HEAVY ION LECTURE SERIES

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Kansas State University
Manhattan, Kansas

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HEAVY ION LECTURE SERIES

KANSAS STATE UNIVERSITY

1972-73

VOLUME I

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MASTER

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HEAVY ION LECTURE SERIES

The Department of Physics at Kansas State University has a research program in atomic and nuclear physics that emphasizes the use of heavy ions (ions of elements heavier than helium). This program, which is based on experiments at the model EN tandem Van de Graaff accelerator, has been in operation since 1969 and is supported by the Atomic Energy Commission and by Kansas State University.

During the academic year 1972-73 a special series of invited talks was given by noted physicists who work in the areas of atomic and nuclear physics using heavy ions. This series of seminars was supported by Kansas State University. The publication of these talks has been made possible by Kansas State University and the Atomic Energy Commission.

Volume I of these lecture notes includes transcripts of the talks by four of the eight speakers, with two atomic and two nuclear physicists represented. Volume II will be similarly composed. The talks represent a useful survey of current interest in these areas and, to some degree, the state of the field at that time. Since the speakers were encouraged to give informal, speculative talks, this manuscript is for background information only. These notes have not been professionally published and, therefore, may contain errors. *Consequently, no statement in these notes should be referred to in a publication without the speaker's agreement.*

The process of transcribing the notes from tape, re-typing the edited versions, carrying out the process of layout, and typing the final copy has been admirably performed by Dea Richard. The editors also wish to thank the faculty of the tandem Van de Graaff accelerator, Dr. J. C. Legg, Director, Dr. C. L. Cocke, Dr. J. S. Eick, Dr. J. R. Macdonald, and Dr. P. Richard for numerous cases of assistance in manuscript editing. The interest and support of Dr. C. E. Hathaway, Head, Department of Physics, is gratefully acknowledged.

J. D. Spangler
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Professor D. Allan Bromley

Professor Bromley, Chairman of the Department of Physics, Yale University, Director of the A. W. Wright Nuclear Structure Laboratory, and Henry Ford II Professor of Physics, is noted for his extensive investigations into nuclear reactions between complex nuclei. He has also served the physics community as Chairman of the National Academy of Sciences Physics Survey Committee which in 1972 produced a comprehensive report on the status of physics education, manpower, and research quality and support.

HEAVY-ION SCIENCE - A BRIEF OVERVIEW

Let me say at the onset that I am delighted to have this chance to come to Kansas and talk to you. My understanding is that these talks are to be informal.

What I want to do in this first lecture is talk about the whole field of heavy-ion physics--a major part of heavy-ion science, but not all of it--and try to give some kind of perspective, some kind of framework, and some discussion of where the field is going. In the subsequent talks I will show a large number of slides, discuss many details, and illustrate with a number of programs, in which we have been involved at Yale, the kinds of things that can be done in this particular area of nuclear science. Today's talk will provide a general overview; and in the next two I shall go into more detail, with more examples of specific experimental and theoretical work.

It is worthwhile to take a moment to provide some historical perspective. Where did the field start? This happens to be one of the few fields of science where it is possible to identify the beginning directly as to time and place. It turns out that the man responsible here was also instrumental in getting many other fields of physics underway. He is Gregory Breit, a former colleague at Yale. The original impetus for heavy-ion science came about when the Manhattan Project officers asked Gregory, "What would happen if this device we are going to test were to initiate the nitrogen-nitrogen reaction in the air [$^{14}\text{N} + ^{14}\text{N} \rightarrow ^{28}\text{Si}$]? Gregory thought about it for a while. Then he said, "Nobody really knows." And he was right. No one had the slightest idea whether this could happen or not; but in principle, it could have happened. Gregory went to work, and a few weeks later, on the basis of his theoretical calculations, said "It won't." Which, in retrospect, was one of the safest predictions anyone ever made, because if he had been wrong, believe me, there wouldn't have been anyone around to question it!

That was the beginning; the time was just about at the end of World War II. Then, in the early 50's, one of our graduate students, Alexander Zucker, who was leaving Yale, went to see Gregory and said, "Professor Breit, what would be an interesting field to work in at this time?" This was about 1951. Gregory thought a bit and said, "Nobody really knows what happens when you smash one large nucleus into another. Why don't you find out?" Zucker went to Oak Ridge and there converted one of the calutrons, one of the hundreds of devices that had been separating isotopes during the war, to a cyclotron--specifically to a nitrogen cyclotron. The first papers were published in 1953 on the interactions of nitrogen with a wide range of targets. It was a pretty crude machine and there wasn't much that could be done that could really be understood other than nuclear chemistry, where the basic idea is to get nuclei together and see what happens.

Then, in about 1954, Breit, working with E. O. Lawrence then at Berkeley and some of my present colleagues at Yale, Beringer and others, convinced them and the AEC that we should build HILACS, heavy-ion linear accelerators. Two were designed and built, one at Berkeley and one at Yale. They became operational in 1958, and accelerate everything from helium to argon. Greg Seaman and I have the unique distinction in this room of being the only people to have worked with these machines. They can be dreadful machines to use for physics. It is very much like taking your wrist watch, putting it on a flat rock, and hitting it with another one and commenting over the fragments, "God, that must have been complicated."

There is a better way, fortunately. At about this same time my colleagues and I at Chalk River convinced our administration that the time had come to build what at the time was called a swindletron, but which now has the more prosaic title of tandem. Our first tandem became operational in 1959, and it has been an expanding field ever since with cyclotrons, bigger tandems, other LINACS, and you name it. That essentially is a vest pocket history of how we got here.

Within the last two years there have been a number of

surveys as to what is interesting in nuclear science. The answer comes out that two things are interesting--actually three things. You have to bear in mind this characteristic of nuclear physics: The field is somewhat unique in all of physics because so many of its phenomena depend on having detailed knowledge about a lot of entities. The nucleus lies between the few-body problem, which allegedly characterizes particle physics, and the complete chaos that characterizes condensed matter. I use chaos in its biblical sense here. Because of this, in nuclear physics we have had to do a lot of work. Many people at many places have contributed to building up the systematics so that finally we can make some sense of it. This we must continue, because it is the base for any future work. But what now are the two major frontiers, the areas of major thrust, that come out of all these discussions.

One area is that of higher energy and the other is that of heavy-ion science. High energy, first because we need to reach inside the nucleus and probe what goes on deep down inside it--to find out how particles are put together inside nuclear matter if you like--and second because we want to be able to go in and pick out individual nucleons by having a very short wavelength probe. In the process, of course, we also want to put in muons, hyperons, pions, and so forth, and do all the exciting things we don't have time to talk about here.

The other area is that of heavy-ion physics, and I am going to try to illustrate some of the reasons why we think this is an important area in this talk. Up to now, it is fair to say that most of the work in this field has been of an exploratory nature; it has concentrated on mapping the terrain; it has been telling us the sort of things we hope to do. We haven't done all that much yet, but we are beginning. In my next two lectures I want to show you some of the things we can do that point the way to specific physics.

To give you some feeling for what we are trying to do, Fig. 1 is a slide I obtained from G. N. Flerov in Dubna. It is well to bear in mind that the stable nuclei number only 300. They

Fig. 1

are represented by black squares in this figure. We used to tell our students that the cutoff in nature was at uranium. It isn't of course. As you know, we now know that we have plutonium in nature, too. In the several decades that we have been studying nuclear physics, we have discovered some 1300 radioactive isotopes. That gives us the small center area on the diagram outlined by the notched peaks. If we look on this as a binding energy map, this is the so-called valley of stability. We have been working down on the surface of the map; we really know very little about what happens when we pump energy into a nucleus. We will come back presently to the question of how we get up off the surface. But more important, how do we get outside the valley? There is a lot of terrain out there. The outer boundaries are calculated from work that Garvey and Kelson²⁾ did at Yale a few years ago. The lower boundary is the neutron-drip line. It locates those species where the neutron binding energy is zero. If you pump more and more neutrons into a nucleus, eventually they are not bound at all, and they drip out. At the upper boundary you might expect that protons would come out, but actually you

gain by putting two protons and two neutrons together, and alpha particle emission competes favorably with proton emission. For very heavy nuclei you find spontaneous fission.

The situation, in a real sense, is a little like asking someone to describe the geography of the United States from a detailed study of the floor of the Grand Canyon. The question is, "How do we get to see what is going on in the wings?" We all know about fission. The fact that protons experience electrostatic repulsion means that the valley of stability doesn't run up at 45° on this figure, so fission takes us to the region that is neutron rich. How do we get to the region that is proton rich? It turns out that heavy ions are ideal for that purpose. A heavy ion--say something like calcium or fluorine--moves us on this map at 45° , taking us up into the proton rich region. We can then boil out a few neutrons, because we have the energy to do so, and thus heavy-ion reactions open up the whole proton rich area.

Incidentally, a question that is frequently asked is, "Who cares about what is going on up there?" Let me give an example. We have come finally, to the age of tailored isotopes. We talk to a clinician, a medical type, who wants to do something using radioisotopes. Let's take the thyroid as a case in point. He wants an element that goes to the thyroid; you pick iodine. But if you take ^{131}I , and use it to irradiate the thyroid, you find that in addition to doing what it is supposed to do, it irradiates the hell out of the patient's head and stays around too long. So you ask, "What would you like?" The clinician says, "I would like something that lasts about 6 minutes and has no gammas, just betas at this energy." So you go back to the accelerator and grind out ^{123}I , which happens to have just these characteristics, and that is what is being used now. That is just an example of a completely new area that we can begin to tackle.

What about coming up out of the end of the valley? You all know about the new supertransuranic islands of stability--the Holy Grails. Per unit nucleon there has perhaps been more garbage written about this area than anywhere else in the periodic table. What about these super-heavy nuclei? Because we have begun to

understand what is happening in heavy nuclei in the lead region from reactions with light projectiles, we can predict what should happen in the supertransuranic region studied with heavy projectiles. This is truly an international activity; people from a number of countries are working on it. We can predict that these species might be stable--stable if we put them together just right, and don't nudge them, and do all the other things that we possibly can. It could also mean that they live something like 10^{-15} seconds; that is stable for our purposes.

Where does the garbage come in? The garbage comes in when we find marvelous statements that note that the same calculations that predict stability predict that the supertransuranics emit 10 neutrons in fission, whereas uranium or plutonium emits 3. What does this mean? It means that we can have a remarkably small critical mass. What does this mean? It should scare the hell out of you! The only trouble with this argument is that you can calculate, as I have done, that the electrical power bill to produce a gram of supertransuranic material, even allowing a 100 millibarn production cross section and ignoring all capital costs, turns out to be \$100,000,000. The chances are good that we will not be making these species in large quantities. You will hear the argument that plutonium also looked like an astronomical oddity when it first appeared. That argument is wrong because then we had large reactors ready to produce it in quantity.

This is, however, a fascinating new area of research. Of course, everyone is going to try to produce superheavies. The Soviets are working hard on it, and so are we. We now have Super-Hilacs; the Soviets have two cyclotrons strapped into a multiple configuration; and in short order we should find them if they are there.

But, what about some physics in the process? There is one area that is particularly pertinent to your activities at Kansas State. It has been known for a long time that if you take a Dirac diagram and plot the energy of, say, a 1s electron, plotting binding energy versus the Z number of the nucleus you are talking about, as you increase Z you bind the K electron more and

more. If you do your calculation in a crude way, at $Z = 137$ the energy eigenvalue plunges into the negative energy continuum. It doesn't work that way because the nucleus has a finite size. If you take the finite size into account, you end up at about $Z = 169$,³⁾ but the plunge still occurs.

Suppose we could make a nucleus with 169 protons. We would then have a situation where quantum electrodynamic effects are not just a very high order perturbation on the observables; they are dominant, because now we have a many-body problem involving a whole horde of electrons in the negative energy continuum (positrons). This was initially noted by Walter Greiner,⁴⁾ and his group, and you will hear something more about it in his lectures. It is now being taken up by many people prominent in quantum electrodynamic calculations. The key question is, "Do you have to make something stable with $Z = 169$?" Fortunately, the answer seems to be no. All we have to do is get enough matter in a small enough volume so that an electron wandering around in a K shell says, "They are close enough together and they have 169 protons." What you get then are x rays from changes in the electron configuration around the complex formed during a collision--K x rays if there are K shell vacancies. The times are such that this should work.

This is the sort of thing that could be done right now--well--not quite right now. It could be done, for example, when we upgrade our tandem, which is happening this year. We don't have quite enough energy to get nuclei with enough protons close enough together, but it's within a year or so. That is going to be an exciting area of new physics on the atomic-nuclear interface.

What other things should be looked for? I mentioned that we want to get up off the surface to regions of high excitation. Take a nucleus--any nucleus. Pump energy into it. We know, and have known since the very beginning of nuclear physics, that if we look at the energy spectrum of the neutrons and protons that come out of a heated nucleus, some of them, at least, show a spectrum like this:

It is a Maxwell-Boltzmann distribution, and it simply says that if you heat nucleons as a closely coupled system some of them, statistically, get enough energy to be evaporated. But if that were all that happened, the nucleus would be a dull entity indeed. We have to ask the question, "If you put energy into a nuclear system, what more sophisticated things can happen as the available energy increases?" I want to show you details of some of the things we have found in this area in my next two lectures.

For example, one thing we have found--surprisingly--are sharp states at about 40 MeV of excitation in ^{28}Si with good quantum numbers, and all the normal characteristics of a good nuclear

state.⁵⁾ What this means simply is that as the nucleus heats, there are a great many degrees of freedom that we have not thought about before. The nucleons can cluster, can clump together in various ways that we had not suspected, while making up the compound nucleus, with very little energy left for the relative motion of these tightly bound clumps. Heavy ions really provide the only way in which we can get at these cluster configurations. This is not surprising; if we want to move a whole chunk of nuclear matter around, we have to start with large chunks.

Some of you already know that I have something of a vested interest in nuclear molecules. Back with the first tandem at Chalk River we asked ourselves the question, "If we take two atoms and bring them together we get binding, we get a molecule, because you have many fermions, electrons, that can bind them, and you get all the rich phenomena of chemistry and biology; why don't we find this in the nuclear domain?" The answer is obvious, of course; we have only positively charged nuclei. But when you ram two nuclei close enough together in a nuclear collision, you can find molecules. We found $^{12}\text{C} + ^{12}\text{C}$ molecules,⁶⁾ which look like ^{24}Mg . In other words, we found states that consisted of two carbons, bonded together, that vibrated, rotated, and did all the other things a molecule is supposed to do. We have recently found some better ones, and I will tell you about them in the subsequent lectures. We have now found the nuclear analogs of things like ammonia, NH_3 , only instead of having a nitrogen atom and three hydrogen atoms, we have a nitrogen nucleus and three helium nuclei. We now find a real kind of nuclear chemistry.

No talk about heavy ions would be complete without this hoary old slide (Fig. 2), which dates back at least 10 years. Actually, it has some scientific content. There are three kinds of collisions. The distant, the grazing, and the head-on, and they have quite different characteristics. The next slide (Fig. 3) shows a more detailed categorization.

First, consider the distant collisions. Distant collisions in heavy-ion physics are determined entirely by the

HEAVY ION REACTION CLASSIFICATIONS

Fig. 2

HEAVY ION INTERACTIONS NUCLEAR PHENOMENA

A. DISTANT COLLISIONS	1. Coulomb Excitation
2. Elastic Scattering
3. Elastic Transfer
B. GRAZING COLLISIONS	1. Inelastic Scattering
2. Single Nucleon Transfer   (i) Below the barrier (ii) Above the barrier (iii) Reaction windows
[Q, E, L] (iv) Momentum matching 3. Multi Nucleon Transfer   (i) $O^{16}, O^{16} \rightarrow$ (ii) O^{16}, C^{12} and O^{16}, C^{16} (iii) O^{16}, α and C^{12}, α 4. Direct Impact   (i) Dynamics of nuclear matter (ii) Time constants (iii) Adiabatic vs. Sudden (iv) Fusion (v) Inverse Fission

Fig. 3

electrostatic field. You get Rutherford orbits. If the energy is high enough, you get that marvelous region where electromagnetic and nuclear phenomena interfere, but you do not really get nuclear forces into action in a very interesting way until you get to what is called elastic transfer. This is an area in which we have been working in the last year. Let me just briefly tell you what it is; I'll talk about it again later. Suppose you study the reaction: $^{13}\text{C}(^{12}\text{C}, ^{12}\text{C})^{13}\text{C}$. When you pick up the ^{12}C in your detector, you can't tell whether it is the beam particle scattered off the target or whether it is the target that lost a neutron to the projectile and emerged as a ^{12}C . The cross section for the reaction has to be written as an elastic scattering amplitude plus a transfer amplitude, all squared: $\sigma \sim |A_{\text{el}} + A_{\text{t}}|^2 = A_{\text{el}}^2 + A_{\text{t}}^2 + 2A_{\text{el}}A_{\text{t}}$.

Now, why is this interesting? If you use a low enough energy, your Rutherford orbits keep the nuclei maybe 10 fermis apart, way outside the normal nuclear dimensions. What you would really like to know is, what is the tail of the neutron wave function doing a long way from the nuclear center? The transfer amplitude, A_{t} , is directly proportional to this neutron tail. If you are far enough away from the carbon nucleus, it is so small it cannot be measured directly. But note what happens here. The cross section is simply A_{el}^2 (which is very large) + A_{t}^2 (which is negligibly small) plus a term that gives essentially a nuclear amplifier. You get a very big factor (A_{el}) by which to multiply the small factor (A_{t}) in which you are interested so that you can measure it. This turns out to be a very feasible technique, and we have been able to probe what goes on in nucleon wave functions way outside the range where we were ever able to study them before.

Next on our list is inelastic scattering. If we want to start a tidal wave running around a nucleus, there is sometimes a great advantage in doing it with a very heavy chunk of nuclear matter so that only the surfaces interact and we essentially get a gear-wheel collision. We set them both spinning; we excite both the target and the projectile. I shall show examples later.

Next we have single nucleon transfer: below the barrier, above the barrier, reaction windows, and momentum matching, a whole mixed bag here. Below the barrier we have the sort of thing we just discussed for elastic transfer. In other words, the nuclei that we are dealing with themselves never get close to one another, but a nucleon manages to find its way across. These we can analyze. We don't really understand the nuclear forces, but we think we understand electromagnetic forces, so these we can calculate with relatively high precision, and we can make a very good determination of what the nucleon wave functions look like.

When we get above the barrier, we get into a region that has been tremendously explored throughout the history of nuclear physics. We have only discovered in recent years, however, that the only good light nucleus that nature gave us is lead. Only in lead do the ideas that we tell our students about all the time about orbits, particles, holes, and what not, work. Here they work beautifully, and we get very clean shell-model descriptions. For five nuclei on either side of the lead region we have now been able to get quantitative descriptions of all the states up to almost 10 MeV on a shell-model basis.⁷⁾ This has never been done before. It means that we can really understand a complex many-body system in quantitative detail.

Now, why heavy ions? What do they have to contribute? Let's look at how we would normally do the reaction studies. We would normally do a (d,p) reaction or a (d,n) reaction, which transfers a neutron to the target or transfers a proton to the target. You ask, "Why use a heavy ion? It is like shooting mosquitoes with an elephant gun." And in part that is true! There has been a hell of a lot of elephant gunning down in the literature; a lot of it doesn't fall into a particularly useful category. But under what conditions will you want to do a reaction such as ($^{16}\text{O}, ^{15}\text{N}$), instead of (d,n)?; there are several reasons.

Let us take a simple region. One where a lot of work has been done is the s-d shell from ^{16}O to ^{40}Ca . We know that in the region around Mg these nuclei look like footballs; they spin, they do all the things that footballs are supposed to do.

Near ^{28}Si the nuclei look like doorknobs, and presumably do what doorknobs are supposed to do. But suppose we do a microscopic study of the nuclei in the region around ^{23}Na . We have a rotor; we know that we should get an energy spectrum $E_J = A[J(J+1)]$ --the usual quantum mechanical rotor. But that is rather a pedestrian way to describe the nucleus; it's just spinning. The more interesting question is, "What are the individual nucleons doing? What would the shell-model description be?" We learned a long time ago that in this region around ^{23}Na you can't get any J greater than $19/2$ in a shell-model framework. The spectrum simply cuts off at $19/2$ because you have used up all the angular momentum if the particles stay in the s-d shell. Of course, the question you ask is, "Do the particles really stay down there or do they go adventuring? Do they spend some of their time up in the p-f shell, or somewhere else?"

It turns out, if we look at it carefully, that none of the experiments we have been doing up to now really answer that question. Huge shell-model calculations that have been done at Oak Ridge, for example, say, "By God the nucleons are going to stay in the s-d shell because we put them there." You do the calculations, the matrix elements come out, and sure enough you can fit the data, and everyone is happy. But we have now reached the stage where we can begin to ask questions that are embarrassing. For example, we do reactions of the kind $^9\text{Be}(^{16}\text{O}, d)^{23}\text{Na}$, where you pump in lots of angular momentum. You shoot a big, heavy particle at a target, and your momentum, you know, is $\vec{r} \times \vec{p}$; you have lots of angular momentum. We have seen all the states up to $J = 17/2$ now. But the theory that says there shouldn't be any states above $J = 19/2$ also says we shouldn't see even these, because they shouldn't have any quadrupole moment; the top ones shouldn't have any quadrupole moment. So we have evidence that suggests these nucleons aren't behaving themselves. They are not staying down in the s-d shell; they are spending some of their time adventuring. Can we track them? Indeed we can. If we look at the angular momentum characteristics of the reaction $(^{16}\text{O}, ^{15}\text{N})$ --

and Bob Ascutto at Yale has just finished calculating this, we find this reaction probes the outer part of the wave function. The reaction (d,n) probes the inner part (And you know that f waves, for example, extend much farther out.), so that by studying the two reactions together we can actually sort out the various parts of the wave function to understand how a nucleus is put together. We have that underway at the moment, and fortunately with Ascutto's codes, which are extremely powerful, we believe that we understand what is going on. We calibrate this system where we know what the structure is. This is going to open up a whole new area of nuclear spectroscopy. A nice thing is it can be done with EN tandems; it doesn't need the energy of the bigger machines. Everything that has been done for years really has to be done again with heavy-ion beams to find out what was totally missed in the other experiments.

Next on the list we have reaction windows. We didn't know this for a long time, but it turns out that there are windows such that you can see things at some energies that you can't see at all at other energies. If you plot the cross section of one of these reactions against energy, you get this kind of thing:

For a long time this puzzled everybody, but we now understand what is really happening. It can be shown best if we look at a formulation of the problem put together by Dodd and Greider.⁸⁾ We consider $(a + c) + b \rightarrow a + (b + c)$. The heavy ion b is incident on the compound system $(a + c)$ with relative momentum $\vec{k}_i$. In the final state, $(b + c)$ and a have relative momentum $\vec{k}_f$. We define

the mass ratios $A_1 = (m_a + m_c)/m_c$ and $A_f = (m_b + m_c)/m_c$. The vector $\vec{r}$ is the relative coordinate between the centers of mass of a and b; $\vec{r}'$ is the analogous coordinate for c relative to b. The momentum transfer is $\vec{q} = [(A_1 - 1)/A_1] \vec{k}_1 - \vec{k}_f$. After making some reasonable approximations, Dodd and Greider obtain a transfer matrix element of the form

$$T_{if} = \int d\vec{r} e^{i\vec{q} \cdot \vec{r}} B_1(\vec{r}) B_f^*(\vec{r}) G_{if}(\vec{r}),$$

where the transfer function is

$$G_{if}(\vec{r}) = \int d\vec{r}' u_f(\vec{r}') v_{ac}(\vec{r} + \vec{r}') u_1(\vec{r} + \vec{r}') e^{i\vec{p} \cdot \vec{r}'},$$

which includes a recoil factor $e^{i\vec{p} \cdot \vec{r}'}$ with $\vec{p}$ given by

$$\vec{p} = -[\vec{k}_1/A_1 + \vec{k}_f/A_f].$$

Earlier treatments had ignored specific consideration of recoil effects. The vector $\vec{p}$ is a sum of momenta, so $\vec{p} \cdot \vec{r}'$ can be a large number. How can you get a window? If $\vec{p} \cdot \vec{r}'$ is a large number, $\exp(i\vec{p} \cdot \vec{r}')$ oscillates like mad. It can simply wipe out all the other terms in the integrand and kill the reaction. Only when this gets into phase with the oscillations of the rest of the integrand can Mother Nature do anything for you. By properly choosing your heavy ion you can "tune-in" this term. This is one of the nice features of a tandem; you are not limited to one beam. You can choose the beam you need to give you the characteristics you want in such an integral.

