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PROCEEDINGS OF THE CONFERENCE ON REACTIONS BETWEEN COMPLEX NUCLEI

Gatlinburg, Tennessee
May 5-7, 1958

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PROCEEDINGS

OF THE

CONFERENCE ON REACTIONS BETWEEN COMPLEX NUCLEI

Gatlinburg, Tennessee

May 5-7, 1958

Electronuclear Research Division

Editors

A. Zucker
R. S. Livingston
F. T. Howard

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Foreword

A conference on Reactions Between Complex Nuclei was held in Gatlinburg, Tennessee, May 5-7, 1958. To provide a record of the conference which might be of value to the participants and to others interested in this new and rapidly expanding field, a stenographic record was made of all the papers and the ensuing discussions. Following the conference the transcript of each talk, including the discussion which followed it, was sent to the speaker for a quick editing. This volume is the end result.

The editors have placed a premium on speed in issuing these Proceedings, and have left the papers essentially unchanged. Consequently, the reader will find different notation in different papers, bibliographies appended to some and not to others, and probably many errors due largely to the difficulties in obtaining an accurate stenographic record of highly technical material. The editors assume all responsibility and apologize to those who are misquoted. It is our hope that, in spite of more or less minor errors, the spirit of the meeting shines through, and that the important points of the speakers and the discussion are covered.

Since he was unable to attend the conference and present his paper in person, Professor Flerov submitted his contribution by mail. It is included in the Proceedings.

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1. COMPETITION BETWEEN FISSION AND NEUTRON EMISSION IN HEAVY NUCLEI

J. R. Huizenga
Argonne National Laboratory

The work which I will describe was done in collaboration with Dr. Robert Vandenbosch of our laboratory. The yield of any spallation product obtained by the bombardment of a heavy nucleus with gamma rays, neutrons, or charged particles (including protons, deuterons, alpha particles or heavy ions) is determined clearly by the competition between neutron emission and fission in the intermediate compound nuclei. As an example, if a heavy nucleus is irradiated with gamma rays, the most simple case, and is excited to 10 Mev and if one neglects gamma reemission, the two chief modes of decay for a heavy nucleus (charged particle emission can be neglected) are neutron emission and fission. The yield then of spallation products will be determined by the competition between emission and fission, and the remarks I would like to make this morning deal with the ratio of Γ_n/Γ_f. I will always be discussing the ratios of Γ_n/Γ_f in that the absolute quantities are difficult to derive. I would like to examine the ratio of Γ_n/Γ_f as a function of excitation energy and as a function of the nuclear parameters A, Z, etc.

We will then primarily be interested in the ratio of Γ_n/Γ_f as a function of the parameters, excitation energy, atomic weight, atomic number, etc.

A survey of some of the early work in this field is given on Slide I, where the excitation energy is given in column 1. Values of $\Gamma_f/(\Gamma_f+\Gamma_n)$ are given in the other columns. Column 1 gives the discrete gamma ray energies and these energies represent the excitation energy of the compound nucleus. The ratio of $\Gamma_f/\Gamma_f + \Gamma_n)$ stays essentially constant within the experimental error for different excitation energies. Columns 2, 3 and 4 are different data obtained for uranium-238. At excitation energies from 8 to 11 Mev, the branching ratio, $\Gamma_f/(\Gamma_f + \Gamma_n)$, is about 0.20.

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E(γ)	$\Gamma_f/(\Gamma_f + \Gamma_n)$				
	U^{238}			Th^{232}	
	(1)	(2)	(3) $\bar{\nu}$ = 2.5	(2)	(3) $\bar{\nu}$ = 2.5
6.5			0.24		
7			0.24		0.18
8	0.25	0.21	0.22	0.10	0.14
9	0.21	0.17	0.19	0.08	0.10
10	0.16	0.18	0.23	0.08	0.08
11	0.18	0.20	0.23	0.08	0.08

Slide 1

(1) Duffield and Huizenga

(2) Gindler, Huizenga, and Schmitt

(3) Lazareva and Nikitina

If one considers a different nucleus, for example Th^{232}, one sees that the branching ratio for fission is less than that of U^{238} and has a value of about 0.10. Again within experimental error over the energy range 8 to 12 Mev, the ratio of $\Gamma_f/(\Gamma_f + \Gamma_n)$ for Th^{232} is essentially constant.

We have irradiated a number of other heavy isotopes with gamma rays and the first systemic trend observed for fissionability is illustrated on Slide 2. The relative fissionability of heavy nuclei are plotted on Slide 2 as a function of Z^2/A. If one makes the assumption that the cross sections for making the compound nuclei with gamma rays are essentially constant in this region of A values, then the curve on Slide 2 shows the direct relation of the competition between fission and neutron emission. One sees that the probability for fission increases with Z^2/A.

We will now consider another type of reaction, the irradiation of a heavy nucleus with neutrons. Slide 3 illustrates the neutron reactions. We are now discussing the case when one irradiates a heavy nucleus (Z, A) with energetic neutrons. I would like to limit the discussion to reactions induced by neutrons between 2 and approximately 5 Mev. One makes the compound nucleus (Z, A+1), and its decay depends once again on the competition between neutron emission and fission, neglecting decay by gamma emission at these high excitation energies.

On Slide 3 are plotted the fission cross sections for several heavy nuclides -- Th^{232}, U^{238}, U^{236}, Pa^{231}, U^{235}, Np^{237}, U^{234}, U^{233}, Am^{241}, Pu^{240}, and Pu^{239}. These fine data were obtained at the Los Alamos laboratory by Henkel and collaborators. Two interesting observations which can be made from Henkel's data are the following. First, there is a very long plateau in fission cross section over considerable neutron energy, indicating that the ratio, $\Gamma_f/(\Gamma_f + \Gamma_n)$, stays essentially constant with excitation energy. Secondly, if one makes the assumption that the compound nucleus cross section of all these heavy isotopes is 3.3 barns, then a measure of the fission cross section gives a branching ratio between neutron emission and fission. In addition one observes that some of the heavy nuclei are much more fissionable than others, and one can again plot the data as a function of Z^2/A.

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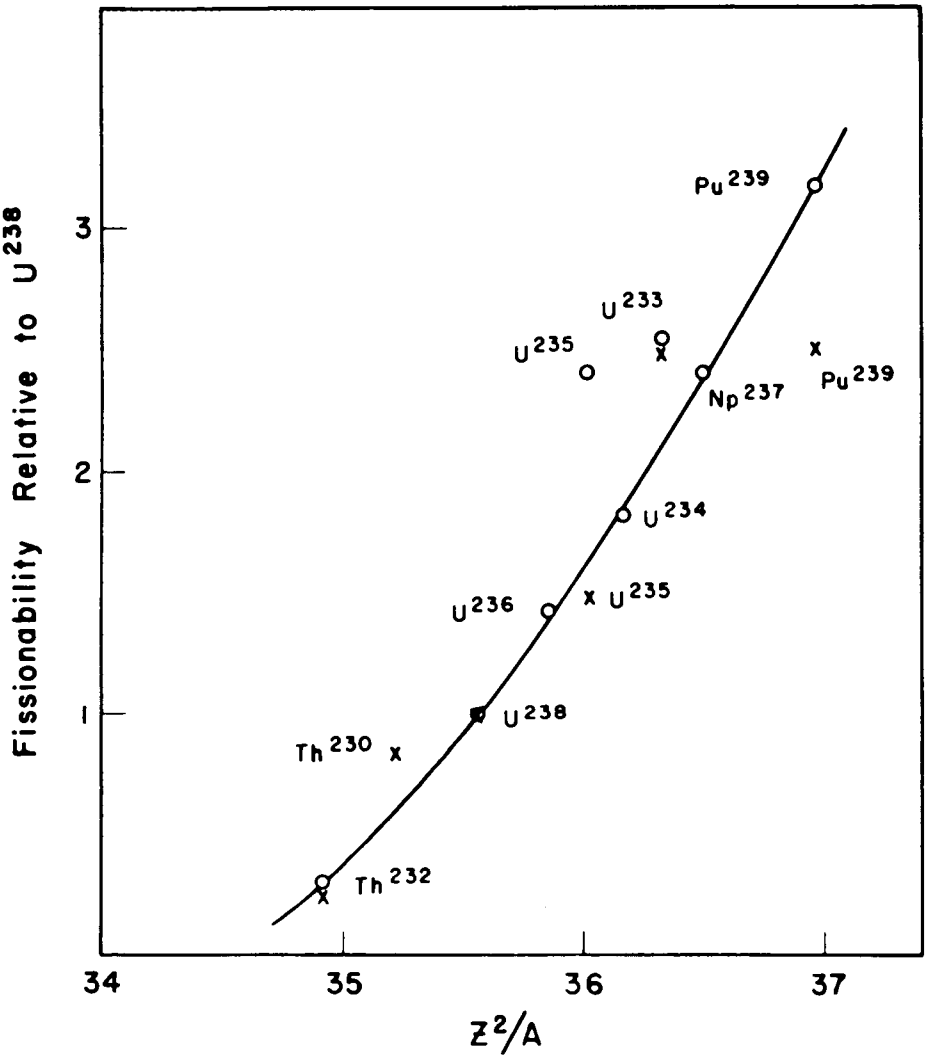
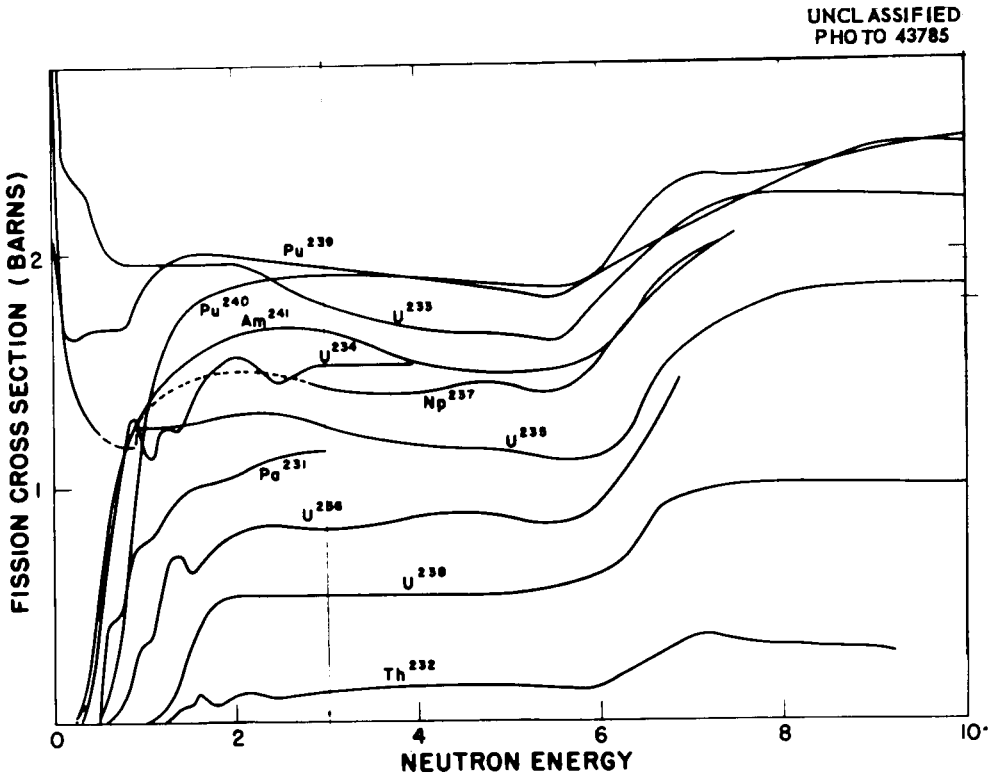


Slide 2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FISSION CROSS SECTION (BARNS)
2
1
0
2
4
6
8
10
NEUTRON ENERGY
Pu239
Pu240
Am241
U235
U234
Np237
U238
Pa231
U236
U238
Th232

Slide 3

Slide 4 shows the photon fission data and the neutron fission data. Once again it can be seen from this slide that the fissionability given by $\Gamma_f/(\Gamma_f + \Gamma_n)$ is directly proportional to Z^2/A. The ordinate of Slide 4 gives the fission width divided by the total width where the total width is assumed to be given by the sum of the fission width and neutron width. The open circles represent data obtained from photon fission and the closed circles from neutron fission. The quantity Z^2/A plotted on the abscissa is a fission parameter first discussed by Bohr and Wheeler. By making several simplifying assumptions, Fujimoto and Yamaguchi derived an equation where the neutron width to the fission width is essentially proportional to the nuclear temperature times an exponent containing the quantity the fission threshold minus the neutron binding energy divided by the nuclear temperature. With the assumption of constant temperature, the quantity given by the difference between the fission threshold and the neutron binding energy is a better parameter for correlating Γ_f/Γ_n than Z^2/A. The fission threshold is related to Z^2/A, but the abscissa of Slide 4 neglects the effect of neutron binding on the competition between fission and neutron emission.

The data which I have shown gives one some idea of the competition between fission and neutron emission at low excitation energy, that is up to something like 12 Mev. One is, of course, interested in examining this ratio at higher excitation energies. To do this we have studied a number of charged particle reactions. I would first like to say that if one examines charged particle reactions by looking at fissionable isotopes and non-fissionable isotopes one can derive Γ_n/Γ_f ratios for a number of heavy nuclides.

If one looks at a reaction that proceeds by processes other than the compound nucleus mechanism one notices that the cross sections for fissionable and non-fissionable target nuclides are very similar. In Slide 5, the (d,p) cross section is plotted for lead-208. The cross section values are for producing lead-209, or the partial (d,p) cross section leading to the capture of a neutron which is energetically bound. The maximum cross section for this reaction is something like 200 millibarns.

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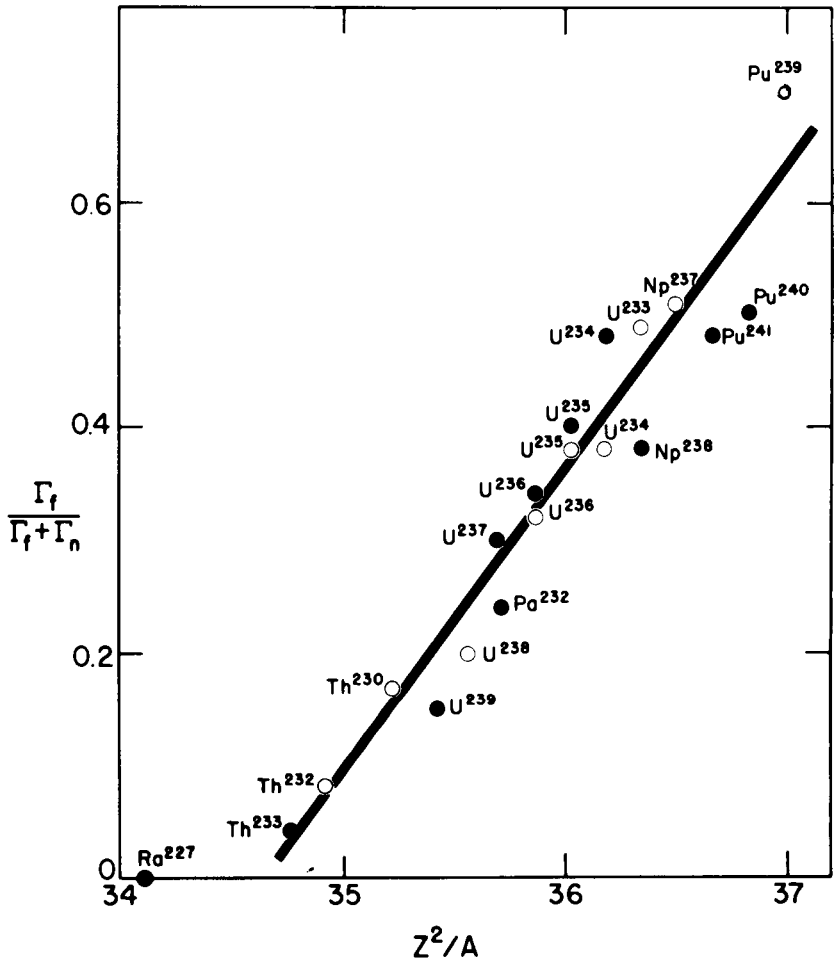
FISSIONABILITY OF HEAVY NUCLEI



Slide 4

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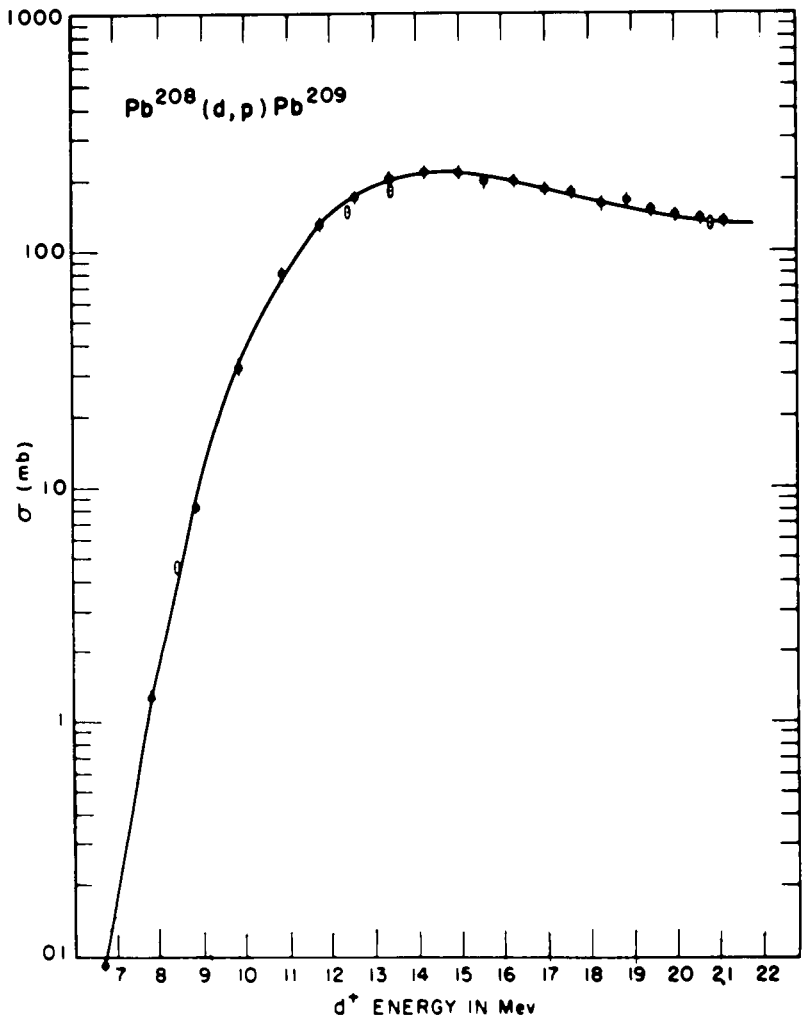

Slide 5

Slide 6 shows the cross sections for a (d,p) reaction of a fissionable isotope. The target is U^{238}. The cross sections are for bound neutron capture giving U^{239}. Again the magnitude of the maximum cross section is about 200 millibarns. There is very little difference then in (d,p) cross sections between fissionable and non-fissionable nuclides. However, if one looks at the compound nucleus reactions one begins to see a considerable difference between targets of fissionable nuclides and non-fissionable nuclides. For example, if one bombards Bi^{209} with helium ions of 40 Mev one gets the compound nucleus astatine-213 excited to the order of 35 Mev. In this reaction the excited compound nuclei decay by multiple neutron emission, giving excited astatine-212 which again emits another neutron to astatine-211, etc.

The assumption is made that the probability for decay by gamma emission at these high excitation energies is very low, and one can essentially treat such a decay by considering only neutron emission when it is energetically possible. These reactions (A,Xn) can be treated almost quantitatively by a theory which was developed by Jackson. The cross section for the reaction (A,Xn) where A is the bombarding particle and X is the number of neutrons emitted is given by the product of the compound nucleus formation cross section and the quantity P(E,X), where P(E,X) is the probability for emitting exactly X neutrons at the excitation energy E. The equation which Jackson derived has incorporated in it several simplifying assumptions. The nuclear temperature is assumed to be constant with excitation energy and he assumes an energy spectrum given by Weisskopf for the evaporated neutrons. The decay by gamma emission is neglected in the early evaporation stages and also, of course, decay by charged particles is neglected. One then can try to fit the experimental excitation functions for (A, Xn) reactions with the simple expression of Jackson.

Slide 7 shows a comparison of the experimental excitation functions for a non-fissionable target nucleus with the calculated curves of the Jackson model. I have taken along only this one example. On this slide we show the reaction of Bi^{209} with 22-Mev deuterons. The points are the experimental cross sections. The solid lines represent the

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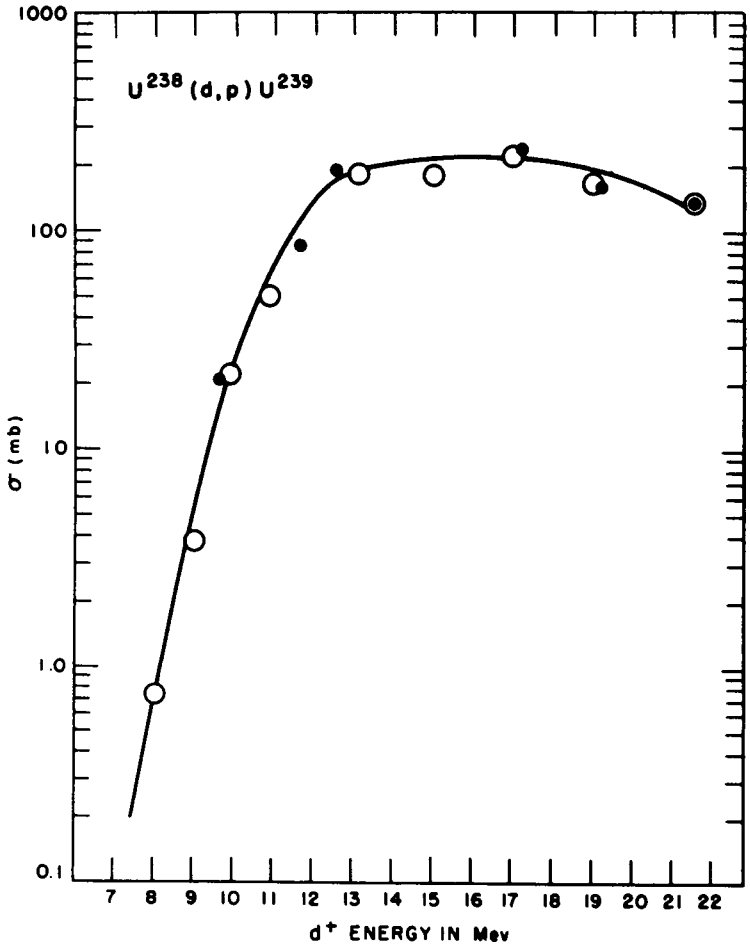

Slide 6

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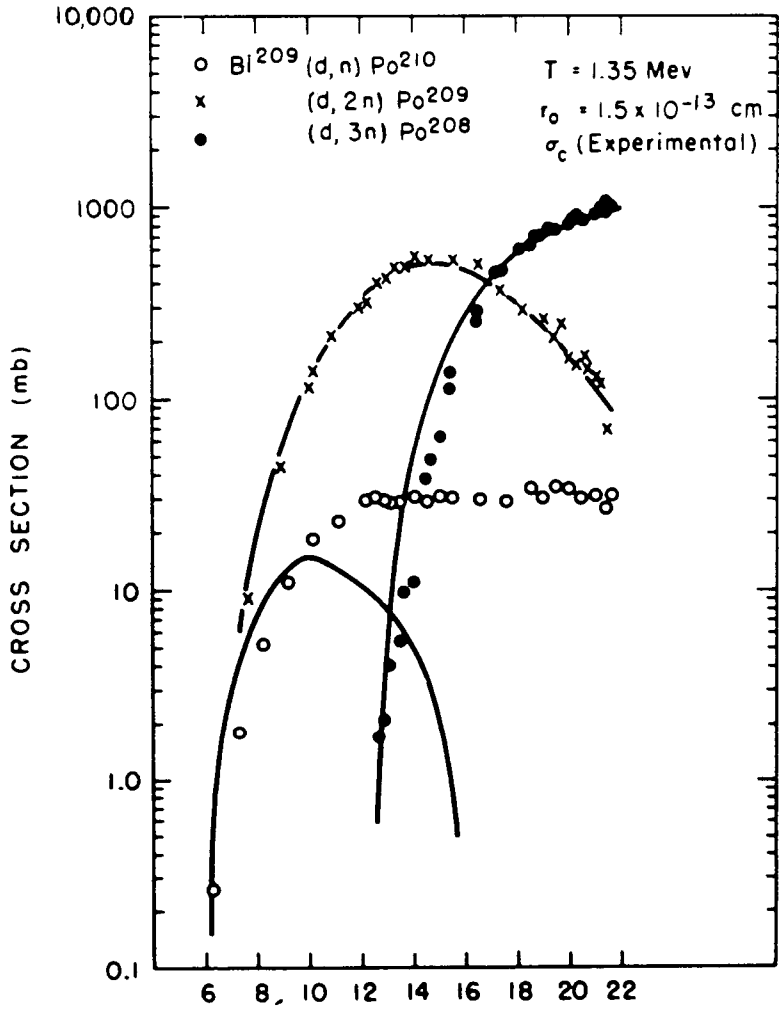

Slide 7

theoretical cross sections derived by the simple expression, $\sigma(A, Xn) = \sigma_c P(E, X)$. For non-compound nucleus reactions, like a (d,n) reaction, the theory of course, does not work. However, for the compound nucleus reactions like the (d, 2n) reaction we see that the theory agrees very closely with the experimental cross sections. The (d, 3n) cross sections are also well reproduced from the above simple expression.

Next I would like to have you take notice of the magnitude of these cross sections. You see that the maximum values are of the order of 1 barn. One can use this simple theory for fissionable targets by incorporating a parameter which accounts for the fact that these excited nuclei decay by fission as well as neutron emission. For example, if I now simply change the Bi^{209} target to U^{238}, the excited compound nucleus can fission as well as emit neutrons. One has to take account of the fact that another decay channel has been introduced, the fission channel. It is therefore necessary to incorporate a quantity, $\Gamma_n/(\Gamma_n + \Gamma_f)$, which is the branching ratio for neutron emission into the Jackson equation. The yield of a spallation product will be considerably smaller than that for a non-fissionable target because of the fission competition at each step. The neutron branching ratio for each competition step must be included in the equation when more than one competition is possible.

Using then an extension of the Jackson model which Vandenbosch had already used at Berkeley, we derived $\Gamma_n/(\Gamma_n + \Gamma_f)$ values from the spallation cross sections measured from the bombardment of heavy nuclei with charged particles. Such calculations have been done for some 40 charged particle reactions and the results are illustrated on Slide 8.

This slide gives one some idea how well the experimental cross section values can be fitted. By adjusting the quantity $\Gamma_n/(\Gamma_n + \Gamma_f)$ in the theoretical cross section equation to give the best fit with the experimental cross sections, one derives the competition between neutron and fission from spallation reactions. On Slide 8 are shown the

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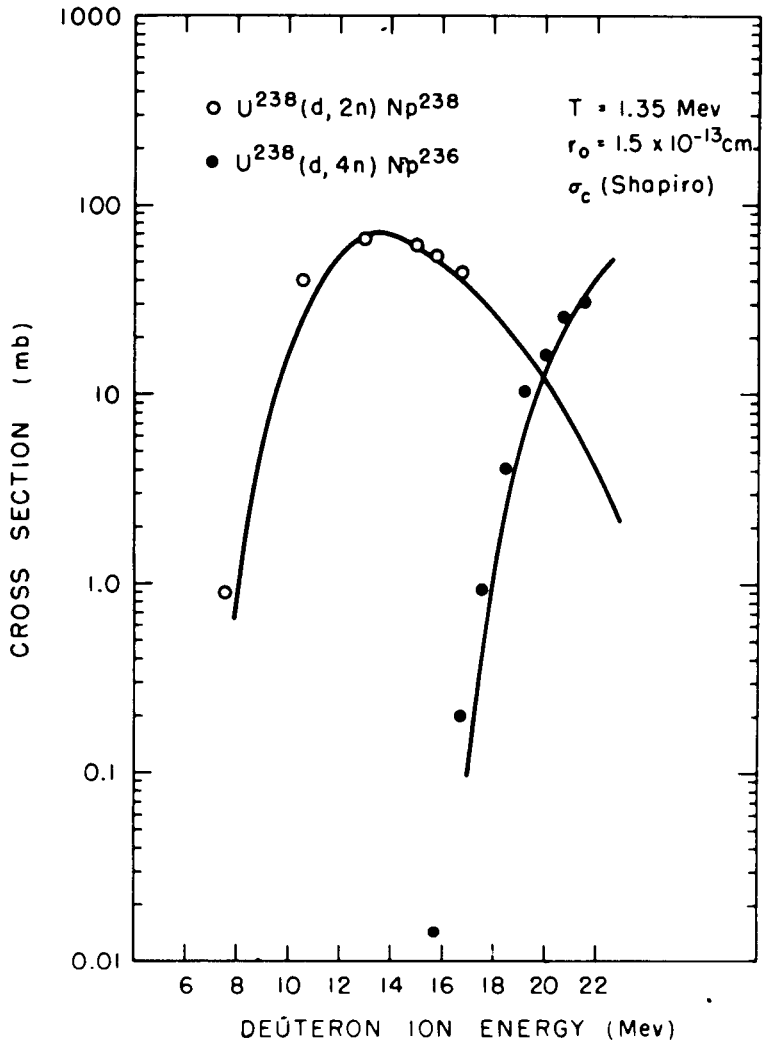

Slide 8

(d, 2n) and (d, 4n) reactions on the target U^{238}. The spallation cross sections for the fissionable target U^{238} are much smaller than the corresponding cross sections for a non-fissionable target, due of course, to the fission competition.

We have used this general approach to derive values of Γ_n/Γ_f for spallation reactions. These values are plotted on Slide 9. Here we have plotted Γ_n/Γ_f as a function of mass number, and each line is a constant Z line. The open points belong to even Z, and the solid points belong to odd Z. There are about 60 points in all on this plot. Furthermore, let me identify some of these points by origin. The triangles are the photon fission data and they represent an excitation energy of about 9 Mev. The squares are from the Los Alamos and Oak Ridge neutron fission data, again representing an excitation energy of about 9 Mev. The circles, the diamonds, and the inverted triangles are from spallation reactions. There are about 40 such points. The circles represent competition at an average excitation energy of about 13 Mev, the diamonds 18 Mev, and the inverted triangles something like 25 Mev.

If you will focus your attention on the data in Slide 9, you see no obvious excitation energy dependence on any of these points even though they are taken at considerably different excitation energies. The value of Γ_n/Γ_f obtained from, say a (A, 4n) reaction has been plotted at an intermediate value of A. The conclusion from these experiments is that: at energies up to about 40 Mev the quantity Γ_n/Γ_f is within experimental error independent of energy. Some have argued in the past that at high excitation energy Γ_f goes to zero. I would now like to discuss some experiments which give some indication of what is happening to the ratio of Γ_n/Γ_f at higher excitation energies. I think it is obvious that for reactions of heavy ions on heavy elements one is interested in the excitation energy dependence of Γ_n/Γ_f up to about 100 Mev.

The evidence for the fission width going to zero at high excitation energies has come chiefly from the reaction of Th^{232} with high energy protons. These are the fission experiments of Steiner and Jungerman at the University of California. They bombarded Th^{232} with protons rangining from 100 to 340 Mev, and measured the fission cross section

in this energy range. They observed a fission cross section which was relatively constant in this excitation energy region and had a magnitude of something like 0.8 of a barn.

The total inelastic cross section of these very heavy nuclides is known to be something like 1.5 to 1.6 barns. I think it is immediately obvious that if one is making a compound nucleus here we have many possibilities for competition between neutron emission and fission, and one comes to the conclusion that the fission cross section should approach the inelastic cross section even though the fission width is not very large. In making that deduction one has, of course, to be very careful, because at these high proton energies one has to consider in some detail the cascade protons. In order to evaluate the effect of cascade protons at these energies, we have done an analysis of the reactions of Th^{232} and U^{238} with high energy protons. To make these calculations, one has to make use of Monte Carlo calculations for the evaluation of prompt cascades of protons and neutrons. We have used the calculations for high energies performed by Jackson at McGill University. He made use of the unpublished Monte Carlo calculations of Sharp and McManus. One then proceeds in a way which I think is quite obvious but let me illustrate some aspects of the calculation in the next few slides. Compound nuclei with a distribution of excitation energies are formed since the cascade protons and neutrons which are emitted carry away part of the excitation energy. The important fact is that if several cascade protons are emitted, the resulting nucleus and the nuclei formed by subsequent neutron evaporation have small fission widths. For example, the emission of several cascade protons from the target nuclei Th^{232} and U^{238} makes compound nuclei which do not fission very well, and this is the clue to understanding the sizeable spallation cross sections which have been observed.

Slide 10 gives us some idea of the numbers of prompt cascade particles. Here the ordinate gives the cascade protons on a relative scale. The abscissa gives the number of cascade neutrons ejected for fixed values of prompt protons. Slide 11 gives the excitation energy distribution of the residual nuclides following various prompt events. The top figure in Slide 11 gives the excitation energy distribution for no prompt neutrons and one prompt proton.

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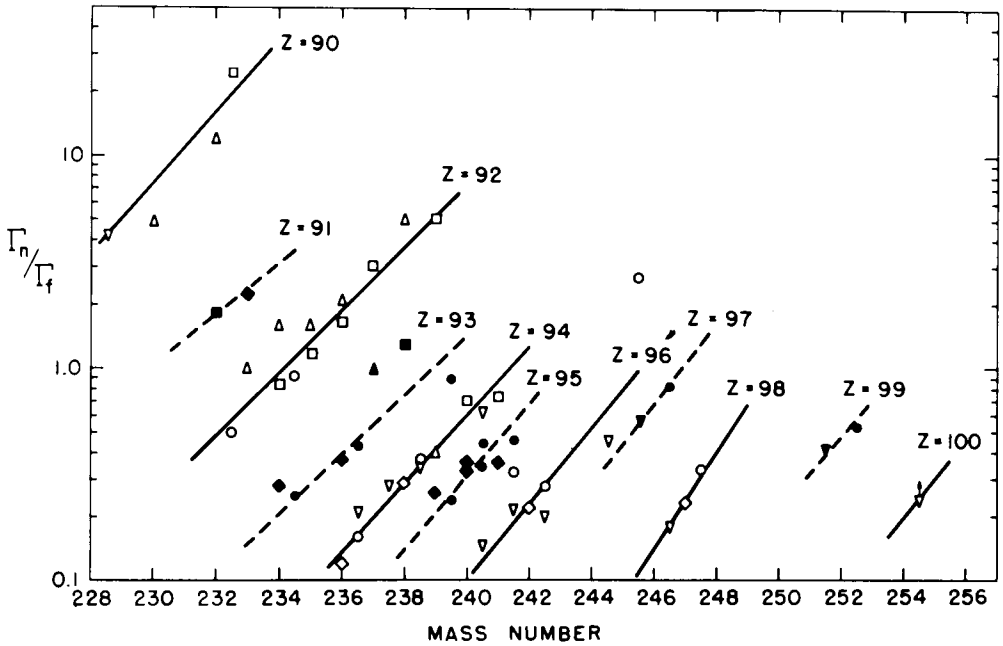

Slide 9

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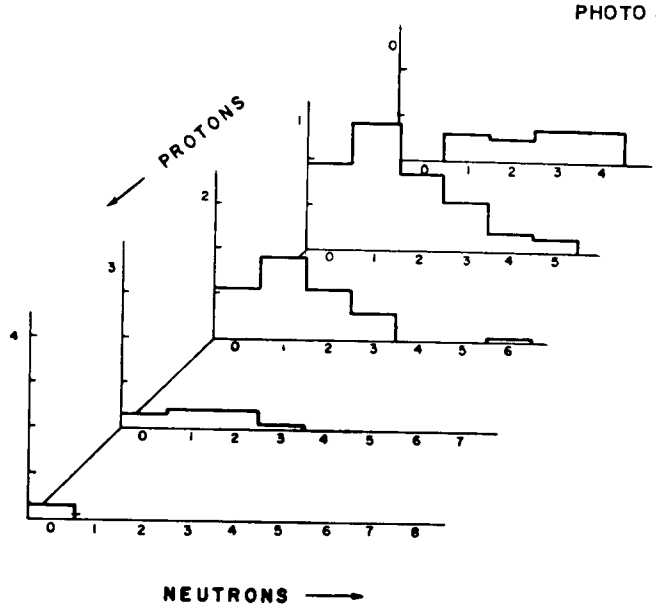

Slide 10

Calculated frequency distribution of numbers of prompt protons and neutrons ejected from a heavy nucleus bombarded by 400-Mev protons (McManus and Sharp, unpublished).

The second figure from the top in Slide 11 gives the excitation energy distribution when 1 cascade neutron and 1 cascade proton are emitted. For the various probabilities of cascade particles one gets the different excitation energy distributions given in the remainder of the figures of Slide 11.

Slide 12 shows the results of Jackson's calculations. These results are for a target nuclide which does not fission. I don't have time to discuss the difference between the dotted and the solid curve. It is based upon a different group of assumptions about the nuclear temperature, etc. But let's look, say, at the solid curve. The curve Y = 1 is for 1 cascade proton. This curve gives the yield of various spallation products following the prompt and evaporation events with one ejected proton. The X on the abscissa is the total number of prompt and evaporated neutrons. The ordinate gives the cross sections. We have taken these calculations and have essentially added only one factor. The factor which we have added for the target nuclei Th^{232} and U^{238} corrects for the decay by fission in addition to neutron emission. We have taken the Jackson data, therefore, and have made corrections for fission. In making these corrections we have used our low energy values of Γ_n/Γ_f which I showed in Slide 9.

Slide 13 gives the results of our calculations which essentially are Jackson data (400 Mev H) corrected for fission on the target nucleus Th^{232}.

The yields of protactinium isotopes (no cascade protons) are small and I have neglected them. The solid circles (for 1 cascade proton) are the calculated cross sections for thorium isotopes. The crosses (x) (for 2 cascade protons) are the calculated cross sections for actinium isotopes. The (+) (for 3 cascade protons) are the calculated cross sections for radium isotopes. Linder and Osborne, at the University of California, have measured with 340-Mev protons, several spallation cross sections for target nuclei Th^{232} and U^{238}. The calculated cross sections were for 400-Mev proton bombardments but since no other experimental data is available we will compare the cross sections even though the bombarding energies are different. The experimental points have circles around them. The experimental spallation cross sections

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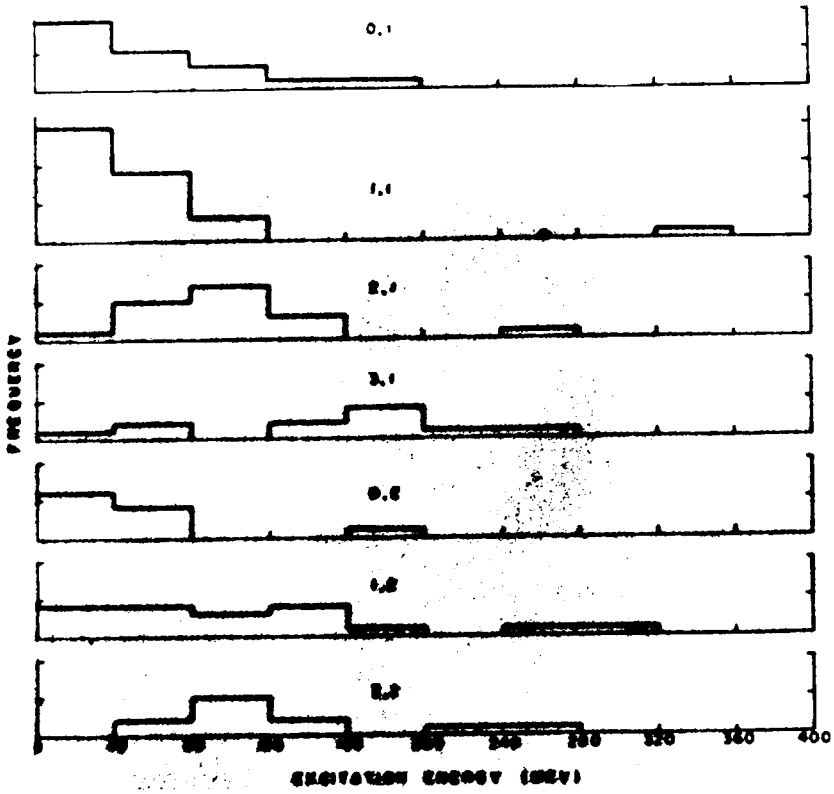

Slide 11

Calculated distribution of residual excitation for several types of prompt events. The pair of numbers (i, j) correspond to ejection of i prompt neutrons and j prompt protons (McManus and Sharp, unpublished).

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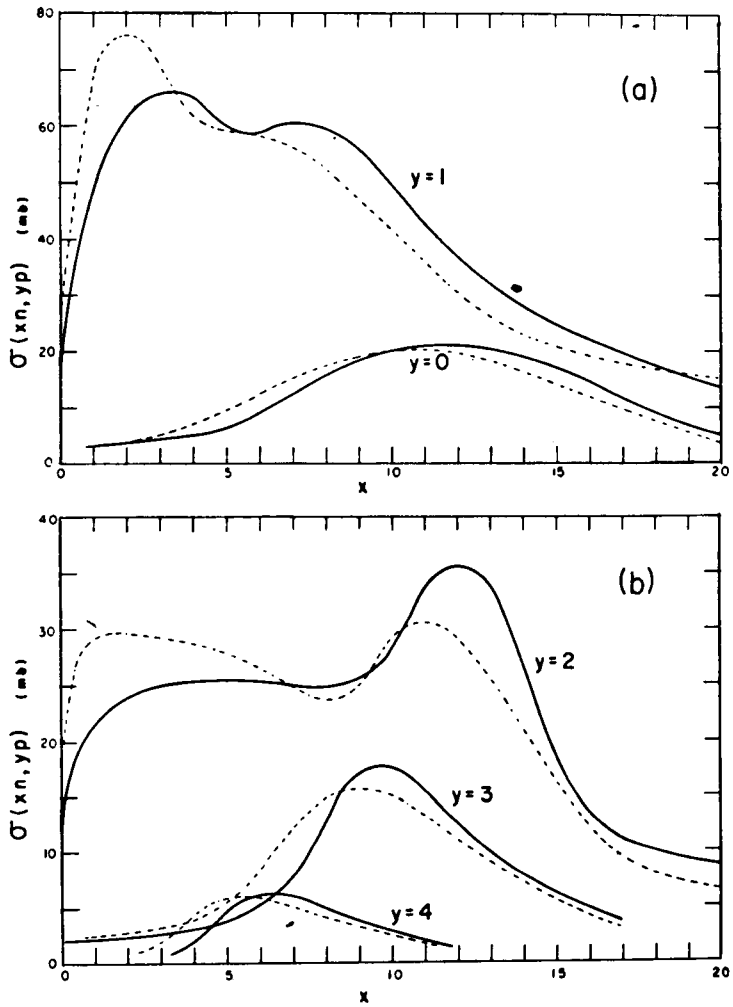

Slide 12

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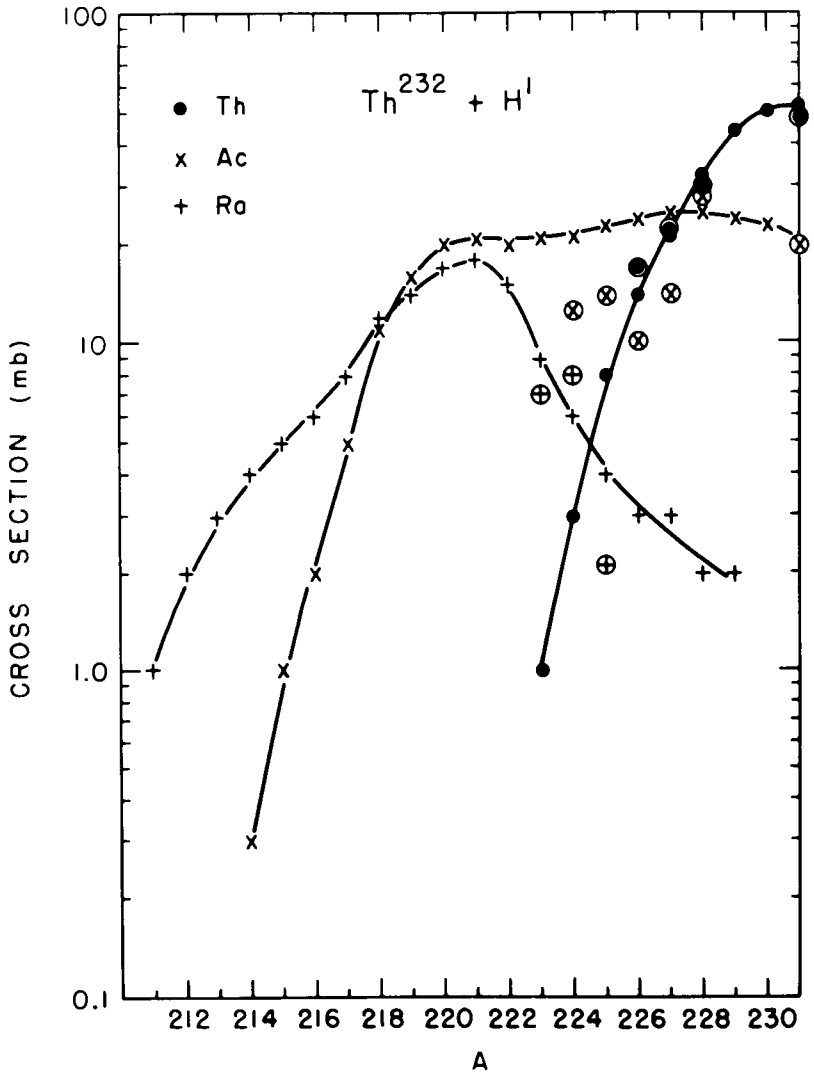


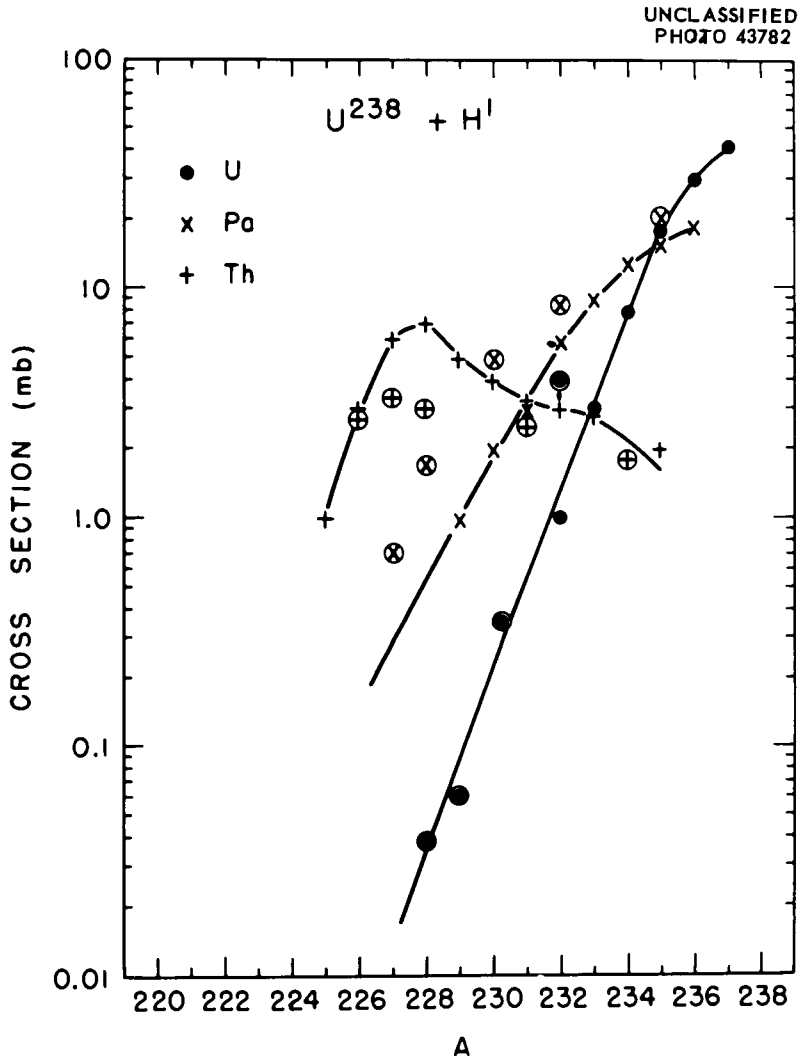
Slide 13

giving isotopes of thorium and radium agree rather well with our calculated values. Some of the experimental cross sections for actinium isotopes have, however, lower values than the calculated cross sections.

Slide 14 shows a comparison of spallation cross sections calculated for the target nucleus U^{238} with some experimental cross sections measured by Linder and Osborne, again with 340-Mev protons. Here again the three curves represent the data for 1 cascade proton, 2 cascade protons and 3 cascade protons. The points with the circles around are the Berkeley experimental points. One sees that even after many neutrons are emitted, the calculated cross sections which depend on our ratios of Γ_n/Γ_f are in good agreement with the experimental uranium cross sections. The protactinium isotope cross sections of Linder and Osborne are somewhat higher than the calculated values for these isotopes.

By integrating over all of the spallation cross sections, we calculate that the spallation cross section for thorium with 400-Mev protons is about 0.7 barn. This large spallation cross section arises from the fact that the fission probability is greatly reduced by the cascade protons. We see no reason to conclude from these experiments that the fission width is going to zero at high excitation energies, because the sum of the fission and spallation cross sections (within our experimental error) gives a reasonable inelastic cross section. The integrated spallation cross section for uranium is about 0.3 barn. Even though the calculations we have discussed are very crude, we feel that they serve to illustrate that Γ_n/Γ_f values which are independent of excitation energy give spallation cross sections which are consistent with experimental data.

Recently I received some information (from the Copenhagen meeting on fission) about the heavy ion work going on in Russia. If I could have Slide 9 back again please, I will illustrate the magnitudes of Γ_n/Γ_f obtained from carbon and other heavy ion bombardments of uranium and other targets. I think that Dr. Sikkeland will essentially cover these experiments. I might give just a brief introduction.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$U^{238} + H^1$
U
Pa
Th
CROSS SECTION (mb)
100
10
1.0
0.1
0.01
220 222 224 226 228 230 232 234 236 238
A

Slide 14

If one bombards uranium-238 with carbon-13 ions, the compound nucleus is californium-251. The bombardment of uranium-238 with carbon-12 ions gives the compound nucleus Cf^{250}. The (C^{13}, 5n) and the (C^{12}, 4n) reactions both give Cf^{246}. From a study of these two reactions one can calculate the quantity Γ_n / Γ_f for Cf^{251} at a rather high excitation energy. Heavy ion reactions are in many ways convenient for determining Γ_n / Γ_f values at high excitation energies. The value of Γ_n / Γ_f obtained for Cf^{251} is in good agreement with the value predicted from an extrapolation of the californium line in Slide 9. This result is again in agreement with the assumption that Γ_n / Γ_f is independent of excitation energy.

Let me close by saying that in general the Russian data fit our curves in Slide 9. We believe that the experimental data are such that one is not justified in drawing the conclusion that the fission width is going to zero at the excitation energies we have discussed.

CHAIRMAN BOYD: Thank you very much, John. That was a very beautiful paper and there is certainly lots of order as you can see from these plots. I think that we have time to invite several questions from the floor or comments.

C. E. PORTER: This is perhaps a rather detailed question. I wonder how sensitive these calculations are to the radius. It seemed that the one you had on the slide was quite large.

HUIZENGA: Yes. We have not looked into this in any great detail, but we have gone ahead simply on the basis that we are able to fit the theory to the non-fissionable case using an interaction radius, $r_o = 1.5 \times 10^{-13}$ cm. There is some uncertainty, it is true, in the interaction radius but we feel that this is not a very important factor as far as the Γ_n / Γ_f values are concerned.

H. L. REYNOLDS: We have made some measurements of the fission cross section for C^{12} bombardments of gold. For a carbon energy of 120 Mev we find that the fission cross section is about 90% of what we think the total cross section would be.

HUIZENGA: The Russians have done quite similar work, have they not? We have extended the kind of plot I have shown here down into the region of lead and bismuth and feel, at least at present, that it is reasonable on our kind of plot to get that kind of conclusion only if the angular momentum is playing a significant role.

R. B. LEACHMAN: An experiment was done several years ago by Harding and Farley in which they looked at the angular correlation of the fission fragments and neutrons from the reaction of U^{238} with 147-Mev protons, and they deduced from their data that most of the neutrons are emitted from the excited nucleus before fission. I wonder if there is an explanation for that.

HUIZENGA: Do you want me to take five minutes for that?

CHAIRMAN BOYD: We are a little behind time.

HUIZENGA: The conclusions of Harding and Farley are I think in error for several reasons. The Harding-Farley experiment measured alternately the neutrons emitted in the direction of, and perpendicular to, the direction of emission of fission fragments. Neutrons emitted before fission will be isotropic apart from a small effect due to recoil of the target nucleus. If the neutrons are emitted from the fission fragments isotropically in the frame of reference of the fragments, then in the laboratory system the intensity of neutrons will be more along the line of motion of the fragments. Harding and Farley measured a neutron intensity ratio, $N_o/N_{90} = 1.27 \pm 0.11$.

To interpret this experimental ratio of 1.27, one must know the energy spectrum of the fission neutrons. For thermal neutron fission the N_o/N_{90} intensity ratio is about 4 in the laboratory system. Harding and Farley assumed a neutron energy spectrum from fission which was too low and therefore arrived at their conclusion. Marquez has pointed out in the Proceedings of the Physical Society that with a fission neutron spectrum of higher energy, a ratio of N_o/N_{90} of 1.27 is compatible with the emission of all the neutrons from the fission fragments.

B. L. COHEN: How was that slide that you showed several times determined? Was this determined just by measuring the fission cross section and assuming what the total reaction cross section should be? If it was, it is a poor way of doing it.

HUIZENGA: Many of the points, for example the γ-ray experiments, were determined by measuring both the photo-neutron emission cross section and the photofission cross section. The neutron fission results were determined by measuring the fission cross section and making an assumption about the compound nucleus cross section, but this gives one only a very small error.

COHEN: I don't think that is right at all. Say, for example, there is at least one point that we have measured which seems to be far off that curve. Well, let's say the place where fission occurs 75% of the time, and if you have two or three chances for it to happen -- in other words, you require 3 neutrons to be boiled off.

HUIZENGA: You are talking not about the neutron data or the photon fission data but about the charged particle data? Is that correct?

COHEN: Yes.

HUIZENGA: Yes, there is certainly some uncertainty there, and one obtains these values by using the Jackson theory and simply trying to get as unbiased a value of Γ_n/Γ_f as possible from the Jackson theory simply by fitting the experimental spallation cross section point by point with the theory. These values of Γ_n/Γ_f depend, however, on the spallation cross sections and not on the total fission cross section. The total fission cross section is a very insensitive measure of Γ_n/Γ_f for these reactions.

COHEN: There is at least one point where we measured the competition and it was reasonably far off your curve. Maybe we should talk about it later.

HUIZENGA: Yes. I think that before drawing that conclusion one has to transform the data into the sort of plot we have here. In other words, just taking the raw data from a 30- or 40-Mev run you cannot immediately compare these results with our plot. When one takes into account the A and Z dependence then to my knowledge only about 1 point falls off our curve and we are fairly sure that something is wrong with that experiment.

2. REACTIONS OF CARBON IONS WITH HEAVY NUCLIDES

T. Sikkeland
University of California, Berkeley

In the production and the investigations of isotopes, methods for calculation of excitation functions are of great importance. I will restrict myself to talking about some carbon-induced reactions in heavy nuclides.

One year ago we bombarded U^{238} with the C^{12} beam from the Heavy Ion Linear Accelerator at Berkeley. Just before this bombardment we were aware of Jackson's neutron evaporation calculations of cross sections for (p, xn) reactions on lead and bismuth[1] and we applied these to the (C, xn) reactions observed. We could not do absolute calculations but we got agreement on a relative scale.

What we observed was the (C, 4n) and (C, 6n) reactions; we also observed (C, α4n) and (C, α6n) reactions[2], as seen on Slide 1. We now modified the Jackson formula to also include fission. Dr. Huizenga has quoted this formula, which simultaneously was applied to helium-induced reactions in heavy nuclides.[3] I will just write it again. The cross section for the evaporation of x neutrons is:

$$\sigma(C, xn) = \sigma_c(C)\, \bar{G}^x \quad P(E^*, x) \qquad (1)$$

We had to apply a small correction in the Jackson formula for $P(E^*, x)$ because we actually have fission below the binding energy of the last neutron. In other words, in the case where the fission threshold is less than the binding energy of the neutron we had to introduce this characteristic.

We didn't know the cross section for the compound nucleus. We then assumed that (C, 4n) and (α, 4n) reactions proceed by compound nucleus formation, and if that was the case the mode of decay of the same compound nucleus, at the same excitation energy, was independent of the way of formation. The reaction U^{238} (C, 4n) Cf^{246} should therefore be equivalent to the reaction Cm^{246} (α, 4n) Cf^{246} and have

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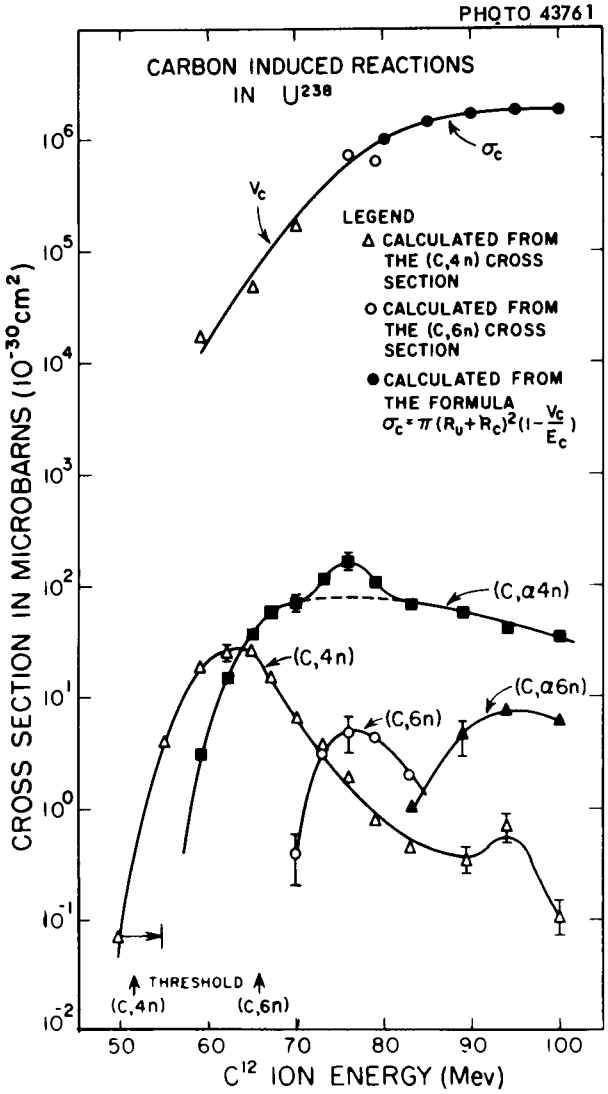

Slide 1

the same G value. This quantity has not been measured, but could be estimated from the systematic compilation of Γ_n / Γ_f by Vandenbosch and Seaborg based on (α, 4n) reactions.[4]

Unfortunately no G values exist for the (α, 6n) reactions. But assuming the $\overline{G}$ is independent of excitation energy (as has been shown to be the case), we introduced some average values taken from the (α, 4n) reactions. (If you do the same thing for (C, 5n) you have to do some estimation of the change in the fissionability of the even-even and odd-even nuclides.) The compound nucleus cross section could now be calculated from the measured (C, 4n) and (C, 6n) cross sections. We tried to fit the values to a smooth curve and it turned out we didn't have too much choice for nuclear temperature; T = 0.96 Mev gave the best fit.

At bombarding energy of the carbon, 1.2 times higher than the Coulomb barrier, we introduced Blatt and Weisskopf's classical formula. A nuclear radius parameter of 1.5 fermis fitted the curve well. If we will go back to Slide 1 again we can see the results.

We noticed one thing; we could not get the points at low energy to fit at all. We observed also one point below the threshold of the (C, 4n). This discrepancy is probably due to errors in the estimated carbon ion energy from the accelerator and uncertainties in the range energy curves. The combined errors may be of the order of 5 Mev. If the energy is 5 Mev higher, the nuclear temperature would be the same as found in alpha reactions, which is 1.35 Mev.[3]

You will notice another thing, that in order to get the curve to coincide at higher energy, we had to increase the nuclear temperature slightly. Around 80 Mev, T was 1.05 Mev. This may be an indication that the nuclear temperature actually increases with increasing energy.

We assumed, now, that the cross section for compound nucleus would be the same for a neighboring nuclide for the same value of energy of carbon. From the G values we can then calculate the cross section for (C, xn) reaction.

The next reaction studied was Pu^{242} (C^{12}, 4n) Fm^{250}. In that case we had experimental data for the B value based on the reaction

Cf^{250} (α, 4n) Fm^{250}. That might be one reason that we obtained such good agreement as seen in Slide 2.

You see a bad scattering here. The reason might be that the recoil technique was used while we used the stacked foil technique in the bombardment of uranium. The maximum cross section here is around 9 microbarns.

I think we can make a simple conclusion from these studies, and that is that in the case of neutron evaporations the decay through the different channels are essentially the same for helium-induced and carbon-induced reactions.

We must be careful, however, to draw quantitative conclusions from the estimated cross section. With another nuclide where the G value has to be estimated you have to expect more than 50% errors in the G value and therefore also in the calculated cross sections.

The other reaction which we observed was the (C, α4n) reaction with uranium, which gives an excitation curve which together with energy considerations, might indicate that the alpha is not evaporated. We can assume that, when the alpha is promptly emitted, no competition occurs with fission and can use the following formula which is based on Jackson:

$$\sigma(C, \alpha xn) = \sigma(C, \alpha)\, \overline{G}^{x} \int \left(\frac{dN}{dE^{x}}\right) P(E^{*}, x)\, dE \qquad (2)$$

$\sigma(C, \alpha)$ is the total cross section for reactions in which an alpha particle is emitted. dN/dE^{*} is the excitation energy spectrum of the nucleus after alpha emission. $\overline{G}$ and $P(E^{*}, x)$ were previously defined.

If we look at this formula we can assume that if we have neighboring nuclides, then at the same bombarding energy, the alpha spectrum is the same. Therefore, the integral will be constant, and we will have $\log \sigma(C, \alpha xn) = A + X \log G$. We should then expect a straight line, as plotted on a log-log scale.

In Slide 3 is shown a series of (C, αxn) cross sections and their subsequent predicted variations with G. Unfortunately only for (C, α4n) reactions are more than one experimental value available. We see that the cross sections for U^{238} (C, α4n) Cm^{242} and for Pu^{242} (C, α4n) Cf^{246}

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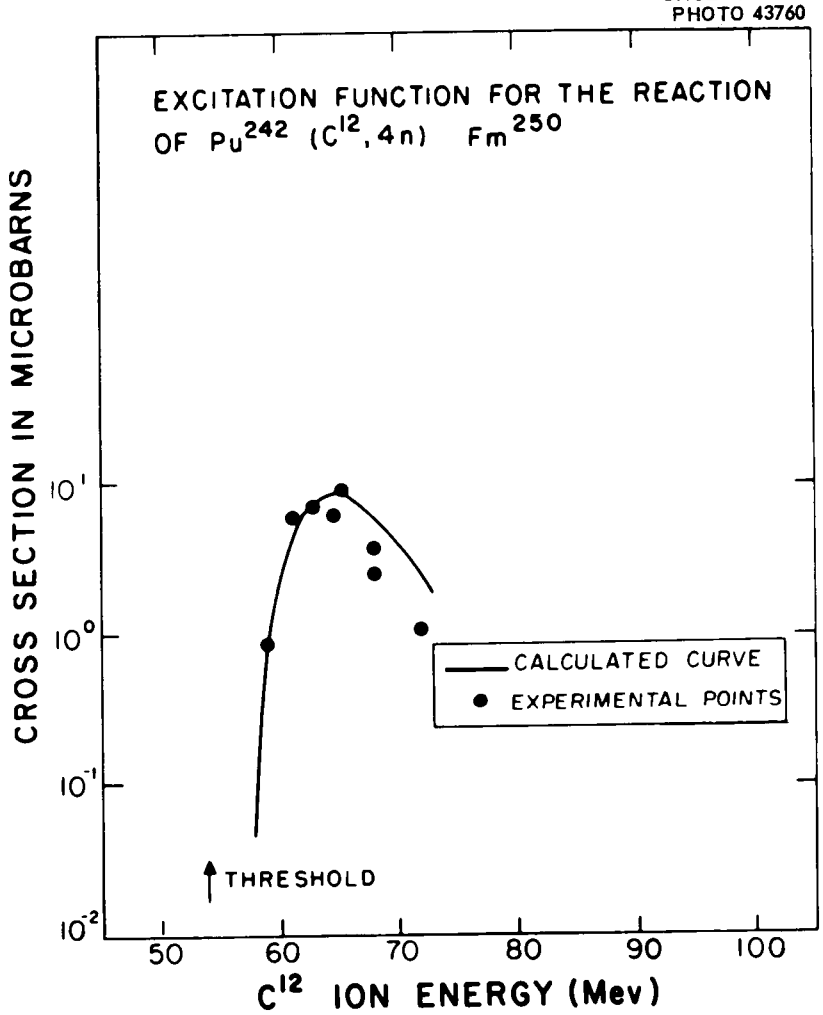
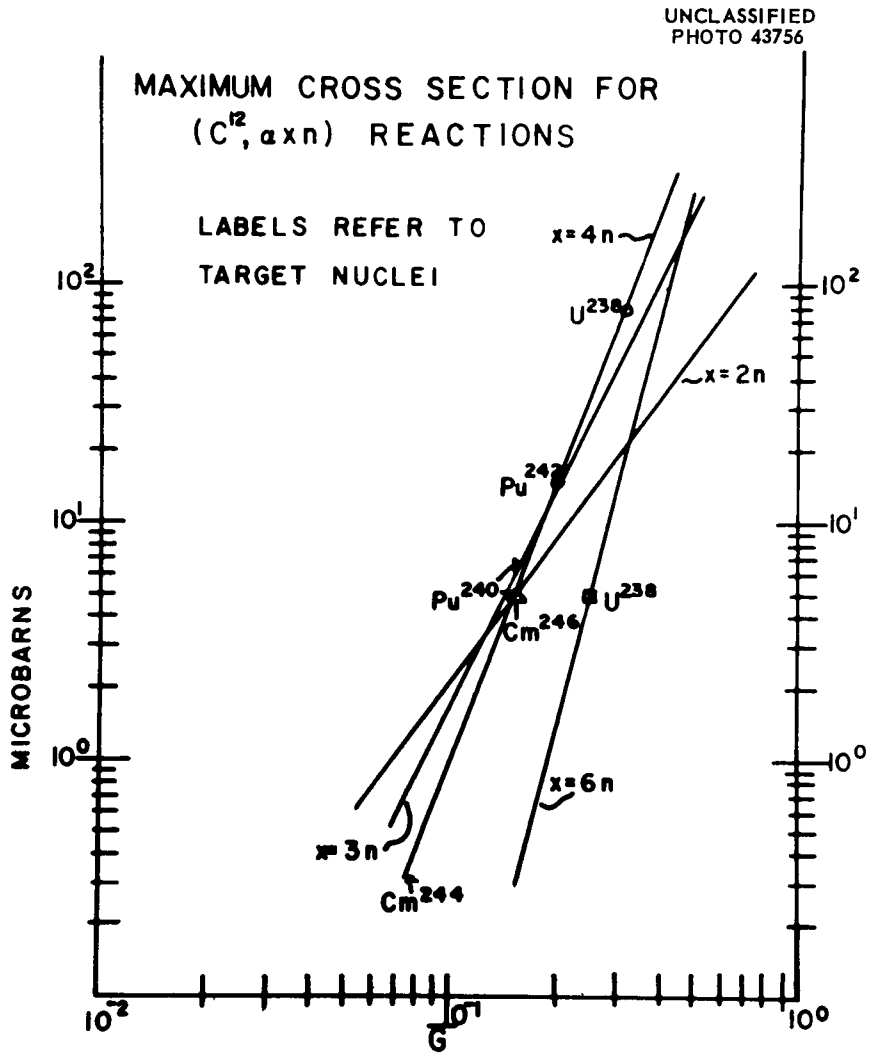


Slide 2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MAXIMUM CROSS SECTION FOR
(C12, αxn) REACTIONS
LABELS REFER TO
TARGET NUCLEI
x=4n
U238
x=2n
Pu242
Pu240
Cm246
U238
x=6n
x=3n
Cm244
MICROBARNS
10²
10¹
10⁰
10⁻²
10⁻¹
10⁰
G

Slide 3

are on the expected slope. But we cannot from this draw the conclusion that formula (2) is correct.

It might be of interest to compare the total cross sections for (C, α) and (C, 2α) [or (C, Be^8)] reactions. In formula (2) the integral must be less than 1 and therefore $\sigma(C, \alpha) > \frac{\sigma(C, \alpha xn)}{\overline{G}^x}$. A similar relationship is obtained for (C, 2α) reactions. By inserting experimental values we obtain $\sigma(C, \alpha > 10$mb and $\sigma(C, 2\alpha) > 100$mb at 70 Mev. If this is correct and we recall that the compound nucleus cross section for the complete amalgamation of a carbon ion is around 1 barn at this energy, we see that the assumption $\sigma_f = \sigma_c$ is not completely justified because the stripping reactions also will lead to nuclei which mostly undergo fission. The observed fission cross section then must be larger than the cross section for the compound nucleus formation.

You see here that the (C, 2α) stripping is bigger than the (C, α) stripping, and this might be an indication of the lower penetration probability of two alphas in relation to one alpha.

I might also mention here that with this formula one can get an estimate of the alpha spectrum by comparing (C, αxn) cross sections. Unfortunately we do not have sufficient data yet, but just from rough preliminary experiments with Pu^{242} and Pu^{240} targets we think that the alpha particle probably carries, on the average, one-third of the kinetic internal and transitional energy of the carbon ion.

CHAIRMAN BOYD: Thank you very much, indeed. We have time for about two or three questions.

A. ZUCKER: I have two questions. First, how do you know that this is a stripping process:

SIKKELAND: Well, we don't know it actually. It is just an assumption. On that basis we end up with this kind of calculation. We think that this assumption is probably true. We don't have any definite work on that, but the (C, α4n) excitation curve was very broad, extending up to 110 Mev. We thought that this might be an indication of the stripping process, but we have just made this assumption.

ZUCKER: Second, if one were to assume such a process it would be much more unambiguous to do it somewhere where fission does not compete so much.

SIKKELAND: Well, I agree, but as I pointed out at the beginning of my talk, the work here was started from a production point of view to get some basis for estimation of cross sections in the heavy nuclide region where we were interested. With the really heavy nuclides the fission competition is the most important part and we thought that it was more realistic to bombard heavy isotopes. And actually the formula I have introduced is not based on a stripping process, the only assumption made is that the alpha particle is emitted promptly and the neutrons are evaporated.

REFERENCES

1. J. D. Jackson, Can. J. Phys. 34 767 (1956)
2. T. Sikkeland, S. G. Thompson, and A. Ghiorso, UCRL - 8142, March 1958.
3. Vandenbosch, Thomas, Vandenbosch, Glass, and Seaborg, (Phys. Rev. to be published).
4. Vandenbosch and Seaborg, Phys. Rev. 110 507 (1958)

3. PRODUCTION OF HEAVY NUCLIDES

A. Ghiorso
University of California, Berkeley

The work which I am about to describe was done by myself plus my colleagues, Dr. Sikkeland who just spoke, Dr. John R. Walton, and Professor Seaborg.

What I am going to talk about is the production of elements heavier than element 101. The work naturally divides itself into two phases. The first, regarding element 102, was an attempt to confirm or to disprove some prior work done on 102 last year. The work was done by an international team consisting of two men from the Argonne National Laboratory, one from Harwell, and three from the Nobel Institute. The work was done between April and July of last year, and if I might sketch it very quickly, the method consisted of using the internal beam of C^{12} ions in the big Nobel Institute cyclotron in Stockholm to bombard a target of curium-244. The transmutation recoils were caught in a plastic catcher; after the period of bombardment this was placed on a platinum plate, ignited and burned away, and then pulse analyzed. In some cases after the ignition of the plastic the material was dissolved and put through certain kinds of ion exchange chemistry.

The experiments were extremely difficult. The beams that they had were not very large, ranging from 30 to 100 mμa. The target was 1 mg/cm^2 thick; in five out of six cases it was made by painting an amyl acetate solution of curium nitrate on thin aluminum foil. The isotopes which they hoped to obtain were to be made by the following reactions with curium-244. The particles they used were carbon 13 so that 4 neutrons out would give 102^{253} and 6 neutrons out would give 102^{251}. They saw alpha particles which seemed to decay with a half life of 10 min, although this was very uncertain and the alpha energy was approximately 8.5 ± 0.1 Mev.

They communicated the results to us very early and after a momentary period of chagrin, we congratulated them on their work. But after thinking more about it we decided that perhaps it was not clear that they had made element 102. We had just turned on the HiLac for the first time and had done experiments, not with curium because we were not ready to do that, but with uranium plus nitrogen. One of the products found was Th^{225} which decays eventually to Po^{213}. This has a very short half life. The half life of Th^{225} is around 8 min. We suggested that perhaps this could explain the alpha particles they had seen in Stockholm. The reason for wondering about this is that with such a high alpha energy an isotope of element 101 would not be expected to decay so slowly. This would be a very unusual case, the only one known with such a high hindrance; an alpha energy this high would imply more likely a half life of seconds. We told them about this work and they checked and decided that it could not be that contamination, and finally published their work later in the year.

To indicate the difficult nature of their experiments, in only three out of their six targets which they used at various periods of time did they see any events. Of those three in only 50% of the time did they see any events, and then after an initial period they could not be produced although other isotopes were produced simultaneously. They attributed this to a thickening of the targets by the action of the bombarding particles. So, all in all, they found that only 12 out of 50 bombardments were successful in producing 8.5 Mev alpha activity.

Slide 1 is taken from their published work to indicate the difficulties of their experiment. There was not a tremendous number of counts available. In this particular run they identified, presumably element 102, by the ion exchange method and saw four events here (drops 5, 6, and 7), a few counts of fermium-250 (drop 11), and a small amount of californium-246 (drop 22-30). The very lucky thing about this bombardment was that, although it was conducted at the lowest beam current of 30 mμa they got more counts than normal. This was 4 counts after a period of decay of the order of 1 or 2 half lives. I should mention that the energy used to produce this activity was mostly around 90 Mev.

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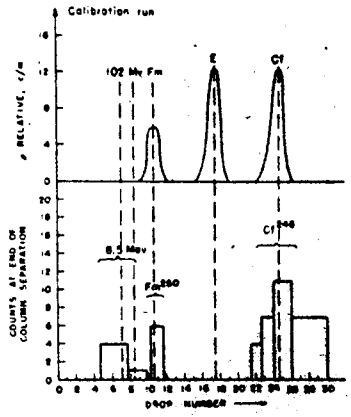

FIG. 2. α-hydroxy isobutyrate elution curve of recoil atoms from curium bombardment.

It was also shown that the 8.5-Mev activity appeared in the actinide position along with californium and fermium, produced in the irradiation, when eluted from a hot cation exchange column with 6*N* hydrochloric acid. On the basis of these experiments we concluded that the 8.5-Mev activity is due to an isotope of element 102, either directly or, possibly, as a result of electron-capture decay of a 102 isotope with a ten-minute half-life to a much shorter-lived alpha emitting mendelevium daughter. In either case, of course, the alpha activity would appear in the 102 fraction.

The [illegible] half-life for 8.5-Mev alpha emission [illegible] isotope is of the order of ten seconds. Thus, even-mass isotopes with unhindered alpha decay would not be observed. The most probable isotopes are 102^{252} produced by the $(C^{12},4n)$ reaction and 102^{254} produced by the $(C^{13},6n)$ reaction. The low number (1–5) of 8.5-Mev alpha decays observed in a single bombardment made the statistical errors too large to determine the energy dependence of the cross section any more accurately than to limit it to one of these two reactions.

We suggest the name nobelium, symbol No, for the new element in recognition of Alfred Nobel's support of scientific research and after the institute where the work was done.

We wish particularly to thank A. B. Beadle of Harwell for his assistance in the chemical work at Stockholm and in England. We would like to acknowledge the aid of R. Barnes and Mrs. R. Sjoblom of Argonne in purifying the curium used, the aid of N. Jackson and Mrs. K. Glover of Harwell in preparing targets, and the excellent cooperation of the cyclotron crew and other members of the technical staff of the Nobel Institute in these experiments. We are grateful to Dr. W. M. Manning, director of the Chemistry Division of Argonne, Dr. R. Spence, Head of the Chemistry Division of Harwell, Dr. H. A. C. McKay, leader of the heavy-element group at Harwell, and Professor M. Siegbahn, director of the Nobel Institute of Physics for their encouragement and interest in the research. The American and British authors would also like to thank Professor Siegbahn for the hospitality extended during this project.

* This work was performed under the auspices of the U. S. Atomic Energy Commission, the United Kingdom Atomic Energy Authority, and the Swedish Atomic Energy Commission.

† Chopin, Harvey, and Thompson, J. Inorg. Nuclear Chem. 2, 66 (1956).

Slide 1

Since the amount of bombarding activity was so low, they were not able to measure an excitation function. They thought it should peak around this high energy--this is in contrast to what Dr. Sikkeland showed in his talk that the C, 4n reaction should peak around 65 to 70 Mev.

By September we were in a position to attempt to reproduce the experiment ourselves, and so for the next several months we tried to do so. Instead of one target we used six targets simultaneously. They were electroplated targets, 0.4 mg/cm^2, each mounted on 0.03 mil nickel.

Slide 2 will show you the theoretical efficiency of getting recoils out of each target (neglecting angular straggling). You can see that with a thickness of 0.4 mg/cm^2 curium oxide that we should get of the order of 80% of the possible recoils from the target. This curve was based on Bohr type calculations which had been substantiated earlier by Dr. Leachman of Los Alamos. His work at the Nobel Institute several years ago had shown that the recoil ranges of astatine isotopes which he measured would fit these calculations nicely. We have since also confirmed that by measuring the recoil range of the atoms of Tb^{149} produced from the C^{12}, 4n reaction with Pr^{141}.

This shows that we should have gotten most of the recoils out of our six targets. Then we proceeded to make bombardments. The curium had essentially the same isotopic composition as that used at the Nobel Institute. We found that we could not use plastic catchers because the beam was too intense and concentrated. Instead we were forced to use palladium. The palladium was 0.9 mg/cm^2 and would easily contain all the recoil range. The palladium had to be separated; a 2-molar, Dowex-1 anion exchange column was used. This type of column separates all of the elements beyond plutonium in one lump. We are able to look at the transplutonium fraction in less than 8 min. We used carbon ions, both 12 and 13, ranging in energy from 60 to 100 Mev with up to 200 mμa.heavy ion current.

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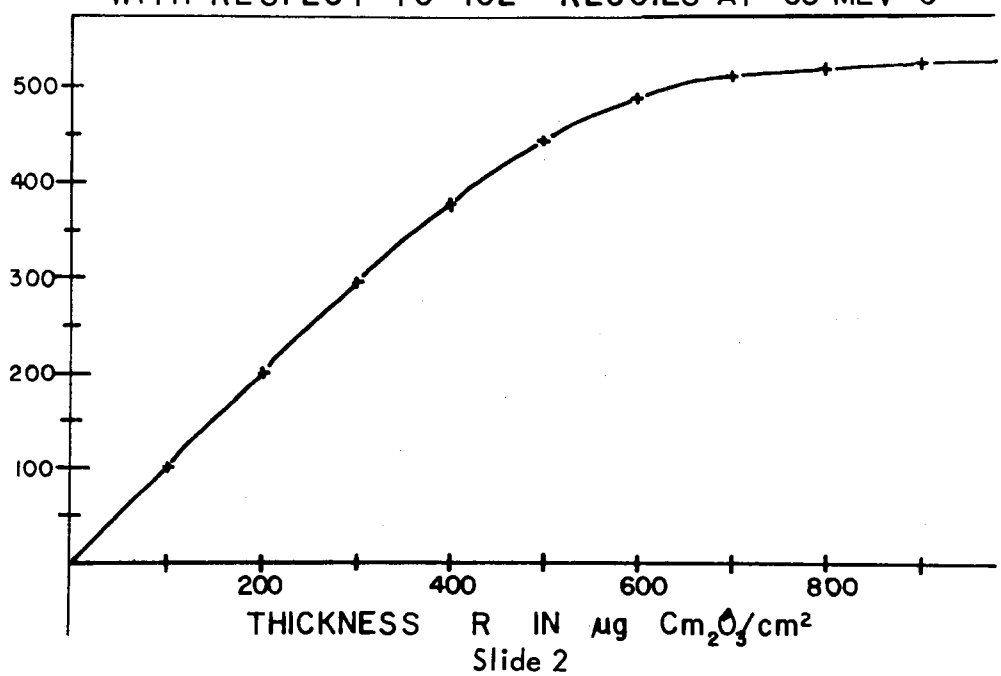
EFFECTIVE THICKNESS of Cm_2O_3
WITH RESPECT TO 102^{253} RECOILS AT 65 MEV C^{13}



Slide 2

The fact that the bombardments were satisfactory was indicated by the very large amounts of californium-246, californium-245 and fermium-250 that were made with these bombardments. Instead of 0.1 count per minute, which is about what the other slide showed for the Stockholm experiments, we made something like 10 to 100 c/m of californium-246.

Now this is a good tracer to normalize bombardments, we became assured of this because the excitation function for producing californium-246 -- I think it is C^{13}, 2α 3n reaction, probably it is something like so. It is relatively flat on top and the cross section goes up to the order of a millibarn. So we thought that we could legitimately compare the experiments by comparing the amount of californium-246. In addition we can compare the californium-245 and fermium-250 production.

Well, there is still some possible objection to this experiment as a comparison, since one could argue that perhaps the recoils were volatilized out of the palladium after being caught. So to take this in to account we next tried catchers of plastic so that the atoms would be oxidized immediately and this could not evaporate. The plastic catchers were cooled with helium and after bombardment were put on a Pt plate and ignited. From the end of bombardment to the beginning of alpha pulse analysis took approximately one minute. Again we saw no long-lived activity that we could ascribe to element 102. In fact, we saw nothing except the aforementioned isotopes of californium and fermium.

In order to make the maximum use of these bombardments we developed a multiplexing system; that is, instead of using one alpha grid chamber for pulse analysis of the activity we used five simultaneously. We managed this by combining the signals from the five grid chamber preamplifiers in one amplifier and then fed its output to one pulse analyzer. An identification signal from each preamplifier was also obtained, and this together with the pulse analyzer output was recorded on a paper tape printer. The size, origin, and time of each event was thus recorded and the data could be analyzed at leisure. This system works well at low counting rates (a few counts per minute), but it is not suitable for very high counting rates.

To take maximum advantage of it, we separated the large amounts of californium and fermium with an ion exchange column and looked at the 102 fraction which is the very heavy fraction. About this time we decided that the Nobel Institute work must be wrong. We felt that we should have been able to easily reproduce their experiments, and so then we went on with our own experiments which were designed to look for short-lived alpha activity.

The section of the isotope chart concerned with the heaviest elements is shown in Slide 3. It was expected that the light isotopes of element 102 that we were trying to make by heavy ion bombardment should be short lived. We looked at the known daughter isotopes to do a milking experiment; the only isotope that was suitable was fermium-250. This has a 30-min half life with a distinctive alpha energy. So we arranged to milk this activity from a short lived isotope of element 102. The method we used was one that had never been used before and it has much utility for other work. Basically, we find that if we take a target and knock the transmutation recoil atoms out into a gas, the recoil energy can be absorbed by the gas and the atoms brought to rest on the surface of a charged catcher plate. This is charged negatively by a voltage which depends on the gas used. In the case of helium and over a distance of a centimeter or so, the voltage has to be anything from 50 volts on up.

After finding that this method would work we then tried a modification as follows: Instead of the stationary catcher plate there is a metallic belt which goes in a direction perpendicular to the beam. The atoms of 102 are pulled down to the belt during each pulse and then are carried along at some constant speed. There is an electrostatic shield to isolate the target. After a length of time, depending on their half life and depending on the speed of the belt, they decay somewhere along the belt path. Those alpha particles that go into the belt kick the daughter atoms off the belt. Let us assume that we have 102^{254} on the belt; the daughter atom, Fm^{250} would be kicked off into the gas. Just above the belt there is another plate which is more negative.

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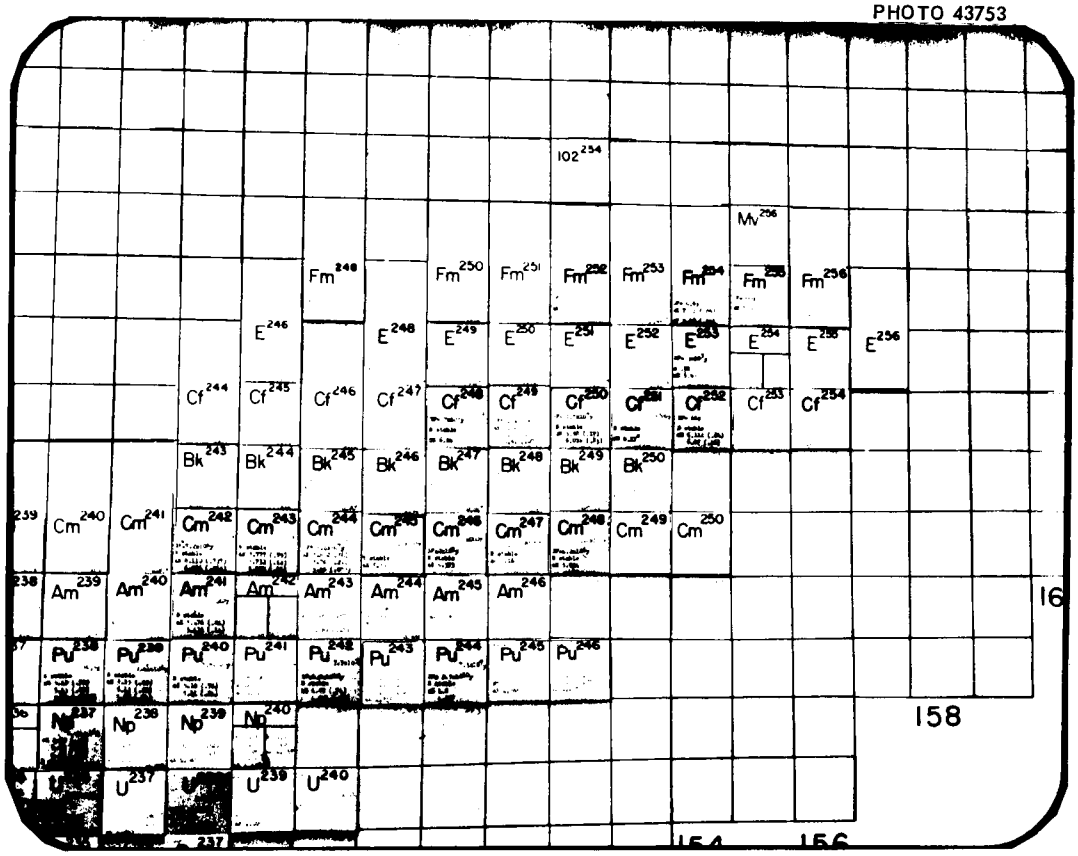

Slide 3

Let us put minus 400 volts on the belt and let's say minus 600 volts on this daughter collector. Then the fermium atoms, the daughter atoms of 102, will be collected on this plate in accordance with the half life of 102. It is just a continuous physical milking experiment, and if this catcher is cut up transversely into five separate sections and analyzed simultaneously with the multiplex alpha grid chamber system you can determine the decay of the parent.

Slide 4 shows our elaborate milking machine. The beam from the HiLac vacuum goes through two windows and through an energy degrading foil. In this volume helium is circulated to keep the beam from burning up the foils. The beam goes through a small target area which is roughly 2 by 5 mm. In the actual case of this one experiment we used only one target. The atom is knocked into the gas, pulled down onto the belt and carried under the second catcher foils. The Faraday cup is used to measure the beam current.

Slide 5 is a photograph of the apparatus. The streak in the center is the beam going through the target chamber and ionizing the helium. Normally it is observed by means of a closed circuit television system so that we can see that everything is working properly. There is a device here which moves the whole target system up and down and back and forth. The reason for this is to keep the beam from burning the target and the windows. It is so concentrated that even with a very small amount of beam, it would burn a hole in a stationary target. By moving the whole system we are able to take a fairly high current. We have used beams of 0.5 μa but we keep it down to 0.3 just for a safety factor.

The amount of curium in a target is quite dangerous, of course, if allowed to get loose. There is a safety circuit, so that if a window burns out a very fast gate is automatically closed to keep the curium from blowing into the HiLac tank.

To show that the method will work there seemed to be no known combination of isotopes that would allow a satisfactory test. We had to resort to finding a new isotope of fermium. We bombarded Pu^{240} with C^{12} ions and a (C^{12}, 4n) reaction produced fermium-248 which

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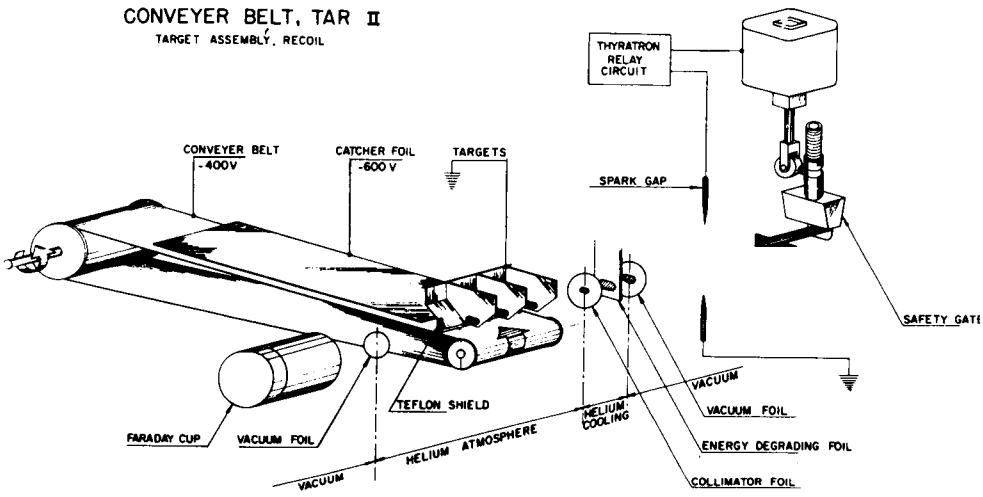
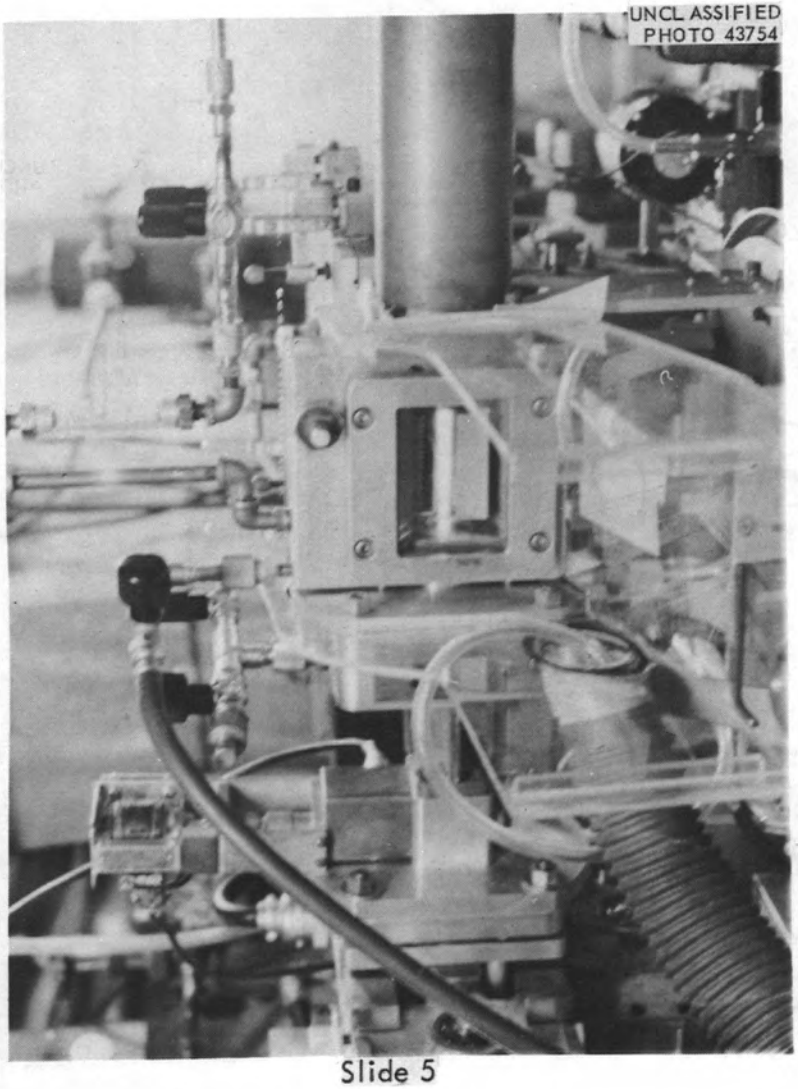


Slide 4



Slide 5

decayed to the known isotope californium-244 which has a 20-min half life and a 7.2 Mev alpha energy. We found that the Fm^{248} half life was approximately 0.6 min, which agrees very well with what you would expect.

Besides this type of experiment, I might mention that by substituting a nuclear emulsion very close to this belt one can measure the alpha energies produced by measuring the range of the atoms as they decay. I will tell you about some experiments a little later on in which this has worked successfully, although it is much more tedious.

Slide 6 is the decay one then gets in this fermium-248 milking experiment. You can see that the data fits very well, except that the first point is off and quite definitely so. We discovered that some of the atoms could collect around the first barrier, and so we improved the shielding and were able to reduce this so that this effect was negligible. By measuring the belt speeds and by comparing the relative amounts of californium-244 collected by the catchers one gets the half life of the parent activity.

The next experiment was, of course, to try to apply this to curium. By this time we had looked at our six electroplated curium targets and found that they had been essentially destroyed. The excessive alpha activity had possibly generated nitric acid by reaction with the humid air; the nitric acid had attacked the nickel mounting of the target, leaving us with almost nothing but a translucent film of curium. The curium was all right, but the nickel was gone. Obviously, we could not use them; so a new target had to be supplied.

This time we used a method that had been used successfully in England by John Milsted in which you spray the activity. This is done by taking a very small capillary with a thin wire in it, dissolving the curium nitrate in acetone and then connecting a potential between the wire and the place where you want to deposit, letting the activity come out slowly. By keeping the foil on which it is caught quite hot, we were able to transform the curium nitrate into curium oxide. If one does not do this you get a film which tends to have too much water of hydration and it just flakes off. By keeping it hot we convert it to the oxide as it goes and this gives one a beautiful target.

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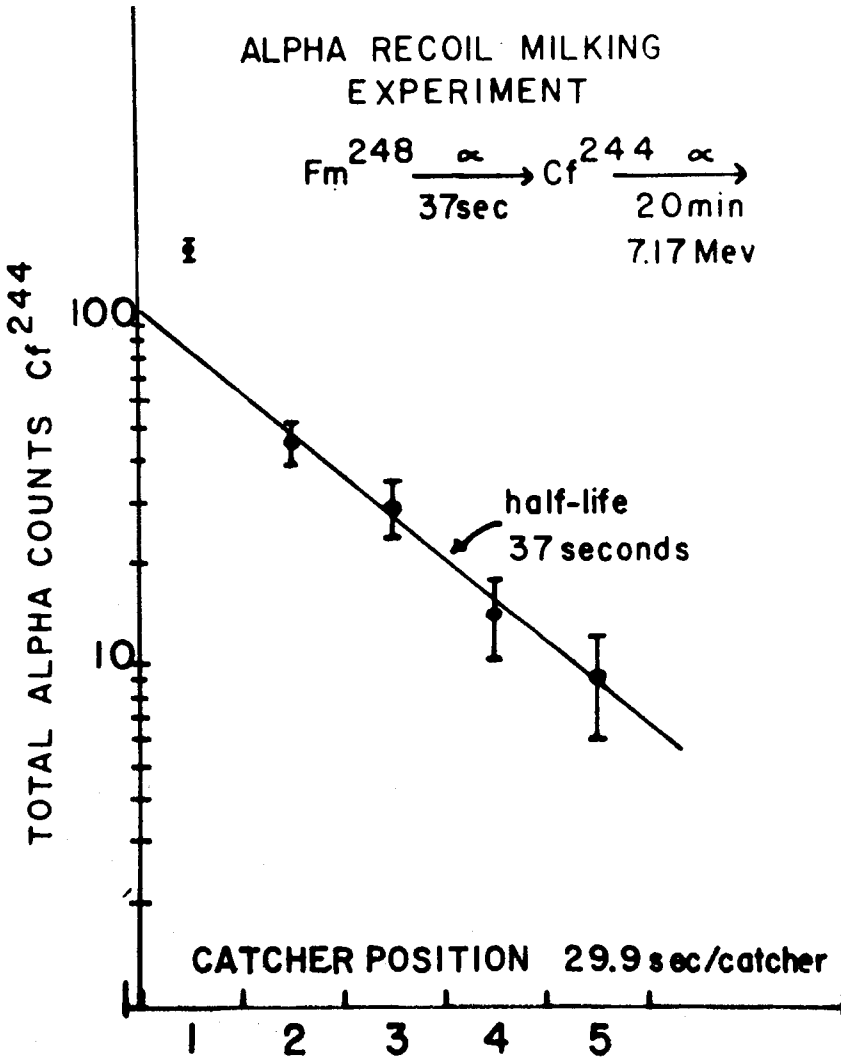

Slide 6

We now had a very nice target with which to do our experiments, and to decrease the chance of contamination we evaporated a small amount of aluminum over the target to keep down any loose material. In addition, later on, we put on an extra aluminum foil, which was completely separate, to absorb any residual knock-overs of curium, and this has been completely successful.

Incidentally, we found that the range of the californium-246 recoils is quite long, which is somewhat surprising. One can compare thin and thick targets by using this activity.

The next thing was to bombard the curium and look for 102. One has troubles as this Slide 7 will illustrate. Some numbers have been crossed out because there are errors in the printing. To obviate this we had two printers, and one tape was corrected from the other printer. These mechanical gadgets simply don't always work properly. We found that this particular printer would go out of whack pretty often. But by being careful and cross-checking in a number of ways one can rule out extraneous effects.

The ones with the check marks are alpha particles due to fermium-250, and the A, B, C, D, and E are the 1 to 5 chambers which analyze successive recoil catchers.

From data like this, Slide 8, but combined with much more, we were able to get a half-life for 102^{254}. It is legitimate to add the experimental data because the runs were all conducted in the same way; you can see from this that we can be reasonably sure of the half life, approximately 3 seconds. We cannot claim any precision yet for the system, since we don't know the systematic errors involved but we don't think they are very large. There is a small possibility of confusion, which we have not yet settled, that the isotope that we are looking at is actually 102^{253}, giving fermium 249. This would be so if fermium 249 had exactly the same characteristics as fermium 250. We don't expect this to be the case, and we have some preliminary evidence to indicate that this is not so. This is probably 102^{254}.

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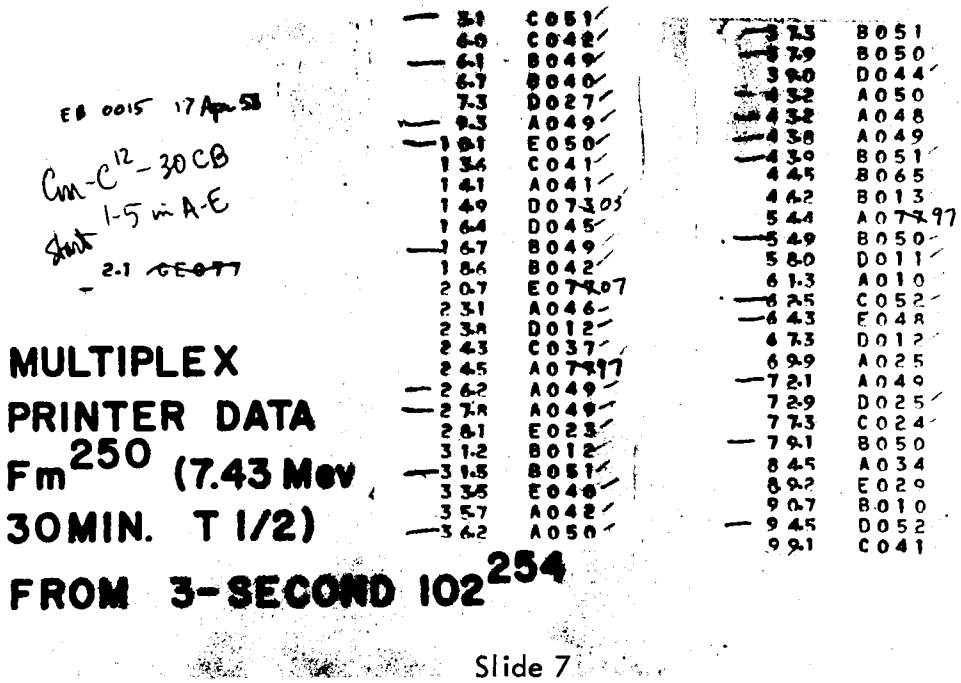

Slide 7

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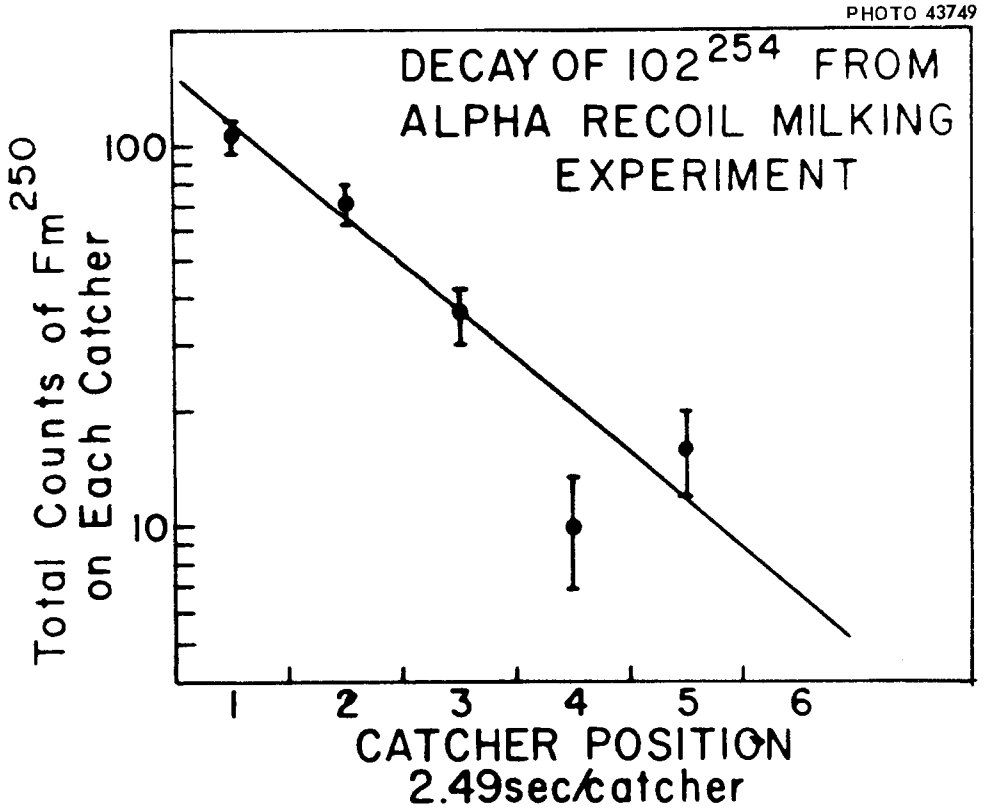

Slide 8

As Dr. Sikkeland mentioned, one can calculate the excitation function for this type of reaction and one gets 3n-, 4n-, and 5n-out cross section curves, Slide 9. I guess I forgot to mention the reaction that we are playing with here. The reaction is $Cm^{246}(C^{12}, 4n)$ 102^{254}. We did one early experiment in which we used carbon 13, using a 5n-out reaction, and we found the same activity. It was only one experiment and has not been repeated yet, but it agrees with the others.

So the question was which curve would fit. The experimental data with the very bad scattered points are the black circles here, and the dotted line is the probable excitation function. The reason the points are low is that we have had some trouble, its cause completely undetermined as yet, which occasionally caused us to lose activity. All points that lost activity, of course, would be low, but there is no way they could be too high. The reason, of course, that the experiment is so sensitive is that the alpha particle decay gives an extremely small recoil range to release the daughter atom from the surface. We find that very small amounts of contaminants in the gas will cover over with a very thin film on these activities and keep them from leaving the belt. This seems to vary and we have not been able to pin it down.

Since we made this slide we have another experimental point which substantially agrees with this (dotted curve). We feel that this is thus a likely reason for the discrepancies. The disagreement in peak energy is probably because we do not have an accurate range energy curve yet for carbon ions. It does not take very much of an error to displace this by a few Mev.

Now, Slide 10, to show that indeed we were dealing with element 100 and thus to prove the atomic number of the 102, it is necessary to show that the daughter product accompanying these alpha particles is fermium. Although they have the same energy and the same half life one could always say, "Well, there is some impurity which causes something which looks about the same." So an experiment was done in which the atoms were dissolved from the second recoil catcher and

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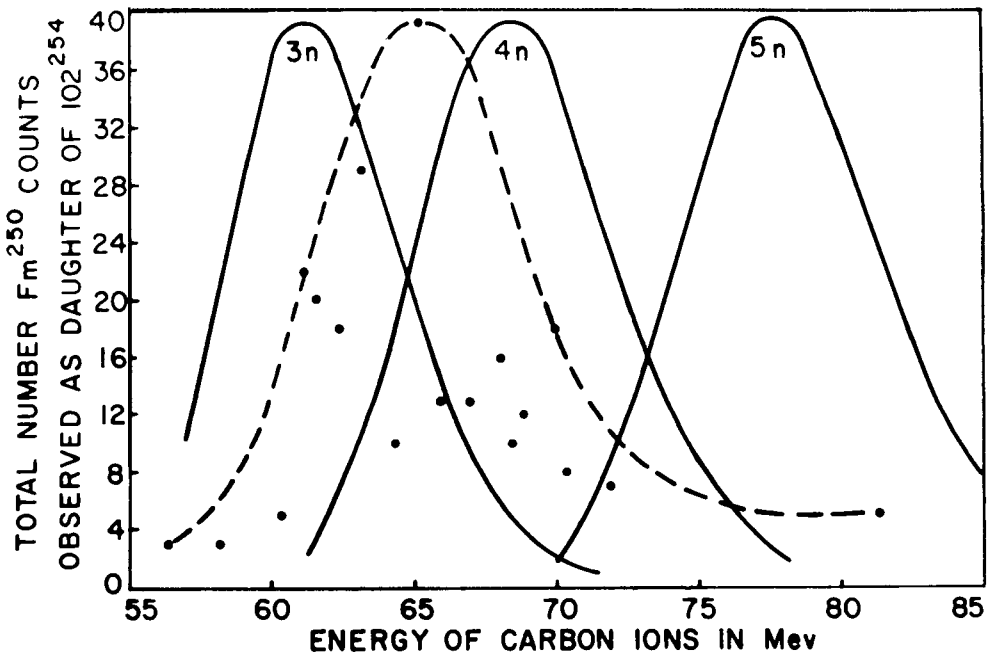

Slide 9

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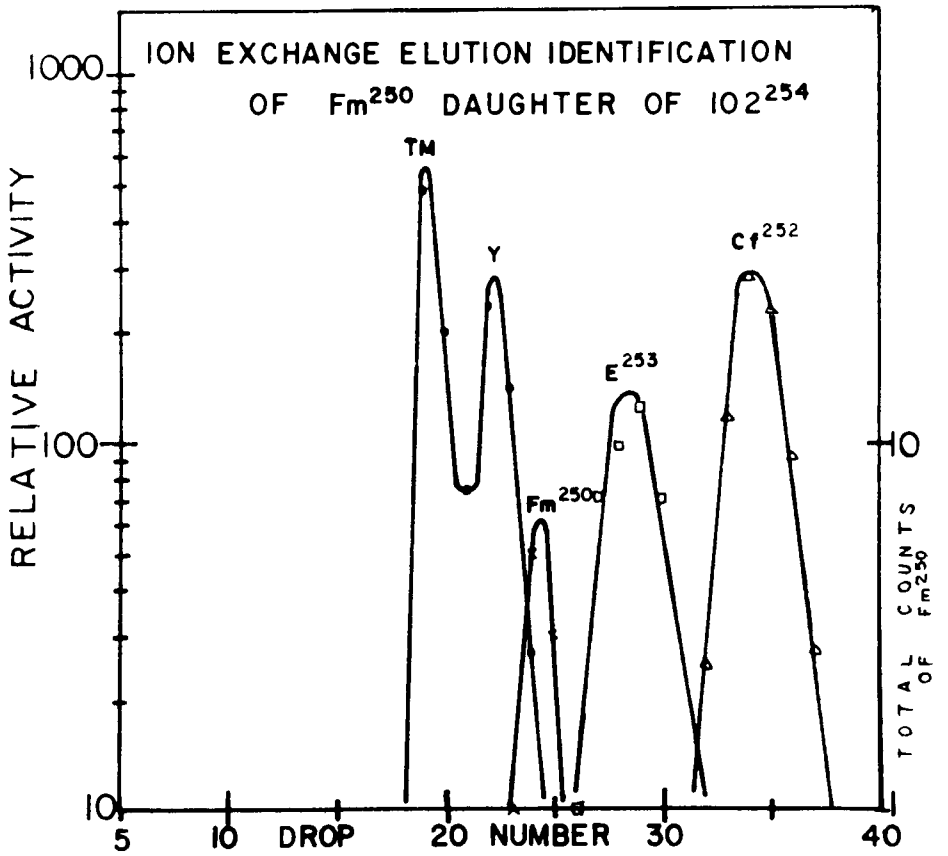

Slide 10

put through a cation exchange column. These tracers (Tm, Y, E, and Cf) were put in to identify the chemical position relative to 100. This is exactly where it should be. It is not a terrribly good separation, but it is good enough to be sure that this is actually fermium; we have one event here, five events here, and three events here.

We feel then that we have proved that we have identified an isotope of element 102. The half life of the isotope is about three seconds. So we went back once more to see if by using these new techniques we could see the activity that was seen at the Nobel Institute last year. Instead of doing chemistry we caught the recoils directly on a piece of aluminum in the same position as the belt. After bombardment we looked directly at this plate for alpha particle activities. Again we saw nothing other than the known isotopes californium-246, californium-245, and fermium-250; we tried a variety of energies, using both C^{12} and C^{13} ions. So we feel that the chance for any long lived 102 isotope of this nature to exist is extremely remote. I mean I would bet heavily against it. Of course, it is quite clear that ultimately there will be a long lived isotope of 102, because in this curium that we have the isotopes 247 and 248 are in very tiny amounts, approximately 0.1% by weight, and one might expect that 102^{257} could have a half life of minutes and 255 conceivably so also, but the alpha energy in any case would be expected to be not 8.5 Mev but more like 8 Mev.

I might mention in relation to this work, the Russian work that was talked about at a Copenhagen seminar early this year bears on this a little. They bombarded Pu^{241} with oxygen ions. Inside the Moscow cyclotron the recoils were caught in vacuum in a catcher plate and it was then moved a short distance away to a nuclear emulsion. With short bombardments they saw activity which conceivably could be due to an isotope of 102; although they were not sure; it could also be due to contamination by lighter element impurities. They did long bombardments, and they saw no long-lived activity at all. This merely would place the mass number of such an isotope as being heavier than 253 from their experiments. From our experiments we think that the 8.5 Mev alpha emitter would surely have been seen. However, we

don't think it is real because the amount of curium we used was essentially five times greater, and the amount of beam we used was essentially five to eight times greater. We feel that in all the dozens of experiments we would be sure to see it.

Well, so much for 102.

I would like to mention one more very preliminary experiment which we did this last Thursday just before we left. In this experiment we bombarded again the same target, but this time we used N^{14} in an attempt to make element 103. We would expect a reaction such as Cm^{246} (N^{14}, 4n) 103^{256} should go and from our systematics of cross section we felt that we certainly must have made element 103. Identifying it is another matter. A nuclear emulsion was placed above the belt, and the belt was run at such a speed as to detect an isotope with a half life of a few tenths of a second. The bombardment was made with quite a large beam, about 300 mua (two bombardments) and the emulsions were scanned; sure enough, we saw a few long range tracks and the energy was of the order of 9 Mev, perhaps a little higher. Our methods of track measuring to determine the range of alpha particles are not very precise yet and I know that we cannot claim a precision greater than 0.5 Mev, but it is indicative that perhaps we have actually seen some tracks due to element 103; however, we would never claim anything from this type of crude experiment.

I just wanted to give you an indication of what one can do. In the future we are going to use ion chambers instead of nuclear emulsions, and by various modifications of this system we feel that we can look at heavier elements than 102 successfully.

I would like to utter a word of appreciation, that, of course, these experiments would not be possible without the wonderful people that we have at Berkeley. Over the course of the last year, led by Ed Hubbard, they have done much hard work to make the machine work properly. It has been really a hard job, but they seem to be on top of the problem now. And just before I left we were very happy that we did what we call an Oak Ridge type of experiment. The Oak Ridge

cyclotron has a fabulous reputation. I went down there once several years ago and their cyclotron beam current was drawing a curve. They had a little recorder. I said, "What does your beam look like?" and there it was drawing a curve that looked like this, just really steady. So every time we get a very steady beam this is called an Oak Ridge curve - very high class.

This wobble in the beam current is due to the wobbling of the target back and forth, ± 5%, and this was for about an hour which was all we asked of the machine. So I think we can claim now that our machine is working beautifully and it will continue to work even better. We have total beams which can be a couple of microamperes. In our work we have all we can handle to take care of all the beam.

I think the boys can be congratulated on a first class job, and for a while it looked like it was going to be kind of hard, but I think they have come through in grand style. Thank you.

CHAIRMAN BOYD: I know that all of us have enjoyed immensely this very interesting talk of yours, this exhibition of marvelous technique, the carefulness of your work, and the very last minute summary of everything you have said. I would like to invite, most cordially from the floor, any comments, questions or remarks.

J. MILSTEAD: First of all I would like to congratulate Dr. Ghiorso and his associates on an extremely elegant piece of work. I don't think this is the time to indulge in a controversy about the relative merits of the two pieces of work. Obviously, various people have very much better facilities than we have. We are, though, very surprised to find that they have not been able to see our activity, and this certainly has prompted us to go back to Stockholm as soon as possible and repeat these experiments in the hope either of proving or disproving what we saw. Meanwhile, I would like to ask everyone to reserve judgement on this particular issue until something more definite is done. Thank you.

GHIORSO: Thank you, John. I agree. I think people should hesitate before drawing any final conclusion. While our first experiments are pretty conclusive, they are certainly our first experiments, and within the next few months we expect to go a good deal farther. We should be able possibly to find quite a few isotopes of 102, and thus be reasonably sure that something peculiar isn't happening. We don't expect so.

