simple replacement for a man operated satisfactorily, and the shell detonated at the proper altitude in a satisfactory manner.

The Army conducted a number of nuclear troop-training exercises in the mid-1950s in Nevada. The point was simply to acquaint some portion of the Army's forces with the circumstances surrounding a nuclear detonation. In general, the troops were brought into the region of the test detonation by truck and marched to prepared trenches or foxhole positions which had been placed in positions agreed upon between the Army Commanders and the Test Director. The troop positions had been determined by the Test Director to be safe from the point of view of blast, thermal, neutron, and gamma radiation. The troops, in general, crouched in the trenches while the devices went off, and were allowed to look up after several seconds to see the detonation. After experiencing the blast wave, they were again marched out. Through those exercises a representative cadre of Army personnel learned that Army maneuvers could be performed, within limits, on a nuclear battlefield.

Teak and Orange shots of Operation Hardtack in 1958 had many of the aspects of an Army operational systems test. The warhead carrier, a Redstone missile, was an early Army delivery system. However, the warhead was different than the operational system, and the guidance system had to be altered slightly to take care of the safety considerations demanded in that peacetime detonation. As mentioned elsewhere, the change led to the Teak and Orange shots going off at the wrong position in space.

No Marine systems tests were conducted during this period.

In retrospect, probably the most beneficial training to the Armed Forces, in a sense, came about from the policy of placing many military people in the AEC Laboratories as staff members, both to help conduct the operations and to work in other related weapons fields. The people generally stayed for two or three-year tours and were integrated intimately into the laboratory work, both at Los Alamos and Livermore.

## Summary of Measurements

In general, the period 1945 to 1958 saw the development of a vast array of weapon diagnostic techniques, many of which could be altered to be useful on underground shots. The period saw the collection, compilation, and theoretical understanding of the effects of nuclear weapons fired low in the atmosphere, on the ground surface, or underwater, and saw a great growth of knowledge in the military on the possible uses of nuclear weapons in "conventional" warfare. However, knowledge of the effects of detonations at high altitudes was still very primitive.

## Organization

The field organizations varied appreciably over the years 1945 to 1958. To a certain extent, the organizational structure, especially in the upper levels, was dictated by the responsible Washington-level agencies. Trinity in 1945 was somewhat unique in that the major technical organization (Los Alamos Project Y) was a part of the branch of the armed forces (Army) responsible for the whole nuclear weapon effort, and hence the effort was all "in house." By the time of Crossroads (1946), the Atomic Energy Commission had been formed, so the problem of proper assumption of authority and responsibility between federal agencies reared its ugly head, never to be really settled to everyone's satisfaction during the period of interest. The problem was not particularly serious on Crossroads, or on the similar later operation Wigwam, because the tests were clearly for effects purposes under the military, and AEC help was required more as a service, although it was never completely one-sided. In the later Pacific operations, 1948 through 1958, where the major purpose was clearly AEC, but the management was military, serious management problems arose. Those management problems never seemed to affect the actual conduct of the operation in any measurable fashion, but were usually serious enough to result in recommendations for organizational changes at the end of each operation. At Trinity, the overall administrative head, K. T. Bainbridge (he seems not to have had a more descriptive title), was part of an organization under direct contract to the Army, and the line of authority to him from General Groves and Oppenheimer was apparently clear and simple. However, when Sandstone (1948) was being put together, the Test Director (Darol Froman) was appointed, and then the AEC, feeling that the large amount of military support needed should not be under the command of a civilian (and knowing that the military would probably not agree to such an arrangement anyway), requested that the military supply a Task Force Commander. In a short time, the Test Director found himself three lines down in the organization chart, without the real authority to guide the operation in the manner he thought best. Fortunately, in that operation and in the later Pacific operations, the personalities involved were such that serious conflict was normally avoided.

The Task Force Commander for Pacific operations in general reported to the Joint Chiefs of Staff, through the particular Chief representing his service. During operational periods, he was also designated the senior representative of the AEC by the Commission, in order to have the top responsibility in the field embodied in one man. However, the Commission also usually made it clear to the Scientific Deputy Commander that he was expected to guard their interests.

After Sandstone (1948), the AEC and the military agreed that the man in charge of the technical work of the operation would be at a level just below the Task Force Commander, and would be designated "Scientific Deputy Commander." In order to assist the Commander, the military also designated military deputy commanders. Early on, the commanders of the Task Groups, the next operational level down from the commander, usually outranked the military deputies. That situation was reversed in later operations, in order to give the military deputies a more responsible role. Neither situation was really satisfactory in the period from 1948 to 1958 because the work of support by any one service did not require the efforts of two senior men. (However, this redundancy became valuable in 1962.)