Of course the window occurs also as a consequence of simple angular momentum matching. If the angular momentum $\vec{k}_1 \times \vec{r}$ in the entrance channel is significantly different from that characterizing the exit channel, $\vec{k}_f \times \vec{r}$, then the reaction is inhibited.

Next let us consider multi-nucleon transfers. As most of you know, up until about a year ago, if you worked in heavy-ion

physics and went to an APS meeting, your paper was always scheduled on the Saturday afternoon of the meeting, after everybody else had gone home. You talked to your wife and your graduate students who had to be around because they didn't have a way to leave because you had the car. There has been a drastic change. This is the hallmark of the fact that heavy-ion science has emerged as a full-fledged science. Two major international conferences in the last year--one was at Saclay and one was at Aix-en-Provence--were both devoted to heavy-ion phenomena. The fact they were both in Europe is not purely coincidental either. The Europeans are pouring money into this field at a rate that makes our heads swim.

One major impetus for this work in France is a discovery that originated in Saclay with Gillet, and his collaborators.⁹⁾ The question was, "How important is it if four nucleons get together and behave like a boson inside a nucleus?" Never mind that they are fermions; never mind that you have four nucleons; put them together and get an alpha model. Once this is calculated the real question is whether the nucleons really have to stick together like an alpha particle. Suppose you work with a quartet model. A quartet is a much more general entity because the four nucleons can, for example, be strung out in a line; they can do all sorts of things that they can't do as an alpha particle. For a long time there has been heated discussion on this subject.

We think we have a way of investigating this. There are lots of reactions. $(^{16}\text{O}, ^{12}\text{C})$, for example, is supposed to transfer an alpha particle. What we have been setting up now is to compare that with this sort of thing: $(^{18}\text{O}, ^{14}\text{C})$. Now ^{16}O and ^{12}C could, we believe, differ by an alpha particle, but ^{18}O and ^{14}C will differ by a quartet. The reaction we have been doing right now is $^{16}\text{O}(^{16}\text{O}, ^{12}\text{C})^{20}\text{Ne}$. We know that at 8.6 MeV in ^{20}Ne there is a state that looks like ^{16}O plus a valence alpha particle. So, we looked at that state in $^{16}\text{O}(^{16}\text{O}, ^{12}\text{C})^{20}\text{Ne}$, then we looked for it in $^{16}\text{O}(^{18}\text{O}, ^{14}\text{C})^{20}\text{Ne}$. In the latter case, we didn't see it, which tells us this measures what happens when an alpha particle is transferred. We have been able to use this as a calibration. We are now going from nucleus to nucleus finding out where the

quartets are and where the alphas are.

Suppose we are not happy with transfer of just one alpha. Suppose we do this reaction: ($^{16}_8\text{O}$, ^8_4Be). That transfers two alpha particles. Or if you are not satisfied with that one, you examine ($^{16}_8\text{O}$, α), which brings 3 alpha particles across. I'll show you in subsequent talks how we actually work with these reactions. It turns out to require considerable instrumentation. All this is helping to answer what happens when you pump energy into a nucleus.

Let's move on to direct impact. When you mash two chunks of nuclear matter into one another, a question that should occur to you is, "What is the relaxation time of nuclear matter?" Suppose we mash two chunks together:

Does the matter in the crosshatched region have time during the collision to readjust itself and flow out to do something else? Suppose we do an experiment with, for example, carbon on carbon. Carbon has a pancake shape. Suppose you fire another pancake shape at it. Do these nuclei line up and strike flat side to flat side? At all the energies that are of any interest above the barrier Greiner has calculated that during the collisions time the nuclear axis will swing by no more than 5 to 10° . The collision time is short compared to rotation time for this sort of system. It would be very nice to be able to say that it is so short that it is a completely sudden collision, and you just crunch the nuclei together so that the crosshatched region has double density. That would be pleasant, but it would be incorrect. Unhappily, nature has given us one of those situations where we haven't got enough time for the matter to readjust, but we can't just mash them

together either; we are in between. Walter Greiner and his co-workers have been doing most of the calculations on this question of sudden versus adiabatic collisions.¹⁰⁾

It turns out that we can learn about nuclear matter in these studies. What can we learn about bulk quantities of nuclear matter, and why would we be interested? We got into this work at a very good time, because it was just when pulsars were discovered. You no doubt remember the question was, "Does a pulsar ring, or does it spin?" Obviously a pulsar, if it rings, rings at a frequency determined by the bulk modulus of nuclear matter. It turns out that a long time ago there had been some estimates of the bulk modulus of nuclear matter; it came out to be somewhere around 600 MeV; never mind the units. About the time we were working on this experimentally, Hans Bethe did a calculation to try and put together a finite nucleus. Nuclear matter calculations previously were always taken in good theoretical fashion, "Let's begin by assuming there are no boundaries and that there is no charge on the proton." You've got to start somewhere! Well, Bethe was trying to take the next step and, in the beginning, make ^{16}O . He was able to show that with a bulk modulus of 600 MeV, he couldn't make ^{16}O ; in fact, he couldn't make anything. In order to make anything, he had to have a bulk modulus in the range 180 to 220 MeV. Fortunately, at just about that time one of my students, James Maher, was engaged in trying to mash two oxygen ions into one another, trying to understand what they did. Out of that work with Walter Greiner's help came a bulk modulus of roughly 200 MeV.^{10,11)} That is of course gratifying, because if it weren't that value none of us would be here. Another interesting thing was that with a 200 MeV bulk modulus you cannot make the Crab pulsar oscillate at 30 times a second, not at all, and in consequence it must spin—as is now well known. It is rather fun in the lab to determine what is going on inside the Crab pulsar.

In the process we also learn something about nuclear matter as such. We have determined interaction potentials, and Keith Brueckner, who originated many of the nuclear matter calculations (until he took up fusion) was able to duplicate some of

our potentials, calculating *ab initio* from just nucleon-nucleon interactions.¹²⁾ What we are trying to do, then, in this work and in the work that leads up to the islands of stability, is to see what happens when we put nuclear matter together in bigger and bigger chunks. If we want to make a real super-heavy nucleus, it is calculated that the best bet is erbium plus palladium. Kelson and coworkers have calculated that this particular reaction has a cross section bigger by a factor of 10^{49} than the one studied so far, which is argon + curium. This is a nice calculation of the astronomical type; if you are wrong by 10 in the exponent, you are still in good shape.

What I am trying to do is give you the feeling that this is a wide open field. Rather than being near the end of the field, heavy-ion nuclear physics is completely wide open. We are just scratching the surface. There is an enormous amount to be done. Let me now list a few other things in basic science; then I want to mention some applied science too.

The next slide (Fig. 4) lists several atomic phenomena

HEAVY ION INTERACTIONS

ATOMIC PHENOMENA

- A: LAMB SHIFT STUDIES - Q E D
- B: SPECTROSCOPY OF HIGHLY STRIPPED SPECIES

Fig. 4

of interest. They are straightforward. You wonder what we can really do with high velocity nucleons, and a thing that obviously comes to mind is to get a hydrogenic ion like, say, niobium after multiple electron stripping because the quantum electrodynamic effects become much more pronounced as the atomic number gets larger. The Lamb shift in helium has been measured; it has also been measured for carbon. All that is done is to strip all the electrons off the atom except one, and then look for the vacuum polarization shift for that one electron. Until the nuclei have adequately high velocity, you don't have a chance of stripping off all but one electron. There is a tremendous amount of activity going on in this area right now. It may be possible to do a better test of quantum electrodynamics in atomic physics with this kind of experiment than can be done in particle physics, where there has been much activity directed toward such tests with the Cambridge Electron Accelerator and similar machines for many, many years. It simply shows that sometimes, if you get clever in physics, you can do things with smaller equipment than you might expect.

The spectroscopy of highly stripped species is something you know all about at Kansas State.

You are probably aware of the great difficulty that presently faces us in neutrino astronomy. Right at the moment there is a factor of 10 discrepancy between what Ray Davis finds in the bottom of the Homestake Gold Mine and what the best nuclear physics calculations and stellar models can predict. This past summer we closed a loop-hole in the nuclear physics calculation. There was a possibility of a resonance in ^6Be which would let ^3He burn with ^3He in an unsuspected way and get us out of this bind, but Parker et al. have done the experiment and there is none out there; there is no resonance. What is the answer? Either the stellar model people are wildly wrong, and they claim they have proof they are not, or the neutrino decays between the sun and the earth. If that happens it will completely foul up what we think we understand about fundamental physics--or something else

is wrong, and the betting is that it is the atomic physics of highly stripped species. Only two years ago everyone thought that the oscillator strengths for highly stripped ions were well known atomic physics constants. When you calculate the opacity of a star, you depend on the atomic abundance in the surface as determined from such oscillator strengths. It turns out they were wrong by a factor of 10; a cunning German group proved that. The question is, how wrong are all the rest of them? There is a tremendous impetus now from the astrophysicists to find out what is happening in highly stripped species. There is also an interest for reentry phenomena for military purposes, which is why a lot of it is funded. Those will be two very active areas of atomic research with heavy-ion beams.

Let's go to the next slide (Fig. 5): solid state

HEAVY ION INTERACTIONS

SOLID STATE PHENOMENA

A: INTERNATIONAL AND CRYSTALLINE FORCES

B: HIGH TEMPERATURE SUPERCONDUCTORS

Fig. 5

phenomena. That's a terrible misprint. It should read inter-nuclear forces, or perhaps interatomic and crystalline forces. You are aware that when you ram a nuclear particle into a crystal, if you adjust the crystal just right, it can channel through the lattice. You can, in fact, obtain information on the interatomic

forces holding the crystal together. If you are very lucky, you can also couple into some of the phonon components in the solid with very low energy beams. There is a tremendous amount of work going on with this. Why do you want to use heavy ions? Simply because you can probe, in a more drastic way, the electrical forces that are at work inside the structure by running something with a high charge through it.

High temperature superconductors are something quite new. You know that any right-thinking industrialist would give his right arm and a large fraction of his left one if he could get a superconductor that would work at room temperature--in fact, if he could get one to work even at liquid nitrogen temperature. There is some work going on at Bell Labs that suggests that if you bombard superconductors with judiciously chosen heavy-ion species (A little newt's blood rubbed on the surface doesn't hurt either!) you can, in fact, increase the transition temperature. They don't understand why or how at this point, but there is beginning to be a field now in solid state that directs itself specifically to the question of whether by doing crystal architecture we might not be able to make superconductors that would remain superconducting to much higher temperatures.

What do I mean by crystal architecture? If you make crystals by growing from a melt or any of the other normal ways, you are stuck with what Mother Nature gives you, whereas if you do it by taking the substrate and simply hammering the species into the place where you want them, you can build a three-dimensional structure that can have all sorts of interesting electronic and solid state properties. This is the game at the moment. Again, it is a game that can be played by anyone who has a machine that will accelerate heavy ions, even to relatively low energies.

Now let's look at applied science (Fig. 6). It is worthwhile mentioning this, because these areas are going to be of increasing importance. There is the possibility of rather impressive support for good, profitable programs. First consider ion implantation. Semi-conductor doping is obvious. If you want

APPLIED HEAVY ION SCIENCE

A. ION IMPLANTATION	a) Semiconductor Doping
	b) Alloying
	c) Surface Treatment
	(i) Surface Hardening
	(ii) Surface Passivation
B. RADIATION DAMAGE	(iii) Ion Beam Polishing
	(iv) Adherence Phenomena
	d) Optical Waveguides
	e) Magnetic Bubble Memories
C. BIOMEDICINE	a) Programmed Dosage
	b) Induced and Simulated Damage
	a) Tailored Isotopes
	b) Clinical Treatment
	c) Fundamental Biology

Fig. 6

to make a very sophisticated electronic integrated circuit device, you can't do it by diffusion because you can't control where the species go; for the really elegant systems you use implantation.

Alloying is again one of the areas where chemistry crosses us up much of the time. The species we would like to put together in an alloy often turn out not to be miscible in one another in the molten state. Chemists are just beginning to realize that they can get some very marvelous materials by actually putting the species together mechanically, hammering the atomic species into the substrate. This is going to be a rapidly growing area.

On a limited base, where you simply implant material into the surface, you get, for example, all these things listed under Surface Treatment. Suppose you take tungsten or tantalum or any of the normal metals and put carbon into the surface. What you can do is make a carbide surface that is enormously hard without getting the internal part of the sample brittle; you get a marvelous combination of characteristics.

What about surface passivation? Those of you who have

worked with uranium know that if you put uranium into a reactor, it does all sorts of exotic things in terms of surface activity. It has been discovered, purely by empirical means, that if you bombard it with argon ions before you do anything else to it, it just sits there. Nobody yet knows what argon does to the surface of the uranium, but it has a tremendous industrial value, and that's important.

Next, ion beam polishing. You probably know that U-2 type camera lenses, for example, are made using heavy-ion beams. What you do is shine light through the lens system, look at the quality of the image with a computer driven system on the other end, and steer in an ion beam to polish out the offending imperfections on the glass surface. In this way you can, in fact, generate for yourself a lens that will show parking lot markings at 15 miles altitude—if not more.

Adherence phenomena. When you try to put that cunning gold band around the top of your wine glass by ordinary means, most of the time it falls off. But if you bombard the glass first with heavy ions, it will stick permanently. You want heavy ions because you can get high density radiation damage in the surface.

Optical waveguides. This is a fascinating area with important applications in the area of computers. The 370 computer and its ilk--fourth generation computers--are really limited in what they can do by the time it takes to get information from point A to point B in the hardware. Obviously, the way to improve that is to throw away the wires and the bus bars and do it with a light beam. Once you can transmit information with light beams, why not do it all within one tiny little substrate? There are some beautiful photographs where you grow a very thin garnet epitaxial layer, on a substrate, and then treat it with an ion beam. Oxygen beams are good. The ion beam changes the index of refraction in a programmed way. You also build a little semiconductor laser right in the solid; the laser beam does all the things that it is supposed to do. You can make lenses and prisms; you can make all sorts of things by just drawing them on the garnet layer

with a heavy-ion beam. Acoustic waves generated in the substrate also provide dynamic as opposed to static modification of the indices of refraction. In the end you've got yourself a little computer that runs entirely by optical beams within a single essentially 2-dimensional substrate, which, once made, is essentially foolproof. I think this is going to be the wave of the future.

What do you use for memory? You may use magnetic bubbles, and the best way to make the bubbles, the Bell Labs people have been finding, is to use ion beams to draw the bubble channels in a garnet layer. Essentially you run a laser through a microscope backwards, and drive the microscope with a computer, and draw beautiful little memory units. The point is, if you are really good at it you can take something like a PDP-15 and put it in a space 1/4" by 1/4". At least that is the hope.

Radiation damage. When you can pick the ion and pick the speed, you can program where the radiation goes, and can see how intense the damage is at that site. If you want to find out the effects of radiation on cell cultures, on materials, on anything, this is clearly the method of choice. If you want to simulate damage done from long exposure to other radiations in a short time, again this is the method of choice. Particularly this induced and simulated damage area is very important, because solid state metallurgists have found interesting things concerning the major damage that happens in a material like niobium. Why am I interested in niobium? Because a fusion reactor is going to have a wall of the stuff, if we ever make one. The radiation damage people have found two very fascinating things. First, the damage is going to be done by neutrons. How do they do their damage? First of all there is an (n,α) reaction on the niobium itself which releases helium nuclei. The helium nuclei migrate to the surface and blow bubbles. If you look at the niobium surface under an electron microscope, there are lovely bubbles that grow on it. Then they break, like soap bubbles, you get all sorts of ragged edges, which then peel off. The whole surface slowly sluffs itself off by pumping out its helium. The second thing is (n,n')

inelastic scattering. When a neutron inelastically scatters, it bounces off a niobium atom and drives it interstitially into a site in the niobium where it shouldn't be. This weakens the niobium and does all sorts of horrible things to it.

If we are going to build one of these reactors, how do we find out how good the wall is going to be? In principle, we make the reactor, then we try it and see if the walls are any good. But that approach is frowned on. The trick is to simulate the damage without actually building the device in the first place. You take a sample of niobium, and drive in alpha particles to whatever depth you want and see how they get out. To simulate (n,n') scattering, instead of using a beam of neutrons you run in a beam of niobium ions. You can't use anything else, because an impurity atom would foul up the whole process. In fact you achieve the equivalent of 2 years of neutron damage in the best neutron fluxes we have with something like 2 hours of the niobium beam. There is a huge future for understanding metallurgy in this fashion.

Tailored isotopes. You may know the technetium story. Remember in the 30's, when chemists looked at the periodic table and said, "Either Mendeleev was wrong, or there are two holes, $Z = 43$ and $Z = 61$." Finally somebody saw $Z = 43$ in the solar spectrum; it was called technetium. Then at the end of the road for the Illinois cyclotron, somebody said, "You know, with all those years of running that cyclotron, the old molybdenum deflector probably has some technetium in it." So they took it out and separated it, and sure enough, there was lots of technetium. But it remained a laboratory curiosity. Then some bright individual decided that the human body had never been exposed to technetium. Nobody knew what would happen if a slug of technetium were injected into a human. It turns out that technetium has a delightful property; it doesn't believe that there is a blood-brain barrier. If you have a lesion in your brain, and some technetium is injected into your bloodstream, the technetium moves up through the bloodstream, crosses into the brain, and concentrates in the lesion, where it emits a characteristic radiation pattern that allows localization of the lesion to a fraction of a millimeter.

In 1971 alone, something like 100,000 people had that particular treatment. One quarter of the entire Oak Ridge production facilities are devoted to making technetium now, and the total economic impact was something like \$2 million in 1971.

Clinical treatment. Heavy-ion beams have some rare characteristics. They also have some amusing characteristics. It shows the level to which a lab director will rise if he thinks it necessary. Ed McMillan, who is director of the Radiation Lab at Berkeley, in order to show that he had a good nitrogen beam, stuck his head into the beam and watched the Cerenkov radiation in his eyeballs as the nitrogen ions went through. I think that is overdoing it, but you see that you can in fact hit lesions inside inaccessible parts of the body without going in surgically. What you do is simply use a large "lens" with a short focus. At the surface regions you aren't getting enough density to do any damage but you eliminate the offending region.

Let me take just a few moments to tell you what is going on in this and other countries in terms of major facilities. This will be gossip more than anything else. The point I am trying to make in all of this is that it is a wide open field. That was the intent of this overview. We have really just gotten started.

What is happening elsewhere? Heavy-ion science is the prime field of Soviet nuclear science; they really are doing pioneering work. The program is under the direction of G. N. Flerov. They have, at the moment, the most impressive facilities for this kind of work available anywhere in the world. Germany

is also making a major move to take over this field. They have invested something well over \$40 million in new facilities for heavy-ion physics in the last year. They are building the Gesellschaft für Schwerionenforschung (G.S.I.) at Darmstadt. It is the only corporation for heavy-ion research that I have ever heard of. It will house a monster machine, with more than 300 scientists working there in a few years when it is underway. In France, there has also been a major decision that this is the field worth pushing. They have ALICE, which is a cunning machine--a linear accelerator injecting a cyclotron. Orsay, Saclay, and Strasbourg are going into this in a big way. The three Scandinavian countries have just agreed to build something called NORDAC, which is a monster after-burner for their tandem at Risø, specifically for all these areas of heavy-ion physics. The United Kingdom has funded a study program to make a tandem with 30 million volts on terminal. That will do heavy-ion science in a rather impressive fashion. Israel and Australia have just placed orders for large NEC tandems with the emphasis to be in heavy-ion physics.

What are we doing? Currently, we have several tandems, and a number of cyclotrons. The SUPER HILAC will come on the air this year. SUPER HILAC will allow us to bombard anything with anything over the barrier; the nuclear chemists' dream. They are also funded to begin work on the BEVELAC. The BEVELAC is obtained by taking the 6 GeV bevatron and injecting it with the output beam from the SUPER HILAC. It will give something like 2 GeV per nucleon heavy ions. Remember, I told you that in all of nuclear physics we now know 1600 different kinds of nuclear species. The entire effort of all living and dead nuclear physicists has been to make 1600 nuclear species. If you bombard a uranium target with a 2 GeV uranium beam, you can calculate that you will make 6000 species!

This is a field that we invented in the U. S.; it is a field in which we pioneered; it is a field in which we are still doing the best work, I think. But nothing has been done in the way of a new facility in this country, other than these--the ones best suited for nuclear chemistry--for over five years. At one

stage, three years ago, 28 institutions in this country had made formal proposals to the AEC or NSF for a large heavy-ion facility. The intrinsic madness inherent in this number percolated through, and so these proposals have dropped, one by one; there are three major ones left. They are from the Argonne National Laboratory, from the Oak Ridge National Laboratory, and from the Los Alamos Scientific Laboratory. Argonne proposes to use a 20 MV tandem injecting into a superconducting linear accelerator. The only problem with that is that nobody has yet seen a superconducting linac work. Oak Ridge proposes a tandem that can inject a little cyclotron or a big cyclotron. They can couple those accelerators in all the ways known to man. LASL proposes a tandem that injects into a room temperature linac.

What will one of these things cost? \$20 - 25 million. What will it cost to run per year? \$7 - 8 million. Only one of them will probably be built. It will be a national facility; its characteristics will be such that it will complement SUPER HILAC and the BEVELAC. In other words, its energy will fall in between those, but it will have very high quality beams so you can really do microsurgery on a nucleus. The decision on this will, I think, be announced soon. This will be a national facility in the sense that much of the research will be done by people at universities.

Question: Can you predict which of these three labs will get the facility? .

Answer: It would be inadvisable for me to predict.

Question: You said only one facility will be built. I was under the impression that probably two would be built; that LASL was going to get theirs from the Defense Department.

Answer: If LASL gets theirs from the Defense Department, that could well kill the other programs for some substantial time. If you look at what is happening in the funding for basic science in this country, you have to make a very compelling case to get even one. You do it simply because we cannot afford in nuclear science to fall as far behind other countries as we will if we don't build it. The key thing we have to keep firmly in mind is that up

Until a few years ago the United States set the style and pace in just about every field of science. If we decided to slow up and not do something until the year after next, then it didn't get done until year after next. We can't do that any more. There are many fields now in which foreign groups are saying, "You won't do it? Great! We'll do it." This is one of them.

Question: Will this money come out of other projects?

