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LOS ALAMOS SCIENTIFIC LABORATORY

OF THE UNIVERSITY OF CALIFORNIA ○ LOS ALAMOS NEW MEXICO

PROCEEDINGS OF THE CONFERENCE ON
SCIENTIFIC APPLICATIONS OF
NUCLEAR EXPLOSIONS HELD JULY 6-8, 1959
LOS ALAMOS, NEW MEXICO

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PROCEEDINGS OF THE CONFERENCE ON SCIENTIFIC APPLICATIONS OF NUCLEAR EXPLOSIONS HELD JULY 6-8, 1959 LOS ALAMOS, NEW MEXICO

Compiled by
G. A. Cowan

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PREFACE

The Los Alamos Scientific Laboratory sponsored a meeting of staff members, guests, and consultants to the Laboratory from July 6 to 8, 1959, to discuss various proposals for scientific applications of nuclear explosions. The purpose of the meeting was primarily to help evaluate proposals for such experiments with respect to their intrinsic scientific value and the uniqueness of the requirement in each case for a nuclear explosion as an experimental source. It is hoped that future experiments with nuclear explosions, if there are to be any, will provide an opportunity to carry out some of the more promising and constructive of the many intriguing possibilities suggested by the conference participants.

The collected proceedings are presented in two volumes, one classified (LAMS-2442) and the other unclassified. The requirement for classification of some of the papers is due, in most cases, to the unavoidable inclusion of weapons data. Some of the papers have been editorially prepared for publication and others are verbatim transcripts of informal talks. A few of the talks presented at this meeting have not been available for inclusion in the collected papers.

CONTENTS

1. HIGH RESOLUTION NEUTRON SPECTROSCOPY WITH NUCLEAR EXPLOSIONS

Donald J. Hughes
Brookhaven National Laboratory

The field of high resolution neutron spectroscopy has advanced rapidly in the last decade and has produced much data of value to nuclear structure theory. On the other hand, the results concerning neutron cross sections in the resonance region have great practical utility in the design and understanding of nuclear reactors.

Let me first spend a few minutes giving you a simple lecture on what the field is all about, then I shall go into the matter of what we can do with the nuclear explosives, hoping that you will then share my enthusiasm for the nuclear explosion as a source for neutron spectroscopy. The results of the measurements (parameters of neutron resonances) furnish a detailed view of the levels in a very narrow region of excitation energy for many nuclei. Most of the results have come from nuclei with mass numbers greater than 80 or so, simply because these nuclei usually have closely spaced levels in the neutron energy range where the instruments have high resolution. The region of excitation energy which we are here considering is obviously slightly above the neutron binding energy, hence about 5-8 Mev, where the level spacing is typically a few volts. Thus at this energy there is a small band, say one kev wide, where there are many levels that we can study with really high accuracy.

The levels appear as resonances in the measured cross section, plotted as a function of neutron energy. Each resonance represents an energy level at some 5-8 Mev excitation although the corresponding neutron energy is only a few ev. It is an interesting and useful fact that the total cross section, which is the easiest type to measure, gives a lot of information about the energy levels. For example, Γ, the total width of the level, which gives the life time of the state, is measured just by the width of the resonance in electron volts. The excited state can decay by emitting a neutron, various combinations of gamma-rays, and in some cases by fissioning, or emitting protons or alpha particles. All of these modes have corresponding partial widths, which can also be measured -- the neutron width, the gamma width, which is in turn made up of many partial gamma-ray widths, a fission width and so on. It is also possible to investigate the spacing of the levels, both the average spacing for various nuclei, and the matter of how the spacings are distributed within a single nuclide, whether at random or with some regularity. Finally, there is the spin of the level, J, which also is now being measured by methods of neutron spectroscopy. All of these results obviously have a close connection with nuclear theory and they have been of great value in the development of such theory, primarily with regard to nuclear models.

The problem of getting good data is primarily one of developing a neutron source of high resolving power. The best way to describe the present status is by means of recent data in the neutron spectroscopy field. Figure 1 shows a surprising change of cross sections with time.

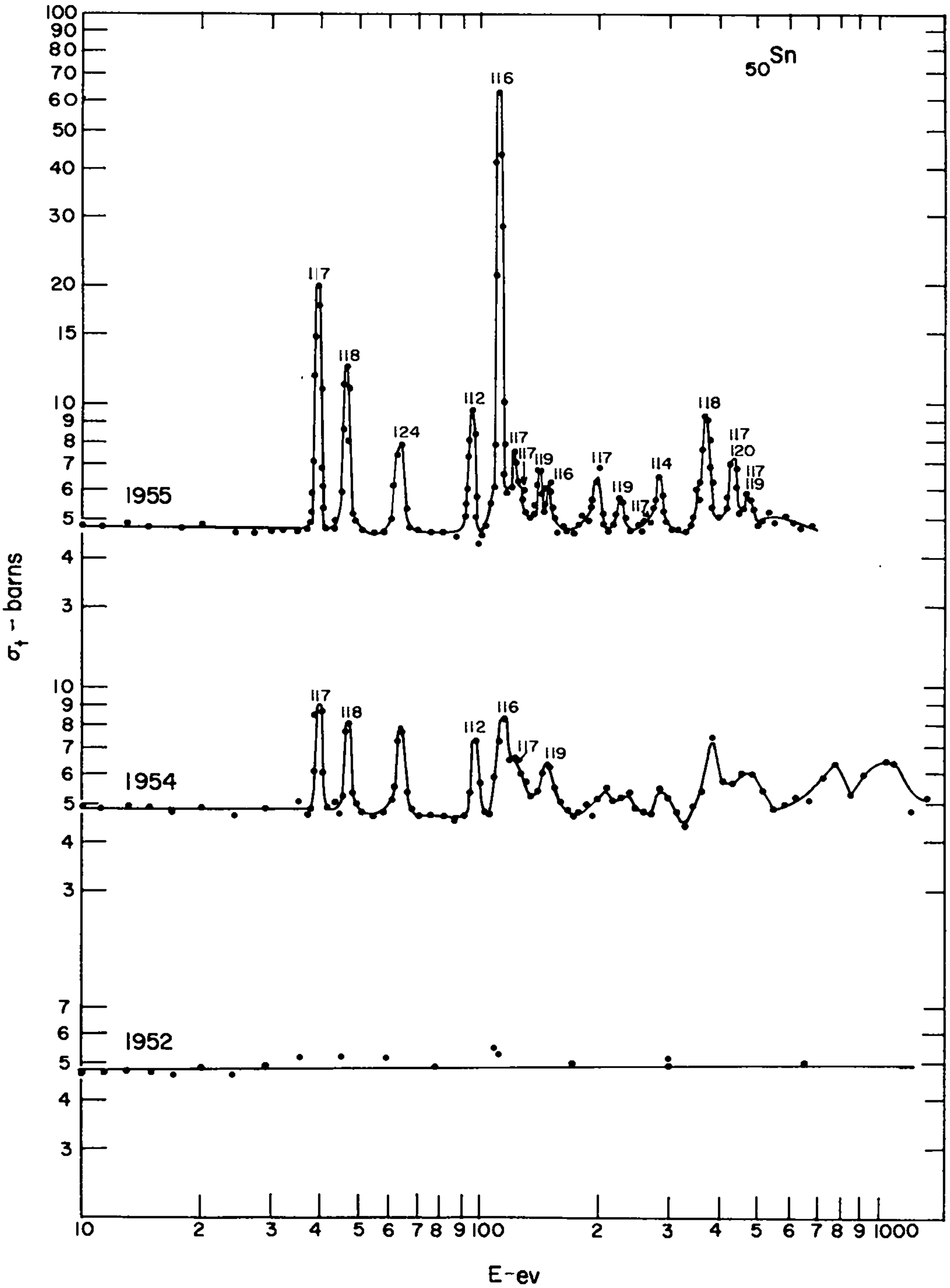

Fig. 1 - The measured cross section of tin published in various editions of the compilation BNL-325. The curves show the marked increase in the number and size of resonances with increased resolving power.

The cross section of tin in 1952 (the dates refer to successive editions of BNL-325) appeared structureless. At the time this behavior seemed reasonable because tin has 50 protons and 50 is a magic number. However, the truth is that magic numbers have nothing to do with the results. In 1954 some resonances started to appear; in 1955 they were much more prominent, there were many more of them, and we even knew to which isotopes of tin they belonged. Should we include a 1959 curve we would see that the low energy region would hardly change at all, but at higher energy we would now have more and sharper resonances. So as resolution gets better and better, the information gets better -- but we must be careful at each stage that the information we give to theoreticians is trustworthy, so that we in turn can trust their theory. Some recent work done at Brookhaven on iridium is given in Figure 2 to show the appearance of resonances as the neutron energy increases up to a few hundred ev. At a few ev the resolving power is sufficiently good that we get the kind of information just described directly from these curves. But as we get into the upper end of Figure 2 we obviously can't resolve all the levels -- the thing that is needed to get more and better data in this field is simply higher resolving power.

Of the instruments that have been used in neutron spectroscopy, the most prolific in data production have been fast choppers; a fast chopper of Brookhaven design, Figure 3, was installed about a year ago at the new NRU reactor at Chalk River, which has a central (thermal) flux of about 2×10^{14}. The fast chopper, essentially a mechanical shutter

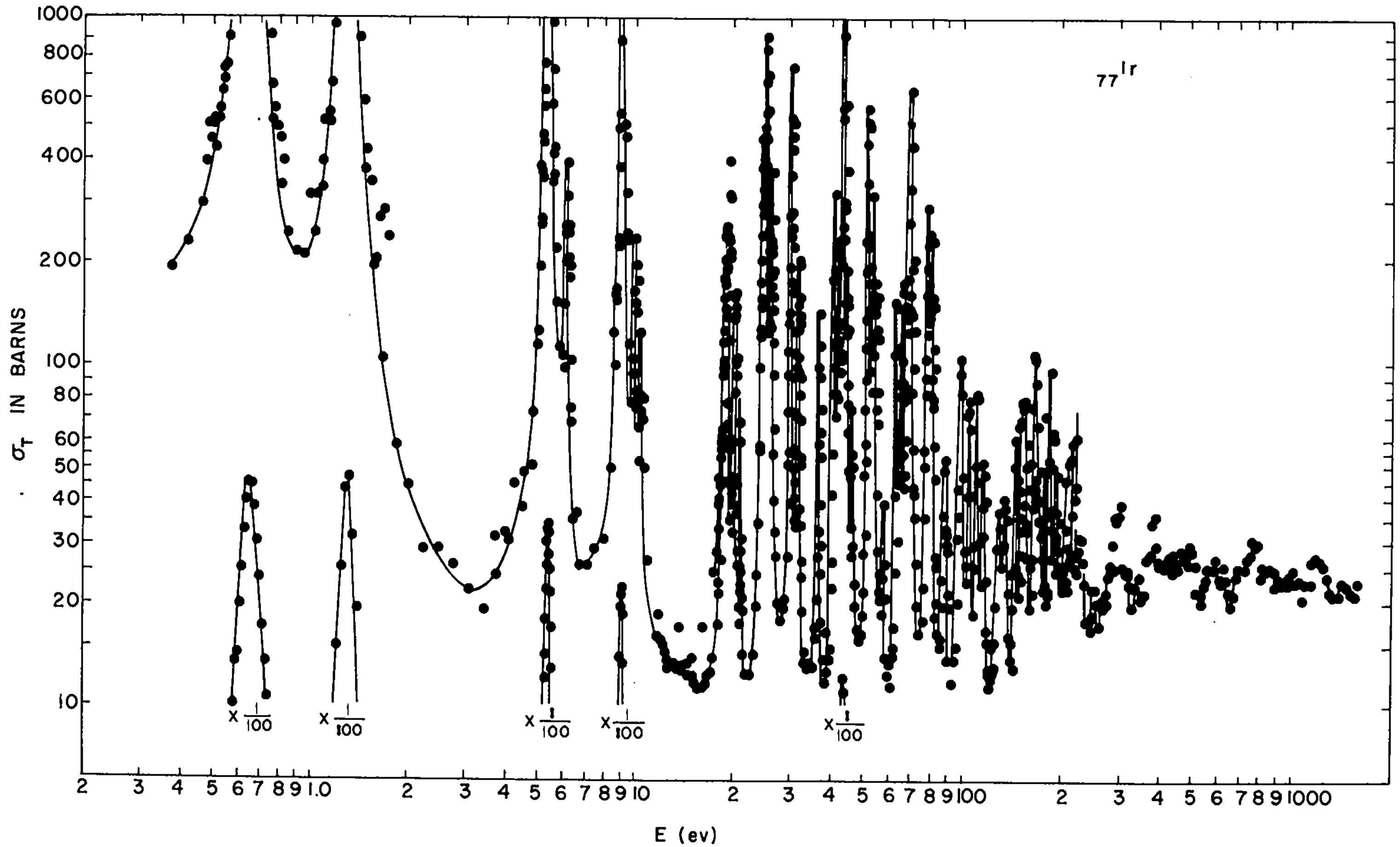

Fig. 2 - The total cross section of iridium as measured recently with the Brookhaven fast chopper. The failure to observe resonance above a few hundred ev is caused by insufficient resolving power.