I would like to raise one point. Since these experiments are very tenuous one has to look into problems of contamination. For instance, our so-called 103 experiment. We could hardly claim anything for that, because one can, by having small amounts of lead impurities, make the alpha emitter, for instance, astatine-212 which has a half life of a couple of tenths of a second and, although it is predominantly an alpha emitter, it should also decay by electron-capture to Po^{212} which has a half life of 3 μs and almost a 9-Mev alpha energy.

Now similarly, something which we are not sure of yet, francium-215 as yet unidentified could electron capture to Em^{215}. This could have the proper alpha energy and this could have the proper half life, and perhaps this is the isotope that you saw. It would go through the same types of chemistry and it might be produced. We have only made bombardments with C^{12} on lead. We have not tried C^{13}. This is a difficult field. One has to be very cautious about what conclusions one draws.

4. FUTURE SYNTHETIC ELEMENTS BY HEAVY ION BOMBARDMENT

G. T. Seaborg
University of California, Berkeley

I am going to talk, as my title suggests, about the possibility of making future synthetic elements, and they must, of course, be trans-102 elements, using heavy ions or whatever techniques are applicable, and heavy ions seem to be by far the most applicable technique.

In considering how to go about trying to synthesize and identify new elements there are at least three factors that must be taken into account, not necessarily in this order. (1) The chemical properties, with such predictions as we can make. (2) The nuclear properties. It is obvious that one must have some notion as to the half life on the basis of the experiments that Albert Ghiorso has described. It makes a lot of difference whether you are working with a half life of tenths of a second or 10 minutes. (3) Finally, as an absolutely minimum requirement, you must conceive of some nuclear reaction that you can cause to take place for producing the new element. I am going to talk about these three very briefly in that order.

At the present time, or up until this last element, the first production and discovery of an element has been made on the basis of what you might call a complete traditional chemical identification. I am going to talk a little first about the predicted chemistry of these elements that might apply if it were possible to make what you might call a traditional chemical identification.

To begin with, before I do this, I want to say that I think from now on the first isotopes of all the elements to be discovered will probably be produced and sufficiently identified as short lived isotopes with which it won't be possible to make a complete traditional chemical identification. In a number of cases, however, for several more elements it may be that later isotopes, some of which Ghiorso mentioned for 102, will be sufficiently long lived to follow this with a complete traditional chemical identification.

I will then speak first about the method of making these latter type chemical identifications, the type that we have made up through element 101,

and in doing this, therefore, I will be somewhat fanciful because it almost surely will be that in a number of these cases it won't be possible to make this type of identification because I am going to range up rather high in atomic numbers.

I will start first with a picture of the periodic table. (Slide 1) It is sort of basic to what I am going to say. As you know, the heavy elements which we refer to as the actinide series, a heavy rare earth series like the lighter or lanthanide series, start back with actinium and thorium, elements 89 and 90; we have an element-by-element analogy as we go across the two series. This has been the basis on which the chemical properties have been predicted for heavy elements from element 94 on; hence the discoveries and traditional chemical identification have been aided, and, in the cases from element 96 on, the predictions have been quite precise. This has made it possible to outline the experiment in some detail before the discovery, and then to carry through with more or less standard apparatus using the ion exchange absorption-elution technique, the experiment that could be used for the identification.

In this prediction of chemical properties, then, one feels safe in assuming that element 102, when its whole elution curve is traced out, will find a position on this more or less well known scheme analogous to terbium, and element 103, a position analogous to lutetium; namely, these will be the first two of the actinide elements to elute in this sequence. We will show later where we think these will elute. Then we come to elements 104, 105, 106, 107, and 108; this is just an arbitrary breakoff point. One can fill in more numbers here.

It just seemed to me to make little sense, at least now in 1958, to write a number bigger than 108 for a discussion like this. If you count across here, of course, you come to 118 for your next rare gas. Thus we can predict the chemical properties by using the most elementary principles of the periodic table. We would predict that 104, which falls into the same vertical column here as titanium, zirconium, and hafnium would be like those elements; and element 105, which falls below vanadium, niobium, and tantalum, would be like tantalum; 106 would be like the elements above it, particularly like molybdenum and tungsten; 107 like rhenium; and 108 like osmium, etc.

An interesting aside here is that element 107 will be the first transuranium element that will have chemical properties like those which were

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1 H 1.0080																	2 He 4.003
3 Li 6.940	4 Be 9.013											5 B 10.82	6 C 12.011	7 N 14.008	8 O 16.000	9 F 19.00	10 Ne 20.183
11 Na 22.991	12 Mg 24.32											13 Al 26.98	14 Si 28.09	15 P 30.975	16 S 32.066	17 Cl 35.457	18 A 39.944
19 K 39.100	20 Ca 40.08	21 Sc 44.96	22 Ti 47.90	23 V 50.95	24 Cr 52.01	25 Mn 54.94	26 Fe 55.85	27 Co 58.94	28 Ni 58.71	29 Cu 63.54	30 Zn 65.38	31 Ga 69.72	32 Ge 72.60	33 As 74.91	34 Se 78.96	35 Br 79.916	36 Kr 83.80
37 Rb 85.48	38 Sr 87.63	39 Y 88.92	40 Zr 91.22	41 Nb 92.91	42 Mo 95.95	43 Tc	44 Ru 101.1	45 Rh 102.91	46 Pd 106.4	47 Ag 107.880	48 Cd 112.41	49 In 114.82	50 Sn 118.70	51 Sb 121.76	52 Te 127.61	53 I 126.91	54 Xe 131.30
55 Cs 132.91	56 Ba 137.36	57 - 71 La Series	72 Hf 178.50	73 Ta 180.95	74 W 183.86	75 Re 186.22	76 Os 190.2	77 Ir 192.2	78 Pt 195.09	79 Au 197.0	80 Hg 200.61	81 Tl 204.39	82 Pb 207.21	83 Bi 208.99	84 Po	85 At	86 Rn
87 Fr	88 Ra 226.03	89-103 Ac Series	(104)	(105)	(106)	(107)	(108)										

Lanthanide Series	57 La 138.92	58 Ce 140.13	59 Pr 140.92	60 Nd 144.27	61 Pm	62 Sm 150.35	63 Eu 152.0	64 Gd 157.26	65 Tb 158.93	66 Dy 162.51	67 Ho 164.94	68 Er 167.27	69 Tm 168.94	70 Yb 173.04	71 Lu 174.99
Actinide Series	89 Ac 227.04	90 Th 232.05	91 Pa 231.05	92 U 238.04	93 Np 237.06	94 Pu	95 Am	96 Cm	97 Bk	98 Cf	99 E	100 Fm	101 Mv	102	(103)

Slide 1

assumed back in the early 1930's when it was thought that transuranium elements had been found and when the carrier used was rhenium, for example, and they were precipitated with H_2S , from acid solution, etc. Thus 107 and the following elements will begin to have just those chemical properties that were predicted at this time, before it was understood.

I might say that although we can make these predictions for the chemical properties for these elements they do not give us the exact chemical procedures, as has been the case for all the elements from 96 on due to the peculiar nature of the heavy rare earths actinides and the special features of the ion exchange absorption-elution technique. We predict something about their oxidation states, their ionic size, etc., but one can't predict properties that will give us exact chemical procedures. Element 104, however, will surely be almost exclusively or essentially exclusively tetrapositive in aqueous solution; instead of going across a series we use a vertical column in the periodic table in order to make our predictions.

To get some kind of a sequence with zirconium, hafnium, and element 104 we may again rely on an ion exchange procedure, and on a later slide, as a matter of fact, I become quite fanciful and suggest an ion exchange procedure which I am sure will not be the one that is used. For element 105 we might use the same kind of sequence. Element 105 should have the +5 oxidation state almost exclusively. It should have very troublesome chemical properties. It should hydrolyze easily and should be difficult to keep in solution and probably require the presence of fluoride ion to form complex ions to keep it in soluble form.

Element 106, probably like tungsten, will have oxidation states of 3, 4, 5, and 6. In fact, I have even tried to estimate what those oxidation potentials would be on the basis of straight forward extrapolation from the molybdenum and tungsten potentials.

Element 107 should have the long range of oxidation states characteristic of rhenium. We should find the permanganate-type ions, for example.

Element 108 should have oxidation states somewhat like osmium and in particular should have the volatile compound MO_4 which might be useful in its identification, etc.

Slide 2 shows the elution predictions for elements 102 and 103 in a system utilizing Dowex-50 and alpha-hydroxyisobutyrate as the eluting

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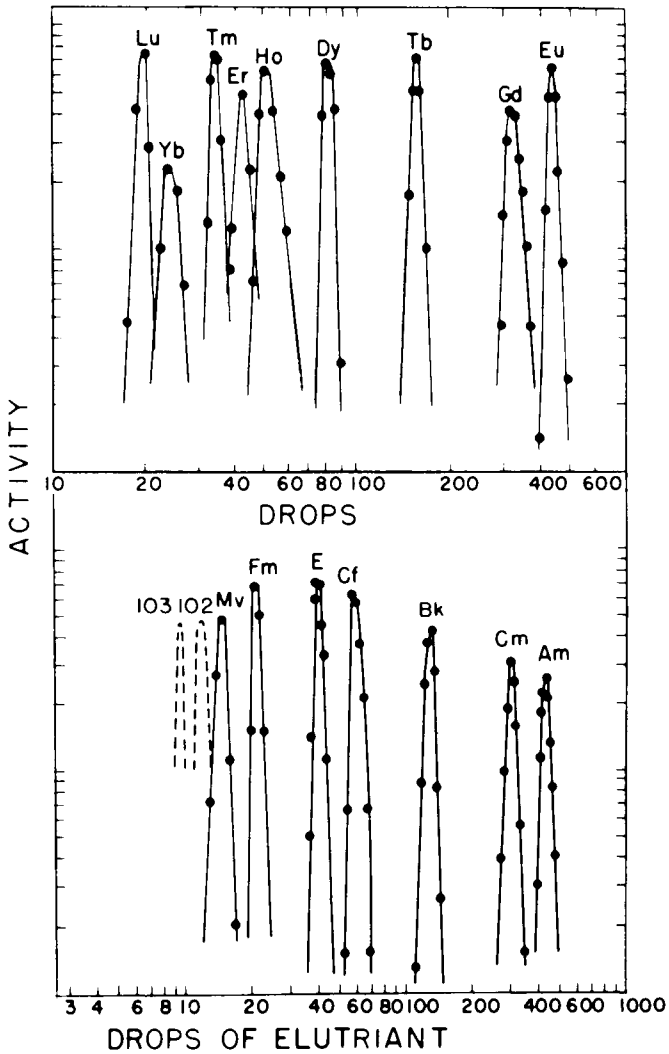

Slide 2

agent at elevated temperature. The comparison with the rare earths is on a log-log plot to give you the extra sensitivity.

Slide 3 is shown because it may not be clear, on a casual look, what the ionic size of some of these elements might be. For example, due to the actinide contraction, element 104 is expected to be a relatively small ion. It will differ from thorium in ways that are characterized by this difference in size. It won't be as electropositive.

The sequence of radii here would suggest that the zirconium, hafnium, and 104 will be of nearly equal size; so that if you are going to try to predict an elution order in any experiment you probably will just assume that they are in sequence, the same sequence as zirconium and hafnium.

Similarly, Slide 3 shows the ionic radii predicted for some tripositive and tetrapositive ions. Element 103, which should probably be the least electropositive of the actinides, should form the strongest complex ions, etc.

I won't have time to dwell on this, and I want to go to the next slide (Slide 4), a fanciful one which includes zirconium and hafnium with Dowex-50 and elution with 6M HCl and traces a position for element 104. I doubt that element 104 will have an isotope long enough lived to do an experiment like this.

Next, Slide 5 shows the same thing for hydrofluoric acid together with niobium and tantalum. You might change the concentration of your fluoride ion in order to decrease the interval between tantalum and 105.

That is all I want to say about the chemical properties.

When it comes to the nuclear properties we have a number of ways of predicting them, as you know. Probably of chief concern to us are the energies and the rates of decay for alpha emission and also the rates of decay for spontaneous fission. Spontaneous fission becomes important in this region. The half life for the beta electron capture processes are also amenable to some prediction, but these are not usually the rate determining steps. If we can come to grips with the problem of estimating the rate of alpha emission or spontaneous fission, we have the problem pretty well in hand.

Slide 6 shows the familiar plot of alpha disintegration energy against the mass number for these various transuranium isotopes. These alpha energies fall on lines of this sort for each of the elements. This is not black magic. This is a simple consequence of just a consideration of the

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Ionic Radii of Some Tetrapositive and Tripositive Elements

Ti	0.60 Å	Sc	0.68 Å
Zr	0.77	Y	0.90
Ce	0.92	La	1.06
Tb	0.84	Gd	0.94
Hf	0.77	Lu	0.85
Th	0.99	Ac	1.11
104	0.77	Cm	0.98
		103	0.88

Slide 3

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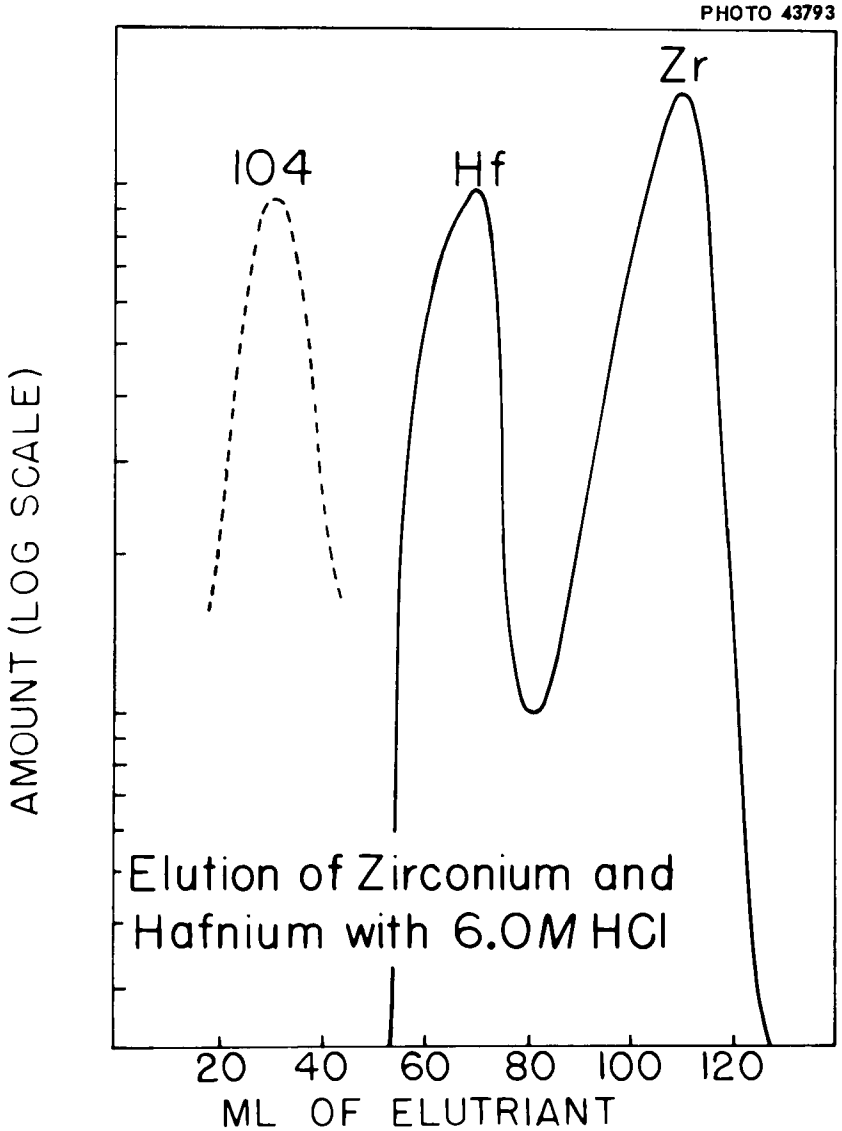

Slide 4

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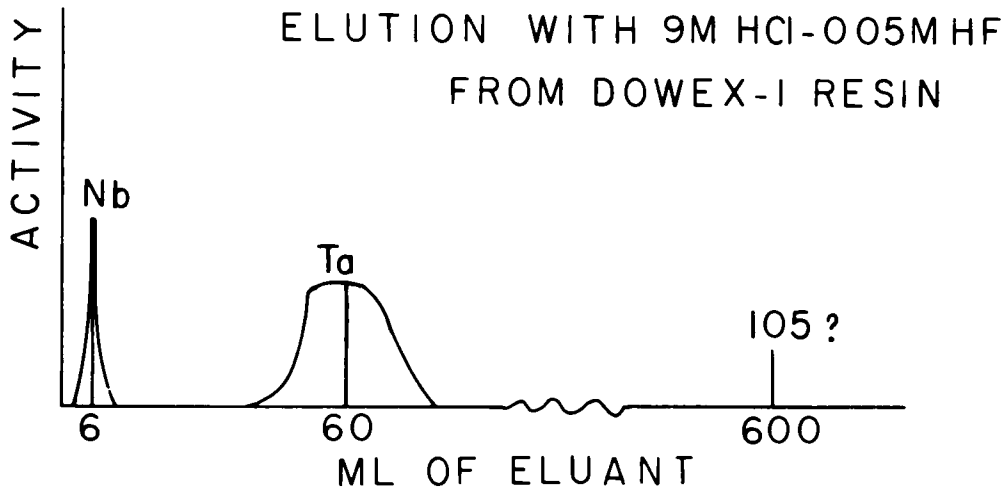

Slide 5

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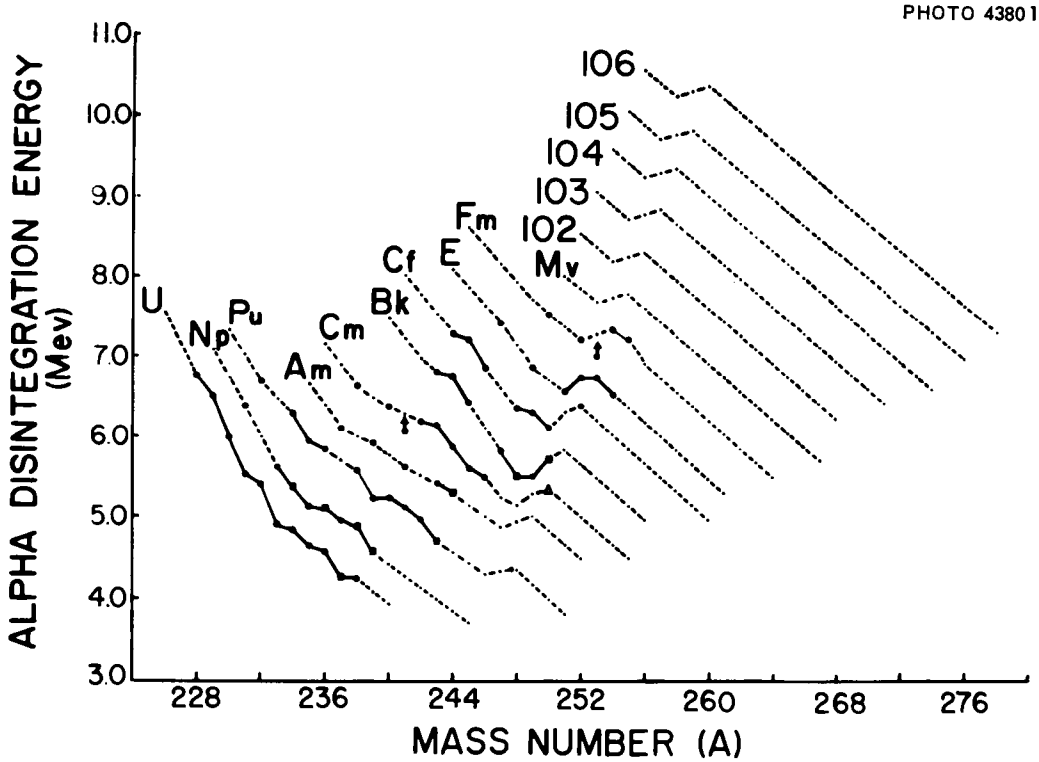

Slide 6

mass surface. Analogous points on the mass surface are at points such that when ΔZ equals $2\Delta A$ is greater than 4, say, 5 or 6, whereas when the alpha particle is emitted, of course, it changes the mass by 4 when it changes the atomic number by 2. It is just a consequence of that simple fact.

There is an interesting little quirk here in the alpha energy corresponding to nuclides with 152 neutrons. This is very real. It is a small effect, a few tenths of an Mev as compared with a few Mev for the similar breaks in the curves down in the region of 126 neutrons, for example. There is some kind of a minor stability at 152. The great difficulty with predicting these alpha energies then is to make allowance for this as one goes up and, as a matter of fact, the new isotope of element 102 with mass number 254 is above the curve, indicating, not to our surprise at all, that this effect at 152 neutrons is beginning to disappear.

Slide 7 shows how one can estimate the half life from the estimated alpha particle energy by using a graphical approach. Here, of course, as on the previous slide, which I neglected to mention, the dotted lines are the projected lines for the future elements. Here we have these projected lines. These considerations involving the relation of the half life to the energy correspond to even-even alpha emitters.

Very fortunately, when we have an odd nucleon, an odd neutron, or an odd proton, or both, the rates of decay are slower, and one will probably be using this effect particularly in making any real chemical identifications. We shall be trying to produce nuclides with an odd number of nucleons.

Slide 8 shows one of the many ways of plotting the spontaneous fission half life. Roughly speaking the half life decreases at a rather high rate with increasing Z. We plotted here the log of the half life against Z^2/A. An interesting effect is that the half life is a maximum for the nucleus with 152 neutrons, and when the nucleus has more than 152 neutrons the half life goes down and the rate goes up. This is particularly well known for the californium and fermium isotopes.

Also in the region where we don't have nuclides with 152 neutrons you see that the half life goes through a maximum and the rate of spontaneous fission goes through a minimum in the region of beta stability.

So how do we estimate spontaneous fission half lives? Do we continue to give credence to this effect here of 152 neutrons? We certainly allow

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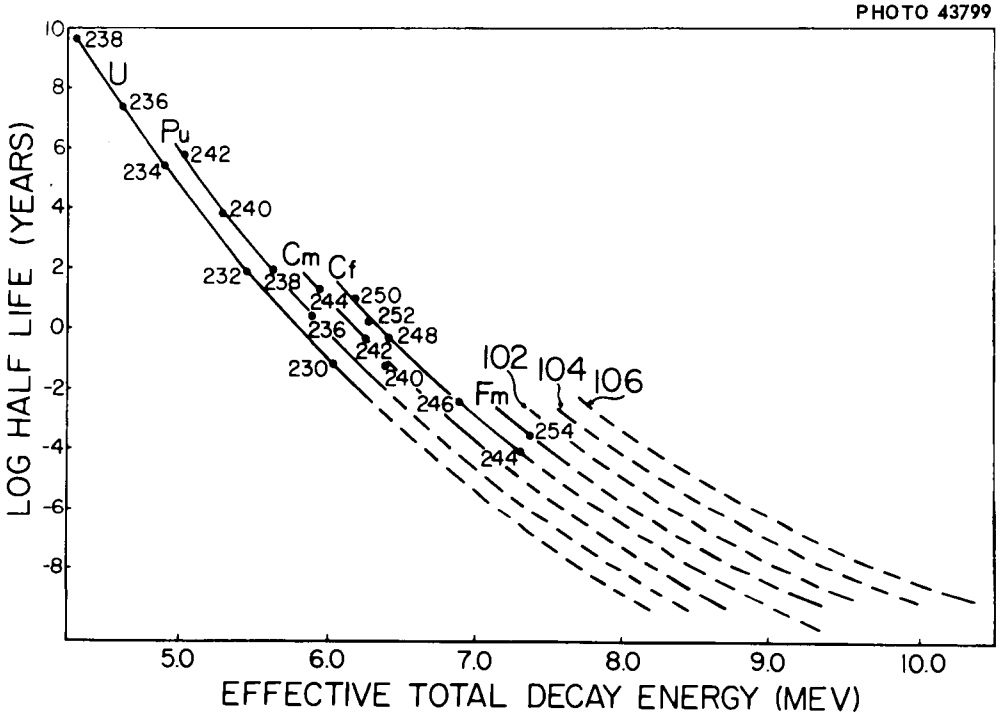
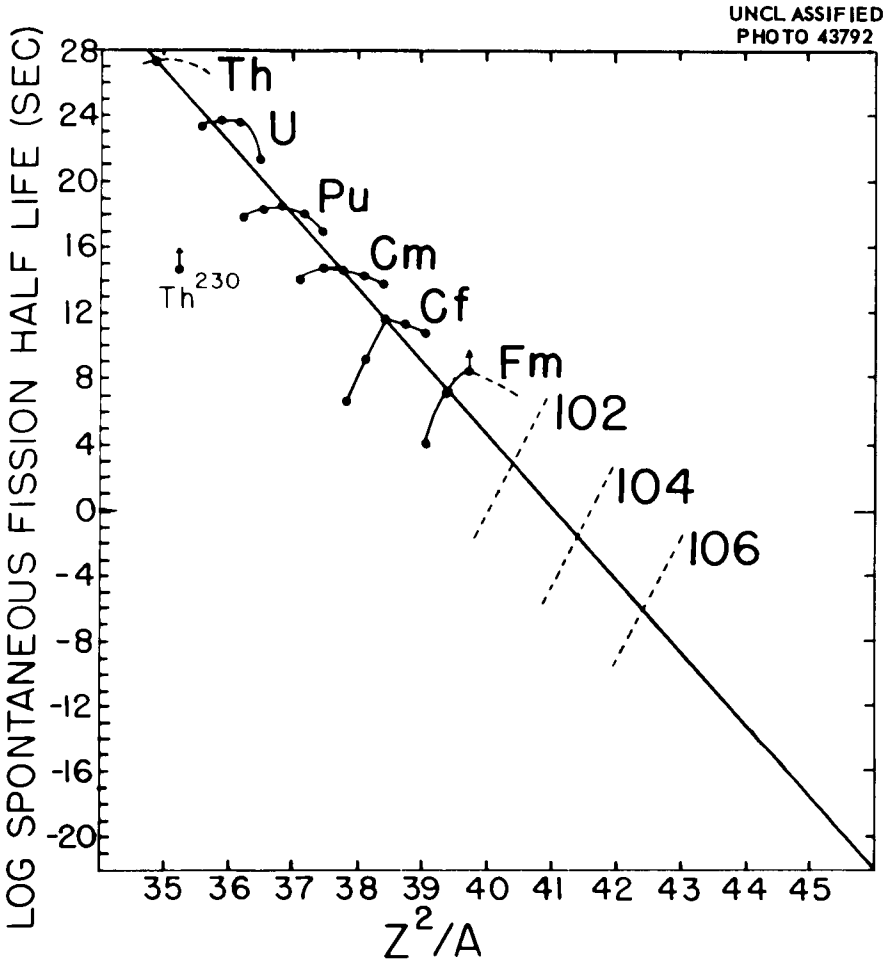


Slide 7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LOG SPONTANEOUS FISSION HALF LIFE (SEC)
28
24
20
16
12
8
4
0
-4
-8
-12
-16
-20
Th
U
Pu
Cm
Cf
Fm
Th230
102
104
106
35 36 37 38 39 40 41 42 43 44 45
Z2/A

Slide 8

for the fact that in any case the maximum in the half life is in the region of beta stability.

On slide 9 I have plotted some of the half lives for the rate determining step here. These are just the total half lives, whether decay occurs by alpha emission or spontaneous emission, for a number of isotopes to show you how short they get even with high mass.

A predicted region of beta-stable isotopes for these heavy elements is shown in Slide 10. These squares with cross hatches indicate the beta-stable isotopes. This is just to give you a sort of orientation. These are the isotopes that are analogous to the stable isotopes lower in the periodic table. Now if we are going to try to decide which is the most stable isotope with the longest life, of course, we will pick on one that has an odd mass for the even Z. We will know immediately that the beta-stable alpha emitter with the longest alpha half life and the slowest rate of alpha emission is the heaviest one.

Which nuclides have the slowest rate for decay by spontaneous fission? If the effect of 152 neutrons persists, nuclides with this number of neutrons should have the slowest rates for spontaneous fission decay; however, the effect of 152 neutrons seems to be disappearing. The longest lived nuclide should be somewhere in between the heaviest beta stable one and the one with 152 neutrons, when we take into account both alpha emission and spontaneous fission. If you go up to too high a mass number the spontaneous fission rate increases due to another effect, probably due to a decrease in the nuclear surface tension.

Now we come to the methods of making these isotopes. Up until now, if you go through the history of the discovery of transuranium elements, you will find that they were produced either by neutron bombardment, just building the mass high enough to get an imbalance in the neutron-to-proton ratio leading to negative beta particle emission, or by charged particle bombardment, such as with helium ions or deuterons. It looks as if neither of these is going to be very useful from here on. Obviously deuterons, helium ions, etc. demand target nuclides that are almost up where you want to go. So that is not possible.

Neutron bombardment runs into trouble chiefly with spontaneous fission. Let me show you Slide 11; this is the way one gets up to these heavy nuclides. This represents the results of neutron irradiation at high fluxes. The atomic

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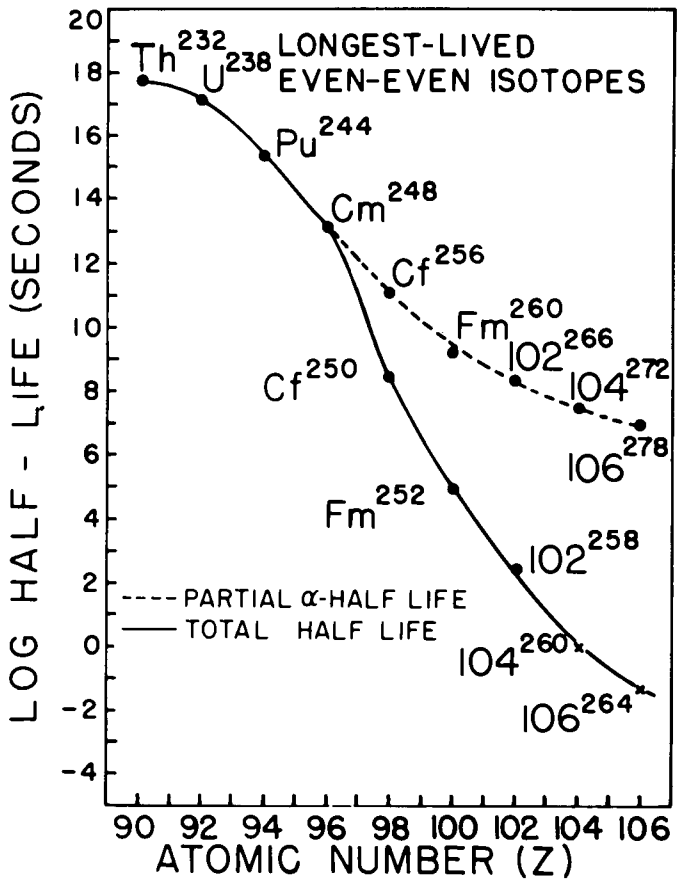

Slide 9

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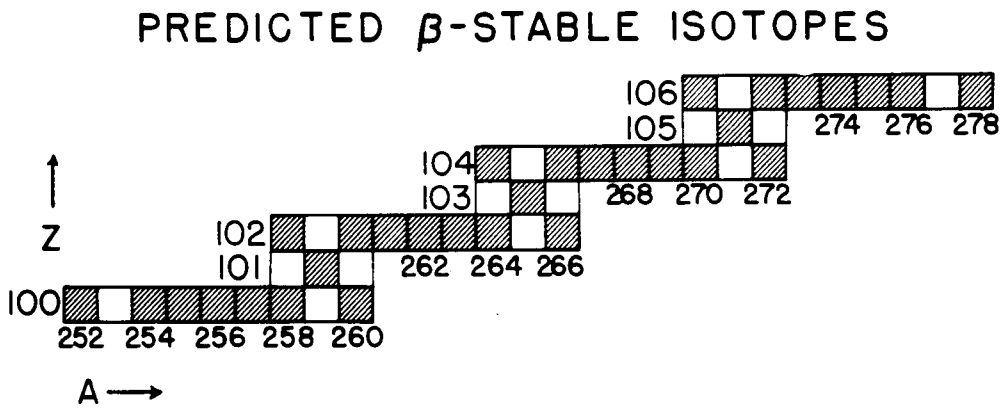

Slide 10

number increases as the mass number increases when neutrons are captured in obvious ways over a period of time. With such techniques it is possible to build up substantial amounts of berkelium and californium. We have, in fact, even weighable amounts at the present time at Berkeley and, as you may have heard, Cunningham and Thompson isolated berkelium in weighable amounts just last month. This then is the chief way of getting these isotopes.

Slide 12 shows how one might predict that you go from here, but such a prediction would simply ignore an important difficulty. That is because some of these intermediate isotopes decay quickly by spontaneous fission; for example, fermium 256 has a half life of 3 hours for decay by spontaneous fission. Fermium 258 has a predicted half life of maybe 1 minute for decay by spontaneous fission. It gets worse higher up, so that this method of building up 102, 103, 104, etc., does not seem to offer much hope. The method that was used for the accidental discovery of elements 99 and 100, einsteinium and fermium, namely, the capture of large numbers of fast neutrons--and in that case uranium was used in connection with a thermonuclear device--still holds out hope if such a device can be placed at the disposal of the experimenter. Then you bypass a lot of this trouble because you go up high in A at a point where Z is small. Then you can beta decay across and pick many paths across actually, because you capture different numbers of neutrons all the way up. If you select a path that does not lose your nuclei by spontaneous fission or alpha decay you might reach the heavy element that you desire. So in a well conceived experiment of this type one could possibly produce isotopes of higher Z.

However, the method that is at our disposal is the method that is being discussed here, the main topic for this conference, and that is bombardment with heavy ions. Now I don't want to take much time to try to write different reactions here, because they are being written right along. It is obvious that what you do here is, in a sense, jump over the trouble, put your charge, usually together with not as much mass as you want, into the nucleus in one step. In this case, unfortunately most of the reaction goes into fission. The overwhelming majority of the reaction goes into fission, and the more of it goes into fission the heavier the nucleus.

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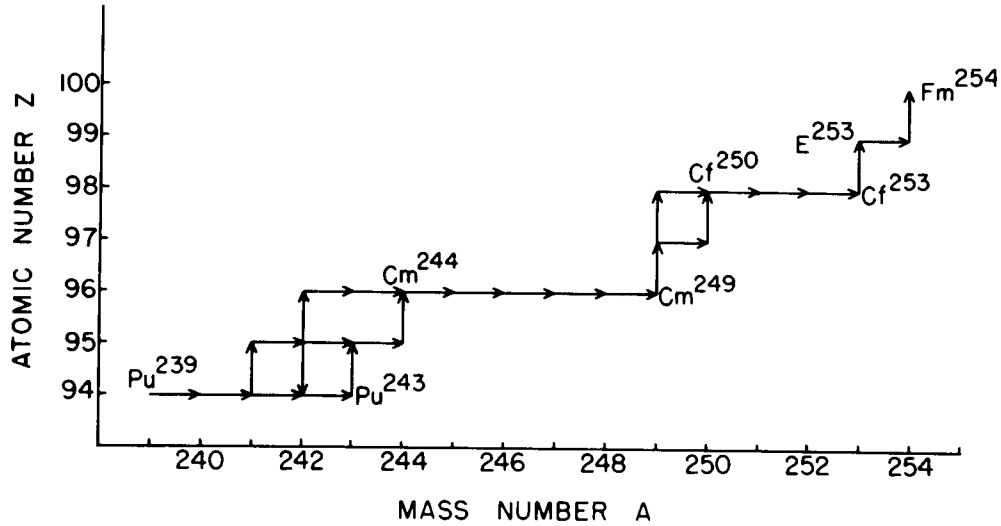
HEAVY ISOTOPES VIA NEUTRON IRRADIATION



Slide 11

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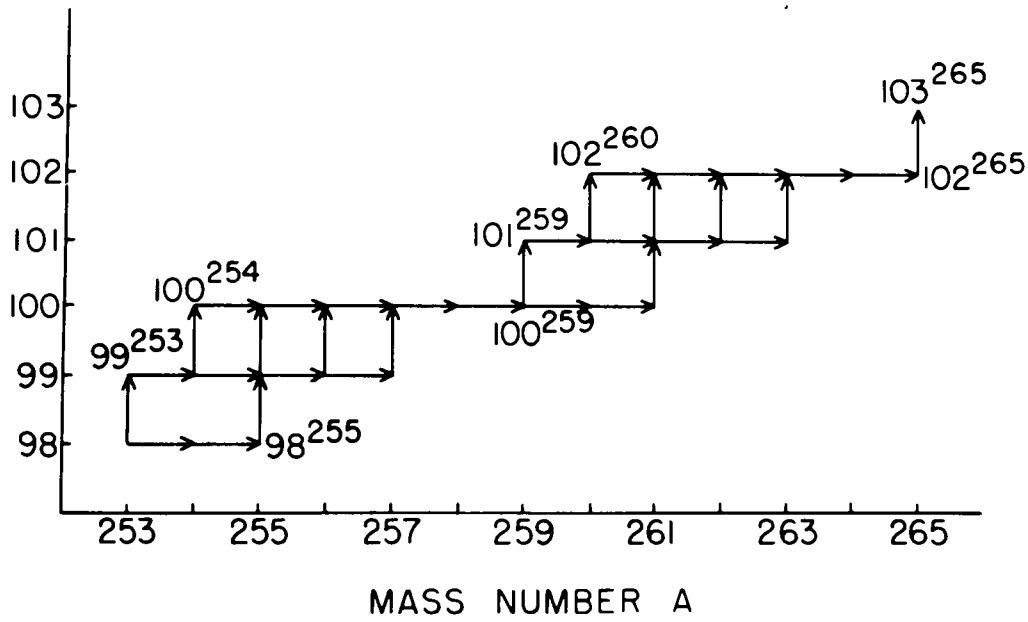
HEAVY ISOTOPES VIA NEUTRON IRRADIATION



Slide 12

It looks as if the best way of minimizing fission is to bombard the heaviest nuclide you have with the lightest particle that gets you there according to simple arithmetic. The reason for this is just that the lighter particle can slide into the nucleus through or over the potential barrier at a lower energy, and hence in this cascade competition between neutron emission and fission, which I am sure John Huizenga told you about in the talk that I missed, you get less loss due to fission. You see, as you get way up to element 102 or something like that, each time you undergo this competition with fission, in order to get rid of your neutrons so that you can settle down to where the nucleus can emit gamma rays and you get down to where you want to be. Each time you do that for these very heaviest elements you lose maybe a factor of 5 or 10; that is, 5 or 10 times more of these heavy nuclei undergo the fission reaction than emit a neutron at each stage. You cannot stand very many of these steps and still have enough atoms left to detect, even though the sensitivity is down to the point where it is possible to detect of the order of 1 atom per experiment.

Slide 13 shows how the problem of building up these isotopes as starting materials is a very difficult one. Because as one goes higher and higher, due to the effect I have just described and also because you need to get more neutrons into the nucleus, you want heavier and heavier materials to start with as target material. Your approach here is either irradiation in a high flux nuclear reactor or irradiation in a thermonuclear device where you recover the products. In reactors of the type that we now have available, namely fluxes of a few times 10^{14}, the curium 244 and hence the isotopes further on, californium, etc., build up at a very discouragingly slow rate. What is needed here is the construction of a reactor with a flux much higher than this, let's say, 10 times higher than this. Due to the higher order of neutron capture one gets there much faster, let's say in our lifetime or something of that order.

It then is an important problem to get these starting materials, even though we are using heavy ions, because we have to get up as close to the boundary as we can for the reasons I have indicated. I am talking about the eventual production of elements as heavy as I mentioned earlier, 106, 107, 108, etc.

I was going to show the Hilac, Slides 14 and 15, but I am sure that someone else is going to show some pictures. I will just flash them on quickly and then go on to my concluding remarks.

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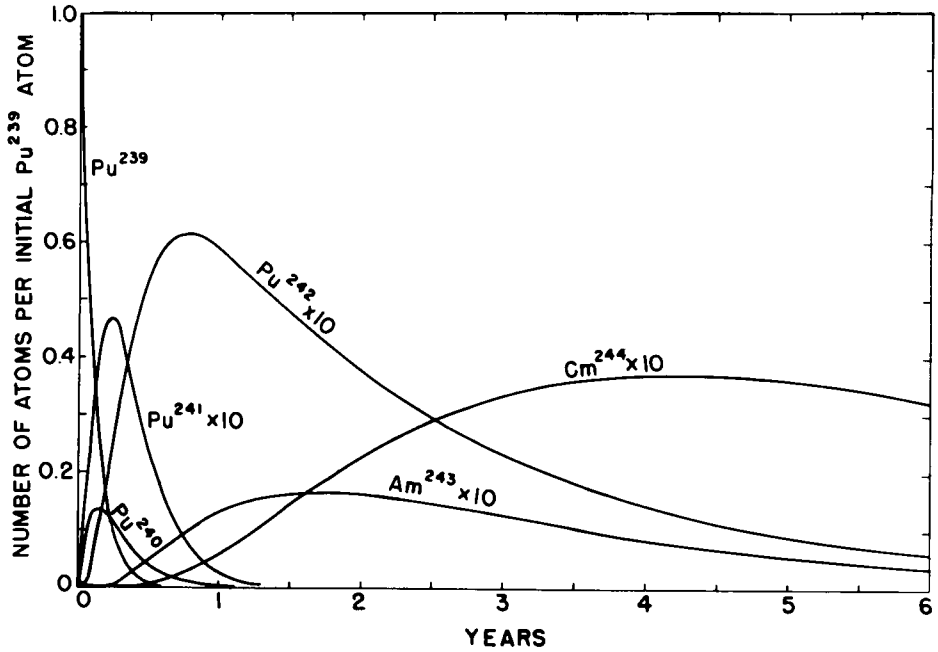

Slide 13

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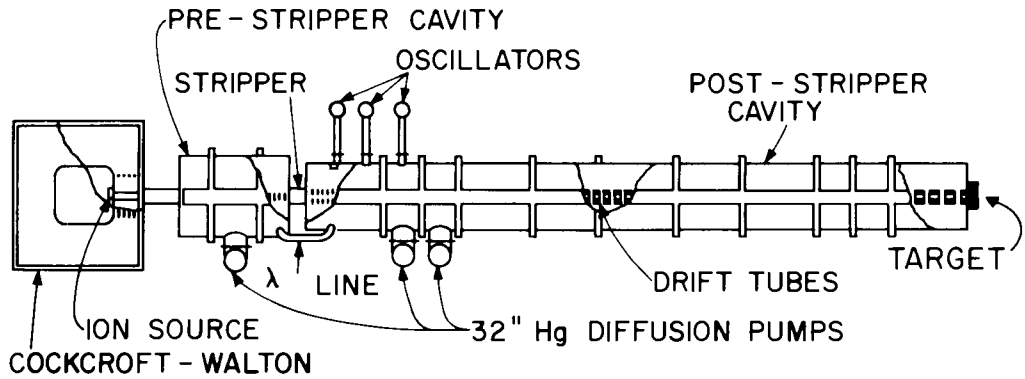

Slide 14

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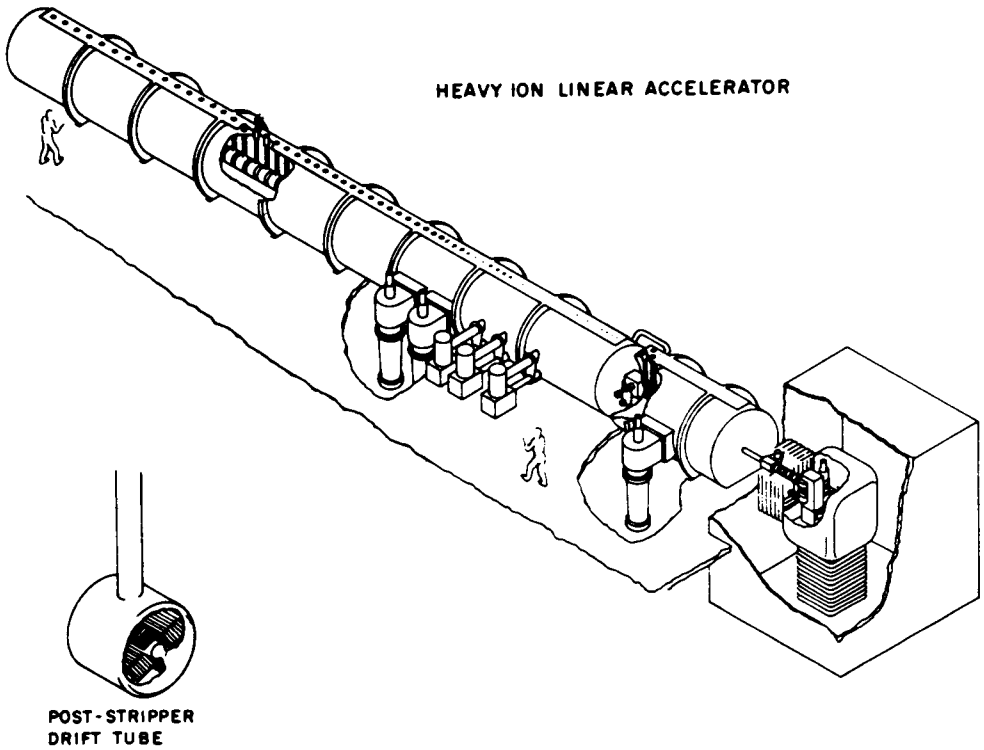


Slide 15

I wanted you to see Slide 16 because it is a picture of a couple of years ago; it shows bringing the tank of the Hilac up the hill, around the turn on Hearst Avenue, the north entrance to the Radiation Laboratory, a tank which weighs about 50 tons, some 9 ft. in diameter. In this case, you see it was tied to this little tree up here at this point to get additional support as it went around the bend. (Laughter) I don't know whether Hubbard and Ghiorso are here at this time, but I am sure they are standing around somewhere anxiously watching the operation.

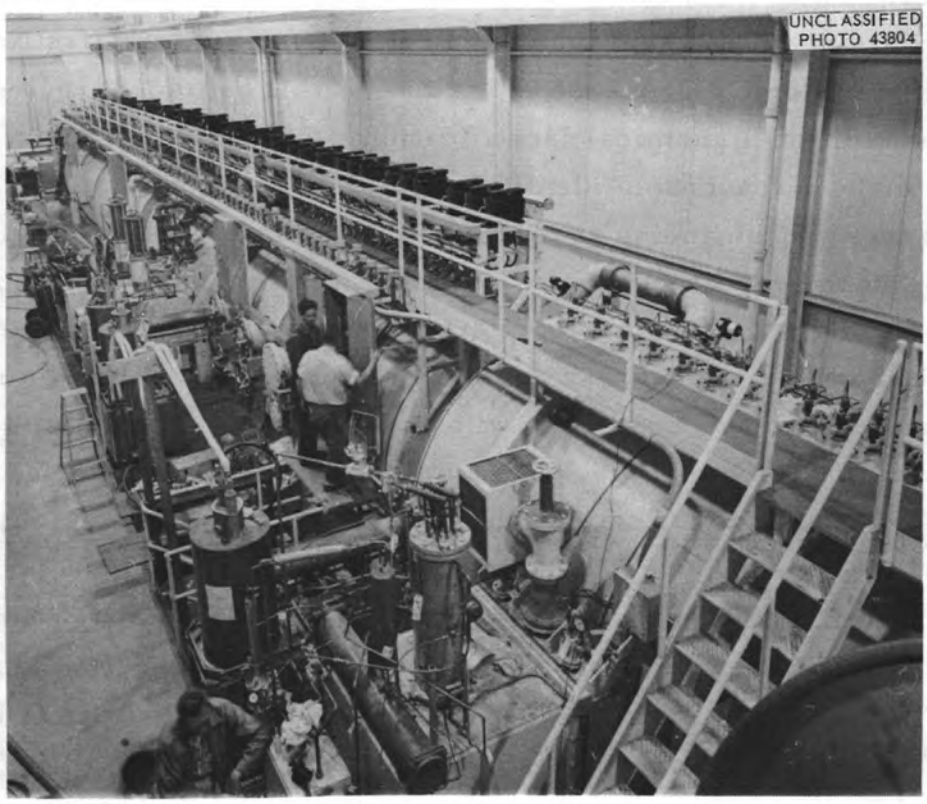
Slide 17 shows the machine about a year ago, just when it was buttoned up and began to produce its first beam.

Slide 18 shows the control desk, and Slide 19, the interior of the tank with the 68 drift tubes.

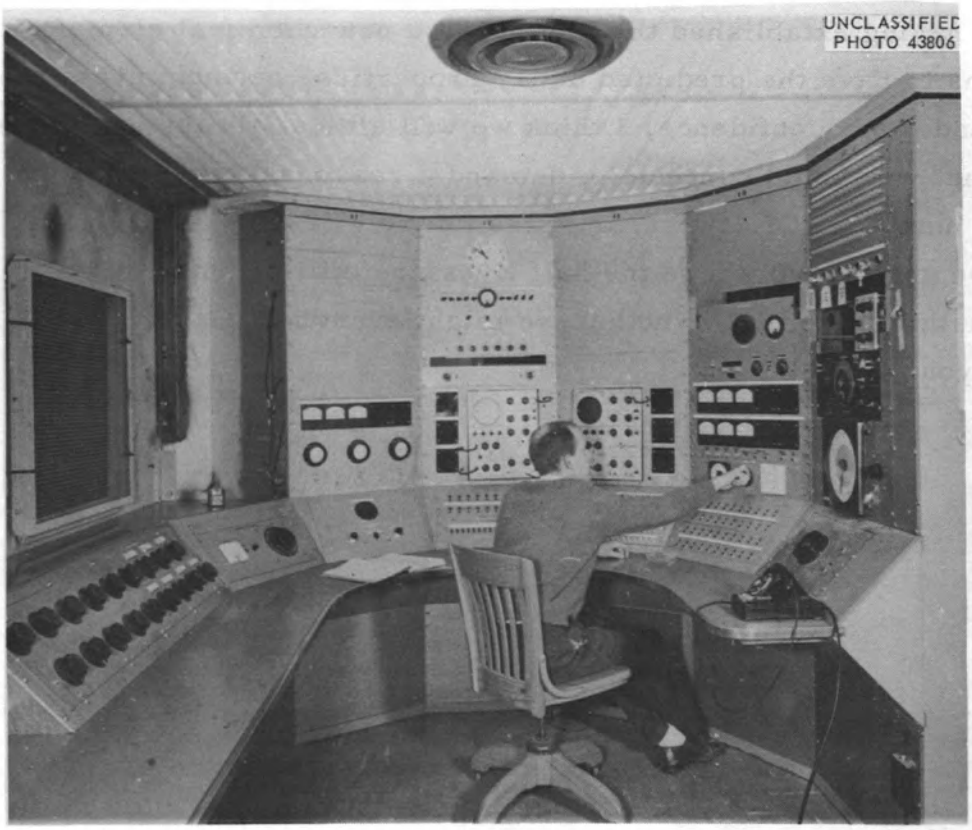
That is the last slide that I wanted to show. I want, of course, to reiterate what Albert Ghiorso said, that is, how indebted we are to the people, Hubbard, Van Atta, Beringer and his group from Yale, etc. for the construction of this machine, which, of course, obviously is going to be the key to any future work we do in this field.

I do want to make a few statements about how we might go on from here, since I talked about traditional chemical identification and other types of identification, etc. I said that I thought from now on the first isotopes of the elements to be discovered in the future will be too short lived in general for the traditional chemical identification. This does not mean that we cannot make a reasonable identification of the atomic number; that is, beyond that of just finding agreement with the predicted decay properties. The use of the recoil technique with identification somewhere in the chain tied in intimately, the complete determination of excitation functions as we learn just what they look like and how well they characterize a nuclear reaction with heavy ions, etc., will probably make it possible to get enough evidence to satisfy anybody. It may also be possible, as we go higher and higher, to make identifications that are chemical identifications of a sort; that is, the rate of migration of gaseous atoms or ions or volatility experiments under conditions that are controlled and understood, or reactions with surfaces of different kinds or maybe even very fast reaction kinetics. It is possible to carry out reaction kinetics in a very small fractional-second time scale. One here, of course, is probably thinking of far in the future.

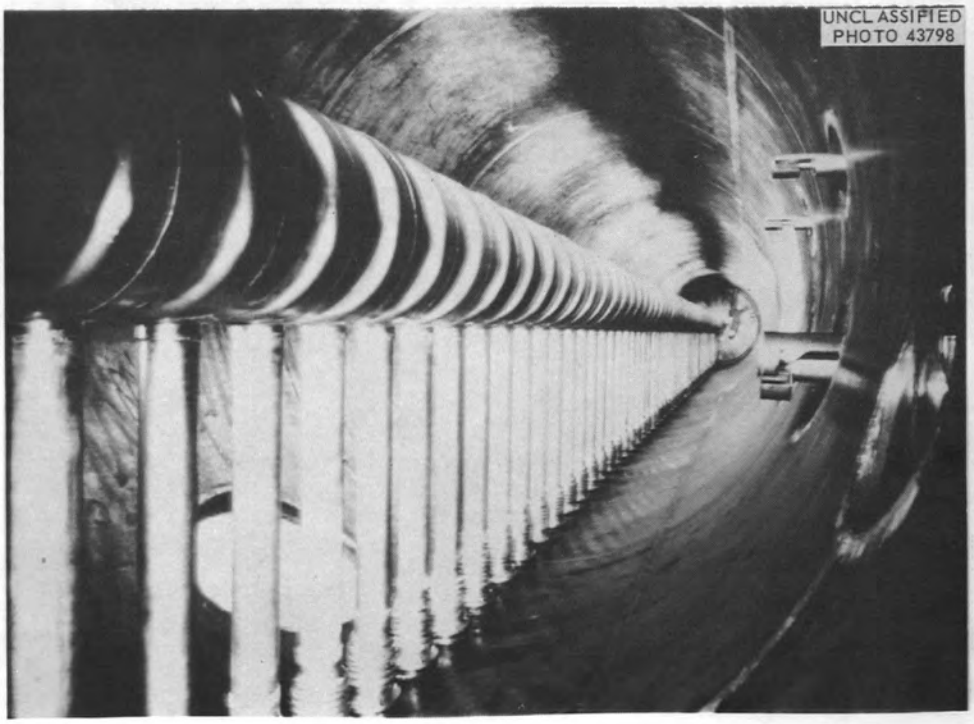
Slide 16



Slide 17



Slide 18



Slide 19

However, I don't think that we should settle for less than a reasonable establishment of the atomic number; that is, it would be very dangerous to regard having extablished the identity of a new chemical element just because it seems to have the predicted decay properties, because even though we gain more and more confidence, I think we will almost always have pitfalls. So I think that we should continue to demand a reasonable identification of the atomic number. Of course, only the future will tell, many years from now when we get way up where the half lives are milliseconds or microseconds or something like that, whether we might eventually have to settle for less. Thank you.

5. STATISTICAL ASPECTS OF COMPOUND NUCLEUS DECAY

G. R. Satchler
Oxford University, England

I was asked to talk about the compound nucleus. The first thing to do is to decide what we mean by compound nucleus. In general, the compound nucleus is not a very well defined concept. Only at low energy, where we see distinct separate resonances in our cross sections, does it have a clear-cut meaning. Then, of course, it means a quasi-stationary state of the system whose lifetime is long compared with the characteristic nuclear periods.[1] In other words, its width is very much less than the average level spacing, $\Gamma/D \ll 1$. This long life means that the compound state has time to forget how it was formed, and the decay will be independent of the mode of formation. In fact, if we are exciting one definite quantum state of the system, the properties of such a state must be independent of the way it was formed. It is this characteristic of independence that I intend to carry over as a criterion of compound nucleus formation at higher energies. Then the question is, can the cross section for the (a, b) reaction be written as a simple product of two factors, the cross section for the formation of the compound nucleus c by capture of a, and the branching ratio for decay by emission of b:

$$\sigma(a,b) = \sigma_c(a)\, G_c(b) \quad ?$$

In the case of discrete resonances, the Breit-Wigner formula for an isolated resonance automatically factors in this way. As we go up in energy, both width and density of levels increase until overlapping occurs, until the widths are of the same order as the spacing, $\Gamma \sim D$. We are now going to excite two or more quantum states at a given energy, and their relative phases will depend upon the mode of formation. Clearly, the simple independence hypothesis no longer holds. On the other hand, we can use the many-level resonance formula, if the overlapping is not too serious, and the concept of intermediate compound

nucleus states remains useful. As the energy increases and the overlapping becomes worse, the many-level formula gets out of hand because of the large number of unknown parameters. When the density of the overlapping states becomes very large, in the so-called continuum region, a new possibility arises. So many states are involved that, even though their relative phases are determined by the way they were formed, we may be able to treat them as random for the decay process. In this way we might hope to achieve the independence form again, at least if we average over a small range of energy.

Evidently there is an upper limit to this. As the energy rises, the decay width becomes very much greater than the level spacing, and as it becomes larger and larger, the time available for reaching statistical equilibrium falls rapidly. In fact, it is not clear that there should be any region of energy in which there is time for the compound system to reach a chaotic state before decay. Experiments indicate that in an energy region of, say, 10 to 30 Mev this statistical picture does, in fact, describe many of the general features of the reaction. There are also many indications that the relaxation time needed to reach thermal equilibrium is just too long for the statistical picture to be applied in any great detail. Quite a large fraction of the decay products seem to appear before complete chaos is reached.

I am not going to describe experimental work; my intention is rather to set the stage for the experimental papers that follow. For this purpose it is useful to use the language of dispersion theory[2], and this is summarized very roughly on Slide 1.

The "black box" in the middle of the picture shown is the interior region of the configuration space which contains the compound system; on the outside the various decay channels are denoted by C_1, etc., To describe that goes on inside this black box we define a complete set of standing waves, x_λ. The equation is inside the box. We define these by setting up suitable mirrors(that is to say, boundary conditions) at the ends of the various channels. Then the cross section σ_{12} for transfer from one channel to another is related to this description in terms of these standing waves by the derivative matrix, in terms of the reduced widths $\gamma_{\lambda c}$. The $\gamma_{\lambda c}$ is the amplitude for the λ standing

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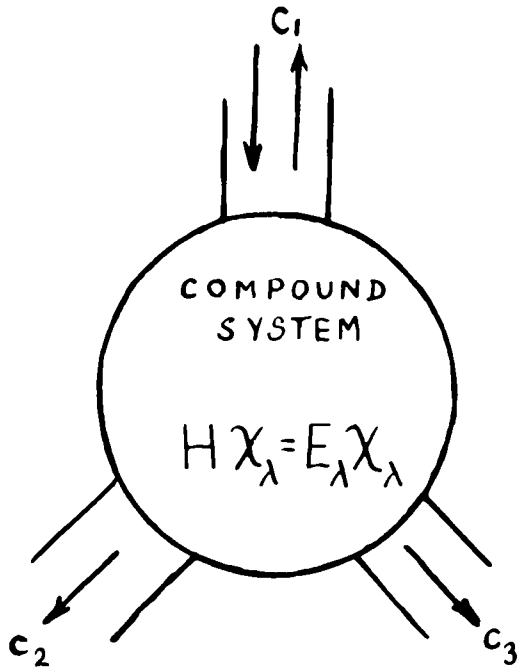

SCATTERING OR COLLISION MATRIX

$$\sigma_{12} = \pi \lambda\!\!\!^{-2}_1 \, |S_{12}|^2$$

IGNORING IRRELEVANT FACTORS –

$$S = \frac{1 + i\mathcal{R}}{1 - i\mathcal{R}}$$

DERIVATIVE MATRIX

$$\mathcal{R}_{c_1 c_2} = \sum_\lambda \frac{\gamma_{\lambda c_1} \gamma_{\lambda c_2}}{E_\lambda - E}$$

REDUCED WIDTHS $\gamma_{\lambda c}$: AMPLITUDES OF χ_λ AT ENTRANCES TO CHANNELS C

Slide 1

wave which is excited, at the entrance to channel C, and these are the parameters I shall be talking about. This description is quite general because there are an infinite number of these gammas to play with, and also arbitrary because there are many ways of defining the x_λ waves.

The standing waves themselves do not have any direct physical significance except in the region of discrete resonances, where we can interpret them as the actual resonance states of the compound system, and that is where we get the one-level formula. In the continuum region, however, they are merely a convenient set of waves with which to describe the actual wave functions inside the box. Because of this I have avoided calling them compound nucleus states.

In the continuum, a large number of these standing waves are required to describe the reaction, and a large number of the corresponding reduced widths. In order to apply statistical arguments we assume that these various gammas are uncorrelated; that is to say, that they have random signs. It seems to be necessary to make this statistical assumption, this random phase approximation, in order to reduce the actual collision matrix to a form which resembles the many-level resonance formula.[2] On transfer from channel alpha to beta, the resultant form is then something like this (E is the operating energy):

$$S_{\alpha\beta} \sim \sum_\lambda \frac{\gamma_{\lambda\alpha} \quad \gamma_{\lambda\beta}}{E_\lambda - E - 1/2\, i\, \Gamma_\lambda}$$

To be able to do this we have, in fact another restriction, and that is, although our total level widths, Γ_λ, are very much greater than the level spacing D, we can get this form only if we impose the restriction that the average of the partial widths $\langle \Gamma_{\lambda c} \rangle$, is much less than D. This average is an average over channels C, as well as levels λ.

I should, I suppose, define these gammas. The total width Γ_λ is the sum of the partial widths, $\Gamma_{\lambda c}$, and the partial width is 2 times the penetrability factor times reduced width squared, $\Gamma_{\lambda c} = 2 P_c \gamma_{\lambda c}^2$. Because of this condition, as we go up in energy eventually even this approximation will break down and the compound system does not stay

together long enough for thermal equilibrium to be reached. In the expression for the cross section, which is going to be proportional to the S-matrix element squared, we will still get a lot of cross terms between different λ- different resonance states. Because of these cross terms, we still are unable to write this cross section as a product of a formation part depending on α and a decay part depending on β. Although the random phase or the statistical assumption does tell us that the most probable value of these cross terms is zero, it also implies that there will be strong fluctuations about zero. We have to average this over an energy region δE which is very much greater than the level widths Γ_λ to damp out these fluctuations, and then we can neglect the cross terms. We get a situation which is summarized in Slide 2. These results I am talking about are the work of R. G. Thomas.[2]

So having gotten this form we interpret this as the cross section σ_α for formation of the compound nucleus by absorption from channel α, and the decay branching ratio G_β for decay into channel β. The $T_c{}^J$ are essentially the transmission factors discussed by Weisskopf.[1,2] The interesting fact does emerge from this that even with the random phase approximation, we get the split up but only between terms with the same total angular momentum and the same parity, and so obviously, since σ is a sum of terms like these, the total cross section cannot be broken up into the formation part and the decay part. We cannot have complete independence; this has been known for some time. The compound nucleus must at least retain memory of the total angular momentum J and parity.

We might remark here that it is still sometimes implied that the compound nucleus assumption, and the random phase or statistical assumption, are separate. If by compound nucleus we mean independence of decay and formation, this is not so. We have just seen that independence itself depends upon random phase.

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STATISTICAL THEORY

1. CONTINUUM : $\Gamma_\lambda \gg D$ $\quad (\Gamma_\lambda = \sum_c \Gamma_{\lambda c})$
2. AVERAGE OVER LEVELS λ AND CHANNELS c

$$\frac{\pi}{2D} \langle \Gamma_{\lambda c} \rangle_{\lambda c} \ll 1 \qquad (\Gamma_{\lambda c} = 2 P \gamma_{\lambda c}^2)$$

3. RANDOM SIGNS FOR $\gamma_{\lambda c}$

AVERAGE OVER SPREAD $\delta E \gg \Gamma_\lambda$ IN C.N.,
DEFINITE RESIDUAL STATE

$$\langle \sigma_{\alpha\beta} \rangle = \sum_J \left(\pi \lambda\!\!\!^{-2}_\alpha \sum_{s_1 l_1} g(J)\, T^J_{\alpha s_1 l_1} \right) \frac{\sum_{s_2 l_2} T^J_{\beta s_2 l_2}}{\sum_{\gamma s l} T^J_{\gamma s l}}$$

$$= \sum_J \sigma^J_\alpha(\text{C.N.})\, G^J_\beta(\text{C.N.})$$

"TRANSMISSION" FACTORS: $T^J_c = \frac{2\pi}{D} \langle \tilde{\Gamma}_{\lambda c} \rangle$

Slide 2

Now Tony Lane pointed out[2] that we can, in fact, get complete independence if we average over the residual nuclear states in a small range of energy, provided we make two assumptions. We just do this averaging by multiplying by the density of the states for the residual nucleus, and if we assume that the density of states for the residual nucleus of spin I is proportional to 2I + 1, and also if we neglect any dependence of the transmission factors T_c^J on the spin, if we assume that they depend only on the orbital momentum and not on the channel spin or the total angular momenta, then it is quite simple to sum over the final nucleus spins, and the channel spins; we then do in fact get the cross section as a separate factor for formation and for decay, in this average sense.

Unfortunately neither of these assumptions is strictly true. The existence of spin-orbit coupling shows that the transmission factors will depend on channel spin and total angular momenta to some extent, even though the dependence may be rather weak. Also, theoretical arguments lead one to expect that the dependence of the density of states on the spin is rather more complicated than the simple 2I + 1 rule. In fact, one expects them to depend on I in the way suggested in Slide 3. There are several forms one can take, but the general form remains of this kind. This merely reflects that the energy of excitation is partly taken up by the rotation of the nucleus associated with the angular momentum I, so that there is less energy available for thermal excitation, so to speak. This is the formula given by Lang and Le Couteur[3], and we see that, even for quite low energies and quite low values of I, the density of states departs quite considerably from the simple 2 I + 1 law. One should not take the numbers too seriously; I used "reasonable" values for nuclear temperature T, and so on (excitation energy E (Mev) = T^2 (Mev) A/10.5, moment of inertia $J = A^{5/3}/75\ \mathrm{Mev}^{-1}$).

Experimental evidence on this is rather scarce, although radiation widths from slow neutron capture in silver seem to support the 2 I + 1, dependence for states of spin I = 0 and 1 (so that the exponential factor does not have much effect), and there is also some similar evidence

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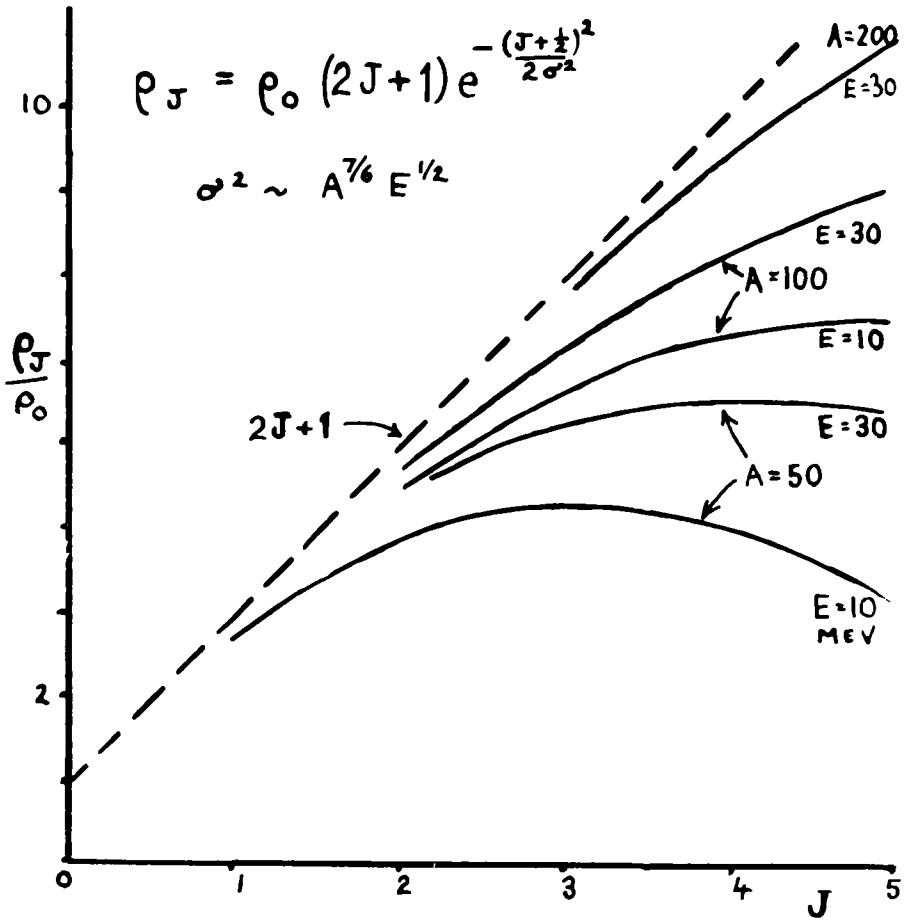

Slide 3

from the high spin states in indium and europium which may support the exponential factor, but really there is not enough evidence to say much one way or the other.

Unfortunately, no theoretical estimates have been made of how this dependence on I will affect the summing over the resiudal nuclear states, and how far the final expression will then depart from the simple independence form we saw earlier. Around 20 Mev a reaction may feel out quite a wide range of residual nuclear spins (one might say, very crudely, that the maximum value for the residual spin is going to be of the same order as the maximum value for the intermediate compound state spin, which itself will be of the same order as the maximum value of the incident orbital angular momenta. In the classical limit, this is the wave number times the nuclear radius, k R, and for, say, 25 Mev this is equal to R (in Fermis) for nucleons, 2 R for alpha particles, 4 R for N ions, etc., and so we can reach quite high values).

Now a few words about angular distributions. The general expression for the angular distribution contains products of collision matrix elements in a way similar to the total cross section, and we use the same random phase argument to say that we can neglect the cross terms between different λ. Since this automatically throws away any contribution from interference between terms of different parity, this automatically gives us the well known result of symmetry of about 90 degrees. But now we are throwing away here also cross products of terms with <u>different</u> orbital and total angular momenta for the incoming and outgoing partial waves and <u>different</u> total angular momenta for the compound state which never appeared in the total cross section formula at all. The total cross-section involves cross terms only between states with the <u>same</u> parity, the <u>same</u> orbital and total angular momentum. So this implies that the angular distribution should show much greater fluctuations than the total cross section. So we should not be surprised to see the angular distribution change with energy.

Since this is a statistical theory, it predicts in this way the average behavior, and if we look carefully enough, we should be surprised if we did not see statistical fluctuations from the average. We can damp out these statistical fluctuations only by doing this averaging over an energy region which has to be greater than the level widths. On the other hand, a back-forward asymmetry which seems to persist over quite a range of energy is clear evidence that the reduced widths are correlated and not random in sign.

It is still sometimes said that the angular distribution will be isotropic on the random phase, or statistical, assumption. This is certainly not true when we are dealing with decay to a definite residual nucleus state. In fact, to get isotropy we have to average over residual nuclear states in the same way that we did to get independence, weighting them with the density of states factor. We have, in fact, to make the same two assumptions; density of states depends on the spin $2I + 1$, and also the transmission factor does not depend on the spins. Then this averaging always leads to isotropy; but as I said before neither of these assumptions is strictly correct, although experimentally the low energy products from reactions up to, say, 20 Mev do seem to show isotropy.

All the conclusions I have drawn so far have been based on the assumption of random signs for these reduced widths for different levels and channels. So that then we do this averaging over an energy range $\delta E \gg \Gamma_\lambda$ we are able to neglect the cross terms, and at the same time we also neglect contributions from levels λ which lie outside this small energy range. But the observation of asymmetric angular distributions which persist over large energy regions, and so cannot be simply statistical fluctuations, immediately implies the phases are not completely random. Also since the asymmetry does not vary rapidly with energy, it must be due to long-range correlations between the widths arising from levels which lie outside this range that we are averaging over. Although each one contributes very little to the cross section, there is an infinite number of them so that, if there is any coherence between them, we can get quite large effects.

I thought I might mention at this point, perhaps off the track, that Brown and de Dominicis,[4] at Birmingham, starting from the dispersion theory, this sort of formalism, have recently succeeded in summing these contributions from far-off levels and finish up with just the usual Born-approximation formula for direct reactions which everybody uses. Bloch, at Saclay, using a slightly different way, has got the same result.[5] So this link between dispersion theory and the usual treatment of direct reaction is very satisfactory. I think at the same time it opens up the possibility of being able to treat more complicated, "semi-direct", processes lying somewhere between the simple direct reactions (where perhaps there is just one encounter between the bombarding particle and the target), and the other extreme, the compound nucleus, where complete thermal equilibrium is set up.

Well, one final remark perhaps I might make is that on reading to prepare for this talk, I became more and more surprised that the statistical picture held as well as it did, rather than being surprised that one found deviations from it.

CHAIRMAN SNELL: Any questions on Dr. Satchler's paper?

G. BREIT: From talking with experimental people I have had the impression that the kind of thing they really want is to have a book that tells them how to do theory, that can be relied on completely so as to interpret their own experiments. Of course, it is impossible to have a formula without limitations. I believe that the worst offender in this respect is the theory of nuclear reactions. You have just heard a very fine presentation, as fine an presentation as can be given in such a length of time. On the other hand, I believe that there are some things that the experimentalist is going to misinterpret. Some confusion can result, for example, in the employment of energy levels in the continuum. What is not realized is that the quantities that look so wonderful and nice as the E_λ can be defined in a great many ways, with many boundary conditions, and mean different things.

A theorist by nature tries to find something simple and, therefore, he does this kind of averaging that we heard about which is sufficient to obtain a certain type of answer which contains factors that one interprets in terms of probability of penetrating inside the nucleus and of getting out.

On the other hand, I believe that one should remember, as has already been emphasized in the talk, that these assumptions are not necessarily true.

I have not studied the work of Brown and de Dominicis referred to by Dr. Satchler as well as I would like to but I should like to call attention to some work by the late R. G. Thomas, who wrote a paper[6] in which he went into the nature of the direct interactions, which seems to be reasonably safe in principle although not terribly practical because it is very difficult to carry out. His point of view seems to me to have the advantage of combining a conscientious and realistic view of the situation with some generality. He surrounded the nucleus by a surface and treated everything inside the surface by the R-matrix scheme as a black box. Outside the surface he used Green's theorem and derived from it a formula which is rather general, which is the equivalent of the formula which many of us are familiar with, I think, which is the one that deals with the calculation of phase shifts, and here you have to know the final wave function in order to compute the phase shift exactly, a view I think that is similar to that of W. H. Ramsey.

The equivalent of that has been carried out by R. G. Thomas, but in order to describe stripping reactions he has to replace the final ingoing wave modification of the exact wave function which presupposes in a sense a knowledge of the results. He has to replace it at best by a distorted wave which does not include in it excitation states of the residual nucleus. I am, therefore, somewhat skeptical of the completeness of the work of Brown and de Dominicis and of that of Claude Bloch, both of which I think are fine, but nevertheless I don't think they cover the situation as clearly as the work of R. G. Thomas. I wonder, Dr. Satcher, what you think about it. It really seems to me that the situation here is that the development of the theory has produced an answer which is deceptively clear.

SATCHER: I don't know that I have very much to say. I think I agree. It seems to me that the work of Bloch, particularly, holds out the possibility of treating reactions from the opposite extreme from dispersion theory, starting from simple direct reactions. I think it is true of both Bloch, and Brown and de Dominicis, that so far only the first order approximation has been treated; they only pick up the biggest terms. I don't think I can add anything further.

N. ROSENZWEIG: I have a remark of much smaller scope; I am in complete agreement with what you said about the angular momentum and the level density, except that I would be somewhat more emphatic about this 2I + 1 law, namely as follows: In so far as the quasi-stationary states are concerned there is no doubt at all that the 2I + 1 law must break down, because if you admit that there is only a finite number of lambdas available then this thing just must come down. I believe the reason the 2I + 1 law has been substantiated so far is that in all these experiments that have been done the angular momenta involved have just been small compared to the turning point which you indicated on your curves.

SATCHER: On the last point, I am not sure that it is true that only small angular momenta have been involved. I have not had time to think about it in any detail, but I think, for example, as regards the isotropy of the angular distribution, this includes some experiments which have, say, 20 or 25 Mev incident energy, so that quite high values of angular momenta are involved.

S. DEVONS: I have a quite definite remark to make. I am not quite sure to whom to make it. This morning, if I understood what was said correctly, in heavy nuclei the ratio Γ_f/Γ_n was shown to be pretty constant with energy; this was interpreted as being somewhat surprising. On the other hand, if one assumes as the basis of interpretation the compound nucleus situation in the sense that Dr. Satchler mentioned, then this should be expected and should be the test, because in the past one tended to think of the test of the compound nucleus as being the similarity of decay when one has very different ways of creating the compound system.

It is an equally sensitive test of the similarity of decay when one thinks of very different degrees of freedom. These are two very different degrees of freedom, but in this thermal equilibrium then the probability should be simply a question of the probability of the energy going to this degree of freedom, and if one regards the fission degree of freedom associated, say, with quadruple deformation and the neutron emission with the energy focusing on the single particle, these are totally different degrees of freedom, but both have the same energy of the order of 6 Mev or thereabouts, the typical energy theshold for neutron emission and the typical fission barrier.

I am not sure of the figures, but both of these would depend on temperature in a similar way, and if I remember the situation correctly, this should be accepted as good evidence that there is good statistical equilibrium, because one has such different degrees of freedom, both participating in a very similar temperature-dependent way.

I am not sure to whom that question is addressed, but does anybody agree or disagree with it?

G. T. SEABORG: Continuing with what Devons said, with which I am in complete agreement, one can even discern a variation of this ratio with nuclear type and binding energy of the neutrons; Prince more or less derived the rationale or the resonableness of such a relation. But if you plot something that is related to the binding energies of the neutrons which change, you see, as you change Z/A and something related to Z^2/A, which probably determines the activation energy for fission, you get a straight line relationship. I mean something very simple like the summation of the binding energy of the neutrons that are involved in the emission, when you get average values of this, as you often do, - Z^2/A or something like this. You get a very simple relationship between the logarithm of this ratio and some simple fission like this.

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3. J. M. B. Lang and K. J. LeCouteur, Proc. Phys. Soc. A 67, 585, 1954.
4. C. E. Brown and de Dominicis, Proc. Phys. Soc. A 70, 686, 1957.
5. C. Bloch, Nuc. Phys. 4, 503, 1957.
6. R. G. Thomas, Phys. Rev. 100, 25, 1955.

6. ODD-EVEN EFFECTS IN EVAPORATION REACTIONS

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Oak Ridge National Laboratory

With the ORNL 63-Inch Cyclotron, we have been studying, experimentally, reactions induced by bombardment of various targets with nitrogen ions which yield protons, deuterons, tritons, and α-particles as reaction products. We have obtained some information about these reactions by looking at certain radio-active residual nuclei. Also we have gotten information by observing the energy distributions of these light particles and also, to some extent, their angular distributions.

What I want to talk about now is the information that we can get from the relative intensities of the energy distributions of these particles from the bombardment of a single target with nitrogen ions--the energy distributions of protons, deuterons, tritons, and alpha particles from a particular target and how these change as we change the target.

Before I present the data, though, I would like to make very clear what the experiment is; so I will start out by describing the experiment.

Slide 1 shows the counter arrangement. Actually I am going to describe two experiments here. One is an experiment by Goodman and Need, which is to appear in an early issue of Physical Review, in which beryllium was bombarded with nitrogen; the other is a new experiment which is just being done. In principle the setups are the same; they differ only in details. The nitrogen beam strikes the target and the product particles traverse the proportional counter and stop in a sodium iodide scintillation counter. The pulses from the proportional counter are amplified, shaped to have flat tops and fed to the vertical axis of an oscilloscope. The pulses from the scintillation counter are fed to the horizontal axis. At the time both of these pulses are at their peaks, the flat tops, the oscilloscope trace is unblanked so that each particle produces a single spot on the cathode ray tube screen.

Slide 2 shows the electronics used in the newer experiment. This is slightly different from the original setup. DD2 amplifiers are used instead of Al amplifiers, which is a convenience. The doubly-differentiated pulses swing positive, then negative, and then return to zero; this makes for a very convenient timing. Since this (the differential) discriminator fires approximately at the crossover point it gives us a very good time reference. One can also feed the scintillation counter pulses into the 20-channel analyzer. We gate the analyzer by an arrangement which I will describe with reference to the next picture, the oscilloscope picture. This corresponds to making the dotted line connections in the figure. With the solid line connections, the proportional counter pulses are fed into the 20-channel analyzer, and we gate from this single-channel differential analyzer on the scintillation counter. I think the reason for this will become clear when we see the next two slides.

Slide 3 shows a time exposure of the oscilloscope screen after a number of particles have gone through. The lowest full band is produced by the protons; just above are the deuterons and tritons. The alpha particles produce the heavy band at the top. When we want to count the energy distributions of, say, alpha particles we make a photo like this and cut out this top band to form a mask. The mask is then placed between the face of the cathode ray tube and a photomultiplier. Only the flashes of light which come through this hole, then, are counted as alpha particles; particles producing the other bands are discriminated against. To count protons we increase the gain to bring these lines up where you can mask them easier and follow the same procedure.

Alternately, we can connect the circuit (block diagram) to give us the pulse height distribution from the proportional counter gated by a certain pulse height from the scintillation counter.

In Slide 4 we are looking at the proportional counter pulses gated as described. To the left is a peak due to protons, deuterons, and tritons. The alpha peak at the right is a little asymmetrical; it may include some He^3.

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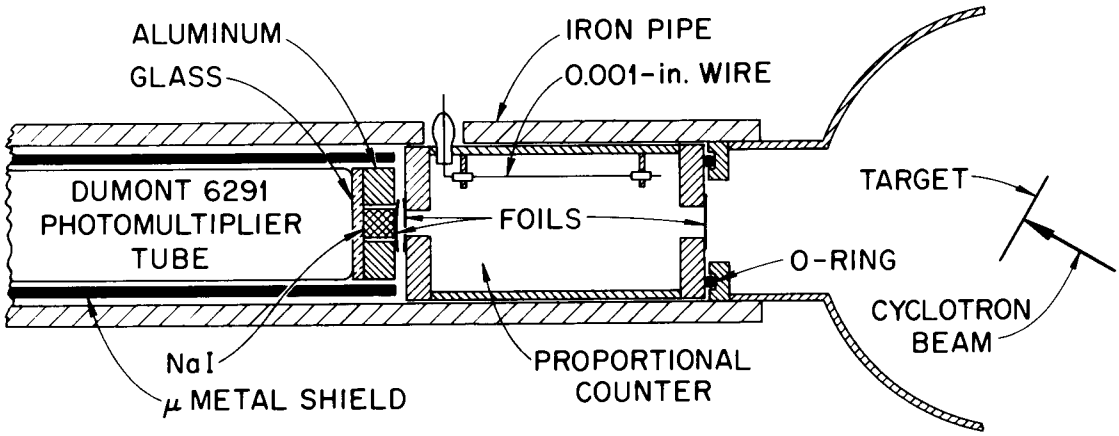

Slide 1

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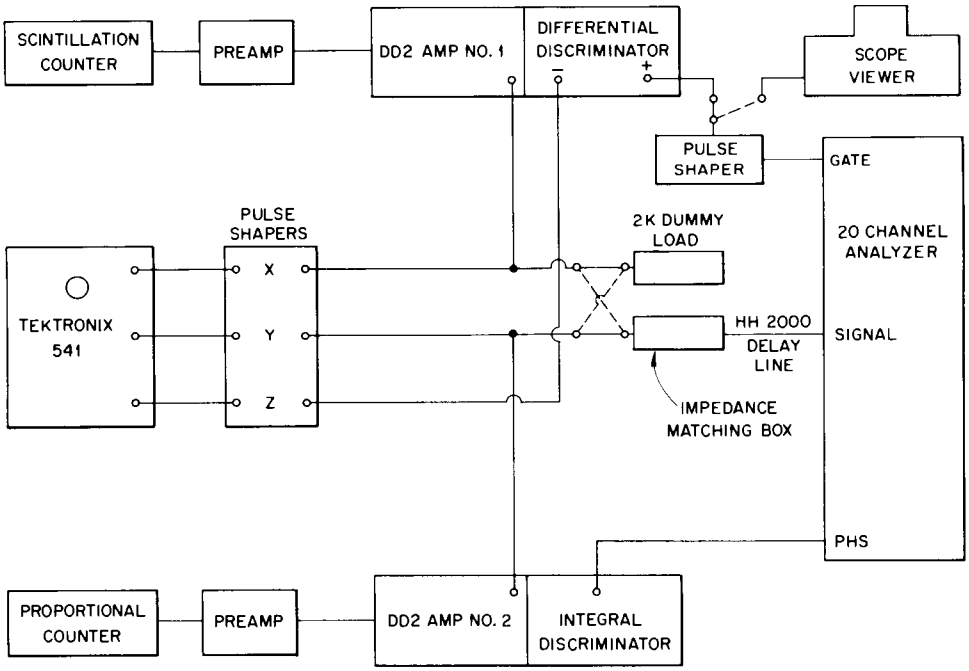
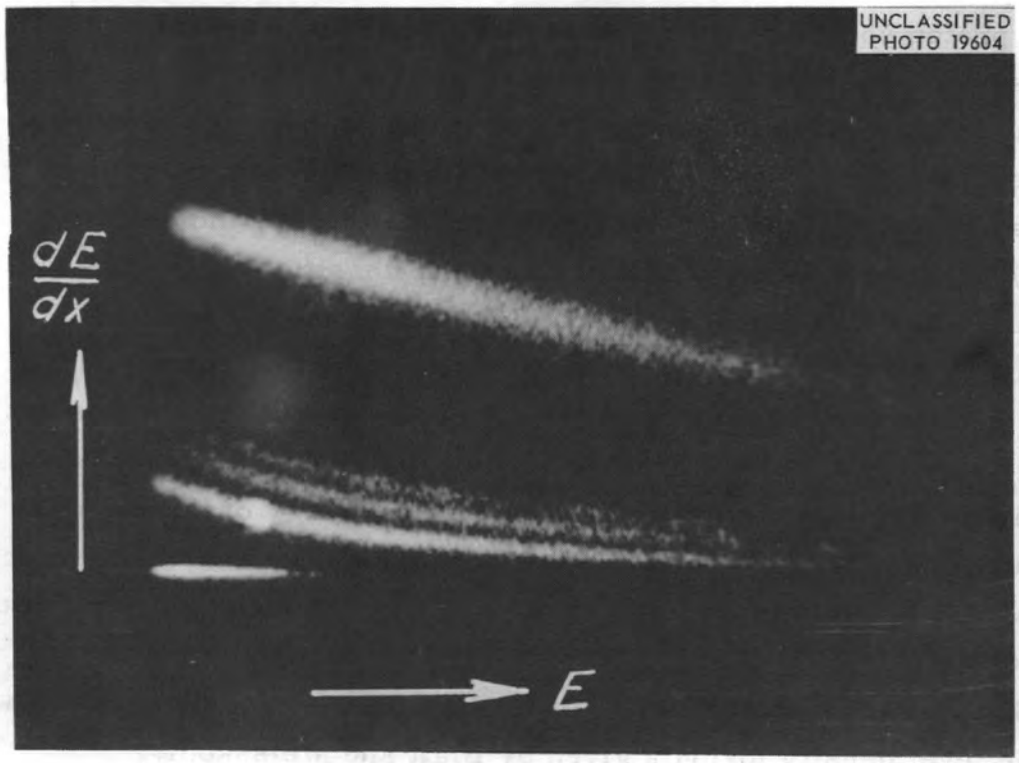
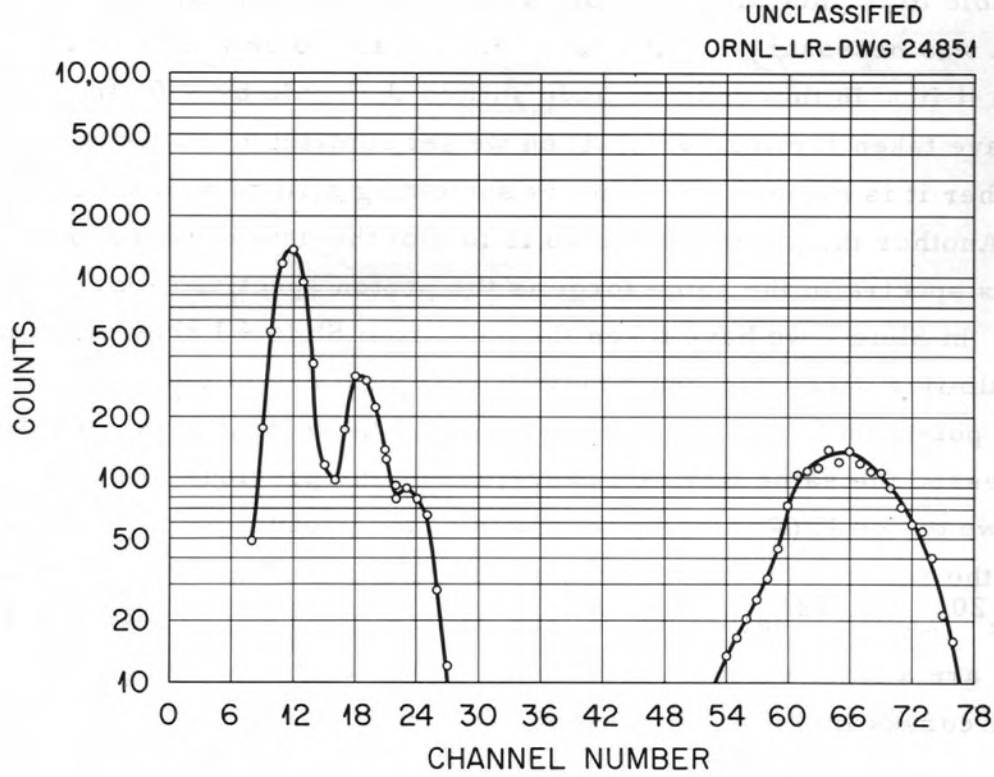


Slide 2



Slide 3

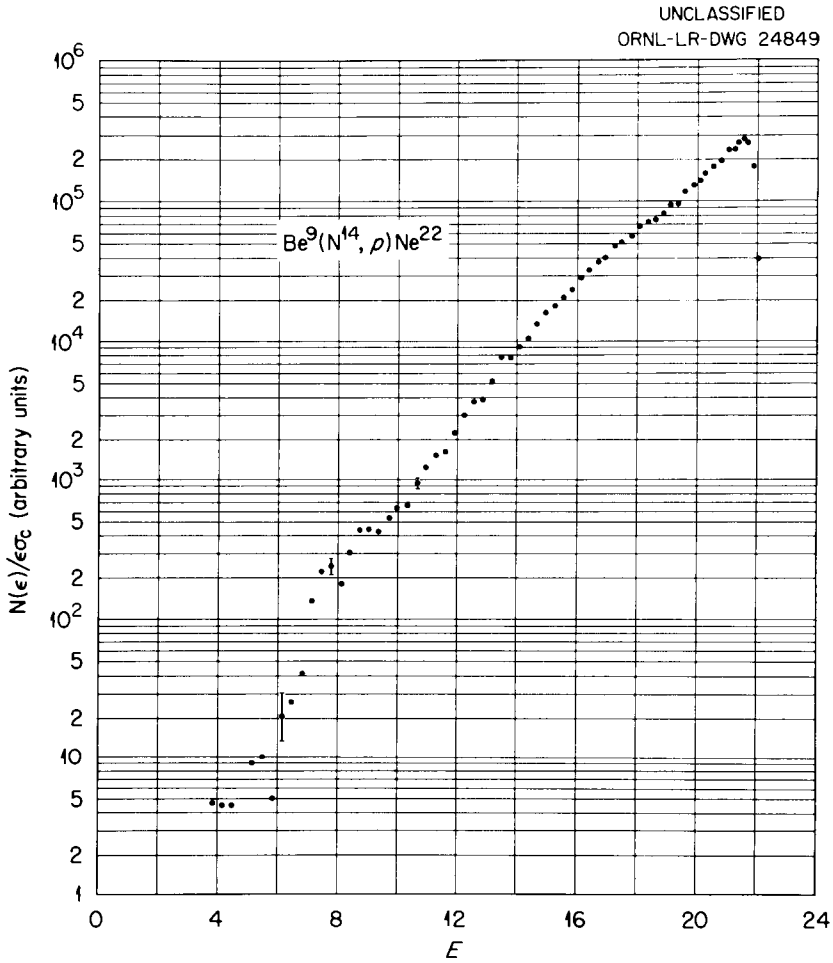


Slide 4

Slide 5 shows data taken from the arrangement where we looked at the proton spectrum from bombardment of beryllium with N^{14}. In this case, the raw data have been processed. What we found from looking at some of these energy distributions is that the gross features are described fairly well by the very simple schematic theory of nuclear reaction given by Weisskopf. In that theory, the quantity $N(\epsilon)/\epsilon\,\sigma_c$ should be proportional to a level density of the residual nucleus. The $N(\epsilon)$ means the counting rate at energy epsilon. Epsilon is the kinetic energy in the final system, and this σ_c is the cross section for the capture of the observed particle by the residual. For σ_c we used a table of Shapiro[1], interpolated where necessary. This is plotted as a function of the excitation energy of the residual nucleus in Mev. If this simple theory describes the reaction correctly, this should be a plot on a logarithmic scale of the level density--in this case of Ne^{22}.

Slide 6 shows an attempt to fit the data of the preceding slide to the simple level density formula given by Blatt and Weisskopf, $\exp\left[2(aE)^{1/2}\right]$ where a is a parameter to be determined. It was not possible to fit this where E was taken simply as the excitation of Ne^{22}, but it was necessary to shift the energy scale somewhat in order to make it fit. In this case we have shifted the scale by 4.9 Mev; that is, we have taken E minus 4.9. Then we get something that is not too bad. Whether it is meaningful or not is something still to be decided.

Another thing that we can do is to plot the data obtained from the dE/dx spectra in the same form as the proton spectrum which you just saw. In Slide 7 we have taken the data from Slide 4 I showed before, and similar ones, and plotted--well, for each one of those curves you get 4 points on here, one point for each of these reactions. These are processed the same way as I described on the previous slide except that now we divide $N(\epsilon)$ also by the mass of the product particle. Actually it is the reduced mass. What happens here is that the reactions leading to Ne^{20} and Ne^{22} fall more or less on this lower curve here and F^{19} and Ne^{21} are a little higher. It is possible now to piece in the data from the other curves into this; the next slide shows this.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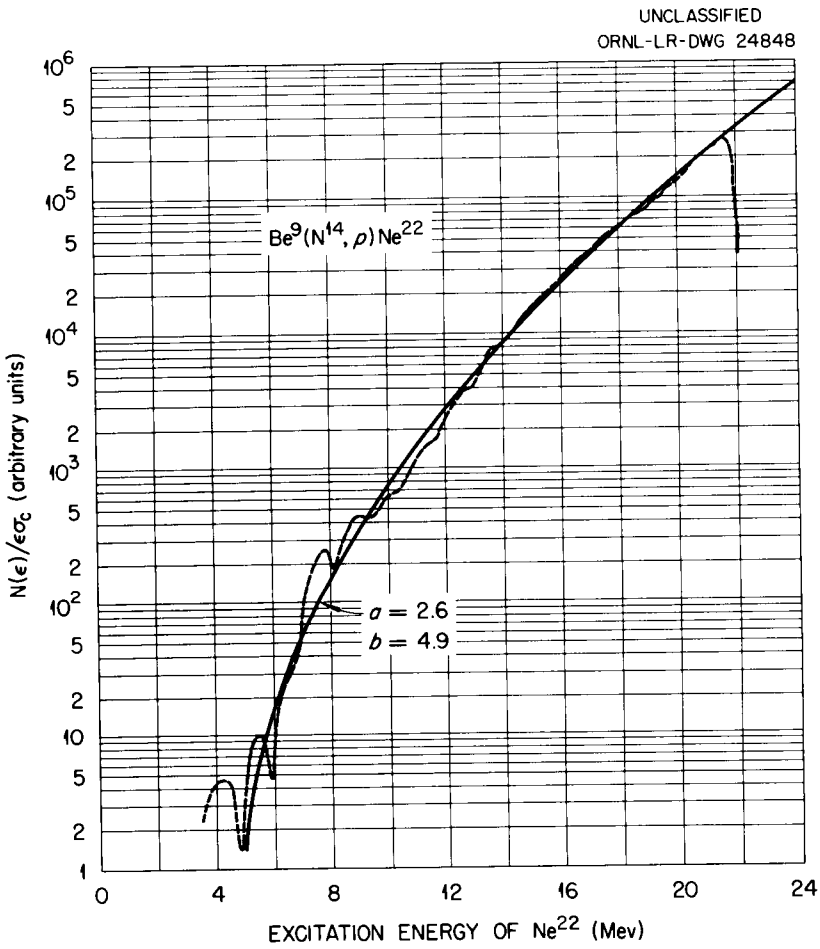
$Be^9(N^{14}, p)Ne^{22}$
$N(\epsilon)/\epsilon\sigma_c$ (arbitrary units)
10^6
5
2
10^5
5
2
10^4
5
2
10^3
5
2
10^2
5
2
10
5
2
1
0
4
8
12
16
20
24
E

Slide 5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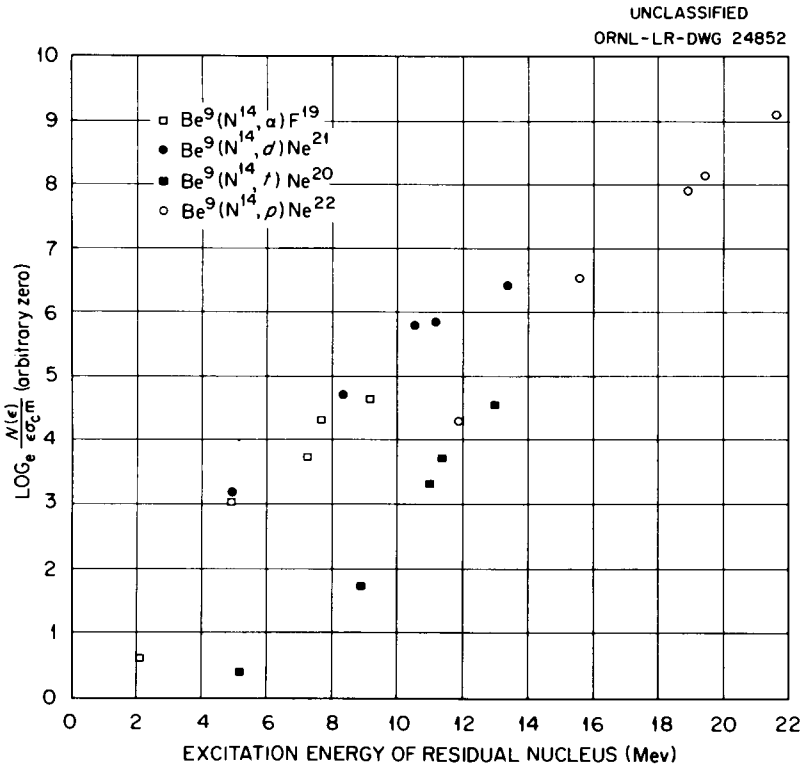
$Be^9(N^{14}, p)Ne^{22}$
$a = 2.6$
$b = 4.9$
$N(\epsilon)/\epsilon\sigma_c$ (arbitrary units)
EXCITATION ENERGY OF Ne^{22} (Mev)
10^6
5
2
10^5
5
2
10^4
5
2
10^3
5
2
10^2
5
2
10
5
2
1
0
4
8
12
16
20
24

Slide 6

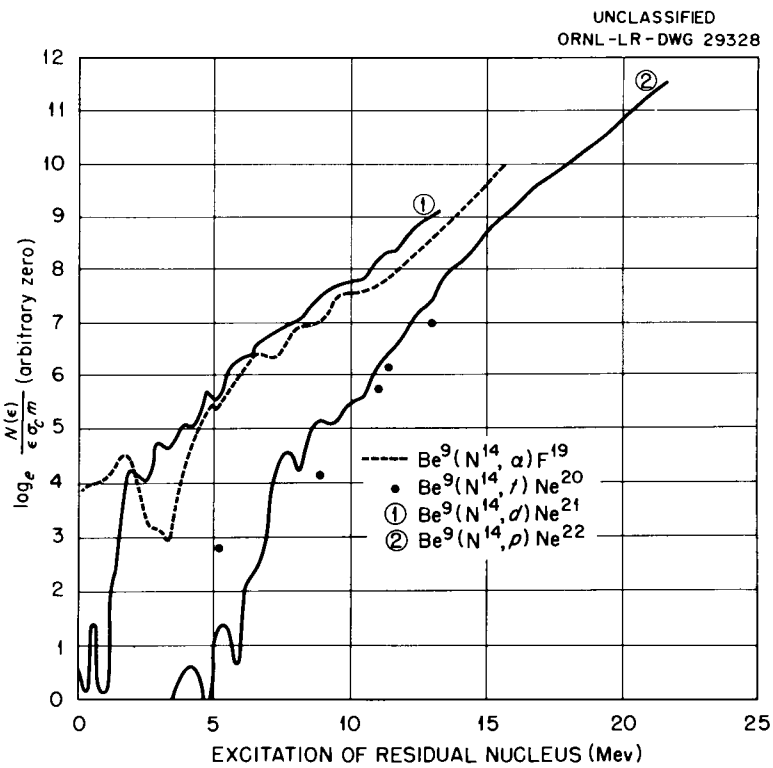
These curves, in Fig. 8, are taken from the many points that we have, and their relative positions here are obtained from the dE/dx data. Here again you see the F^{19} and Ne^{21} curves grouped together, and here are the Ne^{20} and the Ne^{22} grouped together. We never did get a complete triton spectrum, so I have only the five points taken from the previous slide. This figure alone looks interesting, but possibly one would not want to take it too seriously. It is just one experiment, and the fact that these points fall in this grouping might not mean anything. It is not clear certainly whether this pattern is a characteristic of the residual nucleus or perhaps of the product particles. In the lower curves we are looking at tritons and protons, and in the upper curves at alphas and deuterons; maybe this pattern has something to do with them or maybe it is purely accidental.

But it seemed from these experiments that an obvious experiment to try in this regard was to choose targets -- well, here a proton went to even-even residual. Suppose we chose a target such that the alpha particles would lead to an even-even. What would that do to the relative positions of the alphas and protons? A convenient choice of targets for this, for our cyclotron, was C^{12}, N^{14}, and O^{16}. If we bombarded, say, N^{14} with N^{14}, a deuteron out would lead to Al^{26} which has an odd number of protons and an odd number of neutrons. An alpha particle out would lead to Mg^{24} which has an even number of protons and an even number of neutrons. If we change the target to oxygen or carbon, then we change the relationship of the alphas and the deuterons with respect to the odd-odd or even-even character of the residual nuclei.

To perform this experiment we used the arrangement shown on Slide 1. We put an entrance foil on the target chamber, so that we could fill the gap with gas to make a gas target. Otherwise, it looked quite similar to this. With the gas target, the angular aperture is defined simply by the entrance and exit holes of the proportional counter so that the geometry is a little different. With the full aperture it gives about 9 deg.



Slide 7



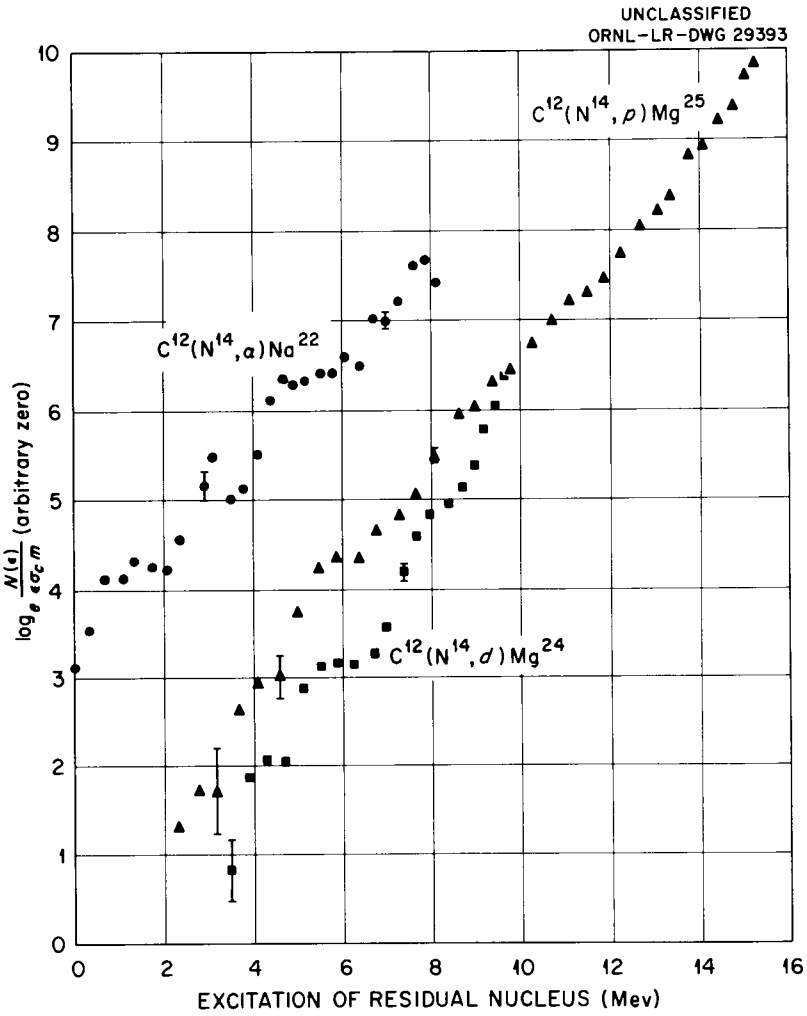
Slide 8

If these angular distributions are isotropic in the center-of-mass system we still may expect a distortion of the energy spectrum because of the change in the laboratory energy corresponding to a given residual excitation as the lab angle is changed. One can calculate this effect by assuming the shape of the spectrum. For a statistical theory spectrum even the 9-deg aperture should not distort the shape within the statistical errors of these experiments. However, we tried both the full aperture and a narrow aperture; the chief difference between the two is that you can see some fine structure in the narrow aperture which gets washed out with the wide aperture, but the gross features of the spectrum are the same.

We used nitrogen gas and oxygen in the target. For the carbon target we tried methane. It is a little troublesome because of a rather large counting rate from elastically scattered protons. We did a little better in that regard by using benzene as the target, although it is a little less convenient to use.

The data of Slide 9 were obtained from a carbon 12 target. Actually, the benzene and methane targets are not distinguished on the slide. The deuterons and protons were run with methane. The alpha run has some points with methane and some points with benzene. It appears here that the alpha reaction which leads to Na^{22} has a higher level density than Mg^{25} or Mg^{24}. Also, for this even-even nucleus (Mg^{24}) the level density is quite steep. This pattern is repeated in each of the other cases. The odd-odd nucleus appears above, the odd-A nucleus in the middle, and the even-even down below.

When looking at Ne^{22} before,I said that we obtained a reasonable fit to a level density formula if we shift the energy scale. The suggestion here is that in going from the odd-odd to even-even, shifting the energy scale would certainly help matters. T. D. Newton, in a paper on the shell effects of level density, does a trick like this before he takes into account the shell effect. Whether this shift is reasonable can be examined by the suggestion that Newton gives in his paper. Suppose you plot the binding energy of an odd-odd, even-even, odd-odd, even-even sequence with thesame number of protons as neutrons. That will cover Mg^{24}, Na^{22}, and the residual nuclei that will appear on the next slide.


UNCLASSIFIED
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$C^{12}(N^{14},p)Mg^{25}$
$C^{12}(N^{14},\alpha)Na^{22}$
$C^{12}(N^{14},d)Mg^{24}$
$\log_e \frac{N(\epsilon)}{\epsilon \sigma_c m}$ (arbitrary zero)
EXCITATION OF RESIDUAL NUCLEUS (Mev)
0
1
2
3
4
5
6
7
8
9
10
0
2
4
6
8
10
12
14
16

Slide 9

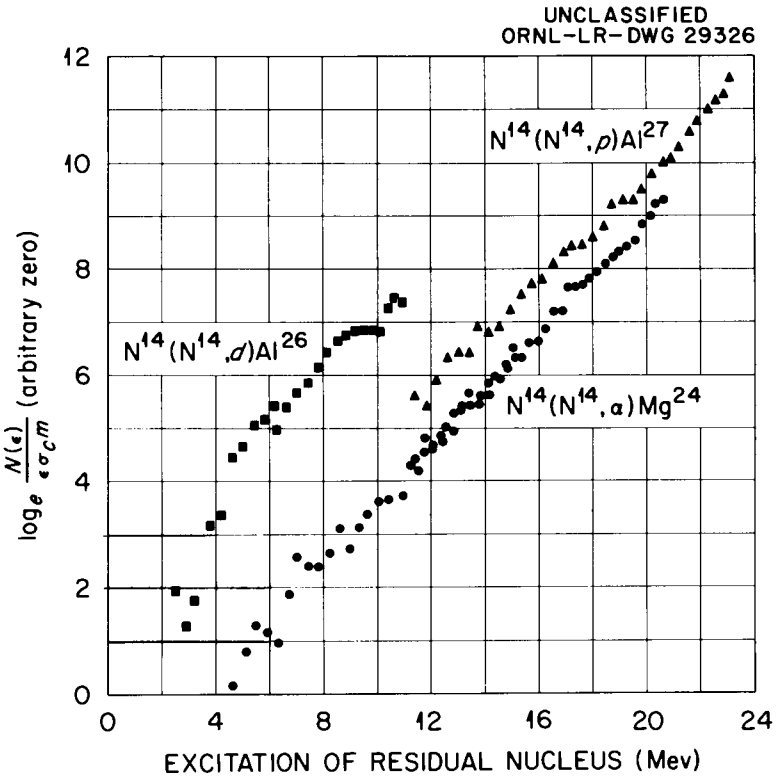
We tried plotting the mass excess versus A for F^{18}, Ne^{20}, Na^{22}, Mg^{24}, and Si^{26}. They fall on curves that are separated by just about 5 Mev. This suggests that this shift in the energy scale should be about 5 Mev.

Let us look at the rest of this data. In Slide 10 Al^{26} is the odd-odd, and Mg^{24} is the even-even. Here, even though the odd-odd now comes from deuterons, it is still on top. Deuterons now take the position that the alphas had in the previous slide.

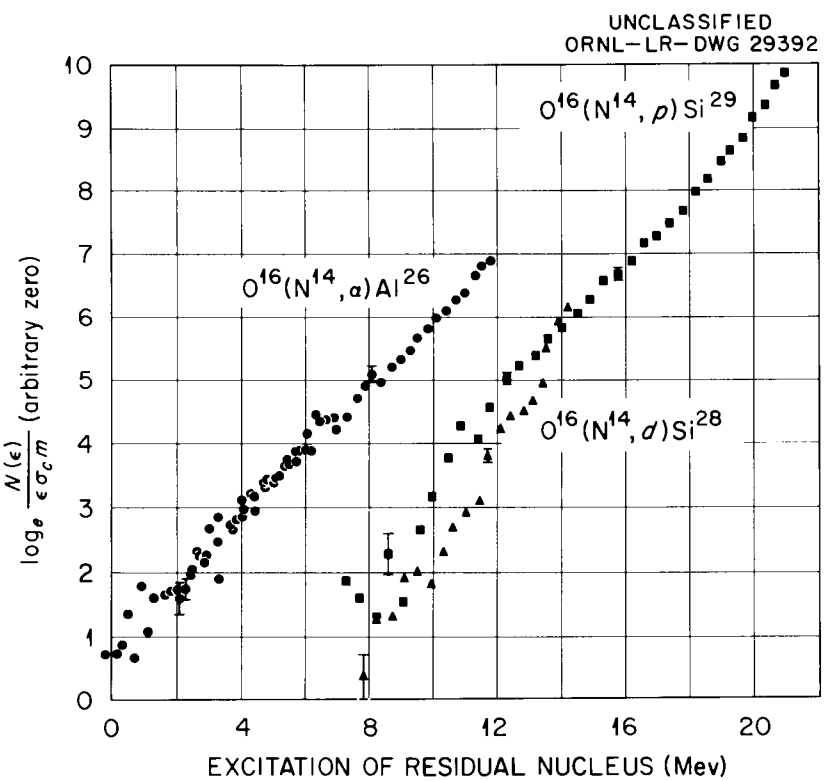
The oxygen target, Slide 11, again shows the odd-odd on top and the even-even on the bottom. You will notice the Al^{26} appears in this slide and the one before, and similarly Mg^{24} appeared in the one before and the one before that. So it looks as if we have the possibility, if we take this seriously, of finding the relative level of density of all of these nuclei. Unfortunately, I don't have a slide on that, but what happens is that they stay more or less like this. Si^{28} and Mg^{24} will be below; Si^{29} and Mg^{25} will be next; and Al^{26} and Na^{22} will be above and fairly close together.

Another interesting question is to see whether this has anything to do with the level density that you might find from actually counting levels where they are known. Unfortunately, it is a little hard to get published data in this region. It does not do too well to count levels in a tabulation like Endt and Kluyver. As soon as you do that and you look up a more recent article, it always has more levels. I did play around with this a little. I will just mention what it looks like but I do not mean to be taken seriously. In the cases of Mg^{24}, a great number of levels are listed in the paper of Bauman, Prosser and others, from Kansas. It is possible to find a level density.

Here again, in Slide 11, is Mg^{24}. In the region between 11 and 12 there are many levels. I tried counting them in small intervals. We count 10 levels here and get the density here, and count another 10; oddly the density of levels rises very steeply. It is about like the curve on this figure. I also tried Si^{28} for which many levels are reported in the literature and got something similar to this figure. Also, Al^{26} has a lot of levels; they too agree remarkably well with this figure.



Slide 10



Slide 11

You take all these curves and plot them on the same graph and then take all these level counts and plot them. They do seem to fall on this kind of a pattern. I should point out before I close, though, that no account has been taken in this plot of the spins. Thank you.

CHAIRMAN SNELL: Are there questions or comments?

J. S. BLAIR: Have you done anything about the angular distributions of these particles?

GOODMAN: No, I have not. I am glad you asked the question. Actually that will be dealt with in the next paper. In this particular experiment so far we have done only one angle. I don't know whether I said what the angle was. It was 45 deg in the laboratory system, which is not a constant center-of-mass angle. It varies from 50 to 75 deg in extreme cases, depending on the energy. We have very little data so far on the angular distributions. It appears that the proton angular distributions are quite isotropic, but the alpha distributions do not appear isotropic, and that should indicate that if we did this same expermient at a different angle it would shift this pattern somewhat.

How this ties in is a little hard to say. In the case of oxygen it may move the alpha curve--well, take it at zero degrees instead of 45 deg--It would appear that it might shift the alpha curve up about a factor of 3. I think obviously the thing to do is to take data at different angles and see how they come out. The first slide I showed was taken at zero degrees, for example.

BLAIR: This is not a question, just a point, that if you are going to make this sort of statistical analysis, this is based upon the proved statistical theory, you would like to make sure that you have no direct processes taking part, and if you start to find a forward peak angular distribution, I think this does raise a question regarding the analysis.

C. E. PORTER: The question I want to raise is how constant are the computed compound nucleus cross sections that you use in analyzing your data as a function of energy? Could they vary a great deal? Are you running into variation in the process?

GOODMAN: All of this is above the barrier. The cross section is fairly constant. Up at this extreme end of the picture (high excitation on the slide), the barrier does matter somewhat. So far as the data I presented here are concerned, small changes in the barrier do not show any perceptible difference. However, we do have other similar data where you can begin to see strong effects when you change the barrier.