There was formal agreement that all of the technical projects to be conducted in a Pacific operation would be under one man from the beginning. The intent to make the Technical Director second in command was always difficult to arrange formally. In practice, except for momentary flurries, it always worked that way because of the personalities of the personnel involved. Since that one man was (from 1948 to 1958) from the AEC side of the house, two levels of difficulty continually arose. At the Deputy Commander level (Task Force), the military deputies, and sometimes the services they represented, tended to resent, or dispute, the apparent seniority of the scientific deputy, and occasionally the Task Force Commander got tangled up in the problem. The upgrading of the rank of the military deputies after Greenhouse exacerbated the problem somewhat. Within the technical community, the AFSWP (Armed Forces Special Weapon Project) doubted the impartiality of the Scientific Deputy, especially in the later operations, and arranged for a military deputy to the Scientific Deputy. That deputy was always helpful. In a similar vein, when Livermore began to test nuclear devices, they too asked for a deputy.

Beginning with Sandstone, the work of the Task Force was divided among "Task Groups." One of these contained all of the experimental programs and projects. The others were thought of as support groups, although on occasion some technical project was assigned to one of the support groups for various reasons (for example, the work of AFTAC). Initially, the senior technical man was the head of the technical Task Group; however, when he was moved to higher level, another senior man was picked to run the Task Group. The relationship between these two men was also initially hard to define. Again, the situation could have become difficult if it had not been for the personalities of the individuals concerned. In practice, it seemed that the main job of the Scientific Deputy (or Scientific Director), aside from his safety responsibilities, was to assist the Technical Task Group Commander in his negotiations with the rest of the Task Force in order that he could accomplish his job.

On occasion, there was a problem brought about by the Task Force Commander getting involved with the DOD experiments in such a manner as to give them a different aim than that intended by the sponsors. Sometimes this helped, sometimes it did not.

Between Sandstone and Greenhouse, a permanent test division was set up at Los Alamos (J-Division). That division not only had the responsibility to plan and carry out the nuclear test work of the laboratory, but by agreement with the AEC Headquarters and the Department of Defense also carried out the administration and planning for the other technical agencies. Thus, through the auspices of "Task Group Point One," a single agency coordinated the technical planning between overseas operations and acted as the administrative agency for that work during the operations. In order to assist, the DOD assigned people to that group in Los Alamos, sometimes amounting to 70-90 people. Later on, representation was also furnished by other users, such as the Livermore Radiation Laboratory. This group dealt directly with the experimenters in arranging such things as physical layout, shipping, communications, construction, classification, etc. It acted as the administrative link between the experimenters and the outside action agencies, such as the Task Force headquarters and the other Task Groups. The existence of this permanent planning group established continuity between the overseas operations after Greenhouse. The group also assisted appreciably in Nevada operations, but only within the framework of the permanent Nevada Test organization.

Looking back, probably the major difficulties in the Pacific operations arose because of a basic inconsistency in aim. There was usually an urgency to start the operation on time and finish it as soon as possible (sometimes Presidentially directed). This urgency could be produced by programmatic aims, economics, or political consideration, or simply the desire to get the operation over with and go home. (A common statement was, "This delay is costing us a million dollars a day.") The personnel of the administrative structure usually felt this urgency strongly. On the other hand, each shot was being fired for a purpose, and each experiment was being performed for a purpose. Most important, the line of responsibility for the success of those shots or experiments was not through the temporary Task Force structure, but through the permanent AEC Laboratory or AFSWP structure. Thus, a person on the technical side of the house might sometimes feel that the shot was being fired without purpose because he was not properly ready to make the appropriate measurements, whereas the person in the administrative line might feel that the need to get the operation over, to get the right weather, etc., should override the needs of a particular experimenter, especially if it were a comparatively small experiment. This tug-of-war eventually led to agreed-upon lists of experiments that had to be ready before the shot could be fired, lists of other experiments that had to take their chances. A great deal of effort at higher staff levels was expended in continually trying to balance the conflicting points of view, and it is to the credit of all of the administrative people, on both sides of the house, that the operations were eventually conducted within moderate time limits, for reasonable cost, fairly safely, and with a high return of experimental data. Toward the end of the period, the suggestion of continuous testing, at a lower rate, was made by a number of organizations. Such a system may well have reduced the philosophical conflict noted above.