Answer: In an indirect sense, it always does. The science budget is part of the rather small fraction of the federal budget that is disposable each year. It isn't the veterans' dollar or the hot lunch dollar or mothers' dollar, or that kind of thing that you can't do anything with because it's political suicide. This is part of the disposable money. It is going to take strong arguments. What all of us in the field are trying to do is make the arguments so that it doesn't come out of existing programs. We cannot afford to have it wipe out existing programs. As I said before, we have a three-pronged field. A lot of institutions are doing a lot of work that provides the base for any further progress. We can't wipe out one of those areas to fund the other; we have been very careful about that. The important point is that all of these areas are complementary. You have to have them all, and if you don't have them all, you really are in trouble in all three.

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SIMPLE HEAVY-ION INTERACTIONS

When you start trying to understand the interaction mechanisms for heavy ions, obviously the first order of business is to select the simplest and most readily studied system and try to understand it. The simplest thing that could happen when you have two heavy ions is for them to elastically scatter from one another, so the first order of business will be to examine elastic scattering. The whole problem will be to see whether we can understand this in terms of simple physical pictures. You will see that we had some rather unexpected results early in the game; this made the job much more interesting. The next more simple thing that can happen after elastic scattering is inelastic scattering. Finally, we can have elastic transfer. These are the three things that I want to talk about today.

When you start any work with heavy ions, the first thing you have to remember is that you are in a qualitatively different region from what you are used to in nuclear physics. That is because of the heavier masses with which you are dealing; the de Broglie wave lengths of the particles are typically much shorter than those with which we have been used to working. What this means is that, to a very good approximation in much of the work, you can forget about quantum mechanics and just try to recall what you know about optics. You are dealing with the diffraction of waves around obstacles whose dimensions are large compared to the wave lengths with which you are working. You will remember from elementary optics that there are two kinds of diffraction: Fraunhofer diffraction and Fresnel diffraction. They have very different characteristics, and the difference depends upon whether the source is infinitely far removed or whether it is up close. You might think this sounds a bit ridiculous in the nuclear case, because any sort of reasonable separation is infinite in terms of nuclear dimensions. But it turns out that is not quite true, although it took us a long time to recognize it.

The first question one might ask is whether the scattering of a great big clump of charged particles, acting together as a nucleus, is going to go according to ordinary Coulomb scattering. Can we consider that these nucleons are uniformly distributed and that the nuclear charges act at their respective centers, so that you get a $1/r$ potential, or do these nuclei polarize as they come together so that you have two dipoles scattering off one another--something more like a $1/r^3$ potential? The first order of business in any heavy-ion scattering is to find out whether deformation is going to be so important that it will destroy all the simplicity we had hoped for in the scattering of two heavy charged particles.

The first slide (Fig. 1) shows some early work where we

Fig. 1

took oxygen ions and scattered them off gold.¹⁾ We simply said, all right, if we have the standard Rutherford-like scattering, the cross section will go like $E^{-2} \sin^{-4} \frac{\theta}{2}$. This is a monopole potential and is quite different from what you would get if you had

dipole scattering. You notice that the cross section goes down six orders of magnitude, and the data are right on the Rutherford curve. We thus know that the nuclei do act as though all their charge was concentrated at a point effectively at their center, and that we can go ahead and use our simple equations.

How well does the optical analogy work? The figure shows ^{14}N scattered by a lead nucleus;²⁾ this really does look like Fresnel diffraction. (What is plotted in this case is the ratio of the cross section to the Rutherford cross section.) On the other hand, also shown is carbon scattered on carbon;³⁾ you see just the standard oscillations that you have learned to recognize as reflecting simple Fraunhofer diffraction where the source is infinitely removed from the scattering object. Nitrogen on lead however appears to give the kind of scattering you would get if the light source were close to the object doing the diffracting. How can this be? It took a long time to realize, but it is actually very simple. You notice that the one difference between these two situations is that one is for a high Z and the other for a low Z nucleus. When the beam of particles comes in, the electrostatic field around the lead nucleus acts like a lens and bends the particles coming in so that their backward projected trajectories all cross the axis very close to the target nucleus. Let me sketch this for you:

Viewed from outside, the effective source is quite close, and so Fresnel diffraction occurs.

What scattering system should you undertake to study? You obviously want one that can be analyzed easily and one that gives the most insight into the physics involved: you immediately decide that you will scatter identical particles. The reason is that whenever you cannot tell the recoil from the scattered

particle in a quantum mechanical interaction, all the odd partial waves automatically drop out; you only have even angular momenta to consider in your partial wave expansion. But, you have to remember that with identical particles you get some rather interesting effects. We can discuss these with the aid of the next slide (Fig. 2).

ELASTIC SCATTERING of IDENTICAL SPIN ZERO NUCLEI

$$\frac{d\sigma}{d\Omega} = \left[\frac{Z_1 Z_2}{4E} \right]^2 \left(\csc^4 \frac{\phi}{2} + \sec^4 \frac{\phi}{2} + 2 \csc^2 \frac{\phi}{2} \sec^2 \frac{\phi}{2} \cos \left[\frac{Z_1^2 Z_2^2}{4E} \ln \tan^2 \frac{\phi}{2} \right] \right)$$

Fig. 2

Remember that for scattering of a heavy ion off gold, you get a $\sin^{-4} \frac{\phi}{2}$ curve like that on the left. But suppose we scatter identical nuclei off one another. Then the detector can't tell whether it is the scattered particle or the recoil of the target that it has detected. It becomes necessary to add the recoil part, which is just the inverse of the scattered part. Classically you would just add the two together and get the center curve. But this isn't classical optics; it isn't classical anything. You have to add amplitudes, and this generates the rather interesting third term in the expression. You see that it modulates the classical prediction with a Mott scattering term. This has been talked about

for electrons over the years, but it is difficult to see with electrons because $Z = 1$, the argument of the cosine isn't very big, and the cross section doesn't oscillate very much. As soon as we go to the Z of oxygen, for example, the argument of the cosine becomes very large, and the cosine term oscillates rapidly unless you observe at $\theta = 90^\circ$, in which case the oscillations vanish. The next slide (Fig. 3) shows what you get if you do the scattering

Fig. 3

of oxygen on oxygen.¹⁾ The dashed curve is the classical prediction, the solid curve is the quantum mechanical prediction, and the dots are the data points.

The next slide (Fig. 4) is one of those classic slides that everybody shows in heavy-ion papers at conferences. It is the earliest work we did back in 1959 with the first tandem.⁴⁾ You see that there are three kinds of characteristic phenomena. In the top

Fig. 4

curve we were scattering oxygen off oxygen. This is pure Mott scattering; we did it at 90° in order to get rid of the oscillations that would be there for quantum mechanical reasons and to see if there were some oscillations left for nuclear reasons. Where the curve deviates from the dashed line the classical trajectory takes the projectile in so that it grazes the target; then the nucleus takes over and starts pulling flux out of the elastic channel, and the cross section drops off. We ran out of energy at the point where the curve ends.

The bottom curve is for oxygen on carbon. You see it falls below the classical prediction and has some wiggles. These are diffraction oscillations; you can't eliminate them now because it's not an identical particle system. The center curve is for carbon on carbon; you notice that it has all sorts of structure. That was quite unexpected.

We have spent a lot of time on it since, and we now fully understand what is going on. These wiggles are fluctuation

phenomena. They reflect a coherent addition, statistically, of compound amplitudes in ^{24}Mg . You make ^{24}Mg , then it decays with two carbons coming back out into the elastic channel. That interferes with the elastic channel. We know what the widths and the lifetimes of the states are and so on. But this is not particularly interesting. At the same time, and I will talk about this in the next lecture, we found a series of the molecular states around 20 MeV in excitation in ^{24}Mg , where two carbons stayed together, spun end over end, and vibrated.⁵⁾ Why is that interesting? It turns out that it has a tremendous impact on the twilight of a star, when a star is trying to get from the red giant stage down to where it is going to end its career as a white dwarf, a neutron star, or a black hole. It has to burn its carbon, and if it can burn it through molecular states, dramatic effects on the whole life cycle in the latter stages of the star can result.

Let's focus on the ^{16}O on oxygen curve for a moment. One of the arguments we made for getting the first MP tandem was that we wanted to see what was going on at higher energies. When we had only this much data, we convinced ourselves and most of the scientific community that we didn't need any more energy anyway; the excitation function was just going to be flat at higher energies. We got the new machine, and the next slide (Fig. 5) shows what we found.⁶⁾ It wasn't flat! When you see this kind of thing, you get the distinct feeling Mother Nature is trying to tell you something. We had a marvelous time for the first week or two after we got these data, because each of us had his own idea of what she was supposed to be telling us.

There are three kinds of structure here. There is a broad structure with several MeV of width; there is an intermediate width structure; and, although you can't see it on this kind of an energy scale, there is fine grass all over the whole diagram, which you can see if you look at it in much finer detail. The next slide (Fig. 6) just shows one triple peak from Fig. 5 in a little more detail. You can begin to see some of the grass coming through. We have to understand the large peak structure, where the peak-to-valley ratio is as high as 100; we have to understand the

Fig. 5

Fig. 6

intermediate structure, with widths of a few hundred kilovolts; we think we already understand the grass as just statistical fluctuation phenomena.

One of the things we learned very early in this game is that nothing is more confusing than to measure angular distributions at one energy, or to measure energy distributions at one angle. If you do that, you might as well fit your signature with an optical model; you will get about the same amount of physics out of it. What we have to do is start trying to span the energy-angle space to really pin down ambiguities that would otherwise be present. First of all we measured a series of excitation functions. The next slide (Fig. 7) shows what we found.⁷⁾ You see data at 90° ,

Fig. 7

80° , 70° , 60° , 50° - we found structure in all of them. Obviously this is a highly structured system; something rather dramatic is

going on in this scattering. The question was to find out what it was.

As you will see later, subsequently we had a lot of additional information, which has pinned things down, but for the moment let's assume we have only this. Just about the time we were working with this, Everhart at the University of Connecticut was doing a very pretty piece of atomic physics. He was firing protons through hydrogen gas and alpha particles through helium gas, and was discovering that if he selected just the right speed, then the electron could flip across once, or twice, or three times, and, depending on the speed of the particle coming in, the interaction was such that he could get an even or an odd number of oscillations in essentially an instantaneous molecular configuration. If he got an even number of transits back and forth, the proton continues. If he got an odd number, he had an atom going by. By just tracking the projectile he could tell whether the number of times the electron went across was even or odd. The next slide (Fig. 8) shows

Fig. 8

typical Everhart results. Each one of the bumps corresponds to an

electron going over and back.

We thought initially that perhaps what was happening in the $^{16}\text{O} + ^{16}\text{O}$ experiment was that we had an alpha particle oscillating back and forth; some of the time the exit channel was $^{16}\text{O} + ^{16}\text{O}$, and the rest of the time it was $^{12}\text{C} + ^{20}\text{Ne}$. Since we were set up to see oxygen, whenever it came out as $^{12}\text{C} + ^{20}\text{Ne}$, with an odd number of transits, we would be at the bottom of one of the deep minima. For an even number of transits we would have a peak.

Nice idea, but wrong, completely wrong, because it turns out that when we looked for the complementary $^{12}\text{C} + ^{20}\text{Ne}$ channel it wasn't there. If we do the calculations in detail, it turns out that we really wouldn't expect it to be there because the energetics are not right for transfer of an alpha particle during the collision time for these ions.

Once we found out that this wouldn't work, we asked, if we believe this optics analogy, what does this look like in optics? What is the most familiar optics phenomenon that gives fringe patterns with black fringes? The next slide (Fig. 9) reminds you

Fig. 9

of that particular system--Newton's rings. What is involved in Newton's rings is simply that light is partially reflected from each interface, and the two reflected components are either in phase or out of phase. What has that got to do with the nucleus? The idea is shown in the next slide (Fig. 10).⁸⁾ Remember the

Fig. 10

potentials that we have to include when we interact heavy ions. There is a nuclear potential, which is of short range; there is the standard Coulomb potential, $1/r$; and there is a centrifugal potential, which is proportional to $l(l+1)/r^2$. If we add them all together, we get the kind of potential shown. Now, let's transfer attention down to the lower part of Fig. 10. With this kind of quantum mechanical potential you get partial reflection from the inflection point. A little bit of the incident flux is reflected back at the barrier; the rest of it goes in, reflects off the back wall of the potential, and some of it comes back out. If it came out with full strength, it would have a much bigger amplitude than

the part reflected at the barrier so you would get just a little interference modulation. We don't want a little modulation; we have zero minima. Obviously what we have to do is attenuate the wave inside the well before it gets back out, so that it ends up outside having the same amplitude as the other component and can kill it. That is easy to do. We simply turn on nuclear absorption inside the nucleus. The emerging waves interfere constructively or destructively as we move up in energy to give you maxima or minima.

It turns out, in a crude sense, that that is what is happening, as we will see later on. The hope was that we were going to be able to really see whether there was a central core in the nuclear potential. We haven't been able to do that yet, because the absorption is so strong that it essentially wipes out any information about the core. We think we may be able to say something about a core when we get a little higher energy, but that's yet to come.

This is enough handwaving. We know about optical models. We can represent the scattering of heavy ions, or any kind of an ion, off anything else by pretending that we have an index of refraction change at the surface and absorption. The index of refraction changes from the free space value to some new value inside, and a wave has a certain mean free path λ for propagating inside. We can relate the real part of the optical potential to the difference in these indexes of refraction, and the imaginary part of the potential is inversely proportional to the mean free path for absorption. We parameterize this potential in the form $V + iW$, insert this into a Schrödinger equation that we then solve. The next slide (Fig. 11) reminds you of the potential form we use.⁹⁾ This is the optical model in its simplest form. This gives the expression for the real part of the potential in terms of the indexes of refraction and the expression for the imaginary part. This is the simplest possible model because we have assumed that both parts of the potential have the same shape. We use the so-called Woods-Saxon shape, not because there is anything special

THE NUCLEAR OPTICAL MODEL

Fig. 11

about it, but because it is one with which it is relatively simple to calculate, whereas if we just closed our eyes and stretched a shape, it would be hell-on-wheels to calculate.

We have a great deal of data. We adjusted the parameters for the best fit to the oxygen-oxygen data. The results are shown in the next slide (Fig. 12).¹⁰ Obviously it doesn't fit the data in detail, because there isn't enough sophistication in the model. For example, it can't possibly show any substructure, and we only get broad bumps. If we take exactly the same model parameters and apply them to all the other systems, we find one interesting effect. The predictions of the model - they don't fit very well at all - all lie above the data. This simply means that oxygen is by far the most tightly bound of any of these systems, so our projections, if adjusted for oxygen, aren't right for any of the rest. It is clear that, although the general idea may be there, a lot more remains to be done. We have to put some nuclear physics in; this actually is very gratifying. It would have been enormously depressing if we had been able to fit everything by just turning a crank with a simple model, because who's to learn anything then; we might as well have been playing billiards.

Fig. 12

Once we recognize some kind of substructure here, the question is to try to understand it. The next slide (Fig. 13)¹⁰⁾

Fig. 13

shows the same five sets of $^{16}\text{O} + ^{16}\text{O}$ data that we fit originally. John Eck and his associates tried an L-dependent potential¹¹⁾ and obtained, as you can see, a much better reproduction of the experimental data. They did this by adjusting the parameters to depend upon the active partial wave, a perfectly reasonable physical thing to do.

We found that we could achieve exactly the same parameterization of our data by just changing the radii of the real and imaginary wells, putting the imaginary well just a little inside the barrier as shown in this sketch:

This means that we could bring a particle in and do something that Hill and Wheeler predicted just before the war; we could have an orbiting resonance. Remember that a particle at the top of the barrier is in a position of equilibrium, even though it is one of unstable equilibrium. This means physically that our particle can come in and, at this radius, can move around the target in an orbiting fashion; it can remain with this separation from the potential center for an extended period of time. We can calculate that the radial wave function will maximize right there; pulling in the absorption allows the particle to move around outside without being eaten up by absorption. This, in effect, can mock-up the L dependence; it works quite well.

What physics is here? Before we go any further, let's see if we can understand what it means to say that this kind of potential will reproduce the experimental data. One of the problems is how to display the kind of potentials that we work with in this kind of problem. The next slide (Fig. 14)¹⁰⁾ shows how

Fig. 14

we do it using an on-line computer. I will show later how we measure the experimental data and only note now that we are set up with an array of position-sensitive detectors, so that we measure systematically as a function of energy and angle.

When you have a computer on line, you can reverse the standard practice. The standard practice is that you do an experiment, then go away from the accelerator and crank in all the corrections, the experimental findings, the resolutions and what not, and eventually you compare some theory with the experimental results that you have massaged. Frequently by that time you discover that some idiot kicked loose the power cord on some pre-amp or something, and that the data are worthless. As experiments get more and more complicated, the chances of somebody having done just this become greater and greater. It becomes a great advantage to be able to see, in-flight, what is happening. If you need

to make a change, you make it. We insert into our computer whatever models we want to test, apply the experimental resolution functions and everything else appropriate to those models, and then let the model generate what it thinks we should be seeing experimentally. This can be very hazardous, because there is a great tendency to make data this way. You have to be very careful.

Let's examine Fig. 14 and see what it does for us. You can see that if we had just one section through this diagram, it would be very difficult to get the broad picture. But if you now just look at the diagrams for the experimental data and the optical model in broad outline, you see immediately that obviously there is a connection. We see the additional Legendre polynomial maxima coming in. As we increase the energy, a new partial wave becomes active, and the additional bump for the next Legendre polynomial begins to grow into the angular distribution. With this kind of diagram you can begin to get a real feeling for what is going on, and are not bothered by little twitches that represent the nuclear information. We find this kind of surface presentation to be very useful, because it allows us to focus at whatever level of gross structure we want; we simply wash out what we don't want.

Once you are set up to do this kind of thing, you can generate some remarkable pictures for yourself just to get some feeling for what is happening. The next slide (Fig. 15)¹⁰ is one of these. It is simply the energy-angle surface predicted by the optical model for our assumed potential. At very low energy, 10 MeV in the center-of-mass system, this is pure Mott scattering. As you increase the energy, nuclear effects begin to be important and to distort the simple Coulomb picture. If the particles are not identical - in other words say you scatter ^{17}O off of ^{16}O -- you get the picture on the right. It is dramatically different. This is simply a reflection of indistinguishability in quantum mechanics. We have actually run the measurements, just for fun. We did ^{17}O on ^{16}O , and indeed it does look like this. It would have been catastrophic if it hadn't, but it was nice to be sure!

Fig. 15

Let's look at what this means from a more detailed point of view, as shown in the next slide (Fig. 16).¹⁰⁾ Incidentally, one

Fig. 16

of the things I should mention to you is that, before we invented this kind of display, we never could understand why we could not

get consistent experimental data at our 50° angle of observation. Once we got this display, it was obvious. At 50° you are riding along the edge of mountains, and if your experimental setup from one day to the next is out of line by even a small angle--less than $1/10$ of a degree from what you think it is--you get very different cross sections. This is another advantage of this kind of display; you can tell at a glance where you are going to run into experimental difficulties.

Now let's see if we can understand the diagram. It is a schematic representation of the $^{16}\text{O} + ^{16}\text{O}$ scattering situation. At the extreme left are shown the real potentials for the different partial waves. Next are shown the real phase shifts δ_L , the reflection coefficients A_L , the 90° differential excitation function, and finally an energy-angle surface of the calculated elastic scattering plotted at 0.25 MeV energy intervals. Several observations are important. At low energies and corresponding low L values, the scattering is characteristic of an absorptive potential in that the corresponding radial wavefunction, being able to penetrate deeply in the nuclear interior, is subject to strong absorption. The corresponding phase shifts δ_L are negative. With increasing energy and L values, the internal reflection barrier moves to larger radii where it affects the radial wavefunctions more strongly, and the phase shifts go positive. It should also be noted that as each phase shift departs from zero the corresponding reflection coefficient A_L changes quite rapidly from 1 to 0. Each partial wave is thus active over only a relatively restricted range of energy. This situation is particularly marked in the case of identical particle scattering, as here, since the total absence of the intermediate odd orbital angular momenta implies that at a given energy, at least above some 15 MeV, the scattering is dominated by a single partial wave. This is strikingly evident in the similarity of the calculated angular distributions at the indicated energies to a simple $|P_L(\cos \theta)|^2$ form.

Reference to the experimental data⁶⁾ shows that the gross structure in the excitation function at 90° is damped out at higher

energies. This is because we are losing the inflection point in the potential. This means something rather important. It means that the real part of the potential has to be fairly shallow. Why is that interesting? Well, we know from work with nucleons that V for a proton or a neutron is about 52 MeV. For a deuteron, it is about 100 MeV; for a helion, it is about 150 MeV; for an alpha particle, it is about 200 MeV. You get the impression that a pattern is emerging; maybe if we multiply the number of nucleons present by 50, we should come reasonably close to the right value. That only works if you have only enough nucleons so that each one of them has a chance of being at the active point on the surface when you need it. When you get something bigger, like oxygen, some of the nucleons are buried inside and can't be active, so it is obvious that you can't extrapolate this pattern. But nobody had much of an idea just what we would get. What we find from these data is something between 10 and 20 MeV. It is a very shallow well. This was quite unexpected. It now has some interesting possibilities for nuclear matter calculations.

Using the next slide (Fig. 17),¹⁰⁾ let me remind you of what Argand diagrams look like. The ratio of the elastic scattering cross section to the Rutherford cross section has a Coulomb term and nuclear terms. I can write various particular terms of interest as a complex number in the characteristic way. I then plot it on a so-called Argand diagram to see what the behavior of the term is as I change energy. The diagram shows this for several of the different partial waves: $\ell = 16$, $\ell = 18$, and $\ell = 20$. The last two diagrams display the total scattering amplitude. Consider the center diagrams. As we increase the energy, initially nothing much happens in angle here. We simply ride out until the nuclear phase shift begins to take hold; then the nuclear phase shift swings around. Notice that the rate at which we traverse the diagram varies. The points are at equal energy intervals. When we put everything together, we get the strange picture shown at the bottom. If the nuclear effects did not turn on, the phase point would simply go around as indicated by the filled-in circles. But nuclear effects do turn on, and the diagram becomes complicated.

Fig. 17

Part h of the diagram shows an enlargement of the central region. The oscillations that we saw in the cross section correspond to this phase point sloshing back and forth on the Argand diagram. You can really understand in rather complete detail what is going on with the various phase shifts by using this kind of diagram.

Just to see that these really are behaving like legitimate resonances, the next slide (Fig. 18)¹⁰⁾ shows what happens if for the moment we turn off the absorption. We take the 18th partial wave and write the imaginary part as a number C times the actual value of the imaginary potential. We thus can systematically decrease the magnitude of the imaginary part of the complex potential in order to magnify the shape effects caused by the real part of the potential by reducing C. Notice what happens at C = 0. We start at 22 MeV moving pretty slowly in phase as a function of energy. As the energy increases, we go through the resonance;

Fig. 18

there we are moving very rapidly. After we get through the resonance, we slow up again. That is the way an honest, standard resonance should behave. But now, why doesn't it behave that way? When you turn on the absorption, you see this locus coils in on itself. When you get the absorption up to the full value that seems to be required to explain the results, you curl the locus right in on itself at the origin. The effect of absorption masks the classic resonance behavior.

The next slide (Fig. 19) lists optical model parameters for a number of systems. We have looked at a number of systems now; I haven't shown them all. We now have a reasonably good parameter space defined, so we know what optical model parameters to use for most systems.