B. L. COHEN: This business of comparing with known levels in the low lying energy region sounds sort of impressive when you first hear about it, but it is also a very dangerous process. For example, he referred to some work by Rubin. This work was done by deuteron stripping, and deuteron stripping excites a very special type of level; namely, those that can be formed by putting a neutron into the nucleus without exciting the core, and there is just no reason the density levels of this type should be the same as the level density, say, of collective levels of various sorts, and so forth.

CHAIRMAN SNELL: I am sure that this was what Dr. Goodman had in mind when he qualified this comparison.

GOODMAN: Yes, I certainly agree with that. The data of Rubin show very few levels at low excitation. The whole statistical analysis would be meaningless at that point anyway. Actually, the only part of the comparison that might possible be meaningful would be up around 11 or 12 Mev.

COHEN: For instance, if you look at the spectrum from the (d,p) reaction, and from the (p,d) reaction the sign of the exponential is different.

GOODMAN: In these other cases what I tried to find was all the known levels, but this is not easy to find. The articles all report a number of levels. It is not clear when you see levels in different articles at nearly the same energy, whether they are two different levels or whether they are the same levels. Maybe I should not have said anything at all, but I didn't mean that you should take it seriously.

BREIT: I just want to say that really when the theory is developed in the usual way, the level is more a mathematical thing than the level that you observed. That is the kind of level we heard about from Dr. Huizenga. Therefore you would not necessarily observe the levels that you want to count.

J. B. CUMMING: This is more a comment than a question. We have had a number of people working on excitation functions by alpha particles, and this sort of treatment where you have a different level density between odd-odd and even-even seems to hold up pretty well. But there is one other thing that I would like to point out. For instance, the work of Miller and Friedlander showed that after forming the compound nucleus Fe^{54} you find a factor of about 40 difference between the 2n and the p,n product which is much wider than you calculate for this odd-even effect here. So that, for instance, in the Fe^{52} nucleus the level density must be quite a bit lower, perhaps by a factor of 10, than you calculate by the odd-even effect.

CHAIRMAN SNELL: Any other remarks?

N. ROSENZWEIG: Your interpretation is really quite consistent with Newton's, as you have mentioned. If you want to compare the level density of an even-even nucleus with that of an odd-odd nucleus, you are supposed to subtact twice the pairing energy. This is something that he inferred in the data on neutron resonance scattering, and I might just remark that recently, in a paper by Torlief Erickson, in Nuclear Physics, a certain amount of rationale is behind this procedure. He considers a very simple model, to be sure, but one which nevertheless takes into account the energy required for breaking up pairs, and in this way he actually arrives at this rule on theoretical grounds of this sort.

G. D. FREIER: I noticed on your dE by dX versus E oscillograms there was a bright spot on the proton curve. I didn't see anything like that in the data.

GOODMAN: Actually that picture was not taken from the same experiment, but I can explain what the bright spot is. It is due to knocked out protons from the hydrogen on the target from the pump oil break down. I probably should have mentioned in the course of this talk that one of the reasons for using a gas target in this is that we have a

good deal of trouble with solid targets due to the carbon buildup and hydrogen with it, and since the experiment is supposed to compare carbon, nitrogen, oxygen targets, we have a difficult time if we don't do something about this. By using a gas target and setting counters so that they don't see any surface on which pump oil is built up, we get around some of this problem.

7. ANGULAR AND ENERGY DISTRIBUTIONS OF PROTONS AND ALPHA PARTICLES FROM N-O AND N-Al REACTIONS

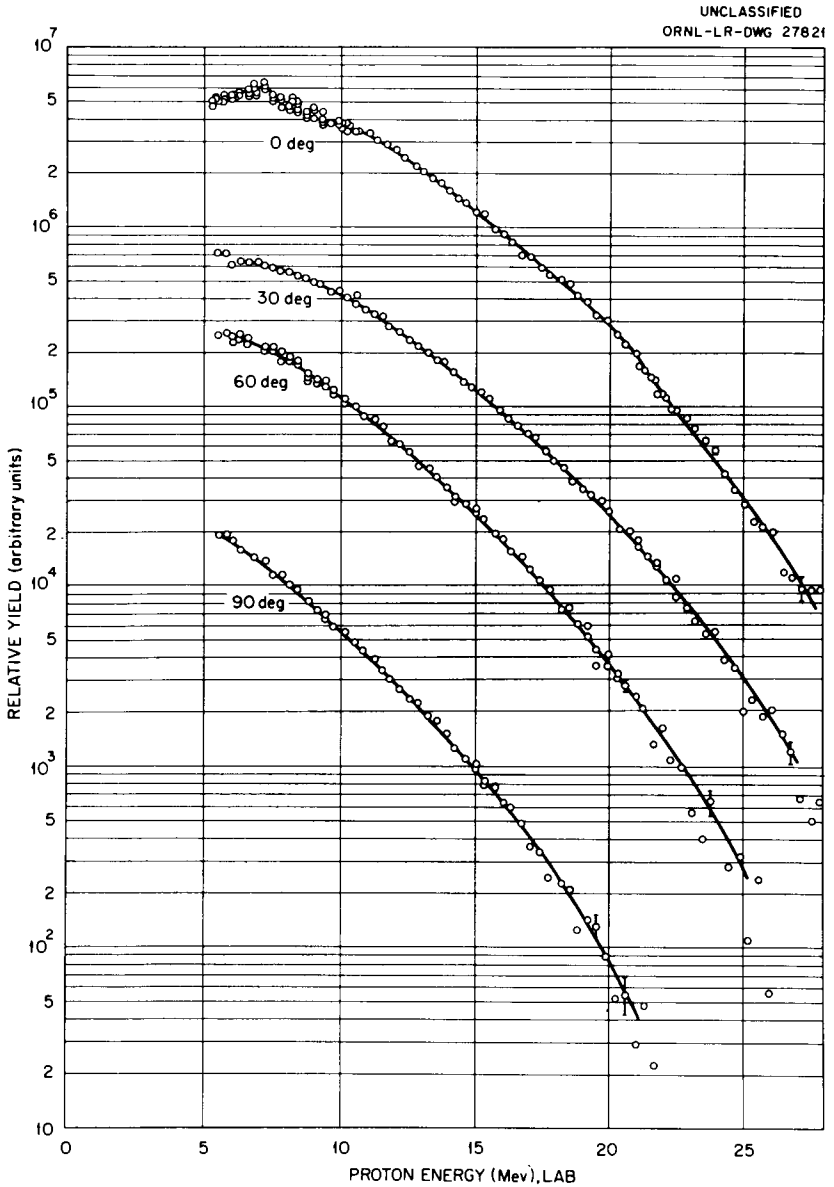
A. Zucker
Oak Ridge National Laboratory

I will present some results that have recently been obtained with nitrogen-induced reactions in aluminum and oxygen. I don't have to go through the experimental details because those have been covered very well by Goodman. I should say that the setup which I will discuss differed from the one he described in that we were able to measure angular distributions of light reaction products, particularly alpha particles and protons, which I will describe, and that we used solid targets throughout. Difficulties in gas targets arise when you take angular distributions.

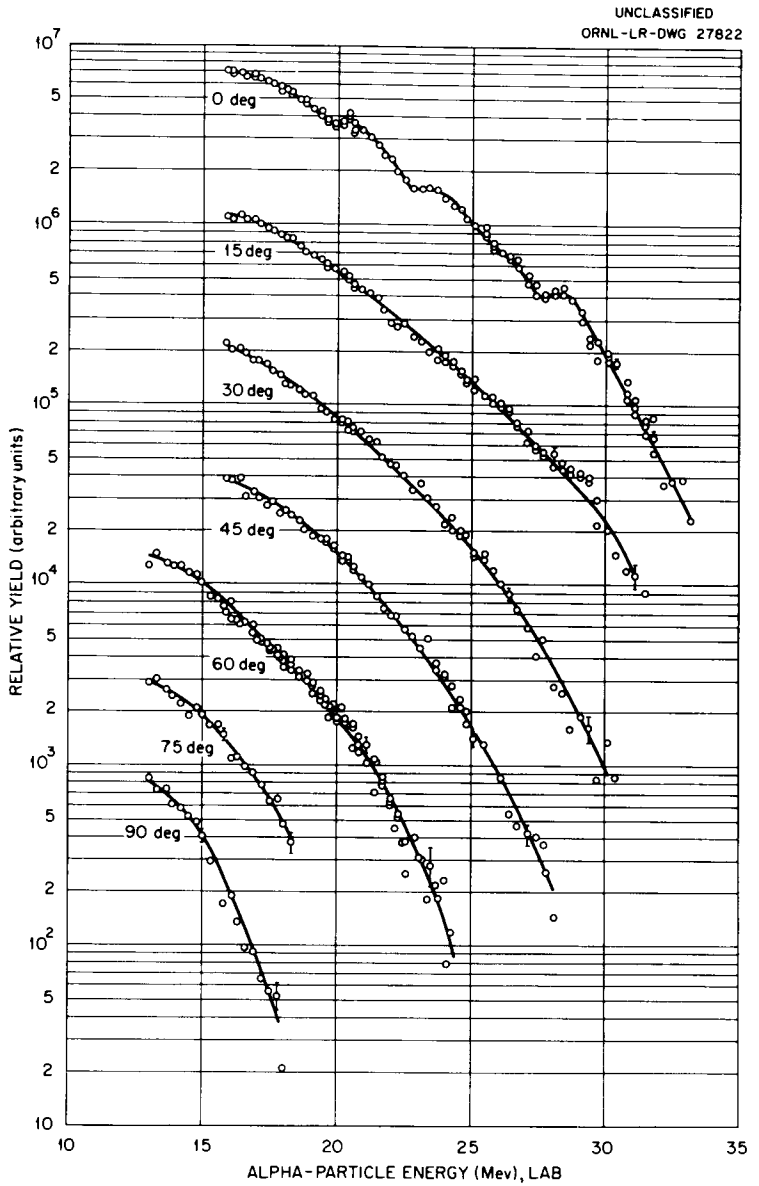
The experiment was done by holding the target in a reaction chamber; the light particles got out of the reaction chamber through a thin Mylar window and entered our proportional counter - scintillation counter telescope in the manner shown by Goodman. I should point out that the scintillation counter part of the apparatus was calibrated continuously, every hour or so, with gamma rays from Zn^{65}; so that you could effectively repeat your results weeks after you had run them. The counter and the amplifier, the pre-amplifier -- all were taken into account in the gamma ray calibration. This proved to be very nice. When you had to stop your runs and let someone else use the cyclotron you could go back and just start where you left off before.

The aluminum target was a 360 $\mu g/cm^2$ foil which contains essentially negligible amounts of oxygen, but picks up pump oil. The oxygen target was copper oxide, thin copper foil heated in an oven. Slide 1 will show the protons from oxygen. (I have a number of slides of this nature.) This is the proton energy spectrum. This is just the raw data with relative yield at various angles. And I think this is that proton that someone asked about before. It appears right here. It will be a little stronger in the aluminum case. There is nothing essentially startling about this.

Slide 2 shows a rather different set of curves. These are the alpha-particle spectra from oxygen at 0, 15, 30, 45, 60, 75 and 90 degrees.



Slide 1



Slide 2

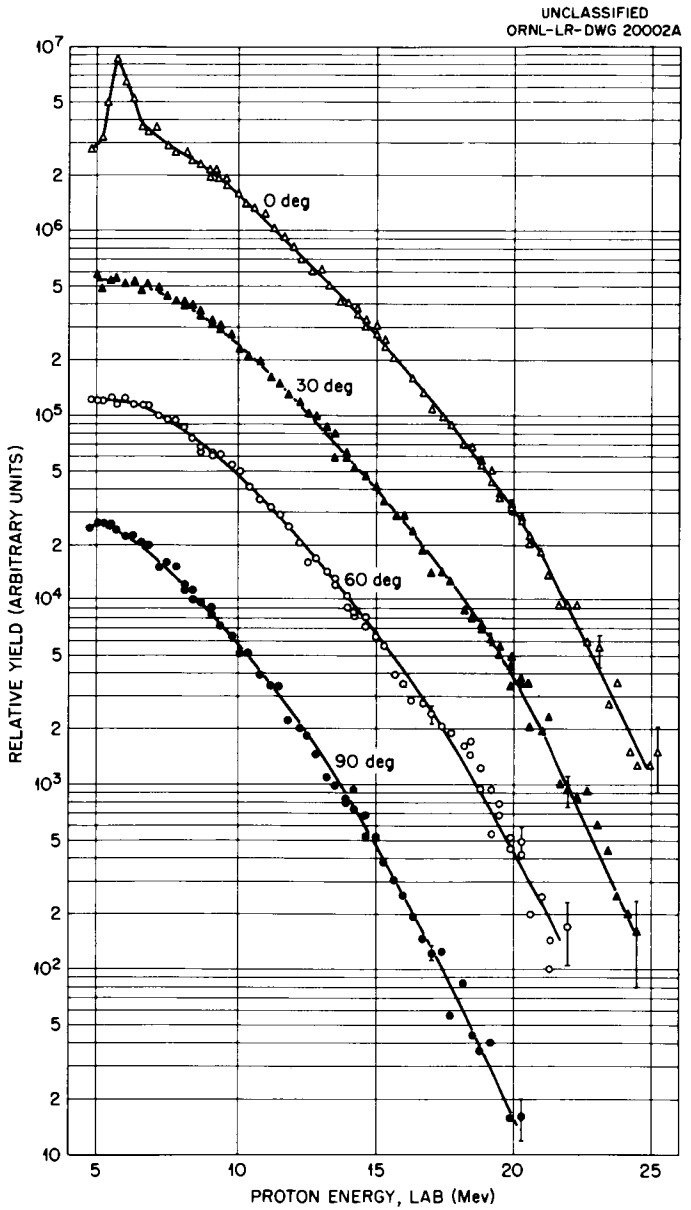
At zero degrees you will see that there is structure, and that was quite unexpected. You will see later that this structure does not appear in aluminum, and I still don't know to just what it is due. The fact is that these humps are alpha particles, and they are all alpha particles, because when you place absorber between the counters and the target, these peaks move down just as alpha particles should, and these peaks come from the target. If you put a different target in, and with no target, these peaks disappear. Whether there is structure here, (at 15 deg) I really don't know. It is our practice in this kind of an expermient to plot all the data, then draw a smooth curve through the data and work on that smooth curve, except in this alpha-particle spectrum at zero degrees where, with the strongest bias, it is difficult to draw the smooth line. (And we have some pretty strong biases.)

Now one explanation for this is that at zero degrees your angular acceptance is much smaller, because you are looking at 0 deg plus or minus, in this case 3 deg; so that the total spread in angle is only 3 deg whereas here the total spread in angle is ± 3 deg, or 6 deg. Of course, another thing is that the energy is not likely to be washed out at 0 deg, because the laboratory energy dependence varies as a function of the excitation energy. So it may be that the structure is really present in these other curves, but it has been washed out due to the large angular acceptance, and, of course, also due to the not very satisfactory energy resolution of the scintillator.

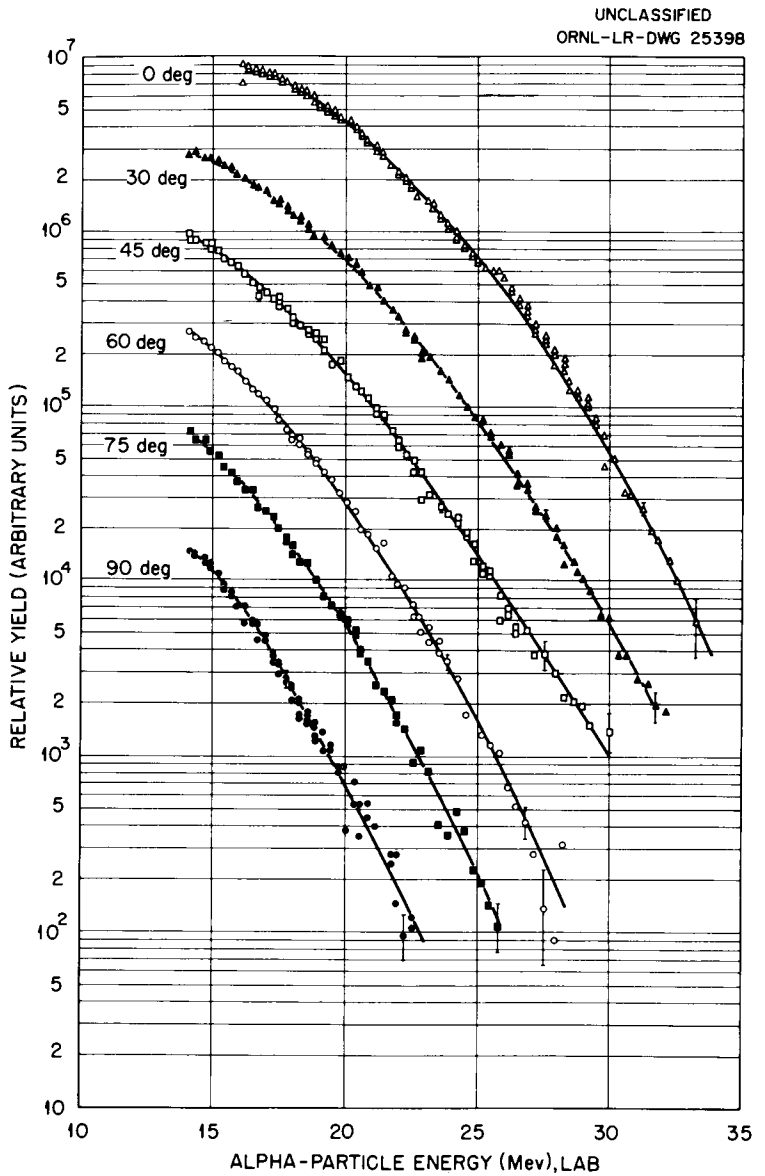
Slide 3 shows the proton spectrum from aluminum, and here you see the proton recoil peak very markedly but, as you can see, it really won't make any difference over most of this region. The errors are indicated in typical cases.

Slide 4 shows the alpha-particle spectrum from aluminum, and here you see there is no structure. One could imagine a peak here and maybe there, but we have chosen to ignore it.

Now in the next series of slides I will show the data treated in the following manner: The simple statistical model which we used for analyzing our data, and which meets with disapproval in general, is that the number of particles of energy ϵ is proportional to the energy times the capture cross section times the level density. The expression for the level density is some constant times exp $\left[2\sqrt{aE}\right]$. I am writing this



Slide 3



Slide 4

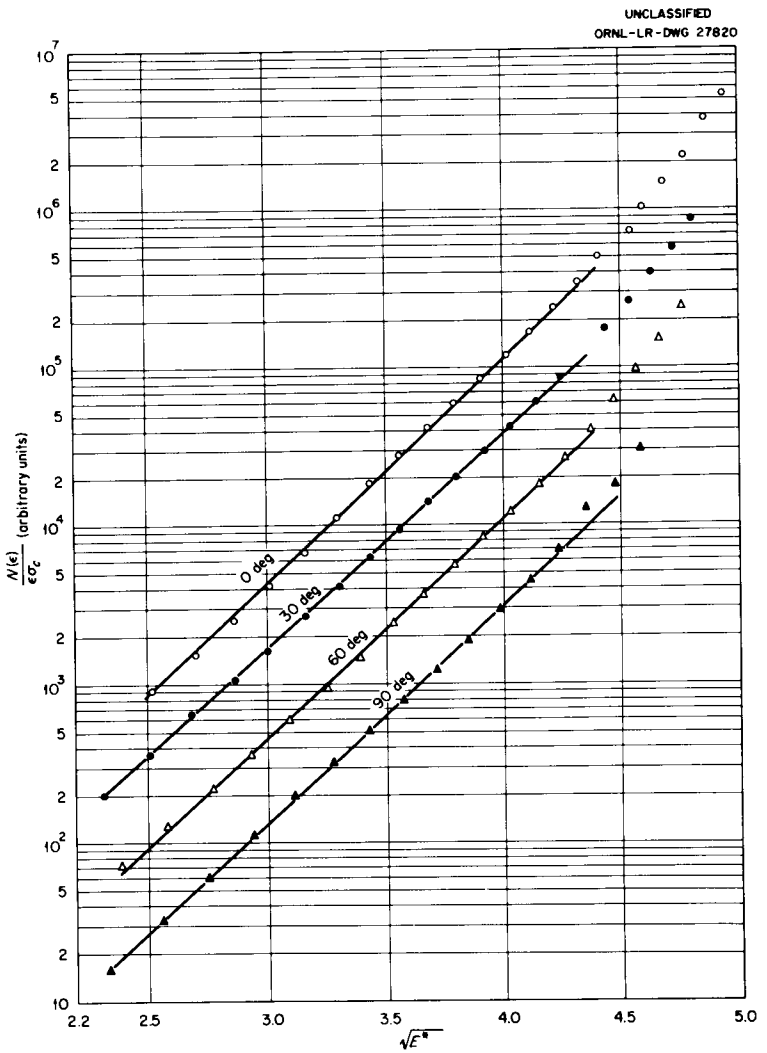
down not so much in defense of a theory which I am not attempting to make, but in order to define my terms. Another reason we use this kind of interpretation is that we want to treat all our data essentially in the same way, so that we can see any systematic trends that are apparent. This seems to be the simplest way of representing the data; therefore, we plot the number of particles as a function of the square root of energy, and from that, if this formula is right, we get a straight line, and from the slope of the straight line we can get the value of a. Now, as you would expect, we do get a straight line which is shown on the next slide.

In Slide 5 these are protons from oxygen. I am going through them in the same order in which I have shown the raw data. The ordinate is arbitrary, and I will present angular distributions later, but these energy distributions fit a straight line relation represented by such an equation rather well. I don't think that this means that this theory is correct, but it does give us a way of talking about this kind of data. In general, a typical curve shows a deviation upwards here at low energies. The abscissa is the square root of the excitation in the residual nucleus and the slopes of these lines from zero to 90 deg are really the same. For protons from oxygen we get a value of a of about 2.5± 0.2. The way we arrive at that error is simply by trying to see how much you can tilt this line and still reasonably well draw your curve through the data.

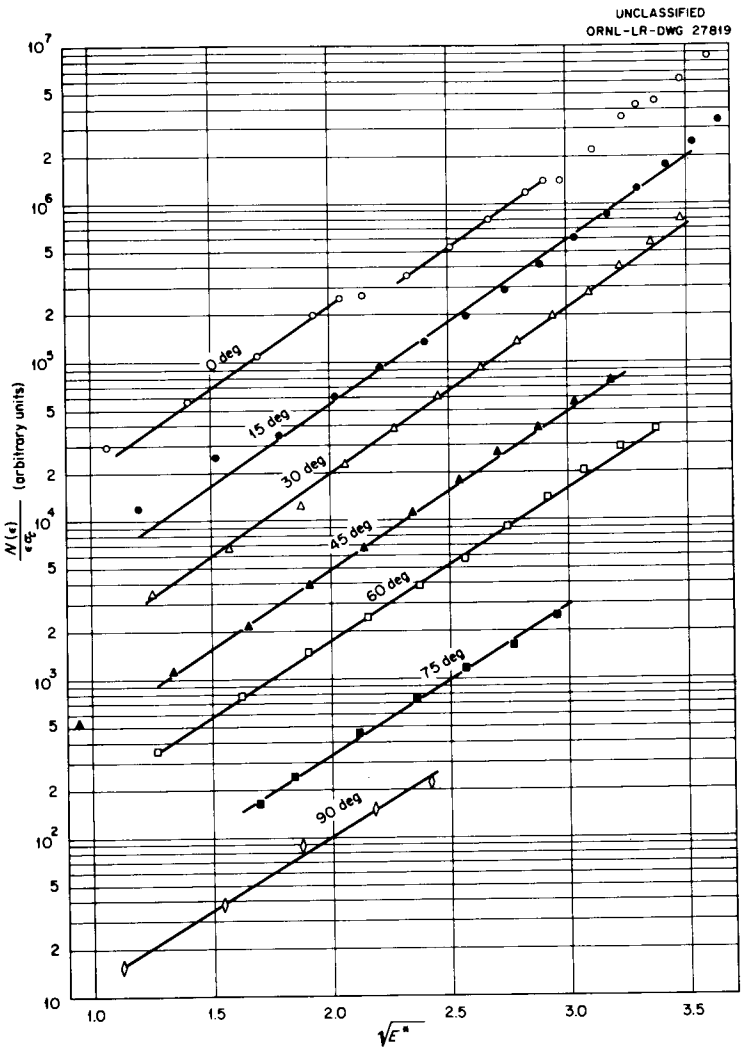
If you look closely, which not many people do, there is a slight change in the slope with angle. Apparently the forward angle seems to have a little steeper energy dependence than the backward angle. This is not outside of statistical error; so I think for our purposes we can use a = 2.5 for all angles.

Slide 6 shows the same sort of plot for alpha particles from oxygen. Now you see this discontinuity in the energy distribution also showing up here, of course. Still in spite of the apparent wiggles in this curve one may draw some straight lines through some points which have approximately the same slope as the rest of the lines.

You will notice that the low energy alpha particles do not deviate strongly from the straight line. This is due to the fact that second alpha particles are not as likely as second protons, simply because the barrier is much higher for alpha particles and tends to keep them down. Again you see that the slope at 0 deg is somewhat less steep than the slope at



Slide 5



Slide 6

larger angles. The value of the a we get is 1.3 ± 0.2, a value which is much lower than one would expect for this kind of nucleus, in particular since it should not be too different from the value of a for protons which is 2.5.

Slide 7 shows a similar plot for protons from aluminum, and here you see the straight lines pretty well. Actually the 0-deg curve bellies out here, but I think a straight line describes the data pretty well. Certainly at 90 deg it follows a straight line over a large energy region. Again we have second protons appearing.

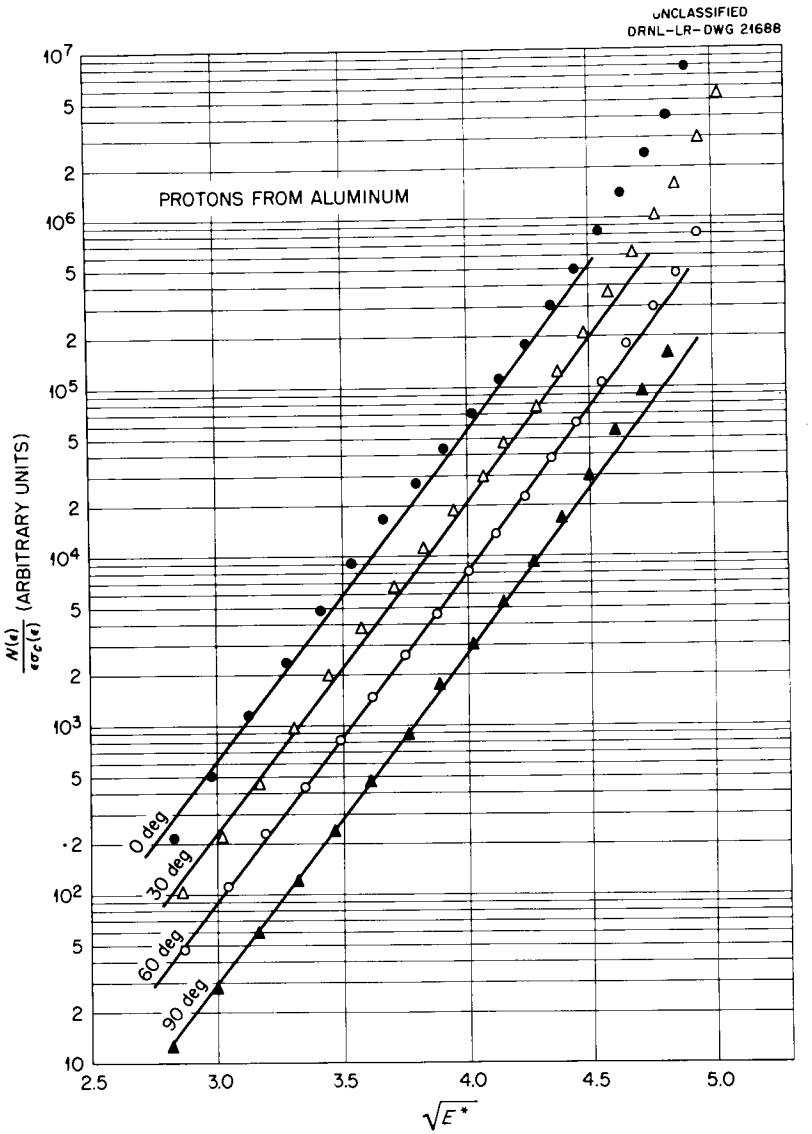
Slide 8 shows the alpha particles from aluminum. We have taken alpha particles at more angles than protons because, as you will see later, the protons seem to have a much more isotropic angular distribution. Alpha particles are a little peculiar in that respect.

I didn't say what a was for protons from aluminum. It was 5. Alpha particles from aluminum give a = 4.1 ± 0.3.

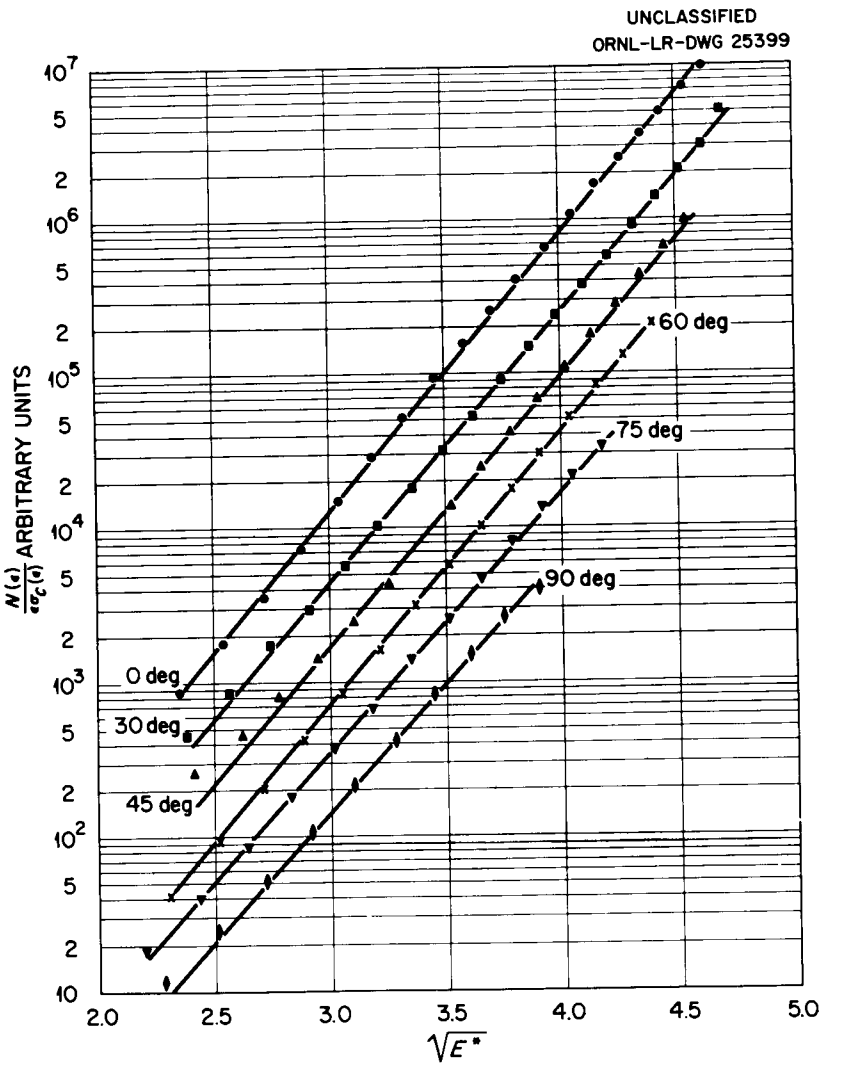
Slide 9 shows deuterons from oxygen at one angle, 15 deg in the laboratory. Here we again have a good straight line fit for this kind of curve and a value of a = 1.8 ± 0.2. This is actually the only deuteron data we have taken from oxygen. We have no angular distributions on deuterons.

The next thing, of course, one would say is to try to fit our data to a more sophisticated model for the level density. We have taken the most sophisticated model that has been published to our knowledge, I think, and that is the one due to Newton.

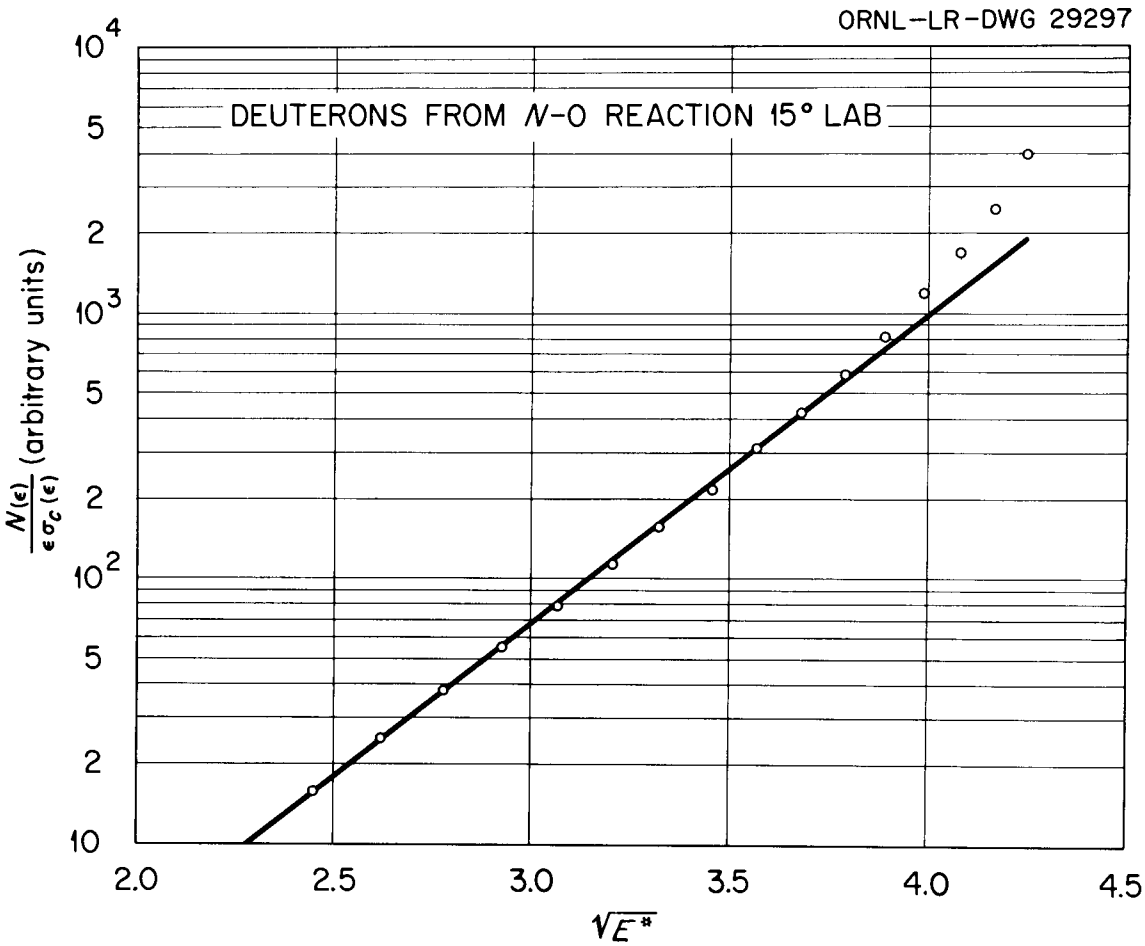
Slide 10 shows Newton's theoretical prediction where the points are experimental values. The two have been normalized at 15 Mev. These are protons from aluminum, I believe. You can see that the fit to Newton's more complete level density formula is not nearly as good as it is for this very simple formula. You can also see that it is not true what many people say, that no matter how you plot data on semi-log paper you get a straight line. However, I think this does not mean that Newton's work is wrong. It simply means that this fit is made for only zero spin, and that if you added other spins, it would tend to raise the high energy part of the theoretical curve. The fit might, in that case, be quite good. However, we don't know where to begin a particular spin value in the residual nucleus in order to add it in, and if we did know where to begin, we don't know if it goes as (2I + 1). It probably does not go as (2I + 1), and in this sense this is probably not a fair test of the Newton theory.



Slide 7



Slide 8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DEUTERONS FROM N-O REACTION 15° LAB
$\frac{N(\epsilon)}{\epsilon\sigma_c(\epsilon)}$ (arbitrary units)
10^4
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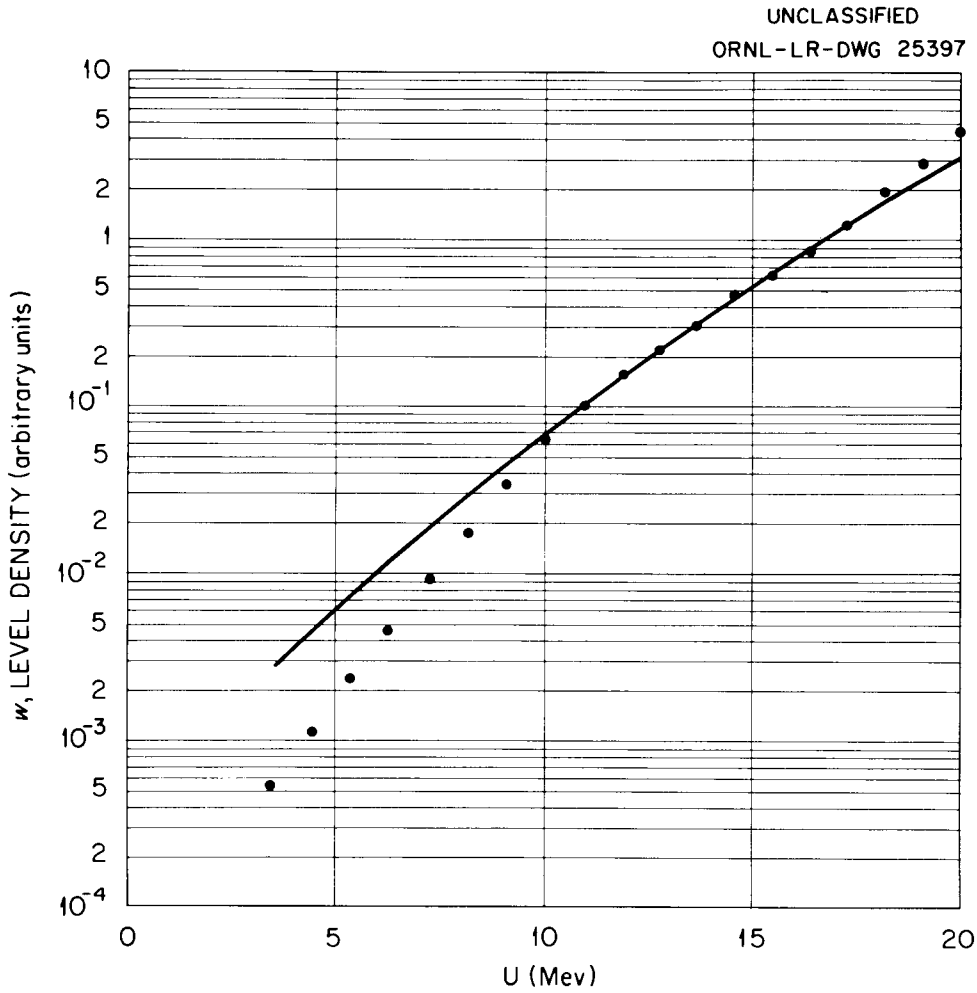
4.0
4.5
$\sqrt{E^*}$

Slide 9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w, LEVEL DENSITY (arbitrary units)
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5
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15
20
U (Mev)

Slide 10

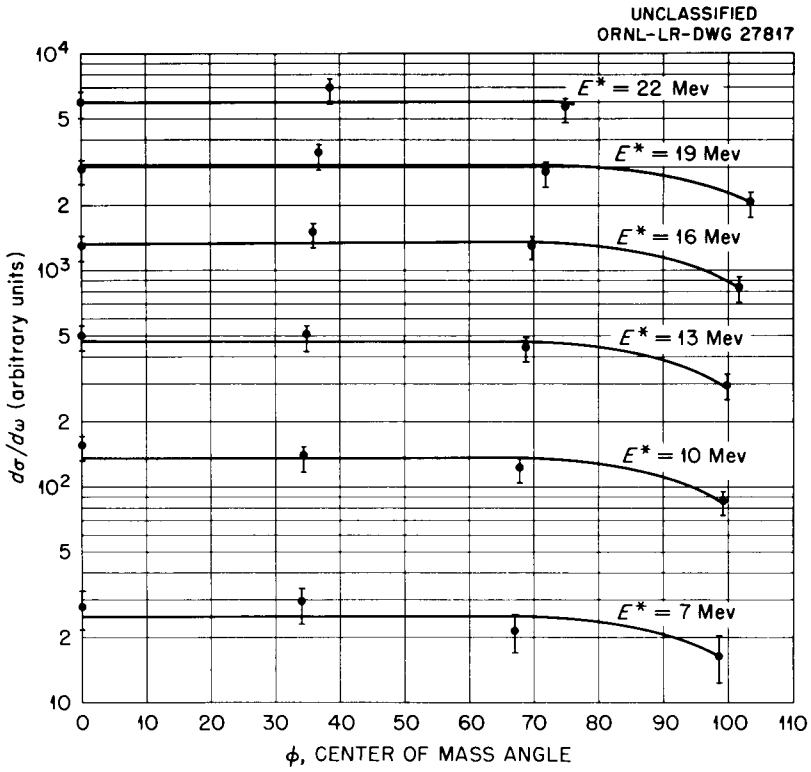
If we could back up to Slide 9, I would like to point something out. In a curve of this sort one thing must be borne in mind. When we talk about a compound nucleus, we say that most of our reactions go through a compound nucleus and that direct interaction is neglibible. Many people have done this kind of experiment, say with alpha particles or protons; they get very good straight lines like this, and they say if the measurement is made in a backward direction, a straight line of this sort indicates that you have a compound nucleus from which you can get information about the statistical theory.

However, you will notice that if a very small amount of direct interaction manifests itself in a few high energy particles these are added in here. Suppose you have 5% of the total reaction cross section as direct interaction. This would still be unimportant so far as the total cross section is concerned but it would all add at the high energy end. By virtue of the high energy, these particles would then tend to give a straight line of an entirely different slope, and in fact we have examined such an effect. That is, I added 5% to the total cross section for the protons from aluminum, and I was able by a judicious fit to lower the value of a from 5.0 to 2. Of course, this is a rather drastic change in the value of a, considering that you have changed the total cross section by only 5%.

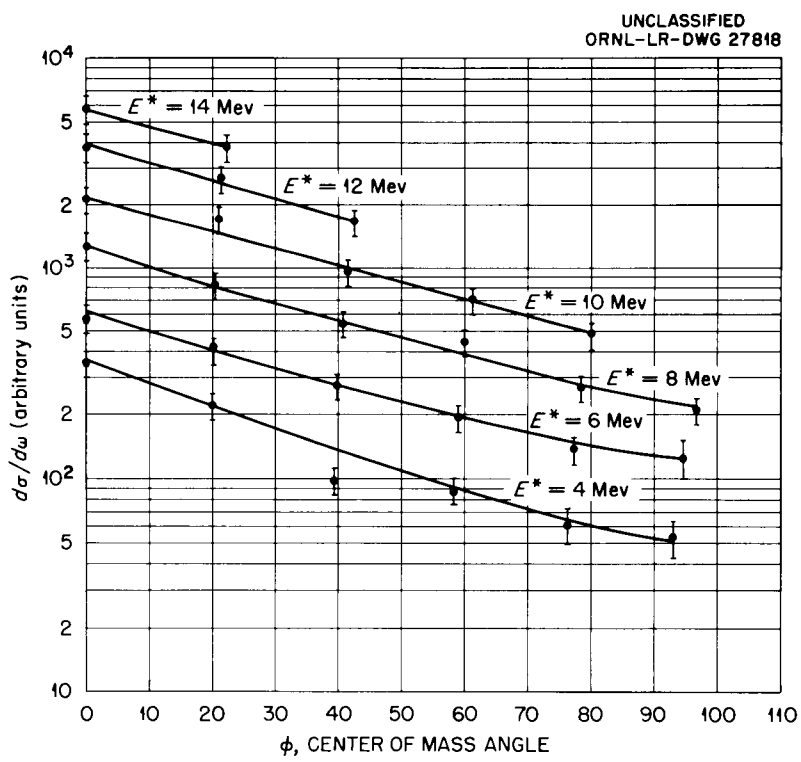
Slide 11 shows the angular distributions, this time for protons from oxygen. In this case the ordinate is meaningful. That is, the relationship between these points is true, and we plot here the differential cross section, and on the abscissa we plot the center-of-mass angle for protons, leaving the residual nucleus excited to 10, 13, 26, 19, and 22 Mev. The angular distributions are pretty isotropic; they seem to drop off at 90 deg, but I don't think that is a really very significant thing, and certainly in a case such as this it may be just due to the errors.

We have considerable normalization problems. When we run it takes a couple of days to run 30 deg and then to run at 90 deg. But it seems true in this case, as it will in the others, too, that at high excitation energy, that is, low energy protons are essentially isotropic and any anisotropy becomes more pronounced at higher energy.

Slide 12 shows the angular distribution of alpha particles from oxygen targets. Here again you have the same sort of plot. Here the



Slide 11



Slide 12

energy in the residual nucleus is 4, 6, 8, 10, 12, and 14 Mev. We don't go quite so high, but there is no question, I think, that the angular distributions are peaked in the forward direction, and in fact, Al^{26} is an odd-odd nucleus, so that there are lots of levels near the ground state and you get density even as low as 4 Mev. Usually you get practically no transitions to such a low state of excitation, but the anisotropy in the angular distribution seems to be of about the same order, or perhaps a little larger, for very high energy alphas. It is peaked all the way up to about 14 Mev excitation.

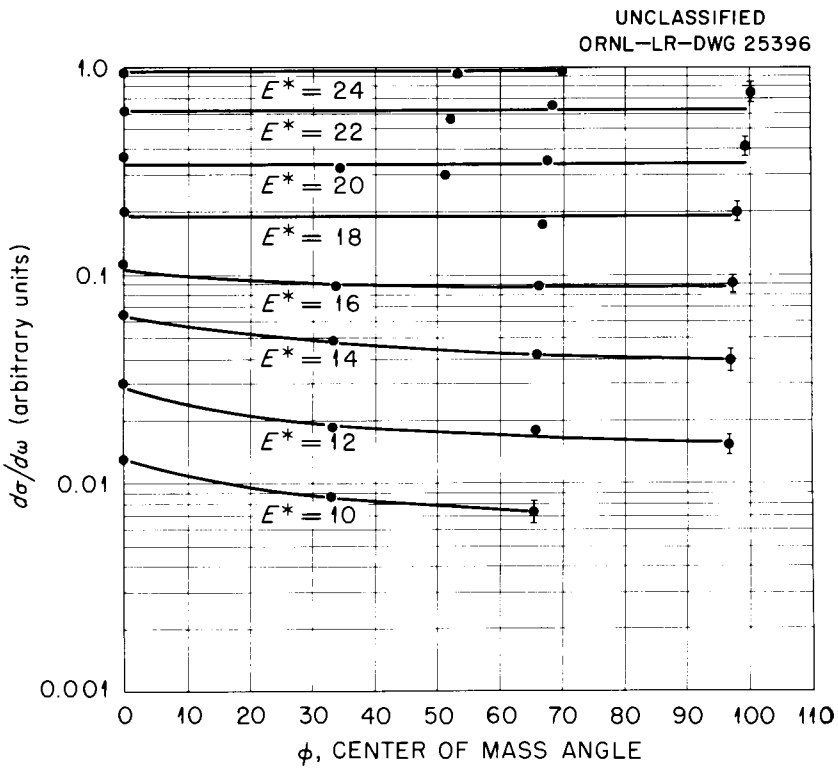
Slide 13 shows protons from aluminum. Here again is the same sort of plot and the angular distributions are isotropic essentially down perhaps to 12-Mev excitation; at 10 Mev excitation, I think there might be some forward peaking. Even the forward peak does not mean that there is not a compound nucleus formed, as we heard earlier this afternoon, but the extent to which this kind of thing is significant I think is not clear at the moment.

Slide 14 shows the same sort of angular distributions for alpha particles from aluminum, and here things are generally more isotropic at low energies, and there seems to be a trough here at something like 70 deg in the center-of-mass for high energy alpha particles. If you look at the errors you can just as well draw a straight line, however.

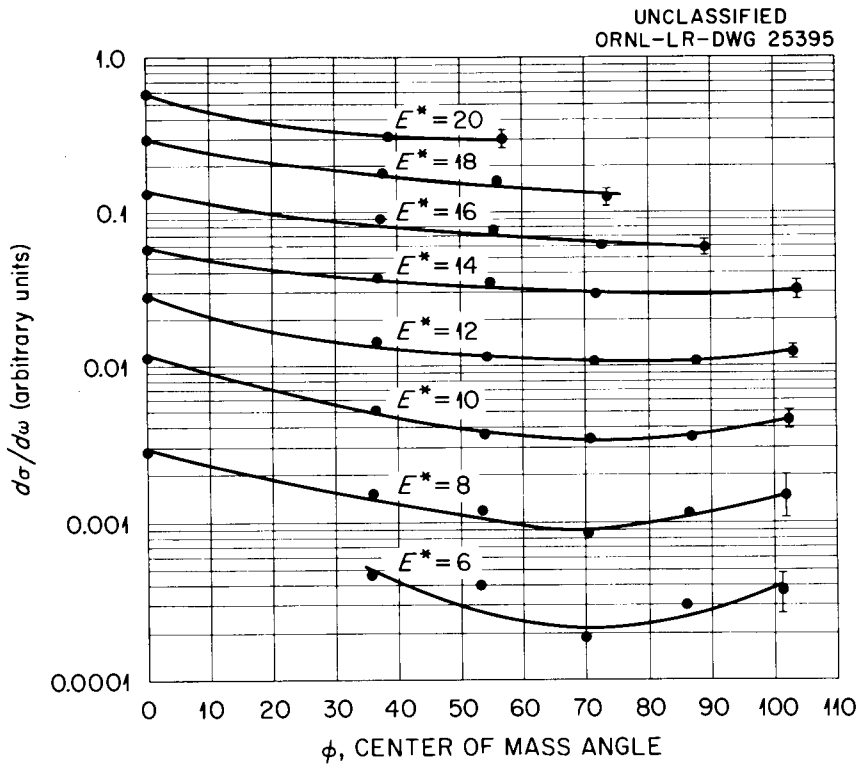
There is another piece of information that I would like to add here, and it is essentially similar to what Goodman discussed. That is the ratio of the level density from the aluminum bombardment; that is, nitrogen plus aluminum going to a compound nucleus of calcium 41, and going by proton emission to potassium 40, and by alpha particle emission to A^{37}. Then we ought to be able to get an idea of the level densities in these two nuclei from the number of protons compared to the number of alpha particles. We find that we can do this, and the level density of A^{37} compared with the level density of K^{40} is 1/2.5. Now A^{37} is an odd-A nucleus and K^{40} is an odd-odd nucleus; so you would expect more levels in K^{40} and, indeed, one finds that.

Now in defense of Newton's formalism, I might add that if you follow his prescription you get a ratio of 1/2.13, and this agreement is much better than one has any right to expect.

I would like to spend the rest of my few minutes just discussing the general state of the statistical theory.



Slide 13



Slide 14

Slide 15 is essentially a summary of the work done at Oak Ridge. We have bombarded lithium, beryllium, nitrogen, oxygen, and aluminum-27. We have looked at the energy distributions of the alphas, protons, and deuterons. Here are listed the values of a that we have observed and the values of b. I think Goodman explained the meaning of b. Now I would like to draw your attention to the case of F^{19}. It appears as a residual for the Li^6, p reaction, Li^7, d and Be^9, α. That is, three particles are emitted and three different targets are involved, and in each case we get a value of a = 1.1. Again the agreement is much better than it has any right being, but we think that this may justify our hypothesis that nitrogen reactions, at this energy, which go to light particles go through a compound state.

Slide 16 shows a sort of worldwide compilation of the constant a. Unfortunately, this is a little different constant a, being a factor of 4 larger than the one we employ. It is taken from a paper by Igo, Wegner and Eisberg. Here you see that there seem to be various schools of thought about a. There seems to be one set of values which rises with the atomic number. There seems to be one set of values which stays level at about 8, or in our case this a = 2. Then there seems to be another school with about half of that or maybe two-thirds of that. This kind of relation of a with the atomic number is obtained from 150 Mev data. That is, these points here are obtained from counting particles in stars by Skyrme and Williams, and this has been analyzed by Lang and Le Couteur. The (α, p) reactions at 40 Mev are these diamonds. They do the same experiment we do; they measure energy distributions of protons at about 150 deg in the laboratory and they get values of a which are much lower than anything we get but they get beautiful straight lines just as we do.

The excitation function measurements are these black points, and they lie generally low. Notice the excitation function measurements are with gamma rays, with neutrons, and with alpha particles. They are done by Porges with alpha particles (α, pn). He gets an a of 2, in our notation. Brolley, Fowler, and Schlacks get a = 2 as does Bleuler. In all those cases the dependence of a on the atomic number seems to be too small. That is the value of a should be A/11 or A/8 depending on details given by Lang and LeCouteur. Still a should have some strong dependence on the atomic number, and in many of these results this is not so.

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TARGET	RESIDUAL	PARTICLE	a	b
Li^6	F^{19}	p	1.1	0
Li^6	O^{16}	α		
Li^7	F^{20}	p	1.4	0
	F^{19}	d	1.1	0
	O^{17}	α		
Be^9	Ne^{22}	p	2.6	4.9
	Ne^{21}	d	1.4	0
	Ne^{20}	t	2.6	4.9
	F^{19}	α	1.1	0
N^{14}	Al^{27}	p		
	Al^{26}	d	2.2	
O^{16}	Si^{29}	p	2.5	0
	Si^{28}	d		
	Al^{26}	α	1.3	0
Al^{27}	K^{40}	p	5.0	0
	A^{37}	α	4.1	0

Slide 15

Slide 17 is a graphic summary of our work. Here is that F^{19} point, and you see a generally rising trend, which is somewhat steeper, I should say, than the one predicted by Lang and LeCouteur, but it shows that even with heavy ions the individual differences are important; that is, here are the alpha particles from oxygen and here are the protons from oxygen and so there is really a large difference which shows up on this kind of a plot very well.

These are a couple of even-even nuclei which seem to fall off in some kind of an imagined line. This is Ne^{20} and Ne^{22}; so that I think interpreting data of this sort should be done with caution. One thing that might by very important is the fact that the target nucleus after all may have some organization; that there may be some kind of an alpha particle structure in an oxygen nucleus and that is why alpha particles are emitted so profusely from oxygen, high energy alphas. I think that what one needs is more data at higher atomic numbers, the region in which we are prevented from working due to the Coulomb barrier. I hope that with high energy machines this kind of study can be extended.

CHAIRMAN SNELL: Are there any remarks about this set of data?

G. R. SATCHLER: You said your angular distribution was peaked in the forward direction. Do you have any information about the backward direction?

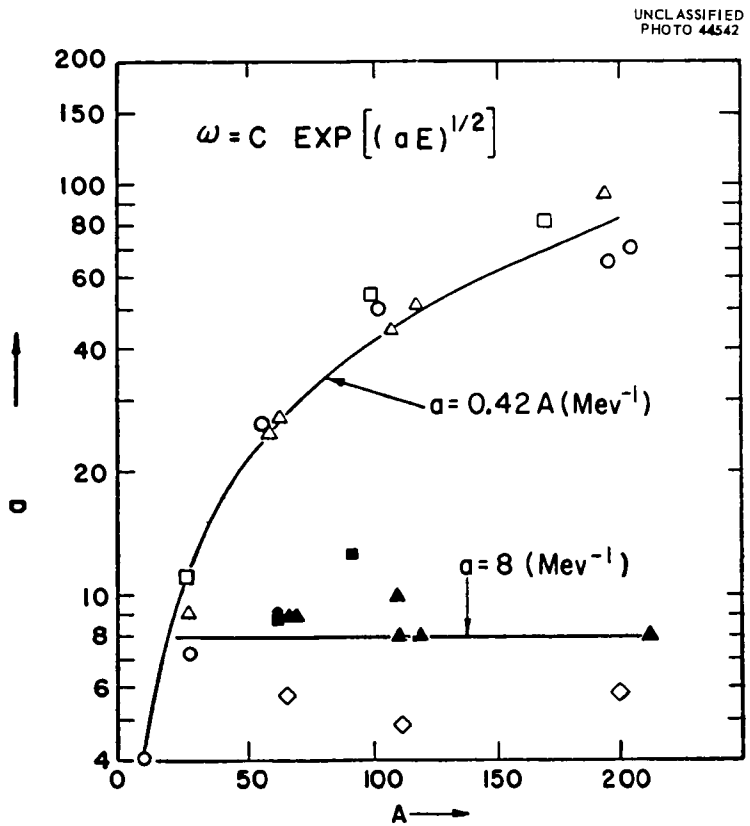
ZUCKER:. No.

SATCHLER: It is 90 deg in the laboratory and that is about 110 deg in the center of mass.

J. SCHIFFER: Your σ_c entered into the calculation, presumably based on some sort of an optical model. Could you say what the parameters were in the potential? Are they based on some average potential?

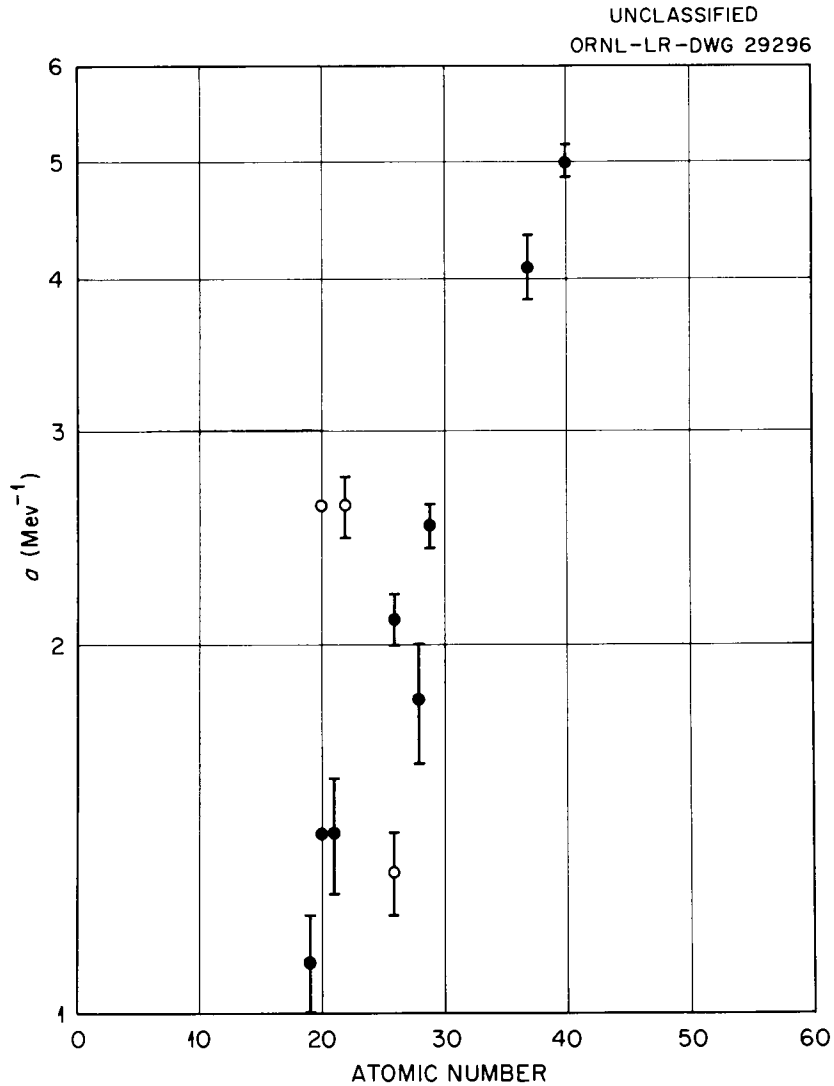
ZUCKER: They are not optical model σ_c; they are just Shapiro's σ_c. They are really based on experimental values and they are experimental fits, I think. Besides that, they don't make any difference, because most of our work is with low-Z nuclei, so that the barrier is low. For low energy particles σ_c may amount to a 20% correction, but the low energy particles are not the ones that determine the slope of the line as strongly as do the high energy particles.

SCHIFFER: Those are based on a potential, I believe, and this is believed to be experimental data.



Compilation of energy level density measurements: (a) Reaction and inelastic scattering: ○ (p,n) at 18 Mev, △ (p,p') at 18 Mev, □ 150-Mev data, and ◇ (α,p) at 40 Mev. (b) Excitation function measurements induced by ● γ rays, ■ neutron and ▲ alpha particles

Slide 16



Slide 17

ZUCKER: I don't know. They are actually based, I think, on experimental evidence rather than a model. How they are calculated is a different story, but they fit experimental data.

R. B. LEACHMAN: I noticed that this morning most of the discussion was based on the level density distribution based on constant temperature. After lunch it is based on the square root of the temperature. I was wondering if there is a better fit this afternoon. Do your data fit the constant temperature expression?

ZUCKER: No.

CHAIRMAN SNELL: Your curve showing the deuterons had the same kind of an upturn as the low energy emitted particles. Do you interpret this the same way, that they are second deuterons coming up?

ZUCKER: No, I wouldn't call them second deuterons. An excess of deuterons at low energies may be due to the fact that the N + O reaction going to the compound nucleus P^{30} deexcites by neutrons and then emits a deuteron. They are not second deuterons in the sense that 2 deuterons are cascaded.

H. L. REYNOLDS: A very naive question. I wonder if someone, perhaps Dr. Zucker or maybe someone else, could say a few words about why one would expect a statistical model like this to apply with this relatively small number of nucleons. Did Newton, for example, intend his theory to apply to these nucleons?

G. BREIT: I don't know that I can answer this in any real sense, but in statistical calculations sometimes a rather small number may be considered to be sufficiently large for a statistical treatment. I mean that it is just a matter of practice somewhat connected with the way in which survey formulas for reportorial purposes seem to be a good approximation for rather small numbers.

Also I don't know whether this really means that the basis of the theory is right. The dependence on A is not working out very well, but it seems to be working out as well as the dependence on the energy of the residual nucleus. Of course, this raises the whole question of what does the compound nucleus mean, and perhaps one might wonder as to whether it means the compound nucleus in the sense of some kind of a spherical blob of the kind that one usually thinks of in connection with neutron bombardment, or whether it just means that there is a system in which a set

of residual states are determined by barrier penetrations and some slowly varying factors, and it seems to me, looking at the data, one cannot go beyond that.

N. ROSENZWEIG: The statement by Prof. Breit with regard to the analogy as to the Sterling formula has actually been verified in this connection: If you calculate the number of ways in which a certain state can be formed, even though say only 2, 3, or 4 particles may be involved, it already leads to this characteristic square root of the energy which is just characteristic of the fact that you are dealing with a degenerate Fermi system, and even for a small number of particles you already get this.

I don't wish to take complete credit for this because, as a matter of fact, the history of this is that several years ago Claude Bloch solved the problem,for, in fact, neon with that many particles, and compared his results using the method of statistical mechanics, in particular the Darwin-Fowler method, and compared this with an exact count, an explicit count, made by Oleksa and Critchfield. More recently, I considered a simpler model, which consists of uniformly spaced levels and allowing only a fixed number of particles in each level in the ground state. That is, assuming that the degeneracy is the same at each level. I could solve the analytical problem with high accuracy in a rather simple way and compare that again with an exact number which I obtained by counting and even for excitation energies of 1 unit. That is, allowing just one particle to be raised to the next level in a shell model, the results are really very accurate.

BREIT: Where can one look that up?

ROSENZWEIG: In the November 1957 Physical Review. I believe Bloch's paper was published in 1954, March or April.

J. S. BLAIR: A question this time. Do you know if one gets a more consistent picture for the dependence of a on A if one limits oneself to analyses of experiments where the reaction particle is known to be isotropic?

ZUCKER: Well, that goes back to that slide I had and I cannot answer it all at once. You can sort of go through that data and throw out experiments that are misleading because they are not isotropic. This eliminates half of them. Then you can throw out the points that are excitation functions where nothing is known about their angular dependence, and this

eliminates them all. Particles emitted in general from high energy reactions are not isotropic. So the part used is in the backward direction. For 150-Mev protons one analyzes stars which are not at all isotropic, but there you subtract out the direct component.

J. B. CUMMING: A question. You apparently showed no b.

ZUCKER: It is zero.

CUMMING: Well, for instance, could the A^{37} to A^{40} ratio be interpreted as being the same a but with a b, say of 2 Mev? Would that be outside the error?

ZUCKER: Yes. You can see that with b the line would curve. I show a straight line like this, then you put a b of 2 in and it looks worse. So it is questionable just what b will do. It is difficult to pin it down, but I think it is certainly 0 ± 1, something like that, and I don't know what that would do to that ratio.

B. L. COHEN: How well does the statistical theory predict the relative probability for emitting protons, deuterons, and alphas in your experiments?

ZUCKER: I discussed protons and alphas. The rest you heard was from Goodman.

COHEN: What about the deuterons?

ZUCKER: Well, it is all right. It all depends on what you call an odd-even difference. Is that what you mean--odd-even effects? That is the thing that enters, not the statistical theory which has the energy dependence in it.

COHEN: Within a factor of 4 or so.

C. D. GOODMAN: The data I showed before doesn't seem to show any difference. It depends on the type of particle that you look at. If you remember, the relative positions of the protons, deuterons, and alphas seemed not to depend on whether they were protons, deuterons, or alphas, but rather on the character of the neutrons.

ZUCKER: Maybe I have the answer. It does not matter what the particle is? Is that the question you have? If you put in a level density according to the statistical model, the energy and the barrier, then you get that number of particles independent of what they are, whether protons or alphas.

COHEN: For the same residual nucleus?

ZUCKER: No, not the same residual nucleus, but for the same target.

COHEN: It should depend very extensively on the energy available.

ZUCKER: It does.

COHEN: And the data follows this?

ZUCKER: Yes, in other words an alpha particle is just as likely to come out as a proton, that is, provided the levels are there and you can get through the barrier, and it requires no pre-formation. We never noticed any need for pre-formation.

BREIT: But must you throw out the difference in the residual nucleus the way Goodman mentioned?

S. DEVONS: I would like to ask a question based on Dr. Breit's remark to the previous question. As I understand it -- I must confess that I am an outsider and that I have never measured your a or b, so I am a little confused on this point -- there are two distinct things as I see it. One is the criterion of applying the statistical treatment. This depends on some number of degrees of freedom, and the mathematics do not require a very large degree of freedom. Then the other question is how many degrees of freedom are there which may not be significant in some analyses? Dr. Satchler pointed out that the time available for equilibrium in all these processes is rather marginal; therefore, you might expect to get processes which can be analyzed and agree with the statistical freedom even though you get different reactions; the number of degrees of freedom involved is quite different.

This does not say that the statistical calculation is wrong. It simply says that there has been a difference between the number of degrees of freedom if one uses very different ways of making the whole compound system; therefore, if you take two half nuclei and put them together you might expect different nuclei.

ZUCKER: Yes, but they are dependent on A. That the value of the energy dependence of the level density is a function of atomic number is really the thing.

S. DEVONS: I still don't understand. That is just what I was thinking would change, that you excite different classes of levels in the two cases, and the experiment will have enough degrees of freedom to be statistical, not necessarily--

ZUCKER: I am not sure this answers your question. One way of explaining a constant a is to say that you are bombarding a nucleus with a proton, and you are heating up a small part where you get a statistical distribution. Now this portion that is being heated up statistically, so to speak, is the same size no matter what the nucleus is. So you would expect, say, no difference in a. This is one way of saying "direct interaction" without saying the words.

DEVONS: Well, I would be surprised if you could figure a as a very simple configuration in this geometrical space. I mean, you must be a little more subtle than that.

ZUCKER: This is no defense of the theory, but it has to do with times and it has to do with the fact that these are high-speed particles. They are high-speed particles compared to 20-Mev nitrogens. I don't know if this has any bearing on what you are asking.

DEVONS: I am still not clear what your answer is.

ZUCKER: Maybe I can answer you if you ask another question.

DEVONS: In the presence of the statistical phenomena, with the same total collection of neutrons is there direct evidence that the number of degrees involved is the same when you get statistical appearances? It does not have to be obviously.

G. BREIT: What is meant by the number of states? I am bothered by that in your question. Do you mean states of the compound nucleus system or do you mean states of the residual nucleus? Which is it?

DEVONS: Well, the states of the residual nucleus are part of the states of the final system. To get it statistically you have to have a mechanism which is fast enough to get there. The mechanism may be selective.

BREIT: Zucker pointed out that perhaps it is not the whole system that comes to a statistical equilibrium but part of it. There might be something left, local heating. Whether it is confined in the geometrical space or some other kind of space would not affect the final answer.

DEVONS: But I am a little surprised that one would expect that, just because you have evidence of a statistical process you would expect the equilibrium to be complete.

BREIT: I don't think from talking with the speaker the idea was that it is complete. In fact, the whole point of view seems to be rather

that this usual statistical approach gives one a way of taking out of the data the major dependence on energy and other factors, and then representing the results in such a way that one can focus one's attention on a and b and then leave for the future what that means. I don't think that the idea is a belief in the full validity of the statistical view. Isn't that right?

ZUCKER: Well, this is an experiment and we are trying to find out something, that is the behavior of nuclear matter when it is excited to 40 Mev. One of the things you are trying to find out is, how are particles emitted. You make certain experiments and see how they fit some assumptions, but really whether this has reached complete equilibrium or not, I don't think we have a way of finding out. We see certain spectra. That is actually all we see. I think that is essentially what you said.

BREIT: Yes.

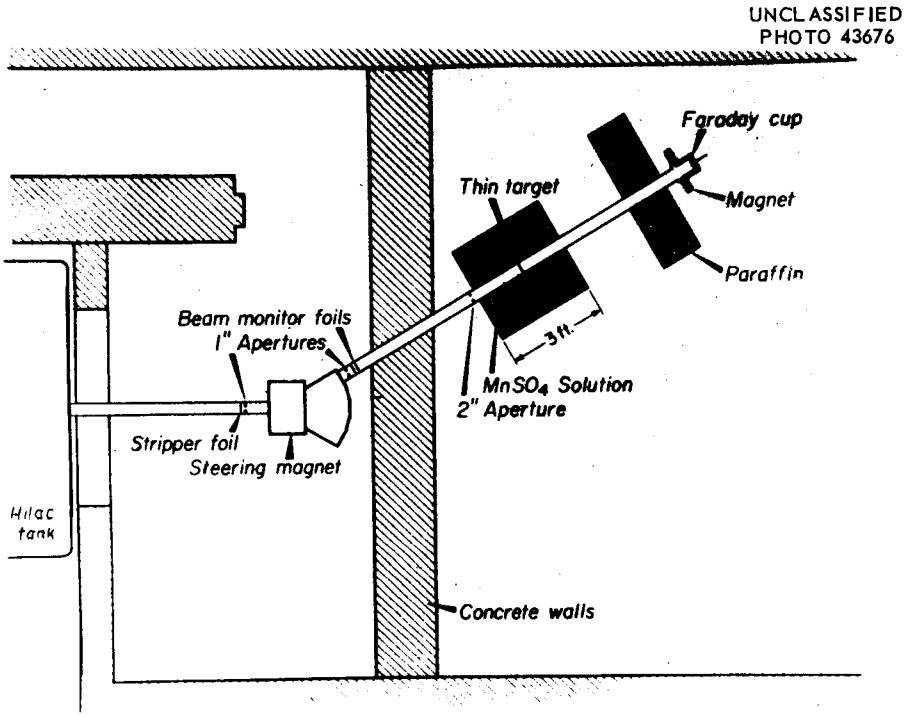
8. NEUTRON YIELDS FROM HEAVY ION REACTIONS

E. L. Hubbard, R. M. Main, and R. V. Pyle
(Presented by R. V. Pyle)
University of California, Berkeley

A number of people have tried to compare experiments and theories of nucleon boil-off at high excitation energies; the experimental technique has always been to use protons, deuterons, alpha particles, and so on. The problem is, of course, that as one goes very high in bombarding particle energy, only a fraction, maybe 40%, stays in the nucleus as excitation energy. So you have first of all to get the excitation energy, either by a Monte Carlo calculation or by measuring the angular distribution of all neutrons, and so on.

We thought that perhaps we could use heavy ions which should not give us so many cascade particles and with which we could get excitation to 100 to 150 Mev and compare the results with some sort of boil-off theory. Well, all we are going to give you today is numbers. We don't have any comparison with any particular theory. The reason is that we have to know more about the interactions, cross sections, and so on, but I would like to take just about five minutes and show you the experimental data to date. The experiment is going very slowly and we are not clear when we will have any more to talk about.

The essential part of the apparatus, Slide 1, is the manganese sulfate tank. The volume is big enough so that it is not very neutron-energy-sensitive. The beam comes out of the accelerator in several charge states, goes through a stripper foil, which brings it mostly to a singly charged state, through a 1-in. collimator, through a steering magnet, and through a pair of foils that are used for monitoring. There is a 2-in. aperture which we used initially to see if the beam wandered around. Then at the end we put a Faraday cup, (or a fluorescent plate), with paraffin to stop neutrons from going back toward the tank. If we use thick targets, they are mounted in a Faraday cup at the center of the tank. The background level for thin targets is in the range of 5 to 7 %.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Faraday cup
Thin target
Magnet
Paraffin
Beam monitor foils
1" Apertures
3 ft.
MnSO4 Solution
2" Aperture
Stripper foil
Steering magnet
Hilac tank
Concrete walls

Slide 1

I don't think anybody wants to read all these numbers, but here's the table, Slide 2. Bombarding particles are, carbon-12, nitrogen-14, and neon-20, and thick target yields run from about 8×10^{-4} neutrons per particle up to 29×10^{-4} neutrons (for C^{12} bombardment on uranium). The definition of thin targets is in the center. They are not really very thin as far as heavy ions are concerned. For example, the average energy loss for C^{12} is about 10 Mev. These are the cross sections for producing one neutron in these particular targets. (It should be σ_{1n} on the slide.) They run from about 1 barn for carbon and beryllium up to around 20 barns for neon.

The thick target data are plotted in Slide 3. The errors shown apply pretty much to all of the points. There is competition here between the change in cross section as one goes from one particle to another and the dE/dX and, therefore, the range, so that these yields all turn out to be more or less the same.

The thin target data in Slide 4 is the cross section in barns for producing one neutron. We have only 3 points for nitrogen, roughly 132-Mev nitrogen. We have a fair complement of points for carbon and for neon. The repeatability of these data is excellent. For example, at one point we repeated five times, and the experimental numbers agreed within 1% in all cases. But the uncertainty that is shown is 9%. The reason is that in the thin target measurements there is some scattering, and then quite a long flight path and we are not sure just how to make the correction. What I should really say is that when we repeat the scattering measurement, it only repeats to about 5%. We have only tried this a couple of times, and we put on here what we considered to be fairly safe.

Slide 5 is just sort of a diversion. This is the same sort of thin target cross section for producing one neutron vs A of the target nucleus, for various bombarding particles of roughly the same energy, 190 Mev for protons, 170 Mev for deuterons, and 192 Mev for neon-20. One practically always finds about the same answer, no matter what bombardment particle is used.

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	Thick Targets-Neutrons/$_{Ion} \times 10^4$				Thin Targets-σ_{in} (barns)		
Target	C^{12}	N^{14}	Ne^{20}	Mg/cm^2	C^{12}	N^{14}	Ne^{20}
Be				9.07	1.15 ± 0.10		
C	8.0 ± 0.5	10.4 ± 0.6					
Al	14.1 ± 0.8			10.66	1.31 ± 0.12	1.66 ± 0.15	1.97 ± 0.18
Ni				7.92	2.44 ± 0.22		2.92 ± 0.26
Cu	17.6 ± 1.1			2.55	4.75 ± 0.42		4.81 ± 0.43
Ag	20.3 ± 1.2	19.8 ± 1.2	16.2 ± 0.9	14.6	7.1 ± 0.7	9.9 ± 0.9	9.5 ± 0.8
Ta	18.4 ± 1.0	19.5 ± 1.2	17.1 ± 1.0	9.38	12.0 ± 1.1	12.2 ± 1.1	18.5 ± 1.7
Au				10.29	12.8 ± 1.2		19.2 ± 1.7
Pb	18.9 ± 1.1			16.26	10.7 ± 0.9		19.9 ± 1.8
U	29.0 ± 1.7		20.2 ± 1.2				

Slide 2

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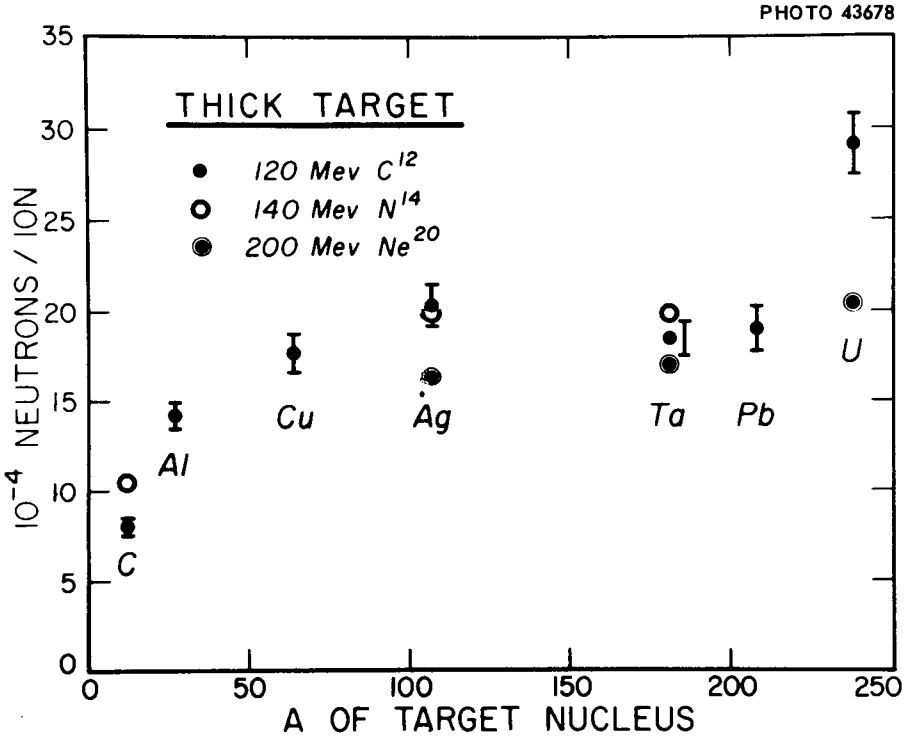

Slide 3

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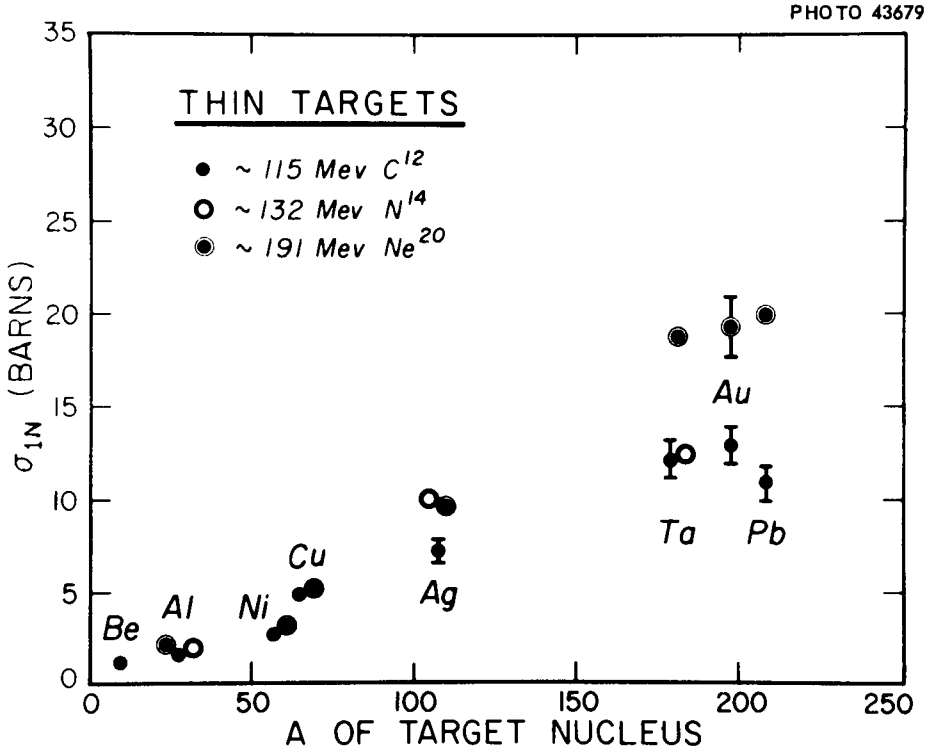

Slide 4

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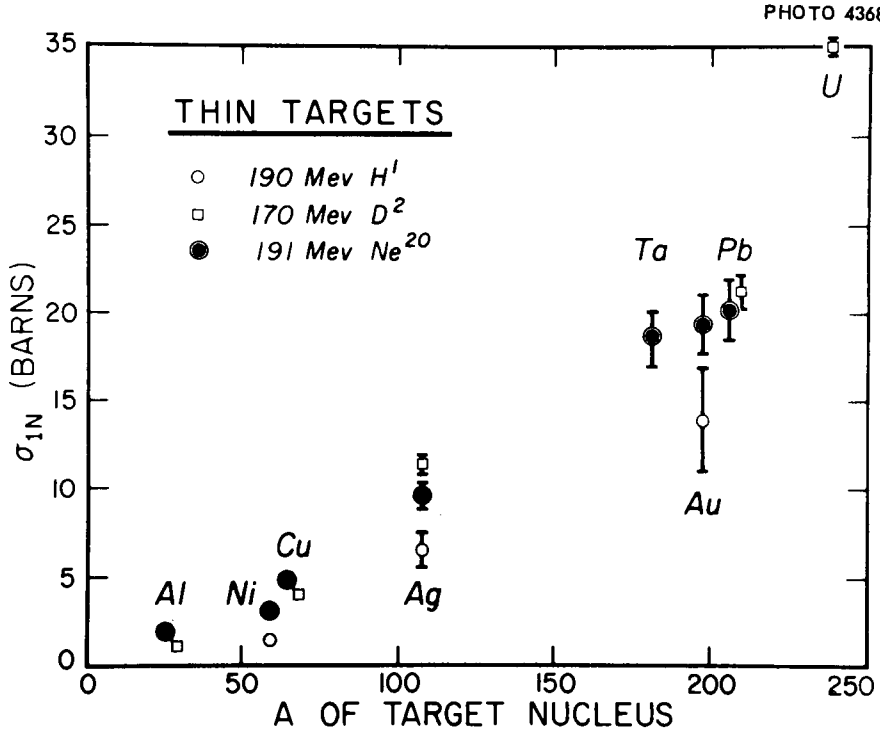

Slide 5

The next slide (Slide 6) will show the first attempt to compare the results with theory. There are two sets of points there. One I am not sure whether it is labeled. The other is labeled "experimental," and the theoretical points are really not theoretical nor are the experimental points experimental. It is a sort of numbers game which one can play, and we have not really tried careful boil-off calculations for various reasons. One of them is that we don't know what fraction of time we really form the complete compound nucleus with, let's say, carbon, how much stripping there is, and so on. But there will be some points on the top of this slide, and neglecting the effect of fission, these will be sort of upper limits to the number of neutrons which one can get out.

We take Levy's mass formula and calculate the excitation energy, use neutron boil-off theory, and calculate the number of neutrons emitted, assuming that there will be no charged particle emission. Of course, that is wildly off down in the low Z region. Up around lead we should get quite reasonable numbers.

Then there are some points marked "experimental." These are obtained by taking the σ_{1n}'s and dividing them by a cross section for inelastic interaction which is not an experimental number. It is just a classical cross section:

$$\sigma = \pi r_o^2 (A_1^{1/3} + A_2^{1/3})^2 \left(1 - \frac{V}{T}\right),$$

where, V is the Coulomb barrier and T is the kinetic energy, and these are the mass of bombarding and target nuclei. There seems to be experimental evidence that compound nuclei are formed with r_o of approximately 1,45 to 1.5, something like that.

This is the carbon data (Slide 7). Up in the region of lead, gold and, of course, uranium fission is going on. I don't know how many neutrons it supplies, but these open circles are the number of neutrons we calculated would be emitted if nothing happened except neutron emission. If we take the classical cross-section formula start with $r_o = 1.5$, from our experimental data we get the lower points, a factor of 2 difference.

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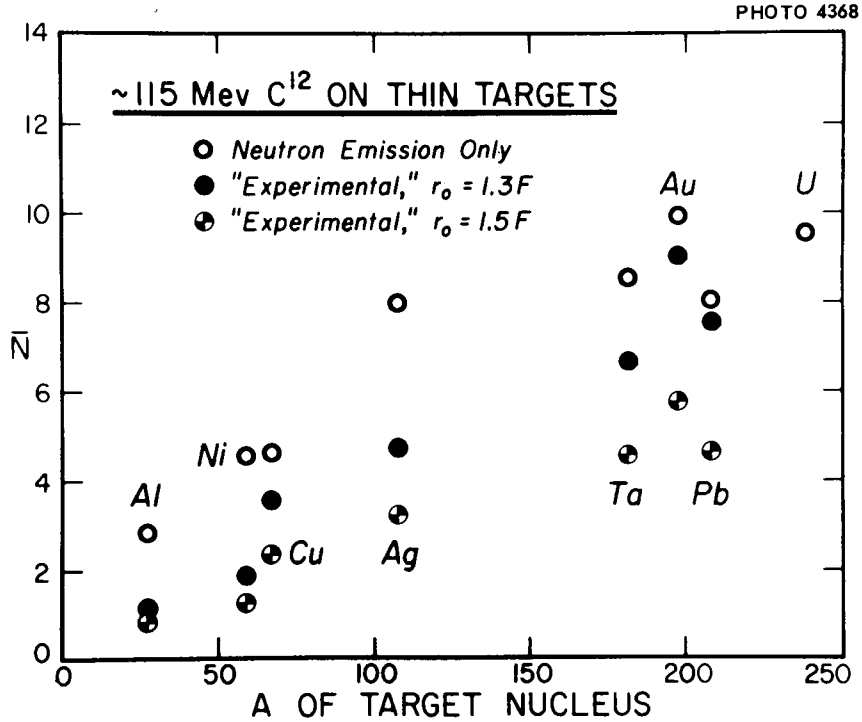

Slide 6

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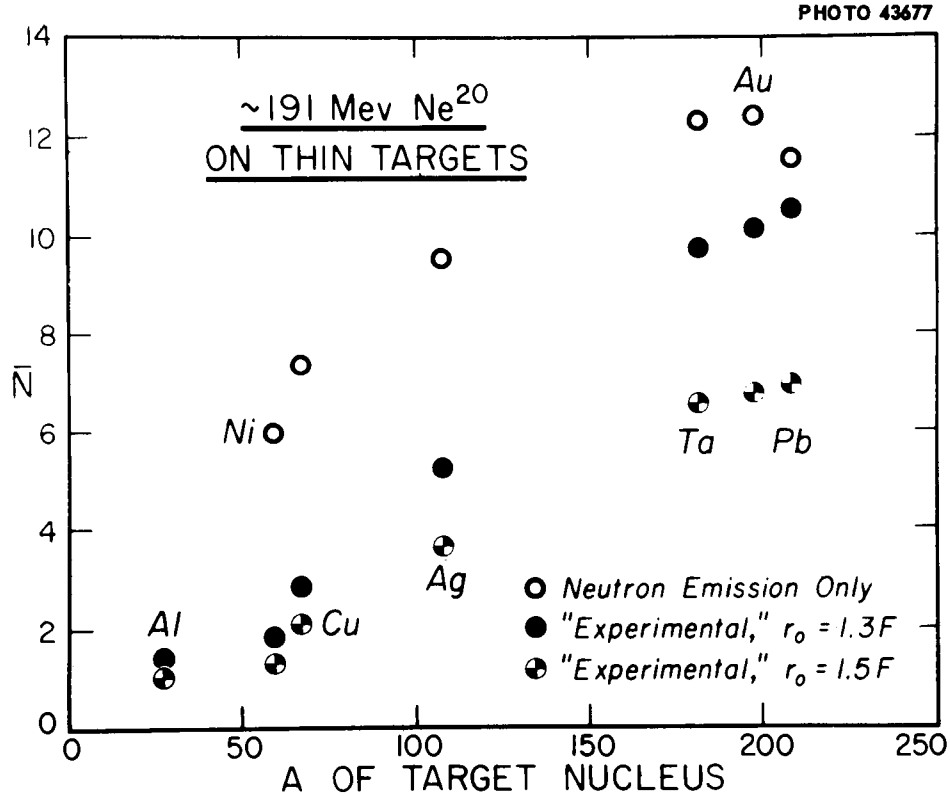

Slide 7

The same thing with neon-20 (Slide 7). These are the same sorts of upper limits to neutron emission, and these are the points which one would get from the experimental data if he used this formula. If one uses $r_o = 1.3$ instead of 1.5 the points are moved upward toward the upper limits, but I don't know of any reason for doing so. Of course, there are a lot of things which can make this number much too high. For example, these nuclei are neutron-deficient so that the probability of charged-particle emission is much higher than it would be for proton bombardments. We have not calculated this effect yet.

This is really all we have to present.

CHAIRMAN SNELL: Are there any questions or comments on Dr. Pyle's presentation?

H. G. BLOSSER: In your upper limits there for the number of neutrons which might boil off, was a contribution included for kinetic energy of the neutrons for temperature correction?

PYLE: Yes.

BLOSSER: What did you use?

PYLE: We used an average nuclear temperature, and I cannot give you a number for what the kinetic energy would be but it has a mean value of twice the temperature, something like that. I suppose several Mev. It is not really a critical thing because so much of the energy at the excitation is taken on by the binding energy.

T. SIKKELAND: (Question regarding fission cross section).

PYLE: The probability of fission you mean might reduce the number of neutrons which came out? If one fissions uranium then you get a contribution there. Perhaps the number of neutrons which come out is actually reduced instead of increased.

SIKKELAND: Well, don't you get many neutrons from the fission products?

PYLE: All I know about it is that we take something like uranium and we get neutrons from fission nuclides, and I don't know whether we get 1 or 2 or half a neutron. Whatever it is, it is certainly much different from this factor of 2 that we see here; but if one fissions

really early in the de-excitation process, let's say the first thing that happened is fission, then I can imagine, although I don't really know that it is true, that one would then de-excite by boiling off more charged particles.

CHAIRMAN SNELL: Do you even know the relative numbers of nuclides that undergo fission as against neutron emission?

DR. PYLE: Only by hearsay.

CHAIRMAN SNELL: I think this points up one thing that must be increasingly clear to everybody. In the afternoon's talks there is apparently a very rich field, all sorts of things yet to measure and yet to think about.

9. ELASTIC SCATTERING OF C^{12} FROM GOLD

H. L. Reynolds
University of California, Livermore

I think that many of you are familiar with the work that has been done over the past few years by a number of groups on the elastic scattering of alpha particles from 20 to 40 Mev by heavy nuclei. This work was first done at the University of Washington by Wegner and others.[1,2] J. Blair, who is here, participated in this work. In general here is what was found. If we plot the elastic scattering cross section $\sigma(\theta)$ dived by the Coulomb cross section, we find that at small angles the ratio is equal to 1. As the angle of scatter increases, in general there is a small rise in the ratio and then a very sharp drop.

This data was analyzed by Blair[3] with what is now known as the "sharp cutoff model". In this model one assumes that the amplitude of the outgoing ℓ-th wave is equal to zero if the corresponding classical distance of closest approach is equal to or less than the nuclear radius, or, if you prefer, the nuclear interaction distance. If the distance of closest approach is larger than the nuclear interaction distance, then the wave goes out with the amplitude for Coulomb scattering.

This model fit most of the data surprisingly well and, in fact, it has been used with success on the heavy ion scattering experiment done by Reynolds and Zucker[4] in which the elastic scattering of N^{14} by N^{14} was investigated. I think we will hear more later about some further analyses of this data.

The experiment that I want to talk about concerns the elastic scattering of C^{12} from gold at four different energies, the maximum energy being approximately 118 Mev, the minimum energy being approximately 70 Mev; the latter is rather close to the Coulomb barrier.

Slide 1 shows the experimental apparatus. It is rather simple in nature. The scattering chamber was connected directly to the end of the accelerator. There was no separating foil between the chamber vacuum and the accelerator vacuum. The beam passed through the collimator

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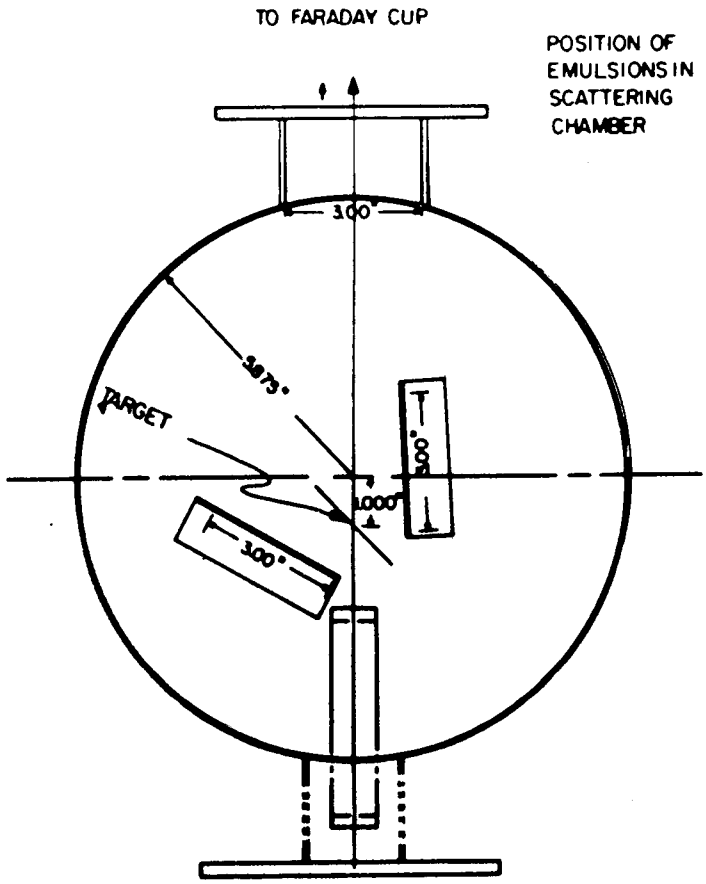


Slide 1

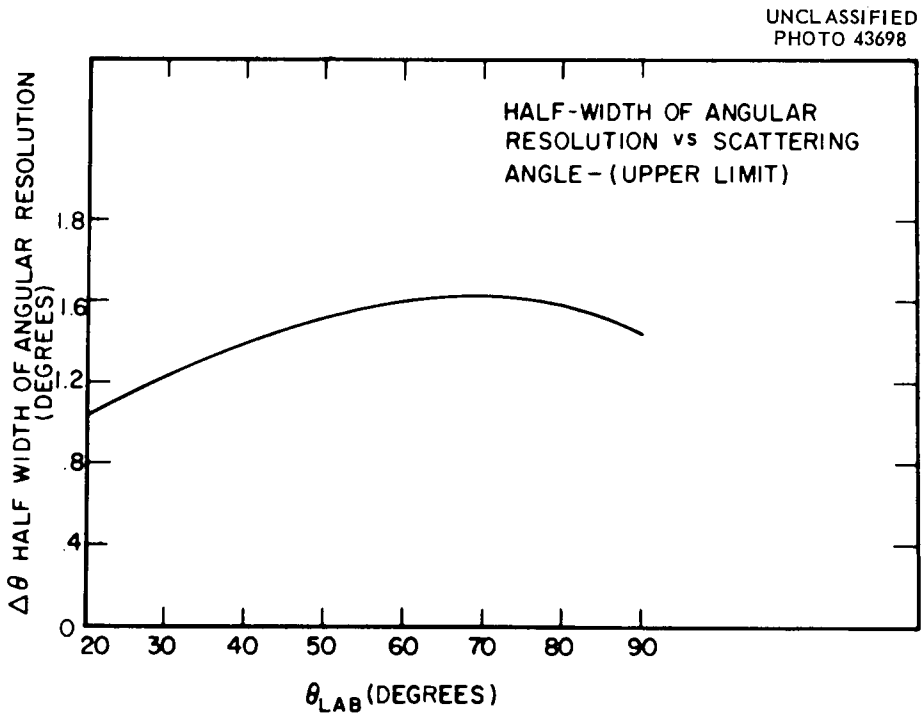
which was approximately 8 in. long. The slits were 1/16 in. wide by about 1/2 in. high. The beam then passed through the target and into a Faraday cup so that one could measure the current. The scattered particles ended up in 1-in. by 3-in. Ilford E-2 emulsions. Insensitive emulsions were used because the carbon ions are heavily ionizing particles. The emulsions were carefully placed against a milled edge so that one knew quite accurately their position.

The collimator, plateholders, and the target were all attached securely to the bottom plate, so that in disassembling and reassembling, these parts always maintained the same relative positions.

It is necessary to measure the number of tracks per unit area as a function of the distance along the plate, apply the appropriate geometric corrections, and one has the angular distribution for elastic scattering. The target was approximately 3/4 mg thick, resulting in a loss in energy of the carbon particles of about 1 Mev. The energy of the beam was changed by placing aluminum foils in front of the collimator.

As is usual with such an experiment, the first few exposures resulted in completely black plates. We attributed this first of all to low energy gamma rays and x-rays. Also electrons accelerated in the last few gaps of the accelerator could pass through the collimator and then scatter into our plates. We solved this problem in two ways. First we put a magnetic field of approximately 400 gauss just in front of the collimator, with the idea that 2-Mev electrons would be deflected approximately 15 deg. by the field whereas the heavy particles coming through would be essentially undeflected. Also we placed in appropriate spots throughout the chamber some lead absorbers. This did away with the difficulty and the following exposures were successful.

In a scattering chamber of this type, the angular resolution is naturally a function of the angle of scattering, since the distance of the plate from the target changes with the angle. In Slide 2, we have plotted the half width of the angular resolution in degrees as a function of the laboratory scattering angle. This includes the multiple scattering in the target, which is about 1/2 deg. In general the angular resolution is about 1 1/2 degrees.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HALF-WIDTH OF ANGULAR RESOLUTION vs SCATTERING ANGLE - (UPPER LIMIT)
Δθ HALF WIDTH OF ANGULAR RESOLUTION (DEGREES)
1.8
1.6
1.2
.8
.4
0
20
30
40
50
60
70
80
90
θLAB (DEGREES)

Slide 2

We had to know the energy of the particles not only at the maximum energy, but also after the foils had been inserted for the lower energy exposures. The maximum energy we knew, from the length and frequency of the accelerator. We have plotted in Slide 3 a range energy curve in emulsion that was obtained by Miller[5] at Berkeley a number of years ago using carbon particles from the 60-inch cyclotron. We have one point, the maximum energy point, that can be placed on the curve; the agreement is not bad.

If for lower energies, we assume that the carbon particles are completely stripped, then we can calculate the loss of energy in the aluminum, based upon energy losses of protons. If we then compare these points with the curve, we find that they also seem to lie on the curve fairly well.

Slide 4 gives the results at 118.3 Mev. We have plotted the elastic differential cross section in the center of mass divided by the Coulomb cross section as a function of the center-of-mass angle of scatter. We have reproduced the data which is represented by the dots three times, so it could be compared with the calculations for different cutoff ℓ values. Cutoff ℓ values of 63, 64 and 65 are shown.

In this calculation what one does is take the Coulomb scattering amplitude and subtract from it all of the contributions of the ℓ wave from zero up to the cutoff ℓ value. Now if anyone present has done a calculation of this type he will realize immediately that these were not done by hand. The calculation was coded for the IBM 650; if anyone desires to make calculations of this type and happens to have a 650, we would be very happy to supply him with a deck of cards. Each point represents approximately 300 tracks, and therefore a statistical standard deviation of approximately 6%.

There seems to be in the data an indication that there may be some oscillations as suggested by the calculation. However this cannot be proven with the present angular resolution and statistics. We are repeating this experiment, improving the angular resolution by a factor of 2 and will look more carefully for the small oscillations. We see that the rise and the sharp drop are present as predicted by the calculation. The Blair model seems to fit the data quite well.

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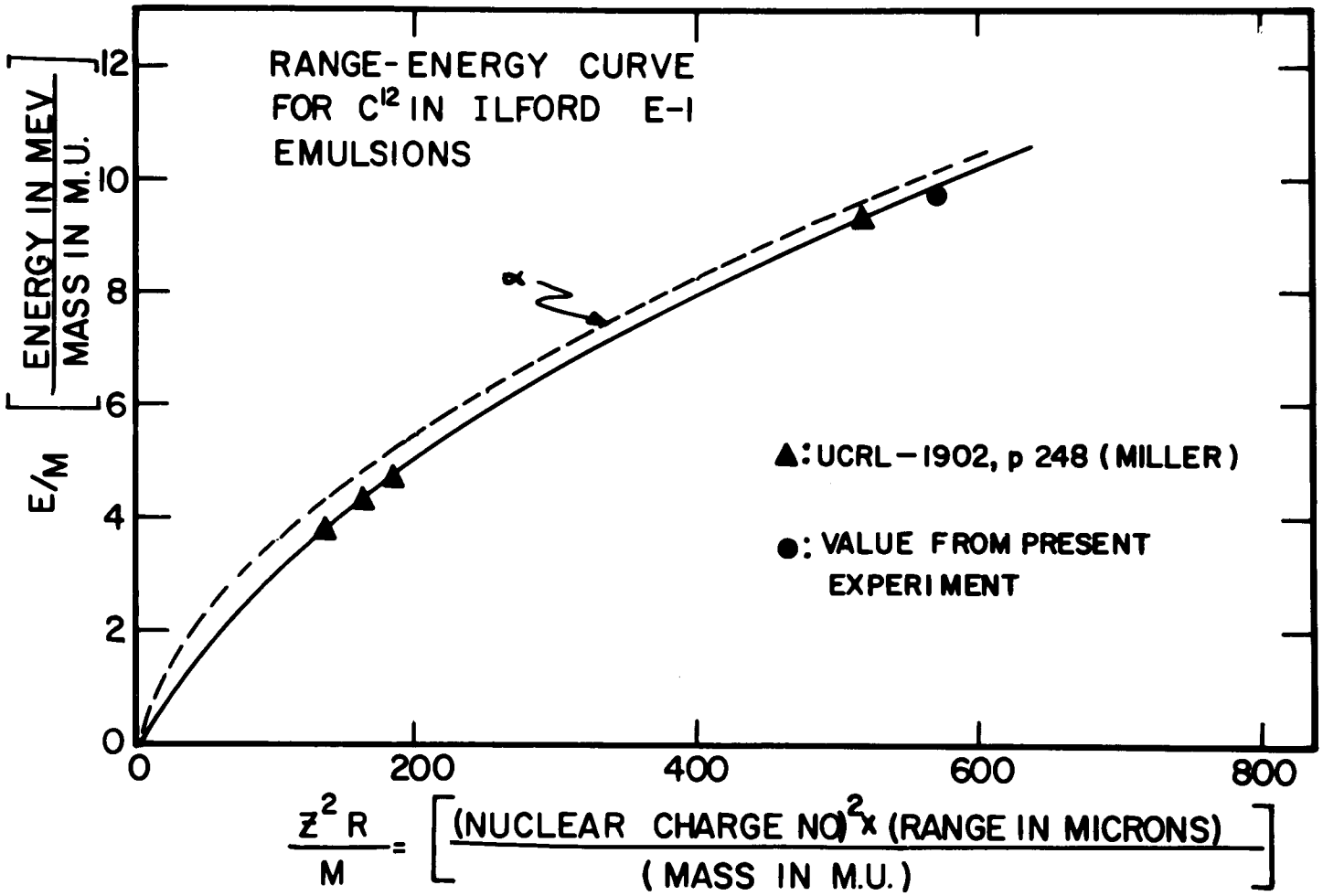

Slide 3

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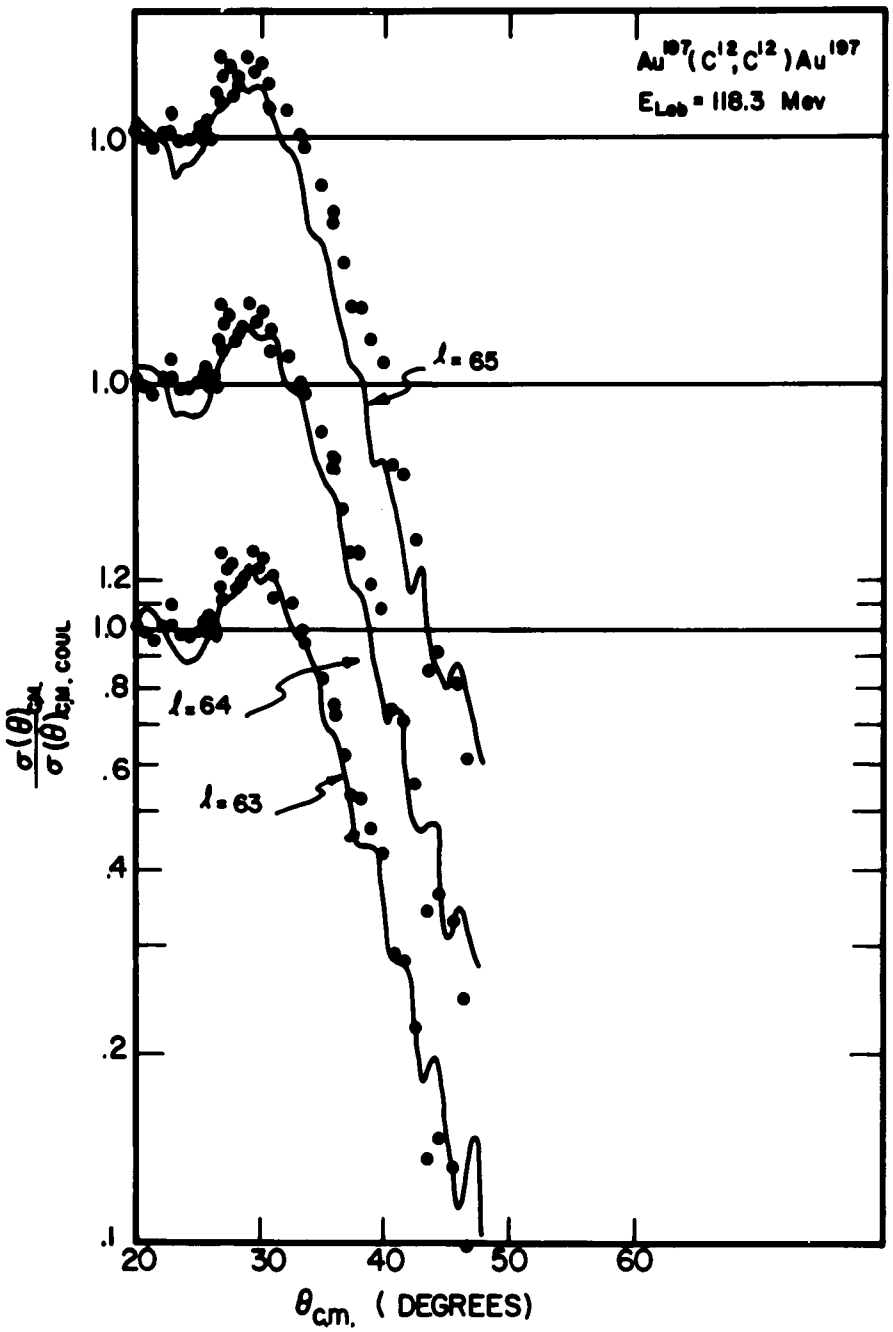

Slide 4

You will notice that before the rise there is a dip of about 10% predicted. This appears in almost all of the calculations. At none of the energies have we observed this. I make a point of this because some optical model calculations have been performed. The optical model tends to wash out the small oscillations, but it retains this drop before the rise, and also the rise. We have put quite a bit of effort into this region in improving the statistics, and there does not seem to be a drop there. For this particular energy we chose a cutoff ℓ value of 63 as the best fit.

Slide 5 is the same type of plot at a laboratory energy of 100.8 Mev. Again you see the rise of approximately 20 to 25%, and then the following sharp drop. The best fit here is approximately 55 for the cutoff ℓ value.

In Slide 6 we have the same type of plot at 79.4 Mev. The peak has broadened a little. You will notice that as the energy decreases the point of dropoff is occurring at larger angles. In this case we chose a cutoff ℓ value of approximately 36. You will notice that one can determine this cutoff value to within approximately ±1 in angular momentum.

Slide 7 is the data at 73.6 Mev. The energy is quite close to the Coulomb barrier of 62 Mev. We looked at the region of the rise quite closely. The statistics on these points are approximately ± 2%. In this case the best ℓ value is about 29.5.

Now one question that I have not answered so far and that I am sure some of you are asking is how, do we know that these are elastically scattered particles?

Slide 8 gives the range distribution of the particles at maximum energy for a laboratory scattering angle of 23.7 deg, where we are definitely in the Coulomb region. This illustrates the spread in energy or range of the incoming beam. The half width is approximately 2 Mev. The ranges are as measured and have not been corrected for dip angle.

Slide 9 is the range distribution for the same incident energy at an angle of 48.9 deg. You will notice an entirely different character for this spectrum. We are now getting a fairly large number of lower energy particles. Presumably what we are seeing here are inelastic events or perhaps even the pickup or loss of nucleons or charged particles.