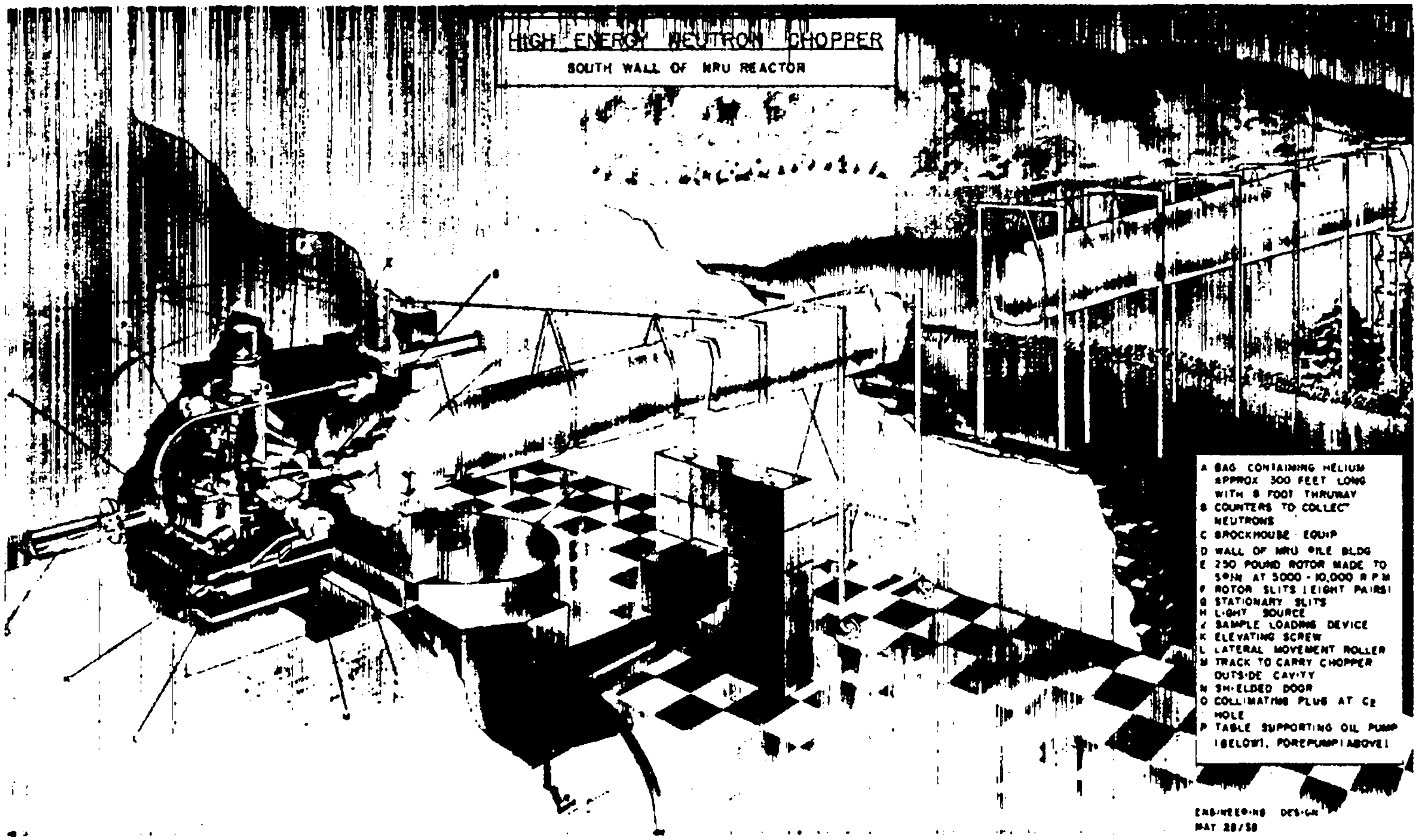

Fig. 3 - Over-all view of the fast chopper recently installed at the NRU reactor at Chalk River, Canada. The chopper is installed within the reactor shield and the flight path extends outside the reactor building.

roughly three feet across, is placed at a beam hole that passes through the center of the reactor. The neutrons are chopped into bursts as the chopper spins, each burst lasting one micro second, about the shortest burst that can be obtained by these mechanical devices. The chopped beam then travels through a helium-filled balloon, which, when completed, will continue over the country side until it ends about 200 meters away on the bank of the Ottawa River.

In resonance analysis the resolving power is always stated in terms of microseconds per meter, or the uncertainty in time in microseconds, which in a device like this would be about a microsecond, divided by the length of flight. If we have a 100 meter flight, the resolving power would then be 0.01 μs/m. In terms of energy we would then have a spread of 0.01 ev at 10 ev, and 10 ev at one kev. This resolution is sufficient for accurate resonance analysis for some twenty resonances in most materials but not much higher.

The pulsed machines are about comparable in resolving power. The best resolution with a pulsed machine, 0.01 μs/m, has been obtained with the Columbia synchrocyclotron. Linear accelerators recently are doing very well also. One of the best is the French accelerator which produces pulses of about one microsecond, and a neutron emission rate of about 10^{15} neutrons per second during the burst. A recent fission cross section measurement of U^{235} with this instrument is definitely superior to previous results.

Now the main thing I want to examine is the potentiality of a nuclear explosion as a source for neutron spectroscopy, relative to the fast chopper at the NRU or a typical pulsed source. First, let's talk about intensity and compare the explosion with a pulsed source. In order to make things international, I'll talk about the pulse source that the Russians are presumably building at Dubno, near Moscow. This is a pulsed source that isn't out of this world by any means, but is designed for an extremely high intensity, 10^{17} neutrons per second during a 10 microsecond burst. This source is a "pulsed reactor", actually similar to the one here at Los Alamos, but they hope to have a high repetition rate. The period would be something like 1/10th of a second. These figures would give 10^{12} neutrons per burst or 3×10^{20} per year of continuous operation.

Now it is easy to compare this source with a nuclear explosion, for it would need a moderator near or around it, and so would the nuclear explosion, in order to produce resonance energy neutrons. One way we could use the nuclear explosion for high resolution neutron spectroscopy would be to place a moderator near it and time the neutrons' flight down a one kilometer vacuum pipe to a detector. Crudely speaking, we can compare directly the 3×10^{20} neutrons per year with a nuclear explosion, which could give of the order of 10^{24} emitted neutrons. From this standpoint, this one shot is equivalent to the operation of the Russian pulsed reactor for 3000 years. From this simple comparison, it is obvious why somebody in the neutron spectroscopy business feels that a nuclear explosion is quite a potent thing.

Now let's rate the explosion with the other instrument, the fast chopper at the NRU, which gives a very high intensity for this type of instrument. Let's consider the explosion, and a few meters from it a slab of moderator about a meter square. The slab can't be too thick because we don't want to introduce too much uncertainty in timing, so let's make it so thin that only 10% of the incident neutrons get moderated in it. Then 100 meters away we place the neutron detector and the samples being measured. Under these conditions, starting with 10^{24} emitted neutrons, it is easy to compute the number in a given energy range incident on the detector. The result is 4×10^{10} neutrons per square centimeter in a one ev interval at 10 ev. For comparison with this result we have the intensities that we are actually getting from the fast chopper at the NRU reactor. There we have a certain counting rate, which converted to 24 hours a day for a year gives a figure of 10^7 neutrons per square centimeter in the same energy interval. So strangely enough, we end up with about the same factor as we did for the pulsed reactor -- one shot is the equivalent to the operation of the fast chopper for a few thousand years.

Upon seeing figures such as these, the usual reaction of somebody in the high resolution neutron field is, "How can you use all this intensity in such a short time?" Well, outside of the difficulty of planning your experimental program for the next 3,000 years, there are other difficulties too. Fortunately, they are not really so bad. There is no real trouble in getting these neutrons out on time, and so on. True, they are much slower than the ones that people have been considering for other experiments with nuclear explosions. Nevertheless, they still are well in advance of a shock wave so that they can get out and data can be recorded in time. The information that must be recorded is rather complicated, as shown by the typical data of Figure 2.

The way these data are recorded in the usual instruments is by very complicated thousand channel time selectors, and the big problem is to keep the data in the machine for many, many days. For this purpose magnetic drums and such storage devices are used. In one way the recording of data for a shot is much simpler instead of much harder, because you don't have to keep the data in the machine for weeks. It is obtained all at once and by recording current, not counts. The information would be recorded by photos of oscilloscope patterns, and from the people who have done this, I am quite convinced that the kind of detail that would be observed in this experiment can certainly be recorded. Thus, I feel that recording data -- the time analysis and the storage of intensities in thousands of channels -- is actually easier.

The second difficulty concerns the type of experiments that can be done. The experiment usually performed is simply a transmission, in which the intensity without the sample is recorded, then the intensity with the sample in the beam. There is no reason why an experiment of this type can't be done with an explosion in exactly the same way. The more complicated experiments concern partial cross sections, where instead of a simple neutron detector, and sample in and sample out, other measurements are made. For instance, a scattering experiment is one in which we have some sort of neutron detector near the sample, in which we measure the intensity of the scattered neutrons as a function of time. Another possible experiment is one in which we have a gamma-ray detector, and we study the capture cross section, or a fast neutron detector for measurement of fission. These partial cross sections, as the total cross section, can all be measured in the same way by using the correct instrument and recording current.

There are one or two things that can't be done, however, in the ways they are now being performed. One is an experiment that is receiving a lot of attention now, the study of the individual gamma-rays following neutron capture. Experiments are now being performed in which the relative intensities of a particular gamma-ray, for example the ground state transition, are measured in the different resonances. The results show how transition probabilities to the final state vary from different initial states. At the present time, these measurements are made by pulse height analysis combined with the time analysis to identify gamma ray and neutron energy. The simple pulse height analysis is obviously impossible when current is recorded, but I am sure that experiments of this type can still be done by using a better type of energy measurement, involving magnetic analysis for example. The only type of experiment that is not feasible with nuclear explosions is the one in which we measure coincidences to study the decay scheme of the capture gammas in detail. But with the exception of coincidence measurements, all the standard experiments can be done, as well as others not now possible.