The next slide (Fig. 20)⁶⁾ shows what the potential looked like for $^{16}\text{O} + ^{16}\text{O}$ scattering. Six fermis is about the radius when two oxygens come together. We asked Brueckner, who has been doing work on nuclear matter calculations, if he could start off from some kind of decent nucleon-nucleon force and calculate what the real part of the interaction potential would be for two oxygens coming together. He and two of his students went to work. Their result is shown on the next slide (Fig. 21).¹²⁾

A. Optical Model Parameters

System	V_{real} (MeV)	R (fm)	a (fm)	W (MeV)	R (fm)	a (fm)	Ref
$C^{12} + C^{12}$	14	6.10 ± 0.35	0.4 ± 0.1	0.41	6.41	0.38	Re72
$He^{20} + O$	57.5	5.3 ± 0.36	0.2	0.34	5.34	0.35	Re73
$D^{16} + Be^8$	14	6.1 ± 0.45	0.4 ± 0.15	0.16	6.16	0.40	
$Na^{23} + p$	54 - 55 E	3.56 ± 0.55	0.3	0.36	3.56	0.47	Pe63
$Me^{22} + a$	18 E - 3 E	3.56 ± 0.43	0.3	0.36	3.56	0.47	Pe63
$O^{16} + C^{12}$	17	6.45 ± 0.40	0.4 ± 0.05 E ²	0.11	6.11	0.35	St72
$Me^{24} + a$	54 E	4.5 ± 0.33	0.3	0.40	4.40	0.50	St65
$Na^{23} + Be^8$	14	6.36 ± 0.48	0.4 ± 0.15	0.36	6.36	0.40	
$Al^{27} + p$	52 E - 3 E	3.75 ± 0.65	0.3	0.35	3.75	0.47	Pe63
$Bi^{209} + a$	48 E - 3 E	3.75 ± 0.45	0.3	0.35	3.75	0.47	Pe63

B. Local Density Parameters

$$V(R, \rho) = \frac{(k^2 + 1)}{12\pi^{1/4}} (U + V)^{3/4} \frac{E}{2\rho} \exp\left[\frac{0.15V^{1/2}}{E}\right] \exp\left[\frac{-(k^2 + 1)^2}{2\rho^2}\right]$$

where $U = E - \rho$, $\rho^2 = 3(E/a)^2$, $U = E^2 - 1$, $J = \frac{2}{5} \ln R^2 (1 + 3\rho + 4\rho^2)$,
and $R = r_0 A^{1/3}$

$r_0 = 0.140 A$ $\rho = 3.6$ for even-even nuclei
 $r_0 = 1.24 A$ 1.8 odd-even
 $\rho = 0.0$ 0.0 odd-odd

Fig. 19

Fig. 20

These curves are for three different assumptions about the nucleon-nucleon interaction--all reasonable ones. W_0 is supposed to be the best one. Notice that they get a depth of about 20 MeV. This is very gratifying, because we found experimentally that it is in the range somewhere between 10 and 20 MeV, rather than being something like 800 MeV on an extrapolation basis. This does two things. First, it gives us some faith that the method that is being used in the nuclear matter calculations - it is a horrendous calculation - has some validity. It does actually seem to be representing something of what is going on. Second, it gives us hope that by going to higher energy, where we can measure the shape of this potential more precisely, we should really be able to get some information on the nucleon-nucleon force. That is an interesting possibility.

About the same time all of this was in progress, Walter Grainger had become intrigued by our data and was trying to put

Fig. 21

together a model, which he will no doubt describe to you in much greater detail when he comes later in the year. I'll show you his method of calculating a real potential in a moment. His method of calculating an imaginary potential in absorption is simply to let the nuclei overlap. The collision time is about 10^{-21} seconds: $\tau_{\text{col}} \sim 10^{-21}$ sec. The characteristic time for motion of nucleons in ^{16}O unhappily turns out to be just about the same value: $\tau_{\text{ch}} \sim 10^{-22}$ sec. What Greiner does is simply to calculate the time that would be required for this overlapped matter to relax, to flow out of the high pressure region in the interface; he uses the uncertainty principle to arrive at $\omega\tau \sim \hbar$. It's pretty crude, but it gives the right sort of behavior. You gussy this up a little bit and get the kind of imaginary potential shown in the next slide (Fig. 22).¹³⁾ The real potential is also shown. Greiner puts those together and gets the kind of

Fig. 22

reproduction of our experimental results shown at the upper right. In the upper half of the diagram the theoretical curve is calculated without the imaginary part of the potential. In the lower half the imaginary part of the potential is included. The experimental points⁶⁾ are shown by open circles. The theoretical cross section is too high in the energy range above 33 MeV, but you can't have everything; it's a crude model.

The next slide (Fig. 23) shows the Ansatz used by Greiner et al. This is their expression for the total energy in a heavy

$$E = u_0 A + \frac{C}{2a_0} \int (\rho - \rho_0)^2 d\tau + \frac{V}{8\pi} \int \rho(\vec{r}_1) \frac{e^{-|\vec{r}_1 - \vec{r}_2|/\mu}}{|\vec{r}_1 - \vec{r}_2|} (\rho(\vec{r}_2) - \rho(\vec{r}_1)) d\tau_1 d\tau_2 + \frac{1}{2} \left(\frac{eZ}{A} \right)^2 \int \frac{\rho(\vec{r}_1) \rho(\vec{r}_2)}{|\vec{r}_1 - \vec{r}_2|} d\tau_1 d\tau_2$$

Fig. 23

ion collision. First, it has a term that depends simply on the number of nucleons that are present; you always have one of those. Next is a term that depends on the difference in density in the overlap region from the normal density; this term obviously is going

to depend on the bulk compressibility, which is measured by the constant C . The other terms are a Yukawa-type interaction term and a Coulomb term. After they make some reasonable assumptions, the only unknown that is left is C . Out of this falls an effective compressibility constant, which turns out to be just about 200 MeV. This, as I mentioned in my first lecture, turns out to be just what the nuclear matter theorists need to be able to bind nuclei. It also turns out to be what we need to show that pulsars have to spin.

By this time I would like you to believe that we know, at least to a fairly good approximation, what is causing the structure that is several MeV wide in this kind of scattering. It is nothing more than a classic textbook example of potential scattering, which we understand in rather complete detail.

But remember that the experimental data had a finer structure (see Fig. 5). Let's now focus on it; can we say anything about it? The next slide (Fig. 24)¹⁴⁾ shows an idea that was

Fig. 24

advanced some time ago by Imanishi¹⁵⁾ to try to explain some results we had measured at Chalk River. That is a wildly confused slide, but we can ignore most of it. The idea is this. Suppose that we have an interaction of two heavy ions. The main difference about heavy-ion physics is that the projectile has excited states that aren't very high in energy. When you have a proton or a deuteron or a triton or an alpha particle, there are no low energy excited states, so you do all the dirty work in the target. But in heavy-ion reactions you can excite the projectile as well. This makes things extremely complicated at times, but it also has some marvelous advantages. Let's see what they are.

I noted that if you did the calculations, you could show that the radial part of the wavefunction does peak near the top of the barrier. What I have done in the next slide (Fig. 25)¹⁰⁾ is to

Fig. 25

plot the radial wavefunction for $L = 18$ in the $^{16}\text{O} + ^{16}\text{O}$ scattering as a function of center-of-mass energy and radius. The

superimposed heavy solid line is the real potential for $L = 18$. You see the radial wavefunction coalesces at the lower right, which shows that at this energy and at this radius you can get orbiting resonances. Keep that in mind, and now let's go back to Fig. 24. The projectile comes in with a certain incident energy that we have chosen to give us an orbiting resonance, so the two heavy ions circle one another for a long time. While they are doing that, what else can happen? One of them can be inelastically excited, and when energy is tied up in inelastic excitation, it is lost as kinetic energy. Thus, the kinetic energy drops, and if the energy of the excited state, E^* , is of the right magnitude so that the kinetic energy is reduced to a value appropriate to a quasi-bound state, you can get a double resonance. The intermediate structure in the data is here assumed to reflect the different possible couplings of the angular momentum of the original orbiting resonance and that of the quasi-bound state. There are residual interactions that will split the double resonance. That sounds great, but does it work?

The next slide (Fig. 26)¹⁴⁾ shows the experimental data⁶⁾

Fig. 26

and the curves calculated in ref. 14. You see the curve you get with no coupling into the inelastic channel is similar to those we have obtained from an optical model. If we now turn on coupling to the inelastic channel, in fact to the 3^- state of oxygen, as has been done by Greiner et al., you get this rather remarkable reproduction. Just to show that this cannot be widely wrong, the amplitude of the inelastic excitation can be extracted; this is compared to our experimental measurements on that inelastic scattering in the lower part of the figure. Again it isn't an exact fit, and you wouldn't expect that because this is a crude model, but the magnitude and the general shape are not seriously wrong. This is very encouraging; it suggests that we can understand some of our data in terms of double resonance phenomena.

Now I want to show you where this shows up more vigorously. The next slide (Fig. 27)¹⁶⁾ shows the excitation function

Fig. 27

for $^{12}\text{C} + ^{12}\text{C}$ elastic scattering. If the double resonance mechanism is involved what is important is that there be a low-lying

state in the nucleus involved that is strongly collective and that can be strongly excited inelastically. It turns out that carbon is just the right nucleus, because it has a strongly collective state at 4.43 MeV, whereas the oxygen state is at 6 MeV and is not so collective. You see in the figure that the carbon on carbon data has intermediate width structure at many angles. I'll show you shortly that underlying this there is a broad potential structure; the intermediate structure has a regular width of from about 600 kilovolts to about 1.2 MeV. So we have two kinds of structure again, broad looping structure from potential scattering and what we think may be double resonance phenomena, which we are going to have to look at with coupled channel approaches. Of course there is grass, which I haven't shown here. The next slide (Fig. 28)⁸⁾

Fig. 28

simply illustrates what happens if you put the whole thing into a computer and turn on a smoothing routine. The energy averaging intervals are shown on the figure for each case. If we smooth the data we begin to see potential maxima emerging from all the hash. This is a convenient way of looking at data so that you can sort

out the different levels of structure that you want to study.

The next slide (Fig. 29)⁸⁾ shows data for the three

Fig. 29

systems we have studied in great detail: oxygen on oxygen, carbon on carbon, and nitrogen on nitrogen. Nitrogen on nitrogen doesn't show much of this structure because nitrogen has spin 1, and the moment you have spin 1 you have many, many other states with which to work. Nor do we have a strongly collective low-lying excitation; there is no structure.

If the double resonance mechanism is important, we need inelastic scattering, so we have to understand something about inelastic scattering. The next slide (Fig. 30)⁸⁾ shows what happens if you simply scatter carbon off carbon. At the far right is the elastic peak. Next is a peak with a Q -value of -4.43 MeV; that corresponds to one of the nuclei being excited into its 2^+ state. Next is a peak with $Q = -8.86$ MeV. We know there is a carbon state at $Q = -9.63$ MeV, but this peak looks like -8.86 MeV. We have studied this in detail, and we know that it is almost entirely 8.86 . What is it? It is simply what happens if the carbons come together, the surfaces mesh and they start spinning in opposite directions; they both go into their 2^+ states. This is a nice, simple gear-wheel collision. We call it mutual inelastic scattering, something

Fig. 30

you can get only with heavy ions. Note that it is almost as probable as the single excitation. Whenever you bring two carbons together, the probability of getting only one of them excited into rotation is not much more than the probability of getting them both excited into rotation at the same time. That is what happens when you hit two doorknobs together; the chances are pretty good that if you get one of them spinning, you will get the other spinning also. The peak at $Q = -14.05$ MeV is the 4^+ member of the ground state rotational band. The peak at $Q = -19$ MeV may be the giant quadrupole resonance. We have to go back and check that for certain, but I think that is the giant quadrupole resonance. The rest of the peaks are not identified with certainty. The important thing is that the inelastic channels are very strong. Now we have to see if we can't study this in detail.

The next slide (Fig. 31)^{10b)} is the sort of kinematic diagram that you plot as a function of angle and energy if you are going to do multiple detector studies to really try to understand a reaction. This shows the kinematic loci for the most important

Fig. 31

interactions in $^{12}\text{C} + ^{12}\text{C}$ and $^{12}\text{C} + ^{13}\text{C}$ collisions for an incident ^{12}C energy of 40 MeV. We now use nothing but position sensitive detectors so that at one look we measure a range of angles, all at the same time. The rectangle in the first quadrant covers the region of observation accessible to our two position sensitive detectors at a given geometric setting. The heavy line segments in the remaining quadrants correspond to those regions of observation that are also selected by the coincidence conditions. We can place the window anywhere we like by moving the detectors; this means that we really can sort out a many-body, many-reaction system very quickly.

The kind of result that comes out is shown in the next slide (Fig. 32);^{10b)} this is what you see on the display screen of our scope. We see the elastic scattering events, the events where only one of the carbons is excited, and the events where both carbons are excited. In order to manipulate the data, we take a light pen and draw a gate around the region we want (Fig. 32c) and tell the computer, "Let me see only what is in this region." We get the result shown in Fig. 32e. We can make various corrections, and then we can have the computer display the actual cross section in 1^0 bites on the screen (Fig. 32f). This is the kind of data that we use to make our big displays; we can do this very quickly

Fig. 32

and very easily. The next slide (Fig. 33) shows you other views

Fig. 33

of this. You see the mountains just appear automatically; you can look at them and sort out all the little ravines and understand

what is going on. It provides a convenient look at the data.

In the next slide (Fig. 34) I want to show that we have

Fig. 34

been doing coupled channel calculations on these data. We start out with crude, arm-waving optics; then we take simple optical models; then we take crude optical models with double resonances built in; now we are going to honest coupled channel codes to see if we can't really understand the data. This is a preliminary result; this particular case was calculated again by Greiner. The experimental data are shown by the dashed line. You can see that all we can say at the moment is we are in the right ballpark, but we are not very close. There is a lot going on that we don't represent by coupling in just the 2^+ . That means we have to couple in the 3^- and probably a lot more in order to reproduce the data.

Once we had these results, we decided to make a Mg beam and fire magnesium on magnesium, because we were trying to find out if, when two cigar shaped nuclei scatter on one another, we get a different result than for two pancakes. Carbon, after all, is pancake shaped; magnesium is cigar shaped. The data are shown in the next slide (Fig. 35). You can see that you would have a terrible time if you couldn't manipulate the data by just drawing

$Mg^{24} - Mg^{24}$ DATA ANALYSIS

Fig. 35

a gate around the region wanted. The next slide (Fig. 36) shows the results obtained for magnesium on magnesium. Note that some of these curves change in cross section by two orders of magnitude in 1/10 of a degree, so that it turns out to be a miserable thing to measure. But, we haven't really seen any significant difference because of nuclear shape. Once again we don't have enough energy. This is one of the nice things about the fact we are now converting our machine to much higher energy; we will be able to push this far enough to begin to ask the significant question, "How does the shape affect the scattering?" We don't know yet. Another thing we want to know is what happens to the bulk modulus as you go from two tightly bound nuclei like oxygen-oxygen to something like carbon-carbon or nitrogen-nitrogen. We haven't had good enough data yet to be able to really answer that. We hope we can pull out some real information on the bulk characteristics of nuclear matter.

Fig. 36

The last thing, which I will show you because it is rather nice experimentally, is elastic transfer. In my previous talk I showed you what it is; now I will just show you how we do it. The next slide (Fig. 37) reminds you what is happening. If I tell the detector to let me know if it finds a ^{12}C and nothing else, then I can't distinguish between elastic scattering and elastic transfer. The amplitudes must add, so the cross section contains cross terms. I adjust the energies so that the nuclei are a long way apart when this happens; I am probing the neutron wave function at a very large distance. I can see it only because I get to multiply the B amplitude by the big A amplitude. The next slide (Fig. 38) shows the kinematics for this process. The rectangles again denote the region we look at with our detectors. The next slide (Fig. 39) shows the lines we get. On the left are

ELASTIC TRANSFER INTERACTIONS

Fig. 37

Fig. 38

$^{12}\text{C}-^{13}\text{C}$ AND $^{12}\text{C}-^{12}\text{C}$ SCATTERING

Fig. 39

the calculated kinematics, and on the right are the experimental data. We can simply overlap them on the computer system screens. At a glance we can tell what is happening, what states are involved, so that we can sort this out with great ease. Fortunately we can do even better, because we have in the computer all the information about angles and energies. We can tell the computer to take any event and do the kinematics on it; I'll show you in the next talk where we really make this pay.

The next slide (Fig. 40) shows that you can, for example, display mass ratios. If you have two particles coming out, the ratio of the masses depends only on the energies and angles. You see at the top alpha particles and ^{21}Ne ; that is one possible channel. You see also ^{17}O and ^8Be , ^{16}O and ^9Be , and ^{12}C and ^{13}C , in which we are really interested. We just draw a light pen window around whatever we want. The next slide (Fig. 41) is just to convince you that we can take any of the reaction channels we want out of this system.

The next slide (Fig. 42) is a very elegant one showing what happens when we do elastic transfer. We have completely analyzed these data, and it turns out that transferring the neutron has a most dramatic effect on what these surfaces look like. Here

MASS RATIO SPECTRUM

Fig. 40

Fig. 41

$^{12}\text{C} - ^{13}\text{C}$ ELASTIC CROSS-SECTIONS

Fig. 42

we have the experimental angular distributions and the experimental excitation functions for $^{12}\text{C} - ^{13}\text{C}$ scattering. They don't look terribly impressive here, but now let's put the whole data surface together. The next slide (Fig. 43)¹⁴⁾ shows what we get. The plots

Fig. 43

show model predictions for the $^{12}\text{C} + ^{13}\text{C}$ interaction with and without consideration of neutron transfer. The plotted quantity

is $\sigma/\sigma_{\text{Rutherford}}$ on a logarithmic scale. For $^{12}\text{C} \sim ^{12}\text{C}$ we have a multi-symmetric curve with all the Legendre peaks marching out towards large and small angles. When we turn on the neutron transfer, at very low energies, way below the barrier, the neutron transfers and anchors this whole pattern. You get quite a dramatically different pattern. We fit all our data here, and are able to extract the reduced widths for the neutron and ^{13}C out to about 15 fermis. That is something which, to my knowledge, had never been done before; it gives us a real hold on the tail of the wave function. Normally we crank in a Hankel function or something and say, "That is good enough." Now we can actually measure it; it turns out the Hankel function is really pretty good but that significant and measurable departures do occur.

Question: I have been wondering about your double resonance phenomenon. It seems as if there ought to be some rather dramatic things happening in the inelastic channels if this is indeed the mechanism; there ought to be some wild l -dependences.

Answer: There are. We made a check in the oxygen case. You predict wild fluctuations in the excitation functions for inelastic scattering, and indeed to the extent we have looked, everything that is predicted by the double-resonance model seems to be showing up in the data.

Question: Do you put any physics into your optical models or just use them as parameterizations?

Answer: We use them, at the moment, purely as parameterizations.

Question: Do you think that the fact that you get such a shallow potential might indicate that there is a repulsive core?

Answer: No, not at the moment, because we are dealing with such high partial waves that the centrifugal potential is swamping what is going on with any possible nuclear core. There are fine effects; if there is a central core, all the radial wave functions are shifted out so they don't start at the origin; they start at the edge of the core. For a time we thought we could

actually determine that, but now we have convinced ourselves that we can't reliably as yet. We have to go up to higher energies. That again, is one of the reasons we want the converted machine. I think we cannot give an answer at this point. What we can say is that the shallow potential tells us what fraction of the nucleons in oxygen are active at any given moment during the interaction. It is only a small fraction.

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STRUCTURE IN THE NUCLEAR CONTINUUM:
HEAVY IONS AS NUCLEAR PROBES

Today I want to tell you about a series of experiments we have been doing recently that are related to the question of structure in the nuclear continuum. This is an old subject, but it has become of new interest. You know that any quantum system is characterized by certain quantum states, and as you add energy to the quantum system, more degrees of freedom become accessible to you; the states become closer and closer together. Because there are more ways in which they can decay, their lifetimes become correspondingly shorter. If the lifetime of a state becomes shorter, then the energy width of the state increases. As the width becomes greater than the average level spacing, you obtain essentially a continuum. In the early days of nuclear physics, attention was generally devoted to trying to understand what goes on at low energy. There are two reasons for this. First, one had rather poor experimental conditions. Since these states were rather well separated in energy, the thought was that one could isolate them for study. Second, the feeling was that unless you could understand these simple excitations of the nuclear system, you really weren't going to have much hope of understanding things that were much more complicated. Over the years we have built up what, at times, is a depressing bulk of information about the low-lying states of nuclear systems.

When you get up to high excitation energy, you find that statistical techniques work surprisingly well. This is true, for example, in neutron work where you populate enormous numbers of states near the neutron binding energy. You can learn a lot about what goes on in a nucleus from purely statistical considerations; you develop Porter-Thomas distributions of levels and the like.

I have never found myself tremendously enthralled by all of this however. With a number of colleagues at Yale, I have been

interested in looking for the spoor of some simple structure that remains at high excitation. In other words, if we pump lots of energy into a nucleus, are there some simple things that still happen that we can understand without getting terribly involved or going into statistics. I have to tell you that there was an adage in the Chalk River laboratory (Gove's lemma) that says the moment you feel that you have to go and get a book on statistics, then you should throw away both the book and the data and do something else. That is not quite fair; however, fortunately there are some suggestions that there is simplicity at high excitation.

First, we know about the giant dipole resonance. If you take a bound ground state nuclear system and promote a single nucleon across a major shell, we can get a situation where the residual interactions take one of the particle-hole configurations and shoves it up to say 20 MeV. It is a simple state described primarily by one particle-one hole wavefunctions dissolved into those of the continuum. You get a width of around 4 MeV for this state.

More recently, the isobaric analog states have appeared as examples of relatively simple configurations appearing at high excitations.¹⁾ These states can appear at high excitation because of the electrostatic energy of the protons present. You get states up in the continuum, typically a few hundred keV wide. The states are fairly sharp simply because they have a different isospin from, and thus don't mix strongly with, the underlying continuum.

I want to talk to you today about another set of states, high in the continuum region, that we have been looking at recently, with widths typically of the order of 10 kilovolts. These states have both rather peculiar structure and high spin. The peculiar structure means there is very little overlap between these states and the continuum states beneath them, and the high spin also means that their decay is inhibited, so their lifetime is long and their width is small.

We have worked with a number of nuclei but I am going to take the particular case of ^{24}Mg for the rest of the afternoon; we might as well stick to one nucleus. The first slide (Fig. 1) shows

Fig. 2

so we pick a variety of other reactions to see if we can find a reaction that is specific and will isolate something worth looking at.

The next slide (Fig. 3)³⁾ shows results for $^{12}_0 - ^{18}_0$

Fig. 3

giving $^{26}_{12}\text{Mg}$. There is a continuum again, but there are a few twitches showing some structure near the ground state of $^{26}_{12}\text{Mg}$.

There also is a large peak near channel 450. When we first saw that, everyone was enormously interested for a moment. Then we realized there was a little hydrogenous contaminant in the target, and we were just studying the (p, α) reaction on ^{18}O the hard way.

By this time we realized that maybe we should pick a reaction that has some rather obvious transfer characteristics. The next slide (Fig. 4) shows results obtained for $^{12}\text{C}(^{14}\text{N},d)^{24}\text{Mg}$.

Fig. 4

You say, let's consider that ^{14}N looks like ^{12}C with a deuteron glued onto it, and you take off the deuteron so that you can put two carbons together and make ^{24}Mg . This begins to work; in fact, it works so well that you get all sorts of states, and it is rather messy. They are not overlapping, but there are too many of them to really be fascinating. When you find 48 states of this type, nobody cares what one of them really is.