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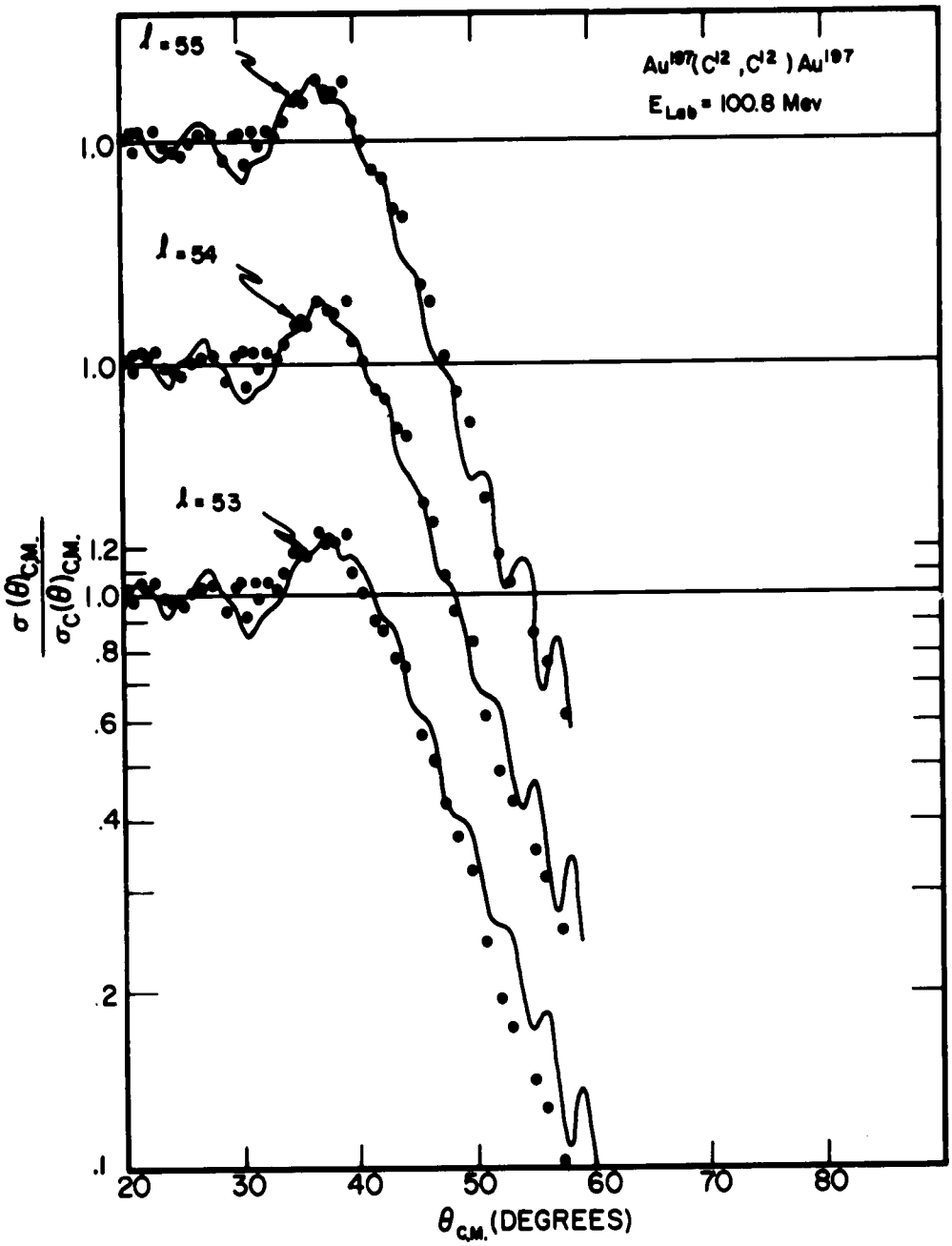

Slide 5

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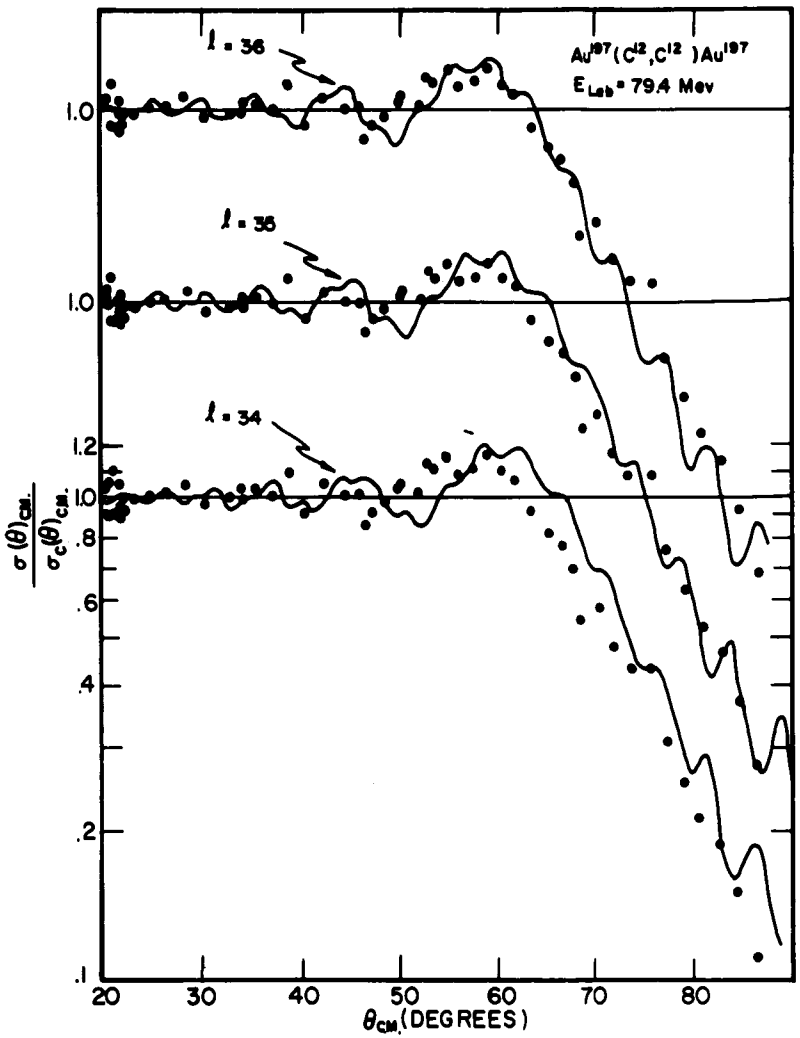

Slide 6

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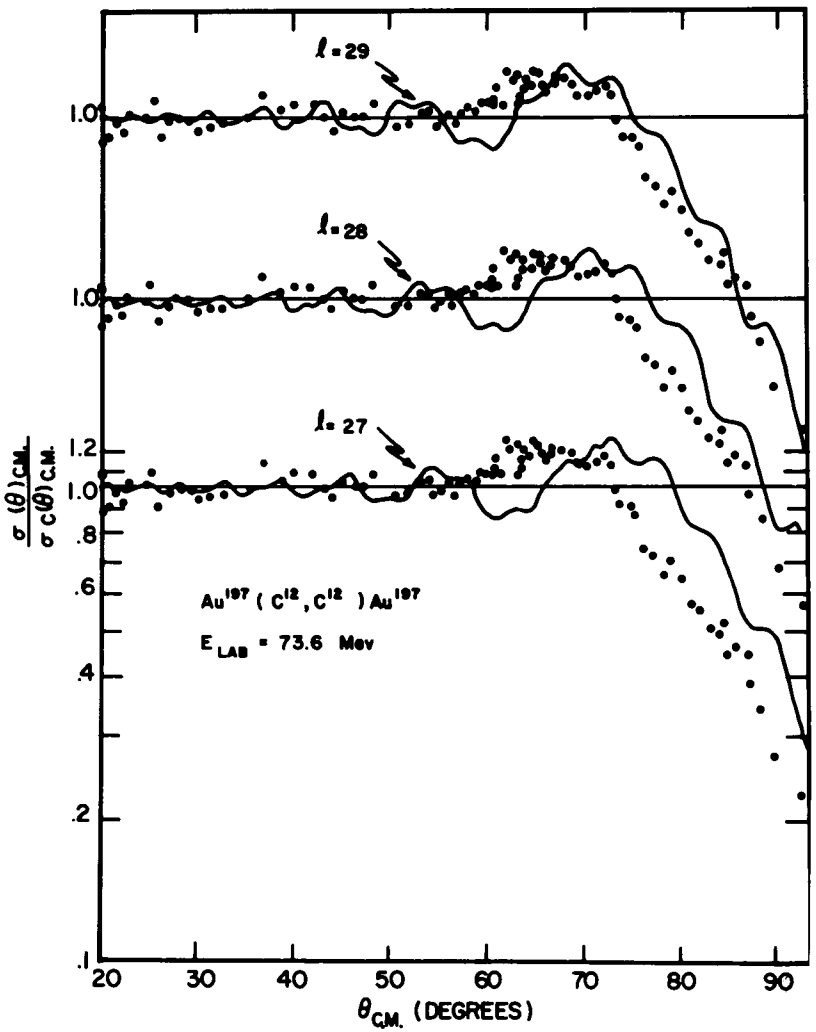

Slide 7

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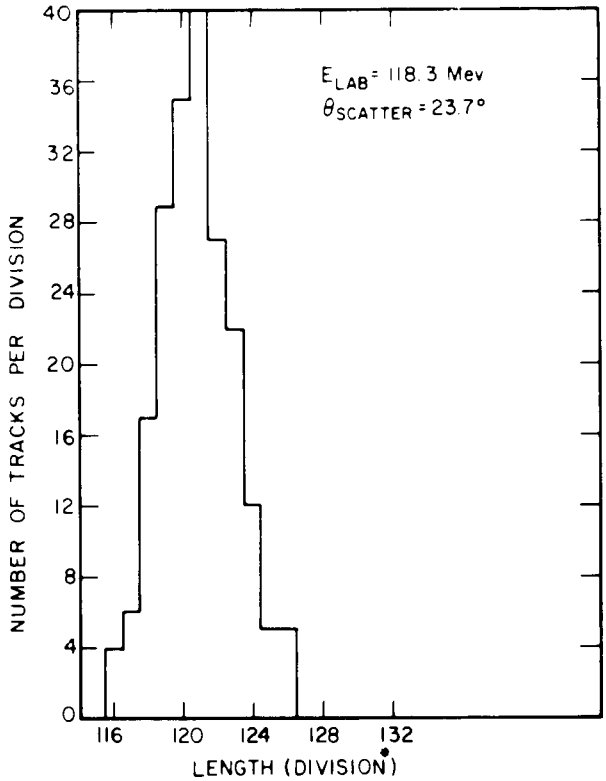

* 100 DIV. = 141 μ

Slide 8

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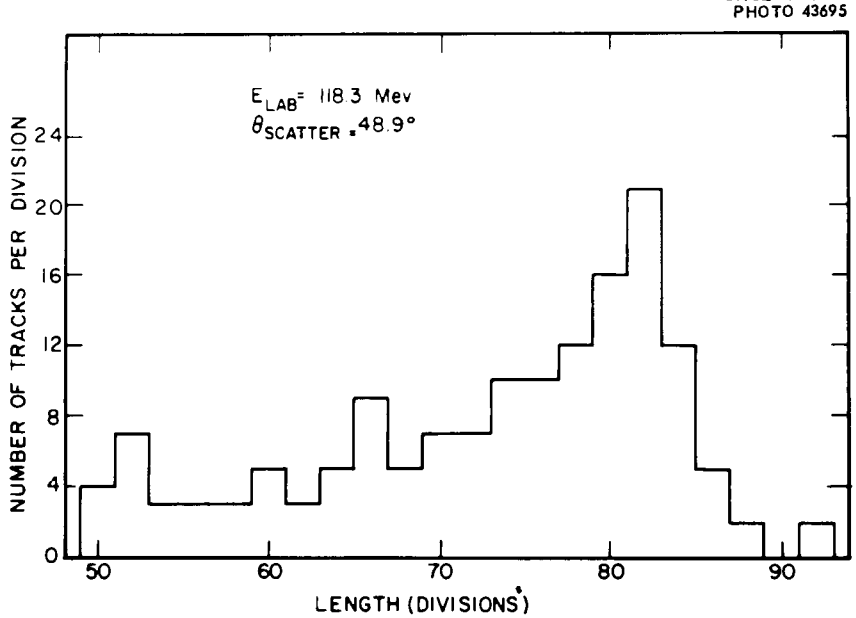

* 100 DIV. = 141 μ

Slide 9

Now at this point it would be rather difficult to pick out what are the elastically scattered particles.

You will notice that in all of the results that I showed we stopped at a point where the scattering cross section was approximately 1/10 of the Coulomb scattering cross section. This was done because we were getting into the angular range where the identification of the particles as elastically scattered was difficult. However, down to about 0.2 of the Coulomb cross section the rather sharp character of the range spectrum was retained. We feel that the majority of the particles are, in fact, elastically scattered down to where the ratio is is about 0.2

We have plotted in Slide 10 the cutoff ℓ value vs the center of mass energy for several radii, using the standard relation shown on the slide. We have plotted on the curve the points as determined from the best fit for the cutoff ℓ values from the Blair model. The points lie quite well on the line for a radius of 11.8×10^{-13} cm. The point at 95 Mev is a little high, but still within errors. The errors that we have used are ± 1 in ℓ. Therefore the interaction distance is $11.8 \pm 0.2 \times 10^{-13}$ cm.

Everyone likes to see what this means in terms of r_o in the relation $R = r_o (A_1^{1/3} + A_2^{1/3})$. The value of r_o turns out to be 1.46×10^{-13} cm. Now this, of course, is not a radius in the usual sense. If you were talking about optical model potentials or something of that type you find that r_o is 1.2 or 1.25×10^{-13} cm. This r_o means many things. It may be where the potential is half of its maximum value of something of that type.

If you look at the usual type of Saxon potential you will find that the particles in this case are interacting where the nuclear potential is only 1 or 2 Mev deep, and you are in fact destroying the particles far out on the edge of the nucleus. Therefore we term the value of R found the interaction distance.

Now we can ask what this means as far as cross section is concerned. Having found our cutoff ℓ value we can determine the total nuclear cross section as

$$\sigma = \pi \lambda\!\!\!^{-2} \sum_{0}^{\ell'} (2\ell + 1),$$

where ℓ^1 is the cutoff value. You will find that this is in fact

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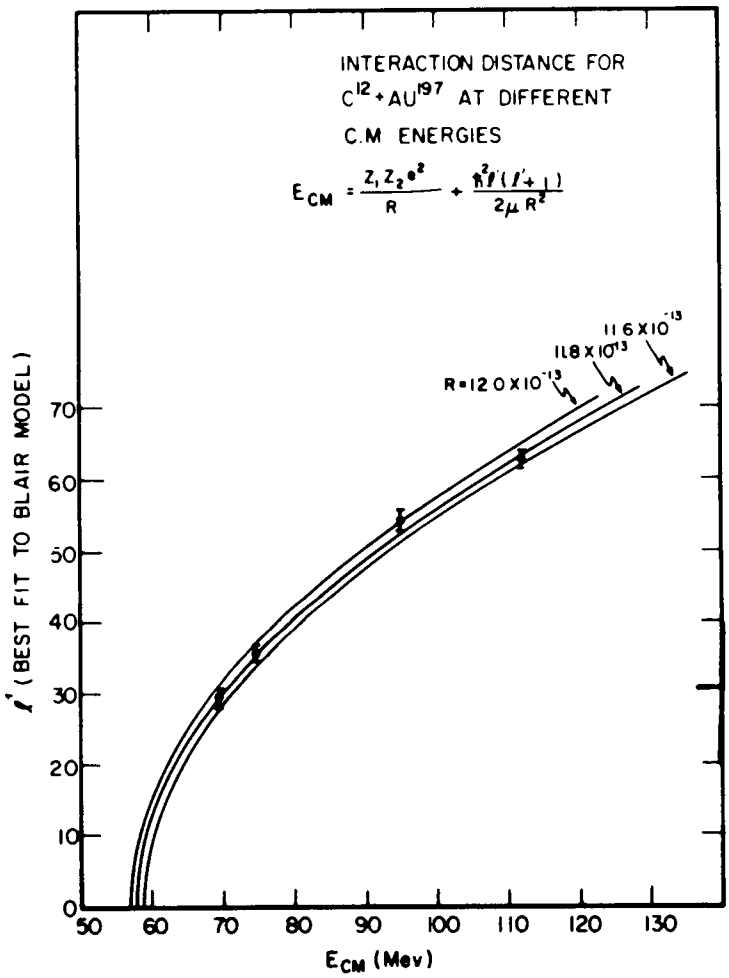

Slide 10

equivalent to the relation that you were seeing yesterday,

$$\sigma = R\left(1 - \frac{Vc}{E}\right).$$

I will write a little table. The first column is the energy in the c-o-m system and the second column the total nuclear cross section. Elastic nuclear scattering is, of course, not included.

E_{cm}	σ (barns)
111.6	2.1
95.1	1.9
74.9	1.0
69.4	0.77

If you don't like the Blair model you can get at the cross section in another way, which is just as approximate, by assuming that the nuclear elastic scattering is essentially zero, and then merely comparing your measured differential cross section with what you should have if it were all Coulomb. In effect you decide how many particles are missing.

This is the status of our elastic scattering experiment at present. We are now embarked on the same type of experiment with a bismuth target, with both carbon and oxygen bombardments.

I would like to change the subject slightly for a few moments. It would apply to some of the things that were said yesterday. During this experiment we were very much annoyed by a fairly large number of short tracks that got in the way and made the counting of the charged tracks a little difficult. Some time during the experiment we managed to find a few moments to think about what we were doing, and at that time it became very obvious that these tracks were in fact fission tracks and they were very easy to see and very easy to count. Therefore we measured an angular distribution for the fission tracks at our maximum energy of 118 Mev. The angular distribution has been measured from about 40 to 140 deg, and we found that the differential cross section was proportional to $1/\sin\theta$.

I mentioned this to John Blair the other day, and he pointed out that this is exactly what you would expect in this situation. One can integrate over the whole solid angle, and determine the cross section for the fission process for 118-Mev carbon on gold. This turned out to be approximately 90% of what we claimed for the total interaction cross section or about 1.8 barns.

CHAIRMAN BURCHAM: Well, thank you very much for this very interesting and excellent paper. Are there any remarks?

A. ZUCKER: I have a question. How do you measure the energy of the incoming carbon?

REYNOLDS: I discussed that a little previously. We measured the range of the particles and used Miller's range energy curve. The maximum energy is based on the known energy that comes out of the HILAC.

ZUCKER: Well, you get 0.1 Mev essentially. How well do you believe it?

REYNOLDS: Perhaps we ought to ask Ed Hubbard this. He gave me a very accurate length and a very accurate frequency.

E. L. HUBBARD: I would not believe it more than 1 or 2%.

R. L. GLUCKSTERN: Not to more than 3 or 4%.

ZUCKER: How closely can you tell the loss in energy because of the elasticity of the cross section?

REYNOLDS: I would think you could see something like perhaps 3 or 4 Mev, if you used a great deal of care.

ZUCKER: Wouldn't it be good to scatter from lead then?

REYNOLDS: Yes, I suspect it would be. We have been picking mono-isotopic targets for obvious reasons. One could make a lead target of separated isotopes.

M. L. HALBERT: What are the values of the parameter

$$\frac{Z_1 Z_2 e^2}{\hbar v} .$$

REYNOLDS: I am not sure of this. As I recall, I think something like 20.

HALBERT: It is the parameter that goes directly to the calculation.

REYNOLDS: I think it is about 20.

HALBERT: And the minimum energy, is it still close to that?

REYNOLDS: Yes.

E. NORBECK: I am wondering if you could explain in just a few words why it was an expected thing that the fission products would have that particular angular distribution.

REYNOLDS: John, would you care to tell them what you told me?

J. S. BLAIR: Supposedly, we have been talking completely classicially about the problem. Suppose you have a nucleus like this. Your carbon ion comes in and it starts this spinning just like a fireworks demonstration, and after a while you spill out a couple of fission fragments which come off at random in angle. You can write down the probability of having particles coming off at angle θ as: $p(\theta)\, d\theta = K$, a constant. Then this is proportional to $\sigma(\theta)\, d\Omega$ or $\sigma(\theta)\, 2\pi \sin\theta\, d\theta$. Then solving for the differential cross section one has this equal to $C/\sin\theta$ where C is a constant. This is just based on the idea that this compound system revolves for a long time and spills off the fission fragments at random in theta space.

C. E. PORTER: I wondered a little about the exact optical model, which seemed also to be giving this drop after the rise. Is this a surface absorption model?

REYNOLDS: I am not qualified to talk about the optical calculations. These are being done by Joel Bengston at Livermore. About all I can do is show you his results. I have a few graphs that he has done, and if you want to know the details I think it would be better to write to him.

CHAIRMAN BURCHAM: Are there any further questions? May I ask, how long an exposure and with what beam do you make these experiments?

DR. REYNOLDS: In general they required something like 10^{-4} microampere hours. Now, using the full beam from the machine which amounts to approximately 1 microampere we required exposure times of about 15 to 20 minutes. In general we would make two exposures, one a rather heavy one, requiring 15 minutes or so, and another a rather short one, requiring sometimes a minute or 2 minutes. This was so that we could cover a wider range in density of particles.

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10. ELASTIC SCATTERING OF N^{14} BY Be^9

M. L. Halbert
Oak Ridge National Laboratory

The experiment I am going to talk about is still in progress and the data I present is preliminary. I will say that we believe it today, but we have yet to repeat it. So keep that in mind if you want to do anything with the data. The present experiment was done in collaboration with A. Zucker.

Beryllium-9 is an interesting nucleus to study in any experiment, and in this kind of experiment it is quite interesting because the center-of-mass energy at our maximum bombarding energy would be 10.6 Mev, but the Coulomb barrier is only about 5.4 Mev; therefore, we get large nuclear effects and the deviation from Coulomb scattering should be large and interesting.

A further reason for choosing beryllium was that we have a thin target of beryllium already available. For this experiment we have used a target made by evaporating beryllium, about 0.18 mg/cm^2 thick, which corresponds roughly to an energy loss of 1 1/4 Mev. The mean laboratory energy in the foil is about 27.2 Mev.

The first thing we tried was simply the detection of the N^{14} singles with a scintillation counter. We had the idea that this was just a simple experiment, that nothing could go wrong, so let's just put up a counter and move it around and see what happens. We found pretty shortly that this would not work beyond 25 deg in the laboratory system. The continuum of charged particles from nuclear reactions becomes so large in comparison with the scattered particles that it becomes rather difficult to lift the scattered peak off the continuum.

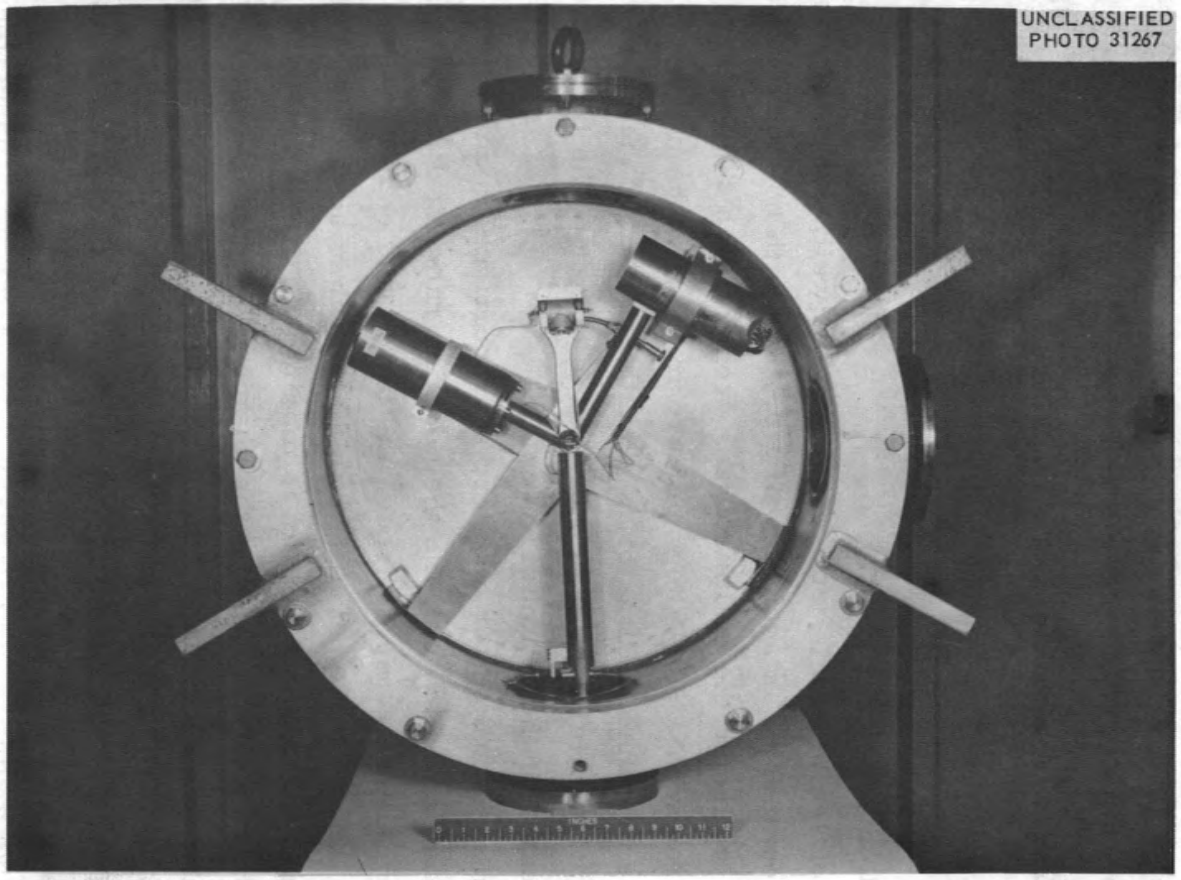
I neglected to say, of course, that we take pulse height analyses of the singles. The scintillator used was 5 mils of cesium iodide, thallium activated, with no reflector or hermetic seals or anything on it. It was directly exposed to the particles. This makes it very easy to interpret our results.

The experimental difficulty mentioned beyond 25 deg was not quite as serious as another difficulty in which the peak, which was fairly sharp at small angles, began to spread out and then began to split up into several peaks. Obviously something else was giving us scattering besides the beryllium. Since any impurity that we were likely to have here has a Z larger than 4, and since the cross section goes as Z^2, we could really run into trouble. The trouble is compounded by the fact that the deviation from Coulomb should be expected to drop the beryllium cross section below Coulomb at a much smaller angle than it would for any impurity, and it is not difficult to estimate that quite a small amount of impurity will give a large contribution to the scattering.

The obvious answer to this problem was to go to an arrangement in which the beryllium particles are detected in coincidence with the nitrogen. Our first attempt at this was to use for the beryllium detector a proportional counter, because it is somewhat simpler than using a scintillation counter. Mechanically it is simpler. However, the window on the proportional counter takes out a good fraction of the beryllium energy and it makes it rather difficult to decide exactly what you are doing. So we were forced into using another scintillation counter for the beryllium.

Slide 1 shows a picture of our scattering chamber, seen head-on. I hope this can be seen because it is a photograph. The size of the chamber is 24 in. inside diamter, or 60 centimeters. The cylinder with its axis tangential is our nitrogen detector and the one with its axis radial is our beryllium detector. Now actually there is no real difference. We could use one for the other. But one is further away, and therefore gives smaller angular apertures than the other. We wanted the nitrogen counter to be the defining counter for the solid angle.

An iron pipe is used as a magnetic shield for the particle path coming in; the beam comes in from the bottom. A piece of iron pipe in front of each counter serves the same purpose. The bending of the particle in this magnetic field is not very great unless the energy is below a few Mev, but since our scattered particles will be in some cases only a few Mev, shielding becomes essential for a clear-cut understanding of what we are doing.



Slide 1

Another reason for using magnetic shielding is that the particles coming out of the targets are in various charge states which are bent in different amounts by the magnetic field; this would tend to spread out our angular resolution beyond what we absolutely have to put up with.

I should describe the cyclotron in relation to the chamber. The center of the cyclotron is about six feet below and to the left of the center of the chamber. (The median plane of the magnetic field of the ORNL 63-Inch Cyclotron is vertical and the deflected beam is vertical). You see, we are quite close to the cyclotron, and we are in the fringing field. The magnetic field in the lower left portion of the chamber may be of the order of perhaps 2,000 gauss. The field in the upper right is much weaker but nevertheless there is a magnetic field there which has to be shielded out.

You cannot see the angle indication here too well, but it is possible to set these counters to an accuracy of ± 0.1 deg with no trouble at all. It is possible to change the collimators in front of each counter. Normally we run with a ± 0.4-deg aperture in front of the nitrogen detector. The entrance collimator is near the bottom of the chamber. There is an anti-scattering slit just below the target. The target is right at the center of the chamber.

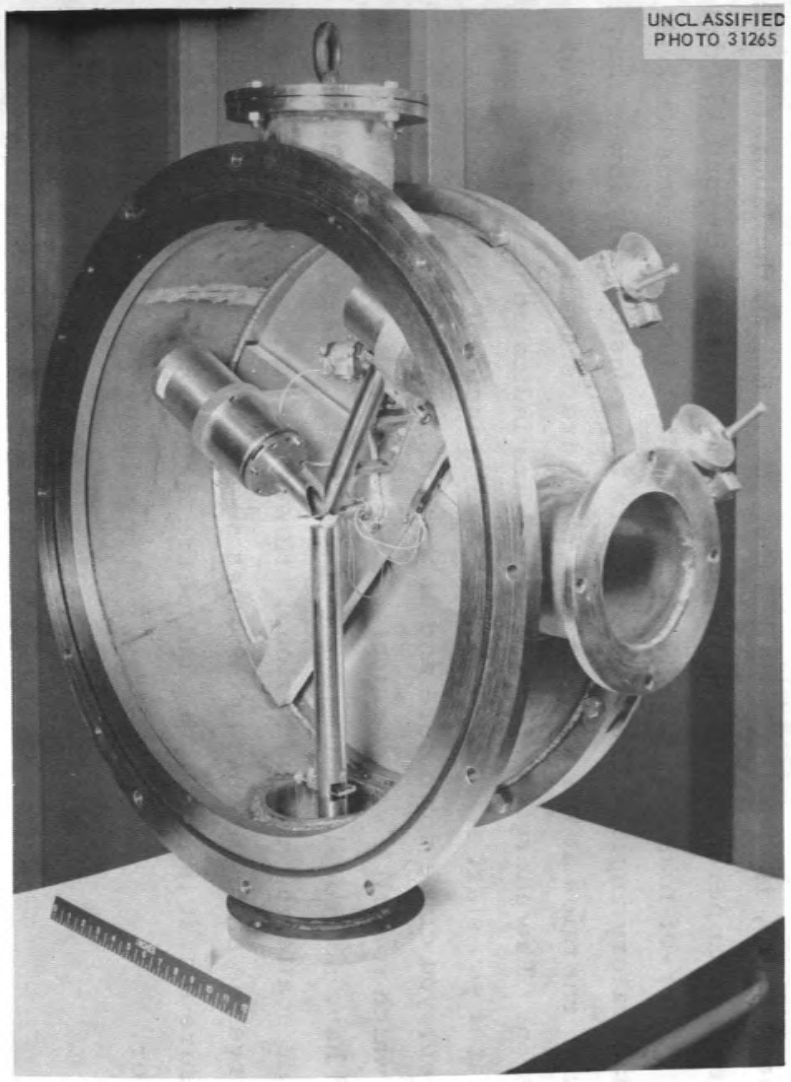
This counter is a monitor counter, a proportional counter, which is fixed and enables us to compare the counting rates at different angles. The beam normally goes right through the target and ends up on the plate above. In a normal operation this plate is insulated and serves as a Faraday cage with which the beam is monitored.

Slide 2 shows an angle view of the chamber, and you can perhaps get a little better idea of how this thing is set up. The arms that carry the counters are very substantial. They were designed to be so, because we knew that the magnetic forces on these shields were going to be large, and we didn't want any bending and we didn't want any breakage; so we made them quite heavy. The electrical connections go through a hollow tube which is concentric with the target support.

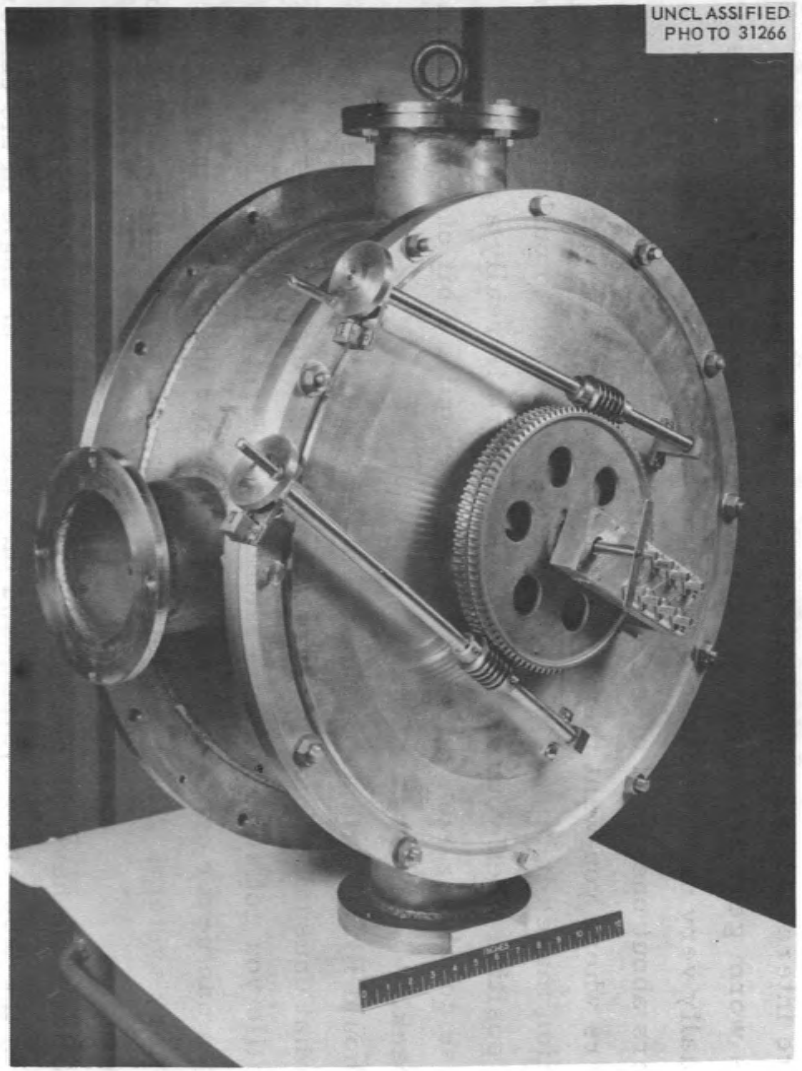
Slide 3 shows some of the mechanical details of the back, for those who are interested in mechanical details. The counters are moved by this worm gear arrangement with a 100 to 1 gear reduction. This is actually very nice, because one turn of the hand crank moves the counters about one-third of a degree. It is very easy to set the counters where you want them. The fact that there is such a large reduction makes it impossible for the magnetic field to pull the counters out of position when you turn your back. There is really a great force on these counters; even with 100 to 1 it takes quite a bit of force to turn the crank.

I should like to talk about the kinematics a little because they are somewhat unusual. Since the target is a lighter mass nucleus than the projectile you get some unusual situations. First, you find that the nitrogen can never come out at an angle greater than 40 deg in the laboratory system. The beryllium can come out at any angle from zero to 90 deg.

In Slide 4 we have the beryllium angle plotted against the nitrogen angle in the laboratory system, and then the nitrogen angle in the center-of-mass system is also plotted on the right hand scale; so you see that the cutoff angle, if you like to call it that, of 40 deg in the laboratory corresponds to perhaps 130 deg in the center-of-mass. Beyond 130 deg in the center-of-mass the nitrogen particles then begin to come back to zero laboratory angle. The energies here are considerably lower than the other energies at the same angles, so there is really no danger of confusion. The energies of the particles are indicated here. You can see that if you start at 27.2 Mev as being the incident energy of the nitrogen, you come down here and at the cutoff angle, you are at 5.9 Mev, which is still a very respectable energy for the nitrogen. It still should have no trouble getting out of the target, for example, although multiple scattering may be serious. However, in this lower section of the curve the nitrogen energy becomes so low that it is rather difficult to be sure that it is all coming out of the target and reaching the detector.



Slide 2



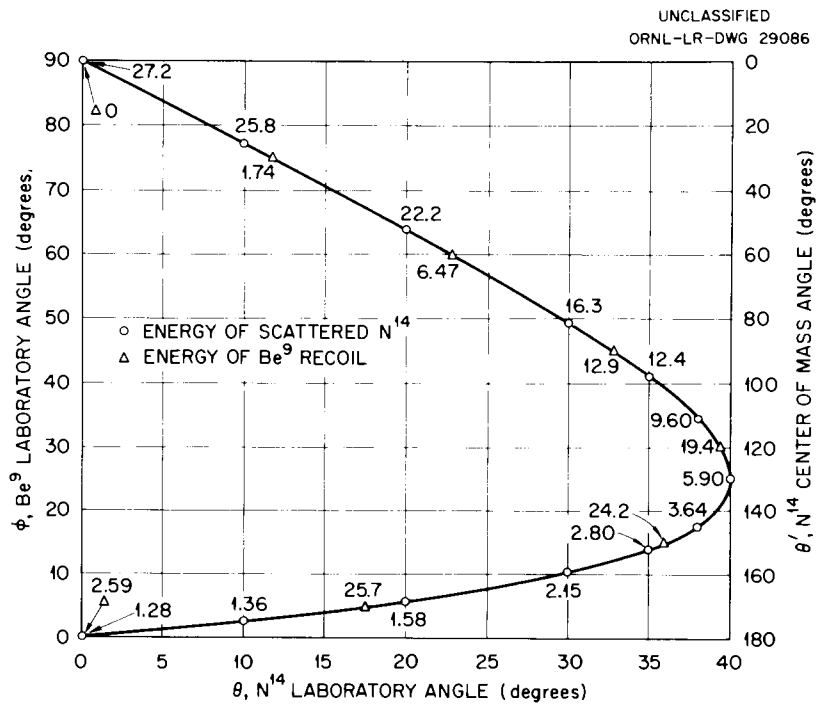
Slide 3

Thus far we have gone out to an angle of 32.5 deg. We plan to keep pushing this as far as we can. The beryllium energy is zero if the nitrogen scatters in the forward direction in the center of mass. All the beryllium gets out of the target completely at about 20 deg laboratory nitrogen angle, but going to smaller angles will be difficult because the beryllium may not be getting out of the target. We have not yet gone to angles smaller than 20 deg with the coincidences, but we are planning to push that also as far as we can.

The next slide shows a portion of the data. Slide 5 shows the coincidence counting rate at 25 deg nitrogen angles as a function of the setting of the beryllium detector. The angle scale here is actually off by a degree or two. We found this out since the slide was drawn, and if you make that correction then the larger peak comes exactly at the correct angle for a beryllium in coincidence with a 25 deg nitrogen. We don't know what the other peak is for sure. We suspect that it is an impurity like oxygen, let's say, on the target. If you calculate the kinematics you find that the recoil oxygen in coincidence with a 25-deg nitrogen scatter will peak at about this angle.

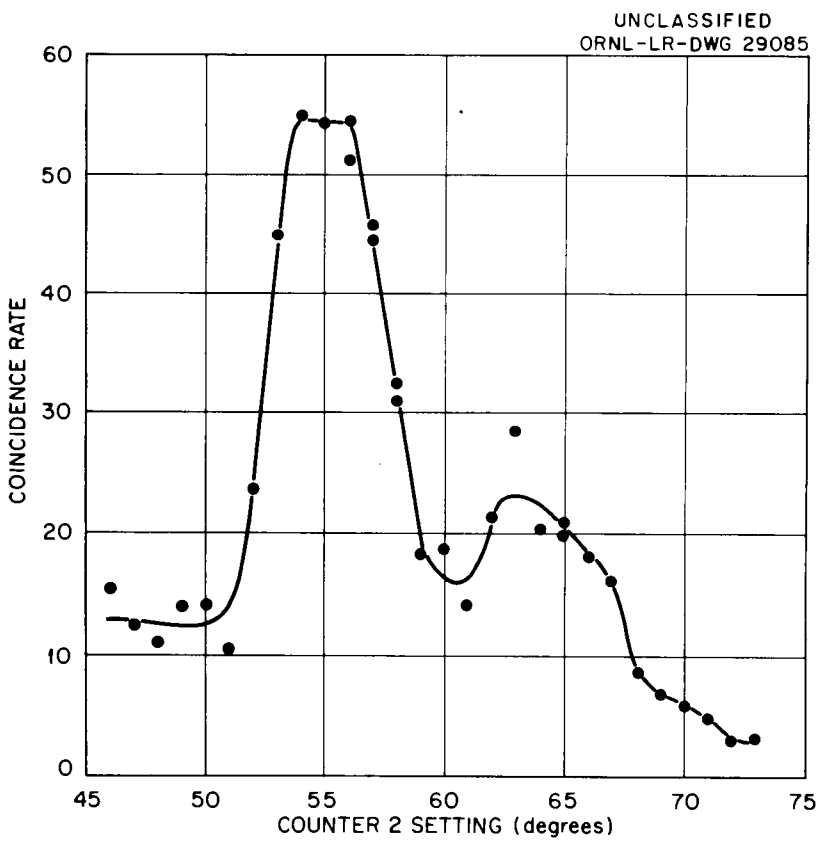
On the small-angle side you will notice that we have some background; in fact, we see some background on the other side, also. We don't know what this is. It probably is some kind of random coincidence, or perhaps it might be that some of these impurity particles are multiply scattered to very large angles, but I really doubt that you would get so many of them at such large distances from the peak. However, the main peak is a very nice peak. You can see that there is some uncertainty in subtracting the background.

Well, we have already used spectrometry on the nitrogen pulses, and here we have coincidences. So why don't we put them together? In Slide 6 we have a block diagram of the electronics used for the experiment, in which we do both pulse height analysis on the nitrogen counter and we make use of the fact that the coincidences are necessary for the detection of the nitrogen particle. This is a very simple experiment. We feed the pulse from the nitrogen counter to a pulse height analyzer, and we open



Elastic Scattering of N^{14} by Be^9

Slide 4



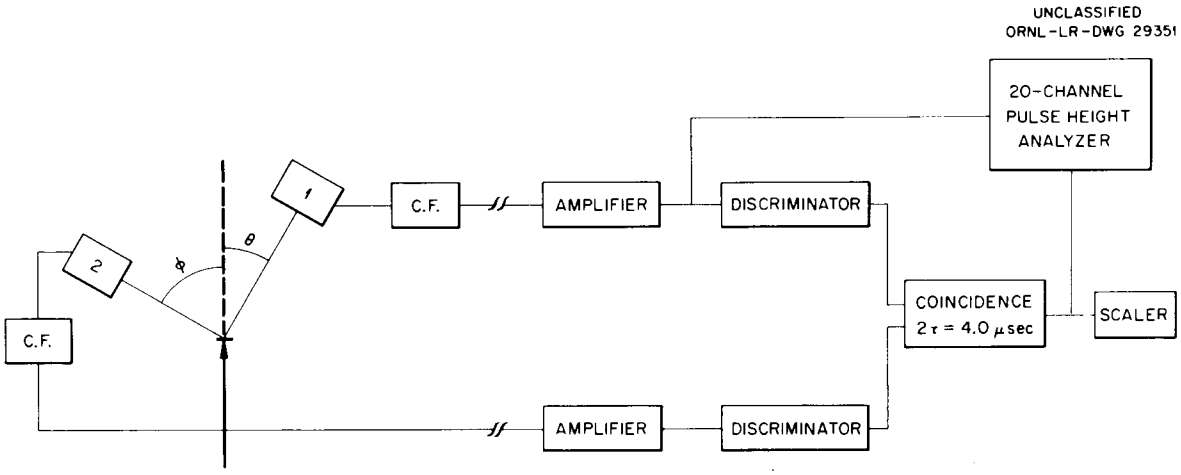
Slide 5

the gate of the pulse height analyzer only when there is a coincidence. We also record the number of coincidences, and we find that the number of coincidences as recorded on the scaler is equal to the total number of counts recorded on the pulse height analyzer.

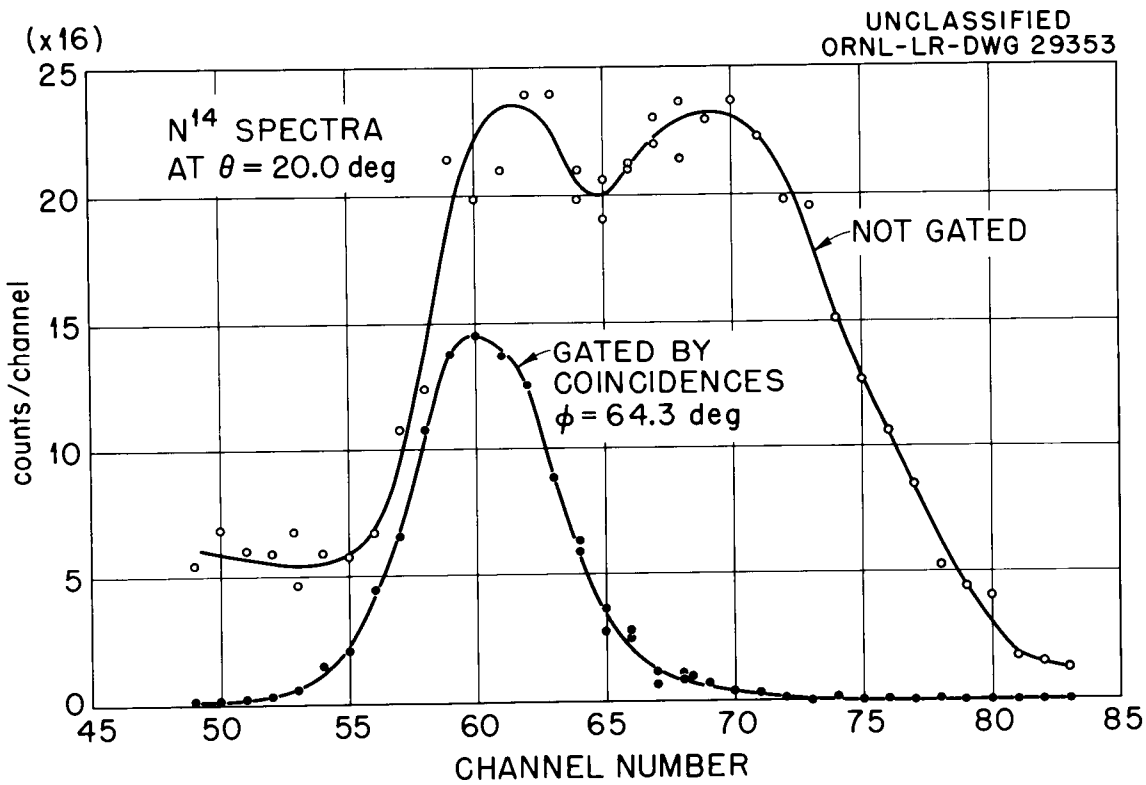
In Slide 7 we have examples of the improvement we get by gating the pulse height analyzer. Here is a multiple peak, such as I referred to. When you place the beryllium counter at the right angle for beryllium coincidences, which in this case is 64 deg, you see that you eliminate all of the peak at high pulse height and you drop all of the lower peak by a constant amount. This constant amount is probably reaction products in the target and you simply eliminate them. The higher peak is probably scattering from the impurities, possibly oxygen. The energy of the nitrogen scattered by oxygen is going to be higher and the pulse height is just about right to be this. So you see we have eliminated this extraneous peak.

Slide 8 shows an angular distribution similar to the one I showed before except that this is at 20 deg instead of at 25 deg. You will notice that the impurity peak is smaller. We believe that this is due to the fact that the energy of the recoil oxygen, if it is oxygen, is quite small if the nitrogen scatter is at this angle. Furthermore, it has to come out of the target at a very steep angle and loses more of its energy; consequently very few of the recoil oxygens are detected. On the other hand, the beryllium still has no trouble getting out of the target.

The distribution has a flat top -- this is important. We can vary the beryllium counter angle over this range and make no change in the counting rate. That means that we are counting all the beryllium in coincidence with the nitrogen at 20 deg; we are not missing any. That is quite important because if we don't count all the beryllium, let's say if the beryllium counter had too small an aperture, the solid angle would then be defined by the beryllium counter rather than the nitrogen counter. Or, we might be in some intermediate situation and would not know at all what was going on. At every angle we always take a distribution and make sure that it has a flat top. Then we are sure that we are getting all the beryllium. The width of this curve is just about the right width for the size of the beryllium counter. It has about a 2.5 deg acceptance angle.



Slide 6



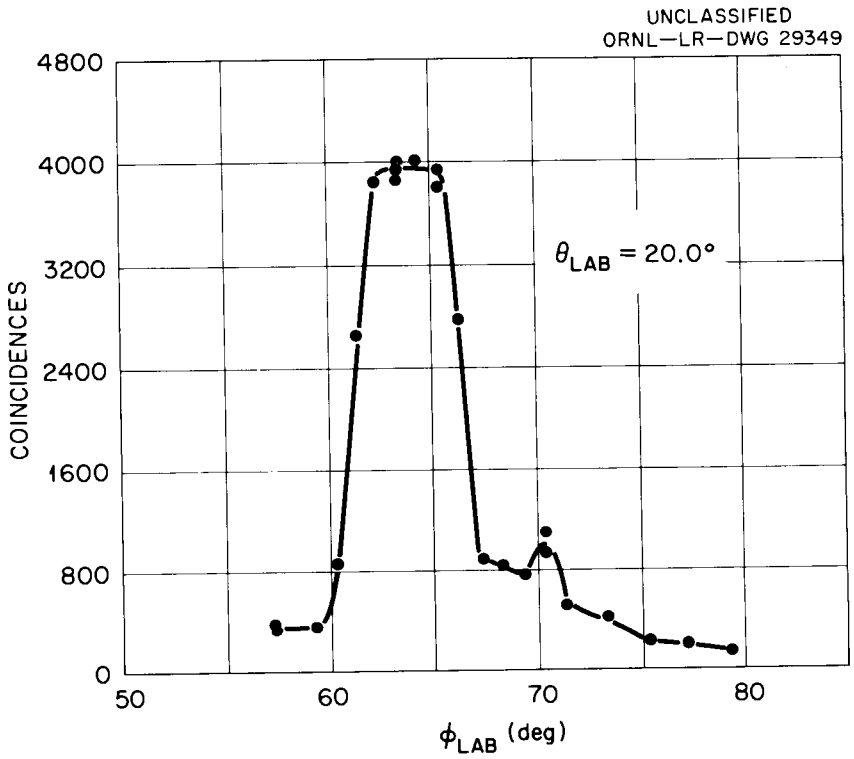
Slide 7

We also make sure we are not missing any events because of the electronics. We check that we are on a plateau of counting rate vs discriminator setting for both counters.

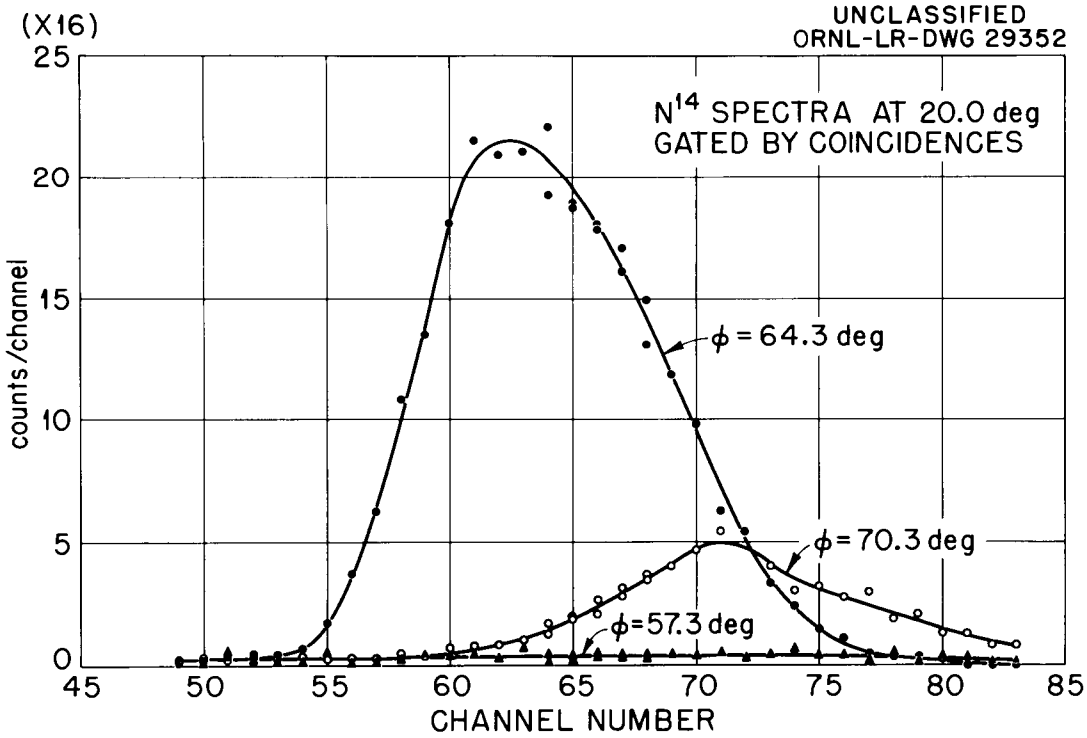
The coincident background here is still present, though apparently not as important, because the scattering cross section has gone up compared to what it was at 25 deg. Now I am going to show you spectra taken right at the center of the main peak, at this point here on the small-angle side, and on this impurity peak. The Slide 9 shows the spectra gated by coincidences at these three angles. The 57.3-deg spectrum is gated by coincidences on the left side of the curve, where we don't anticipate that there is anything. You see, any impurity on the left side would have to be of lighter mass than beryllium, and we don't expect anything to be there. On the other hand, the spectrum gated by coincidence on the impurity peak, on the right-hand side, peaks at the right pulse height to be oxygen. The 64.3-deg spectrum is gated by coincidences at the correct angle for detecting the beryllium.

The nitrogen spectrum here is asymmetric. You will notice that it is fatter on the high side. We are a little worried about this, and we are not sure what to make of it yet. Our first guess might be that perhaps some of these oxygens are giving us coincidences and, therefore, they add a little bit to this side.

Well, in order to test that assumption we took spectra gated by coincidences all along the top of the angular distribution I showed in the previous slide. You would expect that if there is any leakage of these oxygen particles into that angular distribution it will be greater on the large-angle side than on the small-angle side, and thus, if this is due to impurities the spectrum shape should change at various points along the top of that angular distribution. But the fact of the matter is that it does not change. This shape remains the same and the counting rate remains the same. So I think that we can probably reject that explanation; we have no explanation as yet to substitute. It might be a characteristic of the scintillator, but it is not clear to me why the spectrum should not just simply be broad instead of asymmetric.



Slide 8



Slide 9

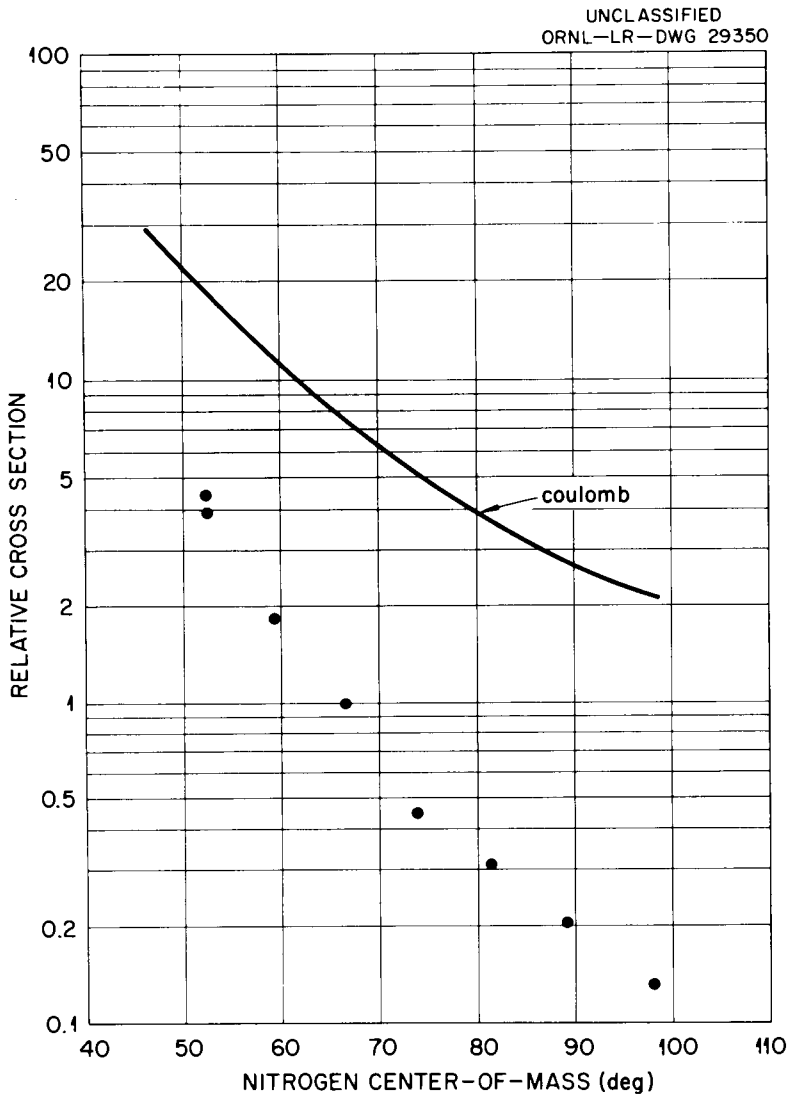
Well, at this point we thought we knew what we were doing. It took us a while to do this. We made many false starts, and at the time the abstract was written we were nowhere near this stage of the game. In fact, the data that I am about to show was obtained about 10 days ago and the cyclotron sprang a leak the next day. When we get back the cyclotron will be in working order and I am sure we will be able to continue.

At each angle we took spectra gated by beryllium coincidences. You see that there is very little problem in the background by now.

In Slide 10 we have the angular distribution after conversion to the center-of-mass system. The experimental data is represented by the points. These are taken at 2.5-deg intervals in the laboratory system, which corresponds roughly to 7 to 9 deg in the center-of-mass in this region.

The nicest thing to do with this kind of data is the sort of thing that Reynolds shows, in which he follows the cross section back up until it joins on to the Coulomb curve. I mentioned the difficulties in going to smaller angles, and we hope to lick those difficulties eventually. But in lieu of having that data on smaller angles, we wanted to make some comparison to the Coulomb curve. What we did was merely estimate the Coulomb counting rate from the known thickness of the target and the beam that hit the target. This estimate is necessarily an approximation because the target thickness is known only to about 20%. We know it is not uniform because we know if we pull the target out a little and use a different portion of it we get 10% more counts. Furthermore the beam is not known better than a few per cent. So this normalization of the Coulomb curve relative to the data is good to perhaps 25%, assuming, of course, that I have not made any mistakes in the calculations.

(Writing) What I would like to show now is a table of the values of the cross section relative to the Coulomb, since that seems to be the favorite way of showing up the differences. Theta prime is the center-of-mass angle for the nitrogen. We have two points of the smallest angle.


UNCLASSIFIED
ORNL—LR—DWG 29350
100
50
20
10
5
2
1
0.5
0.2
0.1
RELATIVE CROSS SECTION
coulomb
40
50
60
70
80
90
100
110
NITROGEN CENTER-OF-MASS (deg)

Slide 10

The reproducibility leaves something to be desired. We have some ideas as to why these did not reproduce, and we will study this further.

θ^1	$d\sigma/d\sigma_{Coulomb}$
52.2°	0.239 0.211
59.2	0.157
66.4	0.131
73.8	0.086
81.2	0.082
89.0	0.073

Maybe I should not say these out loud, and they won't go into the record. Well, if they change don't be too surprised. Actually, we believe these things right now but we have to repeat the experiment because we are not completely confident.

If you plot $d\sigma/d\sigma_{Coulomb}$ versus the angle theta prime, the first few points come down smoothly, and then there seems to be a little break. I don't know yet if the break is significant.

Comparisons are often made of elastic scattering with semi-classical approximations, for example, Blair's version of the sharp cutoff theory. In order for that theory to be valid or to be a good approximation we need the condition $Z Z' e^2/\hbar v >> 1$. In this experiment this parameter is equal to 3.16; thus there might be some difficulty experienced in comparing these cross sections with what is predicted on any semi-classical model. Perhaps 3.16 is not too small; sometimes people consider an experiment classical if this parameter is anything greater than unity. But I think that the fit to a semi-classical approximation will be nowhere nearly as good as Reynolds showed where that parameter was 20 or so.

For the future I should like to say that the scattering chamber described will be used not only for elastic scattering, but we hope it may be useful for other reactions. We might have some luck in seeing transfer reactions. We might have some luck in seeing inelastic scattering, although that is a little hard, and there are probably a number of other things that can be done with the chamber once we understand how to use it.

CHAIRMAN BURCHAM: I thank you, Dr. Halbert. Are there any questions on the development of this technique or the results obtained?

E. NORBECK: One would expect with something like this that by far the strongest nuclear reaction that would take place would be the formation of N^{15} with probably a Q value of 8 or 10 Mev. Where would that appear on these graphs that you have here?

HALBERT: I don't know.

NORBECK: Because you might actually get more N^{15} than you do scattered N^{14}.

HALBERT: At some angles that might be true. No, I don't know.

J. B. CUMMING: That would be associated with the Be^8 on the other side.

HALBERT: Oh, yes, that saves us; Be^8 breaks up before it reaches the counters.

ZUCKER: Also the big Q value makes the angles wrong.

HALBERT: Yes, but the question was, what are the angles?

G. BREIT: What is the lifetime of the ground state?

HALBERT: The ground state of the Be^8 may have a lifetime of something like 10^{-15} sec. It can't get very far before it breaks up.

F. E. STEIGERT: What about the possibility of the pump oil carbon particles? Wouldn't that bring your impurity peak a lot closer to the oxygen?

HALBERT: It would bring it in a little closer but not much. As a matter of fact, I glossed over this. The impurity peak in angles usually comes between the carbon and the oxygen peak; there is probably not a single peak there but several. The pulse height, however, comes closer to the oxygen; so it is not completely clear whether it is carbon or oxygen that is mainly contributing. Undoubtedly both are present.

S. K. ALLISON: Just on this point I might mentioned that some time ago, quite a few years ago, at Chicago, we did some work on the scattering of protons from thin beryllium foils. We got the foils from Berkeley that had been made for the linear accelerator, and there was no doubt that there was a strong oxygen peak in the scattering. It occurred from both sides of the foil. You could see it when the protons

come through and came back. We thought we had made some sort of discovery, and we rushed over to our metallurgy friends in the same building and announced this and they yawned and said they had known it all the time, that it was like aluminum and it does have oxide. So I have no doubt that it is oxygen in your work.

HALBERT: Aside from oxygen that might have been originally present on the target, we know that when we bombarded the beryllium it turned black; so something else was happening too.

J. L. NEED: Is there any possibility that your nitrogen sticks in this like a tritium target on zirconium, so that you are getting a reaction from those?

HALBERT: Well, it is possible that at some scattering angles, where the nitrogen energy is very low, it might never leave the target. However, the cross section for scattering at such large angles is small, no doubt, and the accumulated amount of nitrogen from the amount of bombardment we have done should be small. Then you have to scatter from that, which has a small probability.

J. S. BLAIR: Suppose you had appreciable inelastic scattering to low-lying states in Be^9 or N^{14}. I am particularly thinking that the 2.4 state of Be^9 is a good candidate. Could this possibly change your kinematics to be confused with the oxygen contamination?

HALBERT: Yes, if you assume an energy loss of 2.5 Mev in the scattering process, you move out to that angle. Did I misunderstand you?

BLAIR: The 2.4 state in Be^9 is very strongly excited in almost any inelastic experiment, (p, p'), (α, α'), and so on, and it is very likely some sort of collective state which might compete quite effectively with the elastic.

HALBERT: As I said, the angle is about right for 2.5 Mev. However, the pulse height of the nitrogen when we gate by coincidence at that angle is larger than the pulse height for elastic scattering; that is a little inconsistent.

G. F. PIEPER: What about the pulse height as a function of energy for nitrogen? Do you have any information on it?

HALBERT: Yes, that has been published in a recent issue of the Physical Review -- maybe several months ago. (M. L. Halbert, Phys. Rev. 107, 647 (1957).)

PIEPER: What about the background that you have in this? These are pretty big pulses that you get from this.

HALBERT: The pulse height say of 16 Mev nitrogen is equal to the pulse height from 5 Mev alpha, and we went up from 16 Mev in these experiments to perhaps 23 Mev, or something like that.

PIEPER: What is the background from that?

HALBERT: Which background now?

PIEPER: Well, when you say that it was necessary to separate the N^{14} from the background.

HALBERT: Well, I think that the background on the ungated spectra is probably reaction products from the target, protons, alphas, or deuterons, which just go through and give us some pulses. The problem with the background on the curves of coincidence rate versus beryllium counter angle which I mentioned is another problem again, because those are coincidences and it is not entirely clear where they are coming from. However, we are able to eliminate those completely by combining the pulse height analysis with the coincidence gate.

M. MALKIN: What is the magnitude of the field inside the shield that you have in your chamber, because the whole thing is immersed in 2 kilogauss field? What sort of reduction are you getting inside?

HALBERT: Well, I could not measure it when I tried because I used a bismuth probe meter, and that was not sensitive to less than about 100 gauss. It is certainly less than that. Let's say you are looking at pulses from a source and then you turn on the magnetic field. The pulse height does not shift which means according to the manufacturer's specification that the field inside is probably less than about a gauss or so. We have two concentric iron shields, and a mu-metal shield inside them.

MALKIN: I was not worried so much about that as about the magnetic shields along the track of the particles.

HALBERT: You have much less stringent requirements.

MALKIN: Yes, but 2 kilogauss would be bad.

HALBERT: Well, the 2 kilo gausses are in the lower left portion of the chamber. Most of our scattered particles travel in a region where the field is more like 500 to 200 guass. One thickness of iron should be adequate shielding here.

The particles that come up from the bottom, through the strongest part of the field, are the fastest. The radius of curvature of these particles is something like 100 feet. Is that right? I don't remember exactly. So there is really relatively little need for the magnetic shield on the incident beam because those particles are going so fast. That is just an added fillip.

G. BREIT: I wonder whether I understood your argument regarding the Be^8, that it lives a long time. Probably you know that it does not. Even if it should spread afterwards your measurements would be somewhat off.

HALBERT: Well, let's assume that the velocity of the Be^8 is something like 10^9 cm/sec. That is probably a reasonable average value to take. And the lifetime is 10^{-14} sec.

J. B. CUMMING: It is shorter than that, 10^{-15}, or something like that.

HALBERT: The average distance traveled then is 10^{-6} cm before it breaks up.

BREIT: That is not what really matters. If it is a 3-body disintegration, you don't care where the introduction of error goes. The question is not as to whether the Be^8 comes off as a whole if it splits off afterwards.

HALBERT: Do you mean to say that both alpha particles could enter the counter?

CUMMING: They go in the opposite directions though.

HALBERT: In the center-of-mass they do, but in the laboratory system they do not.

CUMMING: But you get down to the collimators.

BREIT: The binding energy of Be^8 is practically nothing. I wonder if I may ask another question, which is really a question of Dr. Reynolds. That is, if you cannot tell the loss of energy to about 3 Mev, as I understood you to say, how do you know that there isn't some excited state, say in the gold, that does not participate in the process, and that what is observed is partly inelastic scattering?

H. L. REYNOLDS: I think my figure of 3 Mev was ultra conservative; you can probably do a little better than that. The answer is, of course, if the loss of energy were less than just 2 or 3 Mev, we would not see it.

BREIT: Now you are coming down to 2 Mev.

REYNOLDS: I don't know what the exact answer is.

BREIT: What is the possibility of sharpening up on this so as to be sure that one is not observing inelastic scattering?

REYNOLDS: I think that we could do this quite easily.

G. F. PEIPER: Can you get down to 100 kv?

REYNOLDS: No.

CHAIRMAN BURCHAM: I think we shall have to bring this to a close very soon.

B. L. COHEN: This thing has been troublesome in every elastic scattering experiment that has ever been done by alpha particles, protons, or anything else. They cannot get rid of the first excited state in gold, for example.

J. O. NEWTON: One knows that exciting the state at 550 kv in gold will be supposedly of the order of 100 or 200 millibarns. So this might cause you some trouble.

CHAIRMAN BURCHAM: Thank you. Well, this experiment has obviously provoked a lot of interest and discussion, both technical and theoretical, but I think we must, in the interest of future speakers, now leave it at this point.

11. SCATTERING AND REACTIONS OF HEAVY NUCLEI

J. S. McIntosh
Yale University

Some angular distribution data on the scattering and reactions of heavy nuclei is already available, and by the looks of things this morning there is going to be a lot more forthcoming in the near future. Certainly it becomes interesting to try to analyze this data. Dr. Reynolds has already referred to some of the preliminary data published in this field on the scattering of nitrogen-14 by nitrogen-14 by Reynolds and Zucker.

They did their work with bombarding energies from about 15 to 21.7 Mev and analyzed their data with rather striking success in terms of the Blair model, which has already been discussed. Now I am not going to talk in any detail about the Blair model (Dr. Blair himself, of course, will talk to us at the end of this session), except to make a few comments on Reynolds' and Zucker's analysis.

For laboratory bombarding energies of 21.7 Mev they found that the angular distribution curve lies well below the Mott scattering curve, and it agrees pretty well with that of the Blair model in which all partial waves from L=0 to 6 are absorbed from the Coulomb wave. The corresponding nuclear radius they determined from this fit is about $1.66\ A^{1/3} \times 10^{-13}$cm, and it might be said, of course, that this data is symmetric about 90 deg and, therefore, might mask any deviations beyond 90 deg, where one might expect perhaps larger deviations for the Blair model. However, it really fits quite well. In any event, it is of interest to see if one can use a slightly less specialized model than this, with somewhat less drastic assumptions, also to fit the data, based on the general properties of the S matrix. One such model has been suggested by Professor Breit, and some calculations have been carried out by J. E. Turner, S. Park, and myself which try to do something of this sort.

Firstly, the spin-dependent forces are neglected. In this kind of business, at this stage, it would certainly complicate things considerably to bring them in, but this does not mean that we really believe that they are not important.

Secondly, it is assumed that for each partial wave there are only two channels available, one for the elastic scattering and a second one in which is lumped everything else, all the inelastic scattering and all the reactions.

Now the S-matrix elements for each partial wave L for this second group of channels, the reaction channels $(S_L)_R$, are then estimated in the following way: A reasonable nuclear radius something like $1.45A^{1/3} \cdot 10^{-13}$ cm is assumed, and the values of the regular Coulomb function, F_L, are then ascertained at this nuclear radius. Now, since so many inelastic and reaction processes can take place with these heavy ions, it is assumed that if a particle leaks into the nucleus, it probably reacts very strongly, and therefore for large L, the reaction S-matrix element is assumed to be proportional to the value of the regular Coulomb function F_L at the nuclear boundary.

Of course, if there are no interactions at all, the radial parts of the partial waves are essentially just F_L's, and when various other processes set in, scattering or anything of the sort, these change from F_L; but for the larger L values assumption will be made just in order to pin down $(S_L)_R$.

In the opposite extreme, for small values of L, where there is probably very great absorption or, in other words, inelastic processes and reaction processes take place very probably, we have assumed complete absorption, as the Blair model does, in the case that the regular Coulomb function at the nuclear boundary has attained its maximum. In other words, if one plots F_L as a function of R_N--suppose this is the nuclear radius--if the particular value of F_L has essentially gotten up to its maximum at R_N we assume complete absorption. This does not mean that F_L has very much to do with the actual wave function for these cases in which strong interactions have taken place. It is just used as a measure. Certainly, if F_L is large there is probably a good chance that the particle has gotten close enough to the other nucleus to cause some sort of reaction.

Now for intermediate L, the values of $(S_L)_R$ are simply interpolated between these two extremes. So, for instance, if one plots $(S_L)_R$ as a function of the square of the regular Coulomb function, the model that we have chosen is something like this; for $F_L(R_N)$ small, corresponding to the large values of L, the curve starts off linearly. For F_L large, corresponding to the small values of L, one has complete absorption $(S_L)_R=0$, and then in between we interpolate a smooth behavior. As a matter of fact, the value we have chosen to end our linearity is about $\left[F_L(R_N)\right]^2=0.3$, something of

that sort which may be a bit high, but this is the model we have assumed. This means that in the N^{14}, N^{14} case at 21.7 Mev, the L=1 and one waves are considered to be completely absorbed and then the others partially so.

Now once you have fixed the value of the reaction matrix element, then by conservation of particles, the unitary property of the S matrix allows you to fix the magnitude of the scattering matrix element for each L wave, and since we only have two of these matrix elements for each L wave, this is just simply given by $1=(S_L)_R^2+(S_L)_S^2$ where subscript S stands for the scattering. So as I said, once $(S_L)_R$ is determined, the magnitude of $(S_L)_S$ is also determined. The phase of $(S_L)_R$ does not make any difference because only the square of its absolute value enters. Of course, the phase does make a difference as far as the scattering matrix element is concerned.

The real parts of these phases have to be determined, and I might write down $(S_L)_S$ for scattering is equal to $e^{2i(\delta_L^{(Re)}+i\delta_L^{(Im)})}$ for each value of L. Then, of course, this means that one has a term that is $e^{-2\delta_L^{(Im)}}$ (which is a number less than 1, and corresponds to the fact that part of the wave has been absorbed due to the inelastic scattering and reactions) times $e^{2i\delta_L^{(Re)}}$ where δ_L^{Re} is just the ordinary real phase shift. In the case of complete elastic scattering, of course, $e^{-2\delta_L^{(Im)}}$ would just be equal to 1, and one would have the usual S matrix element in terms of the phase shift.

For the large values of L, the real part of the phase shift is determined very much as it would be by a potential, since optically speaking it probably makes some sense to talk about a potential for these higher values of L. For the cases in which the wave is completely absorbed it does not make any differerence anyway because the phase does not enter, but for the phases of the L waves in the intermediate region--there are three or four of these--the phases have essentially been chosen to fit the data. Now this, of course, is a strong limitation, and if we had available angular distributions at several different energies, the energy-dependence of the angular distributions would give a strong limitation on the phase shifts and, therefore, would give a much stronger test of the model. At present the N^{14} data is such that for the lower three energies from 15 to 17.7 Mev the data essentially follows that of the Mott curve, that is, the Coulomb scattering curve and at 19.2 Mev, as Reynolds and Zucker point out, the angular distributions are not symmetric about 90 deg as, of course, they must be for identical particles and, therefore, there must be some

uncorrected error still in the data. So we essentially only have the data at 21.7 Mev bombarding energy to fit. The phases, of course, might all be determined in optical fashion. There is some question as to whether for the "inner" L waves, the smaller L waves, an optical model would be expected to be meaningful, but I think we will hear about that probably from the next speaker.

In our work, geometrical considerations, as I said, lead us to the total reaction cross section which includes all inelastic processes. We used a radius of a more usually assumed type of $1.45 \times A^{1/3} \cdot 10^{13}$ cm and then this determines the total reaction cross section which for these identical particles is given by an expression of this sort:

$$\sigma_{\text{total } R} = \frac{\pi}{k^2} \; 2 \left[\frac{2}{3} \sum_{L \text{ even}} (2L+1) \left| (S_L)_R \right|^2 + \frac{1}{3} \sum_{L \text{ odd}} (2L+1) \left| (S_L)_R \right|^2 \right]$$

I could, of course, write this without the absolute value signs in perfect generality. The reason for the factors, the 1/3 and the 2/3, is simply that for identical particles there are only certain states in the incident beam. The 2/3 means that 2/3 of the states are associated with even values of L. This corresponds to five quintet states and one singlet state, that being a total of six, and then there are three triplet states. So two-thirds of these spin functions have to be associated with even space functions and one-third with odd.

The $(S_L)_R$ are determined in connection with the above prescription. From this by the unitary condition the magnitudes of the $(S_L)_S$ are determined. Then the phases are fixed, and when one does this one fits the angular distribution curve which is given by

$$\sigma(\theta) = \frac{1}{k^2} \left[\frac{2}{3} \left| \frac{\eta}{2} \left(\frac{e^{-i\eta \ln s^2}}{s^2} + \frac{e^{-i\eta \ln c^2}}{c^2} \right) - \sum_{L \text{ even}} (2L+1) P_L(\cos\theta) e^{2i(\sigma_L - \sigma_0)} \frac{(S_L)_S - 1}{2i} \right|^2 \right.$$

$$\left. + \frac{1}{3} \left| \frac{\eta}{2} \left(\frac{e^{-i\eta \ln s^2}}{s^2} - \frac{e^{-i\eta \ln c^2}}{c^2} \right) - \sum_{L \text{ odd}} (2L+1) P_L(\cos\theta) e^{2i(\sigma_L - \sigma_0)} \frac{(S_L)_S - 1}{2i} \right|^2 \right]$$

where

$$k = \frac{\mu v}{\hbar} \quad ; \quad \eta = \frac{Z_1 Z_2 e^2}{\hbar v} \quad ; \quad s = \sin\frac{\theta_{c\text{-}o\text{-}m}}{2} \quad ; \quad c = \cos\frac{\theta_{c\text{-}o\text{-}m}}{2}$$

σ_L = Coulomb phase shift of L'th partial wave

I might write the phase shifts down quickly. For the inner two values of L = 0 and 1, we have complete absorption. Perhaps I had better put these on the board here. Anything over L = 10 is assumed to have no contribution at all. Well, I will write these numbers down quickly for what they are worth. This is one fit to the data.

Phase Shifts

L	$2\delta_L$	
0		$i\infty$
1		$i\infty$
2	0.000	+i0.350
3	0.274	+i0.171
4	-2.471	+i0.094
5	0.160	+i0.062
6	1.175	+i0.026
7	-1.159	+i0.016
8	-0.284	+i0.004
9	-0.175	+i0.002
10	-0.087	+i0.001
11	0.000	+i0.000 etc

Well, when one does use these values and compares the computed $\sigma(\theta)$ at 50, 60, 70, 80, and 90 degrees, (of course, $\sigma(\theta)$ is symmetric about 90 deg) with points taken from Reynolds and Zucker one has values of the center-of-mass cross section as something of this sort:

θ (c-o-m)	$\sigma(\theta$ c-o-m)	
	experiment	model
50°	$99.5 \times 10^{-26} cm^2$	$103.1 \times 10^{-26} cm^2$
60°	36.5	35.1
70°	13.4	13.5
80°	7.3	7.2
90°	6.4	6.4

This is not so terribly significant because, of course, we made wild assumptions in neglecting spin forces and also in the determination of these parameters exactly. Uniqueness is certainly not claimed for this solution, but it does show that with a reasonable value of the nuclear radius in this model one can fit the data reasonably well, and if we were to get data for five different energy ranges we would probably be able to test them all a great deal more exactly.

Now I might make just one or two comments on some rough considerations by Breit on what the value of the nuclear radius $1.66 \times 14^{1/3} \times 10^{-13}$cm $= 4.0 \times 10^{-13}$cm, as found by Reynolds and Zucker, actually means. If one considers the charge distribution as given by Hoffstadter something of this form, $\rho(r) = \dfrac{\rho_1}{\exp\left(\frac{r-c}{a}\right)+1}$ where c is the value of r at which ρ is equal to $1/2\rho_1$. (ρ is a function of r). Then c turns out to be something like $1.07\ A^{1/3} \times 10^{-13}$cm. So, for instance, one may consider the nitrogen nucleus in this fashion, in which c is equal to about 2.58 cm where one puts in A = 14. Then if one considers the chance that one proton is outside a particular distance R, then the thing that is important is $\dfrac{\int_r^\infty \rho(r)\, r^2\, dr}{\int_0^\infty \rho(r)\, r^2\, dr}$

If one sets this equal to about 1/7 one finds the value of R for which this is the case.

If one does something a little more realistic and considers the chance that one nucleon or 1/2 proton will be outside this distance R, then one can replace 1/7 by 1/14, and one finds then the value at which this occurs is $r = c + 1.11 \times 10^{-13}$cm.

Then if this proton is to reach into the nucleus one might assume that it would cause a reaction if it crept into the potential. Now the radius for the potential is about $1.28\ A^{1/3} \times 10^{-13}$cm and if one leaves something like 0.52×10^{-13}cm for barrier penetration, then the total distance between the centers of these nuclei turns out to be about something like 7.29×10^{-13}cm, which is a little less than the value of 8×10^{-13}cm, i.e. twice 4×10^{-13}cm that would be predicted by Reynolds and Zucker, but this certainly isn't bad because we have not taken into account direct interaction, or deformation of nuclei, which certainly take place, and then there is really no reason this prescription of one particle outside the shell should be the criterion, although it is fairly reasonable.

CHAIRMAN BURCHAM: Thank you very much for that very clear talk. Are there any questions?

M. L. HALBERT: I guess I didn't hear how you decide on the 1/7 or the 1/14.

J. S. MCINTOSH: Well, the 1/7 would be the chance that one would be at the radius outside of which one would expect one proton, say.

HALBERT: Merely because there are 7 protons or 14, I see.

C. E. PORTER: Perhaps I missed the point of one comment you made. I wonder about how you got the real part of the phase shift.

MCINTOSH: The real part of the phase shift is certainly the most arbitrary thing in this calculation for the intermediate values of L. It is determined, well, initially roughly essentially by optical considerations and letting the phase shift go to zero as L gets larger. However, in the intermediate region for three or four values of L it is determined quite arbitrarily actually to fit the data. This is certainly not a true test, therefore, of the theory, until one gets more angular distributions at other energies which would put definite limitations on the energy-dependence of these phase shifts.

H. L. REYNOLDS: I still don't exactly know what you mean when you say the radius is 1.45.

MCINTOSH: This does not necessarily correspond to your 1.66 at all. Your 1.66 is half the interaction distance as determined by Blair's model. Our value of 1.45 corresponds more to the usual consideration of the size of the nucleus in terms, say, of the radius at which nuclear forces become appreciable, something of this sort. It isn't terribly precisely defined. It is the point at which one takes the Coulomb functions; in that sense of the

word we don't mean it to be terribly precisely defined. I would simply say it is what is usually considered in connection with the size of the nucleus, not in connection with something way out here in the tail.

A. ZUCKER: Is that what comes out of the 7.29?

MCINTOSH: One has $1.28 \times 14^{1/3} \times 10^{-13}$cm = 3.08×10^{-13}cm. This added to 0.52, 1.11, and 2.58 gives 7.29×10^{-13}cm.

ZUCKER: In other words, 1.45 corresponds to 7.29? The whole thing is self-consistent with your model?

MCINTOSH: No, these are just simply some considerations as to how the 8×10^{-13}, which is gotten from the strong cutoff model, might arise. They have nothing directly to do with our model.

ZUCKER: You don't actually use that for your calculation?

MCINTOSH: No, this was just a separate consideration.

DR. BREIT: I think perhaps in connection with the last question we might say the significant thing is the difference that is caused by the consideration of the diffuseness, that we got an addition of about 1.1 plus 0.656 or so for Coulomb penetration, and essentially a correction of about 1.1 fermi for the diffuseness of the boundary, and that total number is about 7.5. So you have somewhere near 20% correction. This 20% correction, when applied to the 1.45, would bring you down into the radius of the main part of the Hoffstadter distribution. So either way it seems to agree with the Hoffstadter distribution.

On the other hand, in this case the Hoffstadter distribution is not very good because we are looking at the gaussian error distributions that Hoffstadter gets. I mean, the Fermi distribution is not quite right. It does not tell one whether one takes the other model or this one--but those were crude estimates.

12. NITROGEN-NITROGEN ELASTIC SCATTERING

C. E. Porter
Los Alamos Scientific Laboratory

Essentially, any introductory remarks that might be appropriate at the outset of this talk have been fairly well covered by the previous speaker in terms of setting up the way in which one thinks he would like to do the problem, and so there is not so much need for me to go into a great deal of background. The data are, of course, the data of Reynolds and Zucker here at Oak Ridge. The data of four energies have been mentioned: 15.0 Mev (this is nitrogen-nitrogen) 17.7, 19.2 and 21.7 Mev. Now one is interested in describing somehow the collisions of these two nitrogen nuclei, and there is always the issue of how to do this.

I think it is fair to say that every model of any type that has been made so far has always involved at least the assumption of a spherical interaction region. This is, of course, questionable, but I think only by the pressure of more data will these assumptions be really changed. The best fit with approximate models so far I think has been the fit by Blair, and this, as described by McIntosh, was essentially based on chopping up the exact amplitude in a way that would make it possible to do a calculation in a reasonable length of time.

Slide 1 shows the fit to the high energy data. This is the 21.7-Mev data (taken from the Reynolds-Zucker paper). The curves here are the classical Coulomb scattering and the Mott scattering. That is to say, these wiggles in the Mott curve come from the interference terms that arise. For instance, this puts in wiggles about the classical Coulomb scattering. Then the points, of course, are the data. This is the laboratory angle. The lack of symmetry here is not the lack of symmetry that McIntosh mentioned that appears in the center-of-mass data. This comes, in part, from the transformation to the laboratory system.

The solid curves are calculations based on Blair's model for these different maximum ℓ-values, and you can see the motivation for picking the ℓ-maximum of 6. This fit is extremely good, probably as good as the data. I don't want to argue about the accuracy of the data because I am not qualified to do that. This was not the only prejudice that was flourishing

about this time with respect to how to handle such things as alpha scattering and also the nitrogen scattering. A completely classical model was also being used. This is shown in the next slide.

The results of these are shown in Slide 2. Again the data is the same. The oscillational wiggles that come from the diffraction effects in the series are simply not present, of course, in the classical model, but at least one can get something like the data, but how you pick the parameter of the interaction radii divided by the mean free path at the center of the nucleus is not completely clear. I mean, you don't have the structure fit, for instance, that Blair fitted so beautifully.

Well, I think what happens then is essentially that one would like to know two things. First of all, you would like to get an accurate fit to the data because these two models that have been talked about here are good in the sense that they are suggestive. (Blair's is, of course, the best fit.) So one would like to get a better fit to the data from something that isn't an approximate model. Secondly, one would also like to ask the question: Is there some meeting ground between these two models? I think that the answer to this second question is, "yes". Well, you can judge for yourself the type of fitting you can get from a model that can be calculated exactly.

The optical model calculations that I will talk about involve the symmetrization, of course, that McIntosh wrote down, namely, you have

$\frac{d\sigma_{el}}{d\Omega} = \frac{2}{3} \left| f(\theta) + f(\pi-\theta) \right|^2 + \frac{1}{3} \left| f(\theta) - f(\pi-\theta) \right|^2$. This differs a little bit from such things as for example alpha-alpha scattering in that in the alpha-alpha scattering you pick up only the even space amplitudes, so that a term of the odd sort just does not appear. In order to carry out this calculation we simply modified the same charged particle code that Igo and Thaler used to do the alpha scattering; this was just modified to sum these amplitudes in this way to calculate the cross section.

Now one can ask about what kind of parameters were used in the calculation, and I think here we come to where one can say that all these large interaction radii can be at least very much questioned. What was done, for simplicity and to conserve time, was to pick a Coulomb radius. First of all, I must emphasize again that it is always the spherical interaction region type of argument. So what I am dealing with is essentially a Saxon type of nuclear potential and a Ford-Hill charge potential.

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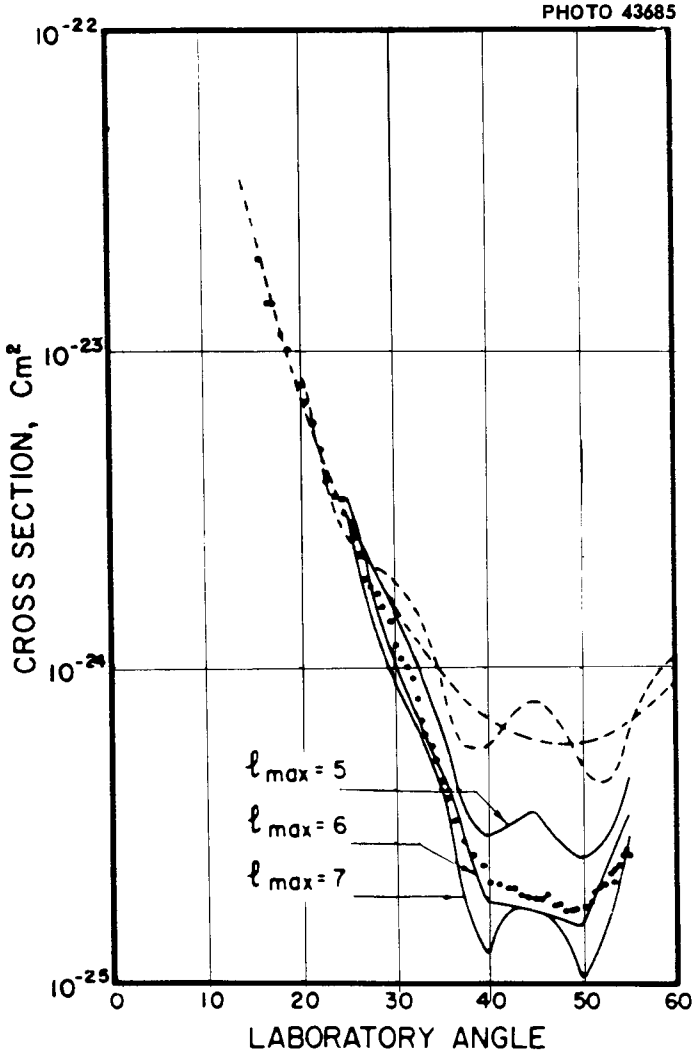

Slide 1

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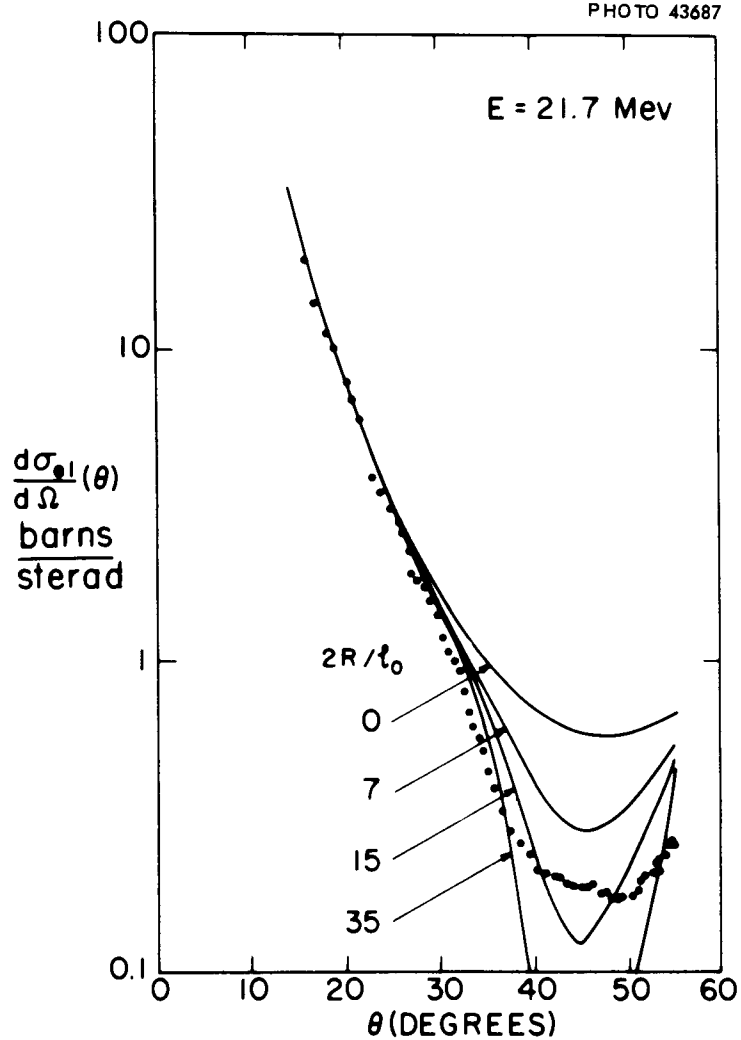


Slide 2

The parameters involved were the following: the charge radius was picked as 2(1.04) $A^{1/3}$fermi, and then the charge fall-off distance was about 1/9th of the charge radius. All of these things can, of course, be questioned, but the parameter space gets too large otherwise if you don't say something specific.

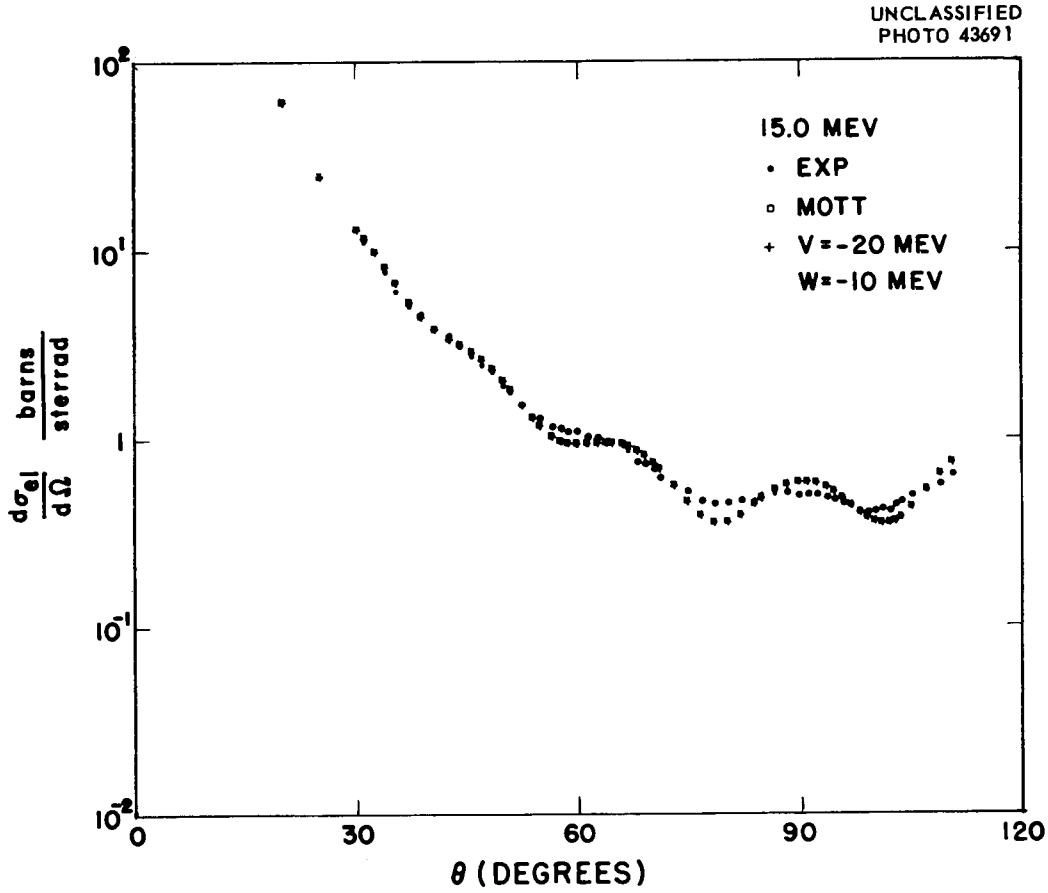
Then for the nuclear part of the calculation we took a nuclear radius of 2(1.25) $A^{1/3}$fermi (this is sort of based on nucleon scattering prejudices) and a fall-off distance in the Saxon potential type form factor of 0.6 fermi. Then, the real and imaginary parts of the potential were adjusted. V and W were adjusted to give the best fit to the data. This was done in the reverse energy order, for the reason that the high energy data was more sensitive to what you did; the low energy data, being almost Mott scattering, is simply not sensitive to the nuclear potential.

Well, what do we find? I think there is at least one small surprise and maybe two. I don't know the reason for it. It may be that we should pick other parameters and try again. But we find that V is about 20 Mev--minus 20 Mev. This is very small. I think that is the strange thing. The imaginary part is not so well specified in that we found values of -10 Mev; let me say it is ±6 Mev, but it is pretty well isolated to this low region here, and we explored qualitatively from zero to -200 Mev in both V and W. W is only isolated in the sense that 4 is less than the absolute value of W which is less than, say, 16, and so this makes a center of about 10.

Well, let me see what kind of fits these give. We have a sequence of slides now.

Three things are plotted in Slide 3. First this is the 15-Mev low energy data that is not changed very much by nuclear effects because you are not making a very hard collision. The experimental data are the points. The squares, which are almost always plotted over again, are the Mott data; this is the nuclear fit for 20 and 10. This is essentially one of the best fits that we got, and I think that again you will see that the fit is perhaps within the asymmetry qualifications of the data. I will leave that to the experimentalists to criticize if they feel that we are not doing nearly well enough.

Perhaps one technical point. The way in which these plots can be made is essentially such that they don't have to be touched by human hands. We take the cards out of the IBM machine, load them into the plotting machine, and let the draftsman draw a square around them.



Slide 3

Editor's Note - The author has called attention to the misspelling of "sterad" in this several subsequent slides. To avoid delay no corrections were made.

S. K. ALLISON: Where are the open squares?

DR. PORTER: There is almost no nuclear effect on the scattering, and so Mott and nuclear are plotted on top of each other here, and they don't show separately. In some cases you can begin to see a little bit of the square, but mostly these are squares and crosses almost on top of each other.

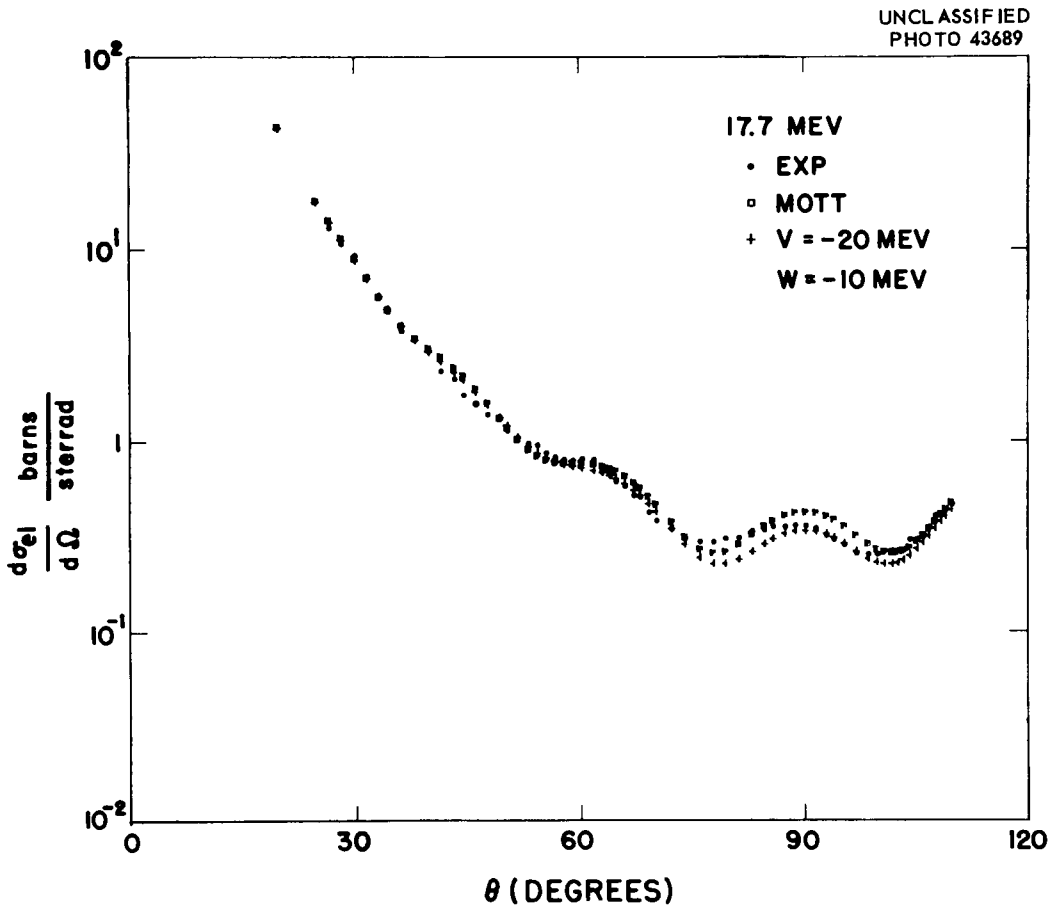
Well, let's go to Slide 4; here the energy is 17.7 Mev. Again the data does not differ very much from the Mott scattering. This set of squares with the points in the center is Mott scattering, and I think again this question of how symmetric is the data about 90 deg says that this fit isn't so bad.

Now we go to the next energy, 19.2 Mev in Slide 5. We are beginning to see the experimental data drop below the Mott scattering. The data are the points, and the crosses are the nuclear calculation with the optical model, and I only point out the data--well, one can argue a little that there is not as much symmetry here again as you would like, but we think this fit is probably adequate at the moment.

In Slide 6 again we have the Mott scattering squares. The experimental data are solid circles, and the optical model calculation is plotted as crosses. I would like some comments actually from the experimentalists on the next point. We ran, I am guessing, something like 50 tries to get these best fits. The point is that we think this fit is correct and we found these wiggles in almost all cases and they may be an important check point with respect to the data.

In addition, of course, in the optical model you will also get a prediction for the cross section for compound nucleus formation. In this case, Slide 7 with this fit, πR^2 is about a barn, a little over a barn. The barrier energy is shown and you can see that the cross section rises rapidly below the barrier. It is up to 100 millibarns about 5 Mev below the barrier. One additional comment on the slide. The importance of these measurements is tremendous in the sense that there are also other good fits which give different reaction cross sections, and if there were any way of measuring these total reaction cross sections, it would be of great help in the fitting.

We wanted in some way to go inside the optical model to find out what was going on with these phase shifts; I won't go into all these slides because I don't have time, but you can see, Slide 8, that the imaginary part of the phase shift qualitatively falls off as a function of angular momentum, and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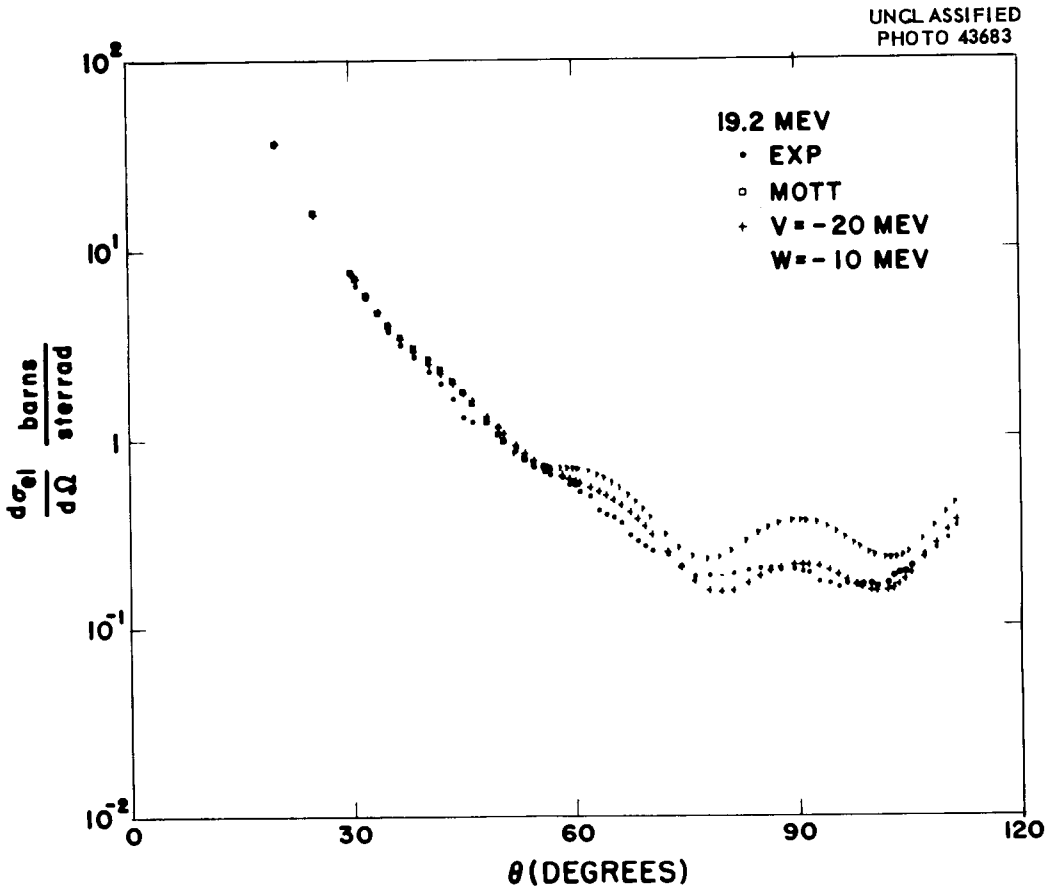
17.7 MEV
• EXP
▫ MOTT
+ V = -20 MEV
W = -10 MEV
10^2
10^1
1
10^{-1}
10^{-2}
$\frac{d\sigma_{el}}{d\Omega}$ $\frac{\text{barns}}{\text{sterrad}}$
0
30
60
90
120
θ (DEGREES)

Slide 4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19.2 MEV
• EXP
▫ MOTT
+ V = -20 MEV
W = -10 MEV
10^2
10^1
1
10^{-1}
10^{-2}
$\frac{d\sigma_{el}}{d\Omega}$ $\frac{\text{barns}}{\text{sterrad}}$
0
30
60
90
120
θ (DEGREES)

Slide 5

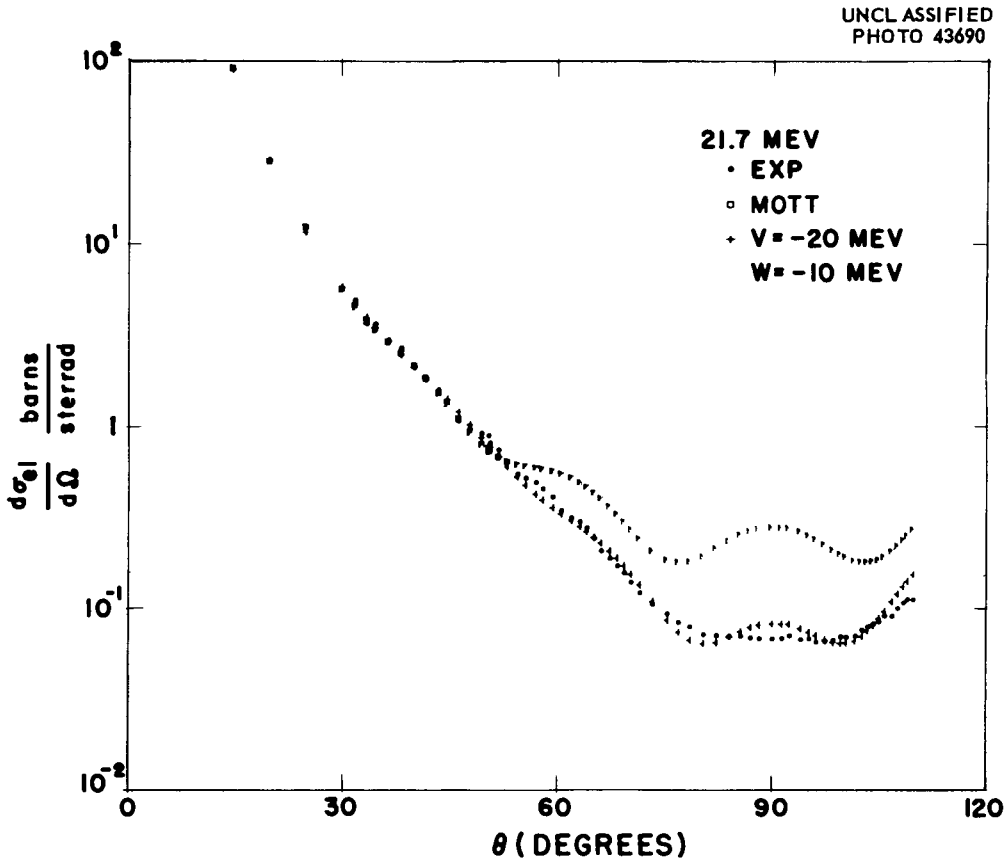
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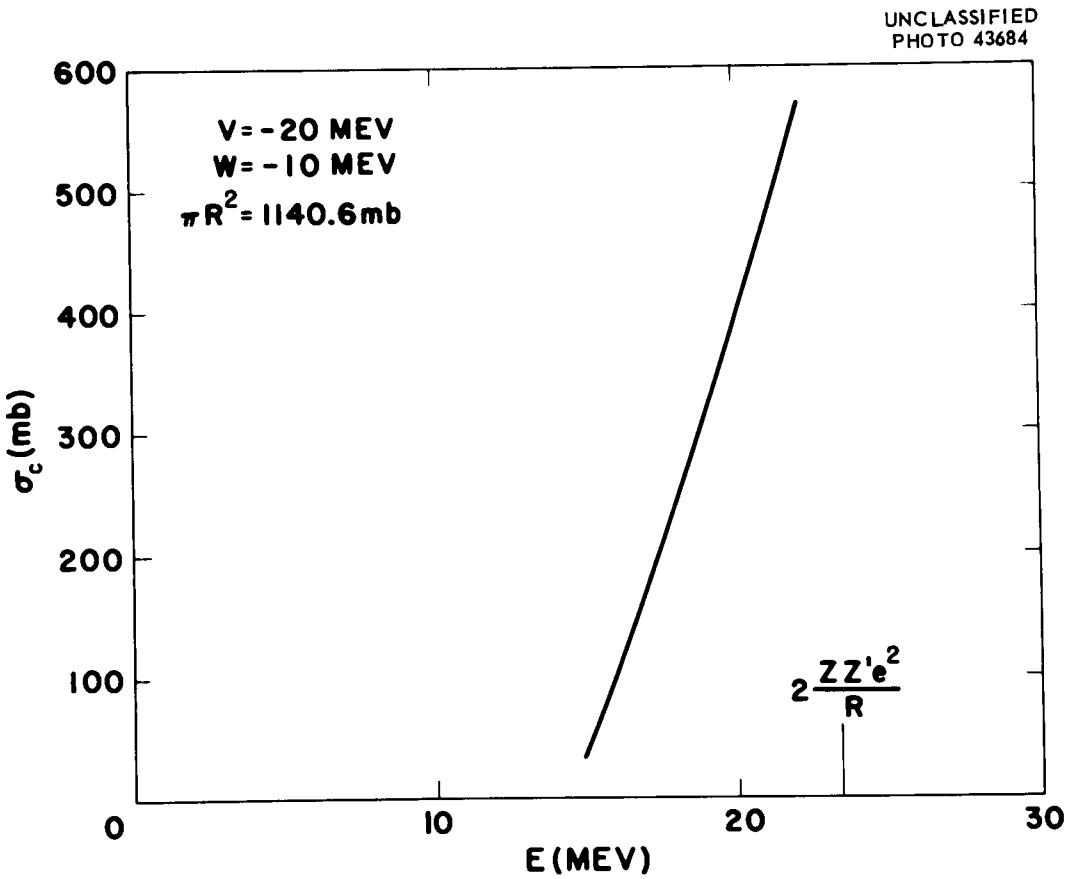
PHOTO 43690
21.7 MEV
• EXP
▫ MOTT
+ V = -20 MEV
W = -10 MEV
$\frac{d\sigma_{el}}{d\Omega}$ $\frac{\text{barns}}{\text{sterrad}}$
10^2
10^1
1
10^{-1}
10^{-2}
0
30
60
90
120
θ (DEGREES)

Slide 6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V = -20 MEV
W = -10 MEV
πR² = 1140.6mb
σc (mb)
600
500
400
300
200
100
0
10
20
30
E (MEV)
2 ZZ'e²/R

Slide 7

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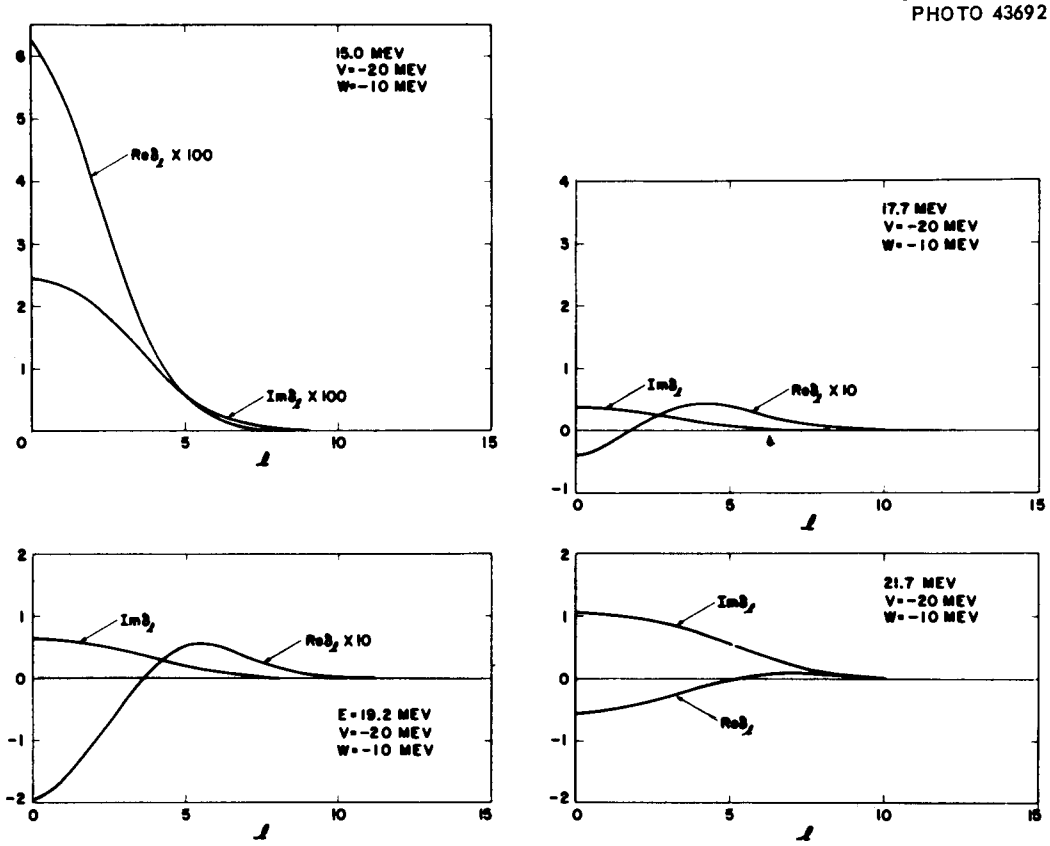

Slide 8

In the 15.0-Mev plot (upper left) the Re and Im labels should be interchanged.

that the real part arises up in the (Ford-Wheeler) classical real potential sense, but then it goes strongly repulsive because of absorption. The repulsive character of strong absorption was pointed out by Ken Ford.

Slide 9 is an attempt to reach a meeting ground between the Blair model and a strictly classical approach. We ran the optical model calculation at the highest energy with this best fit, but we set the real part of the nuclear phase shift equal to zero, so that the total real part of the phase shift was only a Coulomb phase shift. One can say essentially what was being done was to send particles in on Coulomb orbits and calculate the absorption actually from the optical model. Plotted here are first Mott and then the nuclear calculations. In this case the spots are the best fit and the open circles are the approximation of the real part of the nuclear phase set equal to zero. We think that this is evidence that the ideas at least in these classical models were correct.

Slide 10 shows how the exact Im δ_ℓ and an approximate classical calculation using a pure Coulomb orbit and a Saxon density form compare.

One final comment. Of course, one would like to try to understand the potential numbers, and you ask yourself how you can understand model numbers. Well, there is a very crude estimate in the sense that you can integrate, say, 2 densities times the potential for these two density distributions that are colliding with the 2-body potential.

$$V(R) = \int d^3n_1 \int d^3n_2 \; \rho(\vec{n_1}) v_{12} \rho(\vec{n_2}).$$

I won't go into all the details. I only say that one finds numbers if he doesn't put in any Pauli principle for instance, to keep the densities from colliding with each other, and so on, but if you put in numbers that have something to do with 2-body forces and you do very rough calculations, you get numbers out. You use gaussian shapes for all of these potentials, because then you can do the integrals--you get a potential as a function of the ineraction radius that looks like

$$\left[V(R = -760 \exp(-R/4)^2 \text{ Mev} \right]$$

R is in fermis. This number is very much too large; so there must be some repulsive effect in there, perhaps the Pauli principle again.

For the imaginary part Ken Ford and I made a very rough estimate in the following way: Imagine that you have a system of A particles colliding with a wall of nuclear matter. If all of these particles have to go through

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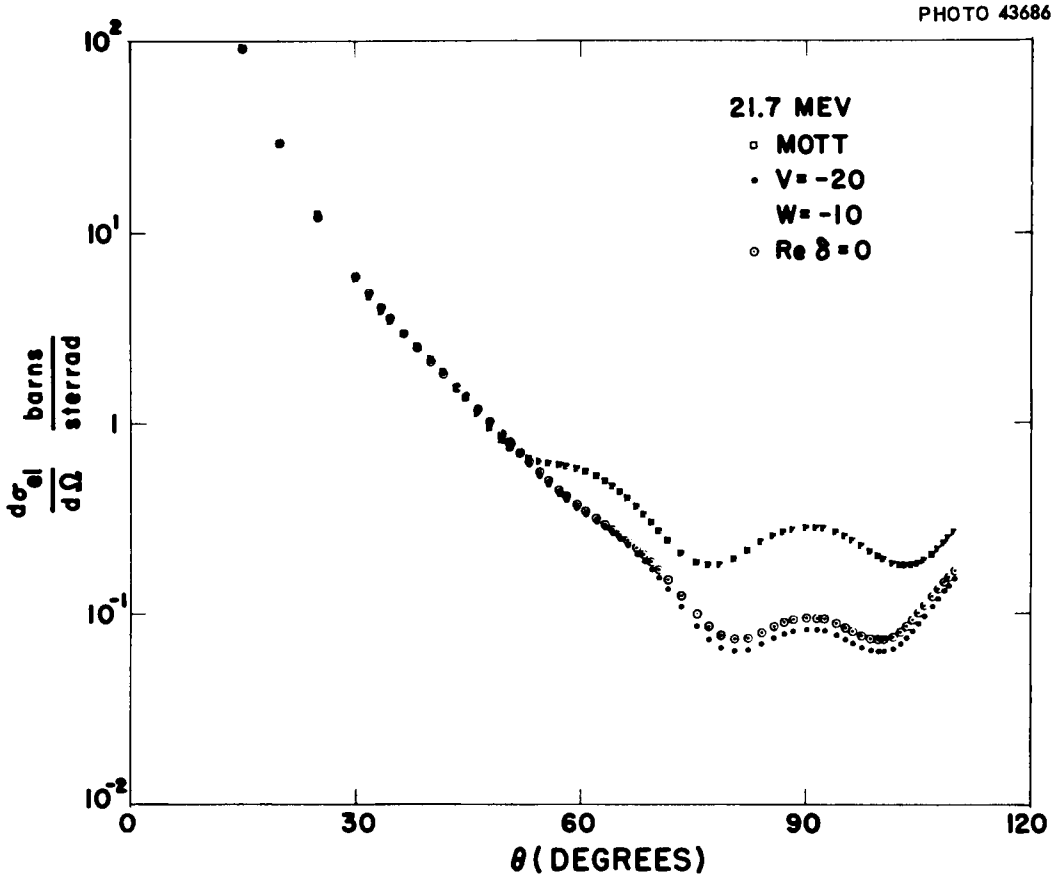

Slide 9

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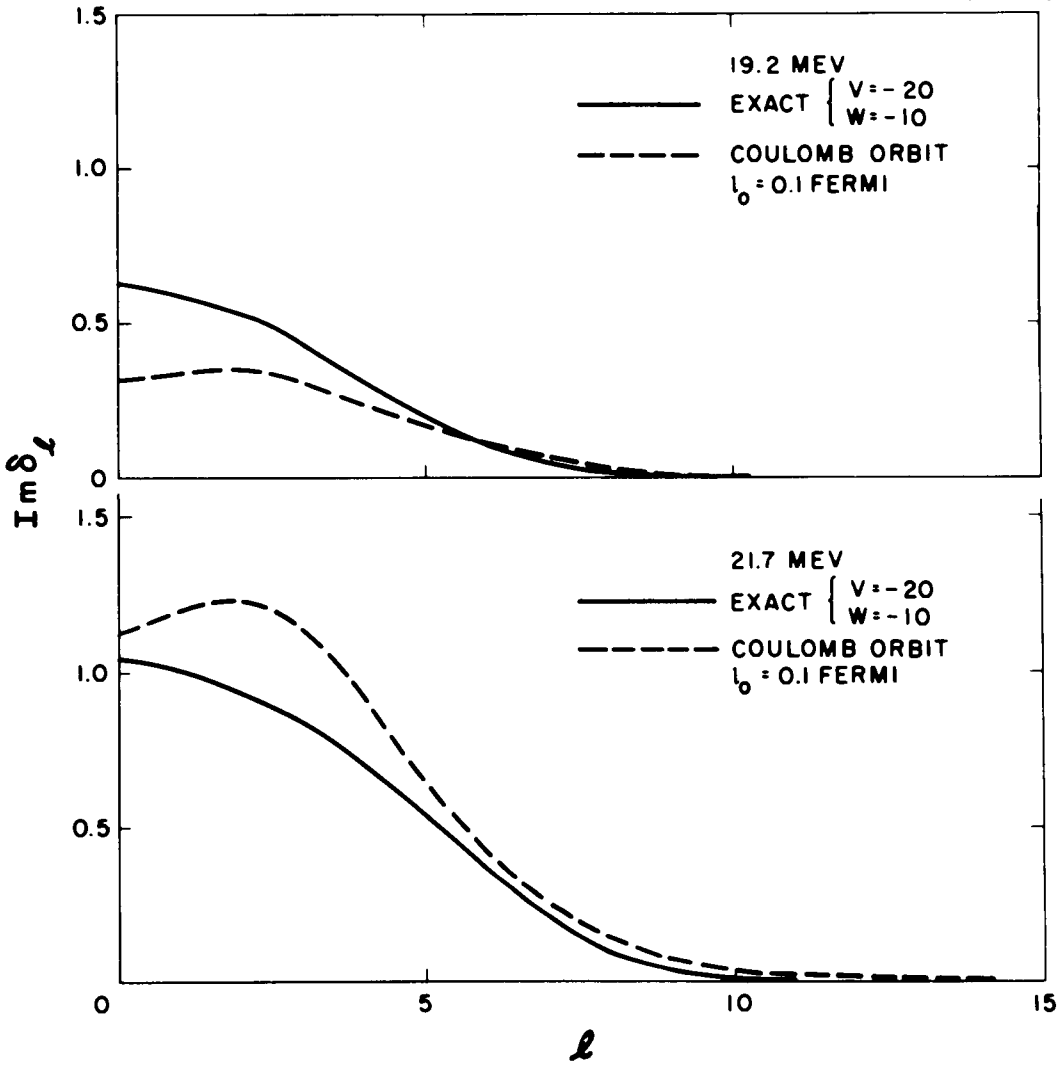


Slide 10

separately, then the probability that they get a distance X with the mean free path for each nucleon equal to ℓ nucleon is: $P^A = \left[e^{-x/\ell_{nucleon}}\right]^A$. So that essentially what one does is divide a nucleon mean free path by A. If you put in an A that is of the order of 10 effective particles, then you get something that is in the right neighborhood for the mean free path of the complex system.