As far as the type of experiment to do with the high intensity of a nuclear explosion, I think the first thing that would occur to anyone in this field would be to do a really complete analysis of U^{233}, U^{235}, and Pu^{239} in the resonance region. This analysis would mean measurement of total cross section, fission cross section, fission fragments and so on. These measurements are all of the type that can be done now but relatively so poorly that nuclear explosion results would constitute a completely different order of magnitude in accuracy. Another possible experiment would be the detailed study of neutron capture, not only in resonance levels but between levels as well. This latter is essentially the type of measurement that can't be done at all with present techniques.

In order to impress you with the need for high intensity, let me tell you a little about the partial gamma ray experiment. In this particular case, Figure 4, the compound nucleus is W^{184}, and there is a ground state gamma ray of 7.4 Mev, as well as a transition to the 2+ state about 100 kilovolts away. The point is that we can identify the spins of the levels; if we find the ground state gamma, the level has J=1 because this gamma can not be emitted for levels of the other possible spin, J=0. Figure 5 gives the pulse height in the crystal, showing that the ground state gamma, the last bump in the curve, is down in intensity by a factor of 1000 from the low energy gamma rays. The experiment consists of measuring this gamma ray from level by level, and it is not surprising that the counting rate is so low.

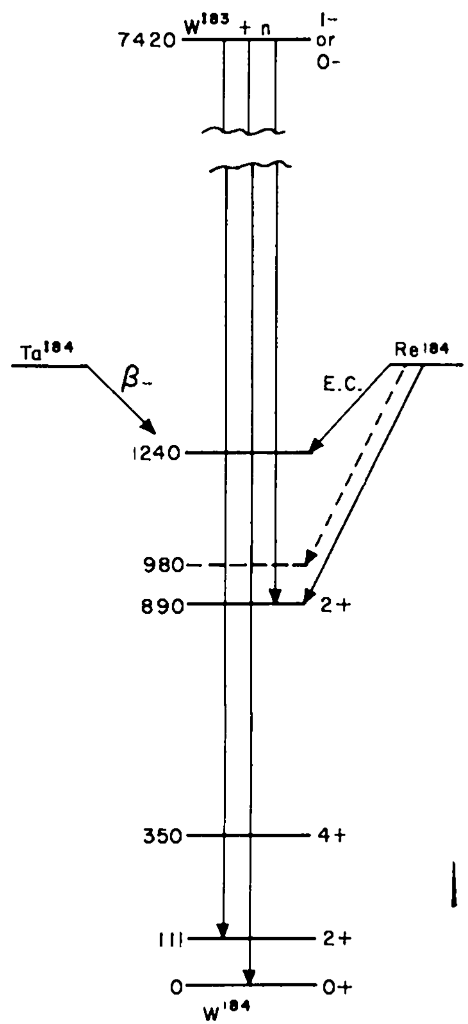

Fig. 4 - The energy levels in the compound nucleus W^{184} formed by capture of slow neutrons in W^{183}. As the initial states have spins of J = 1 or J = 0 only, the spins can be identified because only the J = 1 level can emit a 7.4 Mev gamma ray to the ground state.

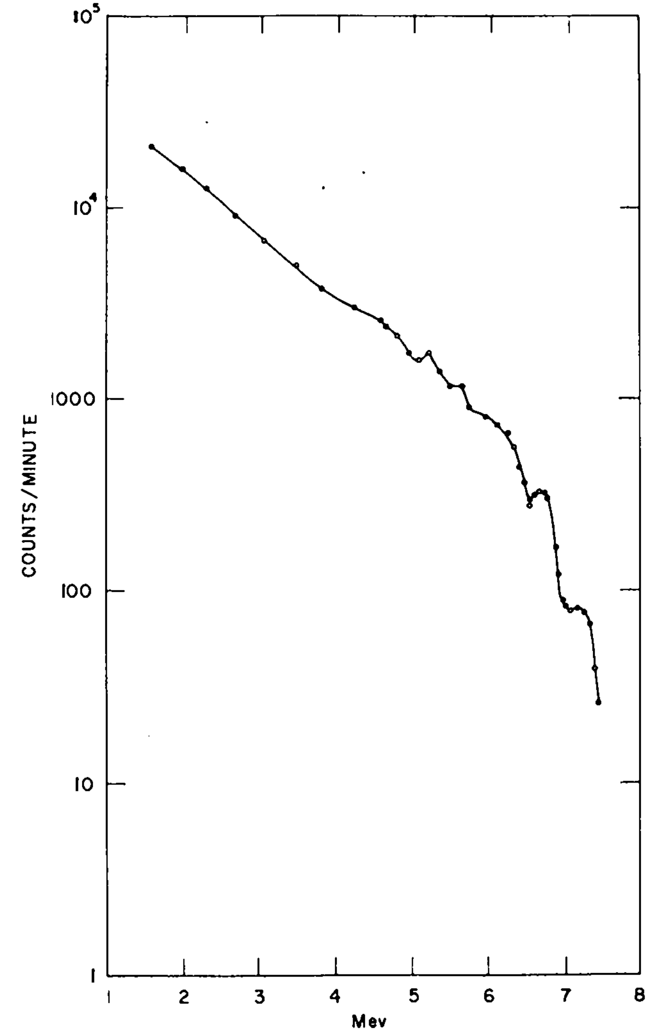

Fig. 5 - The ground state gamma ray of Fig. 4 as identified by pulse height in a crystal detector. The intensity of the ground state transition is measured by the slight peak just above 7 Mev.

An example of results obtained at Brookhaven is shown in Figure 6 - fortunately we gain a factor of about 80 in intensity by moving to Chalk River. We have here an analysis in terms of time-of-flight (or neutron energy) and gamma energy as well. The time-of-flight plot for a low bias setting on the crystal shows low energy gammas and the high bias setting responds only to the ground state gamma in W^{184}. Some of these levels appearing at low bias such as W^{183} and W^{187} (compound nuclei), but as these isotopes have low binding energies, the levels disappear at high bias. The levels in W^{184} that appear at high bias, such as those at about 8 and 27 ev, are thereby shown to be J=1 levels. At about 48 ev there are two levels we can't resolve, and together they appear at high bias, but weakly. We have since found out by running at high resolution that one of these levels is J=0 and the other is J=1, thus explaining the results of Figure 6. However, I am not here to report physics, but to emphasize the need for increased neutron intensity. The ordinate in Figure 6 is counts per channel in 40 hours and rates run about 40. In other words, we get about one count an hour, with a background of a count every 2 hours!

I think it is quite obvious from those counting rates that if someone would guarantee that we would have about two of these explosions a year available as neutron sources, we would gladly pack up our equipment, move out, and start planning measurements.

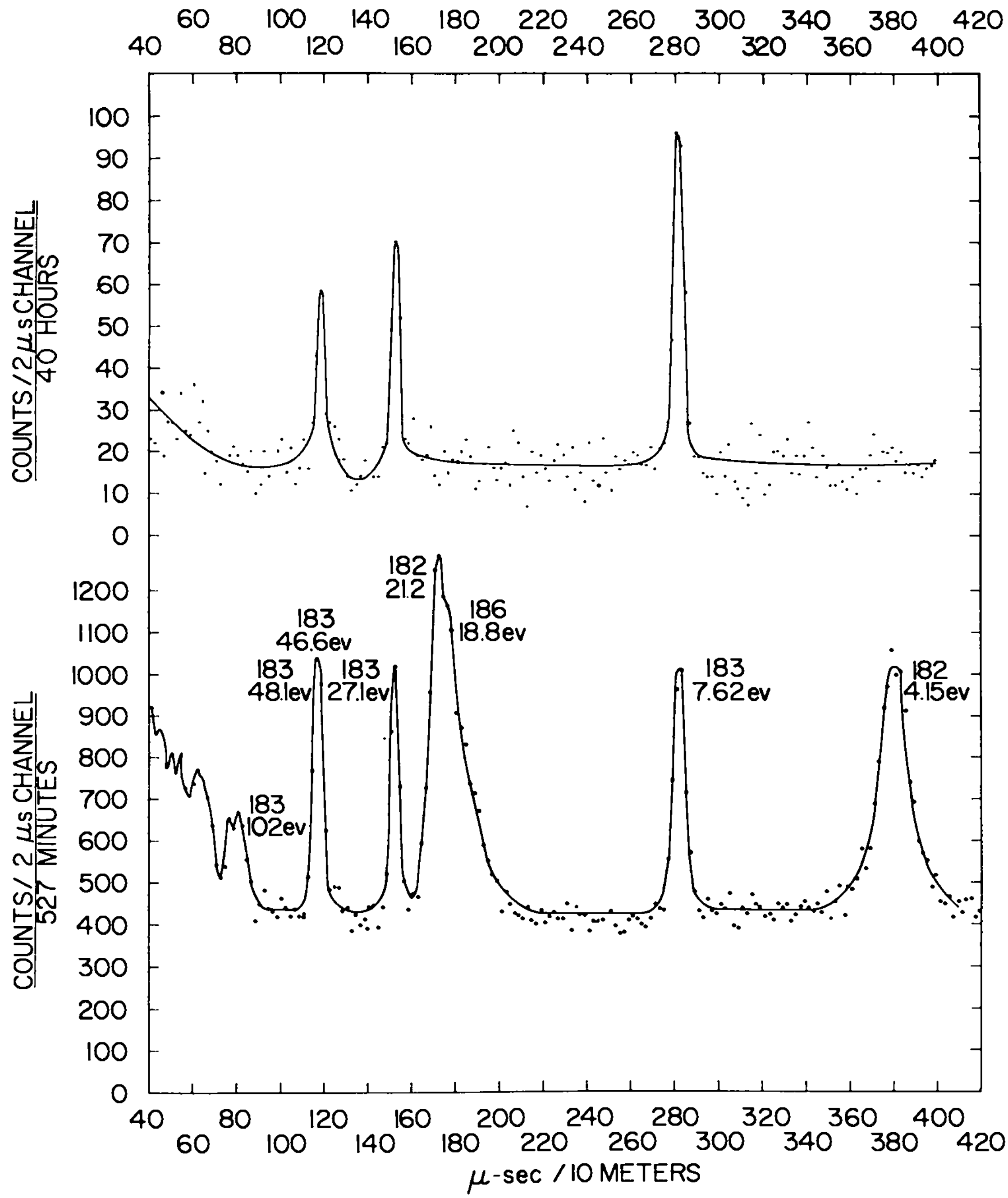

Fig. 6 - The intensity of low energy (lower curve) and ground state (upper curve) gamma rays of various resonances in tungsten. The appearance of a level at high bias shows that the level is W^{184} and has spin J = 1.

2. PROPERTIES OF VERY HEAVY NUCLEI PRODUCIBLE BY MULTIPLE NEUTRON CAPTURE IN PLUTONIUM

J. D. Knight

Los Alamos Scientific Laboratory

As part of the preparation for the heavy element production experiments of the HARDTACK Butternut test, the Los Alamos Radiochemistry Group made a study of the probable properties of the undiscovered nuclides which might be produced by multiple neutron capture in plutonium. The study involved an examination of the systematics of decay energies, neutron binding energies, alpha and spontaneous fission half-lives, etc., of the known nuclei in the transuranium region and the extrapolation of these properties, either directly or with the aid of published semi-empirical mass formulae, into the unknown areas. Its purpose was to provide a set of predictions to serve as a guide for the separation, identification, and exploration steps in the radiochemical analysis of the heavy-element fraction of the shot debris.

The principal properties which we were equipped to measure are: a) alpha, beta, and gamma spectra; b) half-lives and alpha/beta, alpha/spontaneous fission, etc. branching ratios; c) element identification and decay genetics, i.e., what decays to what; d) fission product distributions from spontaneous fissioners; e) absolute amounts of radioactive nuclides; and, where sufficient material is available, of the order of a few hundredths microgram or more, f) capture and fission cross sections.