The next slide (Fig. 5)³⁾ shows selected levels of interest in ^{24}Mg and the threshold energies for the available decay channels. For a long time we have been interested in carbon-carbon interactions because we wanted to find out whether we could

Fig. 5

see further evidence for quasimolecular resonances.⁴⁾ I'll show you in a moment some of the evidence we had for them from our Chalk River work. Remember that if you put carbon together with carbon, you have a significant Coulomb barrier, and so you are way up at about 20 MeV of excitation before you reach the top of it. You cannot put carbon together with carbon and see what is going on at lower excitations because you can't get them close enough together for the nuclear forces to take over. We recognized that we should do a stripping reaction to find out what is going on there. We take ^{12}C , bombard ^{16}O , and take off an alpha particle, making the assumption, not particularly valid--that ^{16}O is nothing more than ^{12}C plus an alpha particle: $^{12}C(^{16}O, \alpha)^{24}Mg$. The alpha particle balances angular momentum and takes care of energy and what have you. With this sort of reaction we can populate states in ^{24}Mg anywhere we wish. In particular, we can populate them in

a region that is of astrophysical interest; if we are burning carbon, we are putting them together with thermonuclear energies, which come in at about 14 MeV.

But that is not the only astrophysically interesting region. Truran and Arnett have become famous for calculating what's called explosive nucleosynthesis.⁵⁾ It turns out that when a supernova goes, a shock wave takes off from the core of the supernova, and that shock front carries carbon with it. If you calculate the temperatures in the shock front, it turns out that you get energies all the way up to just slightly over the barrier, so this whole range from 14 to 20 MeV is of interest in astrophysics. Just incidentally, it appears now that this is the way nature makes most of the heavy elements in the universe—in the shock front coming out from an exploding supernova through this carbon burning process, so that it is of considerable interest.

The next slide (Fig. 6) was published by Middleton⁶⁾ at

Fig. 6

about the same time we were working on it. This shows data for the reaction $^{16}\text{O}(^{12}\text{C}, \alpha)^{24}\text{Mg}$ for a bombarding energy of 36 MeV and lab angle of 7.5° . The inset shows data for a limited energy region taken at a lab angle of 45° . This is a pleasant reaction; it does just what we want it to do. We are now in the region from 14 to 22 MeV of excitation in ^{24}Mg . You get sharp states; these

states are actually less than 10 kilovolts in width. Remember that the binding energy of two carbons in ^{24}Mg is 14 MeV. When we saw a spectrum that shows one state there, one state at about an MeV higher, and three states about an MeV higher still, we had visions of a vibrating system with one and two quadrupole quantum vibrations. We were terribly excited and thought that we had found the nuclear molecule, not where we found it before, but right at threshold, and that these were its vibrational states. If that were true, the states would have spins 0, 2, and (0,2,4), so one of the first orders of business was to find out what those spins really were.

Before discussing that, the next slide (Fig. 7) reminds

Fig. 7

you of where we saw the molecular states in the Chalk River data. We put carbon and carbon together, and we found three well-defined

resonances in all the reaction exit channels at all angles; they were real resonances. Almqvist, Kushner, and I⁴⁾ studied those resonances and showed that they were in fact the first three rotational states of a C-C molecule; we investigated all the reaction channels and measured the reduced widths. It is an honest molecule; nobody questions that. But notice that it is up at 20 MeV; that's 6 MeV above threshold. What we would like is to find out what is going on way down at threshold, but you are so far down because of the Gamow factor that you don't have any direct capture cross section left.

The next slide (Fig. 8)^{3b)} shows our own data for the

Fig. 8

reaction $^{16}\text{O}(^{12}\text{C}, \alpha)^{24}\text{Mg}$. We were rather intrigued by this spectrum because it seems to show triplets. Nobody knew what to make of them at first; it turns out not to be important, it is just the angle at which we were looking. The spin and parity J^π of a level in ^{24}Mg can, in principle, be measured by observing the angular correlation of successive alpha particles in the two step reaction $^{12}\text{C}(^{16}\text{O}, \alpha_1)^{24}\text{Mg}(\alpha_2, ^{20}\text{Ne})$. The next slide (Fig. 9)^{3b)} shows the range of excitation in ^{24}Mg over which this technique was applicable for the three separate center-of-mass energies which were used. In order to do the experiment, we produced the compound

Fig. 9

state of ^{28}Si at the appropriate energy and measured the angular correlation of the two alpha particles emitted. The first alpha particle is emitted and we detected it at zero degrees, in the beam direction. Under these conditions the ^{24}Mg state of interest can have only $m = 0$ substates populated; in other words, the spins for that state are all aligned in a disk perpendicular to the beam. Then you decompose that state into an alpha particle and the ground state of ^{20}Ne , and can very quickly show that the angular correlation between the two alpha particles has the simple form $\{P_J(\cos \theta)\}^2$. In practice, the decay branch to the ^{20}Ne ground state must be observed if a unique value for J is to be obtained, since mixed transitions can enter the formalism for final ^{20}Ne states of non-zero spin, and the corresponding correlations have relatively featureless structure. The parity comes very easily here, because only natural parity states ($\pi = (-)^J$) can be populated in this kind of reaction. The parity of a state is then known once its spin is determined.

The next slide (Fig. 10)^{3b)} shows the experimental setup we used. We have a telescope that insures we are just getting alpha particles at 0° . Beams of 48.0, 48.8 and 58.3 MeV ^{16}O ions impinged

Fig. 10

on a $20 \text{ } \mu\text{gm/cm}^2$ carbon foil, the beam being stopped in a series of silver and nickel absorbers placed in front of the counter telescope. The $10 \text{ mm} \times 50 \text{ mm}$ position sensitive detector subtended lab angles from 25° to 90° . Digitized position, energy and time signals were stored in the laboratory's on-line computer and written on magnetic tape.

A multidimensional analysis using the computer display and light pen enables the selection of the desired events; the various stages of this procedure are shown in the next slide (Fig. 11).^{3b)} A position sensitive detector covers the range from 20° to 90° detecting the second-emitted alpha, and the alpha coincidences are analyzed in a computer.

Fig. 11 shows the sort of pictures we get on our oscilloscope display. As you well know, position sensitive detectors have terrible time characteristics. If we plot the time versus position, we get the curve shown at the upper left, which would be difficult to utilize with normal electronics. But using the facility I showed you in the previous talk, we simply take a light pen and draw a gate indicating the region in which an event has to lie to be accepted. We also have what is called a drag routine. You touch the light pen to the display and hold down a drag button; you can then pull the gate to some other place on the screen and freeze it.

Fig. 11

You tell the computer, "Let me see all the events that are in the first region, and correct for background and random coincidences by looking at the dragged gate." At the lower left in Fig. 11 we see the events going to the ground state of ^{20}Ne , to the 2^+ state of ^{20}Ne , and to the 4^+ state of ^{20}Ne . We draw another gate around the one of those we want, clean up some background, and finally display the events on a graph of position for α_2 versus energy for α_1 . Position has been converted to lab angle at the lower right. We haven't written anything down yet; we do this all with the light pen on the oscilloscope display. If a particular region of E_{α_1} , corresponding to a definite state in ^{24}Mg , is projected onto the θ_{α_2} axis, the desired correlation is obtained. We did it first! If you can count, you can measure spins and parities; you can see the maxima of the Legendre polynomials squared. The next slide (Fig. 12)^{3,9)} shows least squares fits of $\{P_J(\cos \theta)\}^2$ to the data. Unique spin assignments for states other than those shown were not possible because of insufficient intensity, relative to the background, of the ground state decay. A plot of χ^2 per degree of freedom as a function of J is given for least squares adjustments to the data for the 12.10 MeV state;

Fig. 12

note the sharp minimum at $J = 4$. You get rather nice correlations; the states are 4^+ , 7^- , 5^- , 8^+ , 4^+ , 6^+ , and 6^+ .

This is great, except that from the molecular viewpoint the wrong spins and parities came out. We didn't get 0, 2, and (0,2,4). This precludes an interpretation in terms of vibrations of a $^{12}\text{C} + ^{12}\text{C}$ quasi-molecular configuration. It was a bit depressing when that first happened, but happily in physics, when you start out to do one thing, you frequently end up doing something else. This is illustrated in the next slide (Fig. 13).

Fig. 13

Let me talk for a moment about the consequences of high spin in these reactions. In the next slide (Fig. 14) what we have

Fig. 14

done is take the standard level density formulation together with known levels, plug them into our computer, and ask it to plot for us the first hundred levels of each spin as a function of excitation energy in the nucleus ^{24}Mg . Now, if you put ^{12}C and ^{16}O in to make this particular reaction, you have to ask what sort of angular momentum you can take out with an alpha particle. An alpha particle can take out only so much angular momentum at a given energy. There is only a region of excitation around 10 - 15 MeV where you can really deexcite the states that you produce in the entrance channel with an alpha particle, which tells you why you see only isolated sharp states. There is no other region of overlap in the reaction between the entrance and the exit channels.

The next slide (Fig. 15) illustrates that we could get

Fig. 15

into trouble if we decided to do it with protons, for example. This is for ^{27}Al + proton channels. You see you get a quite different region that we can populate; it's up higher and it is narrower.

Suppose we do a very simple calculation of the matching of the angular momentum in the entrance and exit channels. The next slide (Fig. 16)³⁾ lists the angular momentum conditions in the entrance and exit channels for a number of reactions for the excitation of a state at ~ 17 MeV in the residual nucleus. The channel radius is given by $1.44(A_1^{1/3} + A_2^{1/3})$ fm. For the reactions such as $^{12}\text{C}(^{16}\text{O}, \alpha)^{24}\text{Mg}$, $^{12}\text{C}(^{16}\text{O}, p)^{27}\text{Al}$, and $^{12}\text{C}(^{14}\text{N}, d)^{24}\text{Mg}$, the population of states in the residual nucleus with high angular momentum should be greatly favored by the large angular momentum mismatch between the entrance and exit channels, as represented by $\Delta(kR)$. The observation of the selective population of high spin states by these reactions is in agreement with these angular

TABLE I

$14_N(14_N, \alpha) 24_{Mg}$	E_{cm} (MeV)	$Q(R)$ (MeV)	E_x (MeV)	E_{cm} (MeV)	$Q(R)$ (MeV)	$\Delta Q(R)$ (MeV)	ΔE_{cm} (MeV)
$C^{12}, C^{16} + Mg^{24}, Mg^{26}$	20.8 25.7	17.8 20.0	27.6 32.5	18.4 15.5	9.4 9.8	9.5 9.8	1.0 0.6
$C^{12}, O^{16} + Al^{27}, P^{31}$	25.7	20.0	32.5	13.9	6.6	15.4	1.0
$N^{14}, N^{16} + Mg^{24}, Si^{28}, S^{32}$	10.8 14.0	12.7 15.0	27.2 31.2	10.3 14.2	6.3 9.6	6.4 5	0.6 0.6
$C^{12}, N^{14} + Mg^{24}, O^{16}$	22.8 25.6	17.6 19.9	27.2 30.5	4.7 8.1	5.2 6.2	12.2 12.4	1.0 1.0
$C^{12}, O^{16} + Mg^{24}, Si^{28}, S^{32}$	28.0	22.8	30.5	6.4	4.7	17.1	0.6
$C^{12}, O^{16} + Mg^{26}, Si^{28}, S^{32}$	29.9	23.8	32.5	29.4	13.0	8.4	0.1

a) Mg^{24} 1.1

b) Mg^{26} 1.5

c) Mg^{24} 1.9

d) Mg^{26} 1.1

Fig. 16

momentum considerations. In the case of the $^{14}N(^{14}N, \alpha)^{24}Mg$ reaction, the angular momenta in the entrance and exit channels are very nearly matched at the ^{14}N energies used. This condition is much more favorable for the population of low-spin states than in the measurements where $\Delta(kR)$ is large. This can account for the evaporation-like spectrum of α -particles²⁾ seen in Fig. 2, since the low-spin states are broader and have a higher density than the high-spin states. Thus, the selective or non-selective behavior of all four of these reactions can be understood in terms of angular momentum balance.

The next question is whether we can tell what structure these states have. The next slide (Fig. 17) shows that we can get somewhat fancier and use Hauser-Feshbach theory, which some of you are aware is just a statistical theory that assumes that you make a real compound nucleus, that you have actually put the nuclei in the entrance channel together to make ^{28}Si and then boiled the alpha particle out in a thermal equilibrium situation. We have

$$A = \frac{d\sigma}{d\Omega}(0^\circ) \text{ for } 10^+ \text{ state at } E_x = 17 \text{ MeV}$$

$$B = \sum_{J=0}^{10} \frac{d\sigma}{d\Omega}(J, 0^\circ) \times \rho(J, E_x) \times 0.05 \text{ MeV}$$

Reaction	A/B
$\text{Cl}^{12}(\text{O}^{16}, \alpha)\text{Mg}^{24}$	~ 4
$\text{Cl}^{12}(\text{O}^{16}, p)\text{Al}^{27}$	~ 2
$\text{Cl}^{12}(\text{O}^{18}, \alpha)\text{Mg}^{26}$	~ 1
$\text{N}^{14}(\text{N}^{14}, \alpha)\text{Mg}^{24}$	~ 0.1

Fig. 17

made the calculation using the optical model potentials and transmission coefficients that we measured earlier. We have chosen to display this in a rather strange way. We have taken, for example, a 10^+ state at an excitation energy of 17 MeV and have calculated the ratio of the area under the peak for this state to the area under an extrapolated background of all the other states of lower spin. We are slowly getting converted so we no longer compare our experiment with calculations we do later. We make the comparison on-line; you just dial the spin state you want, and this system will tell you what the ratio of A to B should be in the exit spectrum if it is a statistical compound nucleus process. In the N-N case, where we didn't see any peaks, the A/B ratio is such that you wouldn't expect to see them, whereas where the peaks are most pronounced, you calculate that the ratio is 4, which is just about

what we were seeing. So, it looks as though there is a strong statistical component in this. If that was all there was to it, it would not be very interesting; fortunately it turns out that there is much more to it.

About this time Roy Middleton and I were comparing our data; he was using one combination of target and projectile; we had the target and projectile roles reversed. We happened to work at exactly the same center-of-mass energy. The next slide (Fig. 18)^{3b)} shows the Yale data and the Penn data in the region

Fig. 18

of two particular ^{24}Mg states at 14.4 MeV and 14.1 MeV. At first we looked at this and said, "This is ridiculous. It can't make the slightest difference whether you bombard oxygen with carbon or carbon with oxygen at the same center-of-mass energy." But after you look at it for a few minutes, you realize that something very different is happening in the two cases. We were both detecting alpha particles at zero degrees, so in one case we have transfer of two alpha particles or 8Be , in the other case we have transfer of three alpha particles or ^{12}C . The next slide (Fig. 19)^{3b)} shows schematically what is going on. If you bombard a carbon target with an oxygen projectile, and you have a forward peaking of the α_1 distribution, then what you are seeing is the

Fig. 19

transfer of either a ^{12}C or three alpha particles. If you do it the other way around, bombard an ^{16}O target with a ^{12}C beam and have a forward peaking of the α_1 distribution, then you are seeing the transfer of two alpha particles or 8Be . What Fig. 18 really shows is that the 14.41 MeV state is strongly peaked forward (for the ^{12}C beam) and there is no backward peak, whereas the 14.14 MeV state has an angular distribution which is just about symmetric fore and aft. This turns out to be a way to sort out what kind of alpha particle transfer you are getting in a reaction.

The next slide (Fig. 20)³⁾ shows a whole series of measurements that we made to try and find out if we could tell what kind of mechanism was going on in producing these states in

Fig. 20

^{24}Mg . We looked at the forward angle ($\theta_{\text{LAB}} \approx 2^\circ$), in one case with a carbon beam and in the other case with an oxygen beam. Notice that these data show all the characteristics of a compound nucleus excitation function; they have random structure, and it is not even vaguely interesting. This is what we get for the low states of ^{24}Mg in the region 6 - 9 MeV.

The next slide (Fig. 21)³⁾ shows what happens when we go up to excitation energies of 13 to 15 MeV, up in the region where we are interested in looking at the new states. You see that there is quite strange structure coming in, which shows a correlated minimum in all states around 19.7 MeV bombarding energy in the center-of-mass system. We are beginning to see something here that has a direct component because of pronounced forward or backward peaking. Also, we are seeing evidence of something rather

Fig. 21

striking going on in the compound nucleus, because at one energy something seems to be happening to all states.

Question: I don't understand your comment about a direct component.

Answer: If you look at some of these curves in Fig. 21, you see states with a large peak at the left, which means the cross section is very high at 0° , and there is practically no cross section at back angles. Just look at the left part of the figure as a cut at 0° and the right part of the figure as a cut at back angles. We are taking two points of the angular distribution.

Question: Does that say excitation in ^{24}Mg in the center?

Answer: This is just the final state. You see, we picked a final state.

Question: Then is that an excitation function?

Answer: The horizontal axis represents the energy of the beam when we bombard with a carbon beam (left) or with an oxygen beam (right). Forget that for a moment, and say that this is the excitation in the compound nucleus, ^{28}Si , feeding the indicated states in ^{24}Mg . On the left we are feeding at 0° and on the right we are feeding at 180° . If I take any given energy in the compound nucleus, then on the left I have the cross section for 0° and on the right the cross section for 180° , for population of the state in ^{24}Mg indicated in the center. It shows what we want it to at the moment; rather than take a whole series of angular distributions, what we have done is to take two cuts at 0° and 180° , because what we are really interested in is to find out whether these states are symmetric or asymmetric.

Once you see that something is happening in the compound nucleus, you attempt to find out if you can see a resonance, if there is a state in that region. So, we did the reaction shown in the next slide (Fig. 22), $^{12}\text{C}(^{16}\text{O}, ^{16}\text{O})^{12}\text{C}$. The upper curve is the ground state-ground state scattering. There are lots of resonances. In particular, there is one that occurs just at the energy where we saw that minimum (19.7 MeV). We do the inelastic scattering (lower curve), and there it is again. Obviously, there are lots of other things, so just seeing a spike at that energy doesn't prove a thing. We don't know that it is a resonance yet, but the fact that it happens to fall where the correlated structure was in all those distributions is suggestive.

The next slide (Fig. 23) shows the results of more measurements. There is no inelastic carbon resonance at $E_{\text{c.m.}} = 19.7$ MeV, but it is present in several of the other channels. Since I made up this slide, we have measured the $^{12}\text{C}(^{16}\text{O}, p)^{27}\text{Al}$ reaction, and there is a very sharp minimum at just this same point. So this is beginning to look like a resonance; it is showing up in a lot of

Fig. 22

different channels. It corresponds to a state at 36.6 MeV in ^{28}Si ; it has a width of about 350 kilovolts. But what are its characteristics?

In the next slide (Fig. 24) we take our potential--the one we have been using all along that we measured from elastic scattering--and ask if we come in at this energy (19.7 MeV), which corresponds to 36.6 MeV in ^{28}Si , what would be the most likely orbiting resonance. The answer is that it would be $L = 14$. Now we go back to the resonance and we measure angular distributions on the resonance and on either side of the resonance. The next slide (Fig. 25) shows the measured angular distributions obtained by Siemssen et al. on the peak of the resonance and on either side. You see $\{P_J(\cos \theta)\}^2$ for $J = 15, 14$ and 13 superimposed on the same experimental data for each J . There is only one of these that really matches, and in fact it is the center one for $J = 14$, which supports the idea that we are looking at a $J = 14$ state that is at this particular energy in ^{28}Si . This is rather nice; it is a very high

Fig. 23

Fig. 24

$O^{16}-C^{12}$ ELASTIC DATA

$[P_J(\cos \theta)]^2$ -Distributions

Fig. 25

spin state, and it is up at about 40 MeV, where nobody has ever seen narrow states before. The question still is whether we can say anything about its structure.

Question: Just looking at that slide, it is difficult to say that the only curve that matches is the center one. Do you have some numerical criterion for choosing that case?

Answer: Yes, we run χ^2 for all of these, and this is the only one that is halfway decent. Just looking at them, you can see that every one of the others gets badly out of phase.

Question: Then chi squared is significantly different for the center curve?

Answer: Very much so; there is a striking dip in chi squared for $J = 14$. You have to remember that once you start playing these games, with very high spin states, there are many maxima and minima, so it becomes significant if you really stay in phase with P_J^2 over an extended region.

What is the structure of this state? In order to really try and understand what is going on, we tried scattering ^{12}C off

^{16}O and asked which states are populated in the inelastic channel. We found the results shown in the next slide (Fig. 26). You get

Fig. 26

the ground state, and there is a little bit of ^{12}C in this first excited state, but not much. You begin to get 6.05, 6.92, and 10.35. What are those states? Anybody who is an old friend of ^{16}O will immediately recall there is a rotational band in ^{16}O that is made up of ^{12}C with an alpha particle outside of it; it is a very strongly deformed band beginning at 6.06 MeV: 0^+ , 2^+ , 4^+ . Just to prove that, we did the reaction $^{12}\text{C}(^7\text{Li}, t)^{16}\text{O}$, which we know just transfers an alpha particle to ^{12}C . In the lower part of Fig. 26 we see that we get the same states. So, the first suspicion we have is what we are doing is loosening up one of the alpha particles from the ^{16}O . We put the two carbons together and we have an alpha particle left over, so we have a carbon-carbon-alpha structure.

The next slide (Fig. 27) shows the sort of thing we are talking about here. We bring in an ^{16}O with a carbon core, an

Fig. 27

alpha structure. If we get a resonance, the alpha doesn't know which carbon it belongs to, and we can, in fact, get a carbon-alpha-carbon molecule. This is rather an attractive idea, but it is purely conjecture at this point. We want to find out, for example, if this is the only channel that is being excited, the one where we can have an alpha outside of carbon, because this is nothing more than an excited ^{16}O . We have brought in the ^{16}O ; we have excited it to its deformed band, and once it is in that deformed band, then the alpha particle doesn't know which carbon it belongs to. It is rather similar to some of the things that go on in atomic physics, as I noted in my first lecture, but we use an alpha particle as our electron.

We decided we really had to know whether or not the 6.13 MeV state, which is a 3^- state, the octupole state, was involved in this as well as the alpha state at 6.05 MeV because we couldn't resolve them directly. We looked at gamma rays at the resonance with a sodium iodide detector. The next slide (Fig. 28) shows the results. You see the gamma ray that you want, and you can show that it is the right energy, so now we want to look at its excitation function. The next slide (Fig. 29) shows that. The arrow indicates where the resonance should be. We picked a couple of other gamma rays that were in the spectrum just to show that it wasn't an accident that gave us that peak. This means we have to do a calculation, which is underway at the moment,

Fig. 28

Fig. 29

to try and figure out, given that structure of an alpha-carbon-carbon, what the branching ratio is for us to be involving this much of the 6.13 octupole state compared to the 0, 2, 4 strongly deformed quadrupole type band. At the moment we don't know the answer. The next slide (Fig. 30) just shows that we are getting

Fig. 30

a resonance that involves the 6.13.

Let's look again at Fig. 5 and consider what we have at this point. We have said that from the heavy ion reactions we can choose a selective reaction that will populate only certain states. Those that come down to low-lying states in the final nucleus go by a compound nucleus reaction. Some of those that go to higher states in the final nucleus also go by a compound nucleus reaction. There is no simple reaction mechanism here, contrary to what we had hoped. But even at very high excitation in ^{28}Si , it appears there are individual states that are participating in this reaction. They have what appears to be simple structure. We are now hot on the trail of additional ones; there are other states in that spectrum that are sharp, and we are looking at them now. What we have to do at this point is understand

in detail, quantitatively, what is going on. But this is not the whole story. You will remember we did not start out to find out what is going on in ^{28}Si . It was sort of a bonus that we found the sharp states at all. What we wanted to find out is whether there are carbon-carbon states that can participate in stellar carbon burning. So, let's get back to that problem.