CHAIRMAN BURCHAM: Well, thank you very much, Dr. Porter. Are there any questions on this paper?

A. ZUCKER: I understand this /V/ is very low. Is that right?

PORTER: Yes.

ZUCKER: Do you have any explanation why this is so?

PORTER: Only the one I mentioned near the end of my talk, that perhaps these densities don't like to get together because of the Pauli exclusion principle.

ZUCKER: What did you say at the end there?

PORTER: I said this very briefly because of the time pressure, but let me say it perhaps just a little more slowly, the point being that there was no exclusion principle put into this density business here, and this 760 says that it is essentially the rule of an A squared. I think you can say that you have pairs of particles and just an A^2 factor, but these things are excluded from interacting with each other in some sense that says this is just a terrible overestimation of nature as it exists. If we have time we hope to see what we can do by putting in some sort of exclusion to see if this will do it. It is not guaranteed.

A.E.S. GREEN: In deriving the nucleon nuclear potential one goes through a rather similar calculation and finds two large interaction terms. The repulsive terms come mostly from the repulsive core interaction, although the exclusion principle makes important contributions. In a typical case of the nucleon-nuclear potential derivation one might find something like 100 Mev attractive potential balanced by a 50 Mev or 60 Mev repulsive part potential coming mainly from the repulsive cores. This may indicate how to reduce your derived potential.

PORTER: I agree.

S. DEVONS: Surely, the repulsive core would not be very effective with the sort of wave beams one is dealing with. I think a more likely thing is what you said before, that it is a many-body effect. Skryme has analyzed

the effective potential, that is, the effective potential put in a sort of self-consistent nuclear model and he put in the effect of delta function interaction, but he put in a 3-body and a 2-body interaction and tried to find the effective potential with one nucleon in a many-body nucleus. He found that the 3-body interaction is quite definitely repulsive and balances out the 2-body attraction by about 20%. This seems quite independent of detailed assumptions in typical nuclear density. So you get a balance between the many-body repulsive effects and the forces which attract.

G. BREIT: I should like to ask in what sense are the optical models supposed to hold here. After all, you deal with 14 particles in 1 nucleus and 14 in another, and to take them literally one deals with the motion of 2 centers of mass. What is really the justification?

PORTER: I don't know. I think that is the first answer. I mean in a complete sense. The only thing I think perhaps one can say is that actually the optical model is only one way perhaps of representing damping plus scattering, and in that sense it seems to be general enough to fit many things, but the physical basis from the more fundamental point of view is not completely clear.

13. THE SHARP CUTOFF MODEL AND HEAVY ION ELASTIC SCATTERING

J. S. Blair
Princeton University

I should like to make two preliminary comments. First, to get the record straight, to my knowledge the sharp cutoff approximation was first introduced by Akhieser and Pomeranchuk in 1945, in discussing high energy proton scattering at forward angles.[1] It has also been discussed by Clementel and Coen.[2] The analogous boundary conditions for neutrons go back at least as far as 1937 to the work of Bethe and Placzek.[3]

Second, I find in actually working up this talk that what I am going to say is primarily concerned with general features of the sharp cutoff approximation and I will not be dwelling so much on the topics which are in the abstract. Some of you have received a copy of an open letter to Dr. J. McIntyre which furnished more details on these matters.

The topics I would like to cover are (1) Reasons why heavy ion elastic scattering should be of interest. (2) General comments on the sharp cutoff model. (3) The relevance of this model to heavy ion elastic scattering. (4) Predicted qualitative behavior of the angular distributions for the comparable experimental situations, alpha particle scattering versus classically corresponding heavy ion experiments. Let us define experiments with two different projectiles as classically corresponding when the Coulomb trajectories and the motion are identical. The necessary condition for this is that the ratios of the energy divided by the projectile charge, E/Z, are the same, and the ratios of mass divided by projectile charge, μ/Z, are the same. Thus, disregarding small center-of-mass effects, we can say that 40-Mev alpha particles on a nucleus classically correspond to 140-Mev nitrogen ions on the same nucleus.

There are at least three strong reasons for initiating a study of heavy ion elastic scattering. First of all, such experiments are going to be a tool for probing the nuclear surface and the interaction of the heavy ion-nuclear system; this information is useful in its own right. Interaction radii in the sharp cutoff model can be obtained by best fit analyses such as mentioned by Dr. Reynolds. If you don't have a high speed machine you can obtain sharp

cutoff interaction radii with the aid of the crossover point recipe, a graphical method discussed in a paper with Kerlee and Farwell.[4] Details of these angular distributions, when coupled with optical model calculations or other improved treatments, should give additional information regarding the nuclear surface.

It may turn out that there are no surprises. The interaction radii may be of the same order as found with alpha particle experiments plus a small increment due to the larger size of heavy ions. But one can't tell until one tries this. Distortion effects of the sort discussed by Breit, Hull and Gluckstern[5] may lead to much different interaction radii.

I would like to emphasize particularly the high resolution in determining the interaction radii that one finds in heavy ion experiments. One can measure an operationally defined quantity, the sharp cutoff radius, to an accuracy of 0.1 fermi. A "one percent" experiment in nuclear physics provides one with an unusually fine tool, which should be put to good use. So I would strongly urge experimentalists in this business to march up and down the periodic table looking particularly for deviations from the usual $A^{1/3}$ dependence due to closed shell, odd-even, and deformed nucleus effects. We think that we have found such deviations in the elastic alpha particle scattering experiments.[5] For example, U^{238} is anomalously large and the lead isotopes appear definitely small.

A second reason for interest in heavy ion elastic scattering is that almost any further study of heavy ion reactions involves the knowledge of the interaction radii. In Coulomb excitation investigations one needs to know how close one can come to the nucleus without getting specifically nuclear excitation. The radius must be known in order to discuss surface reactions such as neutron transfer and compound nucleus formation.

Let me cite a specific example where a direct measurement of the interaction radius is desirable. Some Russian physicists, Baboshkin, Karamian and Flerov[6] have measured activation cross sections produced by energetic N^{14} ions incident on gold. When those are combined with the determination of fission cross sections by Druin, Polikonov and Flerov,[7] total cross sections are obtained which are consistent with a "black nucleus" radius for gold $=9 \times 10^{-13}$ cm. This is a reasonable sort of radius, so they then concluded that the two processes, absorption of the N^{14} with subsequent boiloff of neutrons or fission, completely exhaust the

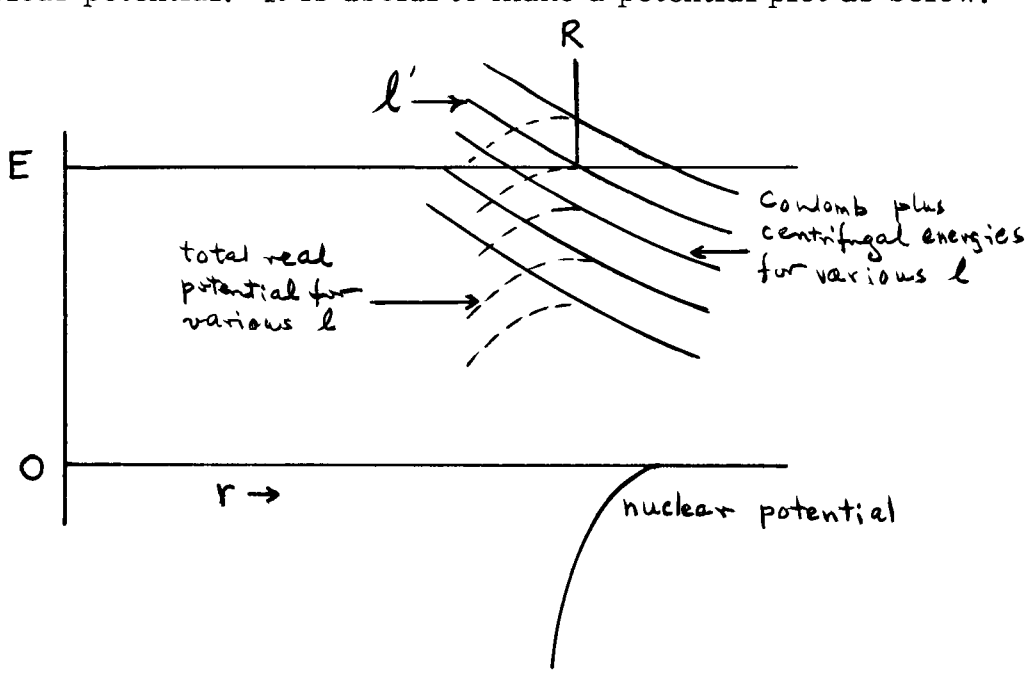
the available cross section. There is little possibility then of splitting off chunks of the N^{14}.

To check this conclusion an independent determination of the radius should be made. I should like to emphasize, as Dr. Reynolds has already mentioned, that the radius, R, which enters into the simple classical absorption formula, $\sigma = \pi R^2(1 - ZZ' e^2/RE)$, is also the sharp cutoff radius resulting from an elastic angular distribution analysis, as long as one forgets small differences between ℓ' and $(\ell' + 1)$. A third reason for interest in elastic scattering is that it is an easy experiment.

Let us now make some general comments about the sharp cutoff model. The sharp cutoff approximation has been mentioned several times by the previous speakers: One makes the very crude approximation that the amplitude of the outgoing ℓth wave, η_ℓ, is equal to zero for all ℓ less than or equal to a particular ℓ', the critical angular momentum. On the other hand, η_ℓ is set equal to the Coulomb value for all ℓ larger than ℓ'. From this one can define an interaction radius, R, by saying that the available energy, E, is equal to the sum of the Coulomb and the centrifugal barriers at R,

$$E = \frac{ZZ' e^2}{R} + \frac{\hbar^2 \ell' (\ell' + 1)}{2\mu R^2}$$

A possible physical justification for the approximation is the following: Suppose that at the nuclear surface--and I am not going to consider what goes on very far into the nucleus--there is an attractive nuclear potential. It is useful to make a potential plot as below.



Let us represent the Coulomb plus the centrifugal energy for a few values of ℓ in the neighborhood of the surface by solid curves. Next let us add on the nuclear potential and represent the total real potential for the various values of ℓ by dotted curves. We now suppose that if the particle can classically get into the nucleus it is done for; if it gets over the hump right at the edge it does not get out. So that the corresponding outgoing partial waves are removed from the elastic wave function. On the other hand if classically the particle can't get through the barrier, we say then that the corresponding wave is described asymptotically by the Coulomb phase shift. The largest angular momentum for which a particle can still classically enter the nucleus will be the critical value, ℓ' . This as well as the corresponding interaction radius, R, are indicated in the sketch.

If a square well is assumed for the nuclear potential, then the radius of the square well is exactly the same as the sharp cutoff radius. If we assume a nuclear potential which varies near the edge in an exponential fashion, proportionate to exp (-r/d), where d is the falloff distance, then an elementary argument[8] shows that $R \cong d + r_b$. In this formula, r_b is the radius at which the ℓth total potential has a maximum. At r_b the nuclear potential can be very weak. With the same exponential falloff in the nuclear potential, one finds[8] that the nuclear potential at r_b,

$$V_{nucl}(r_b) \cong -\frac{d}{r_b} \frac{(2E - ZZ' e^2)}{r_b}.$$

In the usual optical models d is of the order 0.5×10^{-13}cm. Thus $V_{nucl}(r_b)$, for heavy target nuclei, is 1/20th an energy of the order of bombarding energies.

In the sharp cutoff approximation one finds that the scattered amplitude is equal to the Coulomb amplitude minus the contributions to the Coulomb amplitude from all waves ℓ from zero up through ℓ' ; one is merely chopping out a chunk from the Coulomb amplitude. The model has the virtue that it is very easy to calculate a cross section. The first calculations that were done with this model were performed by my wife but she has been replaced by an IBM 604 computer. There is also the virtue that to the extent to which there is agreement between experiment and theory, the one parameter of the model is uniquely determined.

One of the consequences of the model is the 1/4-point recipe, which states that when the cross section has fallen to 1/4 of the Rutherford cross section, the corresponding classical distance of closest approach equals the interaction radius. This 1/4-point recipe is too rough for accurate determination of radii, but it is a very valuable guide in the design of experiments. For more accurate work it has been replaced by another graphical recipe which we have termed the cross-over point recipe; details are given in the paper with Kerlee and Farwell. The crossover point recipe is essentially the following: Instead of making a best fit analysis over all the angles, one matches the sharp cutoff calculations and experiment at one point of the curve. If there is a good general fit between the model and experiment, then the best fit and crossover point radii should be the same.

The physical idea behind the 1/4-point recipe is that the Coulomb wave packet for the scattering into a particular angle is centered about the classical trajectory in the following sense: Half of the partial waves comprising this packet classically penetrate inside of the classical trajectory and half of them don't get in that close. When the interaction radius is equal to the classical distance of closest approach, half of the amplitude is removed and half of the amplitude remains. The intensity, which is the square of the amplitude, is then 1/4.

To see that this isn't just talk, let us look at the Slide 1, which is a plot of the real part for the amplitude of the ℓ wave for pure Coulomb scattering as a function of ℓ. We choose η ($\equiv ZZ'e^2/\hbar v$) equal to 10 and the scattering angle, ϕ, equal to 60 degrees but we could have chosen any other parameters equally well. Further we could have taken the imaginary instead of the real part of the potential; one always obtains this sort of plot. We define ℓ classical, ($\equiv \ell_c$), such that $\hbar\ell_c$ equals the classical angular momentum for this choice of η and ϕ; thus, $\ell_c = \eta \cot(\phi/2)$. We observe that around ℓ_c there is a coherent band of amplitudes all with the same sign. When these are summed up, one essentially exhausts the real part of the Coulomb amplitude. It is in this sense that we say that the Coulomb amplitude is centered about the classical angular momentum.

One can see many things from this plot. Suppose we have a situation where the interaction radius is such that ℓ' is 5. According to the sharp

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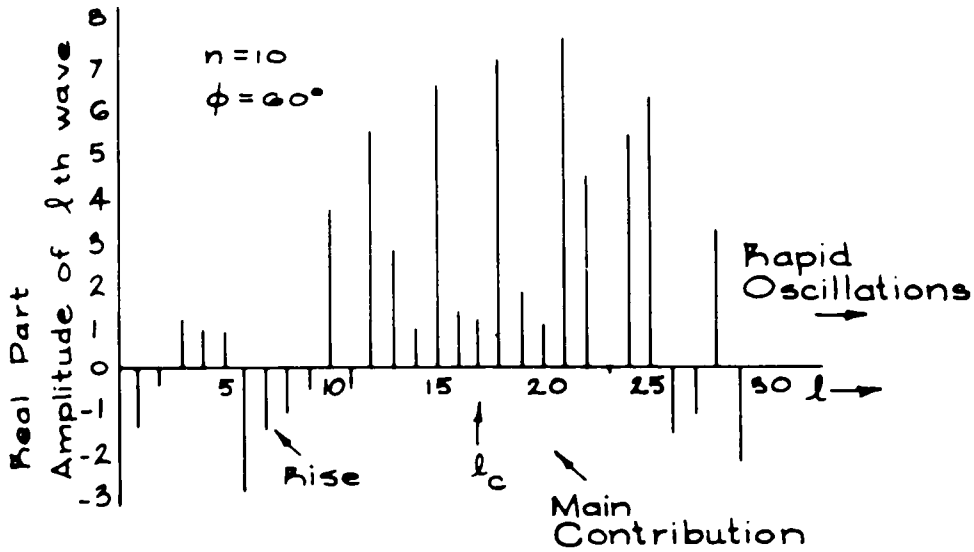


Slide 1

cutoff approximation, the scattered amplitude is obtained by chopping out all the contributions from $\ell \leq 5$. Since these are small in magnitude, and oscillate, their subtraction hardly alters the Coulomb amplitude. Next, suppose that ℓ' is 9. We then would chop out the group which is opposite in sign to the main coherent contribution. When we take this negative part out of the positive sum, we enhance the total amplitude. This is the explanation for the initial rise above the Coulomb cross section. This rise will always be present in the sharp cutoff approximation because one knows that in any sort of wave model, before the big crest there is always a trough. Take away the trough, and the integral of the amplitude is enhanced.

Suppose that ℓ' is between 10 and 25. There is a steady decrease in the cross section because one is chopping away at the main contribution of the Coulomb amplitude. One notes that from 25 on out the amplitude oscillates back and forth and that this oscillation increases in frequency. When ℓ' is out in this region, most of the contribution to the Coulomb amplitude has already been chopped out. There isn't very much total amplitude left over, so that it is a very sensitive business how any further amplitudes are removed. The sharp cutoff model is unable to give good answers from here on out.

One can also see from a plot like this what happens for a more realistic model. Suppose we have a gradual transition between pure Coulomb scattering and pure absorption, and that the effective ℓ' is around ℓ_c. Then the average cross section will be about the same as is obtained with the sharp cutoff model because the width of the main Coulomb wave packet is large compared with the usual transition regions. On the other hand, the diffraction oscillations and the initial rise are going to be sensitive to the nature of the width of this transition region, and one will need more sophisticated treatments to do an adequate job in discussing these.

One can say rather figuratively that the sharp cutoff model corresponds to swinging an ax at the Coulomb amplitude. This is fine for obtaining the gross features of the cross section but if one wants to know some of the details, such as information on the amplitude of diffraction oscillations or if one wants to know what happens when the cross section gets very small, one ought to whittle away at this Coulomb amplitude with a fine scalpel, such as provided by more sophisticated models.

Classical and semi-classical ideas should be particularly relevant to the heavy ion reactions, as has been emphasized by Breit and his co-workers.

In particular, the sharp cutoff model should be much better for heavy ions than it is for the alpha particle reactions.

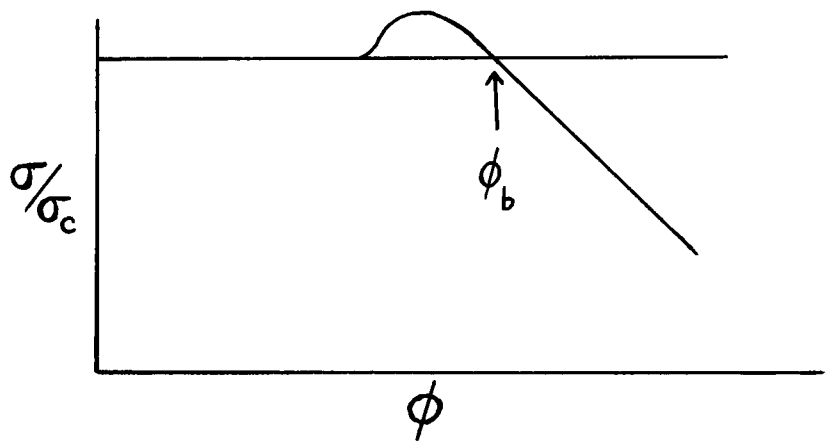
There are two parameters which essentially determine how classical is a given situation. One parameter is the quantity η which always appears in any quantum mechanical treatment of Coulomb scattering. Why is it a parameter? Simply because it is the ratio of a classical to a quantum mechanical distance, i.e. the distance of closest approach for head-on collision divided by twice the deBroglie wave length. When this quantity is large, the Coulomb scattering is largely classical. For 40-Mev alpha particles on lead, η is of the order of 8. For the classically corresponding experiment with nitrogen ions, η is 28. The other parameter is simply the deBroglie wave length, and for 40-Mev alphas this is of the order of 1/3 of a fermi. For the classically corresponding nitrogen ions this is 1/10 fermi. So, from both these considerations one sees that the situation should be much more classical with heavy ions, and one can make detailed arguments which support this view.

Considering the way time is running out on me, I will merely state the conclusions from such a study. The sharp cutoff model will be a good approximation when there is a rapid transition between complete absorption and pure Coulomb scattering. For this to be so there must be little transmission through the barrier of waves with $\ell > \ell'$ and also little reflection of partial waves with $\ell < \ell'$. The transmission can be estimated by the standard penetration factor $T = \exp(-2\int \gamma dr)$ where $\hbar^2\gamma^2/2\mu = V - E$. [There will be little reflection of waves with $\ell < \ell'$ provided the W. K. B. approximation is valid, or equivelently, provided the fractional change in wave length in a distance of a wave length is small in the surface region.] For classically corresponding situations, with the additional assumption that the surface attractive potentials have the same exponential falloff distance, the transmission factors through the $(\ell' + 1)$th barriers can be shown to be about the same. Similarly, the W. K. B. criteria are equally well satisfied for the $(\ell' - k)$th waves, where k is a given integer. The net conclusion is that the width of the transition region between complete absorption and pure Coulomb scattering, as measured in ℓ space in units of $\hbar$, is about the same for corresponding ions. But since the critical angular momenta are proportional to η for equal sharp cutoff radii, the transition width is relatively sharper for the heavier ions.

Let us pass now to a summary of predictions regarding angular distributions in classically corresponding alpha particle and heavy ion experiments. The particular properties considered are the location of the crossover point, the angle of break, the initial rise, and the diffraction oscillations.

If we say that the interaction radii are the same, then the crossover points will be the same in classically corresponding experiments. Now, actually the heavy ion will have a slightly larger radius, so that the heavy ion crossover point will probably occur at a slightly smaller angle.

Let us define precisely the angle of break, ϕ_b, to be that angle where the extrapolation of the break crosses σ_c:



The break will be sharper for heavy ions, i.e. the difference between the crossover angle and ϕ_b will be smaller. (The crossover points are between $\sigma/\sigma_c = 0.4$ and 0.25 for most angles of interest.) This reflects the fact that the Coulomb wave packet is narrower for the heavy ions. A previous slide illustrated how we may speak of the width of this wave packet; when the width in ℓ space is translated into a width in configuration space, one finds that the deBroglie wave length enters in a very essential way. The smaller the wavelength, the smaller is the wave packet.

Indeed, one can discuss this effect quantitatively and predict, for example, that with 140-Mev N^{14} on a Th target the difference between crossover and break angles is roughly 7 deg, whereas in the classically corresponding 40-Mev alpha experiment, it was twice this. The situation is more classical and therefore the break is more striking with heavy ions.

The initial rise is going to be at least as prominent for the heavy ions as it is for the classically corresponding alpha particles, and it is very gratifying to see Dr. Reynolds' information on this.

I would also predict that the diffraction oscillations would be less prominent in the case of heavy ions, and it is a surprise to me that diffraction oscillations are suggested in Dr. Reynolds' experiment. I find that one of the arguments given regarding diffraction oscillation in the letter to McIntyre is not correct, but it still does not change the qualitative conclusion that one would expect the diffraction oscillations to be more damped for heavy ions. Three explanations for this apparent discrepancy suggest themselves: (a) My arguments may be wrong. (b) The angular distributions in the comparable elastic alpha experiments may show more oscillation with finer resolution. This is not impossible, since the work of Ellis and Schechter at 48 Mev did show oscillations with lead and gold. (c) A third possibility is that the surface conditions are different for alpha particles and heavy ions and that this transition region is literally sharper in ℓ-phase for the heavy ions.

CHAIRMAN BURCHAM: Thank you, Dr. Blair. I am sorry that this very excellent talk has to be cut off rather abruptly. There is, however, just a little time left for discussion. Are there any questions, please?

M. L. HALBERT: I mentioned that in the beryllium experiment η was about 3. Can you predict off-hand what effect this would have if you tried to match the experimental data with the sharp cutoff model? In particular do the diffraction wiggles begin at a smaller angle than for $\eta > 3$? Is that what would happen in the sharp cutoff model?

BLAIR: I think $\eta = 3$ is about the smallest η with which we have had any partial success in fitting data. What I am thinking of is the 17-Mev proton scattering on gold where $\eta = 3$ and where things didn't work out too badly. On the other hand, I do know some other experiments where $\eta = 3$, and the sharp cutoff model does not work very well. However, I don't want to discourage you from trying this. I remember Reynolds writing me regarding the N^{14} on N^{14} experiments, asking if I thought the sharp cutoff model would work, and I felt that it would not.

J. O. NEWTON: With heavy ions and high bombarding energy there is a very strong coupling between the incoming particles and the excited states of nuclei. How is this going to affect the sort of considerations that

are being discussed today? Coulomb excitation cross sections are extremely high. This presumably may have some effect.

BLAIR: Yes. I haven't looked at the situation for heavy ions, but I know that for alpha particles this is not a negligible consideration. Let us talk completely classically and consider an alpha particle which is just grazing but just outside the nucleus, and, in a case where one has considerable deformation of the nucleus, ask what is the probability that this grazing alpha particle is going to excite the nucleus. One then finds probabilities of the order of 1/10th and, of course, this will get enhanced in heavy ion experiments. One saving grace in this is that these sort of phenomena are quasi elastic rather than true inelastic complications.

G. BREIT: Are you referring to the inelastic excitations or to virtual excitations?

NEWTON: Well, both. Both seem to be quite probable with these heavy ions, with probabilities of 1/10th or higher. Also I should mention that there is excitation of the low lying rotational states of a deformed nucleus from direct nuclear interaction. One can make estimates of probable excitation and they are not inconsiderable.[4] This effect tends to wash out the diffraction oscillations in the "quasi-elastic" angular distribution.

BREIT: It would seem to me that for the advance of the subject as a whole one of the main difficulties is the experimental one of the differentiation between the elastic scattering and the inelastic scattering in the case where you have excitations to low lying levels, and it would seem that one has to appeal to the experimentalists to use their ingenuity in differentiating these two processes.

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14. REACTIONS INDUCED BY HEAVY IONS IN COPPER AND THEIR INTERPRETATION*

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Centre D'Études Nucléaires De Saclay

The cyclotron in Saclay was able to produce N^{6+} and O^{6+} ions with a maximum energy of 9 Mev/nucleon. Since we have a continuous energy spectrum for the accelerated ions, we can say that most of the ions to be taken into account for the reactions have energies between 25 and 60 Mev. This allowed us to study the reactions in the intermediate mass region where no systemic study had been made. In this region we chose copper which also had an advantage in that most parts of our cyclotron are made of copper which could be expected to contaminate any other reaction.[1,2] The targets used were either natural copper or Cu^{63} and Cu^{65}. We used a stack of thin foils ($\sim$3mg / cm^2) to get a faster chemistry and so to be able to look at the shortest half lives.

In each irradiation, the target was divided into two parts. The first part was chemically processed; the nuclides were identified by their specific chemical reactions, by their half lives as measured by beta counting with a Geiger counter, and by the energy and the half lives of the emitted γ rays as measured with a γ spectrometer. On the other part of the target, we did not do any chemistry; the γ rays emitted by the different residual products were measured by γ spectrometry. Knowing the efficiency of the γ counter as a function of energy we had another way of getting the yields of the emitted particles. We took into account the decay scheme in our results. As we were identifying the nuclei by their radioactive properties, we had to forget all about: (1) formation of stable isotopes, (2) nuclides of very short or very long half lives, and (3) pure K-capture emitters not followed by γ emission.

The results of these different irradiations are shown in Slide 1. We have plotted the percentage of the different residual products versus the

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* This paper is based on work undertaken by the cyclotron group J. Beydon, R. Chaminade, M. Crut, H. Faraggi, J. Olkowsky, and A. Papineau in Saclay and by H. Atterling in Stockholm.

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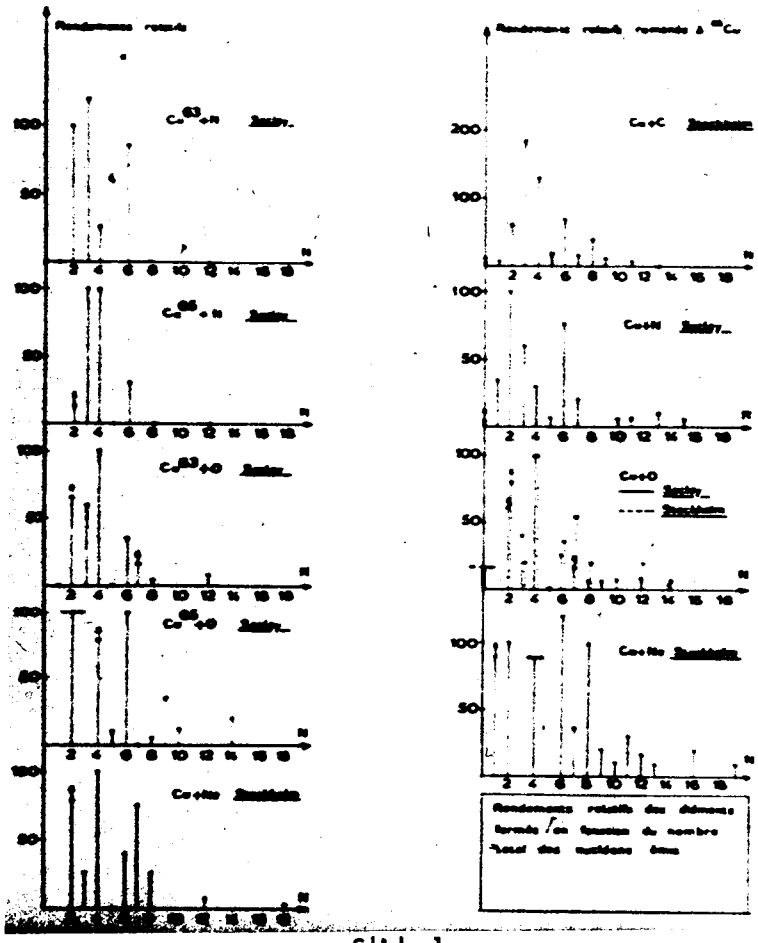

Slide 1

Relative yields of the different elements as a function of the total number of emitted particles.

number of emitted nucleons from the compound nucleus. On the left side of the slide, we give the results of irradiations of enriched isotopes, on the right for natural copper. Some irradiations by C, O, and Ne ions were made at the Stockholm cyclotron. Looking at the enriched isotope part, we see that the emission of one nucleon is very improbable and that we usually get the emission of 2, 3, or 4 nucleons. We do not find the compound nuclei because of the great amount of energy brought into the nucleus. The obvious fact is that we find as final products of the reactions, nuclei whose mass number is near the sum of the masses of the bombarding particle and the target. So, we think we were going through the compound nucleus.

To check this assumption, we have tried to get the same "compound nucleus" by two different reactions.[3] The resulting nuclei, after decay of the compound nucleus, should be found in the same proportions in the two reactions. These two reactions were $Co^{59} + Ne^{20} \longrightarrow Rb^{79}$ and $Cu^{63} + O^{16} \longrightarrow Rb^{79}$. The Q of the two reactions is only different by 2 Mev and the Coulomb barriers are respectively 39 and 34 Mev.

The experiments were made with thick targets and internal beams of 6+ ions, either in Stockholm for neon or in Saclay for oxygen. To eliminate the fact that we were using two different cyclotrons where the energy distribution of ions could have been different, we have looked in the two cases at the transmutation of natural copper by oxygen. The maximum energy of the neon beam (155 Mev) is not very different from the maximum energy of the oxygen beam in Stockholm. So we can say that the excitation energy in the two reactions Cu + O and Co+ Ne in Stockholm is about the same, similarly for Cu + O and Cu^{63} + O in Saclay, but that the two reactions Cu + O in the two cyclotrons are slightly different. We must use a normalization factor. So we have to compare the relative yields of the reactions $\left(\frac{Co + Ne}{Cu + O}\right)$ Stockholm to $\left(\frac{Cu^{63} + O}{Cu + O}\right)$ Saclay.

The results are given on Slide 2. The two first lines give the results of Co + Ne and Cu + O in Stockholm and the following lines Cu + O and Cu^{63} + O in Saclay. If we look at the results, we see that except for the case of Ga^{67} (which appears only in a small amount in the reaction), we find in a first approximation the same results for the four reactions. (Cu^{63} being present at 63% in natural copper, the differences are not big between the results for Co + Ne and Cu + O in Stockholm.) But for the two reactions

Cu + O, we find some differences which could be explained, as has been pointed out before, by the differences in the ion spectrum of the two cyclotrons. But we see that the main products of the reactions agree with:

$$\left(\frac{Cu^{63} + O}{Cu + O}\right)_{Saclay} \Big/ \left(\frac{Co + Ne}{Cu + O}\right)_{Stockholm} = 1 \pm 0.5$$

These are the products corresponding to the emission of a few particles. Ga^{67} which certainly does not come through the compound nucleus (it would correspond to the emission of twelve nucleons) shows a ratio quite different.

On Slide 3 we have the γ spectrum for the two irradiations Cu + O and Co + Ne. The two targets were irradiated for the same length of time and the spectra measured at the same time after the end of each irradiation; 5 days for the first spectrum, 7 days for the second one. Cu + O is not very different from Cu^{63} + O. The two spectra Cu + O and Co + Ne are quite similar, and we see that the different γ rays decay in the same way in the two targets. The only difference is the peak at 180 kev in Cu which corresponds to Ga^{67}.

Slide 4 shows the same results plotted in a different way. To check that we do not get our points inside the two lines 1 ± 0.5 by pure chance we have plotted the same quantities for the two reactions Cu^{65} + O and Co^{59} + Ne^{20} where compound nucleus is essentially different. In this case we find most of the points as expected outside the lines 1 ± 0.5. So it seems quite probable that most reactions take place through fusion of the colliding nuclei and that this provides a way of checking the theory of the statistical decay of the compound nucleus.

We have made the calculations for the two reactions Cu^{63} + N^{14} and Cu^{63} + O^{16}. The excitation of the compound nucleus was in general in the range of 30 to 50 Mev. We used the method given by Blatt and Weisskopf.[4] The energy distribution of the first emitted particle is given by the relation $I(\epsilon)\, d\epsilon = \epsilon\, \sigma_c(\epsilon)\, \omega(E)\, d\epsilon$

where: $I(\epsilon)$ is the number of particles whose energy is between ϵ and $\epsilon + d\epsilon$, $\sigma_c(\epsilon)$ is the cross section for the formation of the compound nucleus by the inverse reaction, and $\omega(E)$ is the level density of the residual nucleus which was taken as $\omega(E) = c \exp 2\sqrt{aE}$. The $\sigma_c(\epsilon)$ was calculated from the tables of Blatt and Weisskopf and of Shapiro. We calculated in this way the energy distribution of the emitted neutrons,

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Reactions \ Eléments	$^{77}Br + ^{77}Kr$	^{76}Br	^{73}Se	^{72}Se	^{72}As	^{71}As	^{67}Ga	
Co + Ne	80	25	40	35	40	25	5	Stockholm
Cu + O	80	20	35	20	25	20	20	Stockholm
Cu + O	60	40	25	5	10	5	7	Saclay
$^{63}Cu + O$	60	60	35	5	10	5	7	Saclay
$\left(\frac{^{63}Cu + O}{Cu + O}\right)_{Sacl} \Big/ \left(\frac{Co + Ne}{Cu + O}\right)_{Stockh}$	1	1,2	0,9	0,6	0,6	0,8	4	

Slide 2

The Stockholm and Saclay data.

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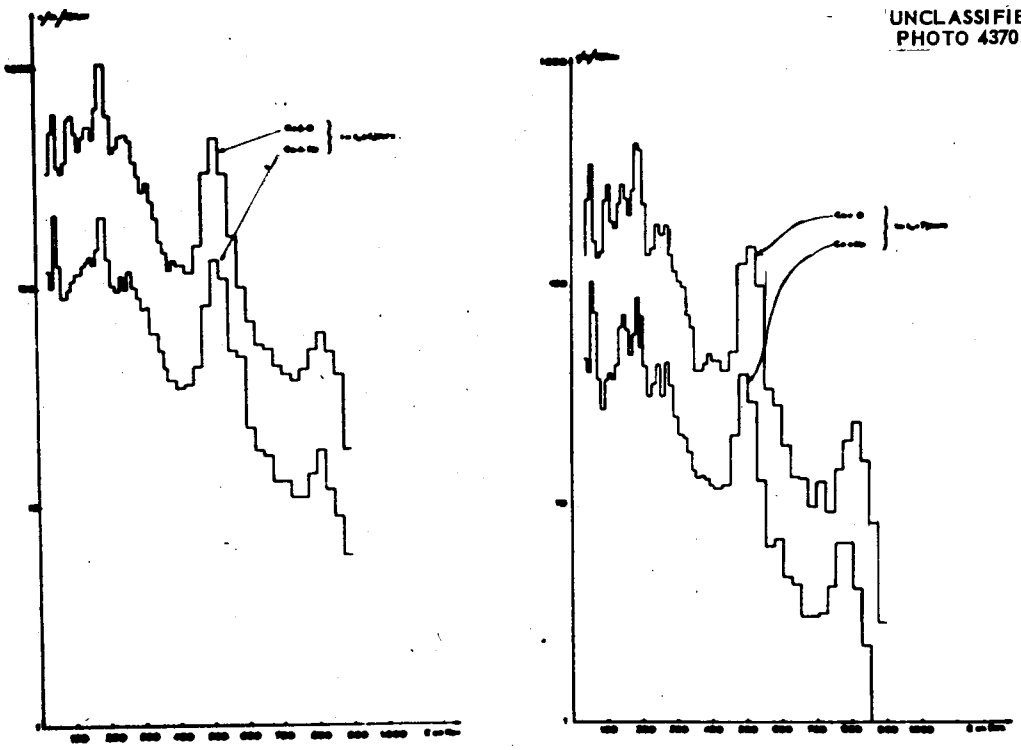
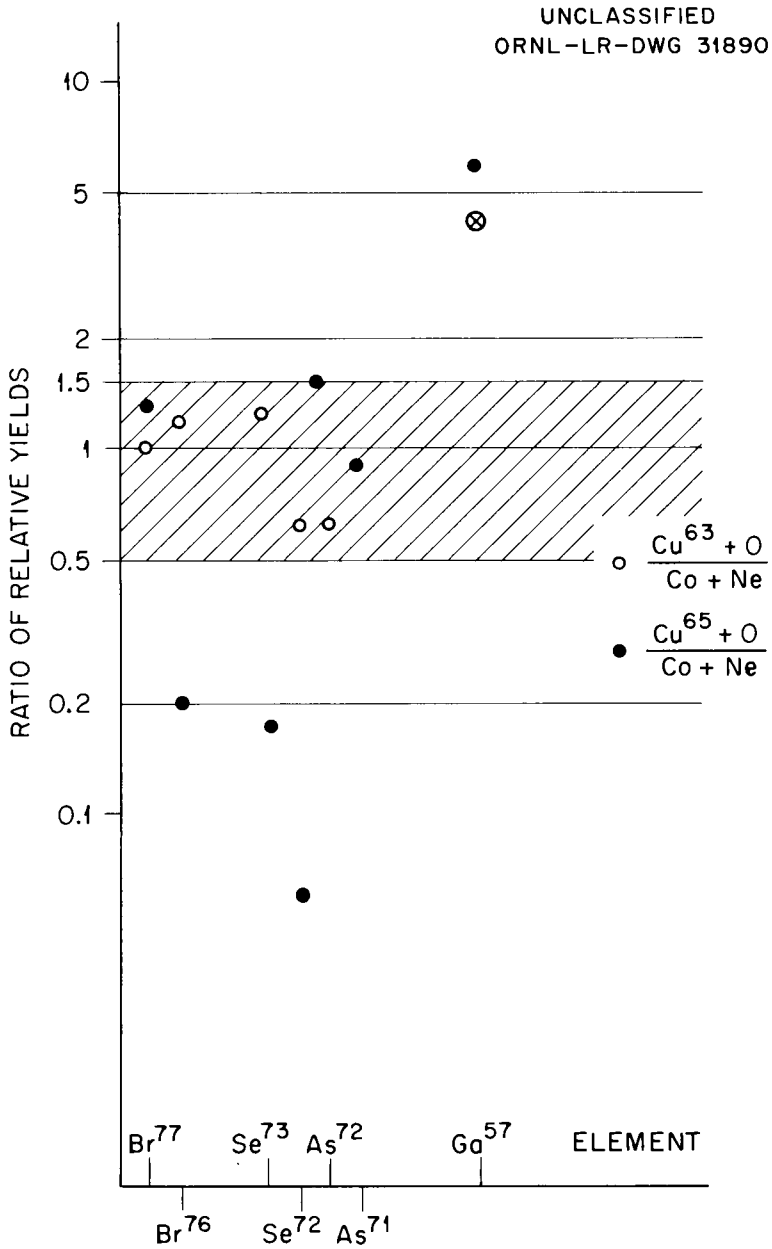


Slide 3

Gamma spectrum measured after four days and after seven days.



Ratios of the relative yields.

protons, deuterons, and α-particles. The calculations were made with a = 5.5 and for excitation energies of 30, 40, and 50 Mev. We found that the first particle emitted in these three cases had an energy not very different from the Coulomb barrier, so that the residual nucleus is still left in a very highly excited state, leading to the emission of other particles. We found that we could explain the formation of nuclei obtained by successive emission of four particles, but it was not possible to explain in this way the formation of the heaviest elements of the reaction. To explain these results, we have either to adjust the parameter a or to make other assumptions. If we try to adjust the parameter in the preceeding equation, we find that we have to take a in the exponential of the square root of aE equal to 2 instead of 5.5. That would mean that the level density is less than we think at 40 Mev.

In all these calculations, we have neglected γ deexcitation, assuming that any time there was enough energy to get emission of a particle, the particle was emitted. But as was pointed out by Butler[5] in the last Washington meeting, the (p, γ) process happens. It would certainly be very interesting to know if in our reactions high energy γ rays are emitted. On an other hand, we have used the inverse cross sections for the formation of the compound nucleus which are given for the ground state and are maybe a little different for an excited state.

We can make the assumption that the compound nucleus is formed in a less excited state than we think, that is to say, that some ions of incident energy below the calculated Coulomb barrier could be able to enter the target. As our beam contains a great quantity of such ions, this could explain the formation of the heaviest elements. This effect in turn, may be explained by using a bigger nuclear radius, or considering a deformation of the nucleus to decrease the barrier in some directions. It may be that the energy taken away by the first emitted particle is big enough to cool the nucleus quite completely. This would be expected if we use the picture of a first direct interaction process, or stripping process. So it would be very interesting to make some angular correlations with a mono-energetic beam, to find out what is the energy distribution of the emitted particles. The difficulty of interpretation would come from the fact that we have a cascade of emitted particles but, anyhow, it would be interesting to know if high energy particles are emitted.

To conclude, I will say that the striking fact of these experiments is that the heavy residual nuclei which really seem to prove the existence of a compound nucleus intermediate state, are the more difficult to explain by the direct application of the statistical decay of the compound nucleus.

CHAIRMAN SEABORG: Are there any questions?

M. L. HALBERT: I have something more in the nature of a comment than a question, but it bears on this problem of why there are not so many particles boiled off from the compound nucleus. In making calculations based on the statistical theory, now I am not speaking on whether the statistical theory is valid or not, but I am just saying, assuming it is valid, how does one make the calculations.

One ordinarily assumes that particle emission is the only process which occurs when there is enough energy to get a particle out. If the excitation energy of the nucleus is higher than the threshold energy, allowing the emission of a particle, you can imagine a process in which a γ ray is emitted with such energy that it leaves the nucleus in an excited state just below the threshold and so no particle is emitted.

Now your first assumption is to say, "Oh, no, above the threshold particle emission is much more probable than gamma ray de-excitation." That is true, but the initial population is much greater above the threshold because of the much greater level density there; so that the number of nuclei that de-excite to below the threshold can be appreciable, even though the probability of gamma de-excitation in any one transition here is small. Thus you can increase the yield of these nuclei at higher mass by a large factor. Under favorable conditions it can be a factor of 10, and it can even be a factor of 100 over what you would expect simply by integrating over these.

Now, of course, I am not saying that is the reason why you see so much of these elements. You gave about five other possible reasons and any one of those might do it.

MLLE . CRUT: What would be the energy of the gamma?

HALBERT: It would not have to be a high energy gamma ray. Most of the contributions actually come from just above the particle-emission threshold. Let's say you populate a state a few Mev above the threshold so that if particles are emitted they have to go to the ground state or maybe the first few excited states of the next nucleus. It would take a

gamma ray energy of no more than the same few Mev.to get down below the threshold. Since there are a million levels sitting here, waiting to receive it, compared with only two, three, or four levels waiting for the particle, the probability of de-excitation can be large. That is an exaggeration but the numbers are important in this calculation.

MLLE. CRUT: Yes, but I don't think that in our case this gamma ray would be enough to explain that our nucleus--

HALBERT: In our case of Fe^{53} to Mn^{52}, the lowest threshold is for emission of a proton, leaving Cr^{51}. Taking account of these de-excitations, we were able to increase the Mn^{52} calculated yield by a factor of about 100.

MLLE. CRUT: Yes, but we had to boil out four nucleons.

HALBERT: I am just trying to explain the large yield of nuclei close to what would be the compound nucleus.

J. B. CUMMING: This is, indeed, an effect. Probably if you are looking, say, at excitation between one and two particles, that is a big effect, but if the theory is six or seven, I don't think that would probably account for the deficiency. I don't think this can suddenly fill in the hole when these go through very high excitation.

HALBERT: I think it can.

ZUCKER: How high an excitation is it?

HALBERT: Between 30 and 50 Mev in the copper reactions.

ZUCKER: That is not so high.

CUMMING: What about gamma emission versus particle emission levels?

ZUCKER: There are also giant resonances that get in there.

CUMMING: It can account for a factor say of 10.

ZUCKER: Did you take into account any odd-even effects when you made your calculation?

MLLE. CRUT: Yes, we took them into account in the "C" of C exp $2\sqrt{aE}$.

ZUCKER: What did you take there?

MLLE. CRUT: By a factor of 2 between odd-odd and even-odd, odd-odd and odd-even, and a factor of 4 between odd-odd and even-even.

CHAIRMAN SEABORG: Are there any other comments on this interesting thought here?

G. BREIT: I would like to ask Dr. Halbert how the gamma emission was estimated.

HALBERT: The gamma emission was estimated from gamma absorption cross sections. The E-1 absorption was included by using experimental data for the giant resonance in the (γ, n) cross section. That absorption begins at a gamma energy of about 15 to 17 Mev. Below that energy I used the very rough estimates of Blatt and Weisskopf for M-1 and E-2 absorption.

CHAIRMAN SEABORG: In terms of times for gamma emission, average life for gamma emission, do you remember any number?

HALBERT: No, I didn't calculate it as a life, but the cross section reaches a peak of about 20 mb, I think, at 16 or 18 Mev. Below that it is maybe 1/10 or 1/20. At lower energies, of course, it goes down to zero.

BREIT: Suppose you make giant resonances followed by a single particle level with cobalt as the nucleus, how do you make that fit into this estimate, because with the giant resonances you go from the ground state up?

HALBERT: As I understood the treatment of Blatt and Weisskopf, except for the symmetry properties of nuclei, the dipole absorption would be very large. However, near the ground state of the nucleus it is suppressed by symmetry properties which make the dipole moment very small, and it isn't until you get to large excitations where it is possible to have appreciable dipole moment and thereby absorb dipole radiation as such. That is my understanding of the explanation.

BREIT: That refers to a given type of transition from ground up.

HALBERT: Yes, but I want the cross section for the inverse process here since we are talking about gamma de-excitation. Oh, I see what you mean. I am not starting necessarily from the ground state. Well, you are right about that and I don't know how to make any estimate of it. I did the best I knew how.

CHAIRMAN SEABORG: Any further comments?

HALBERT: Before we leave that, I will say, though, that where this effect is important is actually somewhat below the giant resonance region. It is the place where the M-1 and the E-2 are doing the job. It is the energy region right above the threshold for the next particle, which is about 6 Mev above the ground state.

G. BREIT: Wouldn't that require additional consideration? I mean the M-1 and the E-2?

HALBERT: I don't understand. I did bring in the M-1 and the E-2, yes.

BREIT: What did they have to do with the giant resonance?

HALBERT: The giant resonance is thrown in for the higher energies. I think if you took out the giant resonance for the reasons you mentioned this effect would still be important.

J. SCHIFFER: It seems to me that there is certainly ample data that for a single neutron channel available the neutrons can be very much larger than gamma. So I don't see how these considerations can be consistent with this.

HALBERT: My point was that there are so many levels to which the gamma ray can de-excite compared to the number--

SCHIFFER: Gammas from neutron resonances are what all these are. The gamma-ray widths are very small compared to the particle widths for just the one channel.

HALBERT: Yes, that is true. The point, however, is to compare the number of nuclei which drop below the threshold with the number there originally, since it is these nuclei which we observe.

ZUCKER: In many cases you are working with protons, such as this for example, where you are competing in the low cross section process. That is, the barrier is beating you down there and the gamma rays are becoming more important. I think in general the gamma de-excitation should be taken into account. I am not sure that this is necessarily the only way in which it can be done.

BREIT: But isn't gamma emission inherently less probable than particle emission if the particle emission is not inhibited by barrier?

ZUCKER: That is true.

BREIT: Then if you have many levels usually they become distributed. Of course, the collective effect is the equivalent of a single particle. As long as it was a matter like that then it appeared probable. But if the giant resonance really does not enter, then it seems to me that one must agree with Dr. Schiffer, or at least consider that he has a strong point.

ZUCKER: Well, I think that is true, but I think perhaps one might look at it a little more in detail, and when protons are coming out you still have to consider the number of levels, which for a high value of a are very strongly rising with energy of excitation. I think it is again a case of trying to use a high value of a which will favor the gamma ray process, especially near the ground state where there are only one or two levels available for the particle.

S. DEVONS: I don't think the details of the gamma ray are even important here, because if you go to high excitations, say if you use a single particle width, the width is distributed over many levels, but the product which is important here, the level density times the radiation for a single level, is even much more model independent. You are just expressing the same radiation with regard to the number of levels. So I don't think the details of the gamma radiation process enter into this.

BREIT: Suppose these many levels are produced by coupling to other degrees of freedom, then the strength of each transition is decreased.

DEVONS: But the number is increased in exactly the same proportion. You can lump it all together. That is exactly what we do. We lump it all together as one particle transition.

ZUCKER: I think that what Dr. Halbert was trying to point out was that we should consider gamma rays more than just simply say: Well, they are gamma rays, forget them, which is generally done.

DEVONS: I think there is a difference between E-1 and E-2 when you don't have a simple--

HALBERT: I still think it is correct that you cannot ignore the gamma rays; so you have to calculate and maybe use a different estimate for the probability.

DEVONS: If you take the gammas into account in not too sensitive a model. I think Dr. Breit remarked it was sensitive.

CHAIRMAN SEABORG: After an intermission we might go back for a few minutes to this discussion of the gamma emission. I have the feeling that Halbert got suckered into something in getting up to energy as high as the giant resonances.

HALBERT: Well, I don't know exactly what to say because I am not an expert on Γ_γ, and this effect, of course, depends very critically on

the numerical values involved. The first question is the probability of gamma ray de-excitation compared with particle emission, and until you have the right numbers you cannot really say very much. But assuming that you do have the answer to the first question, the next question is, how many nuclei above the particle-emission threshold de-excite to the region below it. The third question is, how does this compare with the number formed there originally.

The calculations that I mentioned earlier for Mn^{52} showed that 10 or 100 times more nuclei drop below the threshold than were present there originally. I make this point mainly to emphasize that people should look into gamma de-excitation before they ignore it.

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15. A SEARCH FOR RADIATIVE CAPTURE OF HEAVY IONS

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Heavy-ion reactions often show marked deviations in the yield of products from those to be expected on compound-nucleus theory.

For example, the prominence of β emitters among the products from light elements bombarded with heavy-ions contrasts with the prominence of products obtained by neutron evaporation from heavy elements bombarded with heavy ions.

Thus, in the bombardment of Al^{27} with N^{14} ions[1], P^{32} (2αp emission from compound nucleus) Si^{31}(2α 2p emission) and Na^{24} (4α p emission) occur in high yields. It would seem improbable that such evaporations would occur in such high yield with no neutron loss. Thus, a high proportion of "buckshot" type interactions would seem to suit the observed distribution of products, with evaporation and stripping processes playing only a minor role. No evidence of fission processes was observed by nuclear emulsion techniques.

On the other hand, in the interaction of heavy ions with heavy elements, as is well-known, evaporation of a considerable number of neutrons, followed by progress along the appropriate decay chain, appears to be the chief mode of de-excitation of the compound nucleus. (See, for example, interaction of N^{14} ions with tungsten, this conference). In the reaction of heavy ions with medium weight elements, however, it seems that the "buckshot" hypothesis and evaporation theory are less effective in explaining the observed product yields.

An example of this is the interaction of N^{14} or O^{16} with Cu[2] where a considerable excess of heavier products appears which is very difficult to explain by means of a direct interaction. By far the greatest proportion of product nuclei in the above reactions are those which are only 2 or 3 nucleons removed from the compound nucleus, so that it seems likely that compound nucleus formation is probably taking place.

However, the mean energy of the incident particles is about 50-60 Mev, and consequently, the emission of only 2 or 3 nucleons does not appear to fit with evaporation theory.

Also, the fact that by far the highest yields of the products are close to the compound nucleus indicates that stripping reactions are not very important.

Lastly, for the same reasons, "buckshot" interactions are also less important here than with lighter elements, since a wider spread of products further removed from the compound nucleus would be expected on a "buckshot" hypothesis.

Consequently, these pieces of evidence lead us to consider the possibility of other modes of de-excitation of the compound nucleus.

If there were a significant probability of the loss of perhaps 20-30 Mev by gamma emission, the previously mentioned results would be understandable.

However, since such an excitation energy would make a variety of heavy particle emission possible from a compound nucleus, the half life for the gamma emission might need to be of the order of 10^{-20} sec, or less, i.e. about 10^6 times shorter than for a normal allowed gamma transition.

It seems possible that this may be the case for the emission of giant-resonance gamma rays, for which the absorption resonances are of the order of 10^6 times wider than those of normal gamma rays and hence, which should have correspondingly shorter emission half lives. Alternatively, if the compound nucleus is de-excited by a cascade of gamma transitions, each transition to a lower level would increasingly discriminate against subsequent particle emission, and it may therefore be possible to de-excite the nucleus to the ground state by such gamma-cascade mechanism.

It may be that gamma emission to a lower excitation state, followed by particle emission, would explain the small number of particles evaporated in many of the products observed from Cu bombarded with N^{14} or O^{16} ions. Nevertheless, it was decided to attempt to find out if a total de-excitation by gamma emission could be observed.

Work done in the USA, where x-rays were directed through various target materials, did indicate that giant resonance absorption bands were observable. While such a process of exciting a nucleus to a high energy level by absorption of high energy x-rays is by no means analogous to the

de-excitation of a highly excited compound nucleus by gamma emission, it seemed worthwhile to look for this latter process.

Direct detection of such gamma rays would present extreme difficulty with the very small external beam of heavy ions available at Birmingham. An indirect method was therefore used. Namely, that of searching for reaction products which could only have been made by the emission of high energies in the form of gamma rays, that is the compound nucleus itself.

In fact, on searching through the chart of the nuclides, the number of cases where this can be done satisfactorily is limited, as the most advantageous cases are those where (1) the target consists of only a single isotope, (2) where the compound nucleus is radioactive to facilitate detection, (3) where the target material can be obtained of sufficient purity, and (4) when the chemical procedures can reasonably quickly ensure a radiochemically pure product.

The first reaction investigated was that of $Mg^{24}(N^{14},\gamma)K^{38}$ + 2.7 Mev $T/2 = 7.7m$. In two experiments, a few compound nuclei of an activity which could have been $T/2 = 7$ min were observed in the K fraction, but large amounts of Cl^{34} activity had to be chemically removed first, so that the short $T/2$ combined with the careful separation needed did not make this a very easy experiment. In addition the presence of possible $Mg^{25}(N^{14},n)K^{38}$ and $Mg^{26}(N^{14},2n)K^{38}$ reactions would make the use of separated Mg^{24} a necessity.

The next case tried was that of $F^{19}(C^{13},\gamma)P^{32}$. Since the binding energy liberated is 20 Mev and the potential barrier is probably about 12 Mev, the excitation will usually be above 30 Mev and cannot possibly be less than 20 Mev. Now, the thresholds for n, p and α evaporation from P^{32} are 7.9, 9.6 and 9.9 Mev, respectively. Hence, any of these can be emitted, and either charged particle can have an energy well above the height of the potential barrier against it. Hence, if any P^{32} is found in the ground state, the better part of 30 Mev must have been lost in the form of one or more gamma quanta.

"Spectrographically pure" lithium fluoride was used as target material with a 2-4 hr. bombardment at a beam current of about 3×10^{-8} amperes. Large yields of F^{18} and Na^{24} were obtained, of the order of

10^6 dis/min. A control experiment showed the small amount of Si (2 or 3 ppm) present in the LiF could only give about 1 count per hour of P^{32}.

In several runs, between 10 and 50 cpm of P^{32} were observed, identified by, (a) chemical separation, (b) half life measurement, and (c) absorption measurements. However, at this stage, it was decided to check the lithium fluoride for other contaminants by activation analysis, and it was discovered that about 630 ppm of chloride and nearly 200 ppm of phosphorus were present. Both these contaminants could, of course, give rise to P^{32} by heavy-ion bombardment. This necessitated a careful purification of the "spec-pure" material before further experiments could be done, and these have not yet been completed.

Next, spectrographically pure vanadium was used as target material, in the search for a $V^{51}(C^{13}, \gamma)\ Cu^{64}$ reaction. The elements which might cause trouble if present as contaminants are in this case more easily checked by spectrographic analysis than are chlorine or phosphorous. These elements (Mn, Fe, Co, Ni, Cr, Cu) were present to the extent of only a few ppm. This analysis was first confirmed by C^{12} bombardments of the vanadium, Slide 1; and the non-appearance of Cu^{64} activity was taken as a criterion for the absence of these metals of higher Z than vanadium. In the case of C^{13} bombardment of vanadium, Slide 2, an activity of considerably longer half life than the 3.3-hr.activity of Cu^{61} was detected in the chemically separated Cu fraction. It could have been the 12.8-hr Cu^{64}, but the counting rate was so low after the decay of the Cu^{61} that exact determination of its half life proved difficult. From there preliminary experiments, the yield of such a compound nucleus, even if real, would not appear to be very large.

In the reaction $F^{19} + C^{13}$, the yield of P^{32} is only about 1/500th of the yield of Na^{24}, and in the case of the $V^{51} + C^{13}$, the yields of the Cu^{64}? is only about 1/500 - 1/100th of that of Cu^{61}.

However, the energy of the compound nucleus cannot be well defined in these experiments, owing to the wide range of kinetic energies of the C^{13} beam. Most compound nuclei formed probably had excitation energies of between 45 and 100 Mev, so that the results would not be inconsistent with gamma emission being an important mode of de-excitation near 30 Mev.

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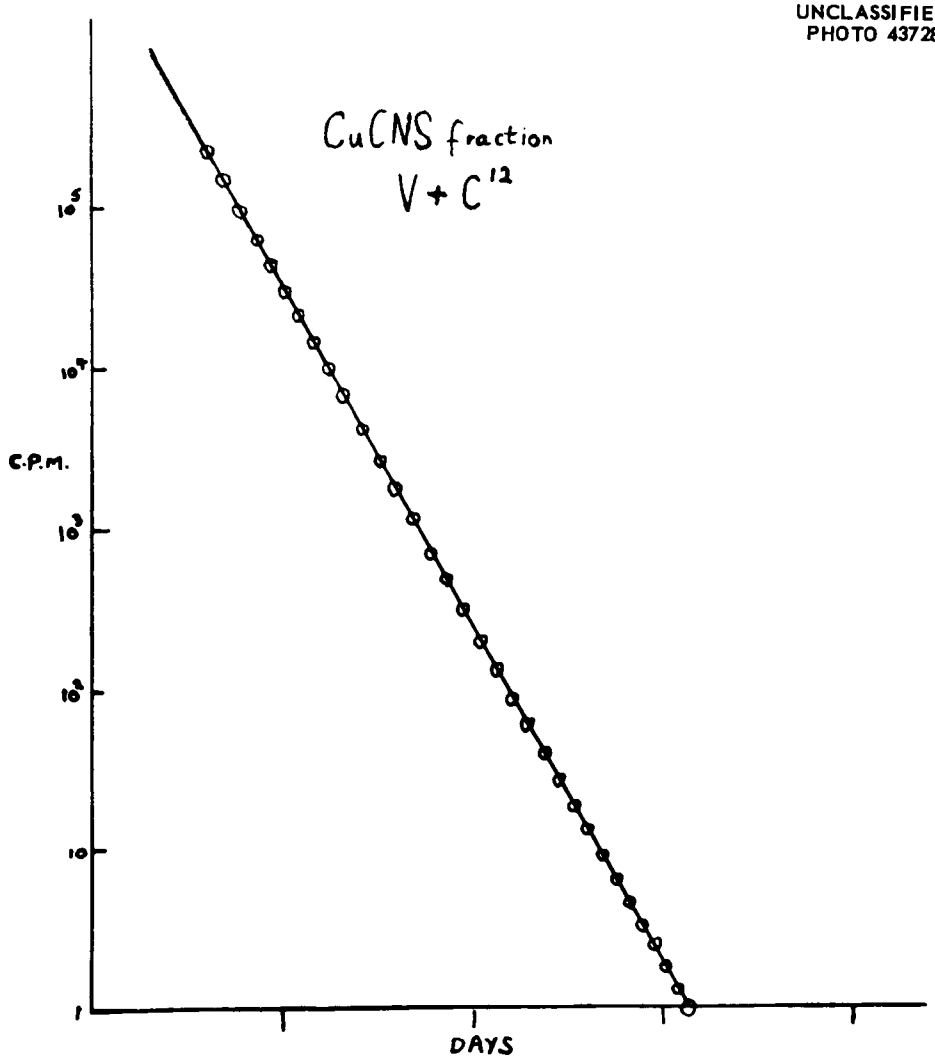

Slide 1

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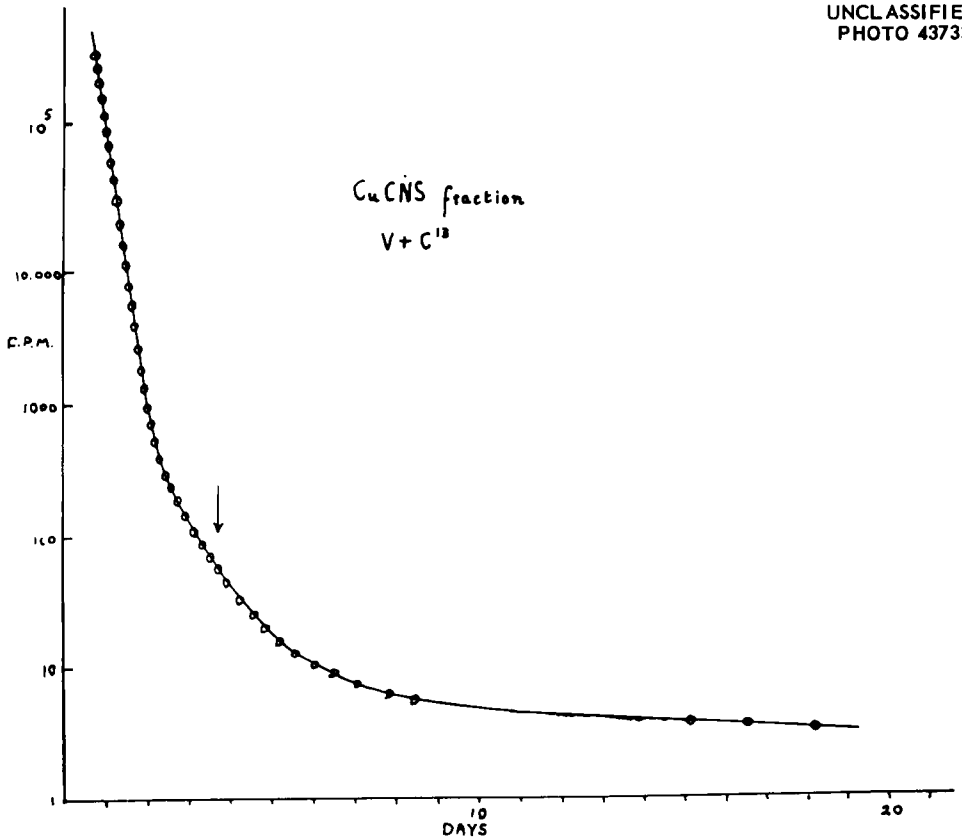

Slide 2

On the other hand, there appears to be some indication that Flerov et al (private communication) have observed Cu^{64} activity in vanadium foils bombarded with high energy (about 115 Mev) C^{13} beams. It is clear that further work would be needed, particularly with heavy ion beams of well defined energy, before the probability of occurence of such a process as the radiative capture of heavy ions could be properly assessed.

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Note: Discussion was withheld until after Dr. Reasbeck had presented Paper 16, which follows.

16. NEW LIGHT ISOTOPES OF THALLIUM PRODUCED BY BOMBARDMENT OF TUNGSTEN WITH NITROGEN IONS

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In continuation of our work on the heavy-ion bombardment of elements in the region of tantalum[1] we now report results obtained by similar techniques with tungsten as the target. The energy spectrum of the nitrogen ion beam has a peak at about 60 Mev, which is close to the Coulomb barrier height, and the relative number of ions of greater energy falls off quite rapidly. In these circumstances the most probable nuclear reaction is the evaporation of five neutrons from the compound nucleus. Evaporation of a smaller, or a larger, number of neutrons will occur with markedly less probability. The experimental results with tantalum are consistent with these predictions.

Since normal tungsten consists of the stable isotopes W^{180} (0.14%) W^{182} (26.4%) W^{183} (14.4%) W^{184} (30.6%) and W^{186} (28.4%) we should expect the lightest thallium isotope produced in good yield to be Tl^{191} from the reaction W^{182} (N, 5n) Tl^{191}. At the other end of the scale one should expect Tl^{195} from W^{186} (N, 5n) Tl^{195}; until now this has been the lightest known thallium isotope.

Bombardments - Thin metallic tungsten foils were bombarded for periods ranging from a few minutes to two hours in the way already described elsewhere[1] [2]. Immediately after bombardment radiochemical separations of thallium, mercury, and gold were made and thinly-spread samples were counted under standard end-window G-M tubes. In addition, bombardments were made of tungstic oxide cemented to the inner current probe with a small quantity of nylon; a 10-micron thick aluminum window was used on the outer probe. In some of these experiments the tungstic oxide had been enriched to 98% in the isotopes W^{182} or W^{184}. Beam currents were sufficient to give initial counting rates of a few thousand cpm from samples separated

from foil targets. Smaller beam intensities were necessary with the oxide targets and this resulted in initial counting rates of only about 100 cpm, except for the mercury fraction from W^{182} which had an initial counting rate of 600 cpm.

Chemical separations - After bombardment the targets were quickly dissolved in hot nitric acid - hydrofluoric acid mixture containing 100 μg of carriers for Tl, Hg, Au and Pt. When solution was complete more carriers were added making up the amounts of 2 mg. After a few minutes heating to ensure chemical equilibrium, solvent extraction with amyl acetate was carried out. This removed practically all the thallium, mercury and gold. The extract was separated and washed with ammonium chloride-hydrocholoric acid mixture. The aqueous extract was diluted and mercuric sulphide precipitated from it. The remaining amyl acetate was evaporated to dryness and the residue taken up with hydrochloric acid. From this solution gold was precipitated with hydrazine and sulphur dioxide, and thallous iodide with potassium iodide.

When time permitted each separated sample was purified by addition of inactive hold-back carriers and further chemical processing, but the cross-contamination in the simple separation process appears to have been quite small.

The precipitates were mounted on filter paper discs for counting; alternatively thallium fractions were sometimes re-dissolved in aqua regia and extracted into amyl acetate. From this solution samples of mercury were milked at fixed intervals by washing with slightly acid ammonium chloride solution containing about 1 mg of mercury carrier. Mercuric sulphide was obtained from each extract and mounted for counting in the usual way.

Counting. End-window halogen-quenched G-M tubes were used to count the samples. Decay of isotopes in this region of N and Z is largely by electron capture to excited states of the product nuclei followed by highly converted gamma-ray emission[3]. Thin windowed G-M tube counters are therefore far from ideal since the complexity of the decay schemes and the softness of the conversion electrons

makes it difficult to estimate counting efficiencies, but they are adequate for the study of half lives and genetic relationships. The short-lived thallium isotopes described in this paper, as well as some of the mercury and gold daughter products, could be detected, however, by using aluminum absorbers sufficient to absorb electrons of 500-kev energy, so that it is likely that these isotopes also decay by positron emission. Annihilation radiation was looked for, with a sodium iodide crystal and single channel analyser, from bombarded tungsten, but it could not be positively identified in the complex spectrum of nuclear gamma rays.

None of the separated samples emitted alpha particles detected by a zinc sulphide scintillation counter to the extent of one alpha particle per 1,000 detected conversion electrons.

The results may be most conveniently summarized by stating the periods found by resolution of the decay curves of the gold, mercury and thallium samples in turn (see Table I).

Table 1: Light Tl, Hg, Au, and Pt isotopes. Taken from the G.E.C. Chart of the Nuclides 1956 edition with the addition of Tl^{191}, Tl^{193}, modification of half life of Au^{188} (1) and Tl^{196} (4).

Tl			191 10m		193 30m		195 1.2h	196 2.4h
Hg		189 23m	190 90m?	191 57m	192 5.7h	193 12h/4h	194 .4 sec/ 180d	195 9.5h
Au	197 15m	188 4.3m	189 42m	?	191 3.0h	192 4.8h	193 17h	194 39h
Pt		187 2.5h	188 10.5d	189 10.5h	190 Stable	191 3.0d	192 Stable	193 4.5d

Gold samples - Found: 3-6 days (weak); 18h; 3-4.5h. The long weak component, about 1 cpm above bacground, we ascribe to platinum daughters Pt^{191} and Pt^{193}. The 17-h component is ascribed to Au^{193} and the shorter component to mixtures of Au^{191} (3.0-h) and Au^{192}(4.8-h) depending on the length of bombardment time and the interval before separation from parent mercury. No shorter periods were found.

Mercury samples: Found: about 3 days; 15-h; 4.5-h; 1-h. As in the gold, the long component is thought to be a mixture of platinum daughters. We believe that the 15-h component represents the daughter 17-h Au^{193} decay mixed with shorter components impossible to resolve from it. Such might be: (a) the 12-h Hg^{193m}, detected perhaps also by decay of its 4-h Hg^{193} daughter, (b) the complex decay of Hg^{192} (5.7-h) decaying to Au^{192} (4.8-h) which, (assuming equal detection efficiency of these isotopes) would appear initially to decay with a period of about 15-h; decreasing eventually to 5.7-h, (c) the 9.5-h Hg^{195}. Clearly great care would be necessary to permit firm conclusions to be drawn from such an extremely complicated mixture. However, the 4.5-h component seems to confirm the existence of the 193 chain (Hg^{193}, 4-h). Also we can only ascribe the 1-h component to Hg^{191} (57-m).

The mercury fractions obtained in the milking experiments were very similar to the above except that the proportion of 1-h component decreased in later samples, see Slide 1. From experiments with milking intervals of 10 min, 15 min, and 30 min, we concluded that the 1-h component must have a Tl parent of half life 10 min. The experiment with a 30-min milking interval, Slide 2, showed that the intensities of the 4.5-h and 15-h components were reduced by a factor of about two in the successive mercury fractions. We, therefore, infer the existence of Tl^{193} with a period of 30 min. We can make no deductions from these experiments about the possible existence of Tl^{192}.

Mercury fractions obtained from bombarded W^{184} gave somewhat more of the 1-h component relative to the others, and those from W^{182} more still. Also instead of a 15-h component this now appeared to be 13-h which can only be due to an increasing proportion of the 192 chain (not of the 195 chain which should not be present). Indeed we could not expect in the W^{182} bombardments the formation of mercury isotopes as heavy as Hg^{193} since evaporation of only three neutrons is highly unlikely on energetic grounds. If such a reaction could occur we would expect to find Tl^{196} and Tl^{197} from the natural mixture of tungsten isotopes, and these we did not detect.

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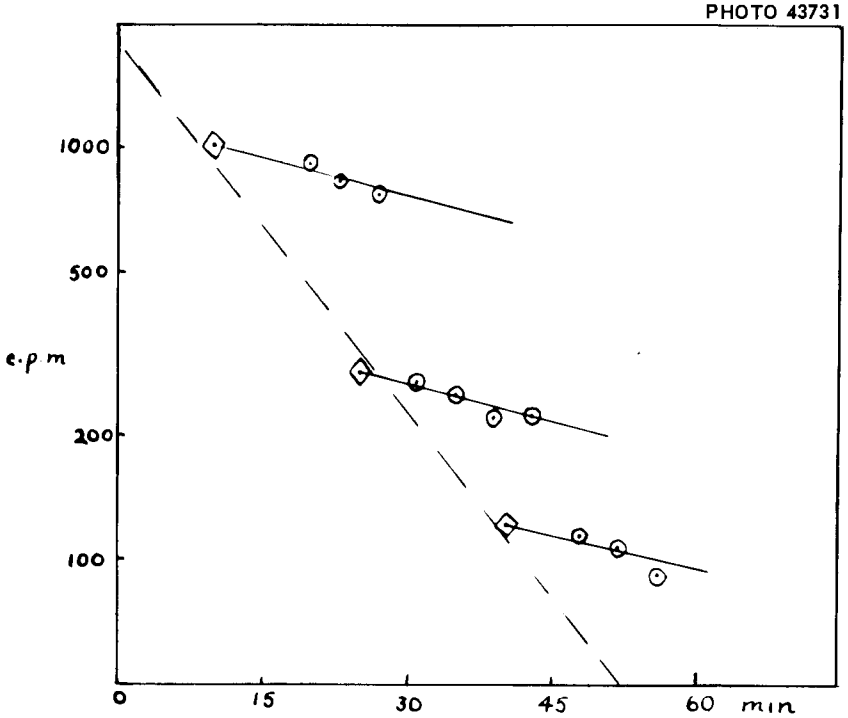

Slide 1

Decay curves of Hg fractions milked from Tl at 15 min intervals. The points shown are the first few counts after subtraction of the 4 hr and 17 hr components whose intensities were fixed from later counts. The resolved points are fitted to lines of 57 min half-life stemming from an approximately 10 min parent.

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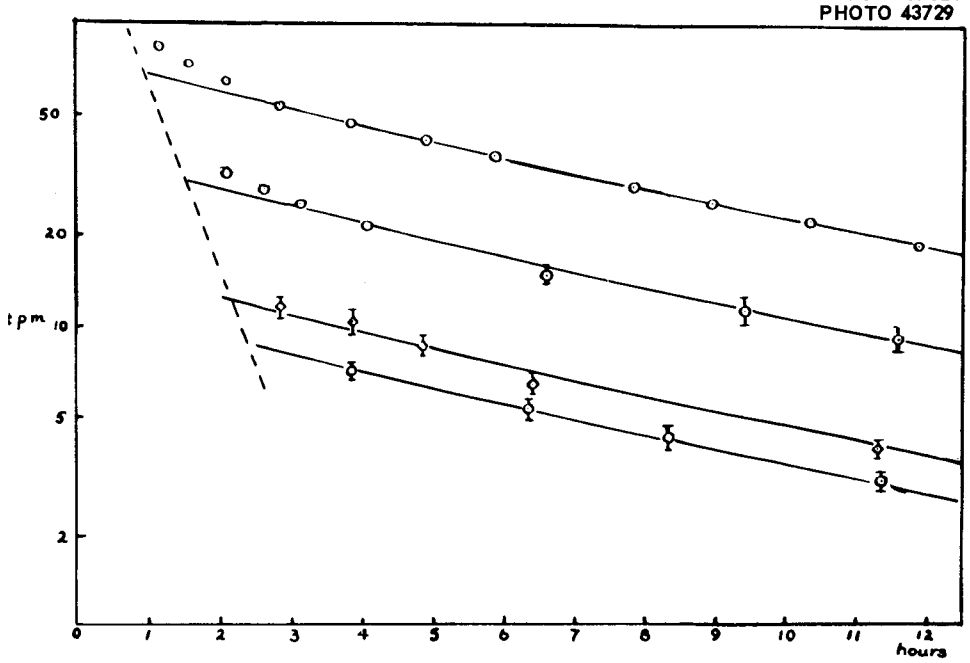

Slide 2

Decay curves of Hg fractions milked from Tl at 30 min intervals after waiting 30 min to allow most of the short-lived activity to decay. The decay lines shown are a composite of 4 hr and 17 hr half-life stemming from an approximately 30 min parent.

Thallium samples. Thallium samples obtained after bombardment of the natural tungsten isotopes showed a very complex decay, with components of about 1-h and about 10-h predominating. Smaller amounts of activities of intermediate half life were present and a shorter component apparently of rather less than half an hour period, but itself clearly a mixture.

In an effort to reduce the difficulty of interpretation, the sources were counted under various thicknesses of aluminum abosorber, Slide 3. With both 88 and 210 mg/cm^2 absorbers the decay curves could now be resolved with much more certainty and periods 10-h 4.5-h, 30-min and 10-min were found. While the former pair of activities may be ascribed to daughters and are themselves complex, the 30-min and 10-min periods could only be assigned to new activities in thallium. These figures cannot be far from the truth even if the assumed values of the periods of the longer components are incorrect. This is because of the much greater initial counting rates (by a factor of 200:1) between the 30-min and the 4.5-h components.

From the decay curves of thallium samples so far described it was not possible to decide whether the 1-h component found in the curves obtained without absorbers was due to the 1.2-h Tl^{195} formed e.g. in the reaction W^{186} (N, 5n) Tl^{195} or whether a short-lived Tl^{191} isotope was decaying into 57-min Hg^{191}, as inferred from the milking experiment. Probably both chains were present. A bombardment of W^{182} was made to confirm the presence of Tl^{191}; the thallium fraction did indeed show a strong 10-min period followed by a 1-h component, together with a weak component of about 12-h half life. The latter is probably due to members of the 192 chain, implying the existence of Tl^{192}, but the counting rates were far too low for a firm statement to be made.

The bombardment of W^{184} gave a thallium fraction which decayed with 30-min, 4.0-h, and 17-h periods, confirming that 30-min Tl^{193} is the parent of 4-h Hg^{193}. It is significant that the resolution of this decay curve was much easier than was the case for the mercury fraction from the same bombardment. This is because the thallium fraction would contain only the 193 mass chain if we may assume a short half life for Tl^{192}, whereas the mercury fraction would contain both 192 and 193 mass chains.

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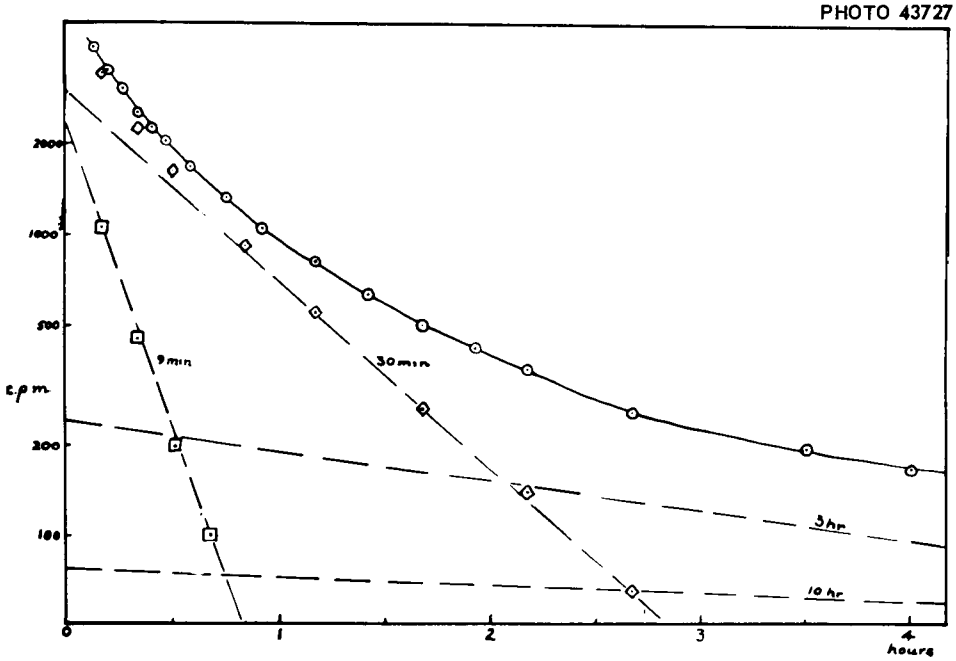

Slide 3

Decay of Tl under 210 mg/cm^2 absorver. The 3-hr and 10-hr components are fitted from points obtained later. The resolution shows 30-min and 9-min components.

The results show the existence of at least two new thallium isotopes; Tl^{191} (10 ± 1 min) and Tl^{193} (30 ± 3 min). Decay is largely by electron capture but positron emission may also occur. The formation process is by reactions such as W^{182} (N^{13}5n) Tl^{191} and W^{184} (N^{14}5n) Tl^{193}. It is possible that Tl^{192} and Tl^{194} are also formed but we have no direct evidence about these isotopes.

We should like to mention that members of the 194 mass chain have also been sought but without success by bombardment of rhenium with carbon and nitrogen ions. The latter have been shown to yield the recently discovered Pb^{195} (17-min) and Pb^{196} (37-min)[4]. It seems probable that the use of heavy ion beams of high intensity might lead to several more new isotopes in this region.

CHAIRMAN SEABORG: Any discussion?

A. BROMLEY: There is a point regarding the radiative capture experiments, and that is with the heavy bombardments. I would expect that, both because of the large energy involved and available and because of the statistics presented here, in order to form predominantly very high spin states in the compound nucleus, so that you would be faced with not a single excitation between the gamma and the particle emission widths but several, you would find a very small transition width for several, you would find a very small transition width for transitions to the ground state, or to states anywhere close to the ground state you would have multiple cascades coming down reducing the spin by 1 or 2 or something of this order all the way down. This means then that you would expect, at least apriori, a very small cross section for the radiative capture because of this multiple competition.

The second point, if I understood correctly Dr. Halbert's explanation here, this factor of 10 or 100 was obtained by taking something resembling the giant resonance gamma width for the higher levels. You can extrapolate up from data, on say C^{12} and O^{16} or something of this sort, and find that above the giant resonance region the gamma widths for transitions to lower states have dropped by two or three orders of magnitude. I am not quite clear whether you have taken this into account. If you have not taken this into account this factor of 10 or 100 would certainly seem to apply.

M. L. HALBERT: It does not matter what happens in the region of the giant resonance for this calculation, since most of the contribution comes from states right near the threshold for the next particle emission, namely, 7 Mev, or so. I don't want to insist on the particular gamm-ray cross section that I used. The giant resonance does not make any essential difference because most of the contribution comes from below it.

S. DEVONS: This effect is going to enhance fast neutron emission just because of the angular momentum. In fact, it is going to enhance the emission of fast neutrons as opposed to slow. So this may enhance the rapid de-excitation, and if you take the evaporation formed you get neutrons off predominantly at high temperature. In a level density with normal distribution, if the nucleus is formed by heavy ion bombardment, you expect this high momentum state. If only one nucleon comes off, you will have to carry off a lot of angular momentum. This will encourage fast neutron emission and rapid cooling as opposed to small.

NORBECK: About a year or two ago we looked at this business of radiative capture and maybe I should say something about this. We looked at this reaction, which is not quite in the same league as these heavier heavy ion reactions, which is Li^6 plus C^{12} to give $F^{18} + \gamma$; this has a Q value of 13 Mev. This particular reaction is one that seems very easy to study, in that there is almost a two-hour half life; there is no other product, from contamination or anything else, that gives as long half lives. We did not, however, push this very far.

We found that we did not get F^{18} within a couple of orders of magnitude. I mean, the yield of this was a couple of orders of magnitude down from anything else we could get from this reaction. If someone wants to coax me a little to convince me that this is an important thing we can go ahead and do it and run it down to 6 orders of magnitude or something.

BROMLEY: One thing that may be quite relevant is the fact that you have a relatively soft nucleus and the E-1 will also be down a factor of about 10^3.

J. R. HUIZENGA: Just to get some order of magnitude of these radiative cross sections, the University of Washington has measured several of them and, for example, the reaction Ni^{58}, γ peaks at 14 Mev, and they get a cross section of about 0.4 mb whereas for the reaction Xe^{136}, $\alpha\gamma$ they get some like 70 μb. That is the peak cross section.

A. GHIORSO: If I understand the Birmingham results, they were operating at quite high energy and undoubtedly were a good deal above the giant energy resonance.

REASBECK: Yes, this is a very wide beam energy and a thick target.

CHAIRMAN SEABORG: The point is with the University of Washington results these are at higher energy just to get the alpha particle to get the peak yield.

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17. POLONIUM ISOTOPES PRODUCED BY BOMBARDMENT OF PLATINUM WITH CARBON IONS

H. Atterling, W. Forsling, and B. Astrom
(Presented by W. Forsling)
Nobel Institute of Physics, Stockholm

Somewhat different alpha-decay data and mass assignments have been reported for neutron-deficient polonium isotopes by different authors[1-6]. The data for Po^{200} listed in Table I exemplifies this.

Table I: Alpha-particle energies and half lives of neutron-deficient polonium isotopes.

Po mass number	Karraker, Ghiorso, and Templeton, 1951		Rosenblum and Tyren, 1954		Atterling, Forsling, and Astrom, 1958	
	E (Mev)	$t_{1/2}$	E (Mev)	$t_{1/2}$	E (Mev)	$t_{1/2}$
196					6.13	1.8 min
197			6.040	4 min	6.03	4 "
198			5.935	6 "	5.93	7 "
199			5.846	11 "	5.86	12 "
200	5.84	11 min	5.770	8 "	5.77	10 "
201	5.70	18 "	5.671	17 "	5.68	18 "
202	5.59	52 "	5.575	55 "	5.58	43 "
203	-	47 "	-	-	5.48	45±10 "
204	5.37	3.8 hr	5.370	3.8 hr	5.38	3-4 hrs

To examine the usefulness of heavy ion bombardments for studying neutron-deficient nuclides in this region a series of neon and carbon ion irradiations was made in the 225-cm cyclotron of the Nobel Institute of Physics. By the use of neon ions and tungsten targets[6,7] several low-mass polonium isotopes were found, one of which was tentatively assigned to Po^{196}. This isotope was produced with Ne^{20} ions in a target which was

enriched in W^{182} and in later experiments it was shown to have a half life of 1.8 min and to emit alpha-particles of 6.13-Mev energy.

The present paper is concerned mainly with an investigation of polonium isotopes obtained by bombarding platinum with carbon ions. In addition, a study of the activities produced by carbon ions in iridium is briefly discussed, as this study is important for the question whether, for instance, possible impurities of iridium in the platinum can give rise to short-lived bismuth isotopes which may be confused with rapidly decaying polonium activities, when fast separation procedures are used.

The bombardments were made with internal beams and the ions utilized were $C^{12(4+)}$ and $C^{13(4+)}$. The gas used for the production of C^{13} ions (CO_2) was enriched to about 60% in the isotope C^{13}.

For collecting the radioactive products the recoil technique was used. A schematic drawing of the target and catcher foil arrangement is shown in Slide 1.

The platinum target was a thin circular platinum foil (about 10μ thick and 8 mm in diameter). Catcher foils of Tygon (700 - 800 $\mu g/cm^2$) were placed close behind the target foil. All foils were attached to aluminum rings which were clamped to the target holder inside the head of one of the standard probes.

A similar arrangement was used for bombarding iridium. In this case the target consisted of an iridium film on a thin nickel backing. This target was prepared by painting an organic iridium solution onto a nickel foil. The solvent was evaporated and the foil ignited in an oven. This procedure was repeated many times. A circular disk of the foil was then mounted onto a supporting aluminum ring as mentioned above.

The probe could be withdrawn through an air lock and the target holder detached within about half a minute after the end of the bombardment.

The bombardment of platinum with C^{12} ions was performed at a radius of 90 cm and the one with C^{13} ions at 85 cm. The bombarding times were about half an hour each. The nominal energy corresponding to the radii used, was approximately 130 Mev in both cases. No determination of the beam spectrum was made in connection with the present investigation, but an estimate of the energy distribution could be made on the basis of foil experiments performed under similar cyclotron conditions. According to this estimate the majority of the high-energy ions

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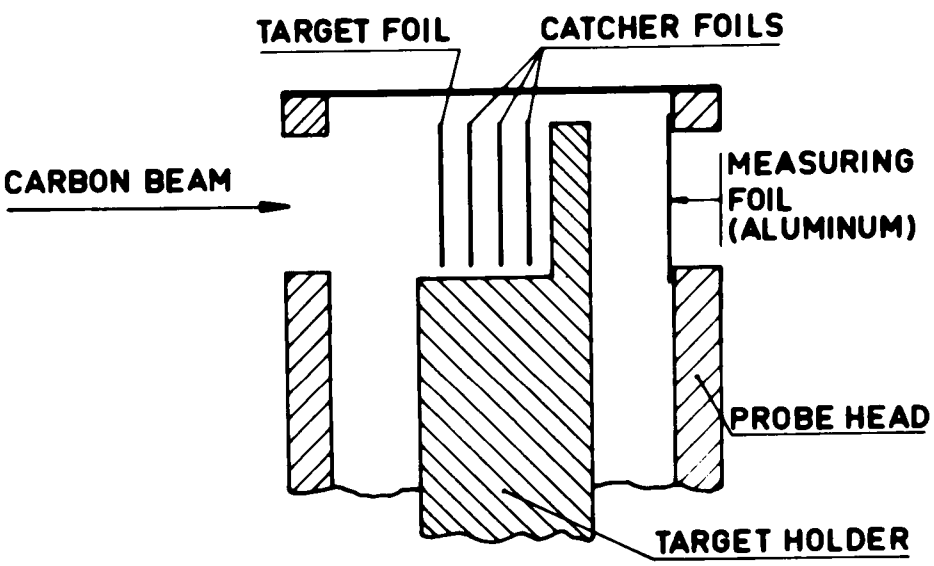

Slide 1

Schematic drawing of target and catcher foil arrangement. The probe can be rotated 180 deg for bringing the measuring window into the beam.

(C^{12} ions as well as C^{13} ions) had energies in the range 90-120 Mev, with the distribution maximum somewhere in the region 100-120 Mev. When bombarding platinum the intensities of the beams reaching the target holder with energies above 90 Mev were approximately 3×10^{-2} μa for C^{12} ions and 2×10^{-2} μa for C^{13} ions. A detailed description of heavy ion acceleration in the Stockholm cyclotron and the bombarding technique used, will be reported elsewhere[8].

In several polonium preparations different methods for the chemical separations were used[9]. As the platinum target gave no activity from At^{203} or At^{205} after carbon bombardments there were no interfering impurities of gold present. From the iridium experiments it was found that bismuth isotopes were produced with a very low yield. Experiments showed that they were not evaporated under the radio-frequency heating conditions used for the volatilization of polonium isotopes from one platinum foil to another just before the starting of ion chamber measurements. Based on these results the following rapid method for preparing the polonium sample was used.

Immediately after bombardment the catcher foil of Tygon was dissolved by a few drops of acetone on a platinum plate and the solution dried under an infra-red lamp. The plate was then placed in an evaporation device on the top of which a water-cooled platinum counting disk served as catcher for the polonium, evaporated from the first-mentioned plate. This procedure was the only one which was rapid enough for the study of the most short-lived activities mentioned in the introduction.

For the counting of the alpha-particles and the determination of their energies an ionization chamber combined with a 50-channel pulse analyzer[10] was used. Measurements could be started within about 4 minutes after the end of the bombardment.

As is well-known, most nuclides in the region studied decay predominantly by electron capture. This causes some difficulties in the alpha particle energy determinations especially in the measurements of the activities induced with carbon ions in iridium targets. The results of these measurements will be published elsewhere[7]. However, in connection with the polonium studies it was, as mentioned above, of value to know if short-lived bismuth activities, which could interfere with the polonium measurements were likely to be formed. The energies

and half lives given by Neuman and Perlman[11] for the neutron-deficient bismuth isotopes were confirmed.

The present authors hope to be able to make definite mass assignments of those isotopes, which are thought to be Po^{199} and Po^{200}, by isotope separation in the near future. So far, the short half lives and the relatively small alpha intensities have made such work unfeasible.

An alpha-particle energy spectrum obtained from a C^{13} bombardment is shown in Slide 2. The alpha-particle energies and the half lives found for polonium isotopes of mass numbers from 196 to 204 in three different studies are summarized in Table I.

The errors in the energy data given in Table I by the present authors are estimated to be ±0.03 Mev and the errors in the half lives in general to be 10%. An exception from this is the error in the half life of Po^{203} which is estimated to be 10 min because of the low intensities obtained.

In the alpha-particle energy spectrum the Po^{203} peak is indeed very small. However, it was easily reproduced and a weak indication of this peak may also be found in the energy spectra shown by the two other above-mentioned research teams[4,5]. As will be shown below, this activity could not be due to the growth of Bi^{199} from Po^{199}.

The half lives found by the present authors for the different neutron-deficient polonium isotopes generally agree very well with those found earlier. However, an exception has to be made for Po^{202}. In this case the half life found by us is about 10 min shorter than the values given earlier. The measurements were repeated with the same result. The decay curve is shown in Slide 3.

The polonium isotope of mass number 205 was obtained in a poor yield under the experimental conditions used. At about 5.32 Mev the energy spectrum given in Slide 2 seems to indicate an activity which we have not been able to identify.

The yields of the different polonium isotopes, given as calculated relative saturation alpha activities from the C^{12} and C^{13} bombardments which, owing to the choice of bombarding radii, could be expected to give approximately the same excitation energies, are given in Table II. The polonium isotopes are, of course, formed by nuclear reactions with all the different stable platinum isotopes constituting natural platinum. A rough estimate of the excitation of the compound nucleus

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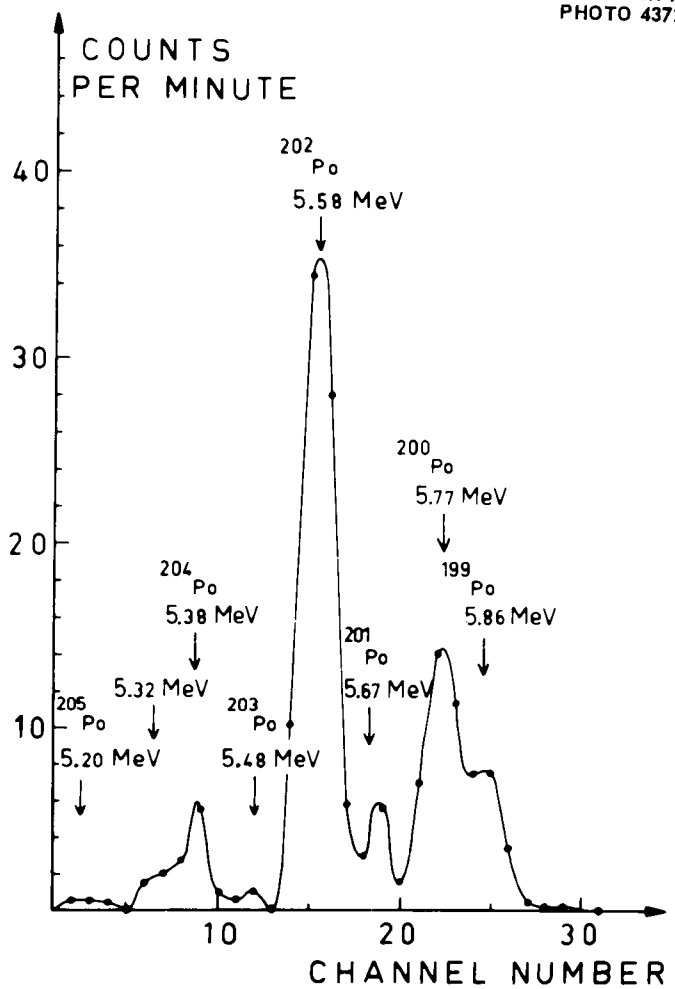

Slide 2

Alpha-particle energy spectrum of neutron-deficient polonium isotopes taken 7 min after the end of the C^{13} bombardment.

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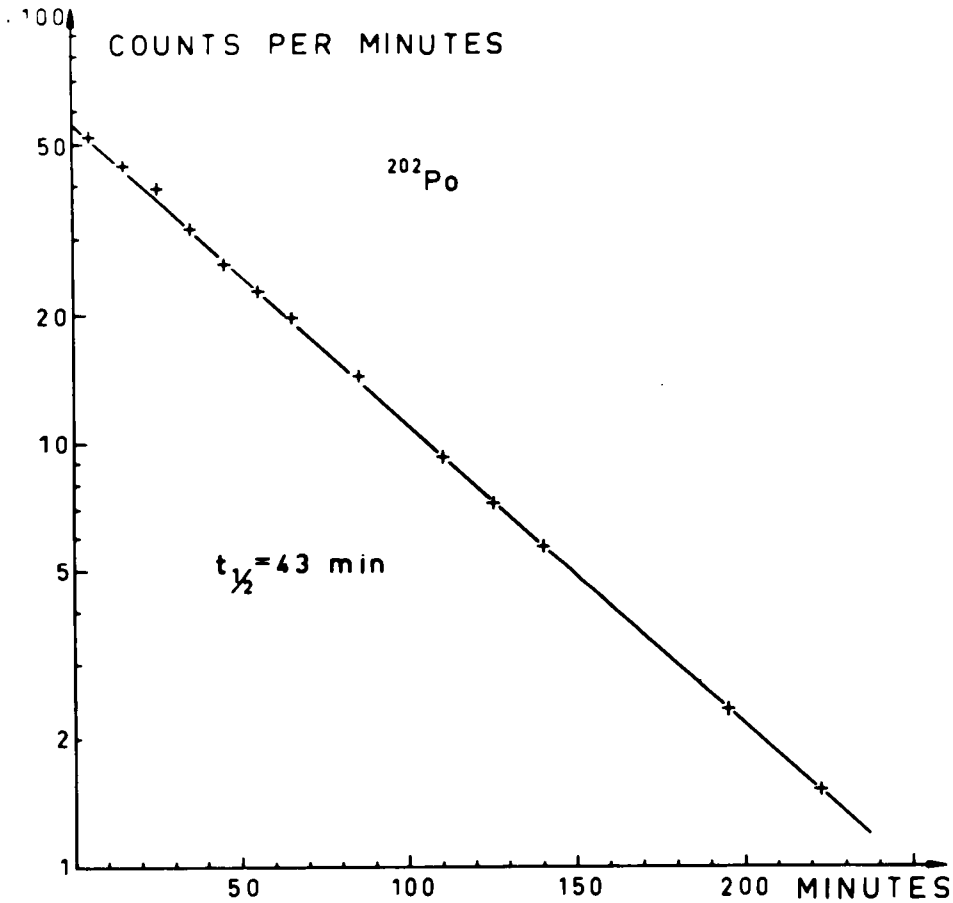

Slide 3

Decay curve of Po^{202} from alpha-particle pulse analysis.

formed by the capture of a C^{12} or C^{13} ion by any of these isotopes has been made in the following way.

Table II: Calculated alpha saturation activities of polonium isotopes obtained by C^{12} and C^{13} bombardments of platinum in percentage of Po^{202} activity.

Po mass number	Relative activities	
	C^{12} bombardment	C^{13} bombardment
197	15	0
198	10	0
199	25	15
200	100	35
201	30	10
202	100	100
203	5	4
204	25	50
205	4	3

The energy peak has been assumed to be at about 110 Mev. Due to the short range in platinum of the polonium atoms formed, only the polonium activity produced in a thin surface layer of the platinum target has been collected by the catcher[12]. Therefore, the ions causing the reactions considered in this paper have been slowed down by about 10μ of platinum. Taking this into account the excitation energies of the compound nuclei is estimated to be in the range 70-75 Mev (corresponding to 110 Mev C^{12} or C^{13} ions). In this calculation the atomic masses tabulated by Martin[13] have been used. By estimating the nuclear temperature on the basis of the formula given by Blatt and Weisskopf[14] and using neutron binding energies obtained from the tables mentioned, we find that up to 6 or 7 neutrons are likely to be emitted at the excitation energies estimated above. For carbon energies in the region 120-130 Mev (which can be attained by part of the ions) the excitation energies will be high enough to permit emission of 7 or 8 neutrons.

The isotope Bi^{198} which is expected to be formed by electron capture from Po^{198} after the C^{12}-bombardment has a half life of about 7 min and an alpha-particle energy of 5.83 Mev[11]. This activity will contribute to the alpha peak of Po^{199}. The genetic relationships involved are the following:

$$\begin{array}{ccccc} Po^{198} & \xrightarrow[t_{1/2} = 7\ \text{min}]{EC} & Bi^{198} & \xrightarrow[t_{1/2} = 7\ \text{min}]{EC} & Pb^{198} \xrightarrow[t_{1/2} = 25\ \text{min}]{EC} \\ \alpha \downarrow & & \alpha \downarrow & & \\ Pb^{194} & 5 \times 10^{-2}\% & Tl^{194} & & \end{array}$$

Even if the EC/α ratio for Po^{198} is of the same order as that of Bi^{198} (2×10^3), the growth of Bi^{198} should hardly have been observed in the decay curves of Po^{199} or Po^{200} from our experiments. Probably the ratio is much smaller.

According to well-known laws of radioactive decay the maximum activity of Bi^{198} will appear 10 min after the purification of the polonium. No growth was observed, which only means that the EC/α ratio of Po^{198} is less than about 10^4.

A similar calculation was made for Bi^{198} formed from Po^{199} according to the following scheme:

$$\begin{array}{ccccc} Po^{199} & \xrightarrow[t_{1/2} = 12\ \text{min}]{EC} & Bi^{199} & \xrightarrow[t_{1/2} = 25\ \text{min}]{EC} & Pb^{199} \xrightarrow[t_{1/2} = 80\ \text{min}]{EC} \\ \alpha \downarrow & & \alpha \downarrow & & \\ Pb^{195} & 10^{-2}\% & Tl^{195} & & \end{array}$$

As the alpha-particle energy of Bi^{199} is 5.47 Mev it should appear together with Po^{203} in the energy spectrum provided the value of EC/α ratio for Po^{199} is not too low. Indeed, the decay curve for Po^{203} gives a weak indication of such a growth of Bi^{199} alpha activity before the time expected for the maximum intensity of Bi^{199} (24 min after the purification of polonium). However, due to the low effect obtained, no quantitative estimate can be made. The half life of 45±10 minutes found shows

that the percentage of Bi^{199} alpha activity in the Po^{203} peak is very small. From EC data the half life of Po^{203} is known to be about 47 min.[4]

In Slide 4, the alpha decay energies of neutron-deficient polonium and bismuth isotopes are plotted versus mass numbers. The circles show the positions of Po^{203} and Po^{196} in the energy diagram.

The authors would like to thank Professor Manne Siegbahn for his kind interest in this work. Thanks are also due to Dr. John Millsted, AERE, Harwell for the loan of the ion chamber used in these experiments. Further thanks are extended to the cyclotron crew and other members of the technical staff for their assistance. The work was facilitated by supply of a C^{13}-compound from AERE, Harwell.

CHAIRMAN SEABORG: Are there any comments or questions?

A. ZUCKER: To what do you attribute the difference in the C^{12} and C^{13} yields of polonium? You have large differences in the yields, depending on the isotope with which you bombarded.

FORSLING: Yes. We want to remember that the platinum used here was the natural platinum and that consists of five different isotopes. However, we, of course, have used the same energy values, so that we should get the same excitation to start with. Of course, with the heavier carbon ion we should have one neutron more added to the platinum isotopes.

ZUCKER: Well, you have a pretty broad energy band, though.

FORSLING: Yes, that is true. That seems to be enough to explain that, don't you think?

CHAIRMAN SEABORG: I think his point was that more with the broad energy bands the differences would wash out. Competition with fission, of course, is a factor, but I don't think a very important factor at these excitation energies with polonium. So it is a little puzzling.

W. E. BURCHAM: Some similar experiments have been done by Harris, of Birmingham, and it may be some comfort to Dr. Forsling to know that he also finds a shorter period for Po^{202}, shorter than listed. He has also calculated fission competition here, using the actual figures published by the Russians for a similar case and interpolating for polonium, and it could be, I think, quite important. Certainly, it ought to be looked at carefully.

He has also formed the same compound nucleus by bombarding iridium with nitrogen. That fills in another one of the squares on your diagram, and this gives similar results. I think when looked at in detail

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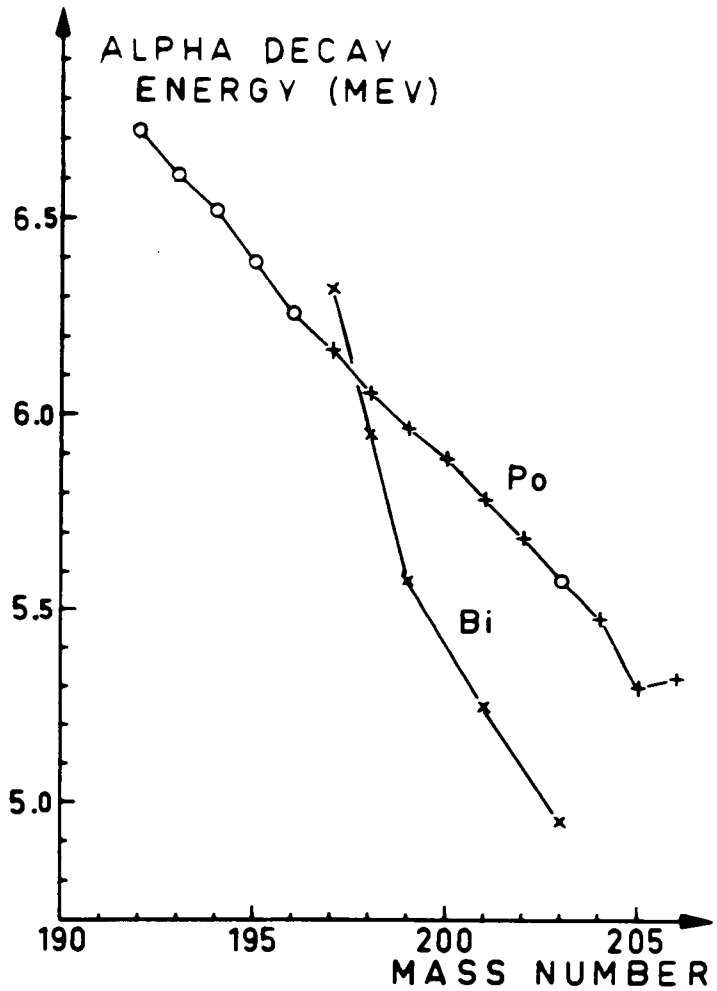

Slide 4

Alpha decay energies of neutron-deficient polonium and bismuth isotopes plotted versus mass numbers.

+ Polonium isotopes with alpha decay properties reported earlier.

o Polonium isotope energy values obtained by the present authors.

x Bismuth isotopes.

in comparison with the Stockholm results there will be some measure of agreement, but what measure of disagreement there is we can always attribute to different energy spectrum.

CHAIRMAN SEABORG: Is the competition with fission at each stage, say, more than 1:1 Γ_f / Γ_n? It could not be more than about 1:1, which would mean that with C^{13} we would just have to go through only one more step and decrease the yield by a factor of 2.

BURCHAM: For the high values of Z^2/A, the lightest nuclei, these things become comparable. Evaporating neutrons I think one is very seriously competing with fission.

CHAIRMAN SEABORG: By high values of Z^2 you mean what?

BURCHAM: Residual nucleus after the evaporation of 7 neutrons. There is a possibility.

CHAIRMAN SEABORG: Yes, I wonder if it ever got more than 1:1.

S. DEVONS: May I make one or two remarks about the general issue of these heavy ions. This question was raised just before by the speaker about the de-excitation. I am going to ignore gamma ray emission completely. If one takes a heavy ion and makes a division of this sort, and one says the average increase in angular momentum is of the order of half the radius, then the typical angular momentum would simply be Mvr/2 divided by 4. If one takes some of these experiments where one has obtained a number of peaks here, which represent one compound nucleus, the residual nuclei representing 2, 3, 4 neutrons, the typical thing, then one has to get down to these nuclei which have low spins, we know. So this momentum has to disappear.

If you ask what this is, it is the order of 10 to 20. Mental calculations give that number. How can that be done by the emission of one nucleon which will be necessary to get that? That will be simply M, which means this v is given by the Q for the reaction times R over h, and this is of the order of 8. So you would expect a gap here. This can happen because when this angular momentum has to exceed this going down in several steps, then, of course, this energy divides up now, say, into three steps. If we neglect binding, which is not quite so simple, but if we neglect binding energy there is some compensation by the fact that there are finite angular momenta in the nuclei.

So this may be a real bit of heavy ion physics, but we have to get the energy and the angular momenta in the quickest possible way, and this might peak say somewhere around this region.

ZUCKER: We will discuss it tomorrow.

CHAIRMAN SEABORG: Are you going to disagree?

ZUCKER: We observed some particle evaporation.

DEVONS: This isn't rigid. This is only a probable factor. We expect the peak to run about 3 on this sort of number.

ZUCKER: Well, I agree with that.

DEVONS: We expect one particle to be very weak and 2, 3, and 4 to be quite strong.

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18. NUCLEAR CHEMISTRY OF THE LOW-Z REGION

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Oak Ridge National Laboratory

During the past several years at the Oak Ridge National Laboratory, N^{3+} ions have been available with maximum energies of from 25 to 28 Mev. The internal beam which was used for the early work in 1953 had beam currents of $\sim$1 μa at maximum radius, with maximum energy at 26 Mev and a peak of distribution at $\sim$21 Mev. Since then a deflected beam has been available with 28 Mev ($\sim$600 kev full width at half maximum) N^{3+} ions giving beam currents up to 0.25 μa through a 1/2 in. collimator.

Cross sections of some fifty nuclear reactions have been studied during this time with target elements ranging from Z = 2 to Z = 19, Slide 1. The second column lists the $E_{cm}/E_{barrier}$. Note that this ratio is equal to 1 at about Al, and that S and K reactions, at maximum energies, represent bombardments made below the Coulomb barrier.

Nuclear reactions induced by heavy ions in light nuclei suggest two major mechanisms:

1. The formation of a compound nucleus and its subsequent de-excitation by the evaporation of light particles.
2. Transfer reactions which result in residual nuclei one mass unit lighter, or heavier, than either of the two nuclei involved.

The work in heavy-ion nuclear chemistry at ORNL has been principally concerned with these two major problems: i.e., the excitation functions for light-particle evaporation reactions and for transfer reactions.

I shall talk of the transfer reactions first. High energy nitrogen ions are particularly well suited for such studies since the transfer of a neutron from N^{14} leads to the formation of radioactive N^{13} which has a half-life of $\sim$10 min and can be readily separated chemically and identified. Residual nuclei, which are one mass unit lighter or heavier than either of the two nuclei involved can be formed by one of three mechanisms:

1. Transfer of a nucleon from one nucleus to the other in the course of a close collision.
2. Fission of a compound nucleus.

TARGET	$E_{Com}/E_{Barr.}$	REACTIONS MEASURED
Be^{9}	1.90	αn, N^{13}
B^{10}	1.42	C^{11}, N^{13}, O^{15}, $\alpha 2n+\alpha pn$, F^{18}
B^{11}	1.52	p,2p
C^{12}	1.56	2p, α, 2α, N^{13}
C^{13}	1.32	2p
N^{14}	1.33	N^{13}, O^{15}
O^{16}	1.22	2p, N^{13}, F^{18}
Na^{23}	1.15	αp, $\alpha 2p$, N^{13}
Mg^{24}	1.14	N^{13}
Mg^{25}	1.17	N^{13}
Mg^{26}	1.19	N^{13}
Al^{27}	1.04	p2n, 3p, N^{13}
S^{32}	0.99	p, 2p, 2pn, 2α, N^{13}
K^{39}	0.92	n, p, α, pn+2n, αp, $\alpha pn+\alpha 2n$, 2α

Slide 1
Nuclear reaction cross sections studied with 28-Mev nitrogen ions.

3. Emission of a nucleon from one of the colliding nuclei, which is not subsequently absorbed by the other.

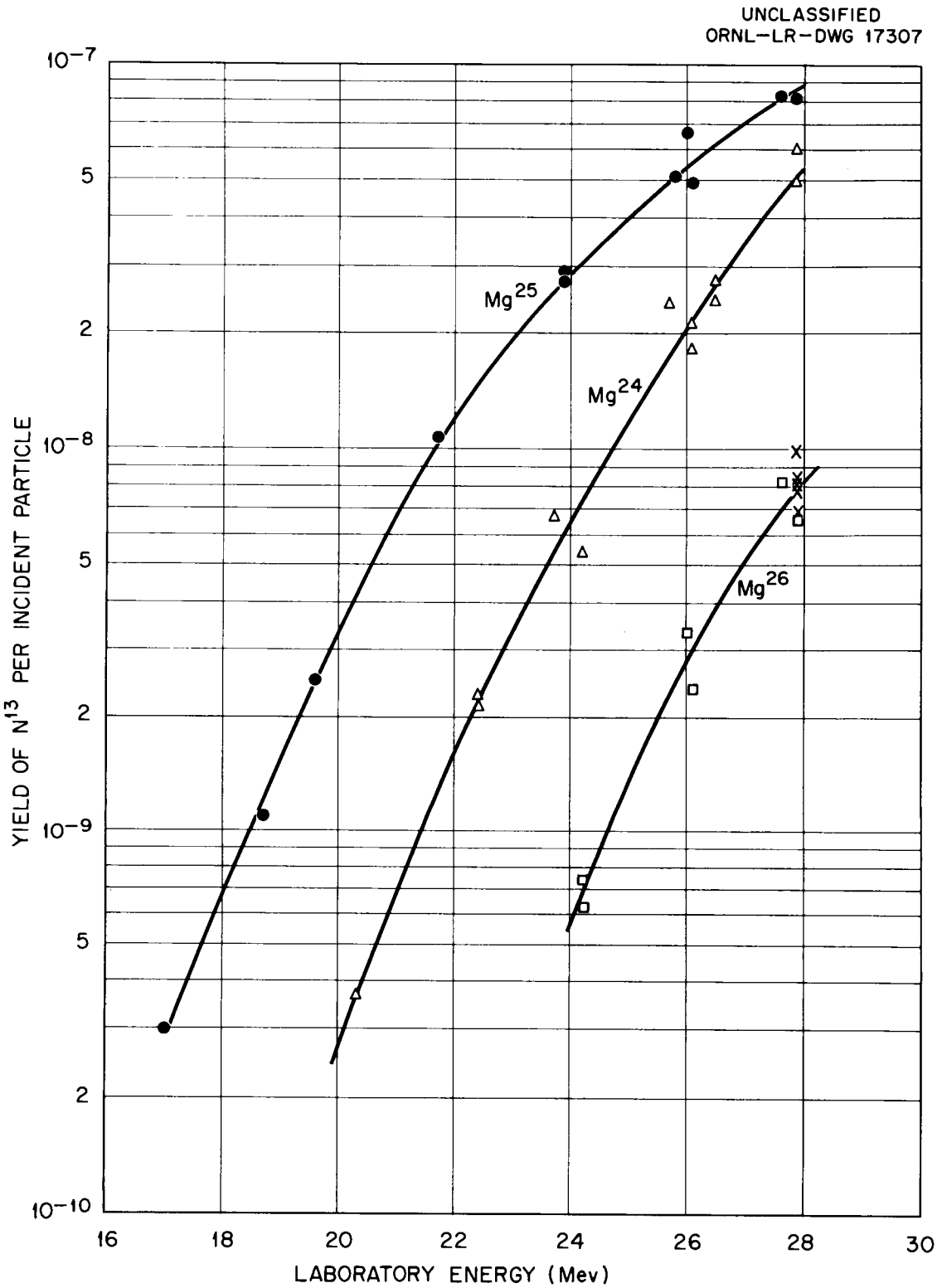
At the bombarding energies available, the evaporation of enough particles to have a residual nucleus of N^{13} is always energetically forbidden. Fission at this end of the periodic table is improbable. It has not been observed in light-particle induced reactions or photoreactions here. The third mechanism is energetically forbidden in many cases of bombardment of light nuclei by N^{14} and is probably not important with incident nitrogen energies of 28 Mev.