If it is assumed that the plutonium is exposed to a neutron flux high enough so that multiple capture produces significant amounts of, say, mass 260, the problem we are concerned with is essentially two-fold: first, the fate of the mass 260 beta decay chain between the time of formation, as Pu^{260}, and the time when the radiochemist gets hold of it, presumably as something like Fm^{260} or Mv^{260}; second, the properties of the multiple beta-decay end-product itself.

Our principal concern in dealing with very heavy elements is their survival for a time long enough to permit separation and measurements. The limiting factor, for nuclides on the neutron-rich side of the valley of beta stability, is spontaneous fission. In the region of interest, the spontaneous fission half-life of even-even nuclei appears to decrease exponentially with increasing mass number, approximately independently of atomic number; at Fm^{256}, the highest mass for which a measurement is known to exist, the half-life is down to 3 hours. The same trend seems to hold for odd-mass nuclei, although only a few have been measured; the half-lives, for corresponding masses, are of the order of 10^4- or 10^5-fold longer than for even-evens. What this means, in terms of radiochemical measurement, is that we cannot expect to find even-mass nuclei much higher than Fm^{256}, nor odd-mass nuclei much higher than mass 259 or 261; the latter might beta decay up to element 102, but beyond element 102 we appear to be stymied. It is of interest that the IVY Mike explosion, from which measureable amounts of mass 255 were isolated, was approaching the estimated mass limit.

The sequence of beta decays between the initial capture product and the product finally isolated may be accompanied by limiting phenomena of a second kind: delayed neutron emission and delayed fission. This effect should arise when the total beta decay energy of a member of the chain exceeds the neutron binding energy or the fission threshold of its daughter. Under these conditions, that fraction of the beta transitions which leads to daughter levels above the fission or neutron boil-off threshold may be "wasted"; in the fission case the loss is complete, whereas in the delayed neutron case, the nuclei lost to the mass A chain reappear as members of the mass A-1 chain. Estimates of this degenerative effect are somewhat hard to make. They depend not only on the beta decay energies, neutron binding energies, and fission threshold energies of nuclides far from the region of stability, but also on the densities of levels to which the beta transitions proceed and on the fission, neutron, and gamma widths of these levels. Nevertheless, some bold estimates have been made to arrive at an idea as to whether the effect is likely to be catastrophic or merely parasitic. With the most pessimistic assumption, namely that the beta terminal levels have a density which goes as $\exp(2\sqrt{aE_{ex}})$, and that all beta transitions terminating above the fission threshold of the daughter result in fission, it was calculated that the loss is of the order of 30% for the Pu^{259} chain, 60% for the Pu^{260} chain, and 80% for the Pu^{261} chain. Recent conversations with B. Mottelson during his visit here indicate that the level density distribution used is far too pessimistic; the density of the levels of interest may vary only slowly with energy. Thus, the losses by delayed fission and neutron emission may be inconsequential.

Even within the limits which appear to be imposed by spontaneous fission and by losses along the beta-decay chains, there remain still a considerable number of new nuclides which should be sufficiently long-lived and abundant to permit discovery and detailed examination. On the other hand, it should be emphasized that the present postulated "limits" are based on trends over limited regions. It may well be that what we see from the data presently available is not an absolute limit but the edge of a limited region of instability, beyond which the nuclei tend toward increasing stability again. Indeed, one of the intriguing incentives for exploration of the very heavy nuclides is the possibility that by efficient multiple capture devices we can reach such a region.

B. METEOROLOGY AND UPPER ATMOSPHERE PHENOMENA

1. NUCLEAR EXPLOSIONS AND METEOROLOGY

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U. S. Weather Bureau
Washington 25, D. C.

One of the shortcomings in meteorological research is the lack of laboratory experimentation. Most indoor experiments fail to adequately reproduce the atmosphere. Field tests in which the atmosphere is disturbed require energy sources in excess of those normally available. It is only with the advent of nuclear energy that sources are now capable of altering the atmosphere. In many cases, the sudden release of heat is not necessarily the most desirable and some day it may be hoped that controlled heat sources of equal magnitudes to nuclear explosives may become available.

There are two prime means of using nuclear explosives for research in meteorology. In this article, only low level of "conventional" meteorology is under consideration; effects from explosions at high altitudes (above 100,000 feet) are not considered. The first uses the radioactivity from the nuclear reaction as a tracer. The second employs the heat or the ionization for weather studies. There is already a body of information of the effects from these phenomena which will be described by way of introduction.

Perhaps the most dramatic scientific findings of the past nuclear tests deal with the tracking of air masses. We have been able to verify air trajectories computed from conventional winds, study lateral mixing as the clouds from Nevada atomic tests move across the United States and watch the debris from the U. S. Pacific tests mix throughout the world. But the most important new discovery pertains to long lived fission products injected into the stratosphere. Knowledge of the circulations and mixing rates in the stratosphere as well as the rate of exchange of air between the stratosphere and troposphere has been very poor in the past.

The deposition distribution of stratospheric strontium-90 as of the spring of 1958 is peaked in the north temperate zone, shows a minimum in the equatorial region and a secondary maximum in the south temperate zone. It is readily demonstrated that the distribution does not follow the meridional rainfall pattern. The source of the stratospheric strontium-90 is from U. S. tests at 11°N and the U. S. S. R. tests north of about 50°N. As of the spring of 1958, the production of strontium-90 favored the U. S. source by about a 2 to 1 ratio and it is felt that this is the same ratio as appears in the deposited fallout. However, the amount of strontium-90 in the northern hemisphere is more than twice that in the southern hemisphere. This suggests that the Russian debris has remained entirely in the northern hemisphere and that even the debris injected in equatorial latitudes is preferentially deposited in the same hemisphere. One also finds a seasonal variation in the rate of deposition of stratospheric strontium-90 with peak in the spring and minimum in autumn. These facts have suggested the existence of a meridional circulation in the stratosphere first proposed by Brewer and Dobson in England several years ago. Recent data on carbon-14 in the stratosphere obtained before the U. S. S. R. could have added any carbon-14 to the stratosphere likewise supports this view. It was found that higher concentrations were present at locations north of the source latitude indicating the likelihood of a bulk transport process rather than a mixing transport.

In recommending radioactive tracers, one must keep in mind that the meteorologist already has some markers of air masses. For example, very fine particles of zinc cadmium sulfide, a fluorescent dye pigment, has been used extensively in short range experiments. Floating balloons have been tracked for many days. Each of these has defects, mainly because they are not gases and can separate from the air mass to which they are initially attached. Thus, an ideal tracer for meteorological purposes should possess the following properties:

1. It should be gaseous. If particulate, the particles should be submicron in size.
2. It should be easily collected and measured. A low background would be advantageous.
3. It should be inexpensive since the need is for large-scale experiments for which very large quantities would be required.
4. It should be non-hazardous.

The prime objection to the use of tracers added by past nuclear tests is the fact that the injection point and time was not necessarily best from a meteorological viewpoint. Further, injections were so numerous as to frequently cause confusion about the source of a particular collection. The kinds of special cases which the weatherman might like to study are: follow specific air currents in the stratosphere, particularly in the polar winter; determine the point of exit of stratospheric air; determine whether transport in the stratosphere is, in fact, the result of circulation rather than turbulent mixing; or find the nature of cross "wind" transport in the vicinity of the jet stream.

The heat of a nuclear explosion has already been shown to produce interesting pictures of the rise of hot bubbles of air. The development of smoke rings which was found by Scorer in England in laboratory work and formulated in a mathematical theory appears to be confirmed in the atmosphere as well by rising nuclear clouds. Further studies of the convection of heated air masses may reveal the law governing the rise of the bubbles of air, can possibly be used to produce artificial water clouds, and may indicate the reaction of the atmosphere to the creation of disturbing convection cells in the presence of natural convection.

It has been clearly established that the ionization from the debris of past nuclear tests has altered the conductivity of the air at great distances and over large volumes of air. It is thus possible to change the atmospheric electrical properties at will with nuclear explosives. Conceivably, the detonation of nuclear explosives in thunderstorms might reveal the effect of increased conductivity on the charge separation process. Since the convection of the radioactive cloud will also have a profound effect, one will have to detonate devices with the same convection but with different amounts of ionizing ability in order to distinguish between the different effects.

There have been several suggestions by which the explosion of nuclear devices might alter the air flow. It is obvious that a short-lived disturbance is possible but it has not yet been established that the effect is long-lived. For example, Reed of the Sandia Corporation suggests the use of exploding nuclear devices to steer hurricanes to courses which will be less damaging.

It appears that nuclear explosives can be profitably used for meteorological research. However, the severe requirements for the reduction of contamination eliminates certain possibilities such as some tracer experiments and alteration of conductivity. Further, research funds for meteorological research are limited and if the nuclear devices are charged to such research at the expense of other undertakings, the meteorological profession may view the possibilities of explosives as less promising.

C. EXPERIMENTS IN SPACE

1. SOME EXPERIMENTS WITH EXPLOSIONS IN SPACE

Thomas Gold
Harvard College Observatory
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I should like to discuss some aspects of explosions in space and the vicinity of the earth, not to make an exhaustive survey of what can be done, but limited to some particular points that occurred to me that are interesting. In the space far away from the earth, but still in our part of the solar system, we have a fair amount of knowledge of gas densities and a very rough order of magnitude idea of the magnetic field strength. It ranges in the order of 10^{-4} - 10^{-5} gauss. However, we will want to know these numbers somewhat more accurately before we can make a good guess as to the development of a bomb shock wave. The movement of the bomb debris, far out from the explosion point when it gets to be affected by the plasma in space and by the magnetic field, will be interpretable really well only after having rather better measurements of the static condition of the plasma that is normally there. So we have to think in terms of making these measurements first if we want to be able to interpret the bomb results fairly well. But, assuming that we have a somewhat better knowledge of these quantities, there are still many effects that would be connected with a shock wave going through the plasma that are not theoretically understood. We have some hints of phenomena of this nature; for instance when the sun makes them, and we would very much like to see them under controlled conditions. For example, the bomb debris will not have gone awfully far, only a metter of a few times 10^8 centimeters when it will have encountered a mass of its own order in gas. The question then is in what manner does it pick up this gas - does it pick it all up and what kind of a shock front gets established under these circurmstances? These are circumstances where the mean free path for collision is not likely to be an important quantity compared with plasma interaction and compared with effects of the magnetic field. We think that in the solar system case, where shock waves move at velocities of the order of 1000 kilometers a second, that quite narrow shock fronts are established and we would naturally like to see this produced under controlled conditions when we know exactly what the input is. That is to say, one would look for such a thing as the distance in which the velocity of the expansion wave is cut by a certain factor and one would look for the thickness of the front within which the shock wave occurs.

But in the solar case there are more interesting phenomena than just the structure of the shock wave. In the vicinity of the sun it appears that whenever a shock wave and material flux at these velocities appears, that this is accompanied by radio noise. From the way in which the frequencies change with time it seems pretty clear that the radio noise production is principally concentrated in the vicinity of the undisturbed plasma frequency in front of the shock. That is the quantity that defines the basic frequency that is emitted. It is a puzzle, and a very great one, why frequencies higher than this do not come out of the solar outbursts with nearly the same intensities as this plasma frequency; frequencies lower than this, of course, could not come out because of the refractive index change of the solar atmosphere. What, in fact, occurs in the solar explosion is that frequently double this frequency occurs, but not a general filling in of other frequencies. We would, therefore, very much like to observe explosion waves possessing nothing other than what we know to be there. In the solar case we don't know what might go on other than the shock wave; here we know the input exactly. We would like to observe the debris expand and see whether it also has this characteristic radio noise production as it plows its way through space. The kind of plasma frequencies involved would be a quarter of a megacycle or thereabouts, and we would naturally need to have a receiver for this frequency out in space because again we cannot get this frequency through our own ionosphere. But that would seem to me a very interesting observation which, if it gave the analogous result of the solar case, would really explain a very important point of solar physics. It would explain to us that we need look for nothing else. We must understand why that happens, but we need look for nothing else in the solar case.