What we have to do is make these states, then look at their break-up and see if we see the carbon-carbon, or the $^{16}\text{O}-^8\text{Be}$, or the $^{20}\text{Ne} + \alpha$ channels. We know the $^{20}\text{Ne} + \alpha$ channel is very strong. We then have to find the C-C states in an enormous background of other stuff, so we have to get fancy with the experimental equipment. The next slide (Fig. 31)³⁾ shows you the kind of

Fig. 31

equipment we use. The beam comes in from the right and strikes a target. Position sensitive detectors are located at ① and ②. At the left are an absorber foil and a hydrogen gas cell used to prevent forward scattered heavy ions from the target and the beam itself from reaching the detector. There is a forward telescope, so we are going to pick up particles going out in the forward direction to define the state of Mg we want to look at. No matter how the Mg state breaks up, we get the decay products on either side of the beam and are able to have kinematic overdetermination. We

have energy and position ①, energy and position ②, and energy at 0° , so we define the whole business. We run triple coincidences among all three.

The next slide (Fig. 32)^{3b)} shows that with this same

Fig. 32

system you can get some very pretty data on carbon-carbon scattering. This is similar to what I showed you in my previous talk. This shows the elastic scattering, the single excitation, and the mutual excitation.

The next slide (Fig. 33) indicates we are now going to do the $^{16}\text{O} + ^{12}\text{C}$ three-body kinematics. It is a horrible mess, because we are going to be looking for all these channels: $^{12}\text{C}, ^{12}\text{C}$; $^{12}\text{C}, ^{12}\text{C}$ with one of them excited; $\alpha, ^{20}\text{Ne}$ with various excited states; and $^8\text{Be}, ^{16}\text{O}$. All of these things are happening; all of them have different kinematic loci. What we are going to do is set windows

Fig. 33

to sort them all out, and look at the individual channels to find out what is going on.

The next slide (Fig. 34)³⁾ shows you a two-dimensional

Fig. 34

time spectrum. The three types of random events that can occur in a triple coincidence experiment are clearly indicated. The

diagonal line corresponds, for example, to events in which a true coincidence takes place in detectors 1 and 2 (see Fig. 31) and an α -particle arrives at a random time in the 0° detector. The intensity of this line arises from the high kinematic coincidence rate produced by heavy-ion elastic and inelastic scattering into detectors 1 and 2. The vertical and horizontal lines arise from events in which a real coincidence occurs between detectors 0 and 1, or 0 and 2, with a random particle in the remaining detector. Triple coincidence events are located at the intersection of these three lines. A rectangular gate ~ 100 ns wide was set around this region, and gates of identical size were placed along each of the three well defined random event lines, and also on the region that corresponds to a random event in all three detectors. Events passing through these gates were then appropriately flagged. A proper random subtraction for a triple coincidence experiment requires such a procedure, which is greatly facilitated by the use of event-by-event recording. It would be horrible to try it any other way.

Then, as I mentioned in the previous talk, you ask the computer to give you mass ratios. The next slide (Fig. 35)³⁾ shows

Fig. 35

you the mass ratios. The peaks at 1/5 and 5/1 correspond to decay to ^{20}Ne plus an alpha particle. The peak at 1/1 corresponds to decay to carbon plus carbon. The little peaks at 1/2 and 2/1 correspond to decay to $^8\text{Be} + ^{16}\text{O}$. The smear underlying them all is simply a true three-body background that comes from four-body events. We can take that out, because we can calculate the kinematics.

Now, what do we have? We have the real events; we have corrected for randoms; so now we ask for those events that decay into $\alpha + ^{20}\text{Ne}$, those events that decay into $^8\text{Be} + ^{16}\text{O}$, and those events that decay into $^{12}\text{C} + ^{12}\text{C}$. In the next slide (Fig. 36)³⁾

Fig. 36

we see the spectra as a function of alpha particle energy, or excitation energy in ^{24}Mg . At the upper left is breakup into alpha plus $^{20}\text{Ne}^*$ (4.25 MeV); we can read off what states in ^{24}Mg decay through each channel. At the lower left are events that decay through $^8\text{Be} + ^{16}\text{O}$. The ^8Be , of course, breaks up into two alpha particles, but our detectors are wide enough so that they pick up the two alpha particles and sum them. We get just a broad smear here; there are no sharp states showing up. At the lower right are events that decay into carbon + carbon with one of them in the 4.43 MeV excited state; again no sharp states show up. Finally,

at the upper right are events that decay into carbon + carbon in their ground states. There are two peaks. We were enormously excited when we first saw this, because Trudy Goldhaber¹⁰⁾ had sent us a preprint--(This shows you one of the evils of preprint distribution.)--that predicted there would be states of the carbon-carbon molecule at 22 MeV and at 31 MeV in ^{24}Mg with spins of 6 and 12 respectively. We did a quick measurement, and the spins indeed looked like 6 and 12. So we reported at the Paris conference on quartet structure. But then we looked at these data again and said, "Do we really know that we went through a state of ^{24}Mg at all in this particular reaction?"

The next slide (Fig. 37) illustrates the quandary.

Fig. 37

Line A) shows one way of doing the reaction, where the alpha particle comes out and then Mg breaks up into two carbons. But there is another mechanism that can go on; ^{16}O and ^{12}C get within one another's sphere of influence, the carbon excites the ^{16}O to the state that I mentioned awhile back that looks like carbon plus an alpha particle, and in the exit channel going away from the reaction site we have an alpha and a carbon and another carbon. It had nothing to do with ^{24}Mg !

Question: But don't you have sufficient kinematics to

pin that down?

Answer: That is just what I am going to show you; we in fact pinned it down.

Question: I thought that was already done.

Answer: No. We thought it was, but if you look at it very carefully, the previous kinematics we have done aren't sufficiently restrictive to eliminate one of these or the other.

Let's go back; we now need several more gates. We can not only ask if we have an alpha plus Ne coming out, we can also ask in what state does the Ne come out. So we set a gate, and we ask for a plot of the Q-value spectrum. In the next slide (Fig. 38)³⁾ we see the number of events that come out into the different

Fig. 38

states in ^{20}Ne . You might ask what happens to the states above 4.25 MeV in ^{20}Ne , and the answer is they are all particle unstable.

Before the complex can get to the detectors, it is no longer ^{20}Ne ; it is ^{16}O plus an alpha, and you have a four-body channel. The center diagram shows $^{16}\text{O} + ^8\text{Be}$; there is some excitation here in the 6.1 states of ^{16}O . The bottom diagram shows $^{12}\text{C} + ^{12}\text{C}$. Now we know how much is coming out in each of these different channels. The question we want to focus on is whether the $^{12}\text{C}(0)$ peak is the result of inelastic excitation during the interaction and decay, or whether it went through a sharp state of ^{24}Mg . This all illustrates that heavy ion reactions can get damn complicated.

The next slide (Fig. 39) shows that what we do is plot

Fig. 39

a kinematic diagram--a Dalitz plot--for this whole experiment. If we were going through a state in ^{24}Mg , there would be a sharp diagonal ridge at 135° ; obviously it isn't there. It is very hard to see here, but in the experimental data, instead, there are 45° ridges modulating this whole diagram. We looked at this for a long time, convincing ourselves that frequently you will find experimental observations that are hard to explain, which the next slide (Fig. 40) illustrates more graphically.

We then set about trying to understand these data. The

Fig. 40

next slide (Fig. 41) shows what we did. We said, "All right, let's

Fig. 41

pretend we know the mechanism; we'll assume it is a direct reaction,

and we'll use Code DWUCK to calculate the inelastic part of the process. We'll break up ^{16}O in a nice simple fashion, and we'll simulate the data." You can see that it doesn't look anything like the experimental results.

For the next slide (Fig. 42) we tried to be a little

Fig. 42

smarter. We looked at the real energies of the process and realized that we are right at the energy where we should see a P_{18} orbiting resonance. We then bring out the alpha isotropically, and we just calculate what we should see. You see there is a lovely set of the 45° diagonal lines. It doesn't show so well here, but you get almost an exact matching of the experimental data, which shows that what is happening is you are running through a P_{18} inelastic process and then dissociating in flight.

But we were not about to give up yet. This tells us that most of the results are going this way, but we set out to find carbon-carbon. The next slide (Fig. 43) shows the approach. We go back to the Dalitz plot (Fig. 39); we draw a window where there should be events going to carbon-carbon; and we say, "The hell with everything that lies outside that rectangle; we are going to focus

Fig. 43

on what is going on in there." We pulled out these events and looked at them. We have Hauser-Feshbach in hand for our calculations. The next slide (Fig. 44) simply indicates how we can calculate what is going on. I don't think I have to remind you of any of this; it is pretty simple stuff. I will just show you the results of our calculations on these various channels. The $\alpha + \alpha + {}^{20}\text{Ne}$ are the dominant channels, and the $\alpha + {}^{12}\text{C} + {}^{12}\text{C}$ is what we are looking for. The next slide (Fig. 45) shows what we get. Here are the experimental results, microbarns per steradian, that we've taken out of those data for ${}^{24}\text{Mg}$ excitations in the range from 16 to 32 MeV. At the top are the ones that break up into $\alpha + {}^{20}\text{Ne}$, and the curves are what the statistical calculation says they should do. When we calculate what we should find for ${}^{12}\text{C} + {}^{12}\text{C}$ with one of them excited and for ${}^{12}\text{C} + {}^{12}\text{C}$ with both of them in the ground state, we get the curves in the lower part of the figure. There is astonishingly good agreement; nothing has been normalized here. You fit up at the top to the $\alpha + \text{Ne}$ data and all the rest comes out. This tells us that we are indeed

$O^{16} + C^{12}$
THREE BODY FINAL STATE

EXPERIMENT

STATISTICAL MODEL CALCULATION

$$O^{16} + C^{12} \longrightarrow \alpha_1 + Mg^{24}(E_x)$$

$$Mg^{24}(E_x) \longrightarrow \alpha_2 + Ne^{20*}, C^{12} + C^{12}$$

$$\frac{d^2\sigma}{d\Omega^2 dE_x} = \sum_i \frac{d\sigma}{d\Omega}(I, E_x) \rho(I, E_x) \frac{dB}{d\Omega}(I, E_x)$$

where B=branching ratio for
decay of Mg^{24}

Fig. 44

making states in ^{24}Mg , that they do have a width for branching out into carbons, but it is not the sharp states that do it. It is instead the broad underlying background in this whole range. In the range where we want to do carbon burning in a star we have an exponential increase in the density of those states, so we have, in fact, measured what we set out to measure, but it wasn't as much fun as we thought it was going to be. We have obtained the answer we wanted, and we can put it into the shock wave calculations for a supernova. It turns out that they come out about right. This is what you need to make the elements come out the way we've got them, so it turns out the Deity wasn't cheating.

Let me finish by showing you some calculations we've done to try and determine if we can predict where we are going to see this kind of effect. The next slide (Fig. 46) shows some rather

Fig. 45

simple-minded calculations for the level density at various center-of-mass energies in these various systems. You can see what is happening; $^{12}C - ^{12}C$ lies low; $^{16}O - ^{16}O$ lies pretty high; $^{18}O - ^{18}O$ is quite a bit higher. As you get more energy in the center of mass, you get higher spin states and the level density drops down. This is one of the reasons why you don't see any structure in $^{16}O - ^{16}O$ elastic scattering down near the barrier, but you begin to see it up higher. It is one of the reasons you don't see any structure at all in $^{18}O - ^{18}O$ elastic scattering; the level density is so high that the statistical fluctuation effect isn't very marked.

The next slide (Fig. 47) shows the number of open channels. On a simple model, you can calculate how many channels there are, because you know how much energy you have, you know how much angular momentum you have, and you know how much you can carry

Fig. 46

away in all the different channels. We simply programmed our machine to take all the channels into account, to count all the open channels at a given energy and calculate how many open channels there were. It is not surprising that the lower curves represent the cases where we see all the interesting structure, and as you go up, things get more complicated. You can run these calculations and predict rather quickly what's going to be interesting and what isn't.

The next slide (Fig. 48) shows the horrendous sort of thing you can produce. It shows, for example, that if you want to look at ^{24}Mg the major cross section is going to involve states of about $J = 10$ and 11 at about 30 MeV of excitation. You can calculate how the cross section goes in all directions on that surface, so you can actually calculate now, without any reference to experimental data, where things would be expected. Of course it is rather gratifying to find that is indeed where they do appear.

Fig. 47

Fig. 48

The next slide (Fig. 49) shows that once you have all

Fig. 49

this, you can try calculating what the reduced width would be of the fluctuation states you would find. What you would expect to find is the average width of the structure in an excitation function, and this is what we calculated for ^{24}Mg . You have to add up all the possible channels - neutrons, protons, alphas, 8Be , ^{12}C - and put them together. The next slide (Fig. 50) shows what you get. Here is an excitation function for $^{16}O - ^{12}C$. The calculated average widths of the compound nuclear structure, which we have calculated from scratch, are indicated. At the left you see that the width of the individual structure is so narrow that the experimental resolution smears it out; you don't see any sharp states. In the center you are beginning to get about the right widths. As you move to the right, it is beginning to open up. You can get a rather nice reproduction of what is going on. We think we now know what these states are, how they break up, and how to go about predicting where to find this kind of structure. That seems like a good place to stop.

Fig. 50

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These lectures were intended to be very informal; only after the fact were tape recordings transcribed to produce the present record. My thanks go to Professors Spangler, Seaman and Ms. Dea Richard for producing the transcripts.

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Professor Eugen Merzbacher

Professor Merzbacher of the University of North Carolina and Kenan Professor of Physics has been active in the theoretical analysis of production of x rays in ion-atom collisions. His text on quantum mechanics is a standard used in many universities.

INTRODUCTION TO THE THEORY OF INNER SHELL IONIZATION

It is very nice to be here in Manhattan, and it is good to see friends from many different occasions and make new friends while I am here. It is particularly gratifying to have the opportunity to visit one of the centers of the renaissance in atomic physics that we have been enjoying in the last few years. I got into this field a very long time ago, and I got into it in a nuclear physics laboratory. It is a subject that has had its ups and downs, and right now it is enjoying a high and broad up. There has been a nice revival of interest for a number of reasons, and I'll try to say something about that.

Since we will be talking a great deal about x rays, I would like to start by showing a slide of Mr. X himself (Fig. 1),

Fig. 1

Wilhelm Röntgen. Actually there is a certain strange continuity

that links me to this man. My father, who lived until a couple of years ago, was an organic chemist who got his training in Germany and took his Ph.D. in Munich. He had a minor in physics, and the professor on his Ph.D. oral examination in physics was, in fact, none other than Professor Röntgen. So, I have always felt there is a sort of live chain. Actually, I don't think my father did so well in the physics part of the examination.

I have written down some of the high points and in between the valleys of absolutely no interest in this subject we are talking about. In 1912, Chadwick¹⁾ first observed x rays that were produced by the bombardment of various targets with alpha particles from radioactive substances. He observed what we now call characteristic x rays. This was just at the time of Moseley's work, when characteristic x rays were just becoming understood. Chadwick saw x rays that were characteristic of the target when alpha particles impinged on the target. I have a feeling that may have been something that Rutherford suggested to Chadwick.

Then there was one of these long periods when virtually nothing happened in this field. X rays obviously were produced much more prodigiously and cheaply by electron acceleration (Dentists do that.), and heavy particles were not used for this purpose. Then, in the late 20's, Gerthsen,²⁾ who was also a nuclear physicist, did some experiments and studied some details of the production of vacancies in inner shells of atoms by use of incident particles that were heavy, in particular alpha particles and also protons.

In 1933 a man named Henneberg became the first person to do a theory about this process.³⁾ He must have been one of the first Ph.D. students Bethe ever had, and he did a very nice piece of work. Bethe had just written his famous article in *Annalen der Physik* on collision processes,⁴⁾ and this was a natural outgrowth of that interest.

Then a catastrophe happened; the neutron was discovered, and most good physicists became nuclear physicists. It was really rather remarkable. If you look at the *Zeitschrift für Physik* in the late 20's, you see a tremendous amount of momentum in atomic

and molecular physics. An enormous amount of work was being done, and more would have been done. You know after all, the Lamb Shift was essentially discovered in the early 30's. I think we would have seen great progress in that general area if there had not suddenly been this very understandable change of direction toward nuclear physics.

So again there is a big gap, about 20 years, and then in about 1953, by which time nuclear physics was well established, attention was again drawn to the generation of x rays through the production of vacancies in inner shells, this time primarily by protons. Accelerators were then available. The two names I have written down here are Huus and Lewis, both nuclear physicists. Lewis is at Duke University, and that is the way I became associated with this kind of work. In the bombardment of, say, tantalum, thick targets, with protons in the energy range from 1 to 3 MeV using the Van de Graaff that Duke had just then acquired, we saw enormous amounts of these characteristic atomic x rays.⁵⁾ Huus⁶⁾ did an experiment at Cal Tech at that time and also saw these x rays, but he was really after nuclear gamma rays from proton bombardment. I guess I should be somewhat ashamed of myself; there was the chance to discover Coulomb excitation of nuclei, and we became interested in these atomic x rays instead. Huus discovered nuclear Coulomb excitation, and to him the atomic x rays were just a nuisance. The cross sections for these x rays, being atomic in nature, were fantastically large compared to any nuclear cross sections, so nuclear physicists were both impressed and embarrassed by the large intensity of x rays that were produced in these bombardments. This was also the time when sodium iodide crystals were becoming popular as detectors for x rays and gamma rays, so there was a new and very good detection technique available, and a number of experiments were done to study these x rays.

Then the subject completely died. There were a number of years in the fifties when there would be no more than perhaps one paper a year in this field. In fact, the field was dead until just a very few years ago when there began this fantastic revival of interest. I wrote down a couple of recent dates that have to

do not with work on Coulomb excitation of atoms, but with the discovery of another kind of excitation. That is what people call quasi-molecular excitation, or they call it the Fano-Lichten process after Ugo Fano and Bill Lichten, who made a theoretical analysis and, at least partly, explained this.⁷⁾ Sometimes it is called Pauli excitation, because it involves many electrons that get into each other's way, and somehow the exclusion principle is supposed to play an important role. I mention this because it is the kind of thing that I am not going to say much about. Rather, these lectures are going to deal primarily with Coulomb excitation, the excitation that comes at somewhat higher velocities when a charged particle is sweeping by and through its Coulomb field simply tickles the atom and excites it. That is what we will primarily be talking about.

I have quite deliberately not listed the most recent developments, either because they are too familiar to you or because I shall be saying something about them later. Nowadays, if you look at Physical Review Letters, it is unusual for an issue not to have a paper in the field of either Coulomb or quasi-molecular excitations of inner shells. The norm is that you will open it up and almost every two weeks it is rewarding.

To a theorist, anyway, these experiments are quite simple. All you do is have a bunch of ions of charge Z_1 ; Z_1 will refer to the incident ions that bombard a target. Then x rays come out, and you locate an x ray detector to collect them, and that is it. Much of the progress in recent years, of course, has come because of the solid state detectors that have replaced sodium iodide crystals and proportional counters, but it is somewhat ironic that some of the most beautiful and interesting work now is being done with crystal spectrometers, which antedate not only solid state detectors, but also sodium iodide crystals and even proportional counters. Incidentally, proportional counters are still useful even nowadays.

In 1926, Gerthsen²⁾ pointed out something very simple. What he said was that what is happening is that the charged projectile collides with an electron in the atom. If that were a collision between a charged particle of mass M and energy E and a

free electron, not bound to the atom, then the largest amount of energy that could be transferred to the electron would be given by this expression:⁸⁾

$$T_m = \frac{4Mm}{(M+m)^2} E \approx \frac{4m}{M} E$$

Here m is the electron mass. If the projectile is a proton, this becomes $T_m \approx (1/500)E$. If you have a 1 MeV proton, then the largest amount of energy that you could transfer to a free electron in a collision would be 2 keV. On the other hand, an electron in a K-shell of an atom is bound with more energy for $Z > 16$ (sulfur). You certainly would not be able to produce a K x ray in tantalum if this were the story. But, of course, it is quite obvious that the electron is not free in an atom; it is bound, and the binding is essential. It is only because it is bound that it can come out. That is perhaps paradoxical, but also understandable, because by being bound it has effectively a greater mass, and by having a greater mass it can accept more energy, and therefore it can accept an energy that for a free electron would be impossible because of energy and momentum conservation. Such a large energy transfer is maybe not probable, but certainly possible, not forbidden by kinematics or by the simple conservation laws of energy and momentum.

I would now like to start giving a description of the excitation or ionization process. Frequently, I am going to use the words excitation and ionization interchangeably. Usually we are ripping the electron out of an inner shell and sending it out to infinity with some finite kinetic energy. Or, we may, of course, be just exciting it to some excited bound state. That also happens, but both of these things are connected, and often I won't make a distinction between them, although we may have to do so in some contexts.

An important parameter for our discussion is $Z_1^2 e^2 / \hbar v$. If v were the speed of light, $e^2 / \hbar v$ would be the fine structure constant. However, v is the velocity of the incident particle, so this number is larger than the fine structure constant; it is

not one over 137, but bigger. In fact, $e^2/\hbar$ is the velocity of the electron in the hydrogen Bohr atom, so $e^2/\hbar v$ is the ratio of the Bohr velocity to the velocity of the incident particle. I multiply it by Z_1 because this is essentially the quantity that measures how strong the interaction is between a charged particle sweeping by and the atomic electron. Notice that it involves the charge of the incident particle; if you make that bigger, obviously the interaction is going to get stronger. The charge of the electron is also there, of course; that is why it is e^2 . It is $Z_1 e$ times e . If the incident particle moves very, very fast (large v), there isn't enough time for it to interact. Thus, in a very crude but nevertheless useful way, this parameter measures the strength of the interaction that does the exciting or the ionizing. It turns out that this parameter appears over and over again in the theoretical description, if you do it quantum mechanically.

Just to give you the nuclear-physicist-turned-atomic-physicist way of feeling for the numbers here, I have converted it into reciprocal MeV per amu:

$$Z_1 e^2/\hbar v \approx Z_1 \times 0.16/\sqrt{(M/E)} \text{ (amu/MeV)} .$$

This quantity measures whether you can use perturbation theory or not. If this number is very large, the interaction is going to be big, and one might worry whether perturbation theory is applicable. Since alternatives are not readily available, I am going to describe the process primarily in terms of perturbation theory. Obviously if you are going to use a very heavy, high Z_1 ion, perturbation theory might be entirely inappropriate. In that case we might have to cancel the bets and try over again. But if you want to decide whether you have to do it all over again, it is first good to know what perturbation theory does say. That is going to be a large part of the burden of the discussion.

We are talking about Coulomb excitation. Sometimes I call it direct excitation as distinguished from the quasi-molecular process. For nuclear physicists this is an analogy, similar to

the distinction between direct nuclear interactions and compound nucleus interactions. The analogy is limited; don't push it too far. But nevertheless, there is something there. The quasi-molecular case is, of course, a sort of many-body affair where many particles can participate. In the direct process you have the charged particle sweeping by and interacting with a single electron directly; this we call Coulomb excitation.

The interaction, then, is $-Z_1 e^2 / |\vec{R} - \vec{r}|$.