Eleven targets ranging from Be to S have been bombarded for $\sim$20 min and examined for neutron transfer. In each case N^{13} was separated chemically and its yield measured by absolute beta counting. Activities ranged from 3 times background to 30 times background. As few as 12,000 N^{13} atoms were involved in the beta counting. The chemical separation involved dissolution of the target in acid solution in the presence of 1 mg NH_4^+ carrier, the conversion of the NH_4 salt to NH_3, distillation, collection in 0.05 N H_2SO_4, and precipitation as ammonium tetraphenylboron. The separations took from 10-20 min and the chemical yields varied from 40-90%.

The chemistry employed was based on the assumption that the N^{13}, at the end of its passage through the target, forms a nitride with the metal. The reaction $Mg^{26}(N^{14}, N^{13})Mg^{27}$ was studied for yields of both N^{13} and 9.5-min Mg^{27}. If a transfer is occuring, yields of N^{13} and Mg^{27} should be approximately equal. You will note in Slide 2 that 5 determinations of Mg^{27} yields at $\sim$27.9 Mev fall very close to the line for the yield of N^{13}.

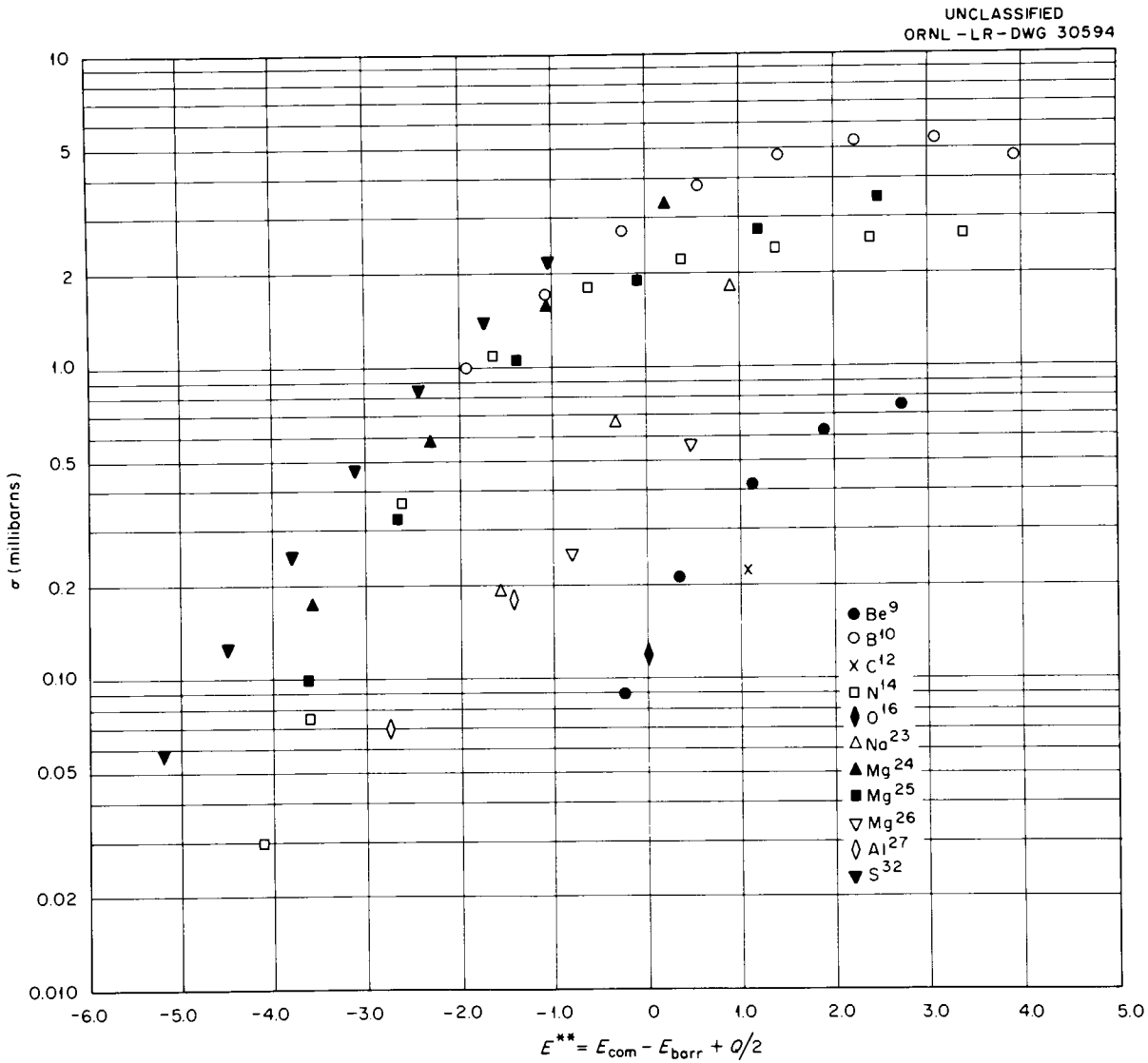
Originally[1] a plot of the results of the study was made using the log of the cross section (σ) vs $E^* = E_{cm} - E_{barr} + Q$ and represented classically the kinetic energy in the system at the moment of contact of the two nuclei just after the neutron has left the nitrogen and attached itself to the target nucleus. Prof. Breit suggested using Q/2 which would in effect shift the entire curve for each element. In Slide 3 this has been done for the previously published data, and the recently measured cross section for neutron transfer in sulfur added.

I believe the main trend and strong Q dependence can be seen. B^9 is an unusual nucleus and perhaps it is more apt to give up its loosely bound neutron than take one on. On the other hand, C^{12} and O^{16} are more



Slide 2

Yield of N^{13} per incident N^{14} ion from separated magnesium isotopes. The crosses represent the yield of Mg^{27} from the reaction $Mg^{26}(N^{14}, N^{13})Mg^{27}$.



Slide 3

Total neutron transfer cross sections for eleven light elements plotted as a function of $E^{**} = E_{com} - E_{barr} + Q/2$, where E_{com} is the maximum incident kinetic energy available in the center-of-mass system and E_{barr} is the Coulomb barrier energy.

tightly bound than usual. Mg^{26}, I can not explain. I might point out that the excitation function for an evaporation type reaction $O^{16}(N^{14}, 2p)Al^{28}$ would rise very steeply to a few hundred millibarns. Here, in neutron transfer, the cross sections gently rise and level off at $\sim$ 5 millibarn. Prof. Breit may discuss this transfer mechanism tomorrow.

Two miscellaneous experiments might be of interest. The first is a search made for the fission of U^{238} by a Coulomb excitation process induced by 28-Mev nitrogen ions. One does not expect to find this reaction from semi-classical Coulomb excitation theory. Two normal uranium foils (each $\sim$ 93 mg/cm^2) one behind the other, were exposed to the 28-Mev nitrogen beam for 2 hours. Eighty-five-minute Ba^{139} was isolated and the yield determined. The front foil should have contained fission fragments due to the nitrogen ions and also due to fast neutron fission. Fast neutrons are produced in the surface oxide layer. Since the fast neutron flux is identical for both foils, differences between the front and back foil should be due to Coulomb fission. Indeed, the front foils showed 25% to 80% larger amounts of Ba^{139} than the rear foils.

In an effort to improve the accuracy of the experiments, oxide-free uranium was used in the form of a 12% molybdenum alloy. The yield of Ba^{139} in both targets decreased by a factor of ten. With the molybdenum alloy targets, no reproducible preponderance of Ba^{139} in the front foil could be detected. It was concluded, therefore, that the 28-Mev nitrogen-induced Coulomb fission cross section was too small to be detected. An upper limit of 10^{-28} cm^2 was determined for the fission mode which produces Ba^{139}.

Possible reasons for the excess of Ba^{139} in the front foils using natural uranium are:

1. The front foils buckled due to heating in the course of the bombardment - thus effectively increasing the solid angle.
2. Neutrons from oxygen are not produced isotropically.
3. Fast protons produced in the nitrogen-oxygen reaction induce fission in the uranium.

Coulomb fission might well be found with high energy heavy ions.

The second miscellaneous experiment is the search for Ti^{42}. In the course of our work on K^{39} excitation functions we had occasion to examine 3-hr Ti^{45}. It seemed that Ti^{42} might exist with an appreciable

half-life, i.e., in the order of minutes. Ti^{42} has an even-even distribution of nucleons which include a magic number of neutrons. It was uncertain whether this 2α3n de-excitation was energetically possible, so we turned to P^{31} $(N^{14}, 3n)$ Ti^{42}. The Q-value was calculated to be equal to -15.3 Mev; the semi-empirical atomic mass tables of Cameron were used for the mass of Ti^{42}. Since we had available 19.4 Mev, it seemed worthwhile to look for Ti^{42}. I shall describe our rather cursory preliminary experiments. Pressed red phosphorus targets were bombarded for 30 min and dissolved in both the presence and absence of carriers. The solution was made $>$10 N with HCl and passed through Dowex 1-X10, 100-200 mesh, anion exchange resin. By having the effluent pass under a Geiger-Muller counter of a counting rate meter connected to a log-scale Brown Recorder, the presence and subsequent removal of the large amounts of 3.9-hr scandium by 12 N HCl washings could be followed. The titanium was removed with 9 N HCl and precipitated as the cupferrate. The column technique in conjunction with the "selective" precipitation reduced the scandium activity in the sample by a factor of $\sim$3500, as compared to a direct cupferron precipitation. This was not sufficient, however, to allow for the detection of a half-life other than four hours.

The experimental study of the compound nucleus is facilitated by the use of nitrogen-induced reactions. These reactions lead to highly excited compound nuclei with little likelihood of competition from direct interaction mechanisms. With 28-Mev nitrogen ions the average energy per nucleon in the incident nitrogen nucleus is of the order of 2 Mev, while the Q for the formation of the following compound nuclei are:

Fe^{53} in the bombardment of K^{39}, Q = + 19.8 Mev
A^{37} in the bombardment of Na^{23}, Q = + 24.4 Mev
V^{46} in the bombardment of S^{32}, Q = + 18.7 Mev.

For the potassium study, KBr crystals were pressed in 3/4 in brass molds and exposed to the deflected 28-Mev nitrogen ion beam, for periods varying from 10 min to 2 days. You will note that Fe^{55} is shown in Slide 4. This compound nucleus is produced from the 6.8% K^{41} isotope in the natural potassium target. Its contribution was, of course, taken into account where it was necessary.

After bombardment, the targets were dissolved in the presence of carriers and hold-back carriers and the reaction products were chemically separated as described in the appendix.

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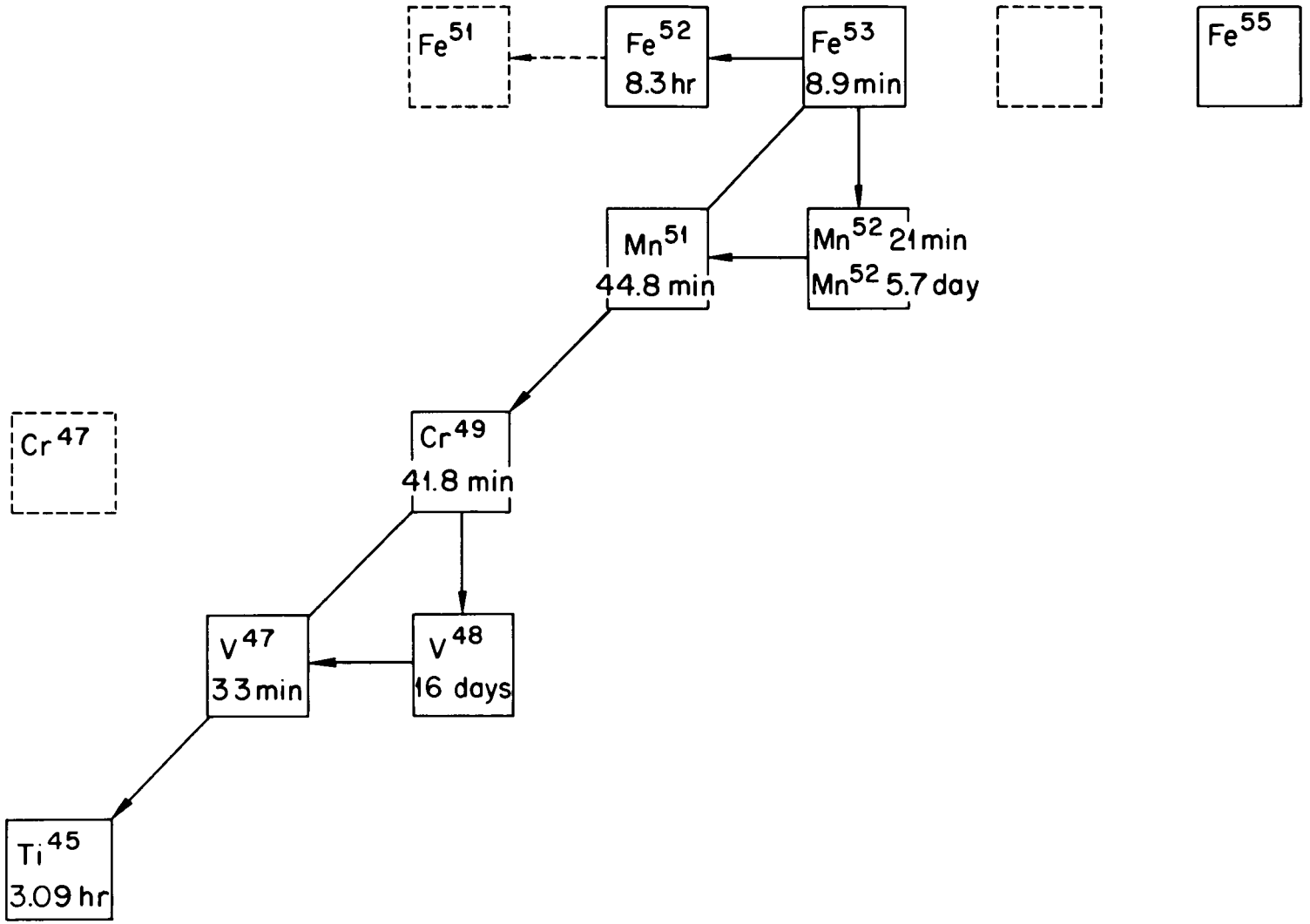
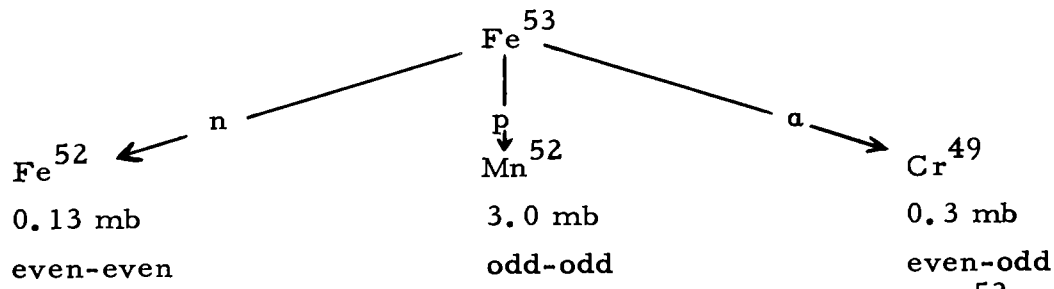


Slide 4

Schematic diagram of the isotopes observed in the N^{14} bombardment of natural KBr.

The yield curves Slides 5, 6, 7, and 8 demonstrate the scatter of the data and the energy range. These smooth curves were differentiated and cross sections were obtained as a function of energy, Slide 9. You will note that Fe^{52} represents one neutron from the Fe^{53} compound nucleus and is about 0.13 mb at 27.5 Mev. In Slide 10, note that Mn^{52} and Mn^{52m} represent the emission of one proton from the Fe^{53} compound nucleus and is about 3.0 mb at 27.5 Mev. In Slide 11, note that Cr^{49} represents the emission of one alpha particle from the Fe^{53} compound nucleus and is about 0.3 mb at 27.5 Mev. Slide 12 shows the cross section in the formation of V^{47} and V^{48}. The gross picture, thus far

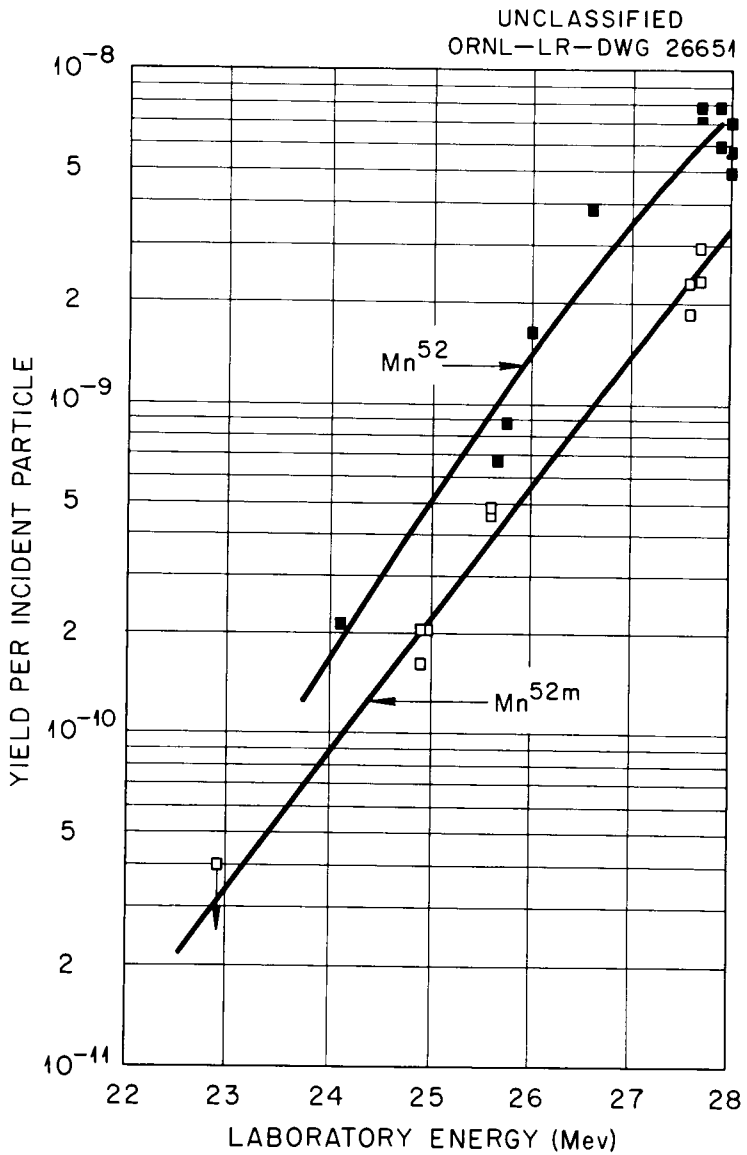


The results are interesting; they suggest that the odd-odd Mn^{52} has many more levels than the others.

Calculations are being made based on the statistical theory of the compound nucleus. The magnitude of the relative cross sections will be compared with the predictions of the theory and will serve as a check of the validity of the statistical model. If valid, the Mn^{52}/Fe^{52} ratio will furnish information on the odd-even factor in the level density.

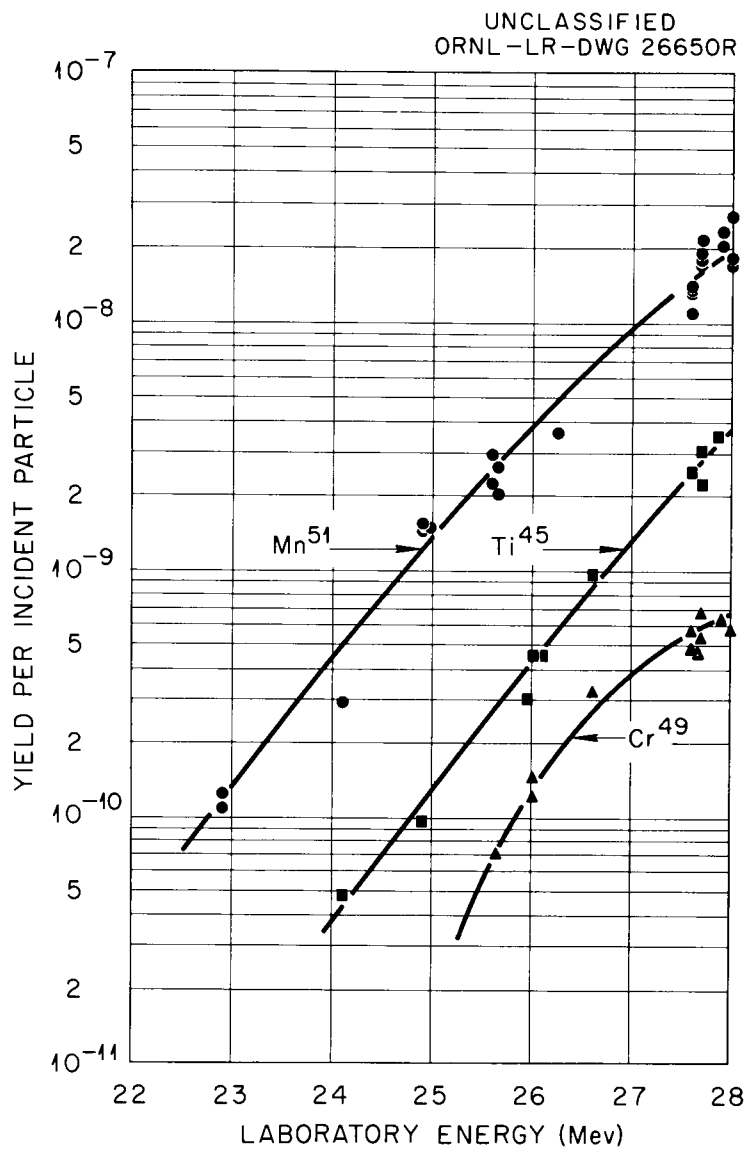
The sodium bombardment work has been published.[2] The compound nucleus is A^{37}. Two reactions were studied: $Na^{23}(N^{14}, \alpha p)P^{32}$ and $Na^{23}(N^{14}, \alpha 2p)Si^{31}$. Slides 13 and 14 show the yields and cross sections, respectively, as a function of energy, for these reactions. The reactions were picked, from the many possible ones, for convenience. In such a study one is interested in the probability for the formation of P^{32} by decay of the compound nucleus. This probability is equal to $\sigma P^{32}/\sigma_f A^{37}$ and can be calculated on the statistical model. The calculation of $\sigma_f A^{37}$ would be a very crude calculation, but by comparing two reactions, i.e.,

$$\frac{\sigma P^{32}/\sigma_f A^{37}}{\sigma Si^{31}/\sigma_f A^{37}}$$



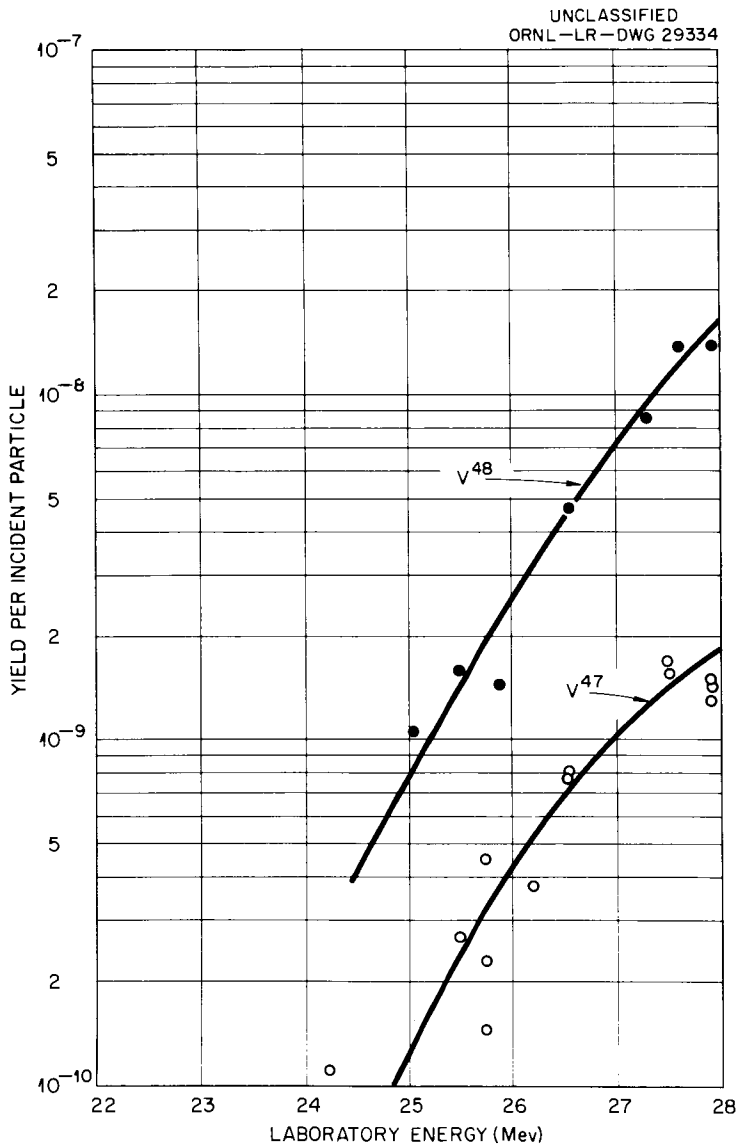
Slide 5

Thick target yields for $K^{39}(N^{14},p)Mn^{52}$ and $K^{39}(N^{14},p)Mn^{52m}$ reactions as a function of incident nitrogen energy.



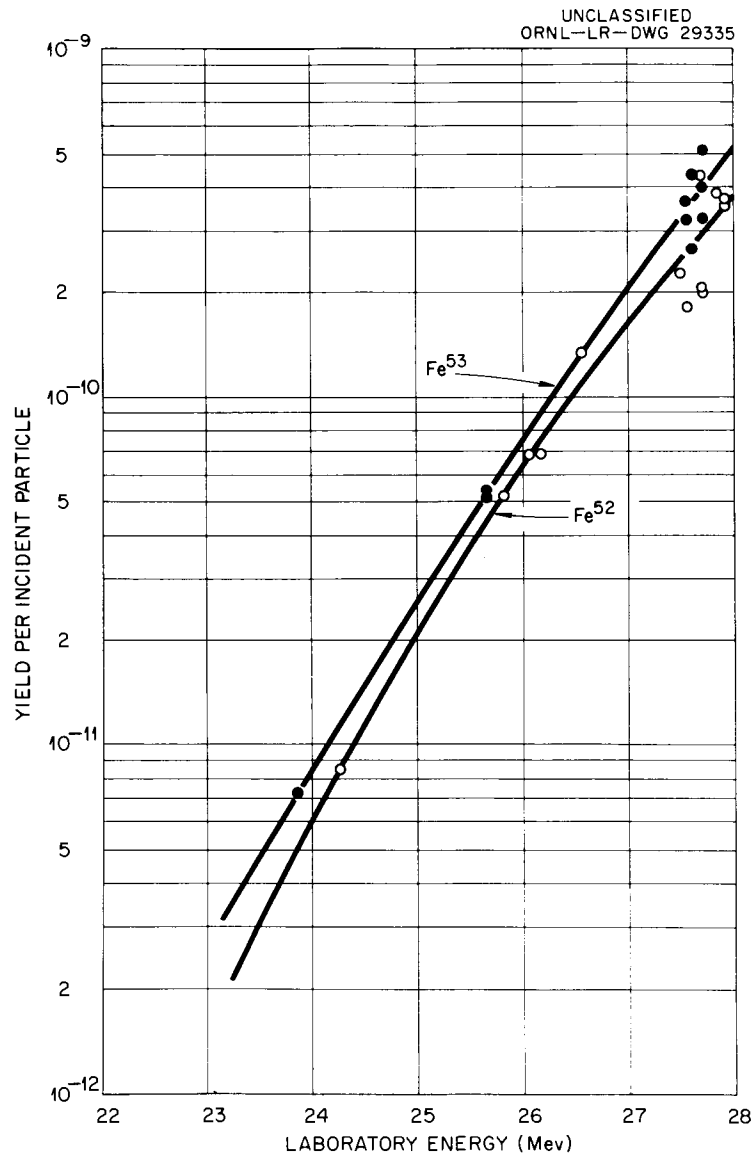
Slide 6

Thick target yields for (pn + 2n) de-excitation with N^{14} bombardment, $K^{39}(N^{14}, 2\alpha)Ti^{45}$, and $K^{39}(N^{14}, \alpha)Cr^{49}$, reactions as a function of incident nitrogen energy.



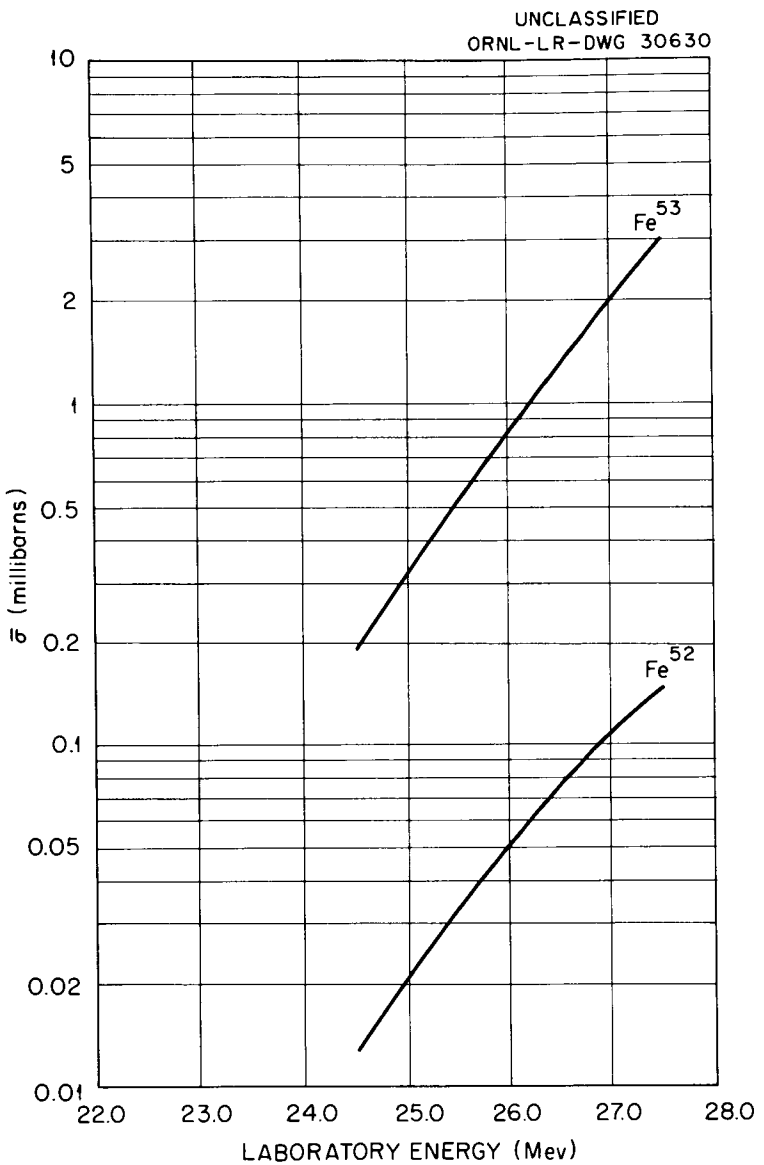
Slide 7

Thick target yields for (αpn + α2n) de-excitation with N^{14} bombardment of K^{39}, and $K^{39}(N^{14}, \alpha p)V^{48}$ reactions as a function of incident nitrogen energy.



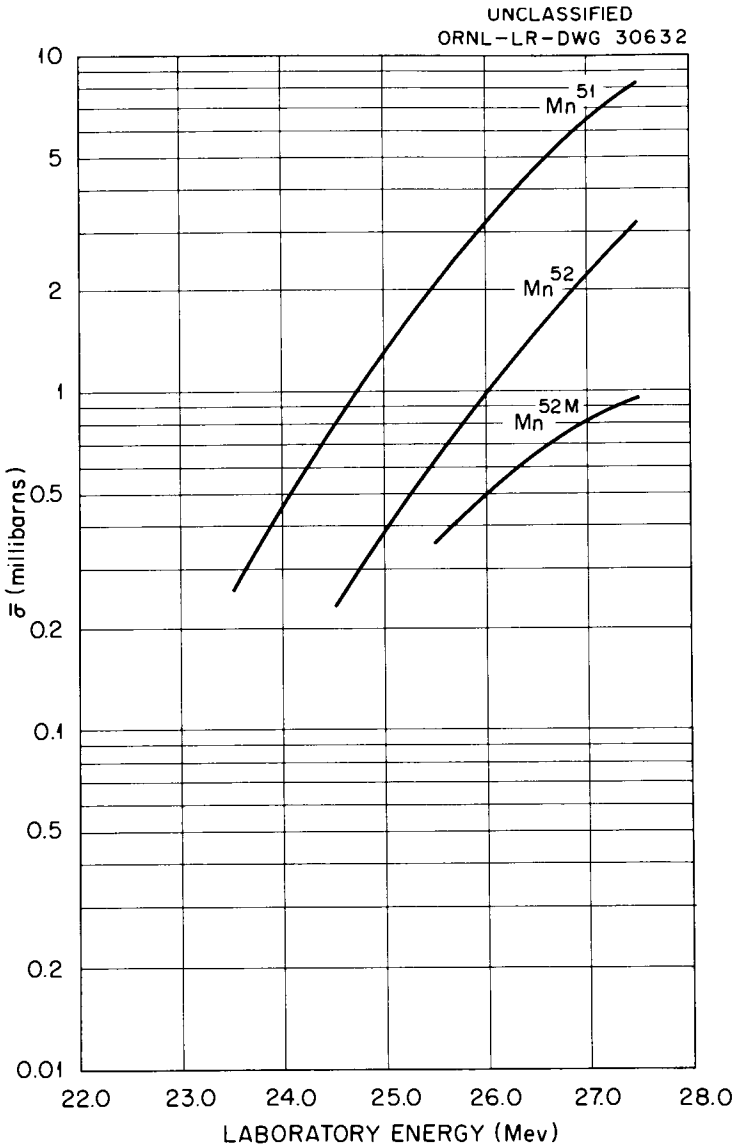
Slide 8

Thick target yields for $K^{39}(N^{14}, n)Fe^{52}$ and $K^{41}(N^{14}, 2n)Fe^{53}$ reactions as a function of incident nitrogen energy.



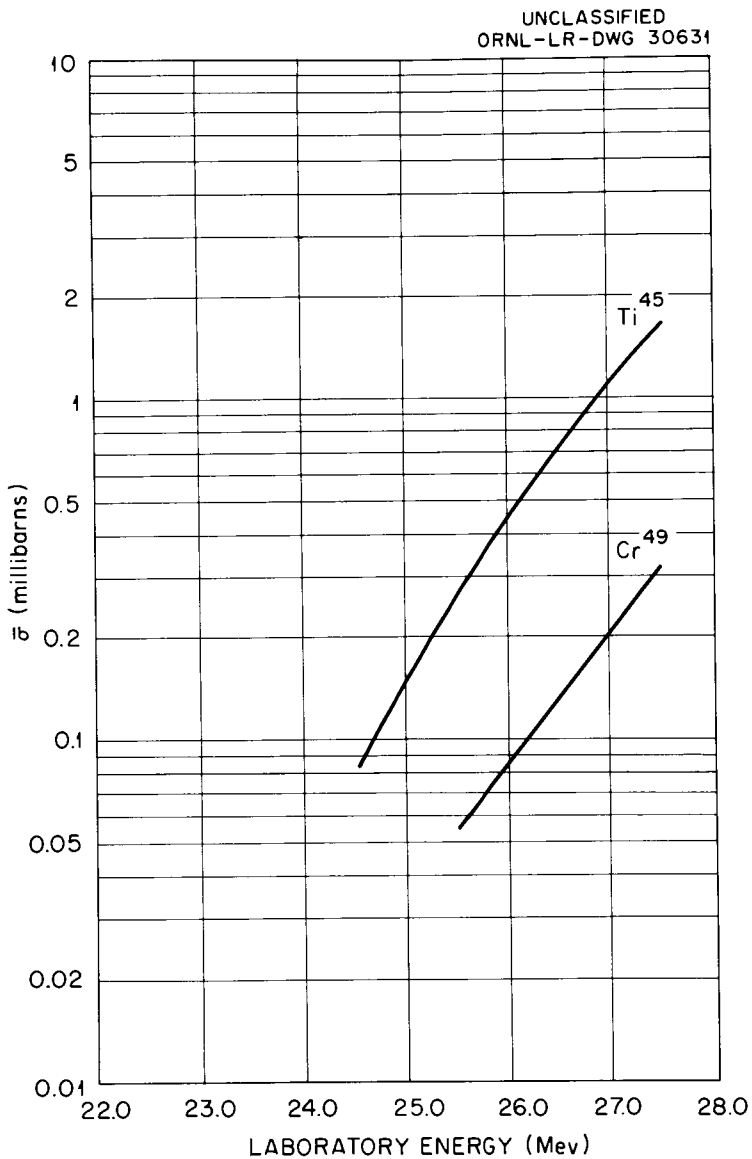
Slide 9

Cross section as a function of incident nitrogen energy for the reactions $K^{39}(N^{14},n)Fe^{52}$ and $K^{41}(N^{14},2n)Fe^{53}$.



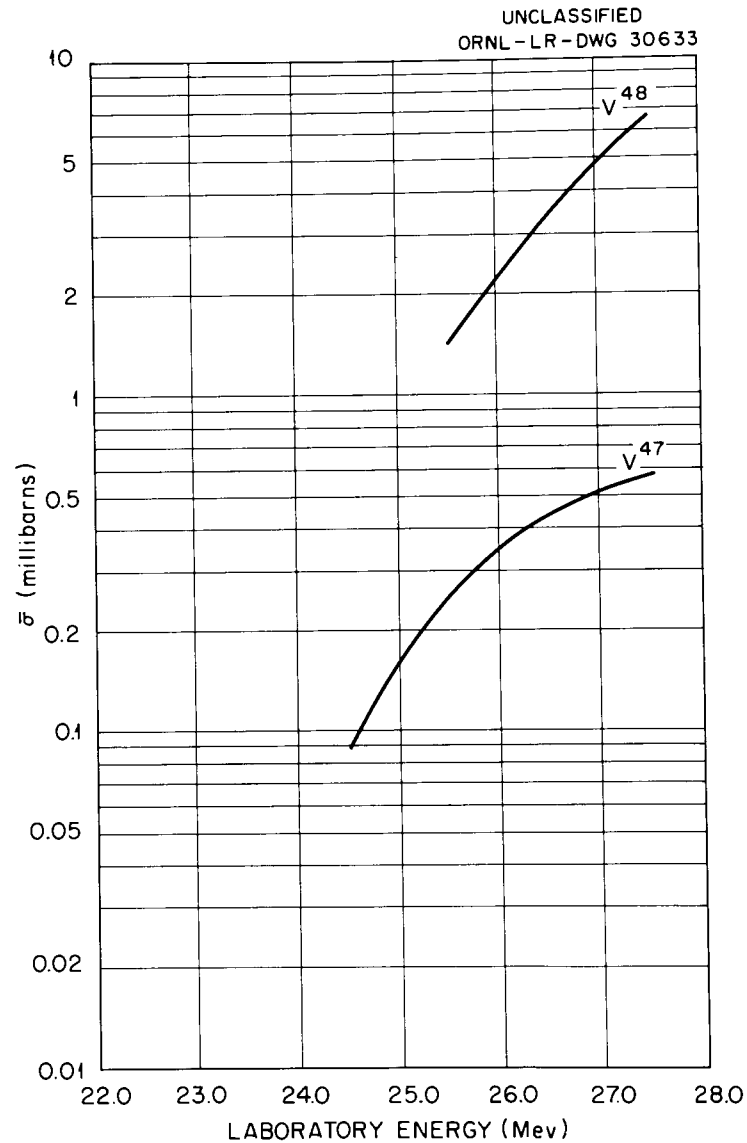
Slide 10

Cross section as a function of incident nitrogen energy for the reactions $K^{39}(N^{14},p)Mn^{52}$ and $K^{39}(N^{14},p)Mn^{52m}$ and for the (pn + 2n) de-excitation with N^{14} bombardment of K^{39}.



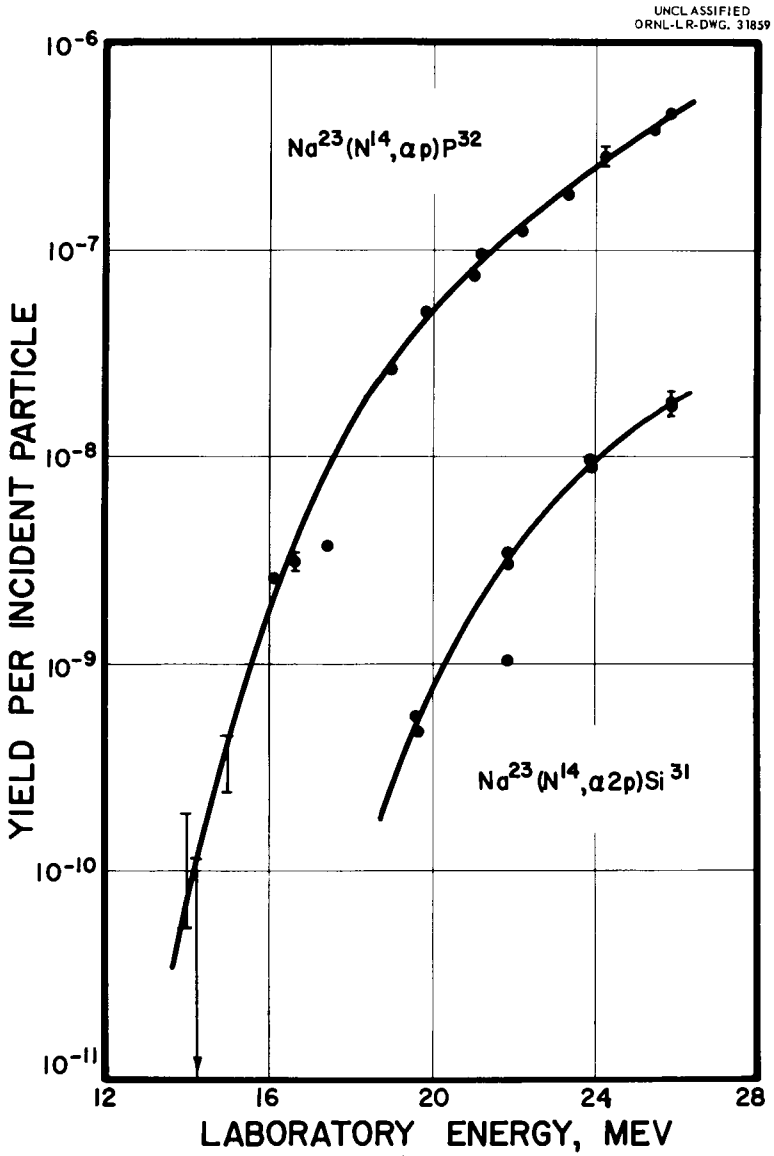
Slide 11

Cross section as a function of incident nitrogen energy for the reactions $K^{39}(N^{14}, 2\alpha)Ti^{45}$ and $K^{39}(N^{14}, \alpha)Cr^{49}$.



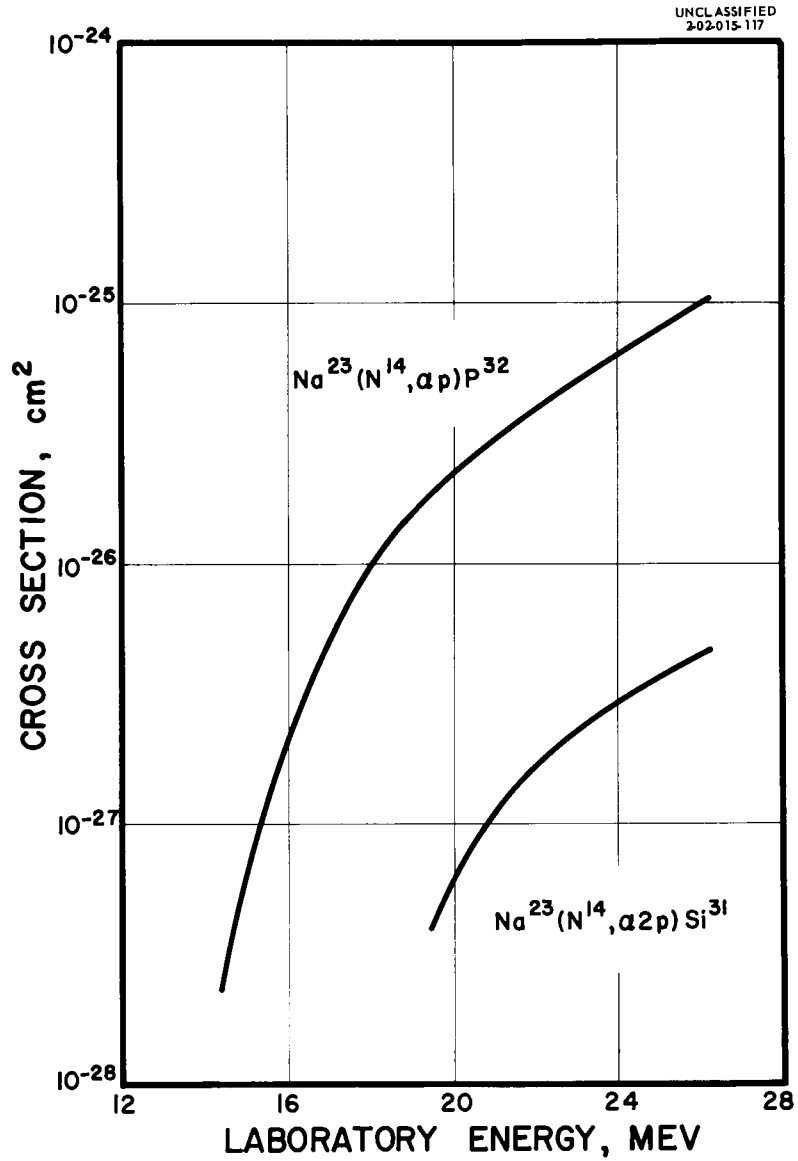
Slide 12

Cross section as a function of incident nitrogen energy for the reactions $K^{39}(N^{14}, \alpha p)V^{48}$ and for (αpn + α2n) de-excitations with N^{14} bombardment of K^{39}.



Slide 13

Thick targets yields for the two reactions $Na^{23}(N^{14}, \alpha p)P^{32}$ and $N^{23}(N^{14}, \alpha 2p)Si^{31}$ as a function of incident nitrogen energy.



Slide 14

Cross section as a function of incident nitrogen energy for the two reactions $Na^{23}(N^{14}, \alpha p)P^{32}$ and $Na^{23}(N^{14}, \alpha 2p)Si^{31}$.

we can cancel $\sigma_f A^{37}$ and have a ratio for our calculations. This, then is compared with the experimental results, i.e.,

$$\frac{\sigma_{P^{32}}(\text{exptl})}{\sigma_{Si^{31}}(\text{exptl})} \stackrel{?}{=} \frac{\sigma_{P^{32}}}{\sigma_{Si^{31}}}$$

The calculations were made for two values of a in the level density formula $W = Ce^{2\sqrt{aE}}$. The magnitude of C is irrelevant here, but the dependence of the level density on the odd-even character of the nucleus was taken into account.

Ratio	Calculated, Where		Experimental
	a = 2.6	a = 14.5	
$\sigma_{P^{32}}/\sigma_{Si^{31}}$	312.2	53.5	22 ±5

Recently sulfur was bombarded with nitrogen ions, giving rise to the compound nucleus V^{46}, Slide 15. Cross sections for Ti^{45}, Sc^{44}, and K^{38}, representing p, 2p, and 2α emissions were compared to that predicted by theory. V^{45} contributes to Ti^{45}, but one can see from Slide 16 that this effect can be ignored. Ti^{43} contributed to Sc^{43}, but the data on Sc^{43} was not considered accurate enough to warrant use. Gamma spectroscopy showed that 83% of the 3.9-hr activity was due to Sc^{44}. The calculations involved an extension of the procedure of Blatt and Weisskopf; included were odd-even effects (Brown and Muirhead), shell structure effects of the nuclear level densities (Newton), and the possibility of the compound nucleus de-exciting by gamma emission. In addition the effects of nuclear spins of the emitted particles were taken into account.

Slide 16 shows diagramatically, in part, the schematics of the evaporation calculations. Note that n, p, and α evaporations from the compound nucleus V^{46} are 21, 65, and 14%, respectively. In the case of n evaporation, 97-99% of the resultant V^{45} nuclei emit another particle. Similarly, for p emission to Ti^{45}, 99% of the resultant Ti^{45} nuclei emit another particle and of the particles emitted, 66% are protons. Gamma de-excitation from the higher levels to the shaded area amounts to 0.6%. This, in effect, adds to the number of Ti^{45} nuclei formed from the p de-excitation of V^{46}, since no further particle emission from this pocket

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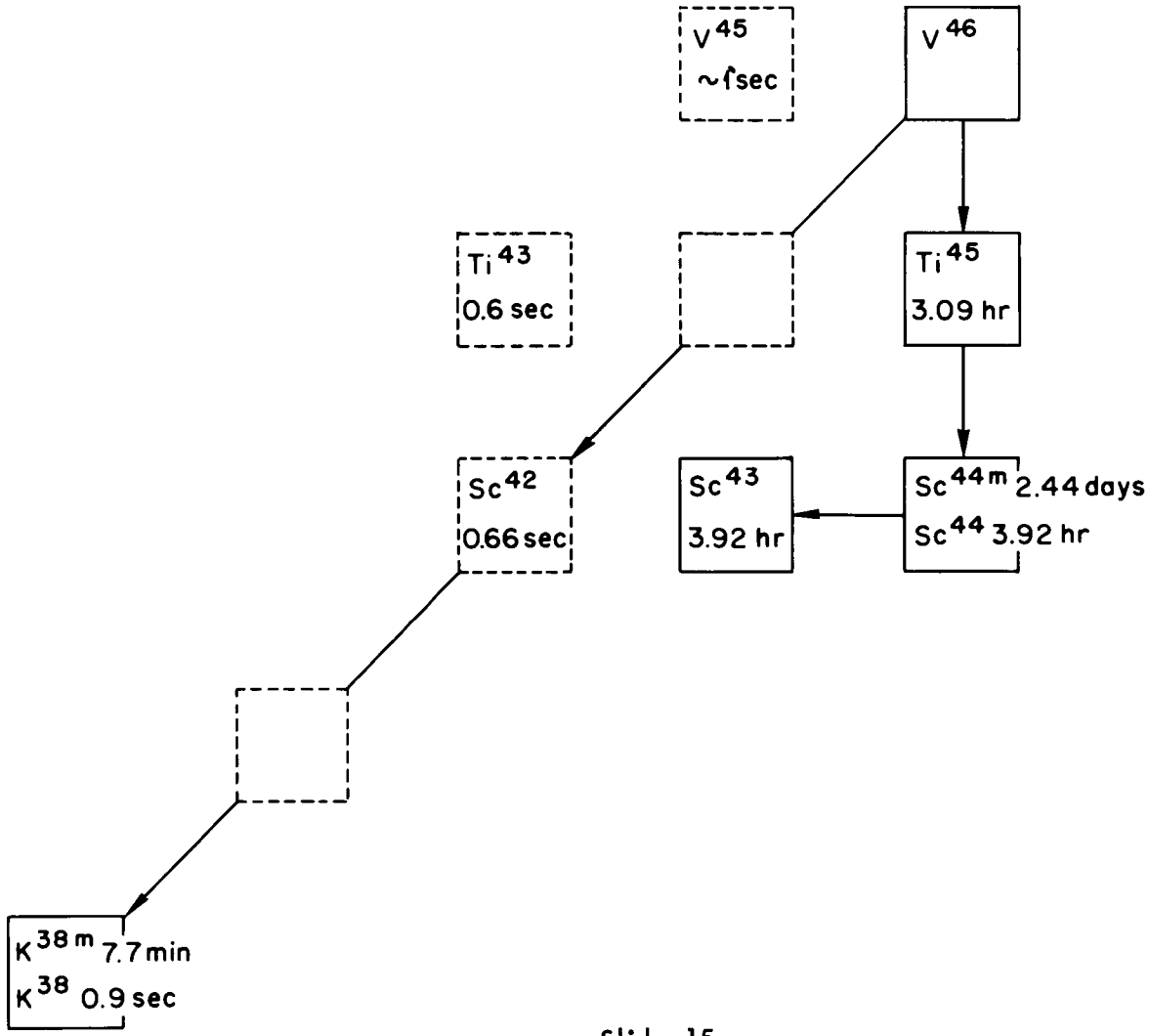
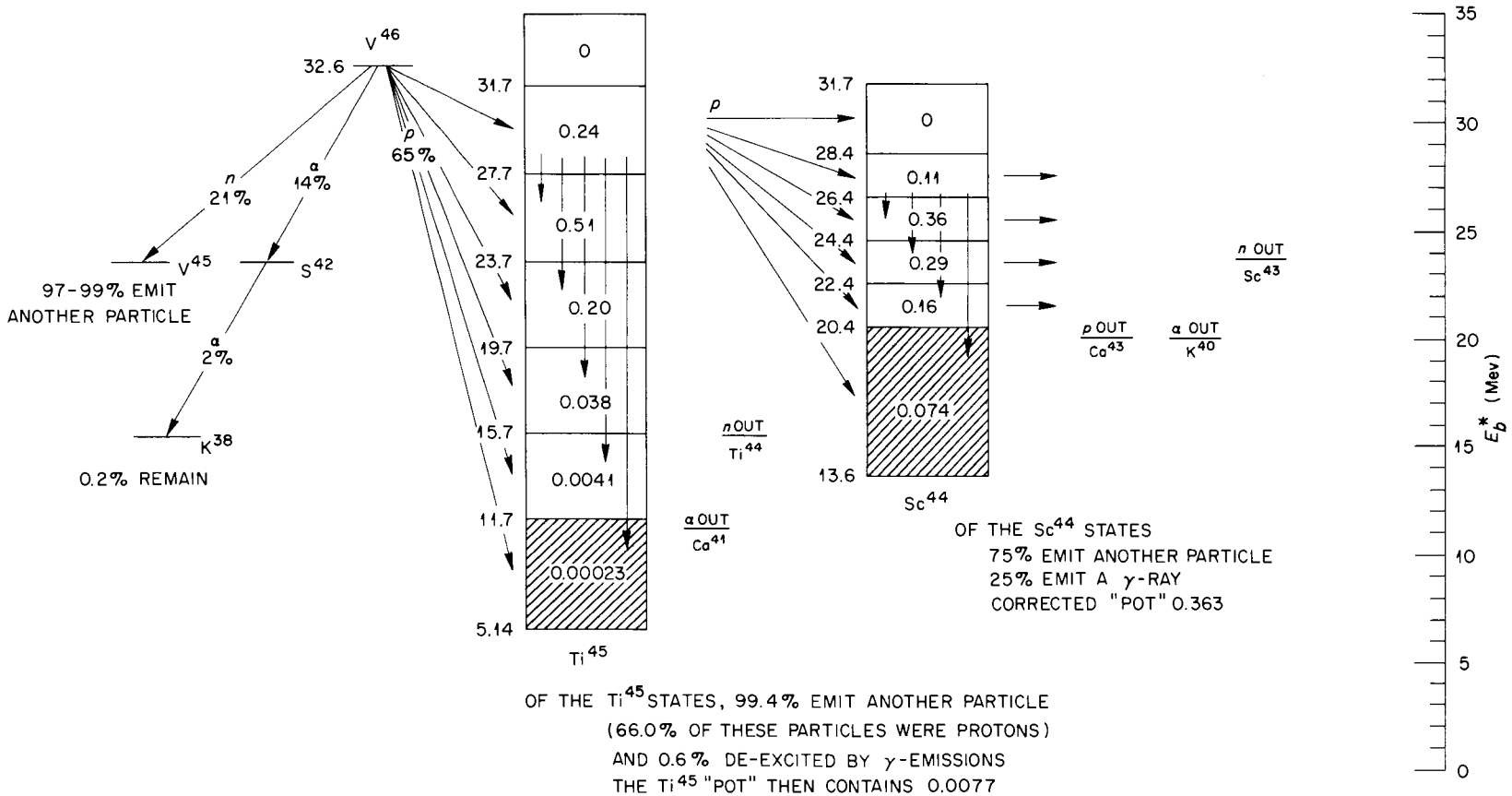


Slide 15

Schematic diagram of the isotopes observed in the N^{14} bombardment of S^{32}.



OF THE Ti^{45} STATES, 99.4% EMIT ANOTHER PARTICLE
(66.0% OF THESE PARTICLES WERE PROTONS)
AND 0.6% DE-EXCITED BY γ-EMISSIONS
THE Ti^{45} "POT" THEN CONTAINS 0.0077

OF THE Sc^{44} STATES
75% EMIT ANOTHER PARTICLE
25% EMIT A γ-RAY
CORRECTED "POT" 0.363

Slide 16

Diagram showing schematics of evaporation calculations of V^{46}.

is energetically possible. The corrected population becomes 7.7×10^{-3} (an increase by a factor of 30). In the case of a proton being emitted from one of the levels of Ti^{45} to yield Sc^{44}, 75% of the Sc^{44} de-excitations were via particle emission and 26% of the de-excitations were by gamma emission. The corrected population becomes 0.363. For α emission, 14% of the primary de-excitations led to Sc^{42}; here, 2% of the de-excitations are by α emissions and led to K^{38}, where 0.2% remain as K^{38}.

Slides 17 and 18 show the yields and cross sections for the Sc^{44}, Ti^{45}, and K^{38}.

The experimental results are compared to the predictions of the statistical model in Slide 19. The work indicates that the compound nucleus is operative and dominant. The statistical theory of the decay of the compound nucleus, well - it is, as Prof. Breit pointed out Monday, surprising that we find surprisingly little disagreement. The α process might be due to another mechanism - for example, incomplete compound nucleus formation.

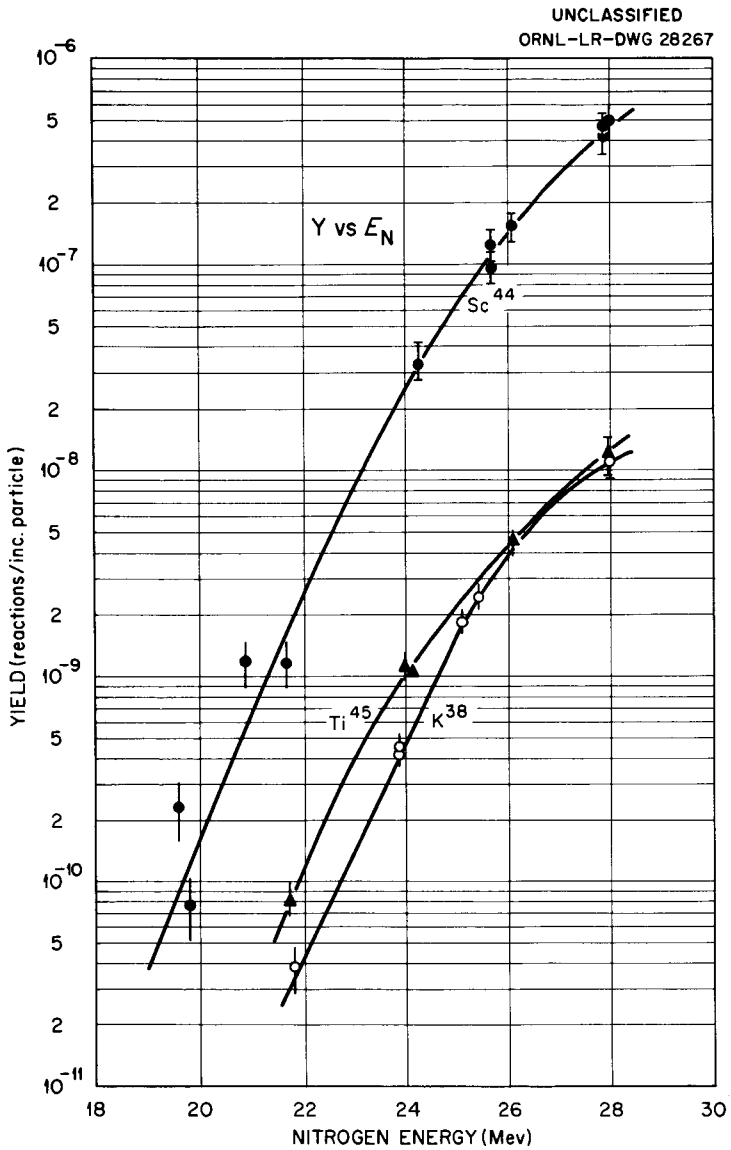
A word of the future: we are finished with nuclear chemistry of the compound nucleus, now perhaps double transfer reactions or radiative capture reaction.

APPENDIX: CHEMICAL SEPARATIONS

Potassium: The KBr targets, after bombardment were dissolved in a few ml of a HCl solution containing weighed amounts of the appropriate carriers. 10 ml fuming HNO_3 was added and one or more elements were separated by standard chemical procedures.

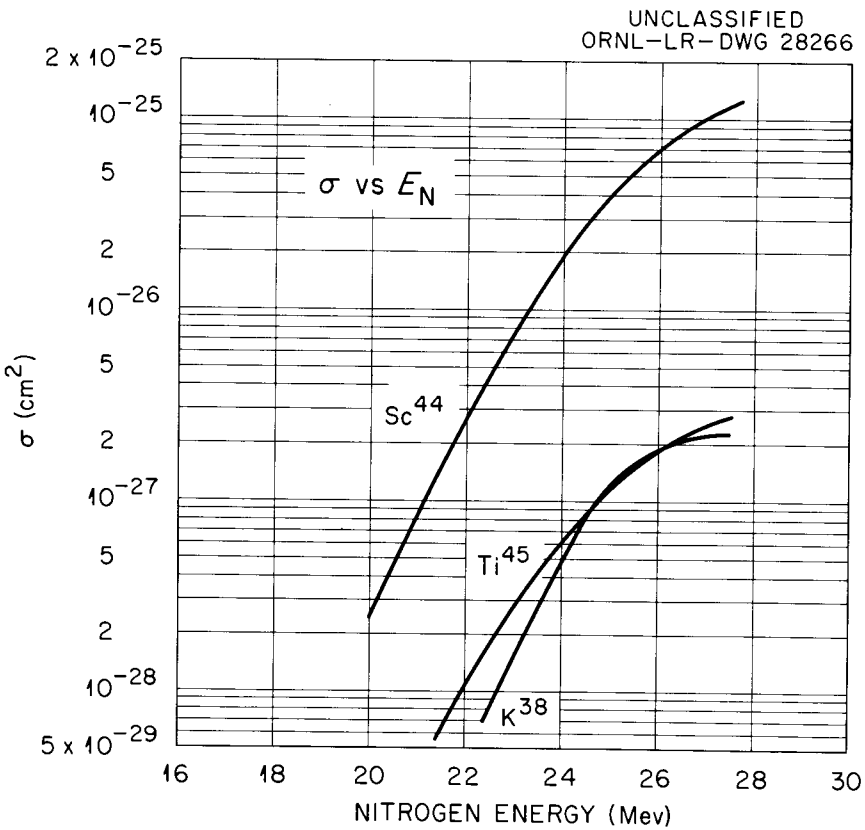
Manganese. The 21.3-min, 44.8-min, and 5.72-day Mn were precipitated out of concentrated HNO_3 by the addition of $KClO_3$, dissolved in 30% H_2O_2 and HNO_3, and reprecipitated. Vanadium carrier was added at each step.

Iron and Titanium. For the analysis of 8.3-hr Fe^{52} and 185.5-min Ti^{45}, the Mn was first removed and Fe and Ti precipitated as the hydroxide, washed, redissolved in HCl, and the Fe extracted with isopropyl ether. The iron was re-extracted into an aqueous phase and precipitated as the hydroxide. The Fe^{52} was counted as Fe_2O_3. The aqueous layer of the ether extraction contained the Ti^{45}. The Ti



Slide 17

Thick target yields for the three reactions $S^{32}(N^{14}, p)Ti^{45}$, $S^{32}(N^{14}, 2p)Sc^{44}$ and $S^{32}(N^{14}, 2a)K^{38m}$ as a function of incident nitrogen energy.



Slide 18

Cross section as a function of incident nitrogen energy for the three reactions $S^{32}(N^{14}, p)Ti^{45}$, $S^{32}(N^{14}, 2p)Sc^{44}$, and $S^{32}(N^{14}, 2a)K^{38m}$.

RATIO OF CROSS SECTIONS	EXPERIMENTAL VALUES	CALCULATED VALUES			
		GAMMA EMISSION INCLUDED		GAMMA EMISSION EXCLUDED	
		$a = A/10.5$	$a = 2$	$a = A/10.5$	$a = 2$
$\frac{\sigma_{Ti^{45},\ p}}{\sigma_{Sc^{44},\ 2p}}$	0.024	0.025	0.16	0.0047	0.032
$\frac{\sigma_{Ti^{45},\ p}}{\sigma_{K^{38m},\ 2\alpha}}$	1.1	4.7	3.0	0.50	0.47

Slide 19

Comparison of calculated and experimental cross sections for $S^{32}(N^{14}, p)Ti^{45}$, $S^{32}(N^{14}, 2p)Sc^{44}$, and $S^{32}(N^{14}, 2\alpha)K^{38m}$ reactions at 27 Mev.

was precipitated homogeneously from this aqueous solution at pH = 2.0 with $KBrO_3$, washed, redissolved in HCl, and reprecipitated. The Ti^{45} was counted as TiO_2. The chemistry had to be modified for the 9-min Fe. Ethyl acetate extraction was used, followed by an extraction into an aqueous phase where the Fe was precipitated as the cupferrate, dried, and counted. A search was made for Fe^{51}, which would decay to 44.9-min Mn^{51}, by milking Mn^{51} from irradiated samples which had been cleared of the original Mn^{51} by chemical separation. A series of such runs, involving three and four milkings per sample failed to demonstrate the presence of a beta activity other than that which could be explained by "leakage" of Mn^{51}.

Chromium and Vanadium. For the 41.83-min Cr^{49}, 31.1-min V^{47}, and 16.07-day V^{48}, the Mn was again removed, and the Cr extracted at zero degrees centigrade as the peroxychromate, taken up in very dilute KOH, and precipitated as $BaCrO_4$. The aqueous phase during the extraction contained the V. This solution was made alkaline with NaOH, scavenged with Fe and Ti, and precipitated as $Pb(VO_3)_2$. After counting, the vanadium in the sample was determined colorimetrically.

Sulfur: The ZnS targets, after bombardment, were dissolved in a few ml of N HCl and weighed amounts of the appropriate carriers were added.

Titanium and Scandium. For the separation of 3.09-hr Ti^{45}, and 2.44-day Sc^{44m}, 3.9-hr Sc^{44}, and 3.9-hr Sc^{43} the H_2S was driven off and Sc was precipitated with oxalic acid. The remaining solution was repeatedly scavenged with oxalic acid, and $Ti(OH)_4$ was homogeneously precipitated from HCl solution with $KBrO_4$. Titanium hydroxide, washed, and ignited to TiO_2. The scandium oxalate was washed, dissolved in nitric acid, reprecipitated with amonium hydroxide, washed, and ignited to the oxide for counting.

Potassium. The 7.7-min K^{38} was precipitated as potassium tetraphenylboron from a buffered HCl solution (pH≈3), washed, and dried for counting.

REFERENCES

1. M. L. Halbert, T. H. Handley, J. J. Pinajian, W. H. Webb, and A. Zucker, Phys. Rev. 106, 251 (1957).
2. M. L. Halbert, T. H. Handley, and A. Zucker, Phys. Rev. 104, 115 (1956).

CHAIRMAN SEABORG: Are there any questions or comments on Dr. Pinajian's paper?

E. NORBECK, JR: Will you say what a double transfer reaction is?

PINAJIAN: I only gave neutron transfer. Actually proton transfer was seen in our laboratory, without chemistry. Double transfer would be one particle going out and the second particle coming in, or both going one way. This has been seen, I understand, by the Russians, and we have just started on this.

J. B. CUMMING: I would like to comment on the yields of Mn^{52} and Fe^{52}, particularly I am interested in what values of a are necessary to fit these data. Friedlander and Miller have measured cross sections for producing Mn^{52} and Fe^{52} from Cr^{50} bombarded with alpha particles and find similarly high yields of Mn^{52} relative to Fe^{52}. This is also the case when the pair is produced in spallation reactions. They have interpreted these results as indicating that Fe^{52}, an even-even nuclide having isotopic number zero ($N = Z$), has a lower level density at a given excitation energy than the odd-odd nuclide Mn^{52} and that this effect is more pronounced for $N = Z$ even-even nuclei than for those for which $N \neq Z$.

Some of Miller's students have recently measured cross sections for producing Ni^{56} and Co^{56} from Fe^{54}, and for Cr^{48} and V^{48} from Ti^{46} by alpha-particle bombardment. These cases are analogous to the production of Fe^{52} and Mn^{52} from Cr^{50}, except that the targets are one alpha particle heavier or lighter, respectively. Again, the yields of the $(\alpha, 2n)$ reactions leading to the even-even, $N = Z$, nuclides are abnormally low when compared with those of the (α, np) reactions leading to odd-odd product nuclei. It appears that abnormally low level densities are a property of even-even nuclei having $N = Z$.

J. SCHIFFER: Can you explain the difference in the energy dependence for the compound state in the isomeric state in Mn^{52} that you showed? The curves seemed to have quite different energy dependences.

PINAJIAN: The errors in the determination of the Mn^{52} and $Mn^{52\,m}$ yields are large. Professor Seaborg suggested that I comment on titanium chemistry being nasty. Well, people use manganese dioxide as an ion exchange resin, if you will, and after you draw a few of these

smooth lines through the yields, even though you are honest about it, I think you will agree that there is a rather large error involved. In the case of MnO_2 you can absorb almost anything on it; so I really would not put too much dependence on the exact fit of a line.

CHAIRMAN SEABORG: I was more worred about tantalum and titanium.

PINAJIAN: You were talking about predicting new chemistries.

CHAIRMAN SEABORG: Isn't there another question in through here somewhere?

A. ZUCKER: I was just going to make the comment that in the potassium case we studied the radioactive nuclide, which is a good fraction of the total cross section, I think this is about as well as one can do, and from this kind of a chemical yield one can measure total cross sections in some cases and not be off more than maybe a factor of 2.

19. THE THEORY OF TRANSFER REACTIONS

G. Breit
Yale University

I want to report on some attempts that have been made in order to understand the reactions taking place when a neutron or a proton jumps over the gap between two nuclei. A good part of this work has been done in collaboration with Marvin Ebel, and the beginning of it has been mentioned in a paper written in collaboration with Messrs. Hull and Gluckstern.

The phenomenon of the transfer of the nucleon appears to have some attractive features, because if you have one nucleus like this and another nucleus like that coming together, one can discuss the motion on a semi-classical basis, and so it appears as though it should be possible to deal with these heavy masses as though they are approximately stationary and consider a neutron which is in the outer portion of the nucleus on the left and calculate how probable it is that the neutron will transfer itself to the nucleus on the right. In this consideration it is clear that a quantity of primary importance is the number of neutrons per cubic centimeter just outside the nucleus on the left. It would appear, therefore, that it should be possible to determine the density of the neutrons and the density of the protons just outside the nuclear surface making use of a careful interpretation of experimental data on nucleon transfer reactions.

It would appear at first glance that the way in which the neutron density decreases as you go out of the nucleus can be calculated simply by obtaining the rate of penetration through the region of negative kinetic energy, a consideration which involves only elementary quantum mechanics. Needless to say, things are not as easy as they look. There are complications which have not been anticipated in this simple picture. I will now try to give a quick review of what is involved in the theory.

Slide 1 is a consideration having to do with how we will treat the motion of the heavy particles classically. Usually the justification of a classical mechanics approach is made by employing wave packets. A parameter known to be very important in such considerations is the quantity η (as defined in the slide), where $\underline{v}$ is the relative velocity of the two heavy particles. According to the well known argument of Niels Bohr, if the quantity is much greater than 1, the motion should be essentially classical. In this argument we have an attempt to consider the matter from the point of wave packets.

The second equation in the slide is the wave equation for a constant acceleration in a uniform field. The third equation is the solution of the wave equation which provides the wave packet. In the fourth equation is a wave packet in which you have an arbitrary function, and then you can use Kennard's wave packets for the case of unaccelerated motion for φ . The sketch on the board shows the way in which the wave packet spreads with time. At time $\underline{t}$ there is a certain spread of the wave packet which is denoted by $\Delta^t x$ in the last formula in the slide. The Δx is the spread at time $t = 0$ according to this formula according to which the addition to the spread increases with the square of time, and the time dependent addition here is determined by the reciprocal of the initial spread, because the initial spread of momentum is inversely proportional to the initial spread in space according to Heisenberg's uncertainty relation. This spread applies for accelerated motion as well as for unaccelerated motion.

In Slide 2 are the quantities which enter in the special case of the collision of nitrogen with nitrogen, and this motion will be approximated by a uniformly accelerated one. The formula which we saw on the preceding slide is used in order to make estimates for these conditions near the position of closest approach. The quantity $\underline{k}$ is 2π times the wave number the latter term being used in the sense of the reciprocal of the wave length. In the case of transfer of a neutron from one nitrogen nucleus to another, you deal with the energy of separation of a neutron from the nitrogen of 10.5 Mev. The wave function for such a neutron in this condition here would vary exponentially with the distance and follows the formula and omits unessential complications.

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$$\eta = Z_1 Z_2 e^2/\hbar v \gg 1$$

$$\left[\frac{\hbar}{i} \frac{\partial}{\partial t} - \frac{\hbar^2}{2\mu} \left(\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2} \right) - \mu \hat{a} x \right] \psi = 0$$

$$\psi(x,y,z,t) = \varphi(x - \tfrac{1}{2} \hat{a} t^2, y, z, t) \exp\left\{ \frac{i}{\hbar} \mu a t (x - \tfrac{1}{6} \hat{a} t^2) \right\}$$

$$|\psi(x,y,z,t)|^2 = |\varphi(x - \tfrac{1}{2} \hat{a} t^2, y, z, t)|^2$$

$$(\Delta^t x)^2 = (\Delta x)^2 + \frac{\hbar^2 t^2}{\mu^2 (\Delta x)^2}$$

Slide 1

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$N^{14} + N^{14}$, $E = 14$ Mev, $\frac{1}{k} = 0.653 \times 10^{-13}$ cm

$N^{14} - (N^{13} + n^1) = -10.55$ Mev; $r\psi \propto e^{-\alpha r}$

$2a'$ = distance of closest approach $= 10.0 \times 10^{-13}$ cm

$T/2$ = time for covering $r_{min} + \frac{1}{2\alpha}$ to r_{min}

$$2\alpha \overline{(x^2)}^{\frac{1}{2}}_{T/2} = 5^{\frac{1}{2}} \frac{(1+\varepsilon)^{\frac{1}{2}}}{\varepsilon^{\frac{1}{4}}} \left(\frac{a'}{\alpha} \right)^{\frac{1}{4}} \left(\frac{\Lambda}{2\pi} \right)^{\frac{1}{2}} \alpha = 2.97$$

Slide 2

In this talk a' stands for one-half the distance of closest approach. As shown in the slide for an energy of 14 Mev in the laboratory system, the distance of closest approach, is 10×10^{-13} cm; T/2 is the time which it takes to go from the minimum distance between two particles to the minimum distance plus 1/2 α. That is the distance within which the neutron density increases by a factor of 2.718. One has to define the position of the particles at least that well if the wave packets are to have a meaning as representatives of postions of the centers of mass for the neutron transfer problem.

If you then minimize the spread and calculate the best T/2 in order to have the wave packet concentrated as well as possible during the time T, you find that the best you can do is to obtain a ratio of the mean square distance to distance $1/(2\alpha)$ which is shown in the last formula in the slide, and that ratio is 2.97, which is not a small number; so the consideration of the wave packet does not turn out to be helpful.

It would appear at this point that under these conditions one cannot consider the motion as being even approximately classical but that one has to really use wave mechanics.

This conclusion, however, is not correct. It is not true because the arguments given are concerned with a sufficient rather than a necessary condition. One finds in fact that for some purposes the consideration of classical orbits is rather good. That can be done by going back to Pauli's old article in the Handbuch der Physik employing the Hamilton-Jacobi partial differential equation, in order to show the connection between geometrical optics and classical dynamics and introducing the action function, expanded in the usual way, much as in the treatment by Pauli, as shown in Slide 3 which makes use of the expansion for the initial and the final states.

I don't want to talk too much about this, but along such lines one can connect the classical quantities with the matrix element in the standard calculation for the transition probability. That comes out through the well-known equations shown in the slide and one obtains the momentum in terms of the action function.

Along such lines one can justify the treatment of the motion of the centers of mass of the nuclei by classical mechanics essentially to the following degree. Considering the motion of a single particle as being represented by a hyperbola and describing the motion in the space of relative coordinates the situation may be described by the sketch on the blackboard. This shows families of hyperbolic trajectories corresponding to S_i and S_f; each family has a parabolic envelope and one of the trajectories is tangent to both envelopes. The region of space surrounding this trajectory – which is the physical trajectory – may be shown to contribute especially strongly to the quantum mechanical transition matrix element which appeared as the last formula on Slide 3. This is a matter of stationary phase. There are some compensations of effects in the integrand of the matrix element which make the classical approximation good. This viewpoint is suitable for a qualitative understanding of the situation but does not lend itself easily to a quantitative treatment. In order to know how good it is, a special case is taken in which you can calculate the results explicitly. Use is being made of work by Ter Martirosian, a Russian who was concerned with some related problems. We will make use of certain integrals that he calculated. By means of them you can solve a schematic problem with a delta function interaction at one nucleus that captures the particle from the other. One does not obtain the solution of the complete problem, but one obtains an idea of how good the approximations are.

In Slide 4 the wave function of the final state is indicated initially by subscript f. The $\psi_{\overline{ki}}(\eta_i, \vec{r})$ outgoing wave modification, and $\psi_{kf}(-)(\eta_f, \vec{r})$ is the ingoing wave modification of a plane wave. Then the matrix element is that shown in the first line of the slide. It turns out to be expressible by means of a hypergeometric series which we can approximate, and you end up with something related to what you calculate by using classical orbits and using the picture of exponential decay of nucleons escaping from the nuclear surface. This is apparent in the two expressions for $|I_o(\theta)|^2$ shown in the slide. The exponential factor is essentially the penetrability in this approximation. In the last line of this slide there is shown a correction term to the penetrability inside the square brackets.

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$$\psi_i = \exp(i S_i / \hbar), \quad \psi_f = \exp(i S_f / \hbar)$$

$$S_i = S_i^{(o)} + \frac{\hbar}{i} S_i^{(1)} + \cdots, \quad S_f = S_f^{(o)} + \frac{\hbar}{i} S_f^{(1)} + \cdots$$

$$p_s^i = \partial S_i^{(o)} / \partial q_s, \quad p_s^f = \partial S_f^{(o)} / \partial q_s$$

$$\int \psi_f^* \nabla \psi_i \, d\vec{r}$$

Slide 3

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$$I_o(\theta) = \frac{\nu}{(4\pi)^{\frac{1}{2}}} \int [\psi^{(-)*}_{\vec{k}_f}(\eta_f, \vec{r}) \psi_{\vec{k}_i}(\eta_i, \vec{r})][\exp(-\alpha_n r)] r^{-1} \, dr,$$

$$\cos\theta = (\vec{k}_f \vec{k}_i) / (k_f k_i), \qquad 0 < \theta < \pi$$

$$|I_o(\theta)| = \frac{4\pi^{3/2} (\eta_i \eta_f)^{1/2}}{\alpha_n^2 + (k_i - k_f)^2} e^{\varphi_f \eta_f - \varphi_i \eta_i} \frac{e^{-\pi\eta_f} \, {}_2F_1(i\eta_i, i\eta_f; 1, -\zeta)}{1 + \zeta}$$

$$|I_o(\theta)|^2 \sim \frac{2\pi^2 \eta}{\alpha_n k \sin(\theta/2)[\alpha_n^2 + 4k^2 \sin^2(\theta/2)]} \exp\Big\{ -4\eta [\arctan(\alpha_n/2k) + \arctan(\alpha_n / (2k \sin(\theta/2)))] \Big\}$$

$$|I_o(\theta)|^2 \sim \frac{\pi^2 \eta}{2\alpha_n k^3 \sin^3(\theta/2)} \exp\Big\{ -2a'\alpha_n [1 + (1/\sin\theta/2)] \Big\}$$

$$-2a'\alpha_n [1 - \frac{1}{3} (\frac{\alpha_n}{2k \sin\theta/2})^2 + \cdots] / \sin(\theta/2)$$

Slide 4

Calculating in this manner, you can take account of the difference between the classical approximation and the quantum result by taking account of the previously mentioned correction term. You can thus examine how good the classical approximation is.

Errors of semiclassical approximation for $N^{14} + N^{14}$ at 14 Mev

$\sigma(90°)/\sigma(180°)$	σ_{total}	$\frac{\sigma_{\text{tunneling theory}}}{\sigma_{exp}}$
-6%	25%	~11

You find that it isn't terribly bad. You have here a 6% error, for instance, in the ratio of the cross sections at 90 and 180 degrees, and you find an error of 25% in the calculation of the cross section. On the other hand, in the calculations that have been made on the actual process, a comparison of the simple tunneling theory with experiment which was sketched during the earlier part of this talk gives rise to a ratio which is off by a factor of 11 as shown in the last column. The accuracy of the classical approximation is thus ample for the present discussion.

Slide 5 is intended to give a general idea of how the tunneling consideration is conducted. Two nuclei are surrounded by surfaces S_1 and S_2. These surfaces separate the interior and exterior regions in the R matrix formulation, the difference from the familiar case being that here there are two systems, two nuclei. In the present case one deals with two systems instead of with one. Occasionally we will refer to the distances r_1 and r_2.

The essential quantities that enter for the tunneling problem are given in Slide 6. You are dealing with the part of the energy matrix of the whole problem, and one stationary state is designated by $\underline{u}$ while another state is distinguished by $\underline{v}$ which is associated with the nucleus shown on the left of the preceding slide. The quantity β that enters in the off diagonal element determines the rate of transition. The formula for β is written out just before the last forumla in the slide. It is written out in the approximation of treating the motion of

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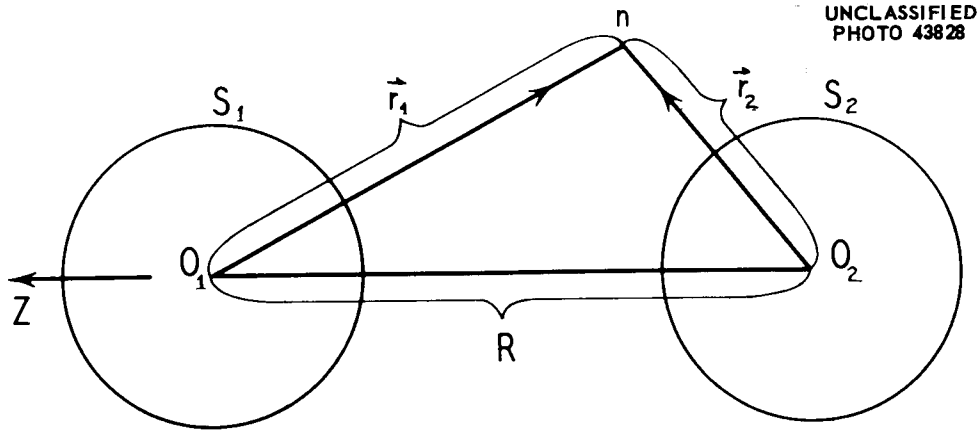

Slide 5

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$$\begin{pmatrix} \bar{E}_u & , & -\frac{\hbar^2}{2M}\beta \\ -\frac{\hbar^2}{2M}\beta & , & \bar{E}_v \end{pmatrix}$$

$$-\frac{1}{\lambda_1} = [\, b_1^2 \left\langle |\psi_1^2| \right\rangle_{S_1}] \Big/ \int \psi_1^2 \, dq_0 \, dq_1 \ldots dq_n$$

$$\beta = (1/\lambda_1\lambda_2)^{\frac{1}{2}} [\alpha b_1/(1+\alpha b_1)][\alpha b_2/(1+\alpha b_2)](1/R)\exp\left\{-\alpha(R-b_1-b_2)\right\}$$

$$\sigma_n = \frac{\pi^2}{2}\left(\frac{\hbar}{Mv}\right)^2 \left(\frac{1}{\alpha^2\lambda_1\lambda_2}\right)\left(\frac{\alpha b_1}{1+\alpha b_1}\right)^2 \left(\frac{\alpha b_2}{1+\alpha b_2}\right)^2 \exp\left\{-2\alpha(2a'-b_1-b_2)\right\}$$

Slide 6

the heavy particles by classical mechanics and treating the motion of the nucleon by wave mechanics. An important quantity that enters β is the ratio of the nucleon density at the nuclear surface, S_1, on the preceding slide to the integral through the whole interior as shown in the equation giving $-1/\lambda_1$. This quantity really means the neutron density at the nuclear surface. It is a kind of reduced width, but with a normalization of differing from that used in the R matrix theory. The difference is contained in the fact that in the present case the integral is taken through the whole region, not just through the nuclear interior. The energy levels "E_λ" in this case are uniquely defined. They do not depend on the surface that you draw, as they do in the R matrix theory. They are the real stationary levels of the system and their choice is not subject to the flexibility one is usually exposed to in theoretical discussions of the excited states of nuclei.

The cross section is given by the equation shown in the last line of the slide. The reduced widths enter and the exponential factor which occurs last varies rapidly with energy.

One calculates the cross section for incident energies of 10 Mev and 15 Mev and compares the results with the experiment, as in Slide 7. The ratio of cross sections at different energies, you will notice, is quite different experimentally and theoretically. That is the origin of the factor of 11 mentioned earlier. This slide shows results of calculations with a specialized model employing potential wells. This, however, does not matter. Only the absolute values are affected by the specialization. The ratios are not affected, as one can see from the formula that was previously shown.

Since there is such violent disagreement with experiment an attempt was made to change the theory. The weak point of the consideration just presented is that it presupposes that the only forces of any importance are short-range forces. As long as all the important forces have a short range, then the matter of going through the potential barrier can be considered in the way it has been. However, the Coulomb force is obviously not a short-range force and, therefore, it appears desirable to take it into account, the general idea being

that the nucleus on the right produces an electric field at the nucleus on the left even if the distance between them is large, and that the electric field can affect the second nucleus putting it into an excited state. If the nucleus is in an excited state, then the wave function can penetrate through the gap between the nuclei very much better, its spatial character being radically changed by its having a higher energy. For instance, if the wave function is continued to great distances it does not decay exponentially and the large effects of energy are absent.

I should have mentioned that the angular distribution in the reaction N^{14} (N^{14}, N^{13}) N^{15} also disagrees violently with the simple tunneling picture. In the simple tunneling picture you get the largest transfer when the collision is close, while experiment indicates that this is not so. So in order to consider this other situation, the problem of the Coulomb field has been treated. There won't be time to go into this matter in detail. As shown in the second equation of Slide 8, the potential energy can be divided into parts suitable for the description of the initial stage. The first part $Z_1 Z_2 e^2/R$, the Coulomb energy between the colliding nuclei, can be used to define the Coulomb functions, the second part is the Coulomb excitation potential, the part referred to as V_2^{eff} may be used in the introduction of internal functions w_μ describing the excitation functions of the second nucleus. The excitation of the w_μ gives rise to the transfer of a proton to nucleus number 1. In the final state the grouping of terms is as represented in the first equation in the slide.

You then have produced a function Ψ_{CN} which is the Coulomb nuclear wave, and $\Psi^{(1)}$ is the correction to the Coulomb nuclear wave, a sizeable one, which is then calculated as shown in Slide 9 by the usual expansion.

The first line in Slide 10 shows the equation used to determine the coefficients in that expansion; in a certain approximation illustrated in the second and third equations of the same slide you find that this correction can be treated as the Coulomb wave for the relative motion of the two particles, corrected for the distance R between the two nuclei.

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Neutron transfer cross sections in cm^2 for the $N^{14}+N^{14}$ reaction.

	E_{lab} = 10 Mev	E_{lab} = 15 Mev
Theory ($b = 3.38 \times 10^{-13}$ cm)	1.3×10^{-30}	8.4×10^{-28}
Theory ($b = 4.0 \times 10^{-13}$ cm)	5.9×10^{-30}	3.8×10^{-27}
Experiment*	2.5×10^{-29}	1.32×10^{-27}

* H. L. Reynolds and A. Zucker, Phys. Rev. 101, 166 (1956).

Slide 7

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DEFINES COULOMB FUNCTIONS — COULOMB EXCITATION POTENTIAL — NON-ELECTRIC — INTERNAL

$$V = \frac{(Z_1+1)(Z_2-1)e^2}{R} + (Z_2-1)\left(\frac{1}{r_p}-\frac{1}{R}\right) + V_i^{eff} + V_2(\vec{r}_p) + V_{11} + V_{22}$$

DEFINES COULOMB FUNCTIONS — COULOMB EXCITATION POTENTIAL

$$V = \frac{Z_1 Z_2 e^2}{R} + Z_1 e^2\left[\frac{1}{\vec{R}-\vec{r}_p} - \frac{1}{R}\right] + V_2^{eff} + V_1(\vec{R}-\vec{r}_p) + V_{11} + V_{22}$$

$$V_2^{eff} = V_2(r_p) + \frac{Z_2 e^2}{r_p}$$ USED TO DEFINE INTERNAL FUNCTIONS w_μ

Slide 8

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$$(H^{CN} - E)\Psi^{CN} = 0$$

$$H^{CN} = H - V_1(|\vec{R} - \vec{r}_p|)$$

$$\Psi = \Psi^{CN} + \Psi^{(1)}$$

$$(H - E)\Psi^{(1)} = - V_1 \Psi^{CN}$$

$$\Psi^{(1)} = \sum_j u_j (\vec{r}_p - \vec{R})\ \varphi_j (\vec{R})$$

$$- V_1(|\vec{R} - \vec{r}_p|)\Psi^{CN} = \sum_j u_j(\vec{r}_p - \vec{R})\chi_j (\vec{R})$$

Slide 9

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$$\left\{ - \frac{\hbar^2}{2\mu} \Delta_{\vec{R}} + \frac{(Z_1+1)(Z_2-1)}{R} e^2 - (E - E_j) \right\} \varphi_j(\vec{R}) = \chi_j (\vec{R})$$

$$w_{-\mu}\psi^{(1)}_{-\mu} \simeq (-)^{\mu+1} \frac{(4\pi)^{\frac{1}{2}}}{3 Z_2 R} \langle r_p \rangle Y_{1,\mu}\ \psi^c\ w_{-\mu}$$

$$\psi^{(1)}_{-\mu} \simeq (-)^{\mu+1} \frac{2 (4\pi)^{\frac{1}{2}} \langle r_p \rangle}{3 Z_2 \beta^2 a R^2} \psi^c$$

$$\beta^2 = - k_w^2, \quad a = \hbar^2 / (\mu Z_1 Z_2 e^2)$$

Slide 10

Then, for one condition or rather one extreme in which you deal with excitation just above the energy for which the two heavy particles would be just able to come apart with zero kinetic energy, you obtain one dependence on R. If you go to another extreme case where the particles can separate in a state of high excitation, then you get another dependence on R.

In these extreme cases estimates were made of the probability of excitation. That probability depends essentially on a kind of Coulomb excitation, in this case a dipole excitation. One deals essentially with the result of estimating the square of the effective matrix elements, employing the general procedure of Bethe and Levinger as illustrated in the first three equations of Slide 11. In the last two equations are shown differential cross section formulae which involve no exponential energy dependence. The dependence on angle may be considered as being in reasonable agreement with experiment. This dependence does not favor the very large angle collisions and is also in this respect better than the simple tunneling theory.

In Slide 12 is shown a relationship, which can be obtained by means of Ter Martirosyan's integral previously referred to, which has a bearing on the question of how to plot the total cross section against a combination of the Q value of the reaction and the bombarding energy in such a way as to expect a universal curve for reactions between similar nuclei. These types of plots were referred to last night in the general survey of nuclear reaction chemistry. The "exact" relation is the one shown in the second line of Slide 12. One finds that in an approximation the quantity shown in the last line should be of primary importance. If one plots the experimental results obtained at the ORNL by Zucker and collaborators employing this quantity, one finds that nuclei are reasonably similar in general structure tend to fall on the same universal curve. This, for instance, is the case for O^{16} and C^{12}. There are, however, other cases with cross sections which are not understood from this viewpoint. A particularly troublesome question arises in the case of Mg^{25} (N^{14}, N^{13}) Mg^{26}. In that case you have transfers to the ground state and also to an excited state, and apparently the two depend on the angle in a rather similar way.

$$(e\,\langle r_p\rangle)^2 = \sum_f \sum_{x,y,z} \left| \mathcal{M}_{fi}^{(x)} \right|^2$$

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$$\int_0^\infty \sigma(E_\gamma)\, d E_\gamma = \frac{2\pi^2 e^2 \hbar}{M c} \sum_f f_{if} = 0.060 \frac{NZ}{A} \text{ Mev-barn}$$

$$\langle r_p\rangle^2 = 0.63 \frac{N Z}{A \Delta E} \text{ barn}$$

$$\langle r_p\rangle \cong 4.7 \times 10^{-13} \text{cm} \qquad (\Delta E = 10 \text{ Mev})$$

$$\sigma_\Omega = \frac{b_1^2}{1760\,[1+\sin(\theta/2)]^4} \quad (E_{bomb} = 10 \text{ Mev})$$

$$\sigma_\Omega \propto \frac{\sin^2(\theta/2)}{[1+\sin(\theta/2)]^6} + \frac{\cos^2(\theta/2)}{[1+\cos(\theta/2)]^6}$$

Slide 11

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$$E^* = E - E_{barr} + Q, \quad E = \frac{1}{2}\mu v^2, \quad E_{barr} = \frac{Z_1 Z_2 e^2}{b_1 + b_2}$$

$$2\alpha(2a' - b_1 - b_2) \rightarrow \alpha(2a' - b_1 - b_2) + \bar{\alpha}(2\bar{a}' - \bar{b}_1 - \bar{b}_2)$$

$$E^{**} = E - E_{barr} + \tfrac{1}{2} Q$$

Slide 12

When I first looked at this case, it appeared in some strange way that for distant collisions the dependence on the angle was about what you would expect from simple tunneling theory and referring to the sketch on the board you could fit the data by a curve somewhat like that, although the data deviated from this curve at the larger angle, this being the angle theta and this being the cross section (these were references to ordinates).

The agreement with simple tunneling theory appeared puzzling because the virtual state formation apparently did not enter for the distant collisions. An offhand attempt at an explanation was to say that the virtual state collides with the other nucleus in such an adiabatic manner that nothing happens. But, of course, then one has to consider why this adiabatic effect does not occur in the case of N. There is a difference between Mg and N in the fact that in the case of N one is really concerned with collisions taking place between 180 and 90 degrees, because only for close collisions are there large effects. On looking at the situation more closely, however, one finds that one does not have to go to quite such extremes, because if you assume that the transfer initially takes place primarily into the excited state, and then calculate the effect of the adiabatic character of the collision, you find that on account of the excitation energy you have an effect that resembles what you would get from the simple tunneling theory.

The question arises as to what can be done with the modification of the theory that is obviously required on account of the electric field. Can one still obtain the important parameter of the nucleon density at the nuclear surface? Little is known about the density of nucleons at the nuclear surface and one would very much like to know more about it in order to have a check on nuclear models such as the shell model. It appears that the attainment of this goal is not so simple but the task also is not hopeless because one can bombard isotopes of different elements and through that obtain in a reasonably reliable way, I believe, the ratio of the densities, and similarly, of course, one can interchange the role of the target and the projectile and employ other nuclei, not

confining oneself necessarily to the isotopes. It may also perhaps be possible with an exact evaluation of the photon disintegration cross section data to make a more detailed calculation than the one that has been attempted and in that way to obtain absolute values. Whether we can obtain absolute values or not, it appears that it should be possible to obtain ratios of reduced widths.

CHAIRMAN DEVONS: Thank you very much, Professor Breit. I think in Professor Breit's case one should avoid the pitfalls of making too simplified assumptions. Would anybody like to fall into a pitfall?

J. S. BLAIR: Is your theory designed to treat the case where the two nuclei really graze, where one is not concerned with just the real tunneling effect; that is where the neutron instead of tunneling from one nucleus to the other just pops from one nucleus to the other?

BREIT: The calculations as they have been performed do not deal with this situation. That was done on purpose in order to try to see whether one could get a clearly defined parameter out of such work.

BLAIR: Does this mean then that one would expect your calculations to hold for the small angles, but that for large angles, where just the classical Coulomb trajectories would lead you to a grazing or a penetration of the two nuclei, you would expect a deviation?

BREIT: Yes. In this case (pointing at the yield-energy curve for the Mg reaction) these data were obtained at about 28 Mev, and this region here corresponds to a close collision. So there is no reason for expecting agreement at the larger angles. On the other hand, in the case of nitrogen, at the lower energy, 10 Mev, the nuclei never come close enough together to make a difference.

C. E. PORTER: Is there a simple explanation for the factor of 1/2 that appears in front of the Q value of the reaction?

BRIET: Yes, qualitatively one can explain the matter as follows. If you consider the initial state condition semi-classically then you expect just Q. If you go in the reverse direction you expect zero. So it is reasonable if you get Q/2 on using the more exact derivation.

CHAIRMAN DEVONS: Would you expect the similarity of the two nuclei in the nitrogen case, the identity of the two, in any way to modify the validity of the classical approximation?

BREIT: Yes, and we have taken it into account. That is, we considered the wave function for non-identical particles as being approximated by the orbit picture, and we have followed the quantum theory for the rest.

G. D. FRIER: Is the R minimum that you described the minimum for all trajectories?

BREIT: In the first consideration it was the minimum R for a trajectory in which a head-on collision takes place. This consideration was intended for an estimate only. Of course, there is another quantity that enters the calculations, which is the minimum R of a general orbit.

PORTER: Did you take into account the absorption of the nitrogen along the orbit or just following some sort of semi-classical orbit?

BREIT: Do you mean absorption on account of inelastic scattering?

PORTER: Inelastic scattering or even in the sense the reaction itself reacts back on the nitrogen within a wave length.

BREIT: Oh, I see. These reaction cross sections are really small. So that for the consideration of what happens in the distant collisions we just neglect the absorption.

PORTER: In a harder collision, though, it might be important.

BREIT: Yes. In the places where the critical tests enter.

20. NEUTRON TRANSFER FROM 140-MEV N^{14} IONS TO METALLIC TARGETS

E. L. Hubbard and G. Merkel
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The purpose of the experiment being reported was to obtain information about the general problems encountered when transfer reactions are done with the high energy ions now available at Berkeley. It was desirable to obtain some rough ideas about the angular distributions and cross sections of these reactions. This information will be useful in doing experiments similar to those done at lower energies by Halbert, Reynolds and Zucker.[1,2]

In these experiments the angular distributions of radioactive ions produced in transfer reactions were observed by catching the radioactive ions in stacked foils. The activity in the stacked foils was then measured. The following results are preliminary in that we now know that they can be repeated with much greater accuracy. The experimental setup was as follows:

A collimated 3/16 inch diameter, 140-Mev beam was incident on the target; 4 in. away from the target there was a brass plate. On this plate there were placed stacks of aluminum foils at five different angles. These angles ranged from approximately 5 deg to 20 deg from the beam direction. The brass plate had a hole in it so that unscattered N^{14} ions could be collected in a Faraday cup. The solid angle subtended by a group of stacked foils was 1.4×10^{-2} steradians. The range in angle of a given stack was approximately 2 degrees. We placed another brass plate over the stacked foils; this plate had holes slightly smaller than the area of the stacked foils. This was to prevent any scattered particles from hitting the edge of the stacks.

After a 10-minute bombardment of the various targets with the 140-Mev N^{14} beam, the individual aluminum foils, corresponding to ranges larger than 3 mils were mounted on aluminum plates and counted with Geiger counters. The resulting decay curves had various mixtures of approximately 2-min, 10-min, 20-min, and 112-min half lives. A gamma pulse

height analysis of the foils with sodium iodide crystals showed the presence of 0.5 Mev gammas. No other gamma activity could be detected. Some of the foils were also counted with two sodium iodide crystals hooked up in coincidence, and the 0.5 Mev gammas were found to behave like annihilation radiation.

We did no chemistry but felt fairly sure that the activities observed came from O^{15}, N^{13}, C^{11}, and F^{18}, respectively corresponding to the addition of a proton to N^{14}, the loss of a neutron from N^{14}, the loss of the equivalent of a triton by N^{14}. Since the N^{13} activity resulted from the simplest transfer reaction, and since this activity was spread over the least number of stacked foils, and since it also had a reasonable half life, that is, a half life of 10 min, we decided to concentrate on the N^{13} activity.

The target foils used were 1-mil aluminum, 0.6-mil copper, and 0.4-mil silver foils. These target foils degraded the incident beam to about 120 Mev. This means that the average energy at which neutron transfer took place was 130 Mev.

Slide 1 shows the relative N^{13} activity in the different foils; the angles are indicated at the top of the slide. The range of the N^{13} ions indicate that a relatively slight amount of energy is lost when a N^{14} ion lost a neutron and turned into a N^{13} ion. The activities were spread over two or three 1-mil aluminum foils; this means that the N^{13} ions have an energy spread of 20 or 30 Mev.

The accuracy of the relatively low activity at 6 deg in the case of silver is fairly poor, because at this small angle a large amount of 20-min C^{11} activity had to be subtracted from the 10-min N^{13} activity. The large angles on silver were obtained by moving the target a bit closer to the brass plate; that is, to 3 1/8 in. rather than 4 in.

I might mention that the C^{11} activity was peaked in a forward direction and spread over a much larger range of foils than the N^{13} activity.

To obtain Slide 2 all the N^{13} activity produced at one angle was summed; these sums were plotted as a function of center-of-mass angle. We assumed a Q of zero to obtain the center-of-mass angles. The ordinates give the absolute cross section in millibarns per steradian. The cross sections probably decrease at large angles because of competition from compound nucleus formation; the decrease at small angles probably occurs because the impact parameters corresponding to these angles are very large.

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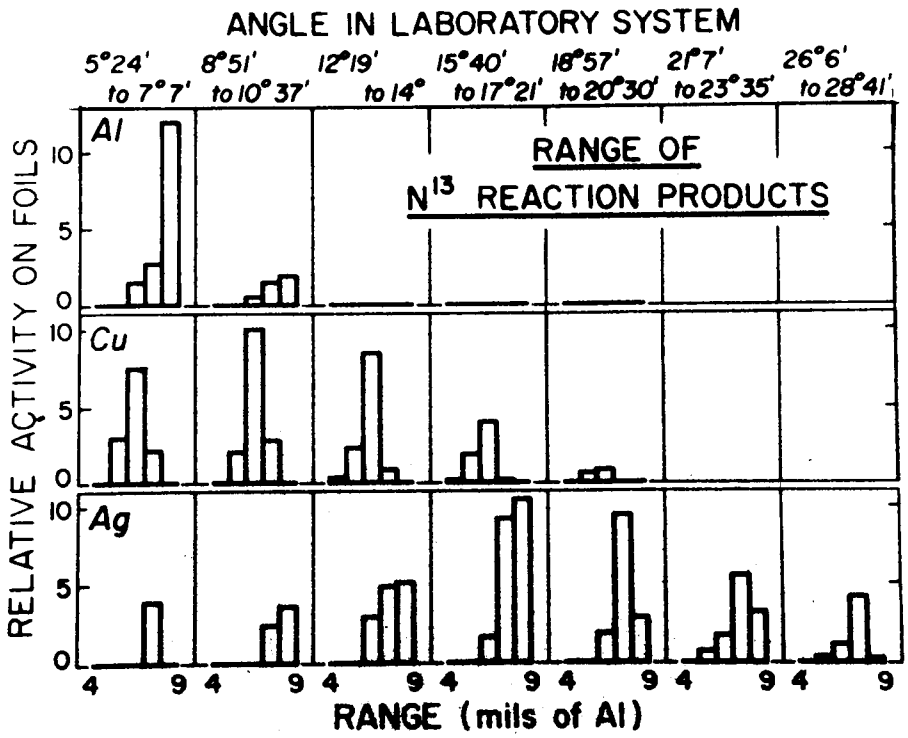

Slide 1

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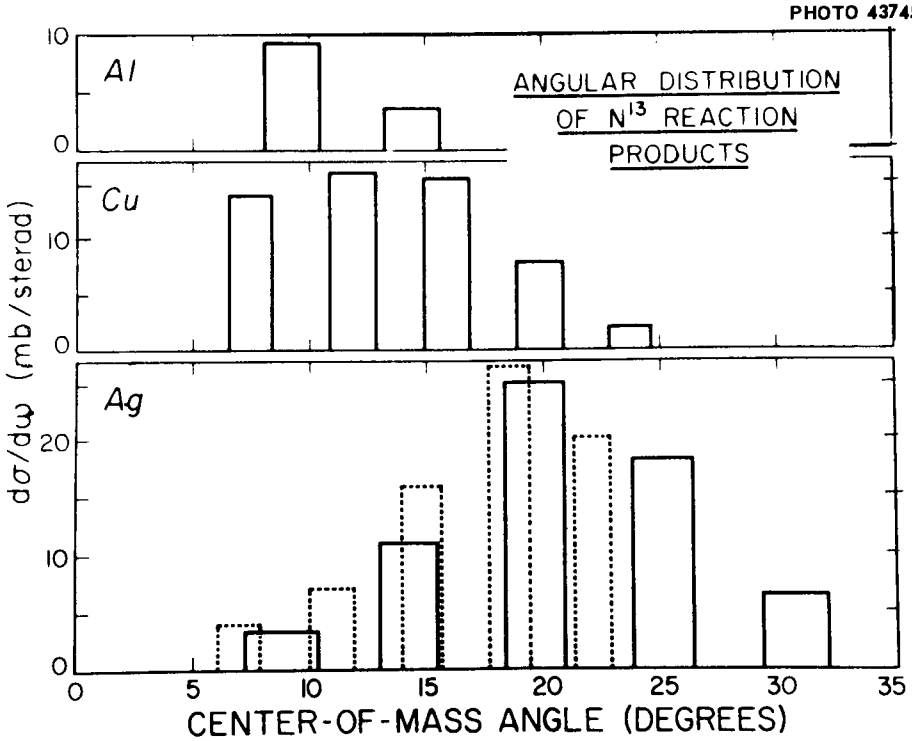

Slide 2

The classical impact parameters corresponding to the peak in the angular distribution for copper and silver were calculated and set equal to $R_o(A_1^{1/3} + A_2^{1/3})$, where A_1 is the mass number of N^{14} and A_2 the mass number of the target nucleus. R_o turned out to be approximately 2.0 fermis.

No attempt has been made to analyze this data on a more sophisticated level.

CHAIRMAN DEVONS: Thank you, Mr. Merkel. Are there any comments on these results straight from the press?

J. S. BLAIR: What do you mean by the classical impact parameter?

MERKEL: Well, $$D = \frac{Z_1 Z_2 e^2}{2E} \left(1 + \frac{1}{\sin \frac{\theta}{2}}\right)$$

A. ZUCKER: What is that dashed line on the last slide?

MERKEL: For silver we had two runs. One for a distance of 3 1/8 in. and another for a distance of 4 in. In other words, we moved it closer to obtain a larger range in angles.

ZUCKER: I have a comment to make then. This classical distance which you find to be 2 at 140 Mev is remarkably close to what it is at 25 Mev, where it is about 1.9.

MERKEL: Actually, we get 1.96.

ZUCKER: Well, they are the same thing. I think it is very interesting to see the transfer reaction essentially go at the same distance regardless of the energy of the particle and the nature of the target.

MERKEL: I wonder if one should just calculate R_o. One might let D equal some distance T plus $1.3\,(A_1^{1/3} + A_2^{1/3})$, one could then see if this T remains the same.

BLAIR: Could I say a word about the use of these formulas with R_o and such and whether you add a constant or not? I think it would be a great help if people just expressed radii in terms of the magnitude of the radius in fermis rather than always jumping to this expression in terms of something times A to the 1/3 plus a constant. There are so many different variations of these formulas going around, some with the constant and some without, I always get confused just exactly what one means, and then I have to get a slide rule out and compute back and I find out that with different constants they are really talking about the same radii. There is nothing that really says that you must have a dependence of this form, and

I think at this stage of the game we should ask what are the radii rather than put it in that form. Clearly this is a large radius.

ZUCKER: All you say the other way is just how large; so it prevents you from having to use a slide rule one way or the other way.

BLAIR: Well, I would like to know, for example, how does this radius compare with the radii that you would get for the analysis of the elastic scattering, the sharp cutoff radius?

MERKEL: Quite a bit larger.

BLAIR: About how much larger? One, two, or three fermis?

MERKEL: I don't remember. The R_o is something like 1.3. Dr. Reynolds, did you calculate what the R_o was?

H. L. REYNOLDS: 1.46.

CHAIRMAN DEVONS: You don't wish to put this to a vote, do you? (Laughter) Are there any more questions?

M. L. HALBERT: Maybe Prof. Breit might have something to say about the point that Zucker made, that this distance seems to be about the same at high and low energy. Maybe he can say something about the importance of the virtual Coulomb excitation process.

G. BREIT: It seems to me that to express it in terms of a radius is rather questionable. A person can always on dimensional grounds introduce a distance and then start to wonder about what happens to that distance, but according to any ordinary view of what might happen, I think it happens in another way. Certainly a virtual state formation would not lead you to expect the simple contact picture to hold, and yet in one characteristic one obtains something like a simple contact picture.

I can understand that it is somewhat easier to make a calculation this way, but I don't see that it is a reason for believing it. There is, of course, the possibility that in some way all nuclei will blow up to about the same extent, but that is so speculative that it seems better to me to take a bit more time to calculate the way one thinks it probably happens.

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21. DERIVATION OF OPTICAL MODEL PARAMETERS FROM TWO-BODY FORCES

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I should like to talk on a subject which may appear to have only a peripheral connection with the topic of this conference but perhaps in the long run might have a more basic connection. The topic is the derivation of the optical model parameters from two-body forces.

My interest in this question came about by virtue of some work on the Weizsacker equation, in which Dr. Lee and I[1] found it possible, using the shell model, to get an expression for the total energy of a finite system of Λ identical particles, (say, up spin neutrons) under the assumption they are interacting by two-body forces

$$E_T(\Lambda) = -1/2\Lambda V^* + \frac{U_o}{a^2}\left(1 + \frac{\beta}{2}\right)\Lambda^{5/3}\left(g_o + \frac{g_1}{\Lambda^{1/3}} - \frac{g_2}{\epsilon_o} - \frac{f_o}{2+\beta}\right), \quad (1)$$

This expression turns out to be quite similar to the expression one gets from the Fermi gas model. It involves the depth of the potential well, V^*, and a radial parameter a. Now to get anything sensible we have to go to a velocity-dependent well so there is a velocity dependence parameter β, which enters into the equation. We assume the well has a depth $- V^* + \beta T$ where T is the kinetic energy. If β is a number comparable to 1 we have the effective reduced mass of the order of 1/2 such as occurs in the Brueckner theory.

In our studies we encounter a number of constants g_o, g_1, and g_2 which we could evaluate by actually summing eigenvalues in a spherical well, and another constant f_o, a small correction that depends on how much the wave functions leak out of the well. The quantity ϵ_o here is a measure of certain combinations of the well parameters, and U_o is an energy constant $\approx$ 20.734 Mev.

*On leave until September 1958.

Without going into details let me say that Eq. (1) gives the total energy of the system of Λ particles obtained by summing the eigenvalues and subtracting half the expectations of the potential energies. This is what you would have to use if your potential originates from two-body forces. Now if you have an expression for the total energy of the system of Λ identical particles, you can assume an actual nucleus consists of four sets of identical particles, and you can go ahead and derive a mass surface. What you get out of this kind of derivation is an expression

$$E = -\alpha_1 A + \alpha_2 A^{2/3} + \alpha_4 \frac{(N-Z)^2}{A} - \alpha_5 \frac{(N-Z)^2}{A^{4/3}} \qquad (2)$$

Where the α's are derived constants. This expression is almost identical with the usual Weizsacker mass formula, however, we obtain an additional term which might be called a surface symmetry energy. This term might have an important bearing on the theory of fission, in that it suggests that the surface tension of the nucleus depends upon the neutron excess.

One might hope to derive the mass surface from two-body forces by deriving the well depth and the degree of velocity-dependence from two-body forces, and then perhaps arrive at the radius constant in some self-consistent way. The rest of my talk will be addressed to deriving the well depth and the degree of velocity-dependence at the center of the nucleus, and finally the appropriate radius constant. In this effort one can make use of the recent work associated with the name of Brueckner and contributed to by many others. I have particularly made use of Gomes, Walecha and Weisskopfs'[2] recent simplifications of Brueckner's work in which he talks about "wounded" wave functions and "healing" distances.

Let us consider now what is the potential depth acting on one nucleon at the center of the nucleus. One can break the potential into the contributions associated with the repulsive core and the contributions arising from the attractive part of the two body well. The contributions associated with the repulsive core can to a very good approximation be calculated on the basis of interaction of Fermi particles in which the attractive parts play no role. This repulsive core contribution can be taken bodily from the work of others [3,4] and can be shown to be given by

$$V_c = \frac{2\hbar^2 k_f^2}{2m}(1+\beta)\left[\frac{2}{\pi} c\, k_f + \frac{12}{35\pi^2}(11-2\ln 2)\, c^2 k_f^2 + 0.26\; c^3 k_f^3\right], \qquad (3)$$

where k_f is the Fermi momentum, and c is a numerical constant. I might say that three-body correlations affect the calculation in the coefficient of the last term.