Now a different subject connected with radio and bombs is the question whether bombs can be made to produce directly as a consequence of the explosion, not as a consequence of the plasma interaction which is weak, but directly as a consequence of the intense explosion, a small proportion of its energy at radio frequencies. I've from time to time thought about how small a proportion it would need to be and still beat all our terrestrial radio transmitters. It would naturally be in the form of a short pulse and, therefore, it would be a suitable thing for radar observations in the solar system to give one such knowledge as detailed radar observations can do, namely the reflectivity of the planets as a function of their angular position, a great variety of surface reflection effects than can be investigated without, of course, having an angular resolution to resolve the planet, but instead resolve the planet by time resolution which is perfectly reasonable in the radar case. If one had radar observations from far away onto the earth, for example, one would be able to conclude that the earth is half smooth and half rough, roughly speaking. Taking it to extremes one could even determine the order of magnitude of the height of the vegetation, so an awful lot could be got about the planets that we have not got in other ways. It may be that we can get this by terrestrial transmitters that are beefed up, with gigantic antennae and so on, but if it could be got with bombs, this ought certainly to be taken very seriously. The terrestrial transmitters would be able to reach something of the general order of 10^{10} watts, or the equivalent of 10^{10} isotropic watts in a microsecond. I say isotropic because maybe it is rather hard to make a large antenna around the bomb although even that I think is not quite impossible. So if bombs could produce more than 10^{10} isotropic watts, they would be in business for this thing.

Then I meant to make a point while I'm out in space about the question of travel times of particles and, in the case of solar disturbances, of disturbances on the earth including making of aurorae. It was mentioned earlier that the travel times don't seem to fit; the particles of higher energy seem to arrive with delays from the sun that correspond to the lower energy particles that are also present. The observational evidence is very clear cut on this and I think that the interpretation is quite simply that the fast particles are only able to reach us when slow speed gas has come to us from the sun and distorted the magnetic field in space in such a way as to connect us to the sun. One will be able to observe a similar class of phenomena also in the case of the bomb debris though how interesting it is I haven't really thought about yet. There will be, in general, in a tenuous gas in space where collisions are unimportant and where the magnetic fields dominate, the situation of particles which travel to a certain place only when the fields have been distorted so as to allow them to reach that place. We will observe a class of phenomena in the case of bombs also of that nature, that the distortion of the fields caused by the bomb will allow access to certain regions of space for faster particles at an anomalously late time.

Now nearer the earth I can think of a variety of effects in addition to those that were mentioned this morning. Several of them I intended to mention also. One is the very interesting range of phenomena connected with "whistlers", which is a low frequency, 10 KC or thereabouts, observation of a radio mode that can propagate in an ionized gas only closely along the lines of force. It gives us a lot of information about the gas in the vicinity of the earth out to distances over few earths radii. Maybe I should draw that. Now what is seen in the whistlers is that a signal has evidently traveled along and been reflected off the ionosphere here and come back. On some occasions one observes a single pulse having traveled over 20 successive paths backward and forwards. Now it is clear that the degree of guiding that is needed to keep the energy undispersed over 20 such paths is much better than the guiding given by the propagation conditions in the field. So in other words one is observing much more than one has a right to expect. The guiding would have made a certain attenuation between one pulse and the next and one was observing much less attenuation between one pulse and the next. The interpretation is that there exist, naturally, bundles of lines of force which are more densely populated with electrons than neighboring ones and therefore they act as a wave guide and this can be calculated to be a sensitive phenomenon, that a 10% increase in the electron density along such a tube will explain the guiding that is in fact observed. In other words, it would suggest that the earth, if you looked at it from outside, had some sort of a plume structure which is, of course, what the sun does have in the atmosphere. Now a plume structure of this nature is very easily explained as the magnetohydrostatic effect of a patchiness in temperature on the surface at some level in the atmosphere. Each hot spot will have a plume sticking out above it. That, no doubt, is the right explanation in the case of the plumes on the sun and I think it is very likely the one here. Now it is of interest that a bomb has got plenty of energy to make hot spots of this nature and to generate plumes of this nature. Actually, the total amount of material that is needed to make a collimated whistler path is on the order of a kilogram. There is little doubt that the heat that could be dumped into a bundle of lines of force by a bomb is amply adequate to construct a whistler path. Now we know that natural whistler paths like that live for a matter of hours. I don't know whether this represents a fortuitously good circumstance or whether this would generally be true, but at any rate it would be very interesting to look for whistler paths like this following a bomb explosion at a high altitude.

Now somewhat related to this is the question of the stability of the gas along certain lines of force if you put a lot of heat into it. If a tube here is heated very greatly, so that the gas sitting on these lines of force expands and pulls the lines of force apart a little bit, which is what happens, then there will be an instability resulting which is of the nature known in the trade as interchange. In the first place that line will want to bulge up a little bit. It is slightly buoyant in the magnetic pressure of the remainder of the field. When it bulges up a little bit, it will exert a little bit of force here, for example. Now the existence of an insulating atmosphere over the entire earth means that the feet, in a very crude way of speaking but which happens to be correct, that the feet of these lines of force are not tied on. There is nothing to fix which foot connects with a particular line down at the bottom. And, therefore, the gas, and whether you call it the field moving as well is your own business, but this gas that sits on that line of force can interchange with successively higher lines of force and can eventually become the gas that sits on a very polar line of force if you had an awful lot of heat dumped into this. This interchange motion implies no magnetic work being done. It implies only that the gas moves in this way. If you like to look at it in another way, it is electric fields that are permitted by the existence of the insulating atmosphere and it is the electric field which equals a certain V cross H which allows a certain V across the earth's magnetic field. The resistance to this motion is chiefly derived from two causes: (1) That, normally, the gas in these lines of force may be in a stable configuration - you may have to beat a stable gradient - it is a little like having an atmosphere possessing a stable gradient and if you want to turn it over, you've got to do a certain amount of work. The stable gradient for this can be calculated and it is a straightforward thing. You have to beat that, and we think that in general the gradient is stable. And secondly, to move such a bundle outwards, you have to accelerate all its mass, of course, and all its mass is resident in that little bit down here, namely the ionosphere. The mass of all the rest above is quite negligible. So if you were to dump hot stuff into such a tube, you will accelerate ionospheric material in this direction and this will give you the sort of time constant with which the motion can occur and it would not seem unreasonable to make velocities of the order of 50 meters a second or something like that. In other words such a tube can be expected to expand in a few hours from an equatorial to a polar one.

Well I think it very likely that such a phenomenon would have occurred on the occasion of the Argus shots but I think that none of the observations that we have would have shown it, or none of the ones that I'm aware o:

because this interchange motion must not be confused with the question of the stability of the shell of fast electrons. If, in such a tube, fast electrons are produced as they were on the Argus occasion, then they will drift up out of this tube in longitude in a matter of a few minutes or even shorter. Because of their rather rapid longitudinal drift, they go once around the earth in a matter of a half an hour. And therefore, they are out of this tube although they would move with the tube if they could stay in it because that is the coordinate system in which there is no electric field; they will not in fact move with the tube because they go out of it, and by the time they come around again, this tube is only a very small fraction of the way around. In any case, continuity of field implies that other regions are moving down to make up for this one going up and when you work it out, it just comes to zero. So the stability of the Argus shell is no indication of this phenomenon one way or the other. One ought to think of methods of making this visible or of observing ionospheric disturbances, as one can observe ionospheric disturbances, in motion and I hope that the next time that we have such an occasion, one will be able to watch the unfurling, as it were, of the magnetic field from the region of the bomb.

Now the last point that I would like to mention is the question whether anything interesting can be done with a lot of light that a bomb might emit when we unfortunately have to compete with the sun. When one works it out, the amount of sunshine that hits the earth is about a kilogram per second and we can make an amount of light that is perhaps a few grams if one finds a way of converting a significant fraction of the X-rays into light from a bomb. One is working with numbers which mean that for a matter of perhaps a microsecond one can make it brighter than sunlight out to distances beyond the moon but not to distances like Mars. I think the space around the earth is pretty transparent and radar light methods in that space, for that reason, have to use an extremely powerful light source.

Now we know of a phenomenon that is not yet adequately interpreted, the so-called gegenschein, the phenomenon in which light seems to be scattered in the sky from a direction opposite to that of the sun. This has, from time to time, been interpreted as preferential back scattering but the degree of preferentiality is unreasonably great and so people have thought about other explanations. The Russians have proposed a theory which, I think, seems as plausible as any, that the earth possesses a comet-like tail that faces away from the sun and when we look along that tail we see the most backscattered light. That is an observation which could be done perhaps much better with the light from a bomb used as a radar set because now we're not competing with the sunshine. We could see that tail in the shadow of the earth where we can have a much higher sensitivity for our receivers and we could see that shadow not only as a diffused cloud, which is all that you can see in the sunlight illuminated case, but you could see also in a time resolved way because of the shortness of the burst of light. You could therefore discover the scattering as a function of distance away from the earth and, if the

Russian ideas are right, then this would be an observation that would have to work out to a few earth's radii. That seems to me entirely all right so far as the numbers are concerned if we can make a few percent of a megaton bomb into light. In fact, if at the time of the Johnston Island shot somebody had gone elsewhere on the night hemisphere where he did not have any direct light from the shot and where he was well away from the direct and atmospherically scattered light and could look into the night sky out there, if he had gone there with a photo cell and observed the time resolved trace, it may well be that he could have had that information even then.

2. PROPOSAL FOR AN EXPERIMENT TO MEASURE THE LIFE-TIME OF THE NEUTRON

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I. MOTIVATION

The life-time of the free neutron is at present one of the most uncertain of the basic constants of physics. There are two reasons for trying to measure it more accurately. First, this life-time is immediately required in order to test various ideas about a universal theory of weak interactions between elementary particles. Second, it always turns out that in the long run it becomes important to measure the constants of nature with as high accuracy as possible. Crucial discoveries often have emerged from the study of very small anomalies revealed by such accurate measurements.

We propose to measure the life-time by a direct attenuation method. A bomb is exploded in space, producing a large number of neutrons. Two or more detectors observe the neutrons arriving at different distances from the bomb. Each detector records a time-of-flight spectrum extending over a time comparable with the decay life-time. Comparison of the neutron fluxes recorded at different times and at different distances from the bomb will give a direct measurement of the rate of decay.

II. COMPARISON WITH LABORATORY MEASUREMENTS

Before considering an experiment with a bomb, one naturally asks whether an equally good measurement might not be made in the laboratory.

It seems that an attenuation measurement under laboratory conditions is not possible. This is because in the laboratory the attenuation of neutrons by decay is always a minute fraction of the attenuation by nuclear capture or leakage. Thus all laboratory measurements of the life-time are based on the comparison of an observed number of decay events with an absolute monitoring of the number of neutrons present. Accurate monitoring of the neutron intensity is very difficult, because one usually does not know precisely enough the limits of the volume at which one is looking, and also because one does not know accurately the neutron velocity spectrum.

The great advantage of the bomb experiment is that the uncertainties in the source volume and in the velocity spectrum do not affect the accuracy of the measurement.

Let us look at the actual experiments which have been done. They are all done by looking at decay particles from a beam of thermal neutrons emerging from a reactor.