I have put the incident particle on a straight line track, but it need not be that way. P is the incident particle and $Z_1 e$ is its charge. It has velocity $\vec{v}$. O is the nucleus of the target atom. The vector $\vec{r}$ locates the electron in the target atom; I just show the particular electron that is going to be excited. The interaction, then, is between the projectile and that electron; and it is just a Coulomb interaction. The quantity b is the impact parameter, in case we choose to make an impact parameter description. The quantity ζ is another kind of impact parameter; it is the distance from the electron to the incident particle trajectory, which is different from the distance between the target nucleus and the trajectory. That may come in handy later on as well.

Since we are doing perturbation theory, the thing to do is calculate the matrix element between the inner shell and the final states of the electron in the atom. The key quantity entering any kind of perturbation theory that we are going to be doing is the matrix element between the initial state, labelled by n , and the final state, labelled by n' , of the electron in the atom:

$$V_{n',n}(\vec{R}) = \langle n' | (-Z_1 e^2 / |\vec{R} - \vec{r}|) | n \rangle.$$

$\vec{r}$ is the electron coordinate, so that is going to be integrated over in evaluating this matrix element. But the matrix element is still a function of $\vec{R}$, the location of the incident particle. It is in the treatment of $\vec{R}$ that several different theories evolve. We are going to compare and contrast some of these theories in the next talk. Today we are just going to stay with ordinary first-order perturbation theory, which is called the Born approximation.

At this point we use one of the celebrated results of classical mathematics. The trouble with the matrix element is that $\vec{R}$ and $\vec{r}$ are intermingled. They are not separated as a product of something that pertains to the electron times something that pertains to the projectile, and that is not neat. We would like to have something separable if we can. That is the motivation for the next step. Namely, we take the Fourier integral

$$1/|\vec{R} - \vec{r}| = (1/2\pi^2) \int [\exp(-i\vec{q} \cdot (\vec{R} - \vec{r})) / q^2] d^3q.$$

This is an integral over three-dimensional q -space and accomplishes what we wanted, a very nice separation of what pertains to $\vec{r}$, and therefore to the electron, and what pertains to $\vec{R}$, and therefore to the projectile. We now put this into the matrix element:

$$V_{n',n}(\vec{R}) = (-Z_1 e^2 / 2\pi^2) \int [\exp(-i\vec{q} \cdot \vec{R}) / q^2] F_{n',n}(\vec{q}) d^3q,$$

where

$$F_{n',n}(\vec{q}) = \langle n' | \exp(i\vec{q} \cdot \vec{r}) | n \rangle.$$

You see that a nice thing happens; everything that pertains to $\vec{R}$ is now in the exponent, and everything that pertains to $\vec{r}$ goes into the matrix element that I call $F_{n',n}(\vec{q})$. So, instead of having the matrix element of $1/|\vec{R} - \vec{r}|$, we have the matrix element of the plane wave $e^{i\vec{q} \cdot \vec{r}}$, and $\vec{R}$ is not there any more. This matrix element is

sometimes known as a generalized form factor. It is related to what is called the generalized oscillator strength; when you take the square of its absolute value and multiply by the energy difference, it is that. It is obviously an interesting quantity that depends entirely on the electron in the atom.

We have achieved our separation at a price, of course. The price is that we have this integral over q -space. The question now is whether it is a worthwhile trade-off or not. Well, I wouldn't have done it if it were not.

So far things have been very general. We have not yet done anything about the motion of the projectile. Now comes the time to do something about that. Also we must worry about the atom and the evaluation of the matrix element $R_{n,n}(\vec{q})$. If you want to calculate that, you have to put in wave functions for the atom, an initial wave function and a final wave function. You must consider all the possible final states and evaluate the various form factors. That is an interesting task; many people have been working on this for a long time. It has lent itself ideally to the use of high-speed computers. Some of the recent progress in atomic physics is tied up with better detectors, some of it is tied up with better computing. We can put in all sorts of fancy atomic wave functions describing the atom as best as we can, calculate these form factors, and see how they behave as a function of the variable q . So far that variable is purely a mathematical thing. It is the variable that comes into the Fourier integral; it has no other physical significance yet.

The other part of the problem is to treat the incident particle in some fashion or other. Let me outline a very simple quantum mechanical treatment. What you do is again use perturbation theory. You have this particle coming in and hitting the electron in the atom, colliding with it, and making an inelastic collision. The incident particle is in a certain momentum state before the collision and in a different momentum state afterwards, because it gets scattered by the process of exciting or ionizing the atom. The initial momentum is $\vec{p}$; the final momentum is $\vec{p}'$; and we put in a matrix element of the interaction, which we call T .

$$\langle \vec{p}' | T^{PWBA} | \vec{p} \rangle = \int \exp\{(-i/\hbar) \vec{p}' \cdot \vec{R}\} V_{n,n}(\vec{R}) \exp\{(i/\hbar) \vec{p} \cdot \vec{R}\} d^3R.$$

The initials here stand for Plane Wave Born Approximation; that is clumsy, but that is what it is called. We take the interaction matrix element that we obtained previously and we put it in between the initial and final wave functions. The integration is over the configuration-space for the incident particle; that is, over $\vec{R}$.

You can see that first-order perturbation theory is simple and straightforward. We will have to worry later on whether it is right, and if it is not right, how we can do better. Remember that the only way $\vec{R}$ came into $V_{n,n}(\vec{R})$ after we had done the Fourier inverse was in the form $e^{-i\vec{q} \cdot \vec{R}}$, so the $\vec{R}$ integration is trivial; it involves all plane waves. If you have a bunch of plane waves, and nothing else, then when you integrate this over $\vec{R}$ you obtain a delta function. That means that this matrix element is zero except when $\vec{p} - \vec{p}' - \hbar\vec{q} = 0$. $\vec{p}$ and $\vec{p}'$ are given, but we have an integration over q -space. We can then perform the q -integration; as soon as you see a delta function, the next integration is automatic. It is at this point in the theory that $\vec{q}$, instead of being a dummy variable of integration, acquires the physical meaning of being the momentum transfer: $\hbar\vec{q} = \vec{p} - \vec{p}'$. This is the negative of the change of the momentum of the bombarding particle, which is the momentum that has been transferred from the bombarding particle to the atom with which it is colliding. From here on, $\vec{q}$ will be used to designate this momentum transfer.

We now have a very simple expression for the matrix element that describes the collision:

$$\langle \vec{p}' | T^{PWBA} | \vec{p} \rangle = -4\pi Z_1 e^2 F_{n,n}(\vec{q}) / q^2.$$

The form factor describing the atom divided by q^2 is evaluated at the particular $\vec{q}$ that we are talking about, $\vec{q} = \frac{\vec{p} - \vec{p}'}{\hbar}$. It is very important that q^2 appears in the denominator of the matrix element. This means the process will be probable if the momentum transfer is small. You might think it will be particularly probable if it

is zero; the cross section will then be very large. But it turns out that it cannot be zero; there is a minimum possible momentum transfer that is imposed by the fact that in the collision energy and momentum have to be conserved. The projectile can't just lose an arbitrarily small amount of momentum. That is obvious, because if it lost no momentum at all, it also couldn't change its energy, and it has to change its energy because it is going to ionize the atom, or excite it at least. So there is a minimum amount of energy that must be transferred to the atom in order to raise the electron from an inner shell--let us say the K shell in whatever you like; argon is popular. It takes at least 3,203 eV to get the K-shell electron out of argon. You have to impart that much energy, so you can't have zero momentum transfer.

In fact, the minimum momentum transfer is given by the following:

$$(\hbar q_{\min})^2 = (\vec{p} - \vec{p}')^2 = 2M(\sqrt{E} - \sqrt{E-\epsilon})^2 \approx (Mc^2/2E) [1 + (\epsilon/2E) + \dots] \\ \approx (\epsilon/v)^2 .$$

Minimum momentum transfer occurs essentially when there is only energy loss, but no change in direction; there is just forward scattering, and the incident particle slows down a little bit. Here M is the reduced mass of the colliding system, and all energies refer to the center-of-mass system. In all of this discussion I have avoided the distinction between the center-of-mass motion and the laboratory motion. That distinction is not negligible, particularly if you go to heavy-ion physics.

Now E is the energy of the incident particle, and ϵ is the energy loss that the incident particle suffers. We expect that ϵ will be small compared to E . You are going to lose a few keV, and you have from your Van de Graaff several MeV, so the energy losses tend to be small compared to the energy that's there in the first place. If that is not the case, of course, you have to be careful, and you can't use the next step. But if it is true--and it usually is--then the minimum momentum transfer squared

is given essentially by the approximation given above. The minimum momentum transfer is essentially given by the energy loss divided by the velocity. Obviously, the bigger the energy loss the greater the minimum momentum transfer below which you can't go. The faster the particle is, the less momentum transfer is needed to provide the required energy transfer.

Now we have to take the square of the matrix element and integrate over possible directions of the various final momenta, $\vec{p}'$. After all, the incident bombarding particle can come off in the forward direction, or it can be scattered at an angle, and we have yet to integrate over that. This can be expressed as an integration over the directions of $\vec{q}$. As a result we obtain the cross section for a transition from an initial state n with energy transfer ϵ as follows:

$$d^2\sigma_{\epsilon n} = 16\pi Z_1^2 (e^2/\hbar v)^2 (dq/q^3) |F_{\epsilon n}(q)|^2 d\epsilon.$$

You see here the factor $Z_1 e^2/\hbar v$ I wrote down at the beginning. If that factor is small, the cross section is small, so the validity of perturbation theory hinges crucially on this factor being small.

Incidentally, very crudely speaking, the electron likes best to go out with as little kinetic energy as possible, just to barely make it. That is the most probable thing, as a rule of thumb. In principle it could take up as much energy as the bombarding particle has, but it doesn't like that very much.

The next step, if you want the total cross section for producing a vacancy in one of the inner shells, is to integrate this over all q and eventually over all possible energy losses. That is shown in the following expressions:

$$\sigma_n = (8\pi Z_1^2/Z_2^4) (a_0^2/\eta) \int_{W_0}^{\infty} dW \int_{Q_0}^{\infty} (dQ/Q^2) |F_{Wn}(Q)|^2,$$

where the following relations hold:

$$\eta = Z_2^{-2} (\hbar v/e^2)^2 = mE/MZ_2^2 R_{\infty} = (Z_1/Z_2)^2 (\hbar v/Z_1 e^2)^2,$$

$$\begin{aligned}
 W &= \epsilon/Z_2^2 R_\infty, & Q &= a_0^2 q^2/Z_2^2, \\
 W_0 &= \theta/n^2, & Q_0 &= a_0^2 q_0^2/Z_2^2 = W^2/4\eta, \\
 \theta &= n^2 I/Z_2^2 R_\infty.
 \end{aligned}$$

a_0 is the radius of the first Bohr orbit in hydrogen, R_∞ is the symbol for the Rydberg, and I is the ionization potential. q_n is the total cross section; it is still proportional to the incident particle's charge squared. That is typical of first-order perturbation theory; it has to be proportional to the square of the incident particle's charge. If that happens not to be the case in nature, you haven't done a very good job by using perturbation theory. You also see the Bohr radius, a_0 , squared, as you would expect in any kind of atomic process. In the denominator you have the fourth power of Z_2 , that is the target atom's nuclear charge, the one that binds the electron that you are yanking out. In other words, the tighter it is bound, the harder it is to get out, even though, as you remember, Gerthsen said that binding was necessary so it could get out at all.

The quantity η is a dimensionless measure of the incident particle's energy. You see again our old friend e^2/kv , this time inverted. I have written η in different ways, which is perhaps nice to see, displaying Z_1^2/Z_2^2 and then putting in $Z_1 e^2/kv$, the famous parameter, so that you can get an idea of how they are connected. When the perturbation parameter is small, the quantity η is large, so when perturbation theory works well we should have relatively large values of η . Forgetting for a moment the possibility of exciting a bound state, the minimum value for W , which is essentially the energy transfer, is of course for just barely ionizing the atom. This energy I , the observed ionization potential, we can look up in Bearden's tables. θ is a parameter that measures the extent to which the ionization potential of an inner shell of the atom is hydrogenic or not. If θ is 1, that means it is like hydrogen. The reason it is not fully 1, of course, is that from a heavy atom the electron is more easily removed than if it

were hydrogenic because there are L and higher shell electrons there to screen it; they help make it easier for the electron to come out. Thus, θ measures the screening effect of the outer electrons.

Now comes a funny kind of point. Even though the atom, say argon, certainly is not hydrogen, nevertheless if you don't have better wave functions to evaluate these form factors, the first thing you do is put in hydrogenic wave functions to see how it goes. If you do that, you find that the form factor is just a function of the variables Q and W that I wrote previously, and that has a very dramatic consequence. Suppose I take σ and multiply it by Z_2^4 . Then it turns out that everything else depends just on η , that is this energy parameter, and on a certain power of θ . I won't go into the details here, I'll just show you the results of this kind of argument. To a pretty good approximation, over a fairly large range of energies and the periodic table, the product of the fourth power of the target nuclear charge times the cross section for producing a vacancy in a certain shell times the screening parameter θ turns out to be a universal function of (η/θ^2) :

$$Z_2^4 \sigma_n \theta \approx f(\eta/\theta^2) .$$

It is universal in the sense that it is the same for all atoms and all energies. You should be able to plot all data on one graph, and they should all lie on one curve. This, of course, is either true or false, and it motivates my showing you a few slides that will test this. Is it true or false? Let's see. You will see some slides that have Z_2^4 times the cross section σ , times that screening parameter θ , plotted against η/θ^2 .

The first slide (Fig. 2)⁹⁾ just shows you what was done in 1953; that's of purely historical interest. These are the data for tantalum. The projectile (proton) energies were between 1 and 4 MeV as accelerated by the Van de Graaff that was then available in the old Duke Nuclear Structure Laboratory. The cross sections are in units of 10^{-16} cm^2 . Since this deals only with one element

Fig. 2

instead of several, the parameter θ wasn't put in. θ is different from element to element, but this is all for tantalum. The fourth power of Z_2 was put in, nevertheless.

The next slide (Fig. 3)⁹⁾ is a theoretical slide; it shows you the first order Born approximation as a function of η instead of η/θ^2 , as I said we should plot it. The ordinate is Z_2^4 times σ for the K shell, divided by Z_1^2 , so as to normalize as if it were for protons. You get a family of curves. If you divide the abscissa by θ^2 and multiply the ordinate by θ , these curves will all collapse essentially into one universal curve. At least down to the lower left they will; at the upper right they won't quite do it, but it is a very good approximation down below.

The next slide (Fig. 4)⁹⁾ is some old data, again, plotted on such a universal plot; η/θ^2 plotted against $Z_2^4\sigma$ times θ . You see it is a logarithmic plot extending over many decades. Log plots were very popular in this field because they conceal experimental

Fig. 3

departures from the theory. The lighter elements Ti, Fe, and Ni lie highest on the curve while Ta and U are down below because of the scaling that is involved. The solid line is a theoretical curve based on the Born approximation. The dashed curve is a relativistically corrected piece; we won't worry about that. That's rather impressive in the light of the fact that it goes over four orders of magnitude.

At the far upper right this curve, like all atomic cross section curves, increases with energy and then gradually drops down. The reason cross sections come down at high energies is that the incident particle goes so fast that nothing can happen any more. This higher regime is sometimes known as the Bethe regime. It is a feature with which you are familiar from energy loss processes that drop off after a maximum. The peak occurs, very crudely speaking, where the projectile's velocity is

Fig. 4

comparable to the atomic electron velocity. I think people have made far too much of that. Velocity matching and things like that occur, to be sure, but I don't think one should look in really great detail at that and take great stock in it.

The next slide (Fig. 5) shows essentially the same sort of thing, but with much more recent data. This comes from Garcia's work,¹⁰⁾ with his way of plotting these things, but I have put in my own symbols. You notice that instead of having η/θ^2 , we have η/θ here, and instead of having θ times $Z_2^4\sigma$, we have θ^2 times $Z_2^4\sigma$. In other words, both abscissa and ordinate have been multiplied by θ . That just expands both scales; something that is a straight line in the first place is going to stay a straight line. So this fit is just as good as the previous one. The only reason I am showing this is that I want to caution you. Sometimes you find a paper on comparison of the binary encounter theory with the Born approximation theory, and one seems to fit better than

Fig. 5

the other. Don't make too much of that because in the region where both of them are sort of straight lines on plots like this, they are both going to fit. Now exactly why the prediction of the binary encounter theory follows this same sort of curve is another matter; maybe we can talk about that later on.

Let me close by showing you the sort of refinements that I want to bring up in the remainder of these talks. We need to ask: Is universality really observed? How meaningful is it? How much should be expected? At high energies it is clearly going to break down.

The next question concerns how well we do when we use hydrogenic wave functions to compute form factors. Will we do better if we use Hartree-Fock wave functions? Can we make an improvement in the theory that way?

The next question is, if we go from the K shell to the L shell with its various subshells, can we see the differences in

excitation mechanism in the cross section? Is the L_I shell excited differently from the L_{II} , and so forth?

The fourth question is, as we go up toward very heavy atoms, in the K shell in particular, do we have to worry about relativistic corrections for the atomic wave functions? Dr. Bhalla is nodding his head because he has, in fact, done exactly that sort of thing. You must worry about it, and it makes a difference.

The next question concerns the fact that in the plane wave Born approximation we forget entirely that the incident projectile is repelled by the nucleus. That is just completely ignored; it is a different perturbation that appears as a higher order effect in the Born theory. The question is, does the repulsion play a role; how important is it at low energies in particular?

The next question is how important is it that, as the charged particle sweeps by, it doesn't see a dead, frozen atom but it polarizes the atom while it hits it; how much effect comes from that? At the bottom I list something that is dear to the heart of many of you at KSU. That is, how important is multiple excitation rather than just single excitation in these Coulomb processes? This is not a complete list, but it is enough for today.

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THEORIES OF INELASTIC COLLISIONS

In the first talk I gave you an introduction and set up the framework for a discussion of inner-shell ionization processes. Today I will go on and get a little more involved in the theory.

The description I gave was in terms of quantum mechanical perturbation theory, the Born approximation. We can be more specific about what we mean by that term. I have tried to use it specifically as meaning first-order perturbation theory, treating as perturbation the interaction between the charged projectile and the atomic electron. The term "Born approximation" implies nothing about how the states of the atomic system are to be described. They could be described by hydrogenic wave functions. If you have almost bare nuclei, as is the case in some of the KSU experiments, you really have hydrogenic states, of course. If you have a many-electron atom, you can try to do better and use Hartree-Fock wave functions, or the like. We are not committing ourselves in advance as to how we treat the states of the atomic system.

Next, the plane wave part enters the terminology; we may decide that the incident particle is going to be described as a plane wave and the scattered particle as a plane wave as well, and that there is a momentum change. $\vec{p}$ and $\vec{p}'$ are the initial and final momenta of the projectile, and the momentum transfer is $\hbar\vec{q} = (\vec{p} - \vec{p}')$. That is the plane wave Born approximation, (PWBA), as distinct from all other Born approximations; and that is the way I shall use the term.

The formula we are led to in the plane wave Born approximation for the cross section for producing a hole in the K shell has the following form:

$$\sigma_K = (8\pi Z_1^2 a_0^2 / Z_2^4 \theta) f(\eta/\theta^2) ,$$

where

$$\eta = (1/Z_2^2) (\hbar v/e^2)^2 = (37E/Z_2^2 M) \text{ (MeV/amu)}$$

and

$$\theta = 1_K/Z_2^2 R_\infty.$$

Entirely similar formulas are valid for the ionization of higher shells. In this plane wave Born approximation of perturbation theory the cross section is proportional to the square of the incident particle's charge and inversely proportional to the fourth power of the target particle's charge. Multiplying this is a function of the energy parameter, η , and the screening parameter, θ . The parameter θ tells you the extent to which the K-shell electron is not hydrogenic, that is, the extent to which the other electrons that screen the K shell make it easier for the K-shell electron to be removed from the atom than it would if the other electrons were not there.

We concluded the previous talk by raising a number of questions, just as an outline of what we need to discuss. The first question concerned universality. This cross section formula is universal in the sense that all the dependence on the target has been absorbed in these dimensionless parameters, so that once you write things in this form, all data should fall on one and the same curve, F versus η/θ^2 . The question that we asked was, is this the truth? This is only expected to be approximately true in the first place. In particular, it is expected to be true only at low energies, not at very high energies, so you do expect a departure from universality at high energies; let's see if that is true.

Question: Don't you mean it should be true at higher energies?

Answer: No, no. I am not saying that the Born approximation is not good at high energies; I am saying that the approximation that leads to this universal curve, which is more than just the Born approximation, is not good at very high energies. Of

course, we must also talk about questions as to where the Born approximation itself is good. Generally speaking, people sort of have a rule of thumb that the Born approximation is good at high velocities, but what velocities are high is a question we need to ask. Later, I want to say a little bit about that. But given that we are in the Born regime and that the velocity is not too low, then the universality is expected to be valid only in the lower region of that.

Generally, the cross section is represented by a curve going up and then coming down with increasing energy, where the peak region was roughly characterized as corresponding to a situation where the velocity of the incident particle is comparable to the velocity of the atomic electron, although that is a very crude way of putting it. There is nothing in the Born approximation that makes it valid only at or above the peak. The Born approximation is perfectly good for lower energies where the particle need not be fast compared to the atomic electron. But how far down you can go with the Born approximation is something we are going to have to worry about.

We then asked about the role that the description of the atomic electron plays. Can we do better, and should we do better, than using crude wave functions? Then there were other questions. If you go to the L shell, do the different subshells behave differently. Also there were questions of relativistic corrections, repulsion effects, polarization, and so on; these things we will come to later.

Let's go to the first slide (Fig. 1)¹⁾ before we do anything else. This is the same slide I showed yesterday. You do see at the higher energies perhaps an indication of universality breaking down.

The next slide (Fig. 2)²⁾ is again directly from one of Garcia's papers. What I am trying to show here is that the general ideas really do work; there is Coulomb excitation, and it is quite reasonably described by this solid curve, which in this case is a calculation using the binary encounter theory, (BEA). We'll talk

Fig. 1

about that a little bit later. It is really not much different from the Born approximation in that we could just as well have put the Born approximation curve on the graph, and it would give very nearly the same thing. The diagram at the lower right is for alpha particles. You see the theory also fits the large amount of data in that case. It is gratifying to see that the cross section turns over in the higher energy region. There is quite a broad peak, incidentally; it is not a sharp thing suddenly falling off. Note that this is a logarithmic scale.

The next slide (Fig. 3)³⁾ shows recent data from Harwell, where an experimental group is producing sort of isolated data that don't connect much with anybody else's, because these are all data for 160 MeV protons as incident particles. They have used quite a large number of different targets. The other data shown

Fig. 2

Fig. 3

on this figure are for much lower energy protons.⁴⁾ They were put on there because they are also in the region of this broad peak. You see that the cross sections indeed go down at high velocities. From this figure, I guess one can say that there is a breakdown of universality, although the error bars are very large. Also, I am not sure to what extent fluorescence yield problems come into the evaluation of these data. In looking at these data, I don't want to place any great stock in comparing the plane wave Born approximation with a similar curve for the binary encounter theory, although maybe the fit is a little better for one than the other. I don't think that the comparison is really significant, because the measurements are not accurate enough.