In Nevada, the situation was different. While appreciable military support was needed, the major "housekeeping" functions of transportation, housing and feeding, shipping, security, etc., could be done by the civilian side, so it was agreed at the Commission and Military Liaison Committee level early-on that the Nevada Test Site would be operated by the Atomic Energy Commission. The AEC appointed a "Test Manager," initially out of the Albuquerque Operations Office, and later from the Nevada Operations Office, to be responsible for test operations. The test manager had no responsibility or authority with respect to the technical program. Operations at NTS allowed an organization much more consistent with the internal Laboratory or AFWSP structure, with military support being integrated, but not controlling. By agreement, the Test Manager appointed a "Scientific Director" or "Scientific Advisor," initially from Los Alamos. Later on, the appointment to the position alternated depending on the sponsor of the particular shot. In the early operations in Nevada, a single "Test Director" was responsible for all experimental projects, but the growing test program of the Livermore Laboratory eventually made that system unsatisfactory, so that "Test Groups" were formed, allowing each major test organization to have its own "Test Group Director," responsible directly to the Manager (and the sponsoring organization). At approximately the same time, the area of the test site was divided in such a manner as to reduce interference between the users.

In general, the Nevada operations seemed to go somewhat more smoothly than those in the Pacific, partly because they were smaller and simpler, and because the participants were closer to home and hence did not feel so captive, but mostly because the chain of command was only slightly skewed from normal by the test command structure.

## Other

A number of other competencies needed for nuclear weapon testing were developed during these years, but will not be covered in any detail here. Most important perhaps was the development of the radiation safety (rad safe) structure in both the AEC and DOD. Measurement and prediction ability grew as a result of the large efforts put in on both sides of the house. This work went hand in glove with the continued effort to understand the effects of radiation. The prediction capability depended strongly on input from the weather prediction units, also gradually developed to work with the rad safe prediction units.

Field construction was handled with growing competence during this period by several companies, the most outstanding being Holmes & Narver (H&N) in the Pacific, and Reynolds Electric and Engineering Company (REECo) in Nevada. Their expertise was essential to the return to testing in 1961-1962.

Other functions, such as shipping, the care of legal problems, security, and safety, were handled by people of growing experience in the nuclear weapon test field.

Of great importance, a small group of people with great and broad competence in the various nuclear weapon effects, and with understanding of operational problems, had come into existence. Sometimes associated with "weather panels," "safety panels," "advisory panels," or with more specific problems, they furnished a cadre of trusted judges to whom the Task Force Commander, a Scientific Deputy, Test Manager, or Scientific Advisor could turn for guidance when the chips were down. In a number of operations, there was a tendency to leave this group off the organization charts, but their help was of great value. In no particular order, some of these people were A. Vay Shelton, O. W. Stopinski, L. Joe Deal, Carter Broyles, Ralph LaChavese, Gordon Dunning, Clint Maupin, Mel Merritt, and John Malik.

## Prologue Summary

During the period of 1945 to 1958, the British, Americans, and Russians tried, both through the auspices of the United Nations and by separate conferences, to arrive at an appropriate agreement for arms control and specifically for the control of nuclear weapon testing and stockpiling. These attempts were in general not successful, in part because of the Russian need to establish a nuclear weapon capability of their own and in part because of the United States insistence on "adequate" control systems. In the late 1950s, because of the rapid growth of Russian nuclear weapon capability, and because of worldwide reaction to the "dangers" of radioactive fallout, the pressure to halt nuclear weapon testing grew strong, and by late 1957, Eisenhower was feeling that pressure and seeking ways to come to some agreement on the subject.

Advancement in American nuclear weapon design was great. [redacted]

Ex.(b)(3)

Many types of testing methods were proven out during the period. Towers, barges, balloons, airdrops, underwater, underground, and rockets were all terms that became familiar. However, by the end of 1958, balloons in Nevada and barges on the Pacific were the most commonly used platforms for testing.

Permanent proving grounds had been established in the Pacific and in Nevada, with permament on-site staffs. The major testing organizations all had permanent testing groups. By the end of 1958, a seasoned, experienced, testing organization existed and was operating. But by the end of Hardtack Phase II, it was tired.

Diagnostic methods were developed during the period beyond that available at Trinity. The reaction history could be measured in great detail. The observation of radiation flow and thermonuclear burn was well advanced. The gamma ray, neutron, thermal, blast, and electromagnetic outputs of nuclear devices over a wide range of yields had been measured for sea-level detonations, and were moderately well understood theoretically.

The effects of sea-level detonations were investigated in great detail. Blast and thermal effects on ships, buildings, animals, etc., were tabulated. Both prompt and delayed radiation effects were well understood by 1958. Fallout predictions and the predictions of other hazards could be made with sufficient accuracy for operational decisions.

However, some things had not been adequately investigated. The effects of high-altitude detonations were still uncertain. A number of possible weapon designs had not yet been shown to be safe. The whole field of reentry vehicle hardness and vulnerability was in its infancy.

In short, by 1958, there was a mature nuclear weapon design and testing system, nuclear effects from sea-level detonations were well understood, the world was afraid of atmospheric nuclear weapon tests, and we were just beginning to learn how to test underground. Many of us did not want to learn, ever!