It turns out that on the basis of Brueckner's[5] detailed calculations and a number of others one can say that the core contribution is not very velocity dependent; so this component can be regarded as a static contribution.

The attractive part of the well can be readily calculated, assuming that the two-body force is of the Serber type:

$$V_a = 1/2\,(1 + P_m)\,V_o\,\eta(r) \tag{4}$$

The form of the two-body function that I have used is the same as the one used by Weisskopf[2], et al, i.e.

$$\begin{aligned} \eta(r) &= 0 \qquad r < c,\ r > b \\ \eta(r) &= 1 \qquad c < r > b \end{aligned} \tag{5}$$

The depth and the radius are adjusted in view of some of the properties of the deuteron.

It is possible now to sum up the interaction of a single nucleon with all the other nucleons in the nucleus. For this purpose you must assume wave functions for each particle, and I have used plane waves. This is not as crude as one might think, and the justification for using plane waves, as shown by Weisskopf, et al, is related to the fact that a given particle cannot scatter other particles into the already occupied states of the nuclear medium; the use of plane waves for calculating interaction of particles imbedded in nuclear matter has a considerable degree of justification within the Breuckner theory providing one independently allows for the effect of the repulsive core.

Using plane waves and this two-body interaction, one can ask what is the interaction of a single particle with all the other particles of nucleus. The calculations are fairly involved[6], but what you find is that the potential energy seen by the nucleon, having a momentum k_a is given by

$$V(k_a) = \sum_\beta U_{a\beta} = -\left[\frac{\Lambda}{R^3}\,\frac{V_o b^3}{2} + \frac{U_o}{\pi}\,g\,(b, k_a k_f)\right] - S(c) \quad , \tag{6}$$

where S(c) means the same function of the core parameter c and R is the appropriate Fermi radius. The function $g(b, k_a k_f)$ is a readily determined analytic function.

The first term is a static contribution. The second velocity-dependent term comes mainly by virtue of the exchange character of the two-body force. Knowing this explicit form for the potential seen by a single particle, one can expand it into the form

$$-V^* \xi(r) + \beta T$$

arriving at an explicit V^* and a β which depends on the radial parameter R and the two body force constants.

It can be shown that this radial parameter R is approximately equal to the radial parameter a in our phenomenological equation (Eq. 1). In this way one can get the total energy of a finite system as a function only of the single parameter R and the two-body force constants. You can then, for a given nucleus, assume R takes on various values. You will find that the total energy goes through a minimum and, rather interestingly enough, this minimum corresponds to a nucleus at about normal density.

Well, let me just conclude by saying that it appears, using relatively simple two-body forces, that you can derive a velocity-dependence and the well depth are a function of the radius. This potential will be much too deep unless you also take into account the core contributions, but if you take the core into account you arrive at a mass surface which is in the right neighborhood. At the same time the well depth that you deduce is something quite comparable to that needed in optical model analysis of nucleon-nuclear scattering. The mass formula derived has a surface tension that is dependent upon the composition of the nucleus which may require a modification of the theory of fission.

This derivation of the nucleon-nuclear potential might perhaps be extended along similar lines to a derivation of the nuclear-nuclear potential needed in heavy ion interactions, although it might not be too simple an effort until we have a clearer physical picture of what is happening when one nucleus merges with another nucleus.

CHAIRMAN DEVONS: Thank you Dr. Green. Are there any comments on this interesting contribution?

W. E. BURCHAM: Do you get a value for the binding energy for the nucleon or is that built in from the mass equation?

GREEN: The depth of the two-body well that I chose was one with a radius 2.3 (in fermis), a depth of 28 Mev, and a core radius of 0.4. This well would just barely bind the deuteron. We know that the true two-body interaction is a much more complicated beast that has the Yukawa forces and the different ranges in the singlet and triplet states and all that. But with this simple well, we get a binding energy per particle at stability of the order of 6 Mev per particle. Now this figure should not be compared with the experimental value of 8 Mev because I haven't allowed for Coulomb effect. Instead it should be compared with about 12 Mev which you would expect from the mass formula. However, if one just increases the attractive two particle well depth by about 20%, one obtains the proper magnitude of binding energy per particle. So it is my feeling that if I went to realistic wells such as the Gammel-Thaler two-body potentials which are much more difficult to integrate, I would get the proper order of magnitude of binding energy.

BURCHAM: And the saturation is looked after by the positive core?

GREEN: The saturation is taken care of partially by the repulsive core and partially by the exchange character of the two-body forces.

BURCHAM: Which I think isn't quite enough to do it, the exchange character alone.

GREEN: Not alone, no, the repulsive core plays an essential part. I might say that the very high dependence of the repulsive core contribution upon density is extremely important. These higher order terms, which you might think are small in an expansion in terms of Fermi momentum, k_F, become quite a bit more important than you would want. Indeed, the last term is influenced by the magnitude by three-body clustering effects. Thus, you will get a somewhat different equilibrium, depending upon how you treat this cubic term so I am not sure whether you can go too far by assuming only two-body interactions.

A. BROMLEY: Have you had an opportunity to compare the mass surface that you obtain this way with the purely empirical result that Cameron obtained recently using a trapezoidal distribution, and using the radius as a variable parameter? Have you been able to compare your results at all with his, because his results at the moment do seem to give quite ample descriptions of some of the available fission data?

GREEN: Well, I don't know that I agree with that last comment of yours; in fact, I definitely disagree. But in this respect it agrees:

Cameron does introduce a surface-dependent symmetry term, $(N-Z)^2/A^{2/3}$, similar to the one which comes out of this derivation. But with regard to how well Cameron did on fission using this surface tension, I believe he ran into predictions of fission thresholds of 80 Mev or so, and so he is in difficulty on the theory of fission.

BROMLEY: That, of course, is the big discrepancy. When he tries to fit the mass yield, for example, it looks much more impressive.

GREEN: In mass fittings he does well. I might say the reason he does particularly well when compared to the old Weizsacker equation is mainly due to the fact that he uses about 350 empirical constants to take into account the shell effects.

BROMLEY: Oh, no more than 200. (Laughter)

N. ROSENZWEIG: I hope this question is not too technical, but surely at one point in this whole derivation you made use of the well-known equation about the number of states in phase space; that is, the number of states contained in an element of phase space. This is where the r_o times $A^{1/3}$ must come in. Now the question is this. Perhaps it is altogether trivial. I just don't know much about this, but is it well known that this whole business is still true, the fundamental theory that when the well is momentum-dependent--that is, don't you make use of the fact that the well depth depends on momentum?

GREEN: Well, the place where I make use of the number of states at various momenta in the Fermi distribution is where I consider one particle with the momentum k_a, and then I ask what is the probability of finding another particle with the relative momentum $\underline{k}$. In this case I use the appropriate functions for an infinite Fermi gas but in which you take into account certain surface effects in phase space. It is essentially Brueckner's distribution in which he went beyond the usual distribution functions for an infinite Fermi gas. To that extent we are more careful than usual. I am not sure that this answers the question you are asking.

ROSENZWEIG: It is probably a fairly complicated problem to see whether this whole thing is consistent, but at the point where you wrote the 1/3rd you ignored that aspect.

GREEN: No, in deriving the result in Eq. (3) I converted the sum to an integral, and to do this, with a given one-particle momentum one must know what is the probability of finding the second particle with the relative

momentum $\underline{k}$? This is where the dependence upon the Fermi momentum comes up and the dependence upon the relative momentum comes in. Maybe we can fight it out together, later.

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22. NUCLEAR TRANSFORMATIONS BY 2-MEV LITHIUM IONS

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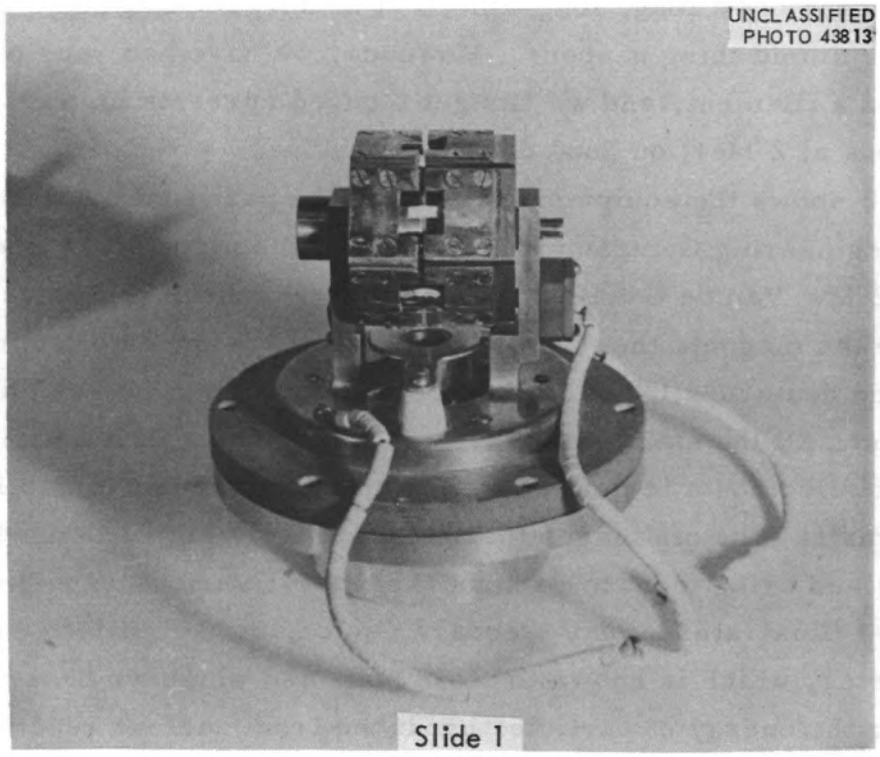
I am acting here as the spokesman for a group of people who have participated in the experiments. C. S. Littlejohn and G. C. Morrison are here and I expect them to extract me from the pit after the talk if the questions get too pointed. Ed Norbeck, Jr. is going to speak for himself in a subsequent lecture, and I am going to speak a bit for P. G. Murphy who worked in this field and is now at the University of Liverpool.

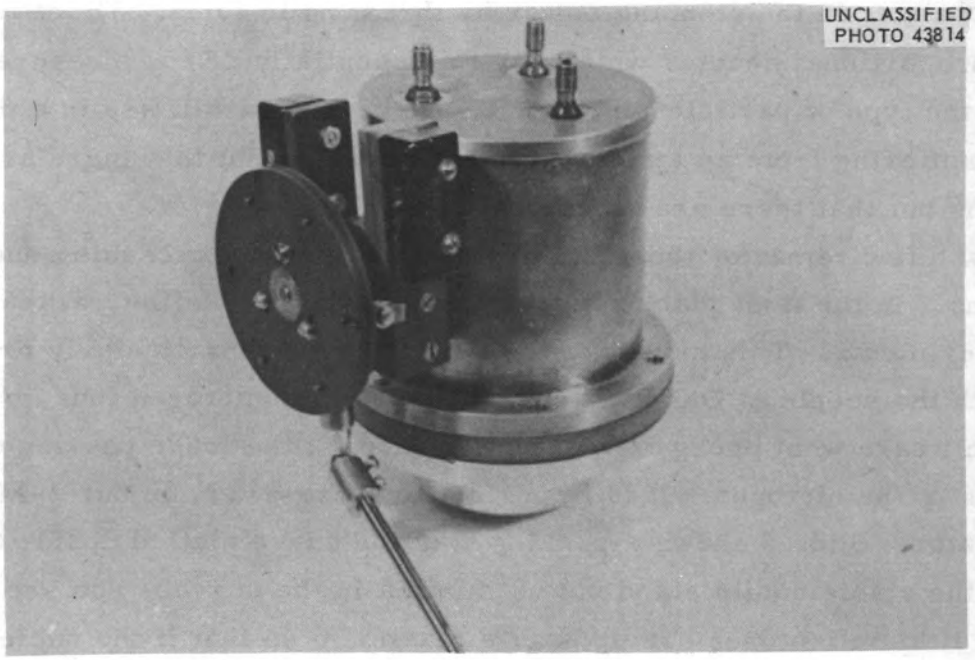
The mass spectroscopists and lesser folk of that ilk have known for a long time that it is very simple to make positive lithium ions. All you have to do is to pester the geologists to give you some lithium minerals. The best known ones are spodumene and beta eucryptite. These are lithium-aluminum silicate eutectic mixtures which occur in minerals and which have a melting point of about 1450° C, and if these things are heated up to about 1100° C on a hot filament, lithium comes out. Strangely enough it comes out as positive lithium ions and not as atomic lithium and the emission is rather copious. It is not difficult to get lots of lithium ions out.

By now we are way beyond the geologists. We don't have to pester them any more, because with the aid of our hosts here at Oak Ridge we have been supplied with separated isotopes of lithium. We make up our own beta eucryptite with Li^6 or Li^7, mix this stuff in the proper portions, grind it up and melt it, and then grind it again and coat the filaments with it suspended in standard amyl acetate, or whatever you use for this purpose, and we get a convenient source of lithium ions.

I am going to show a few slides now just of the gadgetry of lithium ion source. Slide 1 shows hexagonal drum which bears the six filaments. These filaments are coated with the lithium preparation and can be rotated in turn over the throat or the entrance to the 2-Mv Van de Graaff, and so we can call for Li^6 or Li^7. Slide 2 shows how this drum can be rotated from the outside; this is the vacuum housing over the hexagonal drum. People always ask about the lifetime of the filaments. Actually, the lifetime is determined by accidents and operational stupidities rather than by



The six-filament ion source.



Slide 2

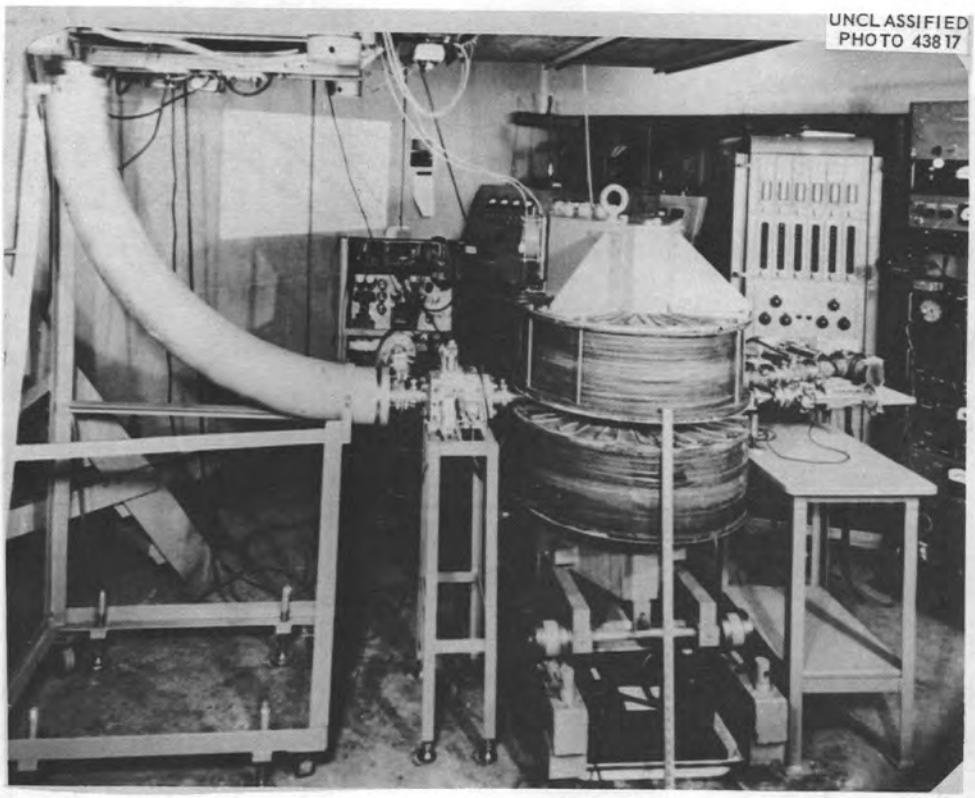
Vacuum housing for the six-filament ion source. The leads to the electrodes are on top and the selsyn-operated filament charger to the left.

the amount of lithium to be evaporated. That is, there is plenty of lithium there to be run for a long, long time, but usually the filaments break or some other stupid thing happens. However, we have had runs of a week or ten days on a filament, and we can get focused currents on the target of the order of 5μa at 2 Mev; on good days we get 10μa.

Slide 3 shows the equipment, all except the part contributed by the High Voltage Engineering Company which is above the picture. It is one of their standard 2 Mv Van de Graaffs, but one thing you have to change is that you cannot use the magnets they supply which were designed, of course, for protons and deuterons. The lithium comes cown from the Van de Graaff and bypasses all the standard magnets. It is brought into a horizontal plane through the electrostatic deflector, and a feedback control stabilizes the Van de Graaff. The big magnet here -- relatively big -- is used to deviate the beams and bring them to various targets for various experiments.

Slide 4 illustrates a very standard device, the scintillating crystal spectrometer, which is known to all of you, and which we have used in measuring the energy of particles produced from nuclear reactions caused by lithium. The particles from the target first pass through a region into which gas can be put to make range measurements. Also we can make larger changes in the stopping power by thrusting in foils. There is also a proportional counter which, by its essentially dE/dx measurement, selects the type of particle we want, because, as you will see in a moment, we are suffering from an embarrassment of riches; not that there are few reactions but that there are an enormous number.

Now a few remarks about lithium as a projectile for causing nuclear reactions. In the first place, it acts like an explosive bullet, at least in our experiments. This aspect of the situation is not particularly prominent with the people at Oak Ridge who have 27-Mev nitrogen ions, because they don't care what energy is carried in through the loose packing of the nucleons of the nitrogen. It is important to us however, in our 2-Mev accelerator. Slide 5 shows a packing fraction curve plotted upside down so that the stable nuclei stand out as minima in the curve. You see that our two lithium isotopes are up near a maximum so that if the nucleons in them are rearranged into He, Be^8, or C^{12}, we get essentially large releases of energy and we can go both ways toward lighter or heavier products. So we find, as you will see in a moment, that there are many



Slide 3

The target room of the University of Chicago 2-Mv Van de Graaff. The 90-deg electrostatic analyzer is on the left, and the analyzing magnet is just to the right of center.

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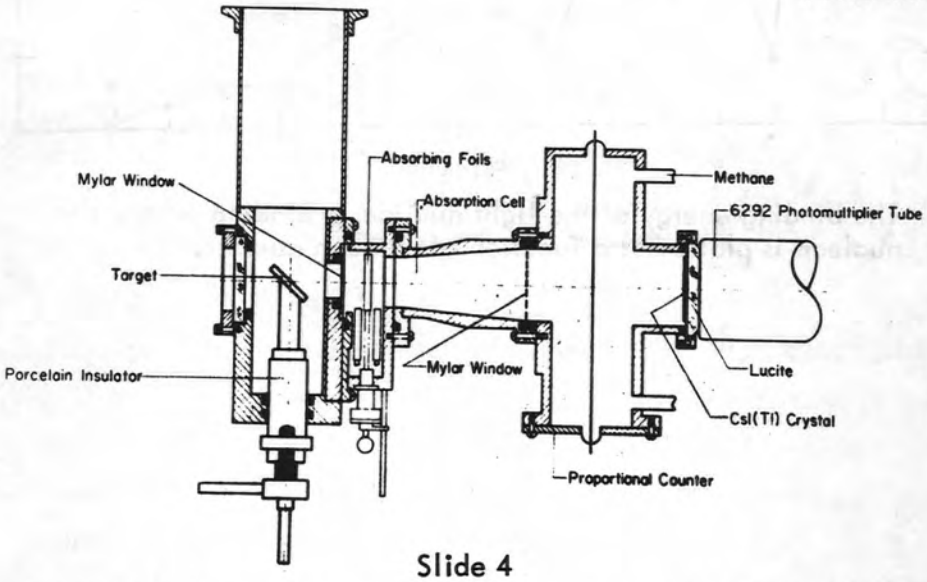

Slide 4

Target chamber and detection apparatus used in the observation of proton groups from lithium-induced reactions.

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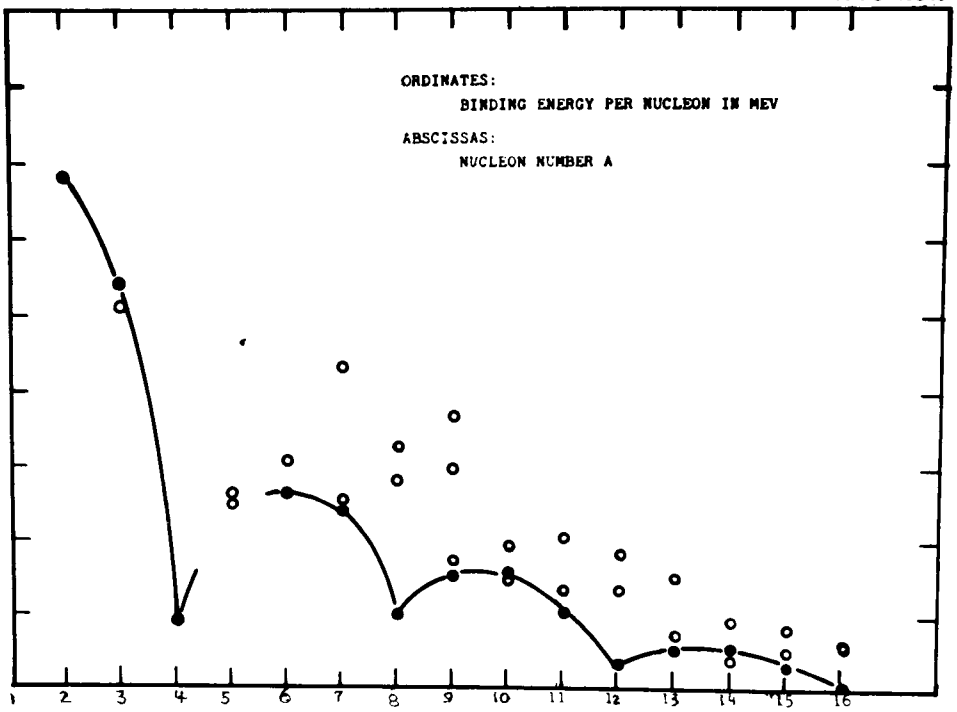

Slide 5

The binding energy of the light nuclides. Binding energy per nucleon is plotted as a function of nucleon number.

reactions of high Q values and we take advantage heavily of this aspect of the situation.

In Slide 6 we show just a few of the reactions that can occur between two interacting lithium nuclei, and I say just a few because none of the neutron-producing reactions are listed, but the list is rather impressive. You see what a mess you get -- protons, deuterons, tritons, alpha particles, He^3, He^6, etc. But it so happens that certain proton groups, stand out well in range beyond the other particle groups, and we have investigated them.

Slide 7 shows a similar list of some of the reactions from the bombardment of Be^9 with lithium. A proton reaction which I am going to talk to you about has a very high Q value leading to the little known nucleus C^{15}. There are other cases where radioactive nuclei are produced and Mr. Norbeck is going to talk about them.

A few other remarks in general about the situation of the lithium. The barrier height problem is not so serious. It limits the reaction cross sections to a certain extent but not fatally so. This is the kind of calculation you make on the back of an envelope that you pull out when somebody is talking to you. But if you consider the impact of two Li^7 nuclei and take r_o as 1.45 fermis and just go ahead and multiply by the cube root of A, nuclear contact would take place at about 5.4 fermis, and the barrier height is about 2.4 Mev. In the laboratory system with 2-Mev particles we have one Mev available. So we are on the order of 1/2 to 1/3 the height of the barrier. We are still well below the barrier. We could not do scattering experiments with our present equipment that would reveal something about nuclear surfaces, but many reactions involving barrier penetration proceed.

Another remark that I might make, that has been made by many speakers, is that in common with all of these heavy particle reactions, it is easy to transfer large amounts of angular momentum. Actually, a little calculation will show that impacts within the nuclear system will transfer up to $\ell = 2$ units of angular momentum. Somewhat beyond $\ell = 2$ the probability begins to get small. So one expects rather large momentum transfer.

Another remark is that at least in our situation the velocity of approach is very slow. We are dealing with 1 Mev in the center-of-mass system. The velocity of approach in the center-of-mass system is about 5.6×10^8 cm/s. The time required to cross the nuclear diameter is about 10^{-21} sec, which

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Reaction	Q	Reaction	Q
	Li^7 beam, Li^7 target		
$Li^7(Li^7,p)B^{13}$	(5.97)*	$Li^7(Li^7,\alpha)Be^{10}$	14.77
$Li^7(Li^7,d)B^{12}$	3.31	$Li^7(Li^7,He^5)Be^9$	6.96
$Li^7(Li^7,t)B^{11}$	6.22	$Li^7(Li^7,He^6)Be^8$	7.23
	Li^7 beam, Li^6 target		
$Li^6(Li^7,p)B^{12}$	8.32	$Li^6(Li^7,He^3)Be^{10}$	1.450
$Li^6(Li^7,d)B^{11}$	7.19	$Li^6(Li^7,\alpha)Be^9$	15.21
$Li^6(Li^7,t)B^{10}$	1.99	$Li^6(Li^7,He^5)Be^8$	12.54
	Li^6 beam, Li^6 target		
$Li^6(Li^6,p)B^{11}$	12.21	$Li^6(Li^6,He^3)Be^9$	1.884
$Li^6(Li^6,d)B^{10}$	2.98	$Li^6(Li^6,\alpha)Be^8$	20.78
$Li^6(Li^6,t)B^9$	0.797	$Li^6(Li^6,He^5)Be^7$	0.898

*Measurement reported in this paper; other values estimated from M-A values in Ajzenberg and Lauritsen, Rev.Mod.Phys. 27, 77 (1955) page 157

Slide 6

Exothermic reactions to be expected from the lithium bombardment of lithium targets. Q-values are given in Mev.

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I. Li^7 Beam

Reaction	Q	Reaction	Q
$Be^9(Li^7,n)N^{15}$	18.08	$Be^9(Li^7,\alpha^4)B^{12}$	10.46
$Be^9(Li^7,p)C^{15}$	9.05		
$Be^9(Li^7,d)C^{14}$	10.10		
$Be^9(Li^7,t)C^{13}$	8.19	$Be^9(Li^7,Li^6)Be^{10}$	-0.43
$Be^9(Li^7,H^4)C^{12}$	3.24-7.74	$Be^9(Li^7,Li^8)Be^8$	+0.37

II Li^6 Beam

$Be^9(Li^6,n)N^{14}$	14.50	$Be^9(Li^6,\alpha)B^{11}$	14.34
$Be^9(Li^6,p)C^{14}$	15.12		
$Be^9(Li^6,d)C^{13}$	9.18	$Be^9(Li^6,Li^7)Be^8$	5.58
$Be^9(Li^6,t)C^{12}$	10.49	$Be^9(Li^6,Li^5)Be^{10}$	1.307

Slide 7

Exothermic reactions to be expected from lithium bombardment of beryllium targets. Q-values are given in Mev.

should be compared with what you might call the period of a nucleon in the nuclear potential well which is 10 times less. So that you have a slow approach here compared to the violent motion of the nucleons in the nuclei themselves. There is time for equilibrium configurations to take place during the approach.

I am going to talk about some energy level determinations and some nuclear spectroscopy that we have been able to do. We can produce nuclei with total, that is, non-projected isobaric spin or isotopic spin of three halves; these are relatively little known nuclei. The B^{13} is produced by this reaction, I believe we are the first to have seen the ground state of B^{13} and now we have one, and possibly two, excited states. The C^{15} is produced by bombarding Be^{9}, and in addition to the ground state which we thought we were discovering while we were working on it, but, later found that the people at Madison were discovering at the same time and published a little sooner -- we have in addition to that been able to get several excited states. I will show you these, and then the nucleus N^{17}. We have been able to get an independent measurement of the ground level and 5 or so excited states.

Let's look at the boron-13 situation first in Slide 8. This represents the energetics, let's say, of the isobars of mass 13. (In the center of the figure B^{12} should read C^{12}.) The zero of energy is arbitrarily placed at the ground state of B^{13} with what I call our poor man's 10-Mv Van de Graaff, making use of the packing fraction of the lithium, we are already far up in the B^{13} energy diagram. We add on a little energy through the Van de Graaff but we can get up to some 6 Mev. In addition to this we now know, since this slide was made, that there is a level at 3.65, and possibly another level at 4.2 Mev.

Now B^{13} is a nucleus of some interest in that it is a possible delayed neutron emitter. From the position of the C^{12} plus neutron energy level, it is energetically possible to emit both beta rays and delayed neutrons. We have looked for delayed neutrons, not with the most profound techniques for neutron detection it is true -- in fact, our health physics group furnished our neutron monitor -- and we did not find any. It is probable that there are very few neutrons because there are plenty of levels known in C^{13} to which the beta rays could go without violating any selection principles and which are below the threshold for emission of neutrons.

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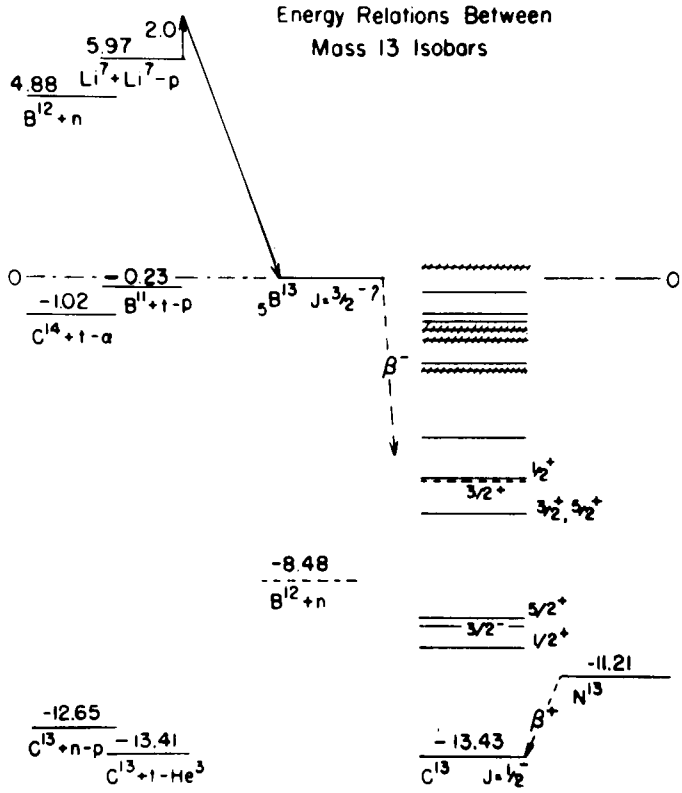

Slide 8

Energy relations among mass-13 isobars. (In the center of the figure, B^{12} should read C^{12}.

Another embarrassing question to us is that we do not really know yet what the beta lifetime of this nucleus is although we have tried to a certain extent to measure it. The trouble is that in a competing reaction, we get deuterons out, leaving B^{12} copiously produced with its lifetime of 18 1/2 milliseconds. Our attempts to observe B^{13} beta rays are against that background. I think what we could say now that either the beta lifetime of B^{13} is very close to B^{12} or it is very different. We would have missed it if it were very close; we would have missed it if it were very long, a very long period of weeks or so, because we didn't activate long enough. The B^{13} nucleus seems to be very similar to B^{12}. It has a possibility of 13.4-Mev beta rays, and B^{12} has almost the same.

Slide 9 shows another delayed-neutron emitter, N^{17}, and I am going to come back to this later. The situation is more favorable to delayed neutron emission. This is the N^{17} ground state up here. There are levels that are prominent where the beta rays can be emitted, followed by delayed neutron emission over here. Here is a highly forbidden beta ray transition due to the large change in angular momentum. So the situation is really somewhat different. It is a path which is much favored in comparison with the B^{13} situation to go through these levels and produce delayed neutrons. This is trying to rationalize our failure to find any delayed neutrons from the B^{13}.

Now let's look at Slide 10 a moment and look at the excitation curve. Here is the differential cross section per steradian for the production of the proton group from the ground state of B^{13}. This is in units of 10^{-30} centimeters; so you see we are quite low here in cross section. There is no indication of anything but an exponential rise in the cross section, which indicates that things are being controlled almost completely by the Coulomb barrier. One would expect it to be so because we are quite far below the top of the Coulomb barrier.

Slide 11 shows the C^{15} situation. This was worked out by P. G. Murphy. Again we cash in on our packing fraction energy and we start high in the C^{15} system.

While we were discovering C^{15}, the group at Madison were discovering it through the $C^{14}(d,p)$ reaction. You see the Q value of $C^{14}(d,p)$ is -1.06 and they did not get as high as our first excited level. Although their value

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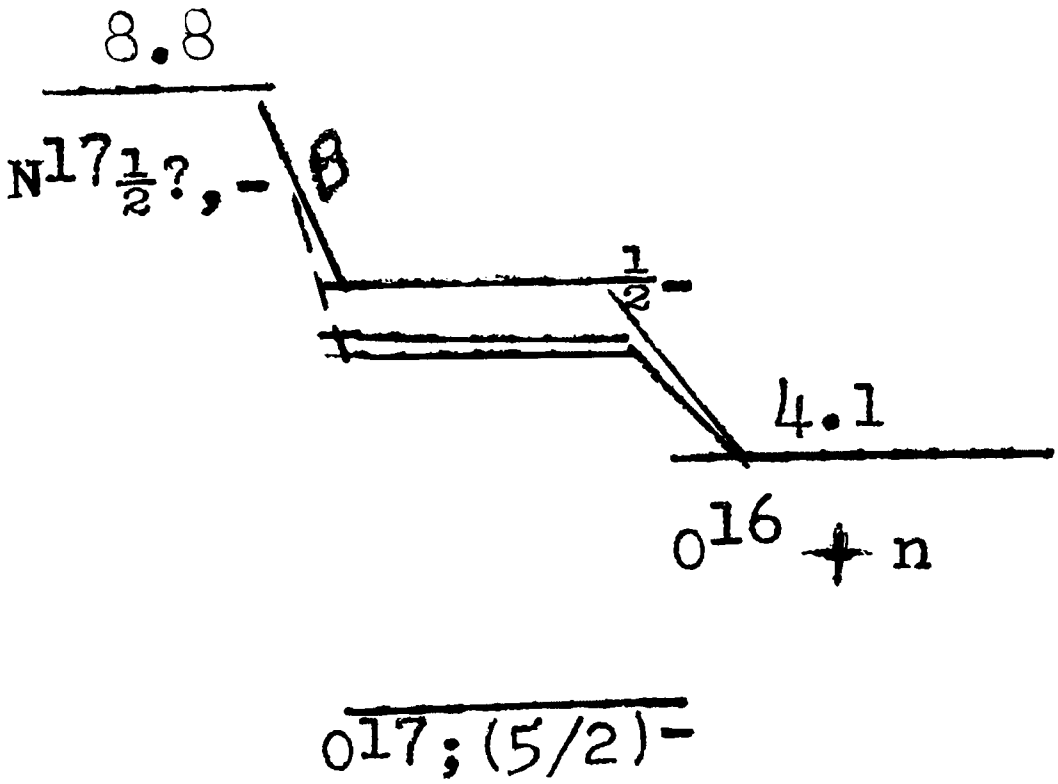

Slide 9

The β-decay energetics of N^{17}. The decay is to a level or levels around 5 Mev in O^{17}, which subsequently decays by neutron emission.

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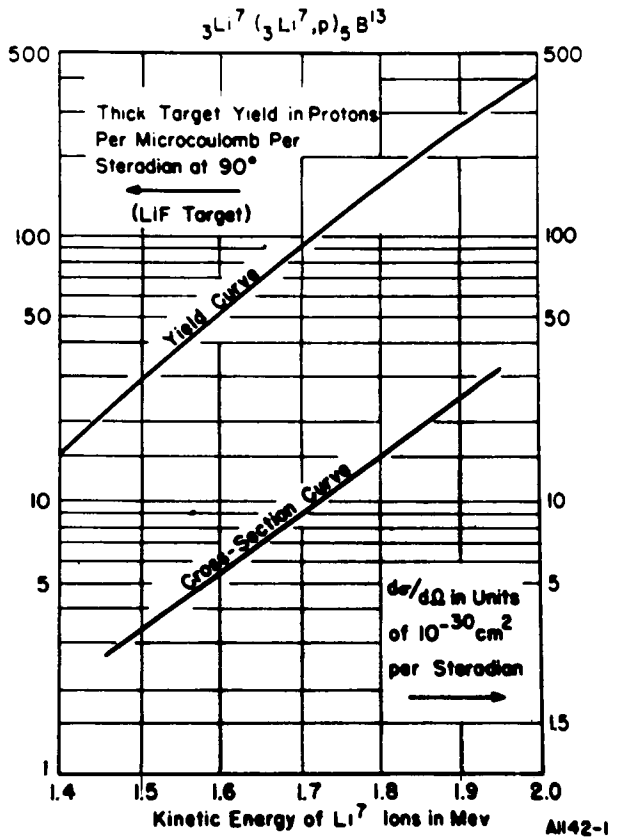

Slide 10

Yield and cross section curves for the formation of B^{13} by the reaction $Li^7(Li^7,p)B^{13}$.

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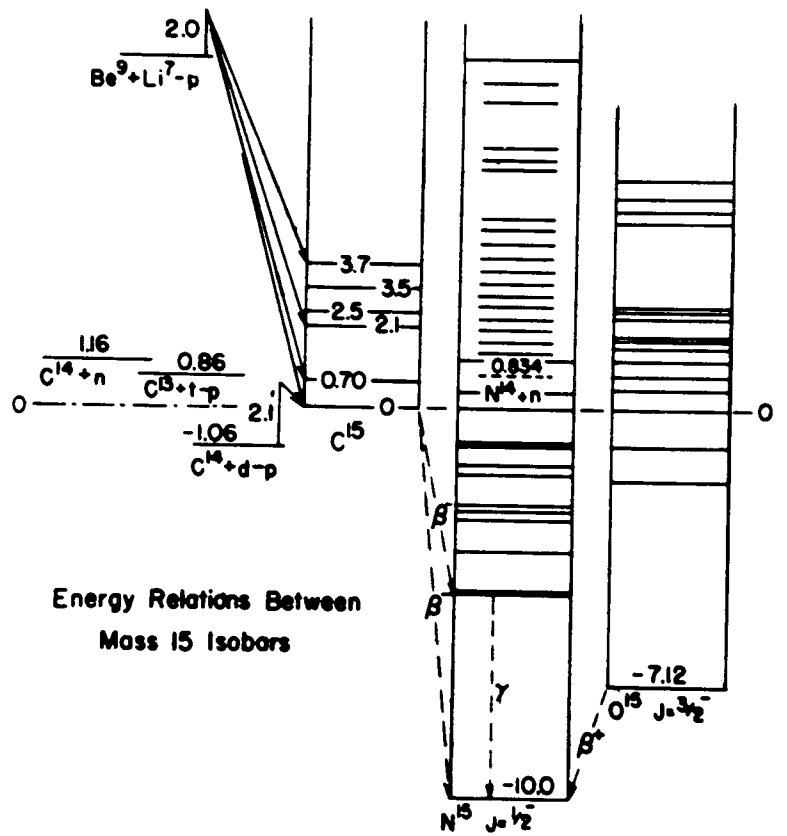

Slide 11

Energy relations among mass-15 isobars.

of this C^{15} ground state is more accurate than ours -- they have a better spectrometer -- we agree very well. There was no doubt about its being the same.

For the identification of the isobaric spin level of N^{15} we had better have Slide 12 because it is a better slide of the energy levels of C^{15} and it is matched up to N^{15}. However, in this case a shift in the experimental data has been made for the 2.68-Mev correction due to the Coulomb shift. It has also been corrected for the neutron-proton mass difference in going between the isobars. The 11.57-Mev level in N^{15} was identified by the group at Chalk River. I am merely stealing their thunder. They have pointed out that this is probably the level which is the analogous level to the ground state of C^{15}, and we have nothing to say except amen to that suggestion. They found this by the $C^{14}(d, \gamma)$ reaction which is a feasible method of producing isobaric spin 3/2 levels in N^{15}.

We would now predict that if these experiments had been extended to higher proton energies they might have found T = 3/2 corresponding to our excited C^{15} levels.

Slide 13 shows some analogies between C^{15} and O^{17}. On the extreme shell model one would expect that both these nuclei would have a neutron outside of the closed shells. Well, I don't know whether you consider this closed without its components of protons, but the configurations are quite similar in their 3/2 levels, and then there is a neutron outside. You see there is a fairly low excited state in both of these nuclei, and here you have higher lying excited states which sort of come along together.

We have not dared to assign any angular momentum characteristics to our states yet. We want to get some theoretical physicist to hold our hand before we do it.

Slide 14 shows the N^{17} situation. These excited states are all new. I believe that no one has observed any excited state in N^{17} before. The ground state was observed by L. W. Alvarez some time ago by adding the energy of the beta rays which precede the delayed neutrons to the energy of the neutrons. Adding up the energies, he got a very good figure for the ground state. I say that in a somewhat patronizing tone of voice because I do believe that his systematic error was probably greater than ours. Nevertheless we actually agree with him very well on the energy of the ground state of N^{17}. Here are some excited states of this nucleus which has

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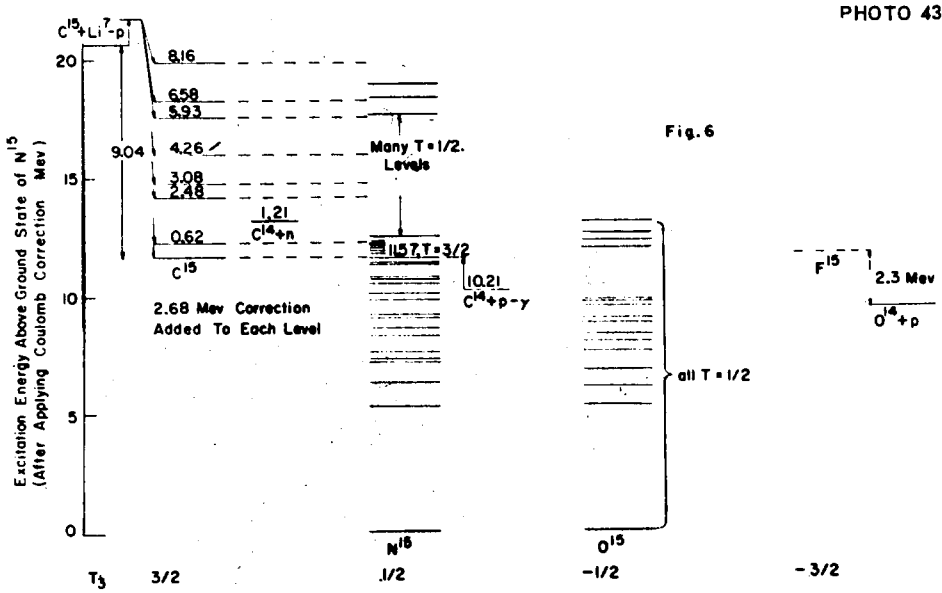

(Energy Levels of N^{15} and O^{15} from Ajzenberg and Lauritsen[7])

Slide 12

A comparison of energy levels of the mass-15 isobars.

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	(1s)	$(1p)_{3/2}$	$(1p)_{\frac{1}{2}}$	....
C^{15}	2n 2p	4n 4p	2n	n
O^{17}	2n 2p	4n 4p	2n 2p	n

O^{17}	0.87	3.07	3.85	4.56
C^{15}	0.62	2.48	3.08	4.26

Slide 13

The shell structures of the ground states of C^{15} and O^{17}. Also, the energies (in Mev) of the first four excited states of O^{17} and C^{15}.

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N^{17}

Proton Energy mev.	Range mg/cm² Al.	Q mev.	Excitation mev.
9.05*	140.6**	8.44	0
7.77	107.4	7.08	1.36
7.26	95.3	6.54	1.90
6.66	82.6	5.91	2.53
6.35	76.5	5.58	2.86
5.88	66.9	5.08	3.36
5.64	62.3	4.83	3.61

* ±0.10 ** ±2.5

Slide 14

Formation of N^{17} from the reaction $B^{11}(Li^7, p)N^{17}$. Observed proton groups and corresponding energy levels in N^{17} are tabulated. Column 1 gives the energies of the proton groups observed, Column 2 their ranges in mg/cm² aluminum, Column 3 the corresponding reaction Q-values, in Mev, and Column 4 the excitation of the corresponding energy levels in N^{17}.

unfilled shells in the shell model and might well have a fairly complicated array of excited states. We obtained these just about 10 days ago, thanks to the cooperation of the Oak Ridge people who sent us in a considerable hurry some pure B^{11} separated isotopes to make the N^{17}. We will look, of course, at the O^{17} levels and see if we can find where the isotopic spin begins, to see if we can correlate any higher ones.

Now one more thing in this connection. We can produce intermediate compound nuclei with our little lithium machine which are very highly excited; Slide 15 shows what we are driving at here. With Li^6 and Li^7 drifting together, as they do at this low velocity business, we produce some configuration of C^{13} and we can calculate its excitation which is 26.78 Mev. Then we can observe the proton groups which come from the B^{12} type of breakup. Other people have observed the same proton groups by quite different methods of producing the same intermediate nucleus at about the same energy. For instance, the experiments at MIT with 8-Mev deuterons. By the way, the intermediate nucleus probably does not figure very prominently in the deuteron reaction, since the angular distribution is more like a stripping reaction. Anyway we have plotted the energy here. Furthermore Rasmussen and some other people bombarded Be^9 with cyclotron alpha particles which produces again C^{13} in this highly excited region, and observed the protons.

The question in our minds is if we observe the relative intensities of these proton groups will they be the same or give us any clue as to whether the intermediate nucleus is an important feature of the reaction or not. Well, we have just had a sniff at the answer. All we have done is look at the proton group intensities at 90 deg. Obviously, one should not even try to make a theory until a complete angular distribution has been made but the next slide shows the situation at 90 deg.

In Slide 16 these are the proton groups from various excited states of B^{12} from C^{13}. These are the relative intensities as given by Elkin from the (d, p) reaction on B^{11}. These are the relative intensities by Rasmussen on the alpha particle beryllium bombardment. And these are ours. Well, you see the first two groups seem to be about the same. There does seem to be some difference in the protons from this level. The other observers find them much more prominent with respect to the above groups than we do. In fact, our intensities seem to run along fairly well the same but

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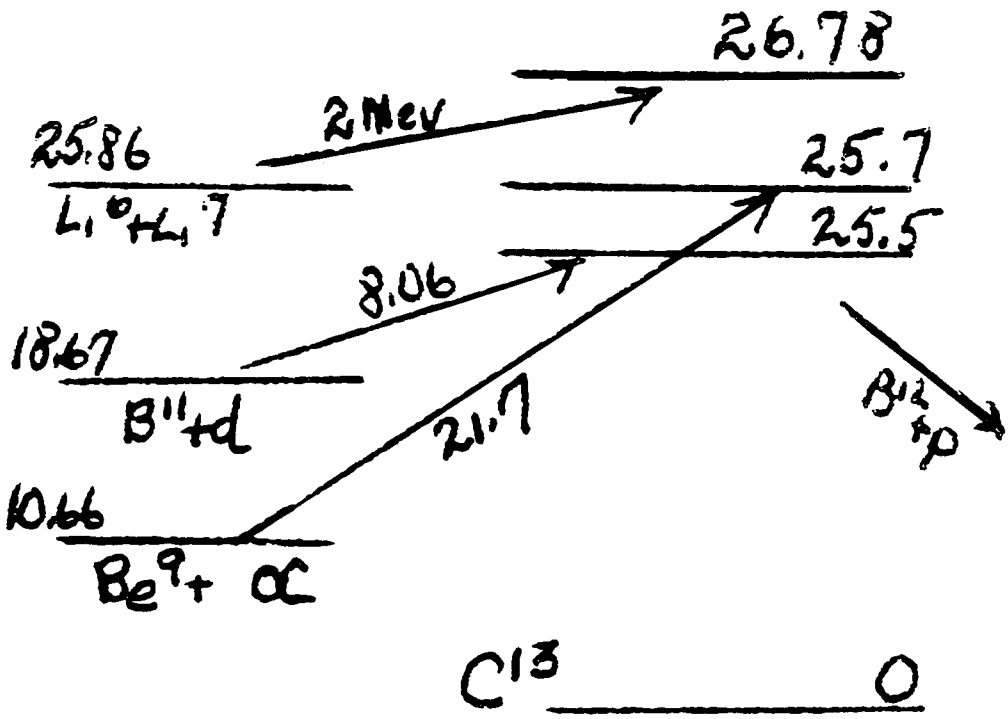

Slide 15

Excitation of the compound nucleus C^{13} to comparable energies by the reactions $Be^9 + \alpha$, $B^{11} + d$, and $Li^6 + Li^7$.

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$$(C^{13})^* \rightarrow B^{12} + p.$$

B^{12} excitation.	Rel Int. at 90° d, B^{11}	α, Be^9	Li^7, Li^6
0	(1.0)	(1.0)	(1.0)
0.95	0.78	1.2	1.25
1.67	2.85	2.0	1.01
2.62	0.69	-	} 0.92
2.72	0.04	-	
3.38	1.86	-	-
3.76	-	4.5	-
$(C^{13})^*$	25.5	25.7	26.78

Slide 16

Relative intensities in the (B^{12} + p) decay of the compound nucleus C^{13*} which is formed at comparable excitation by the reactions α + B^{11}, α + Be^9, and Li^7 + Li^6. The C^{13*} excitations are given in the last row. Yields of proton groups corresponding to various excitation states of B^{12} are given for each reaction.

here you see this very prominent group at 90 deg in Elkin's work. Now, as I said, this is just a whiff of an experiment. It has to be done carefully and angular distributions have to be studied, and then maybe we can get some real information about what goes on in the reaction.

We can also see deuterons. Other people have seen them. From the breakup of the C^{12} the deuterons would give B^{11}. They are 20 times or so as intense as these proton groups. So we have our work cut out for us.

I am going now to leave the topic of nuclear physics completely and descend rapidly through nuclear chemistry just to chemistry, and talk a little about the possibility of preparation of negative lithium ions which might be used, for instance, in a tandem Van de Graaff, the possibilities which are rather intriguing. You could start with negative lithium, strip it to triply charged lithium, and with a 2-Mev electrode you could get 8-Mev lithium ions. The question is, are there any negative lithium ions? Well, the answer is, there are but at the present time, not very many.

Slide 17 shows the parts of an equipment which was actually used in an experiment on helium and after slight modification was used to see how much negative lithium could be produced. So where this says helium just read lithium instead. The positive lithium beam was from a small accelerator, which went up to only 50 kv. With our best chemistry our artificial beta eucryptite has some sodium in it, and it produces a sodium beam which is 1/20th of the magnitude of the lithium beam. We wanted to get rid of the sodium beam, and so we have one magnetic deviation to throw the lithium positive beam where we want it. It proceeds into this region where we let in some gas. The gas cell has narrow apertures for differential pumpings. The Li^+ beam goes through it. We make a magnetic analysis here to see whether there is any negative lithium produced.

Now it is known that if you have positive hydrogen, the attachment energy is about 0.76 electron volts for producing negative hydrogen, and it is known that if you put hydrogen in for protons to pick up electrons from, you get about 2% of the beam changed into negative hydrogen. The question is what about lithium? Someone has calculated, and it is the kind of calculation one trusts, that the attachment energy of an electron to make negative lithium should be 0.54 electron volts.

Hydrogen is pretty good at making negative hydrogen out of protons. So we first tried putting hydrogen in the gas cell. The actual length of the

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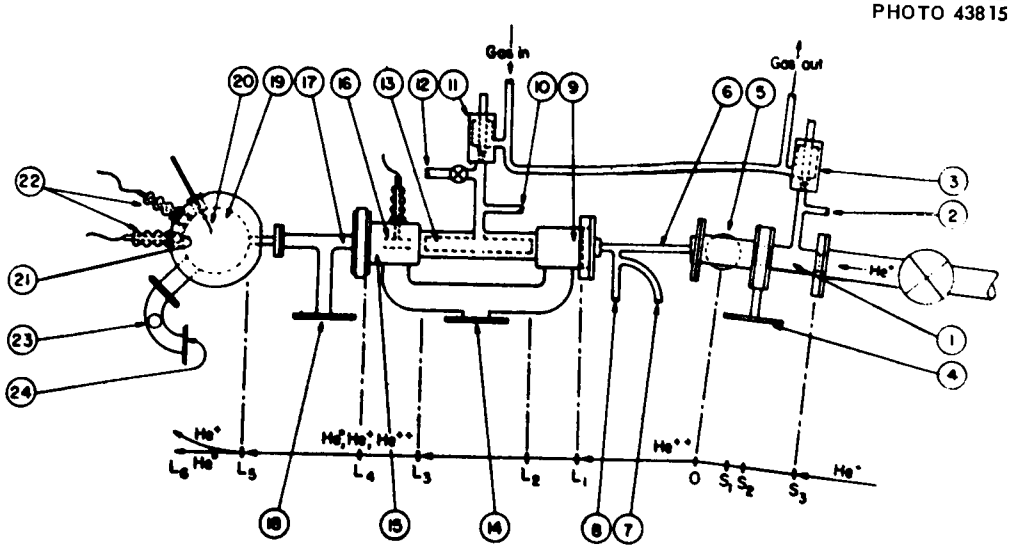

Slide 17

An apparatus for the study of charge exchange, such as was used in the negative lithium ion experiment. The beam enters at the right, undergoes a magnetic deflection at (5), passes through a gas cell at (3), and is analyzed by a magnet at (19).

cell is about 10 cm. We put 1 or 2 micron pressure of hydrogen in and analyzed for negative lithium. Well, negative lithium comes out all right; there is no doubt about it. Neutral lithium, positive lithium, and negative lithium come out if you have 25 kv energy, but in the case of hydrogen the fraction of negative lithium is probably less than 8×10^{-5} of the beam, which is quite disappointing. The fact that I say less than should not lead you to believe that I have any doubt that negative lithium is there. There is no doubt that it is there. It is there even with no hydrogen in. It is hard to get the pressure on a complicated thing like this down below 10^{-5} mm, and the negative lithium is there probably from the residual air or gas. If you put in hydrogen you get a little more but not very much; so if our experiments are correct, hydrogen is a very poor gas to use to feed electrons to lithium to make it negative.

Well, here is where the low-brow chemistry comes in. Some of our chemist friends suggested that we try something with a lower ionization potential than hydrogen. After all, the ionization potential of the H_2 molecule is about 16 volts. They recommended the oxide of nitrogen. Our stockroom had a tank of laughing gas, N_2O, and so we hooked that on and put N_2O in and the results were spectacular. We at once got a factor of about 5 increase in the yield of negative lithium.

At the present time with N_2O in and the beam energy at about 40 kv the fraction of the negative lithium is 7×10^{-4}. Thus negative lithium can be made, comparable in abundance to negative helium, probably more so, and the prospects are fairly good for using it some day in a tandem chain.

CHAIRMAN DEVONS: Thank you, Dr. Allison, for a very fascinating account of a fascinating experiment. Would anyone like to comment?

A. S. GREEN: You mentioned that you can think of many other schemes. What are they?

ALLISON: Well, try letting neutral lithium atoms evaporate from a hot filament. Employ a filament which is hot enough to emit electrons, or pass the lithium through cesium vapor, providing practically a free electron atmosphere. Many things like that.

W. E. BURCHAM: I think probably some quite nice things can be done with Li^6 which is an isotopic-spin-zero nucleus. If you bombard another similar nucleus then officially you create isotopic-spin-zero

compound states. In fact, there will be complications, almost certainly there will be impurities. Perhaps by looking at the decay states one should learn something about the mixtures.

ALLISON: Yes, thank you. We have thought in that connection about selecting C^{12} of isotopic spin zero.

A. BROMLEY: On your first slide on C^{15} you listed the assignment to the oxygen ground state as 3/2 minus. I wonder if that is a misprint or do you have evidence for this change from 1/2 minus. The next slide had 1/2 minus.

ALLISON: We have no evidence from our own experiments. Where that came from I cannot answer.

BROMLEY: Again this is partially related to Professor Burcham's comment. Have you had any opportunity to measure the relative cross sections for a given incident beam energy of say, protons, to one member of the T = 3/2 isobaric spin doublet and say neutron emission to another member, so that you could compare with the predicted cross section ratio, which would be an interesting test?

ALLISON: No, we have not done that. Particularly, with neutrons we are terrified of things. There are 20 Mev of neutrons coming off and these reactions are awfully hard to measure. Of course, that does not vitiate the question of other similar situations.

BROMLEY: We have used these in He^3 reactions and obtained remarkable agreements.

ALLISON: We should do that but we have not.

BROMLEY: In your discussion of the question of looking at the compound state decay in C^{13}, the states formed by deuterons, Li^7, and alpha bombardment, it is not clear to me how you would go about interpreting this because surely you will not be able to isolate individual levels of the compound state; phase relationships between the levels formed would depend on the method of formation to some extent and also the weighting in the angular momentum states, because with your Li^7 you would expect to preferentially populate the first angular momentum states which you cover with your beam spread.

ALLISON: I do not pretend that I know how to interpret these results theoretically. I have the instinctive feeling that the data should be taken and it will be of interest, but I am not prepared to defend a theoretical method of interpretation at the present time.

A. ZUCKER: I think the alpha particle will produce a higher angular momentum than the lithium because of very low energy.

BROMLEY: 8-Mev deuterons.

ZUCKER: But the lithiums are only equal to 2 Mev and I would expect 10-Mev alpha particles to produce 6 or 8, maybe 8ℓ values.

PORTER: Have there been any measurements of elastic scattering?

ALLISON: No, we have not done any.

E. EICHLER: Would you care to comment on the nature of the levels from oxygen-17 from which the neutron emissions would occur. Alvarez in working on N^{17} observed a distribution of the delayed neutron energies. He speculated that this meant that the level was populated by the nitrogen beta decay. Does your work give any indication of this?

ALLISON: No, we didn't make enough N^{17} to see the delayed neutrons, but your question points up my reason for claiming that his systematic error in the mass determination may be greater than ours because he did assign just the maximum of the neutron distribution energy to the maximum of the beta energy and added them up. It came out right. Why, is a bit surprising.

23. NUCLEAR REACTIONS WITH LITHIUM BEAMS

E. Norbeck
University of Minnesota

About 30 different lithium-heavy ion reactions have been studied, some of which have been published already[1-4]. Each of these has some characteristic worthy of special mention, but in 30 minutes only one minute could be allowed for each reaction. Since this is clearly not enough, we will here take the opposite extreme and consider just the reaction, $Be^9 (Li^7, Li^8) Be^8$, in considerable detail. Some of the others will be mentioned in passing.

The Li^7 beam needed for this reaction was produced by the University of Minnesota Van de Graaff accelerator. The ion source problem has already been discussed by Dr. Allison. The next problem was that of bending the high momentum lithium ions through a magnet designed for nothing heavier than deuterons. This was accomplished by passing the beam through a gas cell to strip one more electron from the singly-charged ion (Slide 1). The resulting doubly-charged Li^7 ion requires the same magnet current as a singly-charged ion of mass 1.75.

There are other products besides Li^8 produced by a Li^7 beam on Be^9. Slide 2 gives a list of the competing reactions. Enough of these have been observed by now to indicate that all of them actually occur. The very rough measurements made so far indicate that the most prolific reaction is $Be^9 (Li^7, Li^8) Be^8$, and one of the least is $Be^9 (Li^7, n) N^{15}$, with the others spaced in between arranged according to the charge and size of the particle transferred. If more careful measurements still show this effect, one would be tempted to claim that there never is a compound nucleus formed, all of the reactions proceeding by a transfer mechanism. This is a matter that calls for more study, including angular distribution measurements.

About half of these reactions produce neutrons, the energy of which runs all the way up to 20 Mev, and many of the reactions give high energy gamma rays. Any apparatus for detecting Li^8 must not be sensitive to neutrons or gamma rays over a wide energy range or to any of the other charged reaction products. Since Li^8 is a 13-Mev beta emitter with a half life of 0.84 seconds, a convenient way of distinguishing it from all the

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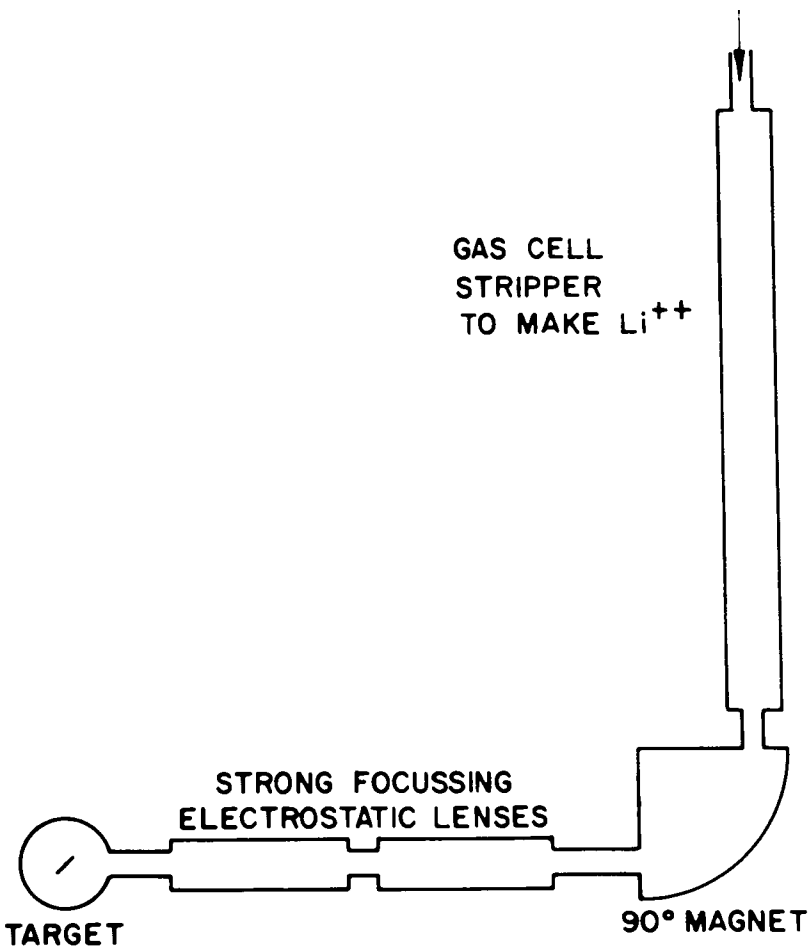

Slide 1

Beam handling equipment used with the 4-Mv Minnesota Van de Graaff showing electron stripping cell.

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$N^{15} + n$	$B^{12} + He^4$
$N^{14} + 2n$	$B^{11} + He^4 + n$
$C^{15} + p$	$Be^{10} + Li^6$
$C^{14} + d$	$Be^8 + Li^8$
$C^{14} + p + n$	$Li^8 + 2He^4$
$C^{13} + t$	$Li^7 + 2He^4 + n$
$C^{13} + d + n$	
$C^{13} + p2n$	
$C^{12} + t + n$	

$Be^9 + Li^7$ REACTIONS

(Li^7 with 3.2 Mev)

Slide 2

Energetically allowed reactions of 3.2-Mev Li^7 with a Be^9 target.

other products is to count its radioactivity after removing it from the target. Slide 3 shows the apparatus for this purpose. The Li^8 ions from the target travel down the copper pipe and stop at the end. A piece of plastic scintillating material in the form of a hollow cylinder counts the beta particles. The copper shielding pipe is thick enough to stop most of the neutrons and gamma rays from the target. The total background may be determined by placing a thin cover over the hole at the target end of the copper shield to exclude Li^8. The difference between the counting rate with and without the cover gives the amount of radioactivity deposited on the end. We will come back later to the question of other radioactive products which must be distinguished from Li^8.

The de Broglie wave length is small enough in these heavy ion reactions that one is somewhat justified in drawing pictures as in classical physics. Slide 4 shows a diagram of the Be^9 $(Li^7, Li^8)Be^8$ reaction drawn roughly to scale for bombarding energies of 3 Mev or so. It should be noted that the classical distance of closest approach is several times the nuclear radius. If this were drawn for 1 Mev, this distance would be 30×10^{-13} cm. A little calculation shows that the uncertainty in position resulting from quantum effects is small compared with the nuclear radius. One might expect that this would be a neutron transfer reaction where the loosely bound neutron in Be^9 gets picked up by the Li^7. If this were the case, the Li^8 would appear in the backward direction, since anything but a nearly head-on collision on the part of the Li^7 would leave the nuclei so far apart that no reaction would be expected. This reaction could also, in principle at least, be regarded as of the proton-transfer type, where with the same argument as before, the Be^9 would give up a proton and then go off as Li^8 in the forward direction. Of course, Coulomb barrier effects would tend to make the neutron transfer mechanism the stronger of the two.

The experimental results (Slide 5) show a $\sin^2\theta$ type of peak at 90° and a very strong, sharp backward peak. The backward peak shown here may actually be only a lower limit. The Li^8 leaves the reaction in the backward direction with only about 40 kev, so considerable amounts of it may be lost by straggling in the target. It was also found that there is no forward peak, unless it is of the order of a few percent or less of the backward peak, and that there is little change in the shape of the angular distribution with a beam energy ranging from 2.5 to 3.5 Mev.

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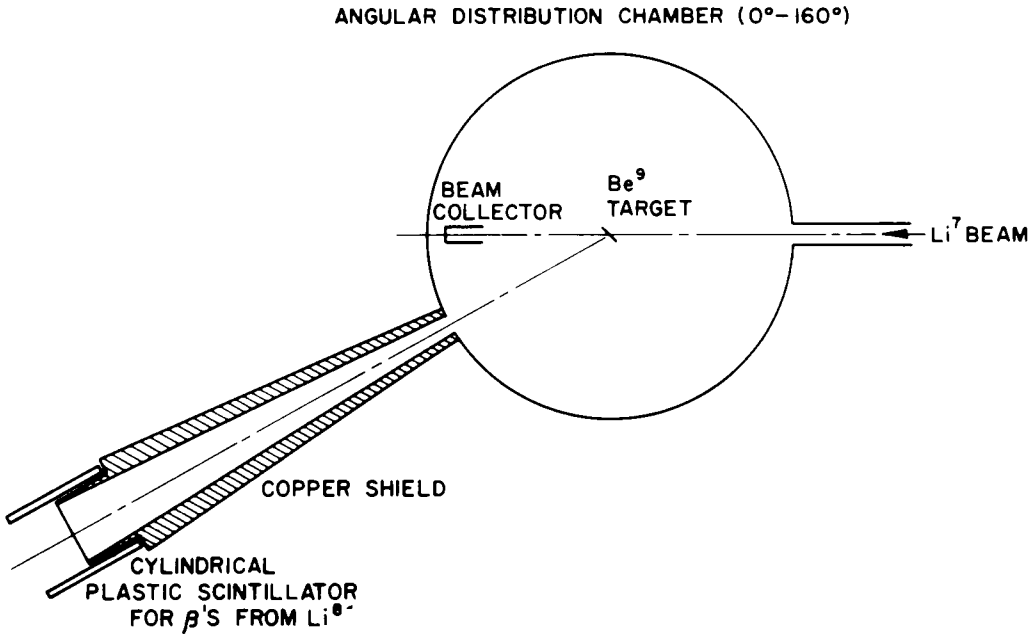

Slide 3

Apparatus for measuring the angular distribution of radioactive products from the Li^7-Be^9 reactions.

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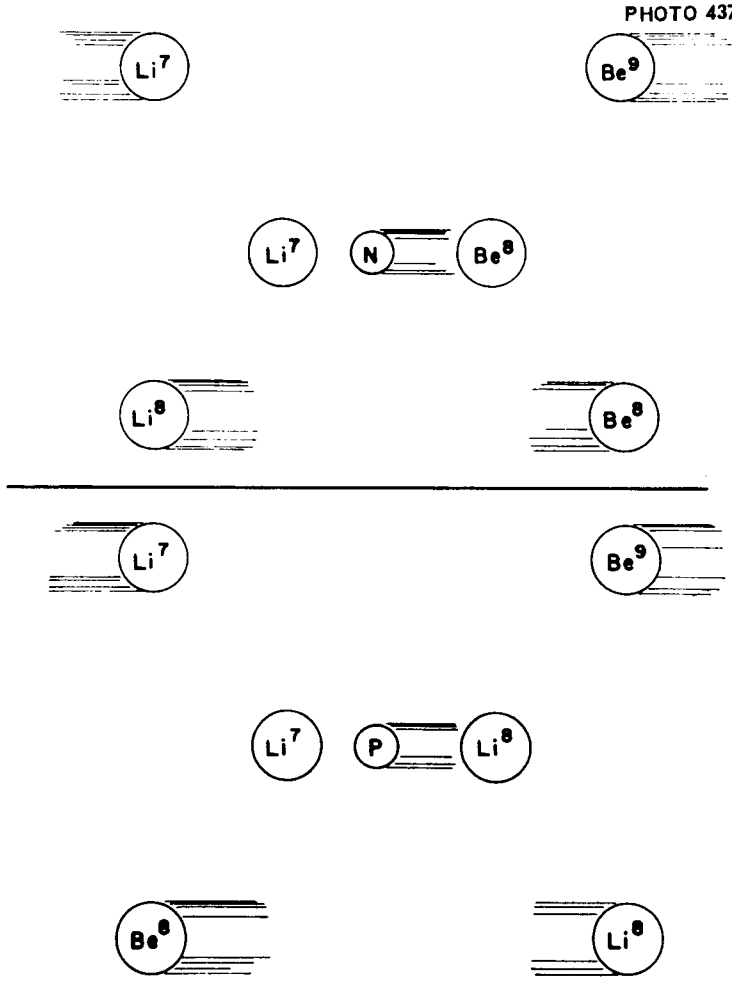
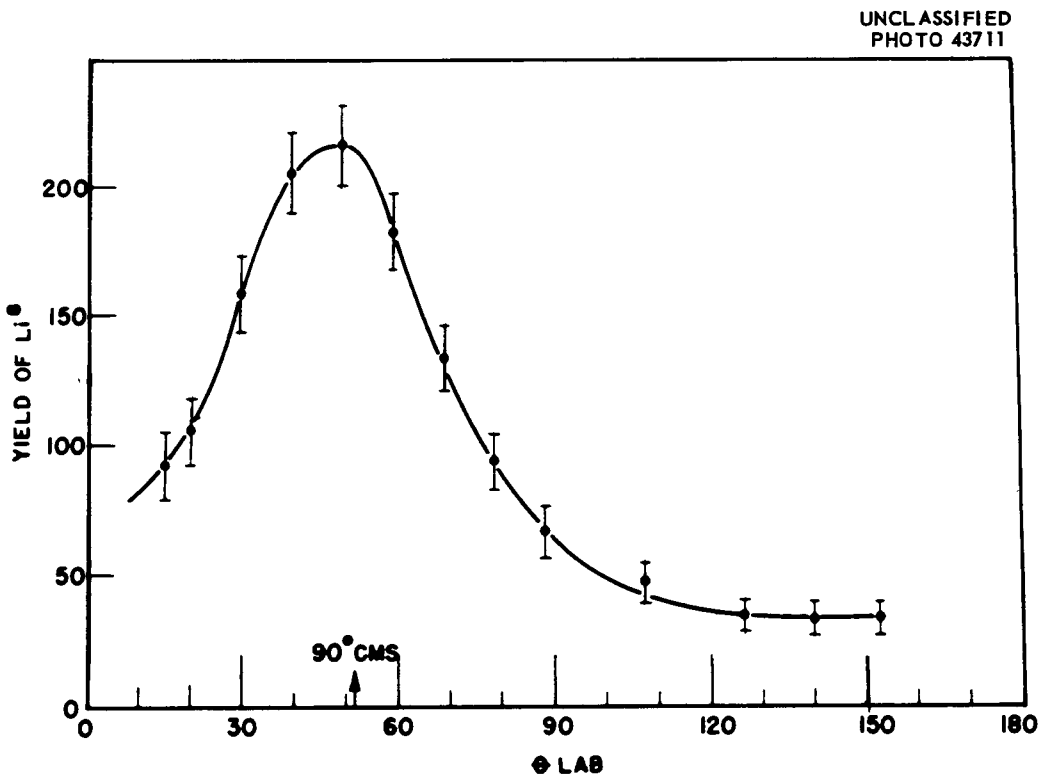


Slide 4

Diagram of the $Be^9(Li^7, Li^8)Be^8$ reaction indicating neutron transfer and proton transfer mechanisms.



Slide 5

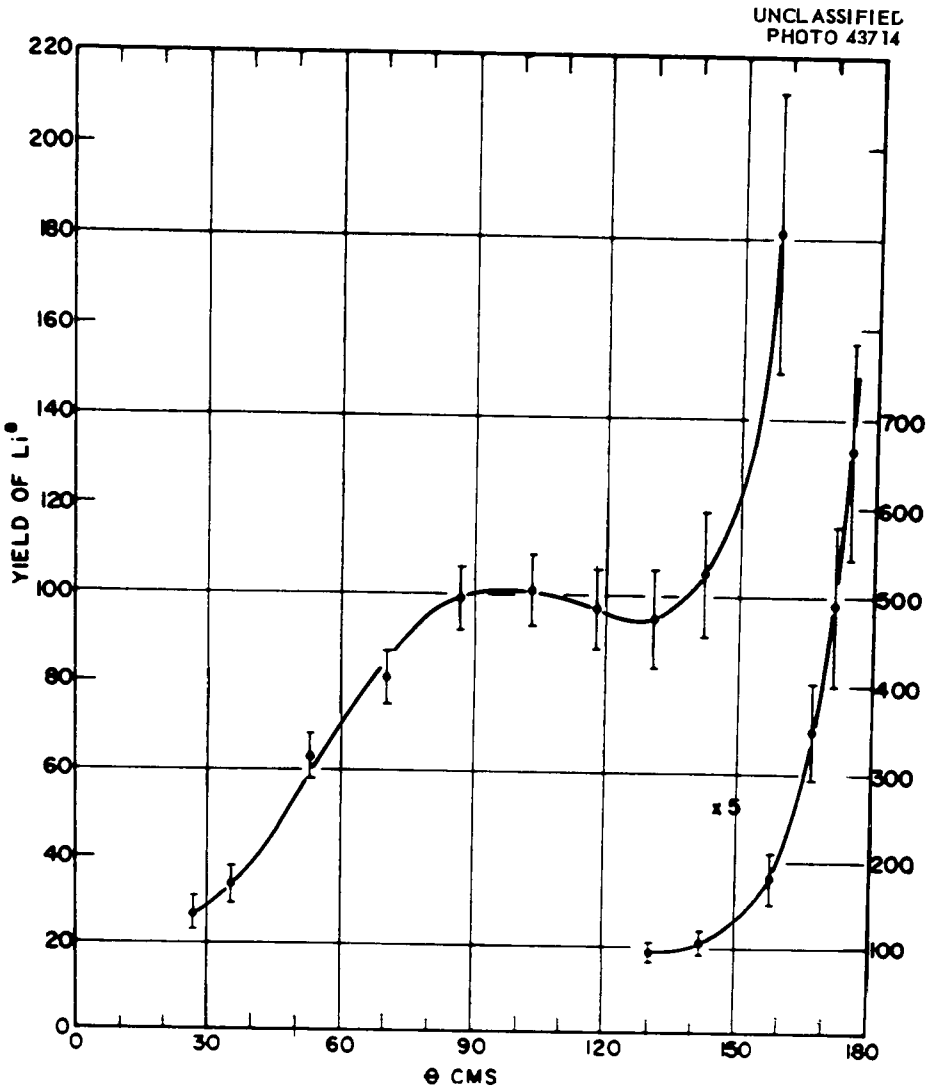
Laboratory angular distribution of Li^8 from the reaction $Be^9(Li^7, Li^8)Be^8$ with 3.25-Mev Li^7 beam.

The backward peak in Slide 6 comes from applying the laboratory-to-center-of-mass transformation to the small backward tail in Slide 5. The rather drastic effect of this transformation is the result of the low positive "Q" value (0.37 Mev) of this reaction and the nearly equal masses of the nuclei involved. The other reactions leading to radioactive products, $Be^9(Li^7, He^4)B^{12}$ + 10.5 Mev, $Be^9(Li^7,p)C^{15}$ + 9.1 Mev, and $Be^9(Li^7H^4)C^{12}$ + 3.2 Mev, have rather mild transformations so that a little of these products in the backward direction might be mistaken for a large yield of Li^8. The total cross section for producing B^{12} is more than an order of magnitude less than that for Li^8, but this may still be enough to show up at the backward angles. A fire which damaged the Minnesota Van de Graaff last February prevented us from measuring the lifetime of the radioactive products. This measurement would have easily distinguished between $Li^8(t_{1/2} = 0.84$ sec) and $B^{12}(t_{1/2} = 0.018$ sec).

From other work[1,2] it is known that the production of C^{15} is considerably less than that of Li^8, also it has a lower beta decay energy, about 9 Mev, and so is cut down somewhat by our counting system which sees only that part of the beta spectrum with energy greater than 5 Mev. When the differential cross section for the production of protons ($Be^9(Li^7,p)C^{15}$, is measured over all angles, the angular distribution for C^{15} will be a known small background which can be subtracted from the Li^8 yield.

If H^4 should be bound and have a beta half life close to that of Li^8, all of the measurements of Li^8 production by lithium reactions will be open to question. When the Van de Graaff operates again, one of the first experiments will be to put a foil in the path of the radioactive products leaving the target so that Li^8 and B^{12} are stopped but so that the relatively long-range H^4 is not.

It does not seem likely that H^4 is bound, however. There is another lithium reaction which is particularly well adapted to the study of this question. This is $B^{10}(Li^7,tn)N^{13}$ - 1.40 Mev. If the threshold for the production of N^{13} occurs at a lower Li^7 beam energy than indicated by the above reaction, the neutron must be bound to the triton as H^4 (see Slide 7). N^{13} is a well known 10-min positron emitter so that its presence of absence can be easily verified. With a thick B^{10} target and a bombarding energy just 162 kev (center of mass) below the threshold for the tn reaction, no N^{13} was observed [1]. This means that less than one H^4 could have been



Slide 6

Center of mass angular distribution of Li^8 from the reaction $Be^9(Li^7, Li^8)Be^8$ with 3.25-Mev Li^7 beam.

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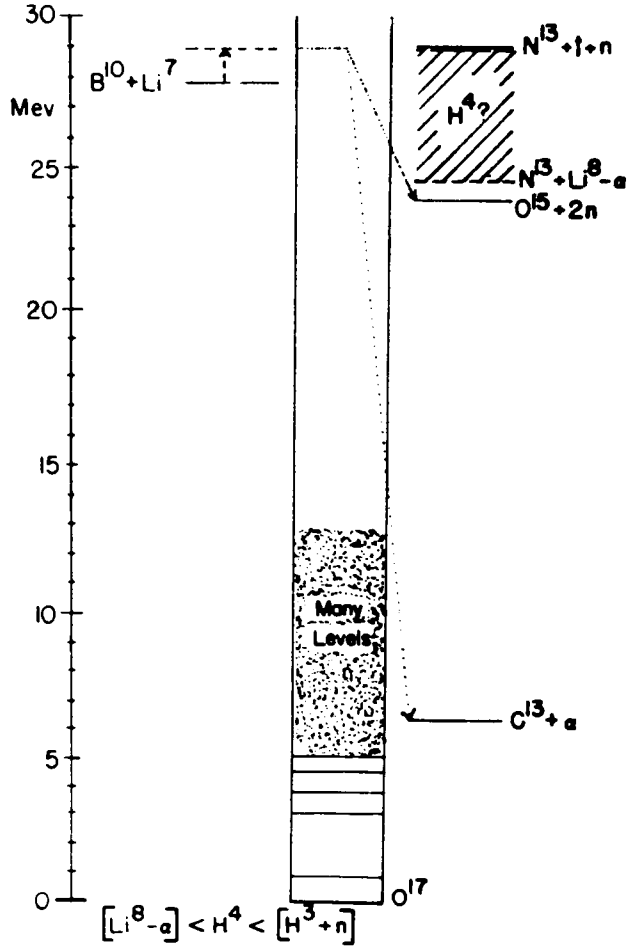

Slide 7
Energy diagram showing how the mass range allowed for H^4 has been narrowed by the $B^{10}(Li^7, H^4)N^{13}$ reaction.

formed per 3×10^{13} incident lithium ions. Even though H^4 could, in principle, be bound by as much as 4.50 Mev before Li^8 would break up spontaneously into H^4 and He^4, the above experiment indicates that H^4 is not bound by more than a few hundred kev, if at all.

The apparatus Murphy used to look at protons from the $Be^9(Li^7, p)C^{15}$ reaction[2] was suitable for looking at H^4 from $Be^9(Li^7, H^4)B^{12}$. He is a careful worker, and I am sure he did not pass up the opportunity to see if he could find H^4. It should certainly be surprising also if no one had ever looked for the 20-Mev beta particles that would be expected to follow the reaction $t + n \rightarrow H^4$. There is a little more evidence of a negative sort in the literature, but I will not try to review it here.

The lithium reactions are particularly well suited for the production of neutron-rich isotopes. B^{13} is a rather new isotope made so far only with lithium reactions. H^4 will be made if it is bound. Some day we hope to produce C^{16} and O^{20} which we feel are probably stable to heavy particle emission and can be formed by the appropriate lithium reaction.

As we have already seen, there is a great variety of experiments that may be done with lithium beams. Of the many things yet to be done, let me discuss one which has already been suggested at this conference. Li^6 is particularly well suited for studying the isotopic spin of levels in certain nuclei. A good example of this is the reaction, $B^{10}(Li^6, He^4)C^{12} + 23.7$ Mev. Since B^{10}, Li^6, and He^4 all have isotopic spin zero ($T = 0$), the C^{12} must also have $T = 0$. Since the angular momentum is 3 for B^{10} and 1 for Li^6, with as much as three or four units of angular momentum carried in by the Li^6, one would expect to obtain a complete set of C^{12} energy levels with $T = 0$ up to about 25 Mev. The levels found with other reactions but not with this lithium reaction would be $T = 1$ levels. Any level found with this reaction but not with the other reactions would probably be a $T = 0$ level of rather high angular momentum.

CHAIRMAN DEVONS: Thank you Dr. Norbeck. Any comments on this work and the things that Dr. Norbeck has discussed?

A. ZUCKER: I have several questions. First, how do you identify the Li^8 when you see beta counts and there are lots of other things that are emitters also? Do you see them too?

NORBECK: Well, in this particular reaction the only beta emitters that we would encounter would be Li^8, B^{12}, and H^4.

ZUCKER: Well, the compound nucleus is N^{16}. Is that right?

NORBECK: Yes.

ZUCKER: You could get N^{13}?

NORBECK: No, because it is energetically forbidden. Another consideration is the energy of the beta particle. A 13-Mev beta particle will go a long way, but an electron under 5 Mev just never gets to the counter. Also we pulse height analyze the signal that is picked up from the plastic crystal.

S. K. ALLISON: Dr. Zucker is talking about 27 Mev. You see we only have a little energy and a lot of the things that you would think of as coming off don't come off in our reactions because the beam energy isn't big enough.

ZUCKER: Do you assume that that is a transfer reaction and are all your kinematics calculated on that basis? It is not at all obvious why it should not be an evaporation of alpha particles which would change the kinematics completely.

NORBECK: We have considered the question of its being a three-body decay, Li^8 plus two alpha particles. In this case there would be absolutely nothing in the backward direction, because what little energy there is to kick the Li^8 back would be dissipated in the three-body decay. Also because of the small energy involved, any Li^8 appearing in the backward direction must be in the ground state. In general unlike with the heavier ion work, we know what the final states are because they are fairly widely separated with these lighter nuclei, and our beam energies are low and precisely controlled.

J. S. BLAIR: I would like to raise the question whether in these reactions one does bring any angular momentum in, and I should like to make the point that as long as one is below the Coulomb barrier, the probability of interaction is strongly regulated by the barrier, not only the Coulomb barrier but the centrifugal barrier that must be added to this. Now it is true that the centrifugal barriers are small but so is the energy that you have and one has the feeling that these barrier considerations are exponential and therefore might magnify the effect.

For example, I made some rough calculations which may or may not be accurate to factors of 2 for the $Li^6 + B^{10}$ example which Dr. Norbeck talked about. The centrifugal barrier, $\frac{\hbar^2 l(l+1)}{2Mr^2}$ with r evaluated at,

let's say, a reasonable radius where one might expect things to start to happen--6 fermis, for L equal to 1 is 0.3 Mev, and for L equal to 2, it is 0.9 Mev. In the center-of-mass system with the 2-Mev lithium beam you only have a little more than one Mev available energy. So, I should think that one would favor S-waves in such a process, and I wonder if one could even say that in these reactions one is bringing in very little angular momentum, and one might have very strong angular momentum selection rules.

In this $B^{10}(Li^6, He^4)C^{12}$ reaction, carbon^{12} plus helium is zero, B^{10} is 3 and Li^6 is 1. One might find that the transition to the ground state is strongly inhibited just by angular momentum considerations and this might actually be a way of looking at the angular momentum in excited states of C^{12}, putting on some restrictions.

ALLISON: This point is discussed in Murphy's paper. This is the paper about C^{15} from $Be^9 + Li^7$. Murphy made a calculation of the cross section as affected by, L using the wellknown formula, $\sigma_\ell = (2\ell+1)\pi\lambda^2 t_\ell$, where t_ℓ is the barrier penetration function from the calculations of Feshbach, Shapiro and Weisskopf. He found that with the conditions of his experiment with 2-Mev lithium he could predict that the cross section for the L = 1 wave was actually higher than for L = 2. The actual numbers are 1.7×10^{-28} for L = 0, 2.9×10^{-28} for L = 1, and 1.2×10^{-28} for L = 2. These are the capture cross sections in cm^2. So at least if he didn't make a mistake in his calculations there certainly was a high probability of L = 1 interaction there.

BLAIR: I withdraw my comment then if this calculation has been done.

NORBECK: On this angular momentum question we have done a little bit of theoretical computation, however, it is not worth going into in much detail, but just considering classical orbits and very simple formulas for neutron tunneling, and so on, if you look at the ratio of neutron tunneling for L = 1, 2, 3, and 4 states, it turns out that relative to L = 1 you still expect to see something as far out as L = 4.

It is a little hard from the semi-classical approach to know quite what to do with the L = 0 case, because classically the beam actually stops at the point of closest approach. But using such considerations, we have speculated that this 90-deg peak is the result of higher angular momentum and the 180-deg peak is the L = 0 contribution. With this reaction the

closest approach between the nuclei with about 3 or 4 Mev beam energy is about 10 fermis. So compound nucleus formation seems very unlikely.

B. L. COHEN: If you had L = 0 particles you would have to get an isotropic distribution; so you must have fairly high angular momentum coming in there.

NORBECK: I am not so sure that you do have to get an isotropic angular momentum distribution when you don't have compound nucleus formation.

COHEN: I'll bet anyone here.

A. BROMLEY: To support Professor Allison, if you use the helium functions and work out the penetrability this way on some of the light element reactions of this sort you will find that the penetrability for the P-waves is actually higher than for the S-waves.

Your claim for this being a transfer mechanism, seems to come from this large backward peak which I guess follows transformation of the tail. Is that correct?

NORBECK: No, the main evidence as far as we are concerned is the fact that this reaction is much more prolific than any other reaction.

BROMLEY: Just for the amusement value if you considered the suggestion that it is actually evaporation of two alpha particles, then your kinematics would not be such that you would get this exaggeration of the backward peak. You would have something more resembling this 90-deg peak which begins to look very much more like a compound nucleus. You could get the boil-off, and I think the thing would be more or less consistent there. Is this a possibility?

NORBECK: This we should look into, but if we were getting almost entirely alpha particle boil-off, we would expect to see nothing whatever in the backward direction in the laboratory.

BROMLEY: Yes, but surely there is a background somewhere in your experiment.

NORBECK: Well, this is assuming that we have eliminated background. If we ever get the machine running again I am sure that we will eliminate any background problems.

CHAIRMAN DEVONS: I think the best contribution to this discussion can be made by repairing the machine.

BREIT: There might be perhaps the possibility of the ground state of Be^8 for the 180-deg peak and the L = 2 for the other one. The L = 2 really means two alphas.

CHAIRMAN DEVONS: I think from what we have heard this morning it is quite obvious that very soon there will be a chance to have a conference dealing with lithium ions only.

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2. P. G. Murphy, Phys. Rev. 108, 421 (1957)
3. E. Norbeck, Jr., Phys. Rev. 105, 204 (1957)
4. Allison, Murphy, and Norbeck, Phys. Rev. 102, 1182 (1956)

24. SECOND ORDER EFFECTS IN COULOMB EXCITATION

R. L. Gluckstern
Yale University

The work which I am going to describe is primarily due to Professor Breit, Drs. James Russell, John Lazarus and myself.[1-4] I will also quote some material from the excellent review paper of the Copenhagen group, K. Alder, A. Bohr, T. Huus, B. Mottelson and A. Winther.[5] Much of what I will say will soon appear in the Handbuch der Physik in an article by Professor Breit and myself.

First order Coulomb excitation calculations have been performed by several groups.[6] The main conclusions from such work are that it is possible by observation of gamma ray yield, angular distribution, inelastic scattering, and various other quantities to make inferences regarding the spins and energies of excited levels of nuclei. In addition one can obtain transition multipole matrix elements between the ground state and the first excited state. These matrix elements are unusually large and give evidence for the collective nature of the nuclear oscillations of these states.

The use of heavy ions represents an improvement over protons and alpha particles because the cross sections for Coulomb excitation are considerably larger. To first order in the excitation they vary as the square of the incident nuclear charge. There are other advantages which have been pointed out by many people.

One advantage in considering heavy ions is that the classical picture is a reasonably reliable one. This comes about because the validity of the classical calculation depends on the magnitude of the quantity η, or $\underline{n}$ as it has been called previously at this meeting, compared with unity. This quantity is defined as:

$$\eta = (Z_1 Z_2 e^2)/(hv) \qquad (1)$$

In the case of heavy ions η is quite large, sometimes exceeding 50 for typical experiments in Coulomb excitation.

In talking about collisions I will use the classical picture of a projectile in a hyperbolic orbit. The calculation which one ordinarily does in this semi-classical approximation is one in which such a projectile causes a time-dependent interaction with the nucleus. This interaction is responsible for the excitation of the nucleus, and is usually an electric quadrupole interaction.

If one now considers the collision in which the projectile moves in the hyperbolic orbit one sees fairly readily that the probability of further excitation in a collision which is close enough to cause appreciable Coulomb excitation is fairly sizeable. The parameter which relates the second order amplitude to the first order amplitude is a quantity λ given in:

$$\lambda = \frac{Z_1 e^2 \langle r^2 \rangle_{ff'}}{7 \hbar v a^2} \qquad (2)$$

This quantity which measures the relative size of the second and first order effects can be as large as 1 for an ion as heavy as neon for an assumed effective quadrupole moment of the order of $10^{-24} cm^2$. This means that in such situations where one expects a sizeable effect due to second order excitation, one should calculate third and higher order excitations as well, but we will here confine our attention to the second order excitation.

The usefulness of such considerations lies primarily in the fact that one can get transition matrix elements between excited states of nuclei. There are several cases in which one can see how this might come about. One particular application which I will describe in more detail in a moment is one which involves excitation between sublevels of a given state, which then allows calculation of the static quadrupole matrix element of this excited state.

Slide 1 illustrates some of the possible second order effects which might be seen. On the extreme left is a second order sequence in which an even-even nucleus of spin zero is excited first to a state of spin 2 and then to a higher state of spin 2 (2'). One can make an estimate along the lines described by the Copenhagen group, which is that ratio of the cross section for cascade excitation to that for direct E2 excitation

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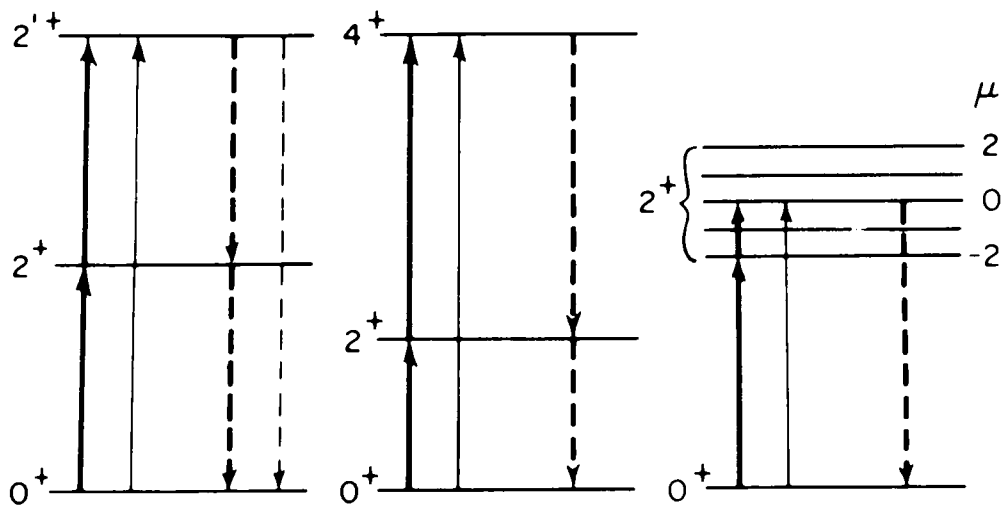
Possible Modes of Second Order Excitation



Slide 1

is of the order:

$$\frac{\sigma_{o \to 2 \to 2'}}{\sigma_{o \to 2'}} \simeq 0.02 \left(\frac{Z_1 e^2}{\hbar v}\right)^2 \frac{B_{02}\, B_{22'}}{a'^4\, B_{02}} \qquad (3)$$

The form of (3) is not surprising since the second order interaction which is responsible for cascade excitation leads to the product of the two B's in the numerator. McGowan and Stelson[7] have seen the $0 \to 2'$ excitation in a few cases and from the branching ratio of the decay gamma radiation have calculated the cascade matrix element, on the assumption that excitation by protons is $0 \to 2'$ rather than $0 \to 2 \to 2'$. For heavy ions, however, cascade excitation will be comparable with cross-over excitation. As a result one might see both processes in such a way as to lead to the appropriate values of the B's without reliance on internal conversion coefficients.

Another possible second order excitation is the cascade excitation of higher rotational levels shown in the center in Slide 1 for $0 \to 2 \to 4$ transition. In all probability the $0 \to 2 \to 4$ cross section will be much larger than the direct $0 \to 4$ cross section, unless B_{04} (E4) is even further enhanced over single particle values than B_{02} (E2) and B_{24}(E2).

What I would like to talk most about is the second order process shown on the right in Slide 1, which comes about by the reorientation of the nuclear spins that exist in a collision. This means a projectile comes in and causes the nucleus to go from its ground state to one of the five sublevels of a spin-2 state; a second excitation then causes a change from one sublevel to another. In the slide these sublevels spread apart, but in the calculation this spread will be assumed to be negligible compared with the separation between the ground state and the spin-2 state.

The calculation of reorientation is being described here mainly as an illustration of the kind of things that are involved in second order calculations. In particular the reorientation is being calculated for transitions from spin zero to 2 and can be generalized without much trouble to other spins. The interaction is that part of the electric interaction which has a quadrupole behavior, and the calculation is performed

with the nuclear wave functions considered on a single particle basis. This does not mean that the calculation neglects the collective nature of the nuclear oscillations. It is just a simple way of doing a calculation where at the end one replaces the matrix element of the square of the displacement for the single proton which is causing the transition matrix element by the appropriate nuclear matrix element.

Slides 2, 3, 4 show the essential steps of the semi-classical calculation, using time dependent perturbation theory. The cross section for Coulomb excitation is proportional to the sum of the absolute squares of the C_μ which then is directly expressed in terms of the sum of the absolute squares of the S_μ. These S_μ are modifications of the $S_\mu^{(2)}$ given in Slide 4 for the first order calculation. The effect of inclusion of a second order interaction can be seen to give rise to a modification of the first order probability amplitude $C_\mu^{(1)}$ proportional to the product of nuclear matrix elements shown at the bottom of Slide 3. As a result, the second order interaction leads to an interference with the first order amplitudes and both cross sections and angular distributions may be expressed as modifications of the first order results, proportional to η in Eq. (2).

The coordinate system for the calculation of the photon angular distribution is shown in Slide 5. This calculation proceeds along standard lines, and the results may be expressed as in Slide 6. Here the quantities D, N_2, N_4 are the coefficients of the angular dependent terms in a representation of the angular distributions in terms of the Legendre polynomials 1, $P_2(\cos\theta')$, $P_4(\cos\theta')$. The representation of the photon angular distribution in terms of the S_μ is shown in Slide 7.

Sample numerical values for reorientation are presented in Slides 8 and 9. The quantity D enters not only in the photon angular distribution but also as a quantity proportional to the inelastic scattering cross section of the projectile. Here the quantity $\mathcal{E}$, which is the eccentricity of the hyperbolic orbit, is related to the projectile scattering angle by

$$\mathrm{Sin}(\theta_s/2) = (1/\mathcal{E}) \tag{4}$$

The parameter ξ is here defined as

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$$H'(t) = Z_1 e^2 (r_p^2 / r_t^3) P_2 (\cos \theta_{pt})$$

$$w_\mu (\vec{r}_p) = R_f (r_p) Y_{2\mu} (\theta_p, \varphi_p)$$

$$v_o (\vec{r}_p) = R_i (r_p) Y_{oo} (\theta_p, \varphi_p)$$

$$\int_o^\infty R^2 (r_p) r_p^2 \, dr_p = 1$$

$$\psi = c_o v_o + \Sigma_\mu c_\mu w_\mu$$

$$c_o^{(o)} = \exp (-i E_o t / \hbar)$$

Slide 2

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$$c_\mu(\infty) = c_\mu^{(1)}(\infty) - \hbar^{-2}\exp(-i\omega_f t)\int_{-\infty}^{+\infty} dt'' \int_{t''}^{\infty} dt' \sum_{\mu'} H'_{\mu\mu'}(t') H'_{\mu' i}(t'') \exp(i\omega_{fi} t'')$$

$$\omega_f = E_f/\hbar\,, \quad \omega_i = E_i/\hbar, \quad \omega_{fi} = E_{fi}/\hbar$$

$$\sum_{\mu'} H'_{\mu\mu'}(t)\, H'_{\mu' i}(t') = \frac{5}{(4\pi)^{3/2}}\, A\, I_\mu / [\, r^3(t)\, r^3(t')]$$

$$A = Z_1^2\, e^4 \left\{ \int_0^\infty r_p^4\, R_f^2(r_p)\, dr_p \right\} \left\{ \int_0^\infty r_p^4 R_f(r_p)\, R_i(r_p)\, dr_p \right\}$$

Slide 3

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$$c_\mu^{(1)}(\infty) = \frac{i\, Z_1\, e^2}{2\sqrt{5}\,\hbar\, a'^2\, v} \langle r^2 \rangle_{fi} \exp(-i\omega_f t)\, S_\mu^{(2)}$$

$$S_{-2}^{(2)} = -\left(\frac{3}{2}\right)^{\frac{1}{2}} \int_{-\infty}^{+\infty} e^{i\xi(w+\varepsilon\, \mathrm{sh}w)} \frac{[\varepsilon + \mathrm{ch}w \mp i\,(\varepsilon^2-1)^{\frac{1}{2}}\, \mathrm{sh}w]^2}{(1+\varepsilon\, \mathrm{ch}w)^4}\, dw$$

$$S_0^{(2)} = \int_{-\infty}^{+\infty} \frac{e^{i\xi(w+\varepsilon\, \mathrm{sh}w)}}{(1+\varepsilon\, \mathrm{ch}w)^2}\, dw$$

Slide 4

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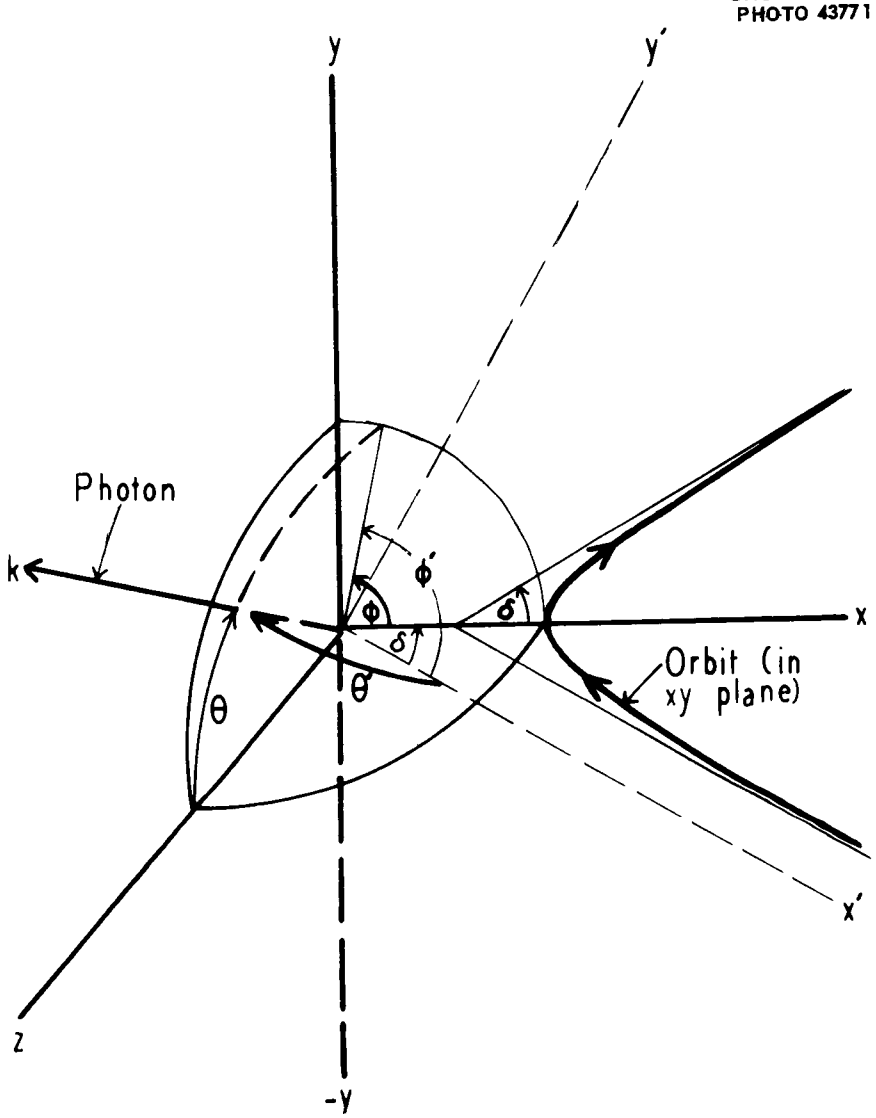

Slide 5

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$$\langle J \rangle = \langle J^{(1)} \rangle + \langle J^{(2)} \rangle +$$

$$\langle J \rangle = 4 \, \mathrm{Re} \left\{ D + \delta D + \frac{5}{14} (N_2 + \delta N_2) P_2 + \frac{8}{7} (N_4 + \delta N_4) P_4 \right\}$$

$$a_s = \frac{N_s + \delta N_s}{D + \delta D} = \frac{N_s}{D} + \frac{\delta N_s}{D} - \frac{N_s \delta D}{D^2}, \qquad (s = 2, 4)$$

$$\lambda = \frac{Z_1 e^2 \langle r^2 \rangle_{ff}}{7 \hbar a'^2 v}$$

Slide 6

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$$\langle J \rangle = \mathrm{Re}\Big\{ |S_2|^2 + |S_0|^2 + |S_{-2}|^2 + \frac{5}{14}\big[|S_2|^2 - |S_0|^2 + |S_{-2}|^2 -$$

$$-\sqrt{6}\, S_0^* \,(S_2\, e^{-2i\delta} + S_{-2}\, e^{2i\delta})\big]\, P_2 - \frac{3}{28}\big[|S_2|^2 + 6\,|S_0|^2 + |S_{-2}|^2 -$$

$$- \frac{10}{3}\sqrt{6}\, S_0^* \,(S_2\, e^{-2i\delta} + S_{-2}\, e^{2i\delta}) + \frac{70}{3}\, S_2\, S_{-2}^*\, e^{-4i\delta}\big]\, P_4 \Big\}$$

$$\delta = \tan^{-1} (\varepsilon^2 - 1)^{\frac{1}{2}}$$

Slide 7

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Values of the coefficients of the angular dependent terms available in a limited coincidence experiment which defines only the eccentricity of the orbit of the incident particle in coincidence with the γ- ray.

ξ	ϵ	$100\,(D+\delta D)$	$100(N_2+\delta N_2)$	$100(N_4+\delta N_4)$
0.0000	1.000	$44.4 - 99\xi\lambda$	$88.7 - 198\xi\lambda$	$-66.5 + 148\xi\lambda$
	1.064	$35.4 - 67\xi\lambda$	$58.2 - 104\xi\lambda$	$-25.8 + 25\xi\lambda$
	1.414	$13.0 - 10.2\xi\lambda$	$3.75 - 15.0\xi\lambda$	$5.77 - 12.4\xi\lambda$
	2.000	$3.82 - 0.37\xi\lambda$	$-2.96 - 5.62\xi\lambda$	$1.00 + 1.44\xi\lambda$
0.1915	1.000	$31.6 - 12.0\lambda$	$63.2 - 24.0$	$-47.4 + 18.0\lambda$
	1.064	$26.1 - 8.5\lambda$	$42.6 - 13.1\lambda$	$-18.5 + 3.5\lambda$
	1.414	$10.7 - 1.72\lambda$	$4.7 - 1.72\lambda$	$3.9 - 1.33\lambda$
	2.000	$3.47 - 0.242\lambda$	$-0.90 - 0.510\lambda$	$0.70 + 0.106\lambda$
0.4028	1.000	$15.5 - 11.0\lambda$	$31.1 - 22.1\lambda$	$-23.3 + 16.6\lambda$
	1.064	$13.5 - 8.1\lambda$	$21.8 - 12.5\lambda$	$-9.1 + 3.4\lambda$
	1.414	$6.39 - 2.03\lambda$	$3.81 - 1.63\lambda$	$1.60 - 1.10\lambda$
	2.000	$2.14 - 0.347\lambda$	$0.37 - 0.389\lambda$	$0.26 + 0.057\lambda$
0.6882	1.000	$4.63 - 4.8\lambda$	$9.26 - 9.63\lambda$	$-6.95 + 7.21\lambda$
	1.064	$4.29 - 3.7\lambda$	$6.84 - 5.62\lambda$	$-2.71 + 1.54\lambda$
	1.414	$2.27 - 1.06\lambda$	$1.64 - 0.73\lambda$	$0.34 - 0.43\lambda$
	2.000	$0.718 - 0.183\lambda$	$0.327 - 0.149\lambda$	$0.024 + 0.0154\lambda$

Slide 8

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Values of the coefficients of the angular dependent terms available from observation of the γ-rays alone

ξ	$100\int_1^\infty (D+\delta D)\epsilon\, d\epsilon$	$100\int_1^\infty (N_2+\delta N_2)\epsilon\, d\epsilon$	$100\int_1^\infty (N_4+\delta N_4)\,\epsilon\, d\epsilon$
0.0000	$28.4 - 21.\xi\lambda$	$-1.54 - 48.\xi\lambda$	$-0.022 - (0.6 \pm 0.3)\xi\lambda$
0.1915	$23.3 - 3.4\lambda$	$8.0 - 4.9\lambda$	$(0.5 \pm 0.1) - (0.1 \pm 0.1)\lambda$
0.4028	$12.6 - 3.6\lambda$	$7.9 - 4.2\lambda$	$-(0.05 \pm 0.05) - (0.11 \pm 0.05)\lambda$
0.6882	$4.01 - 1.82\lambda$	$3.23 - 1.86\lambda$	$-(0.22 \pm 0.02) - (0.04 \pm 0.02)\lambda$

Slide 9

$$\xi = \eta_f - \eta_i \simeq \frac{\Delta E}{2E} \quad \bar{\eta} \tag{5}$$

and for most examples of Coulomb excitation lies between 0.2 and 1. One can see from the tables that for values of λ of the order of unity there might be something like a 30% change due to second order effects. Graphical representations of these results are presented in Slides 10-12. One can thus see that by observation of either the total or inelastic cross section, or by observations of the gamma angular distribution (which has some difficulties to be discussed shortly) one should be able to determine the quantity λ, which according to Eq (2) is proportional to the static quadrupole moment of the excited state.

I mentioned that there was difficulty associated with the observations of the photon angular distribution. This comes about particularly for heavy ions, if there is a finite probability of ionization of the target electrons in the collision. One had known previously that there are possibilities of attenuating the angular distribution due to elastic quadrupole interaction in some target materials, and the possibility of a magnetic hyperfine interaction should give similar difficulties. This corresponds to a reorientation of the oriented nucleus in the magnetic field of the orbital electrons before it has had a chance to decay. One can get an estimate of the order of magnitude of this effect by taking a typical large hyperfine splitting, say one inverse centimeter multiplied by the velocity of light and multiplied by the typical lifetime of these states. One sees then that the precession of the oriented nucleus may be through an angle of the order of π and this causes considerable change in the angular distribution. The situation is quite serious for heavy ions because one has a fairly large probability of ionizing the electrons in a collision.

Estimates have been made of the probability of exciting the electronic structure.[4] As an example, for gold, using Hartree wave functions, a calculation was made for excitation of the 6s electron to a 6p state. It turns out, if one takes a scattering angle of 90 deg, that this probability is $0.0044Z'^2$. This is a probability which is quite negligible for protons, but for projectiles like neon represents a 40% probability. These are crude calculations which indicate, however,

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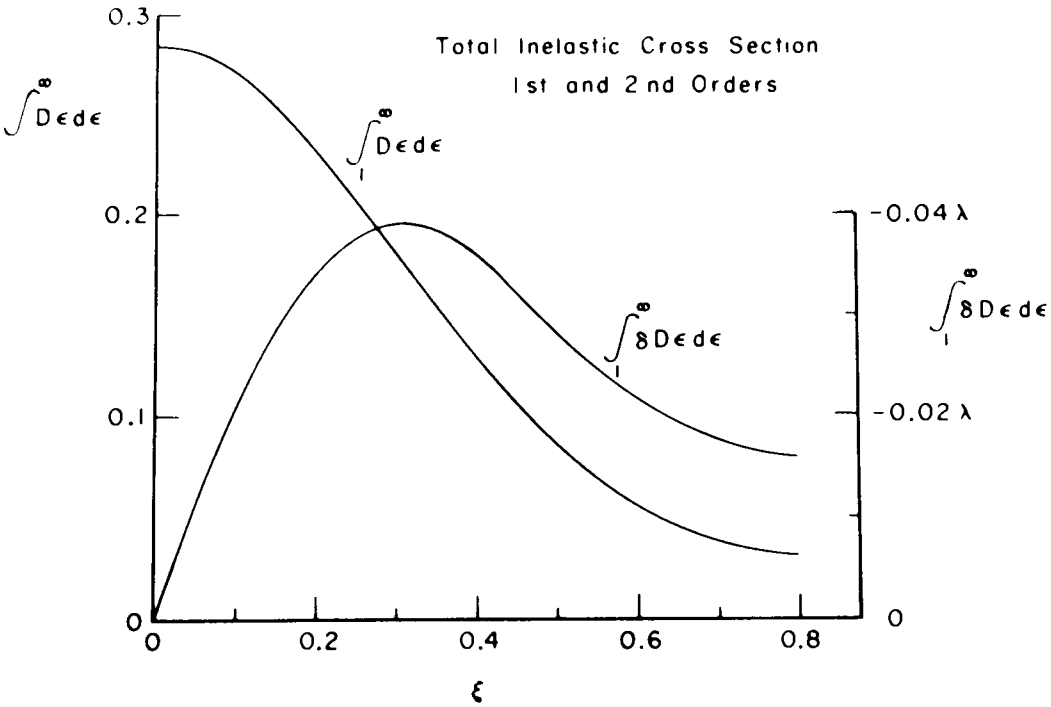

Slide 10

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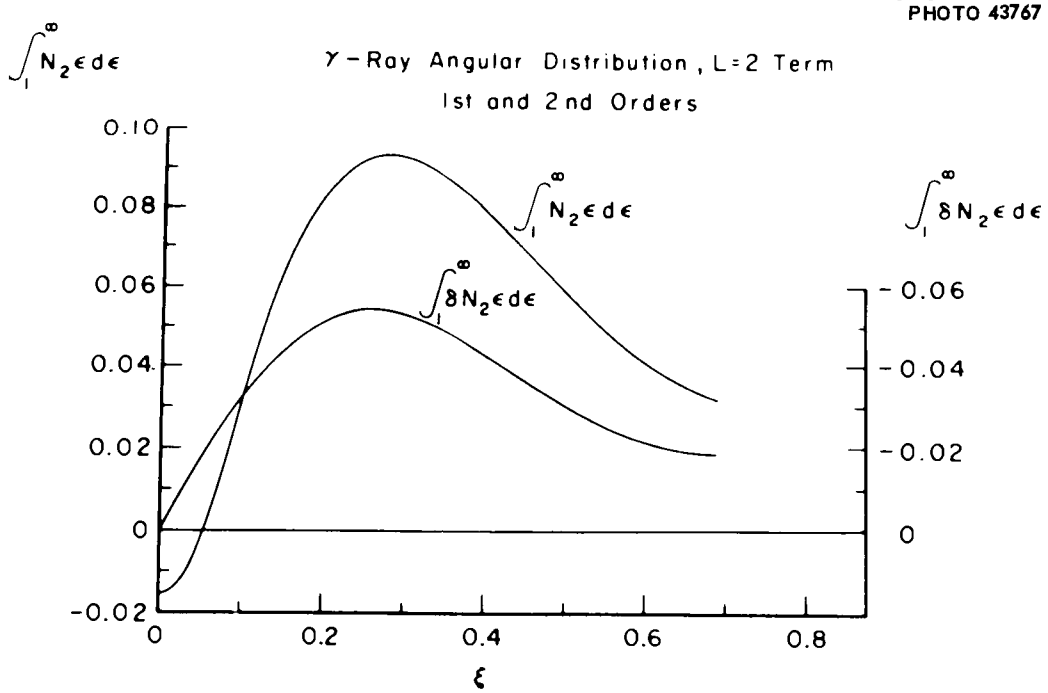

Slide 11

that one has to worry about this effect if one is going to try to make observations of the angular distribution.

There are other serious effects. One of them is the recoil of the target nuclei which, in the case of bombardment by heavy ions, is large enough to cause serious electronic rearrangement in the collision of this recoiling nucleus with other atoms. One therefore will, at least at the beginning, confine one's attention to the observation of the total cross section or to the inelastically scattered particles if one wants to measure some of these second order effects. These quantities are not affected by such things as the hyperfine and electrical quadrupole interactions because the time involved is the collision time which is typically of the order of 10^{-20} sec or smaller, compared to the lifetime of the state.

There are other second order effects, but there is not really time to do anything more than just mention them. A calculation has been performed by Zickendraht[8] of double excitation both of the projectile and the target nucleus, in which he finds essentially that the probability of such excitations is just the probability of individual excitations.

I also should mention the fact that the calculation was carried out quantum mechanically in a couple of cases to find out if the semi-classical calculation really applies.[3] Slide 13 shows the results of the estimate of such effects. The column marked QM contains the quantum mechanical results; the column SCT the semi-classical results. The term b_0 involves the angular independent cross section for the gamma rays; the term b_2 is the coefficient of the P-2 term. (The P-4 term is fairly small and is left out.) One sees that the QM first order term is quite close to the first order term in the semi-classical approximation. This is what one expects from the usual considerations of the validity of the SCT calculation. However, one sees that even for values of η for which there is good agreement in the first order term, there is fairly sizeable disagreement in the second order term. between the quantum mechanical and the semi-classical calculation. The same thing is true for the angular distribution. Here, however, one had already known that the angular distribution depends sensitively on phases which are not taken into account in the semi-classical calculation. The situation is considerably improved if one goes to higher values of η, as one expects, where the differences are not quite as large.