The following values have been found for the half-life:

I. M. Robson, Phys. Rev. 83, 349 (1951), 12.8 ± 2.5 min.

N. D'Angelo, Phys. Rev. 114, 285 (1959), 12.7 ± 2.0 min.

Sosnovsky, Spivak, Prokoviev, Kutikov and Dobronin, Nuclear Physics, 10, 395 (1959), 11.7 ± 0.3 min.

The claimed accuracy of all experiments is about 15%, except for the Russian experiment which is claimed to be good to 3%.

I am exceedingly sceptical about the claimed accuracy of the Russian experiment, not because it is Russian, but because of the objective difficulty of absolute monitoring of neutrons. To see how hard this is, let us take a look at the absolute measurements of the fission cross section of U^{235}. The present state of this problem is summarized by:

Safford and Melkonian, Phys. Rev. 113, 1285 (1959).

The measurements of Safford and Melkonian give a value 580 ± 7 barns at 0.0253 ev. Other experimenters quoted by Safford and Melkonian give the following values:

556 ± 6 ,
638 ± 20,
606 ± 6 ,
568 ± 7 .

You see that the various experiments, though they claim 1% accuracy, are actually scattered over a range of ± 5%.

Now the absolute fission cross section of U^{235} is certainly not an unimportant quantity in the reactor business. In fact, the effort that has been put into measuring it is very great. If it is not known to better than 5% accuracy, the explanation can only be that the experiment is very hard indeed.

I think it is clear that the neutron life-time is even harder to measure with a reactor than the U^{235} cross section. The counting-rates are much slower, the geometrical factors are more uncertain, the radiochemical methods of detection are excluded. Therefore I do not believe that the neutron life-time is known to higher accuracy than the U^{235} fission cross section.

I would judge that the neutron life-time is actually known at present to about 10% accuracy. I would guess that measurements in the laboratory will reduce the error within the next few years to 3% and ultimately to 1%. But it is difficult to imagine a laboratory measurement, based on absolute neutron monitoring, which would be accurate to better than 1%.

So to be really interesting a bomb experiment needs to be accurate to about one part in a thousand.

III. ANALYSIS OF THE BOMB EXPERIMENT

For a preliminary analysis we suppose that there are just two neutron detectors, each having diameter D, at distances R_1 and R_2 from the explosion. The distances R_1 and R_2 can be accurately measured by a simple radar system attached to the detectors. We suppose that the detectors are accurately in line with the bomb, so that it is unimportant whether or not the angular distribution of the neutrons emitted by the bomb is isotropic. Presumably the two detectors could be shot out from the vehicle containing the bomb, a short time before the explosion.

Each detector observes a certain flux of neutrons spread out in time after the explosion. From these fluxes the actual velocity-spectrum of the neutrons can be deduced. The velocity-spectrum need not be known in advance. The small correction introduced by delayed neutrons arriving with the "wrong" velocity can be separately monitored and subtracted.

For a rough analysis it is sufficient to assume that the bulk of the neutrons arrive with a certain characteristic velocity V, but spread out in time over a duration R/V, where R is the distance from the explosion to the point of observation.

Let

N = number of neutrons emitted,

τ = neutron half-life,

λ = decay constant $= (\log 2)/\tau$.

The number of counts observed at the detector R_1 will be

$$G_1 = \frac{N D^2}{16 R_1^2} e^{-\lambda R_1/V} ,$$

and similarly for the detector R_2. Thus the measured value of the decay constant λ is

$$\lambda = \frac{V}{R_2 - R_1} \log \left(\frac{R_1^2 G_1}{R_2^2 G_2} \right) .$$

The statistical fluctuation in λ will be

$$\Delta \lambda = \frac{V}{R_2 - R_1} \left[\frac{1}{G_1} + \frac{1}{G_2} \right]^{1/2} .$$

To make this small it is best to take

$$0 \ll R_1 \ll R_2 \ll V\tau .$$

Then

$$\Delta \lambda = \frac{V G_2^{-1/2}}{R_2} = \frac{4 V}{D} N^{-1/2} .$$

The fractional error in the life-time measurement is thus of the order

$$\frac{\Delta \tau}{\tau} = \frac{\Delta \lambda}{\lambda} = \frac{4V}{D\lambda} N^{-1/2} .$$

In practice we might have

$$D = \text{detector diameter} = 100 \text{ cm},$$

$$\lambda = 10^{-3} \text{ sec}^{-1} .$$

Then

$$\frac{\Delta \tau}{\tau} = 40 \text{ V } N^{-1/2}.$$

Consider a neutron source consisting of a bomb with energy-yield Y and mass M. If the neutrons are thermalized within the bomb material to the velocity V, then approximately

$$\frac{V^2}{N} = \frac{2Y}{MN} = \frac{6 \times 10^{-4}}{M} .$$

So the ratio $VN^{-1/2}$ is roughly independent of the bomb-yield. For a mass

$$M = 1 \text{ ton} = 10^6 \text{ gram}.$$

We have

$$VN^{-1/2} = 2.5 \times 10^{-5} ,$$

and

$$\frac{\Delta \tau}{\tau} = 10^{-3}.$$

A simple bomb source of mass 1 ton would thus be adequate for a measurement with statistical accuracy of one part in a thousand.

A much better neutron source would be a mass of water placed close to a bomb but protected by a lead shield from the direct effects of blast and radiation. Suppose that neutrons carrying an average energy of 10 kilovolts emerge from the bomb, penetrate the lead shield, and become thermalized in the water. Then

$$\frac{V^2}{N} = \frac{3 \times 10^{-8}}{M} ,$$

where M is the mass of the water. Taking now

$$M = 10^5 \text{ gram},$$

we find

$$VN^{-1/2} = 6 \times 10^{-7}, \quad \frac{\Delta \tau}{\tau} = 3 \times 10^{-5} .$$

Statistical errors of less than one part in ten thousand are in principle possible.

IV. PRACTICAL DIFFICULTIES

Three practical difficulties of the bomb experiment come immediately to mind and will now be discussed very briefly.

A. Cosmic-Ray Background

Cosmic-ray background in a counter of diameter 100 cm is roughly

$$3 \times 10^4 \text{ particles/sec.}$$

See Van Allen and Frank, Nature, 183, 430 (1959).

The neutron counting rates may vary over wide limits. A typical set of numbers might be the following

$N = 10^{23}$ (equivalent to about a kiloton of fission energy)

$V = 1.5 \times 10^6$ (neutrons moderated down to 1 volt)

$V\tau$ = 10,000 Km (neutron attenuation distance)

R_1 = 300 Km

R_2 = 3,000 Km

The counting rates are then

3×10^9 neutrons/sec for 20 seconds,

3×10^6 neutrons/sec for 200 seconds, in the two detectors.

The cosmic-ray background will therefore not be a serious problem, especially because it is rather easy to discriminate between an incoming neutron of 1 ev energy and an average cosmic-ray primary.

The background of low-energy neutrons in space is certainly negligible, as soon as one is far enough away from the earth.

B. Gravitational Effects

The neutron trajectories will deviate from straight lines because of gravitational effects. However, the neutrons and the detectors are all falling freely in the same gravitational field, and this makes the effects of the curved trajectories vanish to a first approximation.

It is necessary, in order to make the gravitational effects negligible, to take the whole experiment well away from the earth, say to about the distance of the Moon.

For accurate results we would in any case have to go to the Moon's distance, in order to avoid confusion with neutrons back-scattered from the Earth's atmosphere.

C. Calibration

The major practical difficulty of the experiment seems to be the calibration of the detectors. If we want a result reliable to one part in a thousand, the performance of the neutron detectors must be identical to at least the same accuracy. The detectors can presumably not be examined and checked after the experiment.

In summary, we may say that the bomb measurement of neutron lifetime makes demands on rocketry, telemetering, and detection technology which cannot be fulfilled for several years. However, the experiment has sufficient long-term importance to make it worthwhile to think about seriously.

3. DISPERSIVE PROPERTIES OF SPACE

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The experiment proposed in this paper is designed to measure the relative velocities of photons of various frequencies in propagation through interplanetary space. It involves the use of a nuclear explosion as a pulse source of a broad range of frequencies. It requires the detonation to occur at a large distance from earth (of the order of 10^6 km). And finally it requires a set of radiation detectors to be located above the sensible atmosphere in satellites or probe rockets since much of the emitted radiation cannot penetrate the earth's air mantle. The comparison of the relative times of arrival of the signals of different frequencies provides the experimenter with a measure of the variation of photon velocity and hence of the dispersion of space for electromagnetic radiation.

The experiment is one of high intrinsic precision. With no special effort beyond the assembly and placement of presently available devices and detectors one can expect to be able to detect velocity variations as small as 1 part of 10^8, or 3 meters/second. With special source designs it may be possible to detect 1 part in 10^9, or 30 cm/sec.

The frequency range of the measurable photons extends from 1000 mc (radar) to 10^{21} cps (few Mev gamma rays).

The reason for interest in this experiment is two-fold. The velocity of light is a physical quantity of such fundamental importance to so many physical theories that direct measurement of its properties is desirable - as contrasted to indirect inferences based on reasoning through these theories.

Secondly, the timely possibility of making such a measurement with its improvement in precision over present data by 3 orders of magnitude in the velocity and an extension of the frequency range by approximately six orders of magnitude represents a significant advance in experimental techniques which should be exploited.

The presently available measurements of photon velocity versus frequency does not allow any conclusion to be drawn relative to the possibility of dispersion. The experimental errors associated with the data are too large. It is interesting, though, to speculate on the mechanisms which might produce such phenomena in an amount to have escaped detection. There are the possibilities connected with the electromagnetic theories - non-linearities or frequency dependencies of the field variables; the photon may after all have a (very small) rest mass; it has been suggested that there may be quantum mechanical effects which may become important at very high frequencies - a region of particular interest is the energy range around 1 Mev where the pair production threshold lies.

The experiment has the following structure: A nuclear explosive is placed out in space at a distance of about 3 light-seconds (10^6 km). Prior to its detonation, probe rockets are sent above the atmosphere. These rockets carry the radiation detection instruments and the necessary radio links with the ground laboratory. The bomb is detonated. Three seconds later photons of various frequencies begin arriving at the detectors. An analysis of the rise of each signal and a comparison with the known production of the signal by the bomb will allow a determination to be made of any differences in velocity with a precision which depends on the rise time pulse width and average flight time.

Because of classification problems we are not able to discuss the signal intensities or the details of the source. The experiment is quite easily done, however, with a 300 KT device at the suggested distance (10^6 km). The various signal intensities are adequate and have a rise sufficiently fast for the quoted precision. Instrumentation for detection of the RF, optical photons, and gamma rays should present little difficulty.

A correction which will have to be applied to the data is the effect of the electrons in space. These give rise to a real index of refraction correction at frequencies near the plasma frequency. The best estimate now available is that the electron density between the earth and the moon is 10^3 per cubic centimeter. This slows down 3000 mc radiation by about 1 part in 10^8. For optical frequencies the correction is 1 part in 10^{20} and can be ignored. The electron density over the experiment path can be measured a short time before detonation by transmission back and forth between the earth and the bomb rocket at frequencies near the plasma frequency. This will establish the experimental conditions and, incidentally, give new information about the electron density of space.

The detectors need to be fast response types of fair sensitivity. Standard scintillator-photomultiplier arrays will do for the gamma rays; photomultipliers for the optical photons (with suitable protection) and sensitive radio detectors for the 1000 mc radiation can be made with little difficulty.