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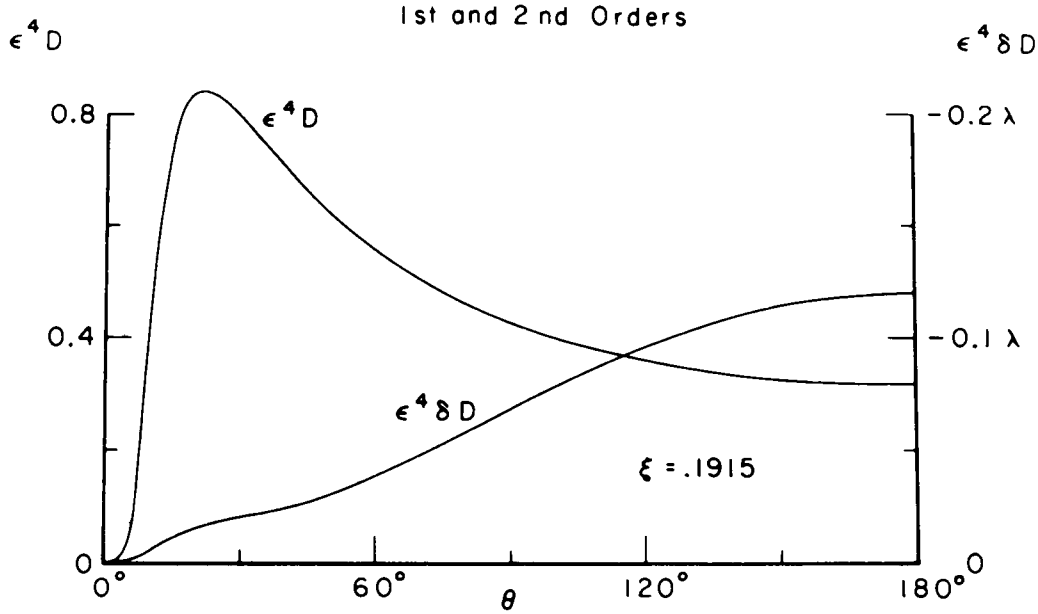
Inelastic Scattering Angular Distribution
1st and 2nd Orders



Slide 12

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COMPARISON OF QM AND SCT REORIENTATION EFFECTS

PARTICLE AND INCIDENT ENERGY	QM	SCT
5 MEV PROTONS	$b_o = 14.7 - 3.7\lambda$	$14.8 - 2.1\lambda$
($\bar{\eta} = 5.6$, $\xi = 0.19$)	$b_2 = 6.2 - 4.5\lambda$	$5.1 - 3.1\lambda$
10 MEV α-PARTICLES	$b_o = 96 \quad -23\lambda$	$97 \quad -19\lambda$
($\bar{\eta} = 15.7$, $\xi = 0.26$)	$b_2 = 48 \quad -29\lambda$	$45 \quad -25\lambda$

$$(b_o)_{SCT} = 2\bar{\eta}^2 \int_1^\infty D \, \epsilon \, d\epsilon$$

Slide 13

CHAIRMAN BREIT: Any discussion of this paper?

M. E. ROSE: I came in a little late; so maybe this question is not quite to the point but I had the impression that your work is largely with the semi-classical theory. Is it very difficult to do the third order calculation in the quantum mechanical theory? You have some numbers so apparently you have done it.

GLUCKSTERN: Well, this is not what I did. This was done by John Lazarus. The calculation is considerably more difficult, because for the large values of η there are contributions from many values of L, and each one of these calculations involves the performance I think of 12 double integrals. With the present-day computers it is certainly within the realm of accomplishment, but it is an order of magnitude more difficult than the semi-classical calculations.

C. E. PORTER: Is any attention being given to calculating the transition region between the Coulomb excitation region and the stronger reaction region, the point being that you might begin to find some sensitive radius in here. The Coulomb excitation must eventually go away.

CHAIRMAN BREIT: Not very much. There has been some thinking done about it and a little estimating, but no real systematic attempts.

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25. COULOMB EXCITATION EXPERIMENTS WITH THE HEAVY ION ACCELERATOR AT BERKELEY

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(Presented by J. O. Newton)
University of California, Berkeley

Before I discuss the experiments, for the benefit of those of you who are not too familiar with the subject I will just briefly mention one or two plausibility considerations, so that you will have the advantage of knowing what I am talking about.

Of course, you all know that Coulomb excitation arises when a particle going close to a nucleus induces transitions to excited states in this nucleus by means of its electric field. The moving particle produces a pulse in the electric field at the nucleus. If $\underline{v}$ is the velocity of the particle at infinity and $\underline{a}$ is half the distance of closest approach in a head-on collision, then you might plausibly say that the width of this pulse is going to be something like a/v. If ω is the frequency of the excited state which we are considering in this nucleus, then in order to get appreciable excitation of this state the Fourier analysis of the pulse has to include appreciable components of this frequency. Thus, the condition for appreciable excitation is that $v/a \gtrsim \omega$. Writing this in another way we have

$$\xi = \frac{a\omega}{v} \leq 1 \qquad \text{Eq. 1}$$

ξ is the quantity that was mentioned in the previous paper.

The only point that I want to make from this is that if ξ gets appreciably greater than unity, then the probability for excitation of the state drops off very rapidly, indeed, in a roughly exponential fashion; if this quantity is less than one, then the probability becomes approximately constant. The cross section σ for E_2 excitation, which is the only one consider here can be written

$$\sigma_{E2} \propto E.\ f_{E2}(\xi).\ B_{E2} \qquad \text{Eq. 2}$$

B_{E2} is the reduced transition probability for the excitation of the state. The energy dependent quantity $E\ f_{E2}(\xi)$ varies with ξ in the manner indicated above, E is the bombarding energy.

Slide 1 shows two things. The two lower curves show the results of a calculation of the potential barriers for N^{14} ions incident on nuclei as a function of the nuclear charge number. The indicated formula, which apparently is not well liked, was used but it gives an idea of the answer. The upper of these two curves gives the energy of the barrier, the lower curve gives the energies for penetrability factors of 10^{-2}. This indicates the maximum energies where nuclear reactions are not too serious. The highest curve shows the energies of the excited states which can be excited if the quantity $\xi = 1$ and the penetrabilities equal to 10^{-2}. It shows that with heavy ions such as nitrogen you can hope to excite states in nuclei up to about 2 Mev.

The previous speaker spoke of the advantages of using heavy ions, so I will not go into that except to make one point. He said that the cross sections for the first order process were proportional to A_1 squared, A_1 being the mass number of the incident particle. We must not assume from this that the yield is going to be porportional to A_1 squared, because the range-energy relation reduces this again; so that there is not a very marked dependence of yield on the charge of the incident nucleus. However, there is another feature where heavy ions are advantageous and that lies in the shapes of the curves of penetrabilities as functions of E/E_B. The penetrability drops off much more quickly with E/E_B decreasing from unity for incident heavy ions such as nitrogen or oxygen than it does for lighter ions such as protons. Thus with protons it is possible to reach a smaller fraction of barrier height than is possible with heavier ions. Hence higher states can in principle be excited with heavy ions than with light ions.

With that preamble I will come to the work which we have done. This work started about Christmas time, and we then used the 1-Mev/nucleon beam. For those of you who are not familiar with the HILAC, there are essentially three parts to it. The first one is a

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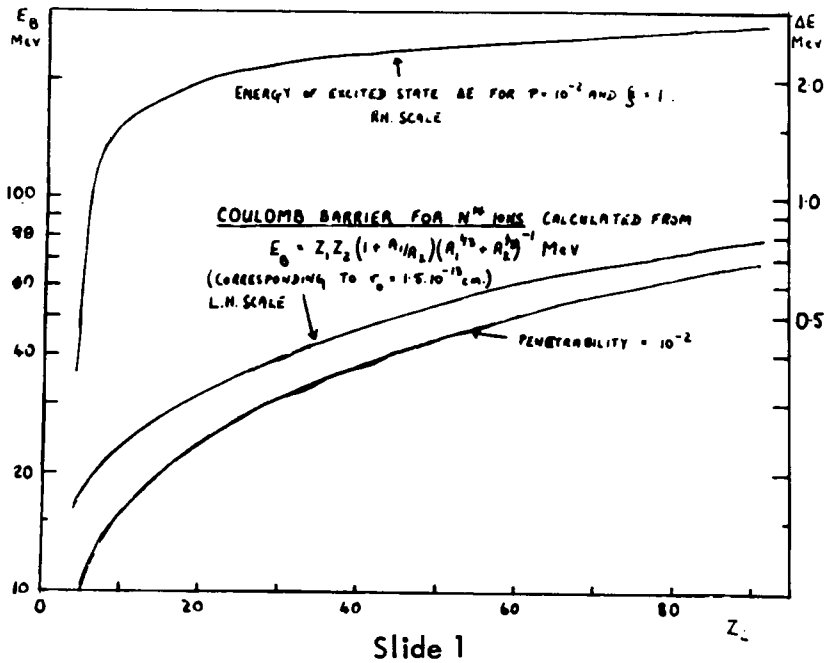

Slide 1

Cockcroft-Walton set which produces 500-kv ions. These go through a stripper and then to another accelerating tank which gives a beam with an energy of 1 Mev/nucleon. The beam then goes into another stripper and then into the main tank where it achieves an energy of 10 Mev/nucleon, which is well above the potential barrier for all nuclei. One Mev/nucleon is rather low for Coulomb excitation.

We were offered the low energy beam for about two days and we used it to look at some light elements, Al^{27}, Mg^{24}, and F^{19}.

Slide 2 shows what we got. The upper curve shows the gamma rays obtained when we bombarded Al^{27} with nitrogen ions. You can see that there are very prominent gamma rays; these arise from the decay of the first two excited states. We are not quite clear from where the 1.3-Mev radiation comes. The B_{E2} for these transitions are only estimated roughly because when this was done we did not suppress the secondary electrons from the target. The lower curve shows what we obtained by bombarding Mg^{24}. You see here the very prominent 1.36-Mev gamma ray which corresponds the decay of the first excited state in Mg^{24}. Very roughly the B_{E2} for this transition is a little larger than that given by the single particle formula. So we have to repeat these measurements in order to get accurate values. I show you this just to show you what can be done with the machine.

After we had the short run about Christmas time we spent a long time doing measurements to persuade the relevant people that we needed some shielding. The x-ray intensity from this machine is very high; by x-rays I mean bremsstrahlung radiation produced from electrons accelerated between the electrodes of the machine. This extends up to about 7 Mev in energy. The x-ray intensity is typically about 100 mr/hr at the end of the machine. It is a high intensity. After about two months we managed to get a 2-ft thick concrete wall built. That was still not good enough, so we had also to build a little cave with 16-in. concrete walls; then we were in business.

Our experimental arrangement is shown in Slide 3; it is very roughly to scale. The ion beam comes out from the machine and then goes through two aluminum windows which are a half-mil thick, the

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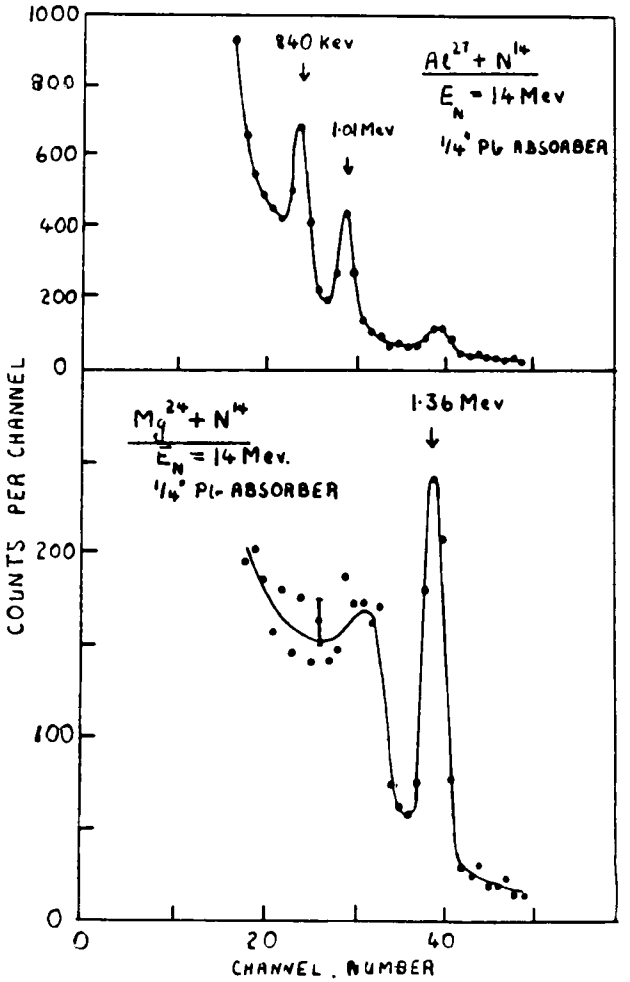

Slide 2

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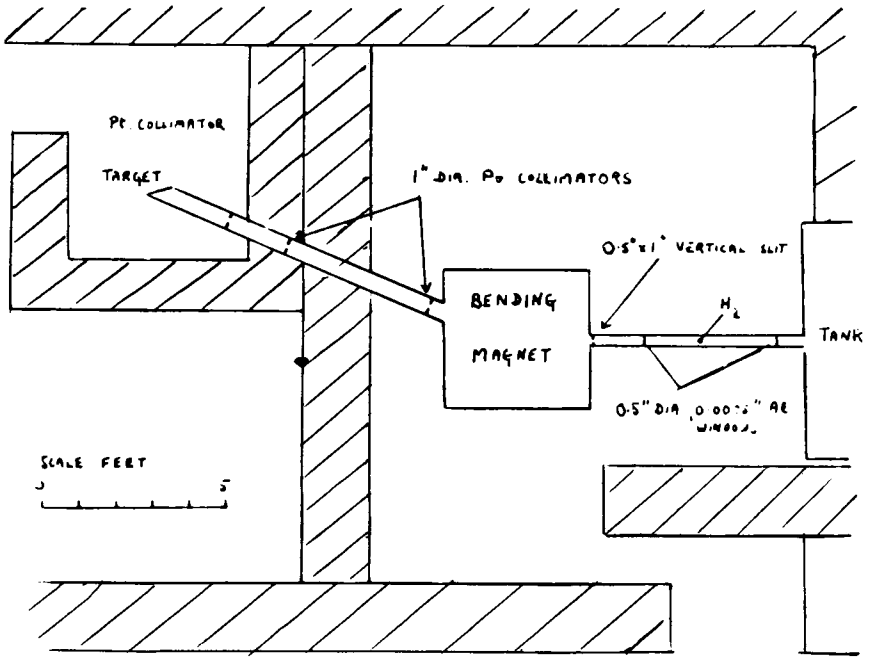

Slide 3

space between being filled with hydrogen. To vary the beam energy we vary the hydrogen pressure. The degraded beam then goes through a vertical collimating slit, through a bending magnet, and then through a tube containing three 1-in. dia lead collimators ahead of the target. It is very important that the beam does not hit the sides of this brass tube, else the background will be high. The gamma radiation from the target is observed with a NaI scintillation counter.

The first thing we tried to do was to look at the background from the target above about 500 kv in energy because we were interested in exciting fairly high excited states. We found that target preparation, just as we had expected beforehand, was going to be one of the most difficult problems.

Slide 4 will give you an idea of some of the things we got. It does not have any intrinsic interest other than that. It shows the sort of variation you get between different targets. We concentrated largely on targets in the region of gold and platinum because these can be obtained in pure form rather easily and they don't oxidize readily. We tried hard to reduce this background with gold targets. We managed to reduce it by a factor of 2 by dissolving the surface of the gold in aqua regia. Then we thought there might be some trouble from absorbed gases in the target so we built an electron heater. With this we were able to heat the target in position to a temperature just below its melting point for several hours. This made an improvement of a factor of 1.5, not terribly encouraging.

We didn't seem to make a lot of progress. It seemed as if each target had to be treated on a strictly individual basis. I might point out here there is a lower limit to the background which you can expect, and that is due to heavy particle bremsstrahlung. This is the lower dotted curve in the figure. You will notice that the best results which we obtained are a factor of 10^3 higher than this minimum background. In spite of this it is quite easy to see transitions which have a B_{E2} having a value of the order of that of a single particle so that one is still not badly off for the work.

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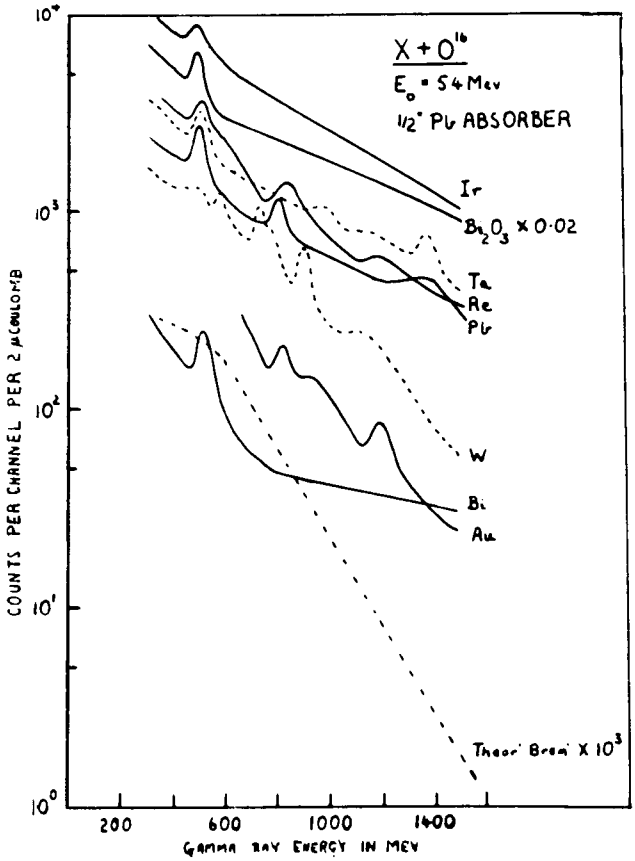


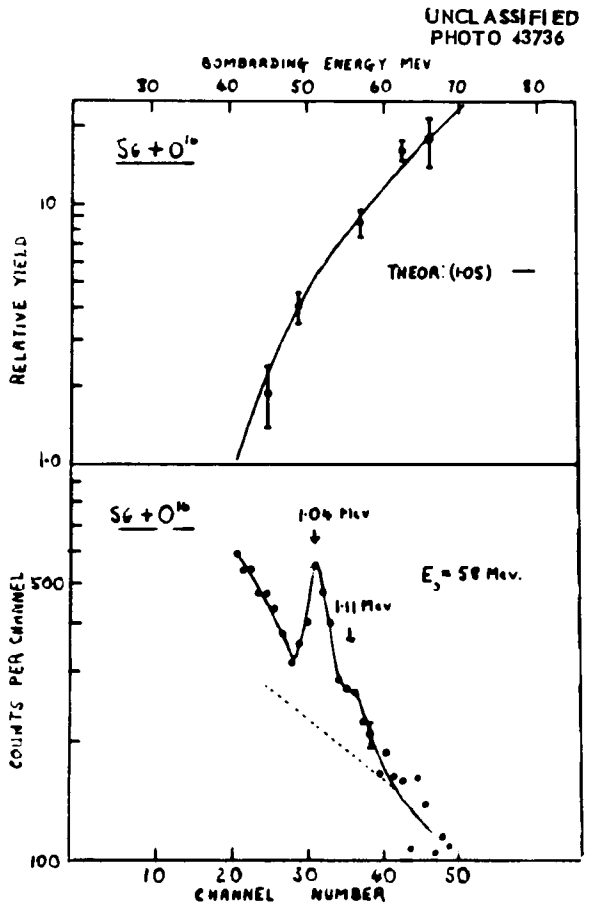
Slide 4

I don't want to go into details of the gamma rays that may or may not show up in these curves except to remark in passing that the two upper peaks in the tungsten curve correspond to gamma rays from the decay of the states which Dr. McGowan talked about in the recent Physical Society meeting. These states are the 2+ vibrational states in two of the tungsten isotopes.

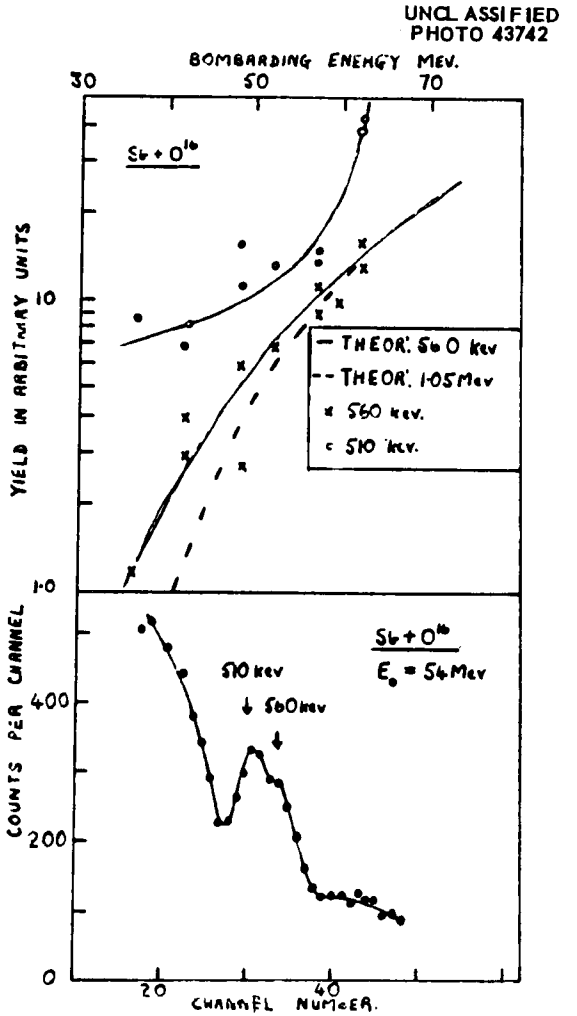
After we had done quite a lot of this we became a little discouraged. We thought we ought to get some results to talk about at this meeting, and so we then went to something else. Before I talk about this may we have the next slide.

This is something we did rather by chance. We had an evaporated bismuth target made for us. We wanted to see how it behaved because there were likely to be no strong gamma rays from a bismuth target. They had some bismuth of their own and wanted to get their apparatus set up using this; we had given them some very pure bismuth. So they set up the apparatus and made a target with our bismuth. Unfortunately, they didn't remove the collimator of the apparatus when they made our target with the pure bismuth. This was unfortunate because their bismuth turned out to be pure antimony! They didn't know it, of course, at the time. We thought we had a pure bismuth target and found some gamma rays in from it. These gamma rays are shown in Slides 5 and 6. They have energies of 0.56, 1.04 and 1.11 Mev. We ran excitation functions on them. The experimental points for the 1-Mev gamma rays agree well with the theoretical excitation function for E 2 excitation of a state at 1.05 Mev.

The excitation function for the 0.56-Mev gamma ray is rather rough; it is shown in Slide 6. It is difficult to say whether this gamma ray arises from the decay of a 560-kev state or from a 1-Mev state. In addition to the 560-kev peak there is a peak at 510 kw which is rather larger; the excitation function of this is also shown. In fact, one sees this 510-kev peak in everything one bombards unless there is some other peak which obscures it; this I think must be due to target impurities. You can see that it is not greatly energy dependent with the bombarding energy is close to that of the barrier. It is probably due to oxygen on nitrogen impurities.



Slide 5



Slide 6

We decided later to try the target they made with their "bismuth"; we found the same peaks but their yield was about 20 times larger than that from our "pure bismuth" target. The B_{E2} for the 560-kev gamma ray has about the single particle value. The B_{E2} for the other two gamma rays are about four times the single particle value.

The next experiment which we did is perhaps the most interesting one I have to talk about; that is the double E 2 excitation with even-even isotopes which the previous speaker has mentioned. We used natural tungsten as a target because we didn't have the separated isotopes at that time, but I don't think this matters too much. The three even-even isotopes W^{182}, W^{184} and W^{186} make up 86% of natural tungsten, the other 14% is W^{183}. The even-even isotopes have rotational states of spin 2+, 4+, etc. The 4+ state can be excited from the ground state either by E4 excitation or by double E2 excitation via the 2+ state. The form of the cross section for double E 2 excitation can be obtained from plausibility considerations. If we take the quantity a (previously defined) as giving an order of magnitude for the interaction distance then it is plausible that the cross section for a single E2 excitation is given by

$$\sigma_{E2} \sim \pi a^2 . P \qquad \text{Eq. 3}$$

where P is the probability for exciting the state in equation when the particle comes within the radius of interaction. Similarly, for a double excitation we might

$$\sigma_{E2E2} \sim \pi a^2 \, P\,(0 \rightarrow 2)\, P\,(2 \rightarrow 4). \qquad \text{Eq. 4}$$

Rewriting this we get

$$\sigma_{E2E2} \sim (\pi a^2)^{-1} \, \sigma_{E2}(0 \rightarrow 2)\, \sigma_{E2}\,(2 \rightarrow 4). \qquad \text{Eq. 5}$$

In the review article of Alder et al an approximate formula of the same form as (5) but with 4 instead of π was given. A more accurate calculation was also made for the case $\xi = 0$; the approximate formula with $\xi = 0$ agreed fairly well with this result. I discovered a few days ago at the Washington meeting that they issued an erratum later and that the theoretical cross section should be about a factor of 11 less than that published in the paper. We didn't know this and thought that we had discovered a serious discrepancy with the theory.

Slide 7 shows the pulse spectrum of the gamma rays that we got when we bombarded tungsten with 45-Mev oxygen ions. Here I should say that there are three even-even isotopes in tungsten. The average energy for the first excited state is 114 kev, and the average energy for the 2 to 4 transitions is about 250 kev. Since the three even isotopes have slightly different moments of inertia the 114- and 250-kev peaks are broad. The 250-kev peak can be plausibly resolved into four peaks, three large ones and one smaller one at about 290 kev. The smaller one arises from the excitation of W^{183}.

Slide 8 shows the excitation function of the 250-kev peak. The line is derived from a formula of the form of equation 5. It is arbitrarily normalized to fit the experimental points below about 55 Mev. The experimental points lie about a factor of two below the curve at the higher energies. Probably one would not expect good agreement here anyway as the perturbation treatment is likely to fail. However this is the only theory that is available at present.

Slide 9 shows the excitation function for the 2+ states, i.e, the 114-kev gamm ray. The line through the points is the theoretical excitation function for E2 excitation of a 114-kev state. You see again that at the higher energies there seems to be a deviation by a factor of about 2 of the points from the theoretical curve.

The lower curve gives an idea of what the background was like in this experiment. It is the magnitude of the background above the peak. You can see that above about 65 Mev it begins to rise rapidly; this indicates that we are getting very close to the potential barrier here. In the region of 50 Mev the cross section for excitation of the 2+ states is of the order of 1.5 barns, a very large cross section.

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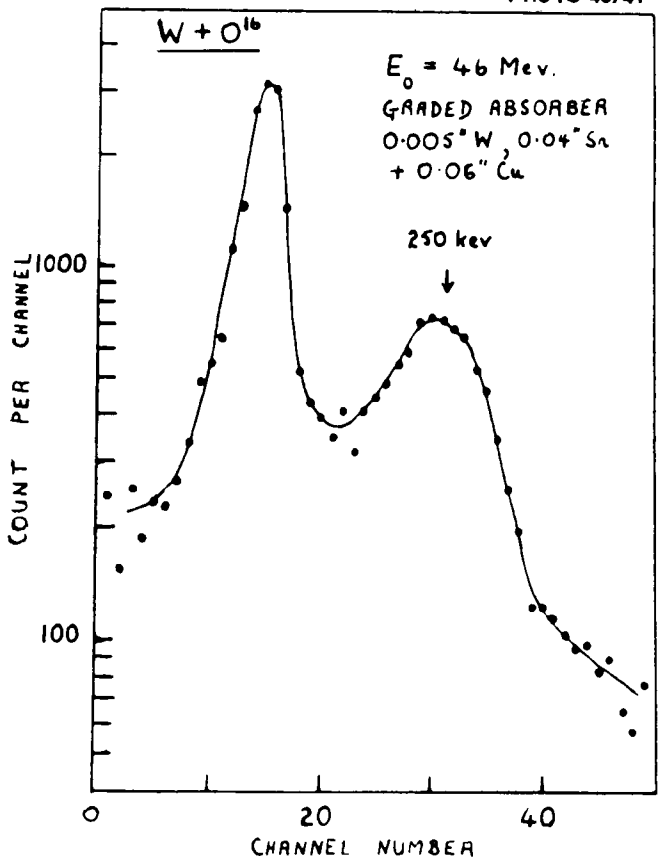

Slide 7

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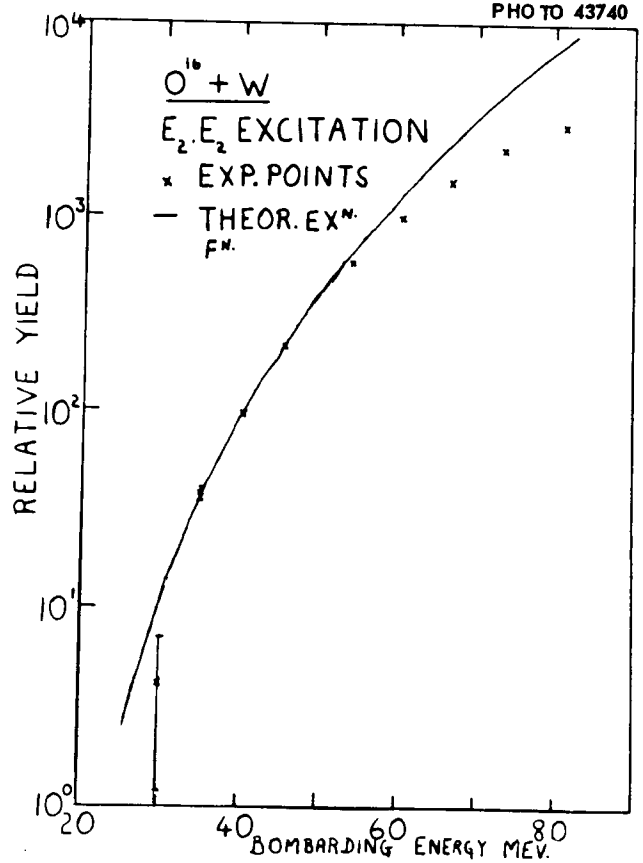

Slide 8

We wondered, having found these discrepancies with the theory at the higher energies, whether there was something wrong with our experimental method. To check this we ran another excitation function with a platinum target. Two isotopes of this give a gamma ray of energy about 350 kev. The states from which this arises are considerably higher in energy than 114 kev and the B_{E2} for exciting them is considerably less than that for exciting the 2+ tungsten states. Thus, the probability for excitation of the 350-kev states is considerably smaller than it is for excitation of the 2+ tungsten states at a given bombarding energy.

We thought that this deviation from the theory might be due to the probability for excitation being very high. If this were so it would not show up in the platinum case and there we ought to get agreement with the theoretical excitation function. Slide 10 shows the results. The lower part shows the pulse height spectrum with the prominent 345-kev peak. The upper part shows the experimental and theoretical excitation functions. You can see that the agreement is good. This suggests that the deviations from the so-called theoretical formula are real and due to the very high excitation probabilities.

Using the formula that was given in the paper of Alder et al, we found that the cross section for E2, E2 excitation should be $\sim$300 mb at 40 Mev bombarding energy. Our experimental value is $\simeq$25 $\pm$ 4 mb, a considerable discrepancy. A few days ago, however, I heard from Dr. McGowan of a correction that has been issued. The corrected theory gives 27 mb for the cross section, which agrees well with our value. I might just remark here that if the excitation were E4 then the B_{E4} would have to be 5000 times that for a single particle; this seems most unlikely.

I would like to point out that all the results that I have given here are somewhat preliminary, and I don't wish to be quoted on them. We finished this work only three days before I left for the conferences in Washington. We would therefore like to think about it a bit more.

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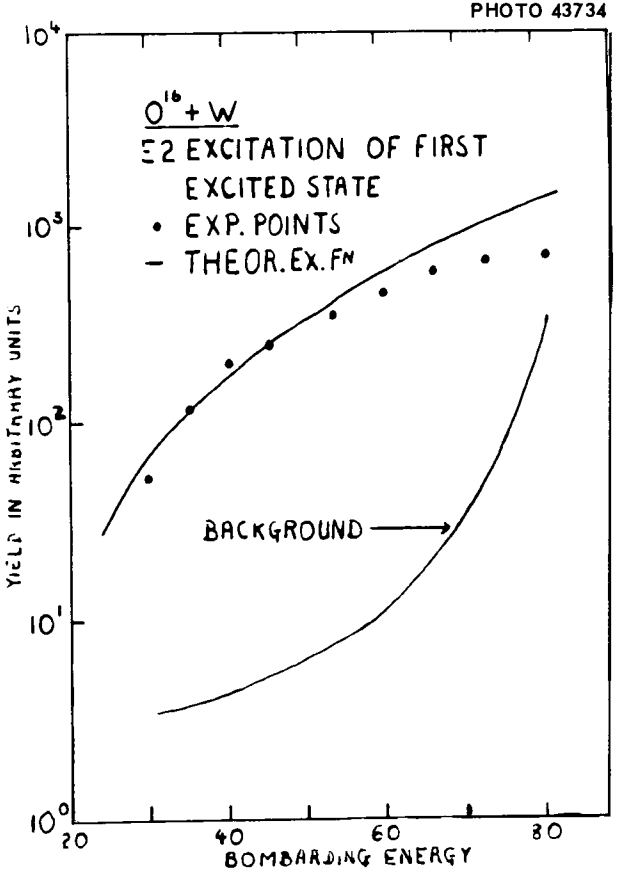

Slide 9

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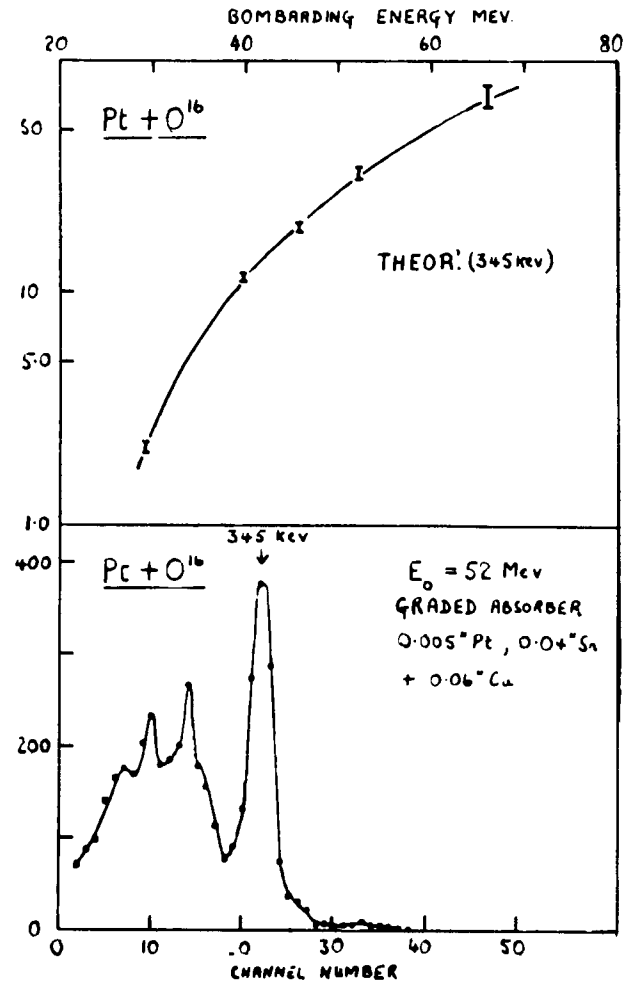

Slide 10

There is one other thing we want to do in order to be sure that we are really exciting the 4+ states. If we are, we should certainly see coincidences between the 114-kev gamma rays and the 250-kev gamma rays. We ought to measure these coincidences. We are getting ready to do this. I had a phone call from my colleague the other day who said he had tried this experiment last Friday and he could not find any coincidences. However, he did find them when he bomarded tantulum. Tantalum is an odd-mass rotational nucleus. In such a nucleus it is possible to excite the first two rotational states by E2 excitation; thus concident gamma rays will be obtained. This experiment was a first try, however, and the accelerator was working very badly. In fact in the last run he did start to get coincidences but then the beam on the machine went very high and unstable; shortly afterwards the machine broke down for the day. The experiment will have to be repeated and we think it is very likely that we will find some concidences, but until we do we have to reserve judgement. Of course, if there aren't any coincidences, then there is a very serious discrepancy with the theory; so it will be interesting even in this case.

CHAIRMAN BREIT: Any discussion of this paper?

R. L. GLUCKSTERN: May I ask what are the relative fractions of gamma rays? In other words, how many of the 114 did you subtract in order to get the direct E2 excitation to the lower level. Presumably the yield of 114-kev gamma rays is affected by the double E2 excitation.

NEWTON: Well, actually this effect does not exist for the double E2 excitation to the 4+ state since the 4+ state decays to the 2+ state. However there presumably is an effect due to Coulomb de-excitation of the 2+ state.

GLUCKSTERN: Would it probably come in the third order?

NEWTON: Would it be third order?

CHAIRMAN BREIT: There is an effect in second order.

GLUCKSTERN: I think one of the things that might possibly be involved in this would be the reorientation effect itself.

CHAIRMAN BREIT: Yes, the reorientation effect. Also, of course, once you get a large second order effect then the third order effect is large. It begins to mount up.

NEWTON: We will look for the 6+ state as well.

CHAIRMAN BREIT: The second order effect is calculated on the assumption that the excitation to the 2+ is correct for feeding the upper ones, providing one neglects the reorientation effect. There is probably no real disagreement. There is apparently no real cause for fear, don't you think so?

GLUCKSTERN: Yes, I think so.

G. F. PIEPER: The Yale accelerator is a little like the Berkeley accelerator, and we, too, have done one day of Coulomb excitation with it and have succeeded in exciting our old friends tantalum and fluorine. The B_{E2} values which he mentioned for excitation probability have been used largely to determine the beam current.

NEWTON: I might say that in the platinum curve we got the right cross section, the error being about 10 to 15%; so we feel that we are probably all right in measuring a cross section of this sort. Of course, one of the things that is not too well known at the moment is the range energy curve. If you use a thick target, as we did, then this can cause some error in the results.

PIEPER: What are you using for dE/dx?

NEWTON: We are using a set of theoretical curves.

26. RECENT RESEARCH ON FISSION PERFORMED AT THE INSTITUTE OF ATOMIC ENERGY OF THE USSR ACADEMY OF SCIENCES, MOSCOW

Translation of a report submitted by Prof. G. N. Flerov to be included in the Proceedings

PART I: THE FISSION OF NUCLEI OF HEAVY ELEMENTS BY INTERACTION WITH CARBON, NITROGEN, AND OXYGEN NUCLEI

In the Institute of Atomic Energy of the Academy of Sciences, USSR, the structure of a cyclotron source was developed, which makes it possible to obtain intense beams of heavy ions with high multiple charge. Using this source in a 150-cm cyclotron, we obtained monochromatic beams of C^{12}, C^{13}, N^{14}, and O^{16} nuclei which were used to study the interaction of accelerated heavy ions with matter.

In the interaction of multi-charged ions with the nuclei of heavy elements, compound nuclei are produced which are excited to an energy equal to several tens of Mev.

The break-up of these nuclei occurs either by evaporation of neutrons, or by fission. The relationship between these two processes depends on the charge and mass of the compound nuclei and on the excitation energy. G. N. Flerov performed a number of experiments to determine the nuclear fission cross section and the cross section for producing isotopes after neutron evaporation by irradiating various elements with multi-charged ions. The results of these experiments indicate a strong dependence of the ratio of the fission width to the neutron width, Γ_n/Γ_f, on the parameter Z^2/A of the compound nucleus.

In addition, a more detailed investigation was made on the nuclear fission process with multi-charged ions. In particular, the mass spectrum of the fission fragments was investigated and the results are presented below.

A. Experiments On the Determination of the Nuclear Fission Cross Section During Interaction With Multi-Charged Ions.

Experiments to determine the fission cross section were performed inside the cyclotron chamber as well as on an external beam of ions,

twelve meters from the cyclotron. We investigated the energy dependence of the fission cross section, σ_f, of various elements on irradiation by C^{12}, N^{14}, and O^{16}.

In the experiments on the external beam, an ionization chamber was used to record the fission fragments. Both the relative form of the curve, $\sigma_f = f(E)$, as well as the absolute fission cross section were determined. In the experiments on the internal beam, only the relative change of σ_f with a change in the energy of the bombarding particle was determined. The β-activity was used to determine the nature of the fragments.

The energy of the C, N, and O ions was measured by absorption in aluminum, by using a relationship between range and energy in aluminum experimentally obtained in our laboratory. A change in the particle energy was obtained by placing aluminum absorbers in front of the target.

In the first experiments on the fission of U, Bi, Au, and Re by nitrogen nuclei, the N^{14} energy was measured and varied by using a focusing magnet placed between the ionization chamber and the cyclotron. Deuterons of known energy were used to calibrate the magnet; however, due to a slight difference in the trajectory of the multi-charged ion and the deuteron and also due to the presence of ions having different angles of admission to the gap of the focusing magnet, the energy of the multi-charged ion, for a given magnetic field value, was determined with insufficient accuracy.

The use of aluminum absorbers made it possible to dispense with the calibration of the focusing magnet and to obtain a more exact relationship between cross section and energy.

The experimental results are given in Figs. 1, 2, and 3. It is apparent that the fission cross section of Bi and U by C, N, and O nuclei are satisfactorily described by the formula

$$\sigma = \pi r_o^2 (A^{1/3}_{\substack{\text{target}\\ \text{nucleus}}} + A^{1/3}_{\text{inc}})^2 \left(1 - \frac{B}{E}\right), \text{ where } r_o = (1.4 \text{ to } 1.55)10^{-13} \text{cm}$$

when the energy of the bombarding particles E exceeds the Coulomb barrier B. Figure 3 indicates the strong dependence of the fission cross section on Z^2/A of the compound nucleus for a given excitation energy.

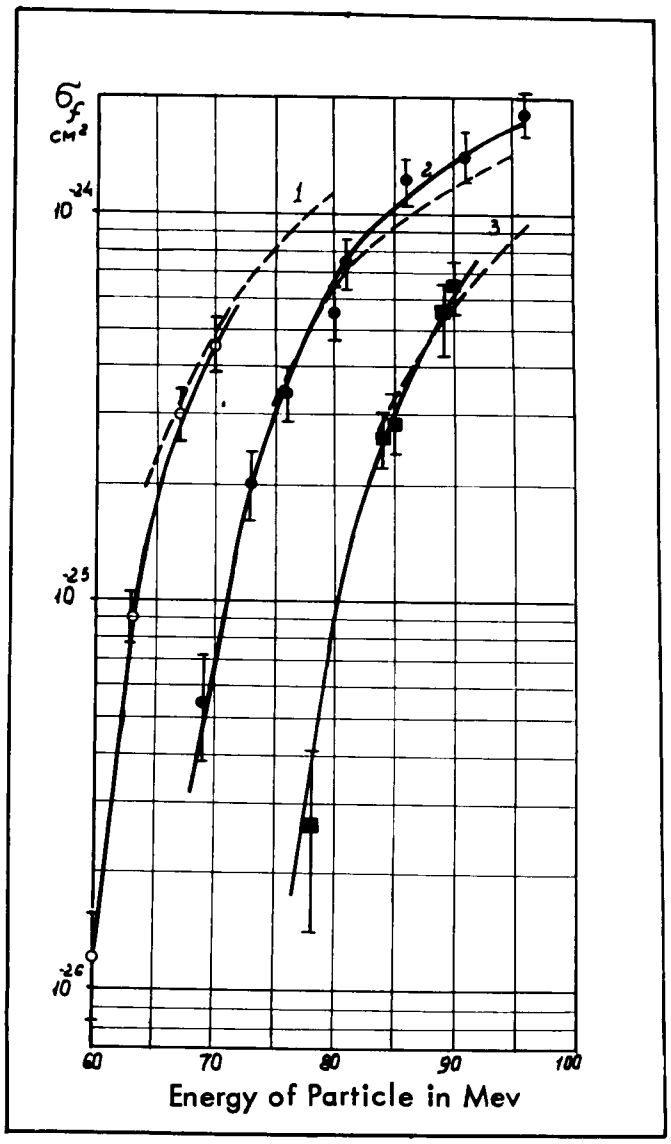

Fig. 1

Dependence of the fission cross section of Bi on the energy of carbon (1), nitrogen (2), and oxygen (3) ions.

Heavy lines - experimental curves

Dashed lines - curves calculated from the formula

$\sigma = \sigma_o (1 - \frac{B}{E})$ for $r_o = 1.5$ f (1)

$= 1.55$ f (2-3)

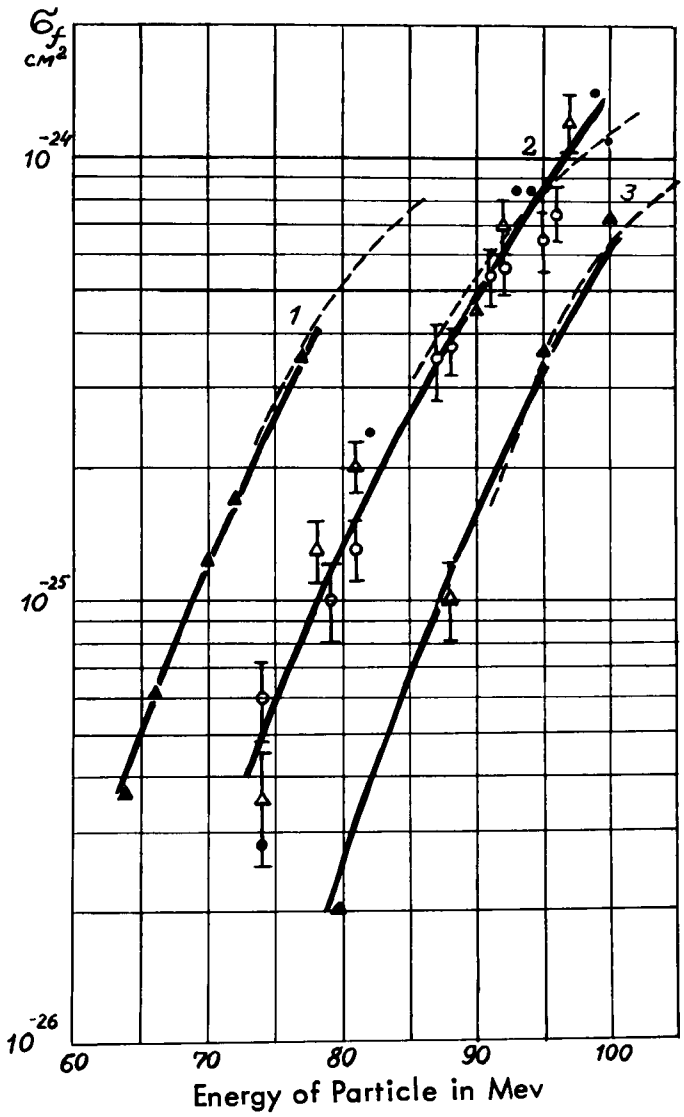

Fig. 2

Dependence of the fission cross section of uranium on the energy of carbon (1), nitrogen (2), and oxygen (3) ions.

Heavy lines - experimental curves

Dashed lines - theoretical curves for r_o = 1.4 f (1)
= 1.45 f (2)
= 1.52 f (3)

Δ - Chamber data for U^{228}

o - Chamber data for U^{235}

oΔ - Activation data for U^{238}

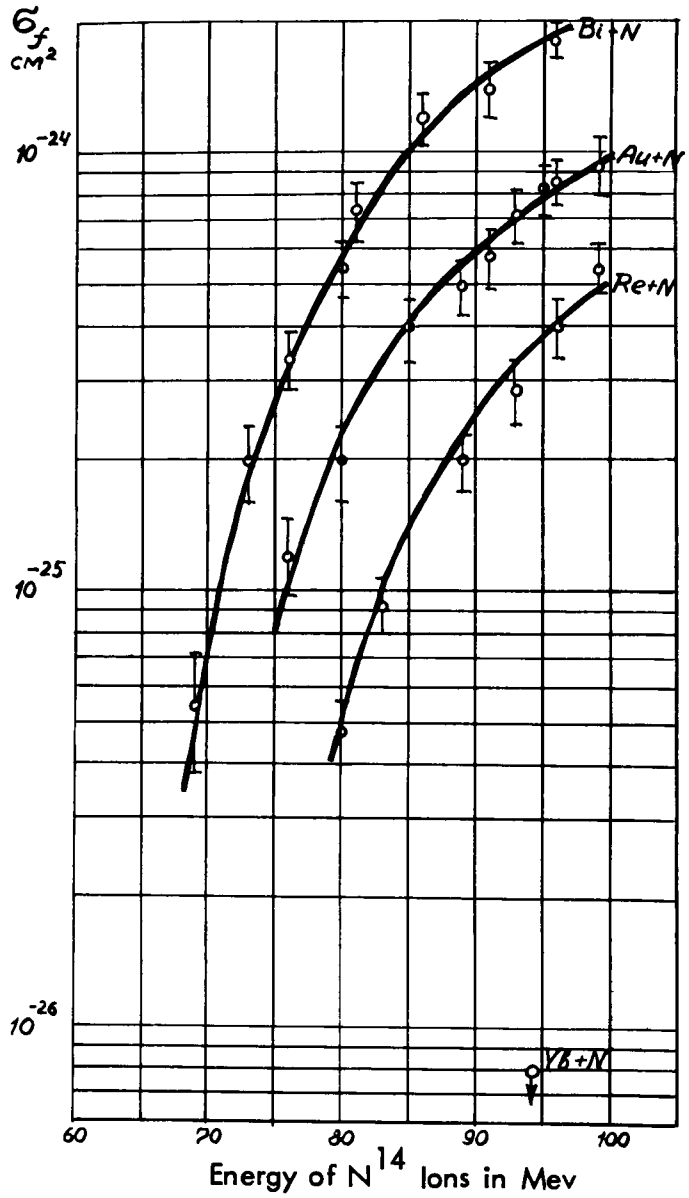

Fig. 3

Dependence of the fission cross section of Bi, Au, Re, and Yb on nitrogen ion energy.

B. Mass Distribution of the Fission Fragments Produced During the Irradiation of Gold and Uranium by Nitrogen Nuclei.

Along with a determination of the effect of fission cross sections, the mass spectrum of fission fragments produced on irradiation of U^{238} and Au^{197} by N^{14} nuclei has been investigated. Targets $\sim$15μ thick were used in the experiments. The irradiation was performed inside the accelerating chamber with $\sim$105-Mev nitrogen ions. Fourteen different elements were chemically separated from the irradiated targets and identification of the isotopes was performed from the half lives, and energy, and sign of the β-particles. The experimental results are presented in Figs. 4 and 5.

The relative fission fragment yields produced during the irradiation of Au are given as a function of the mass number in Fig. 4. The nuclei presented comprised the interval of mass numbers A from 71 to 139 and the Z interval from 32 to 56. These are the isotopes of Ga, Sr, etc. up to the Sn, Sb, and Ba isotopes. It is apparent from Fig. 4 that the nuclear yield sharply increases with mass number from 70 to 100, and then sharply drops. This same figure gives a curve of the mass distribution of fission fragments, plotted by taking into account the yields of the nuclei identified in the experiment as well as the stable nuclei, and also by taking into account the long - and short-lived ones which could not be identified in the experiment. A calculation of the yields of the mass chains was made by assuming an equal displacement of charges of both fission fragments. It is clear that the nature of the distribution of experimental points and the corrected curve is similar. The curve has a peak for $A \sim 100$, and the width of the curve at half maximum is about 20 mass units. The nuclear yield at the edges of the central peak deviates from a monotonic type and is somewhat higher compared with that which should be expected from a monotonic type of curve.

Fig. 5 shows the relative yields of the products of uranium fission by nitrogen nuclei. It is apparent that the experimental points adequately define a curve with a broad maximum. The yields of the fission fragments within the mass number interval A from 90 to 145 remain almost constant. The half width of the curve constitutes not less than 50 mass units.

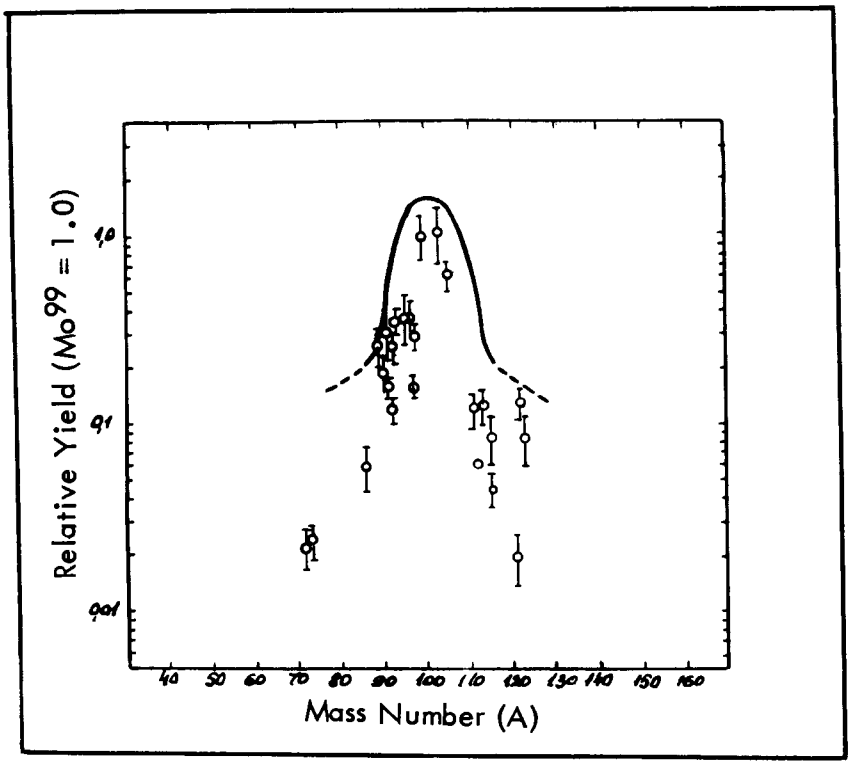

Fig. 4

Mass distribution of fission products produced during irradiation of gold by nitrogen ions.

o - Experimental values of the nuclear yields

—— Calculated curve, taking into account the yields of nuclei not identified experimentally

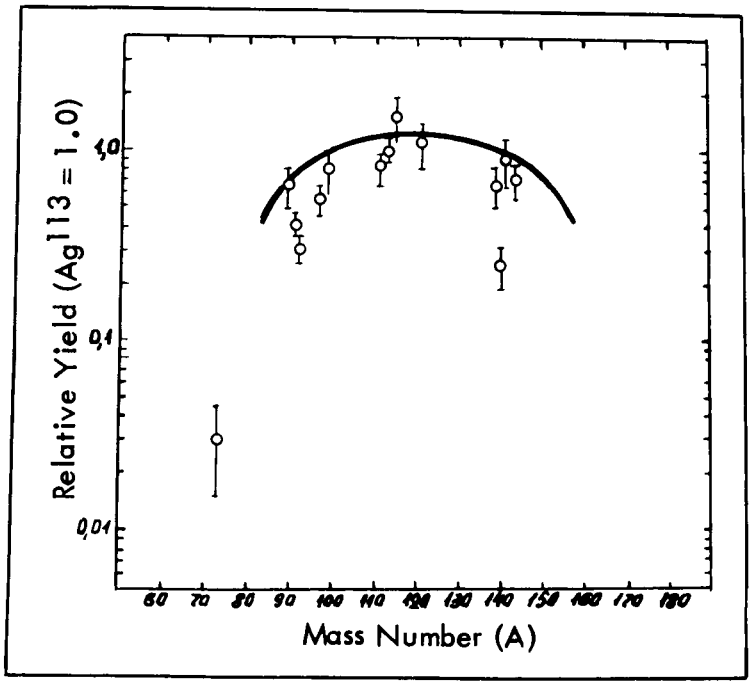

Fig. 5

Mass distribution of fission products produced during irradiation of uranium by nitrogen ions.

The mass spectrum of the fission fragments E_m produced during the irradiation of gold by nitrogen nuclei, is quite narrow; a symmetrical fission takes place.

For the fission E_s, produced during the irradiation of uranium by nitrogen nuclei, both symmetric as well as asymmetric fission is characteristic and results in a wide mass spectrum of the fission fragments.

The fission of uranium by neutrons produced during the irradiation of the target by nitrogen ions, as control experiments have shown, is insignificant compared with the effect of fission by irradiation with nitrogen nuclei and can not cause the wide mass spectrum of the fission fragments.

C. Production of Isotopes of Transuranic Elements in Reactions with Multi-Charged Ions.

For nuclei in which the ratio of the neutron width to the fission width, Γ_n/Γ_f is large, the fission cross section, σ_f, is considerably less than the total interaction cross section of the multi-charged ions with the nucleus. By measuring σ_f, it is possible to obtain certain ideas about the magnitude of Γ_n/Γ_f.

For nuclei with a small Γ_n/Γ_f value, the fission cross section practically coincides with the cross section for producing the compound nucleus and an estimate of Γ_n/Γ_f may be made by determining only the yield of isotopes which are not associated with fission.

To establish the nature of the Γ_n/Γ_f variation with an increase in the charge of the compound nucleus, experiments were performed to determine the yield of californium and fermium isotopes during irradiation of uranium by carbon and oxygen ions. The experimental results on irradiating stacks of thin targets of U^{238} with C^{12} and C^{13} ions are shown in Fig. 6. The californium isotopes were identified, after the proper chemical procedures, from the α-particle energy and the half life. By comparing the data on the fission cross-section of uranium by carbon with those on the yield of californium isotopes we find $\Gamma_n/\Gamma_f \sim 1/5$.

Experiments were also performed to obtain fermium by the irradiation of a thin U^{238} target with O^{16} ions with energy from 80 to 102 Mev. The isotopes were identified in the same manner as in the experiments with carbon. The cross section for the production of the Fm^{250} ($\sim 10^{-30} cm^2$) isotope was approximately 100 times less than that of

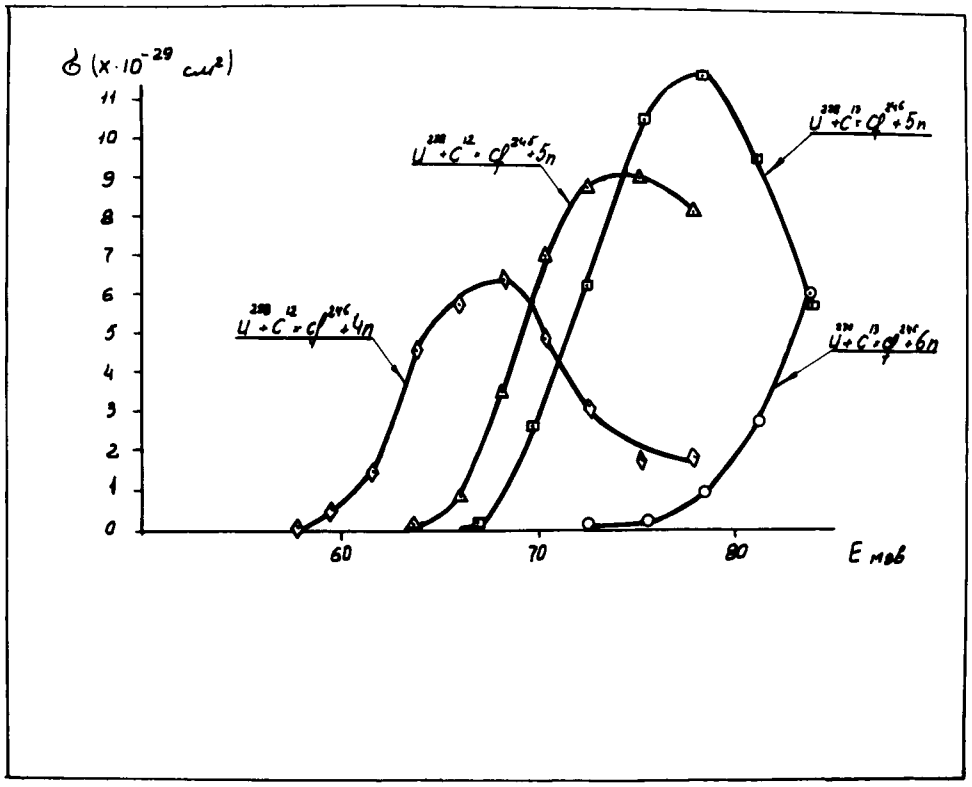
σ (x·10^-29 см²)
11
10
9
8
7
6
5
4
3
2
1
0
60
70
80
E мэв
U238+C12→Cf246+4n
U238+C12→Cf245+5n
U238+C13→Cf246+5n
U238+C13→Cf246+6n

Fig. 6

Cf in experiments with carbon ions, which corresponds to $\Gamma_n/\Gamma_f \sim 1/15$. In the irradiation of uranium with oxygen it should be noted that along with the reaction (O^{16}, 3n) there occurs the reaction (O^{16}, α4n) with a cross section which is about 10 times greater.

In addition to the experiments with uranium, Pu^{239} and Pu^{241} were also irradiated with 102-Mev O^{16} ions. After evaporation of neutrons, nobelium isotopes should be produced. An examination of the α-decay systematics indicates that the majority of the nobelium isotopes should have a short life; therefore, a method was used which makes it possible to observe the α-decay which occurs over a period of several seconds. The experiment was performed in the following way. A thin layer of plutonium on a nickel foil 1.5μ thick was irradiated with a O^{16} ions; the current was $\sim$0.3μa.. Nuclei produced in the reactions were ejected from the plutonium layer and impinged on a collector which was periodically shifted to a photoplate, placed two meters from the target. The photoplate recorded the α-particles from the radioactive decay of the isotopes produced. The exposure time of the collector near the target and in front of the photoplate could be varied over wide limits and the displacement time of the collector was 4-5 seconds.

To record the α-decay, a photo-emulsion of NIKFI-T-I was used. It is an excellent discriminator of proton and α-particle tracks and has a low sensitivity to β and γ-irradiation. The energy of the individual α-particles was measured with an accuracy of ± 0.3 Mev.

In the experiments on the irradiation of Pu^{239} the exposure time was 20 sec; in the experiments with Pu^{241} they were 20 sec and one hour. For each exposure time, 10-12 three-hour irradiations were performed. Fig. 7 gives the energy spectra of the α-particles observed for different exposure times of the collector. It is apparent that in the spectra corresponding to the Pu^{239} and Pu^{241} experiments, α-particles which had an energy of $\sim$9 Mev with an exposure time of 20 sec were observed. In the experiment in which the exposure time was one hour, only one α-particle with such an energy was recorded.

According to the systematics of α-active nuclei, α-particles of similar energy can be emitted by the nobelium isotopes $No^{251,252}$ produced in the reactions:

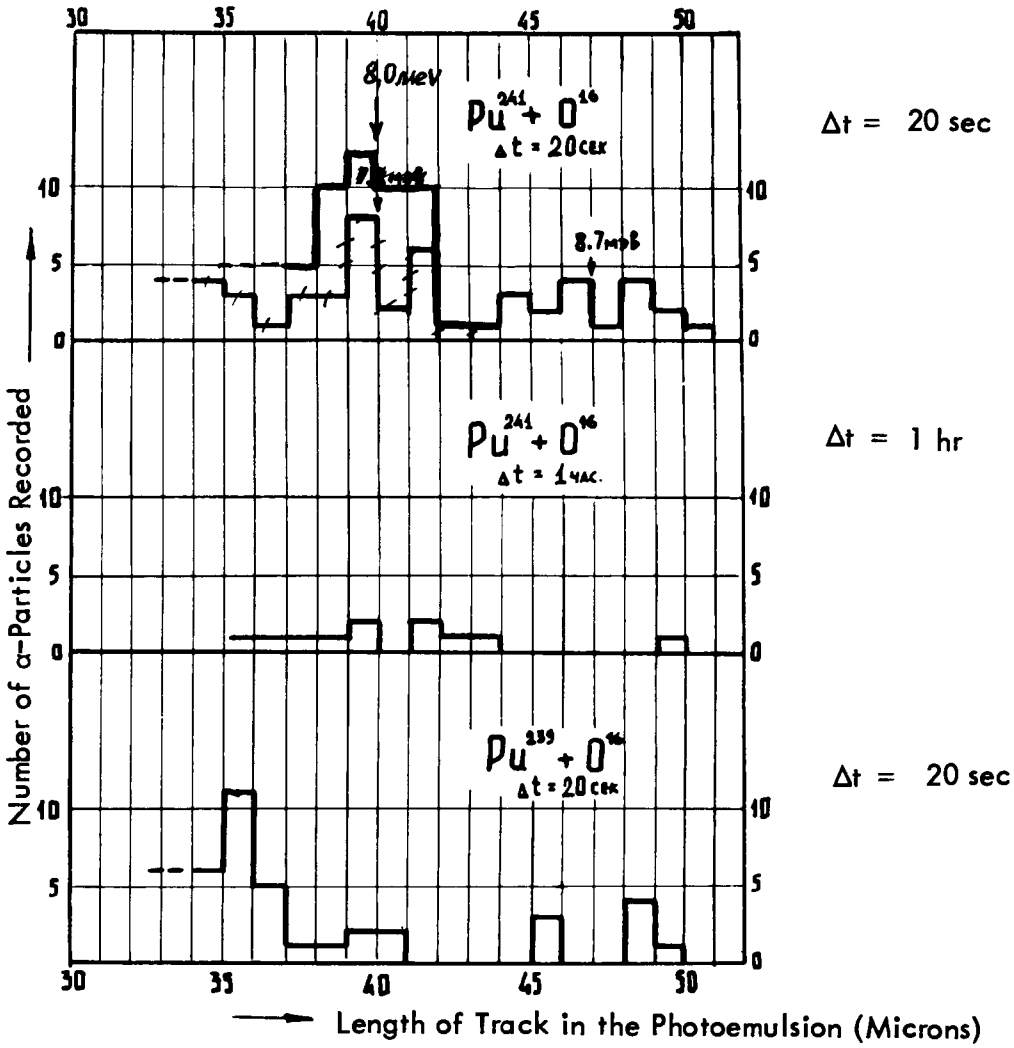
30
35
40
45
50
8.0 MeV
$Pu^{241} + O^{16}$
Δt = 20 сек
Δt = 20 sec
8.7 MeV
$Pu^{241} + O^{16}$
Δt = 1 час.
Δt = 1 hr
$Pu^{239} + O^{16}$
Δt = 20 сек
Δt = 20 sec
10
5
0
Number of α-Particles Recorded
Length of Track in the Photoemulsion (Microns)

Fig. 7

$Pu^{239}(O^{16}, 4n)No^{251}$
$Pu^{241}(O^{16}, 5n)No^{252}$
$Pu^{242}(O^{16}, 6n)No^{252}$

The total number of recorded α-particles having an energy >8.5 Mev was 18 in the irradiation of Pu^{241}, while it was 8 in the irradiation of Pu^{239}. This corresponds to effective reaction cross sections of 3.10^{-32} cm^2 and 5.10^{-33} cm^2, respectively.

For such small effects, a determination of the background becomes especially important. In our experiments the background could be caused by cosmic radiation, by neutron-induced reactions with the emission of energetic α-particles, and by contamination of the photo-plates and parts of the apparatus by products of the radioactive thorium family, including ThC' with an α-particle energy of 8.78 Mev. However, the background due to these factors does not depend on the exposure time of the collector around the target and near the photoplate. The absence of an effect during the irradiation with an exposure time of 1 hour indicates the smallness of the background.

Impurities of Bi and Pb in the target and collector constitute an additional danger. Unknown isotopes, emitting α-particles of ~9 Mev, may appear among the reaction products caused by oxygen ions in these elements. At the present time additional investigations are being proposed to clarify the effect of the above indicated impurities.

The data obtained on the yield of reaction products in the irradiation of Pu isotopes by O^{16} ions indicate a further decrease of Γ_n/Γ_f in going to the elements beyond fermium.

In addition to the isotopes obtained from a complete absorption of a multicharged ion with the target nucleus with subsequent evaporation of the neutrons, isotopes may be produced in reactions when a direct ejection of particles from the nucleus or a local heating of the nucleus occurs with subsequent evaporation of the particles. In such processes, the nuclear fission probability is less than when evaporation of the neutrons occurs from the compound nucleus. Therefore, the yield of isotopes produced during such processes may be comparable or even larger than of isotopes produced during the evaporation of neutrons. This phenomenon was observed in the work of a group of American chemists and physicists (Seaborg and others), who studied the production

of various isotopes during the irradiation by α-particles of fissionable nuclei. It is obvious that a similar situation may also occur in reactions induced by multicharged ions.

At the Institute of Atomic Energy, experiments were performed to ascertain the possibility of such occurences. Non-fissioning elements were used first. The formation of isotopes by the emission of two or three neutrons from the excited nucleus obtained by the irradiation of V^{51} with N^{14}, N^{15}, C^{12}, and C^{13} ions was investigated.

The results of these experiments are shown in Figs. 8 and 9 where it is seen that the curves for reactions with the emission of two or three neutrons change very slightly with an increase in the excitation energy; this apparently indicates the presence of reactions of a local heating of the nucleus.

PART II: EXPERIMENTS ON THE FISSION OF THORIUM

At the present time, experiments are being continued by a group headed by Prof. G. N. Flerov on the study of the spontaneous fission of Th^{232} and its fission by thermal neutrons.

To observe the fragments of the spontaneous fission of Th^{232}, large proportional counters with an operating surface of 8000 cm^2 are used. The use of these counters made it possible to improve the relationship between the pulses from the fission fragments and amplifier noise.

To decrease the background of cosmic rays the experiments were performed in a mine, 36-m deep. Not one pulse was observed during the operation which was 7000 gram-hours. This indicates that the period for spontaneous fission of Th^{232} is more than 10^{21} years.

Experiments were also performed to determine the fission cross section of Th^{232} by thermal neutrons; Sb + Be served as the neutron source.

To produce thermal neutrons, the Sb + Be source was placed in paraffin. The amount of natural uranium in the thorium used is determined from the number of fragments from the spontaneous fission and is 10^{-5}%.

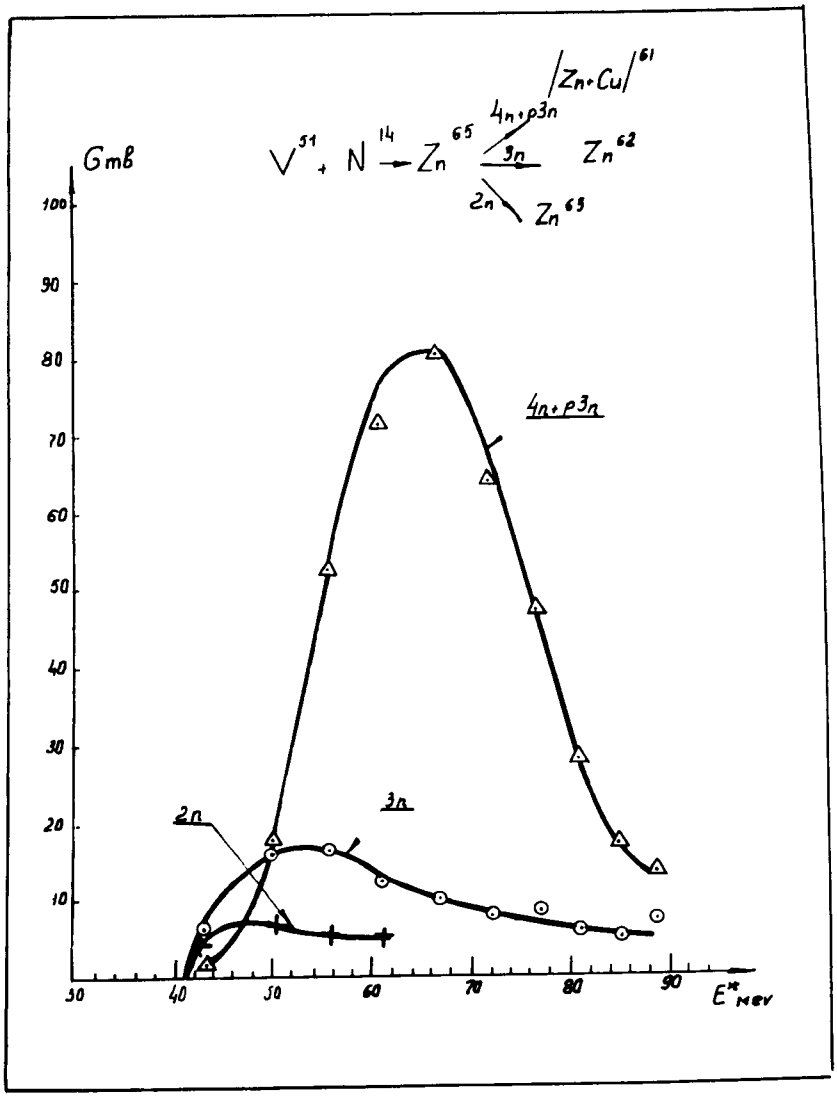
/Zn+Cu/61
4n+p3n
V51 + N14 → Zn65
3n
Zn62
2n
Zn63
Gmb
100
90
80
70
60
50
40
30
20
10
4n+p3n
3n
2n
30
40
50
60
70
80
90
E* MeV

Fig. 8

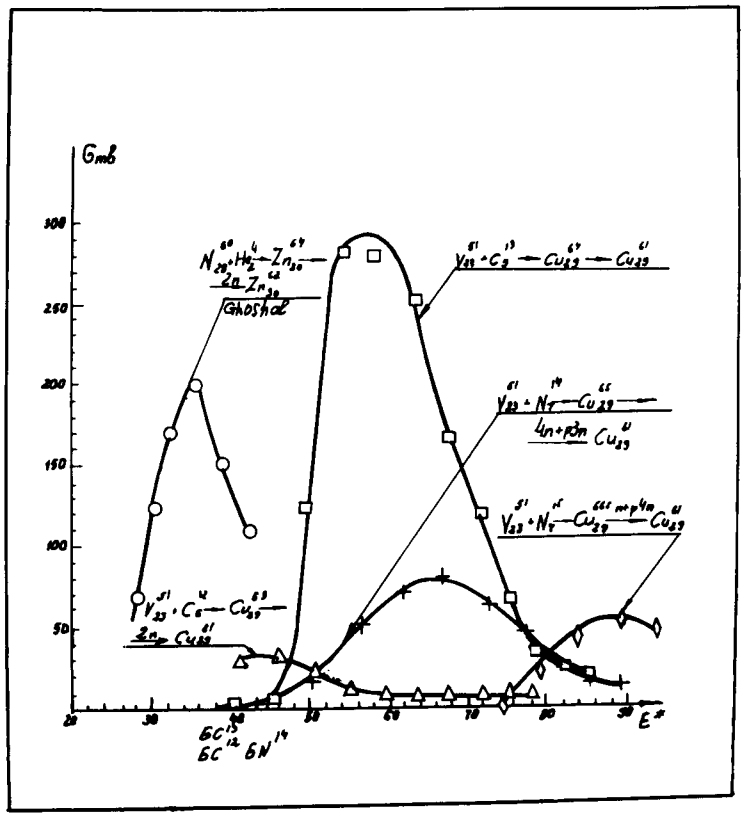
Gmb
300
250
200
150
100
50
20
30
40
50
60
70
80
90
E#
БС13
БС12 БN14
Ghosh al

Fig. 9

Preliminary experiments show that the fission cross section of Th^{232} by thermal neutrons is* . When the parrafin is removed, a certain number of fissions is also observed, apparently due to 400-kev neutrons.

PART III: ANISOTROPY OF THE ANGULAR DISTRIBUTION OF THE FISSION FRAGMENTS OF A SPINNING NUCLEUS

by G. A. Pik-Pichak

If the fission probability depends on the angular momentum of a nucleus[1] other competing processes (in this case, the evaporation of neutrons) must be taken into account to determine the dependence of the anisotropy on $x = (Z^2/A) / (Z^2/A)^{Kp}$, where Z and A are the charge and mass of the fissioning nucleus. The fission probability of the nucleus with momentum $\underline{j}$ and a projection of the momentum K, on the symmetry axis of the nucleus in the direction of $d\omega = \sin\theta d\theta$, where θ is the angle between the direction of the beam of impinging particles and the direction of emission of the fragments, is equal to

$$dW_f(j,K,\theta) = \frac{\gamma_f(j,K,\theta)\,d\omega}{\Gamma_n + \Gamma_f(j)} \tag{1}$$

where Γ_n is the neutron width, $\Gamma_f = \int_0^j dK \int_0^\pi \gamma_f \sin\theta d\theta$ is the fission width for a given momentum. When $j \ll j_{Kp}$, (j_{Kp} is the momentum at which the nucleus does not have a stable state[1]) and $j \gg 1$ the following statistical formula[2] may be used for γ_f.

$$\gamma_f(j,K,\theta) = \frac{\Gamma_f(0)}{j\,\mathcal{T}_1} \frac{1}{\sqrt{\sin^2\theta/2 + \frac{K^2}{j^2}}} \exp\left\{\frac{\hbar^2 j^2}{2\tau}\left(\frac{1}{J_0} - \frac{1}{J_A}\right) - \frac{\hbar^2 K^2}{2\tau}\left(\frac{1}{J_c} - \frac{1}{\mathcal{T}_P}\right)\right\} \tag{2}$$

where $\Gamma_f(o)$ is the fission width for zero angular momentum, $\tau = \sqrt{\frac{10\,U}{A}}$ Mev

*This information is omitted in the original report.

is the temperature, and U is the excitation energy of the nucleus. $\mathcal{J}_o$ is the moment of inertia of a spherical nucleus, $\mathcal{J}_a$ is the moment of inertia with respect to the axis perpendicular to the symmetry axis, $\mathcal{J}_c$ is the moment of inertia with respect to the symmetry axis of the nucleus. $\mathcal{J}_a$ and $\mathcal{J}_c$ are chosen for the deformation corresponding to the height of the fission barrier. If the moment of inertia is equal to that of a solid body, then

$$\frac{\hbar^2}{2\tau}\left(\frac{1}{\mathcal{J}_o}-\frac{1}{\mathcal{J}_A}\right)=\beta^2=\frac{\hbar^2}{2\mathcal{J}_o\tau}\left(1.2\,z+5.6\,z^2\right)$$

$$\frac{\hbar^2}{2\tau}\left(\frac{1}{\mathcal{J}_c}-\frac{1}{\mathcal{J}_A}\right)=\alpha^2=\frac{\hbar^2}{2\mathcal{J}_o\tau}\left(3.5\,z+8.75\,z^2\right)$$

where $z = 1-x$

Inserting (2) into (1), and summing over all K and averaging over j, we find the angular distribution

$$W_f(\theta)\,d\theta = d\omega\,\frac{\Gamma_f(0)}{j^2_{max}}\int^{j_{max}}\frac{e^{j\beta^2}e^{-\alpha^2/2\, j\sin\theta}\, I_o(\alpha^2/2\, j\sin^2\theta)}{\Gamma_n+\Gamma_f(0)\,e^{j^2\beta^2}\,\frac{\phi(j\beta)}{j\beta}}\,j\,dj = \int^{j_{max}} j\,dj\,\phi(j) \qquad (3)$$

where

$$\phi(j\beta)=\int_0^{j\beta}e^{-t^2}dt\,, \qquad j_{max}=\frac{1}{\hbar}\sqrt{2m(E-B)(R_1+R)^2}=\frac{1}{\hbar}\sqrt{\frac{2m\sigma E}{\pi}}$$

I_o is the Bessel function of imaginary argument; m, E, B, R, are the mass, energy, Coulomb barrier and the radius of the impinging particle; R_1 is the radius of the target nucleus and σ is the formation cross section of the compound nucleus.

Expression (3) gives the angular distribution for one stage of the cascade process in the evaporation of neutrons. For $\Gamma_f \gg \Gamma_n$, fission will occur without neutron evaporation and the ratio $W_f(o)/W(\pi/2)$ determines the anisotropy of fission. In this case (3) coincides with the formula obtained in ref. 2. For $\Gamma_f \ll \Gamma_n$, the entire cascade process of neutron evaporation must be taken into account. Then the anisotropy is determined by the relationship.

$$\frac{\sigma_f(0)}{\sigma_f(\pi/2)} = \frac{\sum_{i=0}^{N} W_{fi}(0)}{\sum_{i=0}^{N} W_{fi}(\pi/2)}$$

where i is the number of the cascade stage, and

$$W_{fi} = \int_0^{j_{max}} j\, d_j(j) \prod_{s=0}^{i} \frac{\Gamma_{ns}}{\Gamma_{ns}+\Gamma_{fs}}(j)$$

In W_{fi}, exp $(j^2\beta^2)$ remains in the numerator (for $\Gamma_n \ll \Gamma_f$ the exponent is simplified), which corresponds to an increase in weight of the states with a large momentum um and an increase in anisotropy. The essence of the matter is that for $\Gamma_n \ll \Gamma_f$ fission with various momenta is equally probable, while for $\Gamma_n \gg \Gamma_f$ basically nuclei with large momenta will fission.

If it is assumed that the dependence on Z of the moment of inertia with respect to the symmetry axis is determined exactly as for a solid body ($\mathcal{J}_c = \gamma\ \mathcal{J}_c$ solid body, where $\gamma < 1$), then for nuclei with $\Gamma_n \ll \Gamma_f$, γ may be found, and assuming that γ is uniform for all nuclei and is independent of temperature, the anisotropy for nuclei with $\Gamma_f \gg \Gamma_n$ may be found.

An experimental determination was made of the dependence of the anitosropy of fission on Z during a nuclear reaction with α-particles and deuterons.[3] A calculation made for nuclei for which $\Gamma_f \gg \Gamma_n$, i.e., nuclei undergoing fission at the first cascade level (U^{235}, Np^{237}, Pu^{239}), gives a value of γ in agreement with experiment. For Ra^{226}, $\Gamma_f \ll \Gamma_n$, and the calculation may be made only if a hypothesis is made on the equality of γ for Ra^{226} and Pu^{239}. In this case it is possible to obtain only qualitative agreement with experiment.

It should be noted that a test of the theory for $\Gamma_n \gg \Gamma_f$, as well as for $\Gamma_n \ll \Gamma_f$, may be made by studying the magnitude of $\sigma_f(\theta/\sigma_f)/\sigma_f(\pi/2)$. For $\theta \neq 0$ the ratio reaches a constant limit with increasing j, or with a decrease in temperature or the moment of inertia. Thus $\sigma_f(\frac{\pi}{4})/\sigma_f(\frac{\pi}{2}) \rightarrow \sqrt{2}$ for $j \rightarrow \infty$ and does not depend on any arbitrary parameters of the nuclei.

In conclusion I would like to offer my sincere thanks to D. P. Gretchukhin for discussion of this work, and to N. Halpern for his experimental data.

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PANEL DISCUSSION

CHAIRMAN BREIT: My function seems to be that of moderator, to try to keep these people from getting into each other's hair. But I presume that the idea is to have a little free expression of opinion, and to start things off I should like to ask Prof. Burcham to tell us what he thinks are the things to work on in this field and what he thought were things that were of special interest to him in the material reported on at the conference.

First Panel Speaker: W. E. Burcham

Well, Professor Breit, this is quite a hard assignment. I don't know that I can fulfil the request in any great detail because one needs time to digest the material that has come before us in these last few days. But I can at least say one or two things about some ideas we have had that go along with heavy ion work, in the light of a rather inefficient cyclotron and particularly for the rather higher energy ions, not the lighter or lower energy heavy ions such as we have heard about from Chicago. I think there are three things that I would just like to mention.

The first one is the neutron-deficient isotopes. It is trivial to say that the heavy ion machines are good at producing these things, and so clearly there is quite a lot of spectroscopy of the beta-gamma type to be done with the products of heavy ion machines. I shall not talk about the trans-californium work. That is not my job nor our particular interest. I should say that it is, of course, also possible to produce these particular neutron-deficient isotopes by spallation reactions in synchrocyclotrons, and the way we produce the neutron-deficient material is a matter of convenience. If you have one machine you can use, you use it. If you have another machine, you use it, but of course, you want a good intensity, certainly of the order of microamps of heavy ions at the very least. Once you have this there are a number of things you can do, and these are not particularly concerned with the heavy ions themselves which are only agents for producing the raw material.

One problem that we are interested in relates slightly to the point mentioned by Dr. Forsling. There is a considerable degree of ignorance about the ratio between electron capture and alpha emission in the neutron-deficient alpha emitters. I think that this will be worth studying and we hope to do something of this sort. I think it might throw some light on the deformation parameters in nuclei in the region of mass about 200. That is the first point.

The second one is directed to those people who have both a heavy ion accelerator and a high energy proton synchrotron or synchro-cyclotron. The combination of these two machines suggests that it might be worth while to compare the products of bombardments of a nucleus by high energy protons with the products of bombardment of a lighter nucleus with heavy ions in such a way that the same compound nucleus will be formed, or a very similar one. I don't know what a compound nucleus formed with a spin of perhaps 60 units looks like but I think it would break up in a rather different way, particularly with respect to fission, from a nucleus formed with the same excitation energy but by high energy proton bombardment. The general idea is that you can, of course, heat up a nucleus by absorbing a heavy ion into it. The heavy ion has such a low free path that we presume that it sticks in many cases. You can also heat up a nucleus by letting a high energy proton pass through it. This generates a nucleon cascade, but on the whole it seems to leave in the nucleus, as a result, shall we say, of Monte Carlo processes in this cascade, an excitation comparable with that found in the heavy ion experiment.

We know perfectly well that these nuclei are not really comparable because one has the high angular momentum associated with the absorption of the heavy ion, and it seems to me an experiment worth while doing to compare the decay products of these two. Whether it can be interpreted is quite another matter. I think one may, in fact, need to have a decent Monte Carlo theory or some sort of treatment of the first stage of this reaction so that one knows what is the excitation energy left in this nucleus. Then one can compare it with the results of a similar experiment in which one heats up the nucleus to the same

degree but by quite a different mechanism. Perhaps one would find the ratio between fission and spallation and perhaps this might tell us about the second stage of this particular reaction. There is, of course, a great deal of evidence on this type of thing in the literature now which remains to be interpreted.

The third point relates to the optical model. I am a little puzzled by the status of the optical model in heavy ion work, because my own conception of the optical model is that it is a thing that was invented to explain the apparent existence of a long mean free path for nucleons so that one would get resonances when the de Broglie wave length of the nucleon inside the nucleus was comparable with the nuclear dimensions. You remember the size resonances and also the transparency effects found in cross section measurements. It seems to me that applying the optical model to heavy ion reactions is something of an assumption. Let's put it this way: the heavy ion has, because of its strong interaction with the nucleus, a very short mean free path inside, and hence it would seem to be a good case for illustrating the relations between cross sections, which one had hoped to find on the basis of the original compound nucleus theory. In other words, one might hope to find elastic cross sections of πR^2 and inelastic cross sections of πR^2. Let's not say what R is, because there may be complications about direct interaction process.

One wonders if it would not be possible for somebody to make a study of the cross sections for heavy ions (including individual cross sections if they can be distinguished) to see whether the compound nucleus theory now works without the need for introducting transparency corrections. You would not expect these to appear in the heavy ion field. In other words, I am asking for a plot of $\left\{\sigma/2\pi\right\}^{1/2}$ as a function of $A^{1/3}$ and I expect some kind of straight line. The experiments may well be very difficult, and I think they probably ought to show quite different results from the work done with nucleons of the same energy. Compare, for instance, 100-Mev protons with 100-Mev nitrogen ions. That is the type of experiment that it would seem to me to be possibly worth doing.

CHAIRMAN BREIT: Do you mean on the basis of the same excitation energy approximately?

DR. BURCHAM: No, I think this isn't directed at excitation energy so much as at the mean free path phenomenon, because with 100-Mev protons the points obtained on a graph like this are dropping below the curve expected from the black nucleus. I would expect, if I did such an experiment with heavy ions, that they ought to be on such a curve but I may be quite wrong about this. I chose 100-Mev only because one would need something like that in practice to do the experiment and because in the neutron case it is about that energy where the transparency effects show up.

To extend this a little further, perhaps I should say that if one increases the nucleon energy in the nucleon-nucleus scattering, because of meson production the nucleus does become black, and finally above 300 or 400 Mev these points come back on a line more or less. This is because of mesons being produced and the thermal contact, as it were, between the initial nucleon and the nucleus being improved. So I wonder whether if such experiments were done with heavy ions they would be comparable with the nucleon experiments done above the meson threshold.

CHAIRMAN BREIT: These ions give you particles that are very much slower than 100-Mev protons. When they hit the target nucleus are the conditions comparable?

DR. BURCHAM: They are not comparable, but I think that with heavy ions one ought to be able to illustrate the principles of the Niels Bohr compound nucleus theory.

CHAIRMAN BREIT: I think I see your point of view. While you are discussing these things I wonder if you have any reaction on the results of Reynolds that were commented on by Blair in connection with the possibility of fission being explainable on the simple picture of the rotation of the nucleus that was seen. I understand, of course, it was a very preliminary suggestion, but it has a bearing on this matter of rotation of nucleus as a whole.

DR. BURCHAM: Yes, for this purpose I would want fission to be regarded as part of the inelastic cross section. I feel that with these very high orbital momenta it is probable the fission ought to increase in some way. The nucleus ought to fly apart and maybe the fission part of the reaction cross section is the majority of it. I think Dr. Reynolds said 90%.

CHAIRMAN BRIET: Thank you very much. I wonder whether perhaps we could have quite a different viewpoint now from Dr. Ghiorso regarding his reaction.

Second Panel Speaker: A. Ghiorso

First I would like to compliment the people who put on this conference, this being a very elegant sort of thing. I think that obviously the field is growing so fast that within the next five years there really should be one of these conferences per year. It seems that we ought to keep up with Gatlinburg. So I would suggest as a meeting place each year one of the centers for research where there are comparable facilities for the participants. For instance, Berkeley could use Yosemite Valley. I think people would come there. Yale could use Acadia National Park. There is a HiLac being built in Russia that should be done this year. In a few years perhaps we should go there.

I have been extremely impressed by all the work. I didn't realize there was so much in the heavy ion field. We have no participant from Berkeley to talk about the other nuclear chemistry work, and it must be clear to you people that we have a lot of people — Dr. Seaborg's group — who are going to do a lot of work. Actually, there has not been very much yet because time has not been available. The machine has not been working long enough really to give them a good start. We do have quite a number of people who are doing things, for instance, with fission, studying the mechanism by means of front to back ratios, recoil ranges, nuclear emulsion work, looking for Coulomb excitation of fission, and that type of thing.

We have a counter-experiment which is ready to start going, similar to what the Russians have done, but using a gas scintillation counter which should be in operation in the next few weeks. We have people doing spectroscopy work with neutron-deficient isotopes.

I might mention one thing, since it might be of use to other people. The recoil techniques which we have used obviously can be expanded very much, and I might mention just a couple of ideas. We intend to do the following work. One is to take the atoms which have been knocked out by the beam and stopped in the gas and conduct them over to a thin window, and then the subsequent decay of the particles by alpha emission that can go into this chamber can be analyzed in the conventional manner; eventually, that is, because obviously there are many problems. We know we can do this. Whether we can do it well enough to give a lot of information we are not sure, but ultimately it is bound to succeed, I think. We can expand this type of thing to have several moving belts, if you will, daughters, granddaughters, etc to prove, say, element assignment or isotopic assignment. This is clearly possible, and we have done a small amount in this direction.

It is also a nice tool for studying nuclear reactions. The following simple experiment will illustrate. You have a couple of parallel plates, a target here between them at one end from which the recoils emerge into the gas. Then if this plate is plus and this is negative, and say the recoils are collimated into a small solid angle, when each is almost completely stopped at some point, the electric field, which is very weak, will pull the charged atom to the negatively charged plate. Then by cutting this foil and sectioning it one can get the distribution of the recoils in the gas and get a range curve. By studying the type of function you can tell what produced it. We have done this and it works very nicely.

Another possible device that we have not yet tried but intend to involves a measurement of the drift velocity of the recoil atoms in a gas. Let us say we produce the recoils here and they are almost stopped in a gas; then we can conduct them over quite a distance by means of an electrical field. Of course, they will drift at a certain

velocity, depending on their ionic radius, the charge, and everything else, and if one is lucky and one has, say, a rotating drum here on which to catch these atoms, if you synchronize the drum with the beam bursts, depending on the migration time of the ions, they will end in a particular place on the drum. Depending on the time straggling, which will depend on many factors, then you will get a distribution on the drum which will tell you something about, with everything else constant, the ionic radius. One can use that measurement to show the difference between elements 104 and 103 because, as Prof. Seaborg has pointed out, the elements from 80 to 103 decrease slowly and continuously in ionic radius and then there is a sudden large decrease with element 104. Whereas going from element 96 to 103, the change in radius is about 11%, I think, going from 103 to 104 there is a drop of about 13%. So if your distribution is of the order of 5%, which is conceivable, then you can use this technique to show this abrupt change in radius.

One final thing, the discussion on transfer experiments stimulated some old thoughts in my mind and perhaps this is the place to mention them. A year ago we did some experiments where we bombarded thick targets of U^{238} with carbon and nitrogen ions, just to see what was going to happen, and we made these products: Th^{225} and Th^{226}, Ac^{224}, Ac^{225}, and Ac^{226}. These are very unusual reactions because you see you lost -- well, let's take this case (Ac^{224}), the difference between the target and the product is 3 protons and 11 neutrons. You have a lot of extra neutrons. The question is — what happens ? Well, ones first reaction to this is very straight forward. The heavy ions boil up the nucleus and out comes the stuff. But that isn't so for the very simple reason that the product atom has no recoil. It has very little momentum. Its range is less than 10% of the usual compound nuclei that we encounter; so the mechanism must be different. We feel that it has to be, and perhaps it gives some clue as to what these heavy nuclei look like, as if the neutrons are very dense on the outside.

Well, we are so intrigued by the suggestions about these transfer experiments and the lighter elements that we will go back and repeat this study and get some more data. These are extremely preliminary. They only consist of about three or four experiments done about a year ago, and we have not had time to repeat them and to get numbers which now will mean something. We have always been interested, but until now we have sort of pushed them into the background. With this stimulus we will study them.

CHAIRMAN BREIT: Perhaps while you are at the board you could tell us what you think about two other matters. One is the possibility of obtaining new transuranium nuclides that could not be obtained by other methods and the other is the matter of studies of other isotopes that could be produced, radioactive isotopes, in the work that is now in progress, I believe, at Berkeley, if you happen to feel like talking about those matters.

DR. GHIORSO: Obviously, by means of the new methods I have outlined one can take the recoils of a new element, a new isotope, or what have you, and bring them over to a detector in a matter of milliseconds at the most and thus pulse analyze in between the beam pulses, which on our Berkeley machine is 2 μs long; so you have ample opportunity then to examine everything down to milliseconds half life.

Now as far as new elements are concerned, the likely isotopes won't have half lives of microseconds until you get to around 106 or so. But the real problem there is yield. The competition of fission is so severe, because of the excitation that we have to give to get the particle into the nucleus, that pretty soon you have nothing. Already we are working with cross sections in the range of microbarns; this is not borderline but it is not very far from it. However, I expect that within the next year element 104 is pretty likely to have been discovered some place. Element 103 we feel we can make now. Whether we can identify it or not is another question. We think we may have seen it but actually to prove it is 103 is another matter. Element 104 might be easier than 103 because it gives an even-even daughter. Ultimately people are bound to go beyond this. The techniques will have to become more and more sophisticated, and it will take more time.

more beams, and everything else. We are limited by target material at the moment. The heaviest target material one will use is probably E^{254} or possibly the unknown isotope, Fm^{257}. This is a question. If it only has a half life of a couple of days, it is out. If it has a half life of a couple of weeks, it is possible. In this country we plan to produce milligrams of Cf^{252} in the next few years and consequently micrograms of einsteinium and ultimately one is bound to get to Fm^{257}.

As far as other isotopes are concerned, there are some very fertile fields. For instance, at the region around 126 neutrons with alpha emitters, the cross sections there are thousands of times larger than for the heaviest elements and they stay quite large, so that by using techniques like this or spectrographs you can certainly make isotopes say of element 96 with mass numbers of 220 or so. The heaviest mass of curium known today is Cm^{250}. You could have Cm^{250} on one side and Cm^{220} on the other side which would be quite a span, and this will perhaps give you another interesting test of the theory of magic numbers on both sides. It gives you ample opportunity to study nuclear systematics by means of alpha-decay cycles which can be made quite rigorous.

CHAIRMAN BREIT: In your experience how many atoms of a new element do you need in order to identify it?

DR. GHIORSO: I think it depends on the experiment. To my way of thinking one atom would be sufficient if you felt that you could reproduce the experiment at will and you really believed it. I think the experimenter himself is usually the harshest judge of whether he has proved something. In our latest experiment with 102 we did not have that problem. We were able to make as many as 40 per experiment. If we wanted to do it week after week we could have thousands in a short time, but that was not the crucial point there.

I suspect that when you are "looking" in between beam pulses or at nuclear tracks, at some point people begin to argue among themselves, "Is it real?" and some will believe it and some won't and again it depends on the experimenter. We try to be extremely conservative at Berkeley. We don't feel that we would like to risk our reputation

by making any claim that is likely to be disproved. I think people by and large are this way, and if you are misled honestly because the experiment looked all right and it just didn't turn out the way you thought at the time.

It certainly is possible to do a remarkable amount with a very small amount of activity. If you can show that this is unique and you can satisfy yourself that nothing elese can have these properties or satisfy the requirements of the experiment, that is enough, I think. These are more subtle problems, such as how have you proved that you know the atomic number and I imagine there will be disagreement as to when you have completed your proof.

CHAIRMAN BREIT: Thank you very much. Dr. Bromley, we would like your comments.

Third Panel Speaker: D. A. Bromley

I think that most of you know that the interest of our group at Chalk River in the heavy ion field is motivated largely by the fact that we hope to install our HVEC tandem accelerator shortly. There are several aspects of the heavy ion field which appear promising to us although, at the beginning at least, this will not be our major field of interest. We certainly do not wish, nor would it be profitable for us to try, to compete in aspects of the field which are readily covered by any of the other heavy ion accelerators.

At Prof. Breit's suggestion, I should like to mention very briefly a few of the topics which occur to us as of interest with the tandem.

We anticipate that we shall be able to accelerate all ions up to and including phosphorus, to energies up to $(Z + 1)V$ where V is the terminal potential in the accelerator. The unique characteristics of this accelerator are of course, the precise energy control and ease of energy variation. Consequently, it is almost certain that one of the first things which we will do, perhaps even under some coercion, is to determine the range-energy curves for some of the heavy ions in common use. The need for such data has been mentioned repeatedly during this conference.

One of the questions which is of interest to us is that of parity determinations via Coulomb excitation measurements. In particular in the heavy elements it will be interesting to try in a systematic way to obtain further information on the low lying 1^- members of the ground state, K = 0 rotational bands of even even nuclei: these correspond to the onset of pear-shaped deformations. As Dr. Newton has shown in his paper it should be possible to excite these levels at excitations of ~ 1 Mev with relative ease by using heavy ions. The technique, as you know, is to take advantage of the energy control and variation of your accelerator to either determine the excitation functions for the Coulomb excitation process or, alternately, the relative yields at selected bombarding energies, in both cases using two different bombarding ions, e.g. O^{16} and O^{18}.

Classically, one can write the Coulomb excitation cross section as[1]

$$\sigma_{CE} = \left(\frac{Z_1 e}{\hbar v}\right)^2 a^{-2\lambda + 2} B(E\lambda)\, f(\xi)$$

where a is one half the distance of closest approach in a head-on collision and B (E λ) is the usual reduced transition probability. λ is the multipolarity of the transition; ξ is a dimensionless quantity

$$\xi = \frac{Z_1 Z_2 e^2}{\mu v^3} \quad \omega_{if}$$

where $h\omega_{if}$ is the energy of the transition. By appropriately choosing the energies of the two bombarding ions to give equal ξ, the factors $f(\xi)$ cancel from the predicted ratio of cross sections leaving

$$\frac{\sigma_{CE}^{(1)}}{\sigma_{CE}^{(2)}} = \left(\frac{v_2}{v_1}\right)^2 \left(\frac{\mu_2 v_2}{\mu_1 v_1^2}\right)^{-2\lambda + 2}$$

and comparison of the experimentally measured ratio with this expression, in principle at least, allows a determination of λ, hence of the parity change in the transition. In canceling the $f(\xi)$ for equal ξ the tacit

assumption is that the range energy relations of the ions used are identical. The only measurements to date of this type are those of Temmer and Heydenburg[2] with protons and alphas on Na^{23}; here this assumption is not valid and thin targets were used to minimize the effect. Kuehner, Almqvist and I are setting up at Chalk River to use He^3 and He^4 to test the technique. Unfortunately, as one goes to these and heavier ions of equal charge where this assumption is more nearly valid, the magnitude of the effect drops rapidly, necessitating accurate measurements.

For some time now there has been growing evidence that in direct interaction reactions, such as deuteron stripping and more recently in (He^3, α) reactions[3], appreciable amounts of the so-called heavy-particle stripping are required to fit the experimental observations[4]. This stripping is characterized by strong backward maxima; to date, however, depending upon the ingenuity of the vacuum plumbers involved, angular distribution measurements have usually terminated in the 150 to 160-deg range - often with a tantalizing upward twitch. It is clear that a knowledge of the distribution at larger angles would be extemely useful for a better understanding of the mechanism. The obvious method of course is to convert $C^{13}(He^3\alpha)C^{12}$ to $He^3(C^{13}, \alpha)C^{12}$ or $C^{12}(dp)C^{13}$ to $H^2(C^{12}p)C^{13}$; we hope to obtain information both on the differential cross sections and on the particle reduced widths in this way.

A closely related and very interesting topic is that of the transfer reactions[5]. It seems certain that the tandem characteristics willbe well suited to a study of the mechanisms involved here.

There are clearly a great number of promising problems. I should like to conclude by mentioning one which occurs to me as a result of discussions at this conference regarding the high angular momenta involved in heavy ion reactions and our recent work at Chalk River with the collective model applied to nuclear spectroscopy[6]. I believe that heavy ions may provide an effective tool for examining the relationship between the shell and collective nuclear models. As you know, if you start with a hydrodynamic approach and a nice stable nuclear deformation the number of states expected in a rotational band is not limited,

i.e. if you start with J = 0, you find J = 2, 4, 6, etc. and this goes on ad nauseam. On the other hand, if you are familiar with the recent work of Elliott and Flowers on the interrelation between the shell and collective models[7] you will recall that, working from the shell model, a definite termination is predicted for the rotational bands. For example, in Ne^{20} the ground state K = 0 band would be expected to terminate at J = 8, i.e. no J = 10 member should be found. Unfortunately, in the case of Ne^{20} the deformation is not large and appreciable inter-band mixing would be expected to strongly distort the normal spectrum.

In the regions of large stable deformations, such as in the rare earths (e.g. Ho^{166}), the terminating J values are much higher, however it should be possible, with heavy ion reactions, to form these states (at relatively low excitations because of the high moments of inertia) and study the subsequent deexcitation. Cascade E2-gamma deeexcitation of the band members competes most favorably with particle emission here because of the relative enhancement of transitions within a band. There is the fond hope that by working with this problem long enough one will be able to say whether the rotational bands terminate - and if so at the predicted J - or continue beyond this to the point where they can no longer be resolved with existing experimental techniques. Such information would be of crucial importance in understanding the foundations and interrelations of these models.

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CHAIRMAN BREIT: Thank you very much. You have been in this game a long time, Dr. Zucker. I wonder what your plans are and what you think of the various suggestions.

Fourth Panel Speaker: A. Zucker

Well, I think all of these ideas are fine. But I would like to discuss first of all something that came up throughout the conference and is generally called nuclear chemistry, using the statistical model. I like to think that in the future we will place rather less emphasis on this. We have privately decided that the way to look at nuclear reaction models and nuclear properties is not by way of activation experiments. I think all of you know that there are too many parameters which tend to confuse the issue.

The nuclear chemistry program that we have envisioned is directed to detection of low cross sections for some rather exotic things. The hope there is that you will see them and notice something new perhaps. One of them which was mentioned by Dr. Pinajian was a double transfer reaction, and I would just like to write it down so that you can see what it looks like. It is, say, $P^{31} + N^{14}$ going to $Si^{31} + O^{14}$. The idea is to have two nuclei switching over at the same time. That is, the neutron going one way and the proton going the other way. The obvious question is does this have a very low cross section, is it a product of two cross sections, in which case we would not see it, or is it a process which happens with an observable cross section?

In this same class, I think is the program that is being carried on by the Birmingham group on de-excitation by gamma rays only. I think this ought to be approached with great caution and that one ought to try to produce a compound nucleus with as little excitation as possible.

Another experiment I think ought to be done, but which we cannot do with our machine, is to look for Coulomb-excitation fission and perhaps look at it in coincidence with the inelastic scattered nitrogens. This will require some isotopes which are much more fissionable, probably more fissionable than uranium, and it will certainly require more energetic particles than we have.

I would like to say something about angular momentum, perhaps in the sense in which Professor Burcham talked about it. The effect of angular momentum on nuclear reactions is really not very well understood, and I would like to point out that with heavy ions one can produce rather startling things in that respect. If you bombard nucleus A with ion B, say, then you produce the compound state C. If the energy of particle B is just at the barrier then, of course, you get $\ell = 0$. Well, maybe 1 or 2 ℓ-waves that are important. However, if you increase the energy just slightly, say by 5 Mev you will get up to maybe 15 or 20, the while not changing the excitation of the compound nucleus very much. Then if you look at the reaction products which are emitted from such a reaction you would hope to find something. We have looked at this and we haven't found anything startling, but I think more work ought to go into this. Perhaps particles ought to be used to carry more energy than nitrogen does so that the amount of kinetic energy plays a smaller role than it does in our reactions.

I think the transfer reactions from the standpoint of low energy nitrogen ions are probably the most fascinating ones. We have devised a scheme with which we hope we can identify transfer reactions. It uses the same chamber that Halbert described yesterday where you have a target and two movable scintillation counters. The hope is that by choosing the angles properly – one can limit the coincidences to only those reactions which go by transfer reaction. Say $N^{14} + Be^{9}$ going to $Be^{10} + N^{13}$. In this case, of course, you have to choose something where low lying excited states are well separated or are unstable to particle emission. Nitrogen-13 is very nice that way because it has no excited states stable to particle emission, but Be^{10} unfortunately does. In this particular experiment taking advantage of the kinematics and by choosing very small apertures one can discriminate ont only against

elastic scattering but one can discriminate against inelastic scattering and many other transfer reactions which also happen. One can obtain $O^{15} + Be^{8}$ as well as many other reactions. The experiment is quite complicated, but I think it is really worth while. That is, to understand transfer reactions properly you have to know the state to which it goes and you have to have an angular distribution for each state; otherwise I think you will be dealing with a sort of semi-quantitative result, such as total cross sections with which, we have seen, it is quite difficult to work.

Another thing which has come up lately at Oak Ridge is the case of the Coulomb barrier. There is some reason to believe that for highly excited nuclei which are produced on the Oak Ridge proton machine, the barrier seems to be lower than it should be. That is, alpha particles come out below the Coulomb barrier more copiously than they should. Here there is a rather clear case for heavy ions, because heavy ions can pour a lot of excitation into a nucleus, and it is very easy to look at alpha particle spectra in such a case. For low energy ions, such as we have, the situation is not too clear cut because the alpha particle spectrum is distorted due to recoils of light residual nuclei. When we see an alpha particle which appears to come out below the barrier it may actually be that the recoil particle has taken off the energy, and the alpha particle is emitted forward from a nucleus moving backward. With heavier targets where recoil energies are not so important, I think one can examine the detailed effect of the barrier on particles leaving highly excited nuclei.

CHAIRMAN BREIT: Thank you. I think possibly there might be ideas concerning this last remark that the other people at this table or possibly the audience might wish to comment on.

It would seem that in a general way one has two kinds of approaches. One is the use of the heavy particle as a probe, in which it acts in an essentially new way and can give an electric field which causes Coulomb excitation of the target and causes it in a way that is different from that available otherwise, or in which it furnishes the neutrons and protons which can go across a gap and can determine the reduced widths and in this way aid in forming better nuclear models.

The other way is one in which this heavy particle is made to hit the nucleus and form nuclear matter, speaking somewhat generally, in a new state. This might teach us something regarding the behavior of nuclear matter as a whole. I think largely we have had more discussion about the latter kind of thing than the former but there will doubtless be interest in both.

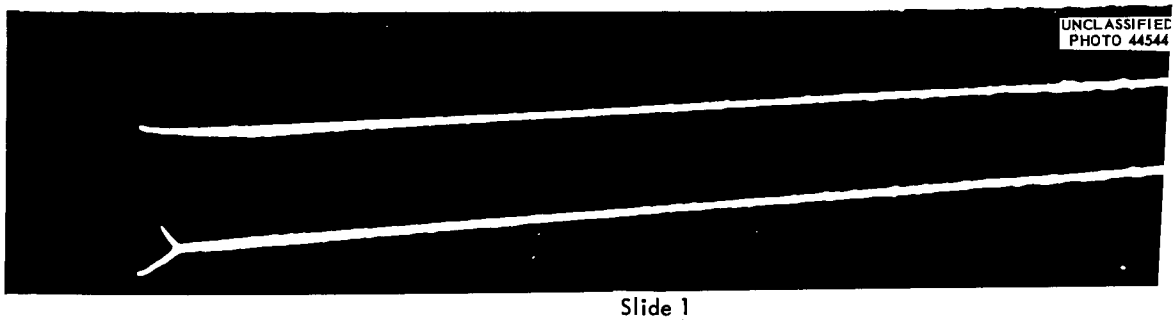
I think by now Professor Burcham is in a postion to show a couple of slides.

DR. BURCHAM: I am sorry to switch the thoughts of the meeting to quite a different subject but it is one that is relevant.

Slide 1 is a picture of nitrogen ions stopping in helium. Of course, there is maximum ionization somewhere near the end of the track but this is not chosen with any particular purpose in mind except to show the sort of things you see at the end of nitrogen tracks. I would like to suggest that the people here who are in the unfortunate position of having beams with only 1 or 2 particles coming out at the end of the tube might be inclined to conduct studies of charge transfer and energy loss, leading up to the calculation and verification of the ionization curves.

I am sure that there is quite a lot of work to be done on this subject relating to the effective charge of the ion. Some work has already been done, as you know. I am sure now, with the many ions that we have, that this can be much extended. One little thing to illustrate this is shown in Slide 2. This is a measurement of the ionization due to nitrogen and carbon ions. Comparison is made with the well-known proton ionization curve corrected for the effective charge of the heavy ion as measured by various deflection experiments. The actual shape of the curve should not be taken too seriously because the fall off on the righthand side is more due to averaging over the beam energy than to the ionization. In fact, it is possible to get somewhere near the observation by a suitable type of effective charge calculation.

I am sorry to have taken the time of the meeting on something as elementary as a Bragg curve at this stage of the deliberations, but I think that these things have some intrinsic interest.



Slide 1
Nitrogen ions stopping in helium.

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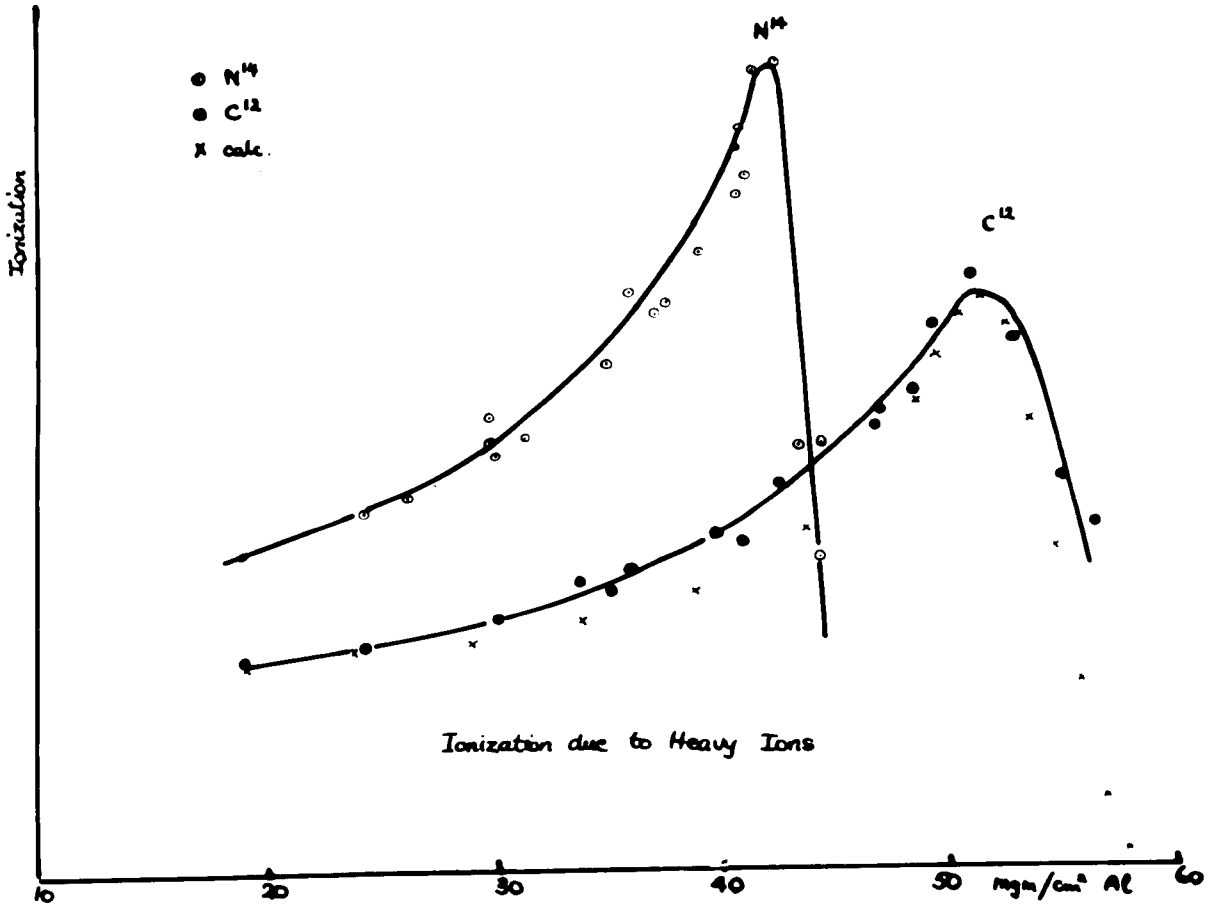

Slide 2

Ionization due to nitrogen and carbon ions.

DR. GHIORSO: How did you take the first slide?

DR. BURCHAM: The picture of the track was taken in an ordinary Wilson chamber, with 60 cm helium pressure, and I should say also in that connection that other pictures in the same series do show the expected delta rays extremely clearly. It might be worth while to see if the delta rays obey the required law, because identification of the charge by means of delta rays is an important matter for cosmic radiation, and this certainly ought to be checked up.

CHAIRMAN BREIT: Thank you. I have heard several people talk about about such measurements and it seems clear that they are a necessary part of much of the work that is being planned in various laboratories.

Before the meeting adjourns I think it is desirable to remember that the organization of such a conference is a very difficult matter and also that the Oak Ridge National Laboratory and in particular Messrs. Livingston and Zucker have done a lot of this very useful work and have furthered the subject by having taken the initiative in organizing this meeting and by arranging for the meeting to be profitable.

I propose, this being the first of such meetings, a resolution to go ahead and tell others that they consider the subject to be important, and that they are willing to go to the trouble and actual work which they have done, deserves special recognition. I propose, therefore, a rising vote of thanks to our hosts.

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