Such an experiment is capable of greatly extending the range of electromagnetic radiation velocity measurements and simultaneously greatly reducing the uncertainties of the relative velocities versus frequency. It appears that the technical means to do such an experiment are at hand. The requirements may be relatively simple additions to already planned extra-terrestrial experimentation. The results, whether they showed an energy dependence or independence of photon velocity, would be of great intrinsic interest to science.

D. HYDRODYNAMICS

1. CONTAINMENT OF BOMB EXPLOSIONS

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I. USEFULNESS OF CONTAINMENT

I started to think about contained bomb explosions in connection with the ORION Spaceship Project. In that project, if it is carried to completion, we shall need a series of test explosions with yields varying up to about a kiloton, with very careful instrumentation to observe the explosion effects. It seems that this whole program of tests would be very much facilitated if we could build a box in which kiloton shots could be repeatedly exploded without destroying the box.

I have in mind a facility which could stand on the surface of the earth or be only lightly buried. That is to say, we do not rely on the inertia or hydrostatic pressure of the earth for containment. The facility would have to be equipped with remote handling machinery for getting experiments in and out of the box. After a few shots the inside of the box would be intensely radioactive.

The purposes to which such a facility would lend itself are the following:

A. All scientific experiments in which bombs are used as neutron sources could be performed under much more favorable conditions. For example, the experiments which have been proposed at this meeting by Aamodt and by Teller. The point is that in a normal underground shot the damage to the experimental equipment is done not so much by the direct effects of the bomb as by the subsequent violent movements of the rock. If a bomb is exploded in a box which does not collapse after the explosion, a lot of quite delicate equipment can be put inside the box and protected from mechanical damage by a modest amount of shock-absorbing materia Close-in measurements are thus much easier to make.

B. Radiochemical analysis of the bomb debris can be made quickly and quantitatively.

C. If new elements are produced in extreme neutron fluxes, as proposed by Teller, they can be collected and identified promptly.

D. The seismic signals detectable outside the box would be so small as to demonstrate clearly the necessity of unrestricted ground inspection for policing any unrestricted test-ban agreement.

II. DISCUSSION OF FEASIBILITY

To fix the ideas I consider the box to be a cube of side 30 meters, volume

$$V = 2.7 \times 10^{10} \text{ c.c.}$$

The bomb-yield is 1 kiloton or

$$Y \doteq 4 \times 10^{19} \text{ erg.}$$

The pressure reached by dumping all this energy into gas in the box is roughly

$$P = \frac{1}{3}(Y/V) = 500 \text{ bars.}$$

To handle the situation we need two basic components in the box,

(a) a fast heat sink, and (b) wall shock-absorbers.

A. Heat Sink

Originally I considered for the heat-sink a mass of steel wool, filling the box except for a multitude of branched channels to distribute the energy rapidly. This arrangement has been studied at Livermore under the name of the Lung. It is obviously hard to be sure that it will work, since the geometrical arrangement is so complicated.

After the meetings I thought of a much surer way of designing the heat sink. The same idea occurred independently to Al Latter about the same time.

The heat sink consists of 2000 tons of dust, made of some cheap material with a high vaporization temperature. Al Latter remarked that carbon dust would be very satisfactory. The dust is made into 100,000 balls each having mass 20 Kg, diameter about a foot. These balls are suspended all over the box, the separation between them being about 2 feet. They occupy 3% of the volume of the box. At the center of each ball is a one pound charge of high explosive fused to detonate simultaneously with the main bomb.

The sequence of events is the following. During 0.2 millisecond after the detonations, the shock from the expanding H.E. is traveling out to the surfaces of the balls. During this same time the energy of the bomb, either in the form of radiation or extremely high temperature gas, flows between the balls and fills the volume of the box more or less uniformly. For the next 0.6 millisecond the dust-balls expand until adjoining balls coalesce. By this time the entire box is filled more or less uniformly with dust. Conduction of heat from the gas into the dust grains will be so rapid that within about 1 millisecond the contents of the box reach a temperature equilibrium of about 1000°C, far below the vaporization temperature of carbon. When temperature equilibrium is reached, the pressure in the box arises mainly from the gaseous products of 50 tons of H.E. The pressure will then be about 5 bars.

We see that the effect of the fast heat sink is to bring the pressure in the box down from 500 bars to 5 bars within about 1 millisecond.

B. Shock-Absorbers

We suppose that each of the 6 walls of the box is lined with shock-absorbers. A shock-absorber consists of a heavy plate, weighing 100 gram/cm^2 (for example, a foot of concrete) and free to move outward into a system of gas-bags maintained at 10 bars pressure.

The impulse of 500 bars acting for 1 millisecond will make the plates move outward at a speed of 50 meters/sec. After the first millisecond the internal pressure has dropped to 5 bars while the external pressure is still 10 bars. The plates will then be stopped and brought to rest after 100 milliseconds during which they travel outward 2.5 meters.

The layer of gas-bags must therefore have a thickness of 3 meters all around the box. Outside the gas-bags the outer shell of the box, having a total diameter of 36 meters, need only sustain a pressure of 10 bars.

To demonstrate conclusively that this system of heat sink and shock-absorbers is feasible would, of course, require a much more careful analysis. But it seems to me extremely likely that something along these lines would work.

III. OVER-ALL WEIGHT AND COST

The three main contributions to the weight of the facility would be

Heat sink material (carbon)	2000 tons,
Inner inertial wall (concrete)	6000 tons,
Outer structural wall (steel)	3000 tons.

The weight of the outer structure is that required for a box of volume 4×10^{10} c.c., holding a pressure of 10 bars, and built out of ordinary structural steel.

It is important that the outer shell can be supported from the inside by a framework of steel girders running through the inside of the box. These girders require only a small amount of shock-absorbing to protect them from the initial effects of the bomb. The girders are included in the structural weight of 3000 tons. The outer shell does not then need to be more massive than ordinary boiler-plate.

The cost of the steel structure at $300 per ton would be $1 million. This is certainly only a small part of the total cost of the facility. Probably the most expensive part of the whole thing would be the remote handling equipment for getting experiments in and out. Such equipment might well cost $10 to $50 million.

It seems that a box of this kind could be built to handle any bomb-yield. The volume and the cost would be roughly proportional to the yield. Only the shock-absorbers would increase with the surface-area of the box and therefore would cost proportionately less in larger sizes.

Because remote-handling equipment is so expensive, it is probably uneconomic to design the facility for as small a yield as a kiloton. If we think of a 50-kiloton size the structural costs might be about $100 million and the handling equipment costs might be about the same.

Perhaps a one-kiloton facility would be a good first step, in order to make sure that the thing really works.

E. NEUTRINO EXPERIMENTS

1. BOMBS AND ANTINEUTRINO EXPERIMENTS

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The first neutrino source considered by the Los Alamos group for detection of the free neutrino was a nuclear explosion. When it became apparent that the use of the delayed coincidence technique would reduce backgrounds sufficiently, the bomb experiment was cancelled in favor of experiments using piles. In response to several requests the bomb possibility is here reviewed with substantially the same conclusion: production reactors such as those at Savannah River are better than bombs for free neutrino experiments.

The argument is as follows:

Assume a contained fission bomb of W KT energy release viewed at a distance of R meters from the center of the explosion. Making use of the facts that there are 6.1 $\tilde{\nu}$/fission and that 1 KT is 1.3×10^{23} fissions, the inverse square law* gives a $\tilde{\nu}$ flux F:

$$F = 6.3 \times 10^{18} \frac{W}{R^2} \qquad (1)$$

where F is the total flux of $\tilde{\nu}/cm^2$ at R meters from a bomb of W KT energy release.

The total number of detected events, η, per liter of triethylbenzene

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*To a very good approximation a non-absorbing spherically symmetric source is, for a detector external to it, equivalent to a point source located at the center of the sphere.

(a possible scintillator solvent) is then given by

$$\eta = F\bar{\sigma}_p N\epsilon = 20 \frac{W}{R^2} \qquad (2)$$

where $\bar{\sigma}_p = 1.1 \times 10^{-43}\ \mathrm{cm}^2/\tilde{\nu}$

$N = 5.9 \times 10^{25}$ protons/liter

and $\epsilon = 1/2$ (a possible detection efficiency)

If we were interested in a statistical accuracy of only 5% in total counts (i.e., 400 counts) then we would require a detector volume of 2×10^4 liters (~ 20 tons) for a yield of W = 10 KT, and a distance R = 100 meters. This is the volume of a cube ~ 2.7 meters on edge, a large but achievable size. Backgrounds due to the bomb do not appear to be a serious problem for a detector buried 20 meters or so.

This experiment using a bomb is to be compared with experiments which can be done at a reactor where ~ 100 events/hr would result from only a few percent of the 20,000 liters required for the bomb detector. The major difference arises from the smaller value of R in the reactor experiment. The reactor experiment is clearly much superior and much less expensive even if we consider only the size and cost of the detector itself. There are of course other complications associated with the protection necessary to preserve the equipment at 100 meters so that it can collect the data through the earth shock which arrives in milliseconds and for some minutes thereafter.

THERMONUCLEAR POSSIBILITIES

Suppose we could make Li^8 by the capture in Li^7 of the copious neutrons from a thermonuclear bomb. The result would be high energy antineutrinos (13 Mev end point compared with the mean energy, ~ 3 Mev, of fission antineutrinos) and a greatly enhanced reaction cross section. The factor of ~ 30 gained in the case of the reaction $\tilde{\nu} + p \longrightarrow \beta^+ + n$, because of the approximately quadratic increase of the reaction cross section with energy, is attractive but still does not, in my view, make bombs competitive with reactors. The $\tilde{\nu}$ + D reaction would benefit relatively more from the use of the higher energy antineutrinos because of its greater threshold energy (4.1 Mev instead of 1.8 for the $\tilde{\nu}$ + p reaction) but the reaction cross section $\bar{\sigma}_d$ is still somewhat smaller than $\bar{\sigma}_p$, an argument against use of an explosion even for this case. It is perhaps more interesting to contemplate building a special experimental reactor with a Li^7 blanket, so reducing the power required by perhaps an order of magnitude.*

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*Relatively more is to be gained from a thermonuclear bomb with Li^7 than from a reactor: In a bomb the mean $\tilde{\nu}$ energy rises while the number of $\tilde{\nu}$/KT stays nearly constant because thermonuclear neutrons are more copious (by a factor ~ 6) than are fission neutrons, whereas in a reactor only something like one neutron per fission is available for capture in Li^7.

F. SOLID STATE STUDIES

1. NOTES ON THE USE OF VERY HIGH RATES OF IRRADIATION FOR SOLID STATE PROBLEMS

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INTRODUCTION

In the basic study of radiation effects one of the very important parameters is the rate of irradiation by fast neutrons. The importance of this parameter has been pointed out many times in the past. More recently, radiation effects have been discovered (enhanced diffusion, solid-gas reactions, etc.) where the simple theories indicate that flux dependence is unusually important. This is primarily because these effects are controlled by a rate process rather than by a simple accumulation of crystalline defects. The same is true of the more sophisticated theories and interpretations of the annealing of radiation damage. The rate of production of damage and its relation to temperature are the crucial experimental quantities for further understanding of this important field of solid state physics and the science of materials. Many practical questions are closely related to these problems and the solid state physicist is unable to give any guidance because of a lack of basic data. Extrapolation to high rates of irradiation is, of course, scientifically useless since it is precisely the theoretically predicted flux dependence that one wishes to test. From the practical point of view, extrapolation to high fluxes is notoriously unreliable and potentially dangerous. It should also be mentioned that quite unexpected phenomena may be found at very high rates of irradiation.

For scientific purposes one would like to go to much higher rates of fast neutron irradiation than can be provided in a continuously operated reactor. The point here is that one would like to produce at a given temperature a certain concentration of defects essentially instantaneously and study the subsequent motion, disappearance and aggregation of these defects. For many materials one needs integrated exposures of 10^{17} nvt or higher although highly sensitive materials (some semi-conductors and insulators) could be examined even at 10^{15} nvt. It is essential that the above integrated exposure be achieved in one burst, the burst lasting no longer than about 10^{-4} seconds (100 microseconds). The corresponding internal fluxes are 10^{19} to 10^{21} n cm^{-2} sec^{-1}. It should be pointed out that preliminary experiments are possible with Godiva II for highly sensitive materials since an average internal fast flux of about 10^{18} n cm^{-2} sec^{-1} is achievable with an integrated exposure of about 10^{14} nvt. This is a sufficient exposure to effect, to an easily measurable extent, the electrical properties of semi-conductors, optical properties of insulators, and the ultrasonic response of many substances.

It is of interest at this point to consider some rather specific examples.

METALS

In the case of pure metals, decreasing the irradiation temperature has revealed a steady succession of new annealing stages. Thus, even at the lowest ambient temperature which can be maintained in a reactor, namely about 6^{o}K, there is some annealing taking place. It is important to know if annealing stages occur below this temperature and the use of high rates of irradiation at low temperatures might be the best way to find out. If no recovery or rearrangement occurs during irradiation the amount of radiation damage will be a function of the total dose only and will be independent of the rate at which the dose is applied. If annealing occurs the damage will be a function of the irradiation rate.

Different annealing mechanisms give different functional dependencies on the flux. Consider the simplest example of defect production (vacancies, interstitials) in a metal by fast neutron irradiation. Assume that defects are produced at a constant rate and that they anneal at a rate proportional to their concentration (to fixed sinks). In this case

$$\frac{dC}{dt} = K - \alpha C \qquad (1)$$

where C is the defect concentration and α is an annealing rate which, as a function of temperature, has the form

$$\alpha = \alpha_o e^{-E/RT} \qquad (2)$$

where E is an appropriate activation energy. Integration of (1) gives

$$C = \frac{K}{\alpha}(1 - e^{-\alpha t}) \qquad (3)$$

which shows that C is proportional to K. The steady state concentration (i.e. $dC/dt = 0$), C_s, is given by

$$C_s = \frac{K}{\alpha} \tag{4}$$

Consider another simple example, namely, annealing by direct recombination of randomly distributed vacancies (v) and interstitials (i). In this case

$$\frac{dv}{dt} = K - \nu_i \, vi \tag{5}$$

$$\frac{di}{dt} = K - \nu_i \, vi$$

and v = i. In the steady state

$$v_s = i_s = \left(\frac{K}{\nu_i} \right)^{1/2} \tag{6}$$

Eq. (6) shows that in this case the flux dependence is square root rather than linear.

These examples were discussed simply to illustrate that the flux dependence can give a great deal of information about the annealing mechanism. It is also clear that until the annealing mechanisms have been studied; extrapolation of technological data to high flux situations is very unsatisfactory and perhaps highly misleading.

Under certain conditions (for example 0° to 200°C and a fast flux of about 10^{12} n cm^{-2} sec^{-1} for alpha-brass) the simple linear anneal has been verified by means of enhanced diffusion experiments. So far these experiments are limited to a flux of about 10^{12} n cm^{-2} sec^{-1} fast neutrons. The steady state concentration of defects in these experiments was exceedingly low and it is not surprising that other mechanisms, such as direct recombination of randomly distributed defects, have been suppressed since they become more important as the concentration of defects is increased [quadratic vs. linear according to Eqs. (1) and (5)]. It becomes imperative to study the flux dependence over several orders of magnitude since in this case also it is of great diagnostic value.

It is easy to construct more complex annealing sequences leading to more complex flux dependence. It should be also emphasized that short-lived intermediate atomic configurations will be favored by high flux irradiations. Thus, a slow irradiation at low temperature is not equivalent to a fast irradiation at a higher temperature unless the annealing steps are well separated in temperature. Another technique comes into play here; namely, the introduction of a given concentration of defects as essentially a delta function in time (relative to the annealing rates). With a suitable high flux machine this can be done at any temperature and the transients studied subsequently at the same temperature.

A specific experiment may be proposed here which is of interest to the work in progress at Brookhaven on enhanced diffusion. As pointed out above, the linear anneal apparently dominates under the conditions of our experiments. A consequence of this, as outlined in Appendix A, is that the radiation enhancement of diffusion is independent of the temperature and activation energies for the migrating defects cannot be determined. As outlined in Appendix B this information is obtainable from a "delta function" type experiment. The experimental determination of activation energies for mobility of defects is one of the important problems in current solid state physics.

In summary then, the following experiments, requiring the availability of high fluxes, are of current interest in solid state physics.

1) Flux dependence of the defect growth curve.

2) Flux dependence of the steady state defect concentration.

3) Flux dependence of radiation induced rate processes.

4) Introduction of defects as a "delta function" in time.

INSULATORS

In a metal the displacement of the electrons from their normal positions by radiation does not lead to observable effects because the electrons come to equilibrium exceedingly rapidly in a good conductor. This is not the case in an insulator and both electronic and atomic displacements are observable. The general considerations given under metals apply to displaced electrons as well as to displaced atoms. For insulators, the additional information derivable from high flux experiments is even more important than for metals since the situation is more complex and data under a larger variety of conditions must be obtained with respect to many physical properties (optical, electrical, mechanical). The four major experiments outlined for metals are directly applicable to insulators.

CHEMICAL EFFECTS

Displaced electrons and displaced atoms may lead to a variety of chemical reactions, particularly in covalent solids, via bond breakage and reformation. Decomposition, gas formation, degradation, cross-linking and polymerization are typical examples of such reactions. Some of these effects are known to be flux dependent.

In addition to inducing a direct chemical reaction, radiation can influence the chemical activity of a solid. A few cases of induced catalytic activity have been studied but nothing is known about the flux dependence of these effects.

In general, rather complicated dependence on flux is to be expected in the case of chemical effects. Essentially the radiation alters the rate constant by a different factor for different reactions. Intermediates may be promoted or suppressed, reactions which do not occur thermally may be induced (via enhanced nucleation or reaction rate). Such effects will be characterized by an intricate interplay of radiation intensity and temperature. Determination of the flux dependence is expected to lead to valuable scientific information and is essential for any reaction of practical concern. This field is rather new and the crucial problems cannot be outlined as yet.

APPENDIX A

Enhanced Diffusion with Linear Annealing of Defects*

It is assumed in this case that the defects disappear by migration to fixed sinks, such as dislocations, internal surfaces, or external surfaces. If v and i are the atomic fraction of vacancies and interstitials in excess of thermodynamic concentrations, then in steady state the concentrations are given by

$$dv/dt = K - K_v v = 0 \tag{1}$$

$$di/dt = K - K_i i = 0, \tag{2}$$

where K is the constant rate of defect production by radiation and is taken to be temperature independent, and K_v and K_i are the characteristic proportionality constants for the rate of removal of the defects. Steady-state approximation is valid as long as the period of defect buildup is short compared to the duration of experiment. At very low temperatures the defects are frozen in and this treatment is not valid.

From (1) and (2) one obtains, under steady-state conditions,

$$v = K/K_v \tag{3}$$

$$i = K/K_i; \tag{4}$$

K_v and K_i are proportional to the corresponding diffusion constants for vacancies and interstititals; namely,

*G. J. Dienes and A. C. Damask, J. Appl. Phys. 29, 1713 (1959)

$$K_v = \alpha_v \nu_v \lambda^2 \tag{5}$$

$$K_i = \alpha_i \nu_i \lambda^2 \tag{6}$$

The coefficients of self-diffusion are expressed by the following relations, for the vacancy and interstitialcy mechanisms, respectively.

$$D_v = v^* \nu_v \lambda^2 \tag{7}$$

$$D_i = i^* \nu_i \lambda^2 \tag{8}$$

where v^* = atomic fraction of vacancies, i^* = atomic fraction of interstitials, λ = jump distance, and ν_v and ν_i are the effective jump frequencies for vacancies and interstitials. Let the thermal equilibrium concentration of vacancies be denoted by v_o and i_o.

Let D' be now the diffusion coefficient under steady-state defect generation. For vacancies one finds

$$D_v' = (v + v_o)\lambda^2 \nu_v$$

$$= \frac{K}{\alpha_v \nu_v \lambda^2} \lambda^2 \nu_v + v_o \lambda^2 \nu_v$$

or

$$D_v' = (K/\alpha_v) + D_v \tag{9}$$

Similarly, for interstitials,

$$D_i' = (K/\alpha_i) + D_i \tag{10}$$

Thus, D' - D is independent of temperature and is proportional to the flux since K is proportional to the flux.

APPENDIX B

Suppose vacancies are suddenly introduced into a system by a burst of irradiation. That is, experimental conditions are arranged such that an excess vacancy concentration v_e is introduced at a given temperature T as a delta function of time. If the vacancies decay to fixed sinks, then, at any time t the excess vacancy concentration is given by

$$v = v_e \, e^{-k_1 t} \tag{1}$$

The corresponding diffusion constant is given by

$$D(t) = D_o + \nu_v \lambda^2 v_e \, e^{-k_1 t} \tag{2}$$

where

$$\nu_v = A_2^v \, e^{-\frac{E_m}{RT}}$$

and D_o is the diffusion constant corresponding to thermal equilibrium. The system is now characterized by a time-dependent diffusion constant, D(t). For one dimensional diffusion the partial differential equation reads

$$\frac{\partial^2 v}{\partial x^2} = D(t) \frac{\partial v}{\partial t} \tag{3}$$

This can always be transformed to

$$\frac{\partial^2 v}{\partial x^2} = K \frac{\partial v}{\partial t} \tag{4}$$

by the substitution

$$D(t) = K \frac{\partial t}{\partial \tau}$$

or

$$\tau = K \int \frac{dt}{D(t)} \tag{5}$$

Solutions of (4) then are of the standard form with time scale τ. A particularly simple case arises if D_o is negligible compared to the second term in (2) (i.e. at low temperature and for relatively short times after the burst where the enhancement is large). In this case

$$\tau = K \int \frac{dt\ e^{K_1 t}}{\nu_v \lambda^2 v_e} = \frac{K\ e^{K_1 t}}{K_1 v_e \nu_v \lambda^2} \tag{6}$$

where we can let $K = K_1$. Thus,

$$\tau = \frac{e^{K_1 t}}{v_e \nu_v \lambda^2}$$

leads to

$$\frac{\partial^2 v}{\partial x^2} = K_1 \frac{\partial v}{\partial \tau} \tag{7}$$

A plot of experimentally determined log (activity) vs. x^2 will give then $\sqrt{K_1\tau}$ as the slope. K_1 itself is expected to be proportional to $\nu_v\lambda^2$ and the experimentally measured quantity $Z = K_1\tau$ is given by

$$Z = K_1\tau = \frac{\alpha}{v_e}\ e^{t\alpha\lambda^2 A_2^v\ e^{-E_m^v/RT}} \tag{8}$$

α and v_e cannot be calculated with any accuracy. However, if measurements can be made as a function of time (at fixed T and still within the approximation of small D_o) then from

$$\ell n Z = \ell n \frac{\alpha}{v_e} + t\alpha\, \lambda^2 A_2^v\, e^{-\frac{E_m^v}{RT}} \qquad (9)$$

one obtains both α/v_e and

$$Y = \alpha\, \lambda^2 A_2^v\, e^{-\frac{E_m^v}{RT}} \qquad (10)$$

If Y is then determined as a function of temperature one obtains directly E_m^v from a plot of

$$\ell n Y = \ell n(\alpha\lambda^2 A_2^v) - \frac{E_m^v}{RT} \qquad (11)$$