The next slide (Fig. 4)⁵⁾ I show to indicate that things

Fig. 4

look pretty nice when you have good statistics. This is for protons on silver, and these experiments were done by Shafroth and his collaborators in North Carolina. They are for projectile

energies ranging from 5 to 30 MeV, obtained with the so-called cyclograph accelerator (FN tandem Van de Graaff with a 15-MeV cyclotron injector). You can see the breadth of this peak; it just bends over and then very slowly comes down. It is not a sharp peak at all at the so-called velocity matching region. You see curves for both the plane wave Born approximation and the binary encounter approximation, and I refuse to get excited about which one fits better at these energies. Both experimental and theoretical cross sections here are absolute.

The next slide (Fig. 5)⁵⁾ is again for protons on silver,

Fig. 5

this time for the L shell. Whereas on the previous slide I don't take much stock in which theory fits better, BEA or PWBA, I think here it is significant that neither of them fits very well; that does mean something. Here, in the case of the L shell, we can easily go above the peak; you see the very large cross sections. The theoretical curves are based on hydrogenic wave functions, and

one suspects that better atomic wave functions would be required for the L shell. For the K shell, as we saw in the previous slide, probably hydrogenic wave functions are adequate; here they are likely to be totally inadequate. One should do this kind of thing over again with better wave functions. I guess various groups do have programs now to calculate generalized form factors using Hartree-Fock techniques. We also are developing such programs; I think we will probably begin to make such calculations quite soon.

The next slide (Fig. 6)⁶⁾ is for L x rays produced by

Fig. 6

protons impinging on gold, with a binding energy comparable to the K shell in silver. For gold, of course, you expect that perhaps hydrogenic wave functions for the L shell are relatively better than for silver. That seems to be borne out here. The dashed curve labeled as Hansteen and Mosebekk is for what is called the

semi-classical approximation (SCA), about which I will say more in a while.

We have seen in terms of the data that universality does seem to break down at the higher energies. We have also seen that probably Hartree-Fock type wave functions, or anyway improvements over just hydrogenic wave functions, are called for.⁷⁾ Incidentally, when I say hydrogenic, I don't mean that one completely ignores the other electrons, but one takes them into account in an extremely summary fashion through the screening parameter, θ , to which I had reference earlier.

In the next slide (Fig. 7)⁸⁾ you see another item from

Fig. 7

our list of problems to consider, and that is the question of whether the cross sections for the various subshells in the L shell behave differently. Here you see some calculations on that, and it looks as though they really do behave differently, especially at the lower energies. They still all have the usual structure, going up as the energy increases and then falling off gradually, but you

see that for the L_I subshell, which corresponds to a vacancy in the $2s_{1/2}$ level, you have a kind of knee here, and for the L_{II} subshell, which corresponds to a vacancy in the $2p_{1/2}$ level, you don't have that. There is no distinction here at all between vacancies in the $2p_{1/2}$ and $2p_{3/2}$ levels in terms of the dynamics, because the major forces that produce the vacancy have nothing to do with the spin. So, except for the fact that the binding energy is different for $2p_{3/2}$ and $2p_{1/2}$, you simply have a statistical factor, namely that there are twice as many electrons in the $2p_{3/2}$ subshell as in the $2p_{1/2}$ subshell. The binding energy is different, of course; the ionization potential is different; you take that into account. Other than that, there is no real difference, so I didn't plot L_{III} on this diagram. That curve would just be up by a factor of 2 from L_{II} .

There is a simple understanding one can get concerning how this knee comes about. It actually tells you something about the atomic wave functions; it is perhaps amusing for that reason. You remember that in the Born approximation the quantity in which you are interested is the square of the absolute value of the form factor. That is a rather rapidly decreasing function of the momentum transfer in the region of interest. Now, remember that there was a minimum momentum transfer, q_0 , which, comparatively, can be very large compared to 1 for heavy particle collisions. The dimensionless minimum momentum transfer Q_0 can be 100 or 1000. The generalized form factor drops rapidly for large q , so when you integrate over q , the only place that really counts is the minimum value. So, crudely speaking, the cross section is proportional to the square of the absolute value of the form factor evaluated at the minimum value of q . The minimum value of Q is the energy transfer over $\hbar v$: $q_0 \approx \epsilon/\hbar v$. Ignoring excitation to bound states of all these minima, the least value is that which corresponds to taking an electron out of the given shell, say an L subshell, and just barely putting it outside the atom with zero kinetic energy. In this case ϵ is the ionization potential. When you put that in for ϵ , that's the least possible value. So what you expect is that

the cross section will be a function simply of the ionization potential I over $\hbar\nu$:

$$F(q_0) = F(I/\hbar\nu) ,$$

whence

$$\sigma \sim |F(I/\hbar\nu)|^2 .$$

Now, remember that the form factor is the matrix element of $e^{i\vec{q}\cdot\vec{r}}$, in this case taken between the L-shell initial state, L_I , L_{II} or L_{III} , whichever it is, and the final state of the electron. We just said that the important contribution arises when the electron just barely makes it to the continuum. Then, not quite accurately, but roughly, you can say the electron has no kinetic energy at all, so the wave function in the continuum is almost a constant. That is not entirely correct; there is a slowly varying phase and all that. But it is approximately constant. This means that the form factor of importance is simply the momentum wave function for the particular subshell:

$$\text{form factor} \sim \int \text{const} \times \exp(i\vec{q}\cdot\vec{r}) \psi_{L_I}(\vec{r}) d^3r = \phi_{L_I}(\vec{q}) ,$$

evaluated at $q = q_0$. What you expect, therefore, is that as a function of velocity the cross section will mirror the momentum transform of the atomic state that you are exciting:

$$\sigma_n \sim |\phi_n(I/\hbar\nu)|^2 .$$

All of this is qualitative, and it is not terribly accurate, but I think it is basically right.

What you want to look at, then, are the differences in the momentum wave functions for L_I , L_{II} , and L_{III} . Do they look different? Consider the 2s and 2p subshells. The 2p wave function is the one that is most like a circular Bohr orbit. It has the highest angular momentum consistent with the $n = 2$ level, so it is described by a simple wave function with no radial nodes. The

probability as a function of r is concentrated at the Bohr orbit radius for the L shell. The $2s$, on the other hand, which corresponds to zero orbital angular momentum, doesn't have as much structure in terms of angular variables, but it pays the price and has an extra node in the radial wave function, both in coordinate space and in momentum space. If you look at the $2s$ radial momentum wave function, you find something like the following sketch:

There is a very large peak, a node, and then a long tail that goes way out. Remember now that as the projectile energy increases, the argument in the cross section expression goes down, so as you increase the energy you are moving from right to left on this diagram. Eventually, as the energy becomes large, you are sampling the region of the large peak. Of course none of this is completely true, so you don't really expect the cross section to go to zero, but you are not surprised that it dips down, makes a knee, and then comes up again. What one sees in Fig. 7 is a result of this minimum in the form factor when plotted in momentum space. The location of the knee in the $L_I(2s)$ curve shown in Fig. 7 corresponds fairly accurately to the node in the momentum distribution for the $2s$ state, which you can check out quite easily. If you could experimentally see this, then you would be happy because it would encourage you to feel you can really learn something about atomic structure from these experiments. Remember these calculations

were made using hydrogenic wave functions, and obviously that is a limitation.

Let me now show you some data. Fig. 8 shows ratios of

Fig. 8

cross sections for exciting the subshells L_{II} and L_{III} , $\sigma_{L_{II}} / \sigma_{L_{III}}$, as a function of projectile energy, after properly, one hopes, treating fluorescence yields, Coster-Kronig transitions, etc.⁹⁾ Admittedly, this is a very tricky business. I also have some examples of the raw data. Fig. 9 shows the type of data that one gets.¹⁰⁾ These are ratios of yields, $L\alpha/L\beta$, and so on. The bottom curve, for $L\alpha/L\beta$, is still puzzling. No one quite understands it, but it may have something to do with multiple excitations. The funny thing about that curve is that both of these x rays have to do with filling the $2p_{3/2}$ subshell, so you wouldn't expect anything but just a flat curve. Something that happens in the M shell of the atom may be causing this unexpected dip, but I don't want to focus on that because I don't understand it.

When you analyze the data, you obtain the ratios of cross sections for producing holes in the L_{II} and L_{III} subshells shown in Fig. 8. The reason that this is not just flat is that the binding

Fig. 9

energies are different, so they don't behave in quite the same way. If the binding energies were the same, this would be expected to be flat at 0.5, but that is not the case. Now let me show you the curve for σ_{LII}/σ_{LI} vs E_p (Fig. 10). Because of the knee in σ_{LI} you see a very decided peak here.

The way one makes contact between experiment and theory here is that a few years ago we calculated subshell ionization cross sections with hydrogenic wave functions and we published them in Atomic Data.¹¹⁾ I have to make a confession about these. They had several parts. There were K-shell results; they are fine, correct. Then there were L-subshell cross sections, excitation functions as we called them, tabulated, and we have found a mistake in these. The mistake is of a rather subtle nature. It came from a phase that was treated incorrectly; I won't bore you

Fig. 10

with the details. But because of the way it came about, the total L-shell ionization cross sections are correct, but the separate contributions for L_I and L_{II} and L_{III} are not correct. We wanted to set the record straight, so we have redone these things. The new results will also appear in Atomic Data.¹²⁾ The new results are a little more useful because you don't have to interpolate so much; we have many more energy and screening constant values than we had in the old tables.

Let's go back to the theory. In the previous talk we looked at the plane wave Born approximation and had the following relations:

$$\langle \vec{p}' | T^{PWBA} | \vec{p} \rangle = -4\pi Z_1 e^2 F_{n,n}(\vec{q}) / q^2 ,$$

$$d^2\sigma = 16\pi Z_1^2 (e^2/\hbar v)^2 (dq/q^3) |F_{en}(q)|^2 d\epsilon .$$

Then the question came up, for what energies is this approximation good? Very crudely speaking, if you use the criterion that I

mentioned yesterday, namely that $Z_1 e^2 / \hbar v$ has to be small compared to one, then you come to the conclusion that you should be o.k. with the Born approximation just as long as n is much greater than $(Z_1/Z_2)^2$, and that is usually much, much less than 1 -- perhaps not in the KSU experiments, but certainly for protons on lead. This is not a velocity matching criterion.

Now I want to say a few words about what is called the semi-classical approximation (SCA). In particular, I want to show the connection of that with other approximations. Remember the diagram I showed in the previous talk. We put the incident particle on a trajectory, just in case, as I said, we wanted to treat it not as a plane wave, but as a particle that moves on a classical orbit and passes by the atom and excites or ionizes it. That is the description that I am now calling the semi-classical approximation. It is also known as the Bang-Hansteen theory, because Bang and Hansteen¹³⁾ some years ago began to pursue this theory. Hansteen has stayed with it, and now Hansteen and Mosebekk¹⁴⁾ are publishing some very nice papers using this technique. Let's see what this involves. The atom is still treated fully quantum mechanically, so it is not a classical approximation as far as the atom is concerned, but it does assume that the incident particle can be treated as a classical point particle sweeping by. The trajectory may be straight or it may be crooked; we are not committing ourselves on that. Then it is simply time-dependent perturbation theory. The projectile sweeps by with an impact parameter, b . Of course we have to worry about various values of the impact parameter.

If you look in any self-respecting quantum mechanics book, you will find that the transition amplitude in first-order time-dependent perturbation theory is simply related to the matrix element of the interaction, the same thing that we had in the Born approximation. Now, you put that into a Fourier integral over all time:

SCA:

$$\begin{aligned}
 T_{n'n}^{\text{SCA}}(b) &= (-i/\hbar) \int_{-\infty}^{+\infty} V_{n'n}(\vec{R}) \exp(i\omega_{n'n}t) dt \\
 &= (iZ_1e^2/\hbar) \langle n' | \int_{-\infty}^{+\infty} [\exp(i\omega_{n'n}t) \\
 &\quad / |\vec{R}(t) - \vec{r}|] dt | n \rangle .
 \end{aligned}$$

Evidently, the only Fourier component that is needed is the one corresponding to the particular frequency of the transition that one is considering. Eventually, of course, one must square this and sum it over all possible transition frequencies. This is for a given transition from level n to level n' , let us say, and so the only thing that the atom responds to is the frequency with which it resonates. That's the easy way of putting it. You might wonder where t is in here. Well, t comes in through $\vec{R}$, the position vector of the particle sweeping by; that depends on the time whether it sweeps by on a straight track or on a crooked track. You have to evaluate this time integral from $-\infty$ to $+\infty$ and also take the matrix element. You can do it in either order; it doesn't matter. The question is, which is most convenient to do first?

If the projectile moves uniformly on a straight-line track, then the time integral can be done. You can look it up; it's a venerable thing:

$$\int_{-\infty}^{+\infty} [\exp(i\omega t) / |\vec{R}(t) - \vec{r}|] dt = (2/v) \exp(i\omega z/v) K_0(\omega \zeta/v)$$

Here, v is the projectile velocity, and ω the transition frequency corresponding to the jump that you are concerned about. The z -axis is the direction of the motion of the incident particle. The z -coordinate appearing here is that of the electron. Reference to the incident particle appears only in the velocity v , because we have integrated over the whole motion of the projectile, its past, present, and future history as it goes by. K_0 is a modified Bessel function of order zero. ζ is the distance of the electron from the straight-line trajectory; I defined that in the previous talk.

Whether the trajectory is approximately straight line or not depends on the situation. Obviously, a straight-line trajectory will be bad at small impact parameters. Also, it will be bad at low velocities, when there will be Kepler orbits. Really you should put a hyperbolic orbit in for $\vec{R}(t)$, so maybe the straight-line approximation is not the best thing to do. If you take into account the possibility that the trajectory is bent, rather than straight line, then you won't have that simple Bessel function anymore. Furthermore, even if you do this, notice that the next thing you have to do is compute the matrix elements between the atomic states. The electron coordinates come in in quite a complicated way; it is obviously a lot of mathematics, and if you look at Bang and Hansteen's paper, you see a lot of mathematics.

The following equations show what happens if I treat it the other way around, first taking the matrix element and then integrating over time:

$$\begin{aligned} T_{n',n}^{SCA} &= (-i/\hbar) \int_{-\infty}^{+\infty} v_{n',n}[\vec{R}(t)] \exp(i\omega_{n',n}t) dt \\ &= (iZ_1e^2/2\pi^2\hbar) \int [F_{n',n}(\vec{q})/q^2] d^3q \\ &\quad \times \int_{-\infty}^{+\infty} \exp\{-i\vec{q} \cdot \vec{R}(t) + i\omega_{n',n}t\} dt \end{aligned}$$

Instead of doing the time integration first, I have concentrated on the matrix element, and used its Fourier representation exactly the way we did for the Born approximation. The mathematical transformation brings in again our old friend, the form factor, the same way as before. That is why the SCA and PWBA are so similar in structure. Again we have the separation of an atomic factor and a factor pertaining to the motion of the projectile. The burden is on putting the motion of the projectile in for $\vec{R}(t)$ and then doing the integral over time. This is purely a classical mechanics problem; there is no quantum mechanics in that at all. We must evaluate that integral at the particular transition frequency of interest.

All nuclear physicists know that Alder, Bohr, Mottelson, Winther, and Huus,¹⁵⁾ in their famous Reviews of Modern Physics article on Coulomb excitation, had to treat basically the same problem. This is one place where nuclear physics is really simpler than atomic physics. Nuclear Coulomb excitation is simpler than atomic Coulomb excitation, treated this way, because in the nuclear case you are sure that the projectile always stays outside the nucleus because of the Coulomb barrier; you don't even talk about ordinary Coulomb excitation when that is not the case. So you have only one regime to worry about, where the projectile is outside the nucleus. In the atom that is flagrantly not the case. The projectile plows right through the atom, and therefore you have to make the distinction between the inside region and the outside region; it is messy, and you have to deal with it. The two regions are where r is less than R and where r is greater than R . It makes the integrals that occur not just twice, but 10 times as difficult. These difficulties can be seen in the paper by Bang and Hansteen.¹³⁾

Let me quickly indicate the connection between the semi-classical approximation and the Born approximation. When you actually make the calculations of cross sections, then lo and behold, you find that they give essentially the same answer. The graphs look exactly the same, so one suspects that there must be some basic, fundamental connection. We want to explore that. There is, of course, the great advantage of the semi-classical approximation that you don't have to use a straight line trajectory if you don't want to. You can put in a Kepler orbit, and then you do, in fact, get differences from the plane wave Born approximation. But I want to show the connection when the trajectory is a straight line. You put in $\vec{R}(t) = \vec{b} + v\hat{k}t$, where $\vec{b}$ is the impact parameter vector and $v\hat{k}$ tells you how far the particle has moved along the trajectory. $\hat{k}$ is the unit vector in the z -direction. You put this into the last formula I wrote for $T_{n'n}^{SCA}$. We are neglecting the fact that in the excitation process the incident particle is losing energy. It is possible to correct

for that, if necessary, but here we have assumed that the energy loss is negligible compared to the incident energy available. If you put that straight-line expression for $\vec{R}(t)$ into the formula for $T_{n'n}^{SCA}$, the time integral immediately gives a delta function. The argument of the delta function is quite simple, and that allows you immediately to perform the dq_z integration, that is, over the longitudinal component of the momentum transfer. The result is the following:

$$T_{n'n}^{SCA}(\vec{b}) = (iZ_1 e^2 / \pi \hbar v) \\ \times \int [F_{n'n}(q_x \hat{i} + q_y \hat{j} + (\omega_{n'n}/v) \hat{k}) / (q_x^2 + q_y^2 + (\omega_{n'n}/v)^2) \\ \times \exp(-iq_x b_x - iq_y b_y)] dq_x dq_y .$$

You are then left with only an integration over the two transverse components of the momentum transfer, q_x and q_y . It is a very simple looking thing; again it is just a Fourier integral.

To get a cross section you have to take the square of the absolute value of this expression and integrate over all possible impact parameters, which gives

$$\sigma_{n'n} = \int |T_{n'n}^{SCA}(\vec{b})|^2 db_x db_y$$

You can find a similar derivation in Bethe and Jackiw's book on Intermediate Quantum Mechanics.¹⁶⁾ I have just adapted it to our problem. Here then is the integration over all possible impact parameters in the x, y -plane, which is the transverse plane, perpendicular to the trajectory of the particle. By using Parseval's theorem from Fourier analysis, we are left then with an integral over q_x and q_y in this transverse momentum-transfer space:

$$\sigma_{n'n} = 4(Z_1 e^2 / \hbar v)^2 \\ \times \int |F_{n'n}(q_x \hat{i} + q_y \hat{j} + q_0 \hat{k})|^2 / (q_x^2 + q_y^2 + q_0^2)^2 dq_x dq_y ,$$

where

$$q_0 = \epsilon/\hbar v = \omega_{n',n}/v.$$

It is beginning to look more and more like the Born approximation. In front, the factor $Z_1 e^2/\hbar v$ has come in, just as you would hope. In the denominator you have something that is like q^4 , roughly, which is too much. But now you change this into plane polar coordinates and integrate over angles. You are then left with an integral over $q dq$. Then you integrate over all possible energy transfers which you must do in either the Born approximation or the semi-classical approximation. The result looks exactly like the formula for the plane wave Born approximation:

$$\sigma = 16\pi Z_1^2 (e^2/\hbar v)^2 \int \int_{q_0}^{\infty} [|F_{en}(q)|^2/q^4] q dq d\epsilon.$$

The integral doesn't go from zero to infinity in terms of these plane polar coordinates, but from the minimum value $q_0 = \epsilon/\hbar v$. This minimum came in through the delta function.

It is quite impressive that it really works out this way. You are now down to q^{-3} in the integrand, so we have arrived at the Born approximation. You might ask if this works also for electrons as projectiles. The answer is that you have to be very careful, because for electrons ω/v is usually not the minimum momentum transfer. That is only true for extremely high energy electrons, by which time they are relativistic anyway. But for electrons of moderate velocity, it simply isn't true, because these electrons will have a kinetic energy that is comparable to the energy loss, and the approximation that was used no longer works. That is why the equivalence of the approximation works for heavy particles, but not generally for electrons, so caution is indicated.

I want to emphasize that in inelastic collisions, the important thing is the longitudinal component of the momentum transfer; when you have a collision, you have a momentum change of the particle. For transferring energy to the system, it's the longitudinal component of the momentum transfer that is important.

For elastic scattering, the important thing is the transverse momentum transfer. That tells you how much deflection there has been. All of this is true when the energy loss is small compared to the incident energy.

Now, having established the equivalence, under certain rather general conditions, of the semi-classical approximation with straight-line trajectory and the plane wave Born approximation, the next question is how are these things related to yet another way of looking at collisions, the binary encounter approximation. How about some connection there? One can show,¹⁷⁾ that if you assume that the ejected electron is describable by a plane wave, the Born approximation reduces to the binary encounter approximation. That is, of course, generally not true, though if the electron comes out at very high energy, for instance, then it is a pretty good assumption. You can see the connection with the BEA in an intuitive way from the formulas we have written, because the $1/q^2$ really comes from Rutherford scattering; when you express Rutherford's scattering amplitude in momentum transfer quantities, it is $1/q^2$. When the final electron state is a plane wave, the form factor just becomes a momentum component of the initial wave function. In this way the velocity distribution of the initial electron enters the calculation, and this plays the central role in the binary encounter approximation. But I think it is absolutely essential that one assumes that the final electron wave function is a plane wave, and the connection stands and falls with that.

You might ask about the conditions for the semi-classical approximation to hold. Unfortunately, that is hard to know. When you treat the projectile classically, you'd say that its de Broglie wavelength has to be small. That's fine, but small compared to what? What is the important thing? Well, perhaps one thing that it should be small compared to is the radius of the atomic shell from which you are ejecting an electron, say the K shell, i.e. the hydrogenic Bohr radius divided by Z_2 : $\lambda \ll a_0/Z_2$. That's one condition that you might invoke. Another condition that is involved leads to saying that η must be much larger than the ratio of the electron mass to the projectile mass, $\eta \gg (m/M)^2$. That is,

in fact, always true and causes no restriction. On the other hand, that is probably not enough. You might also say that the de Broglie wavelength of the incident particle should be small compared to the collision diameter, that is, the distance of closest approach, to make classical arguments applicable and allow wave packets to be used. Now what is the distance of closest approach? I think Hansteen is too conservative. He requires that this be the distance of closest approach to the nucleus.^{13,14)} I don't think that's necessary at all, because the projectile doesn't collide with the nucleus; it collides with the electron. So there is no Z_2 in the condition; there is only a Z_1 : $\lambda \ll Z_1 e^2 / mv^2$. When you use that condition, it follows that η must be smaller than $(Z_1/Z_2)^2$, which is just the other way round from what we had in the Born approximation, where η was to be much larger than that. In a sense, this is strange. We showed that the two approximations (PWBA and SCA) are equivalent, and yet they are applicable in different regimes. Mathematically they give the same formula. The mystery then is, are you right or are you wrong if you use the same formula in the energy region between the PWBA and the SCA regimes where neither of them is valid?

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ENERGY LOSS PROBLEMS

Today I will try to say something about departures from the Z_1^2 law. As I understand it, it was first realized in energy loss measurements that inelastic atomic processes were not proportional to the square of the charge of the incident particle. So far, since we have been using first-order perturbation theory in all the theorizing we have been doing, all inelastic cross sections have been proportional to Z_1^2 . It can't be otherwise in first order for the Coulomb interaction.

Question: Does that remain true if you have a non-straight-line trajectory?

Answer: You mean the Z_1 would go into the trajectory. That's right so I guess it is not true then, because certainly the trajectory will be affected by Z_1 . In the second talk we showed that the plane wave Born approximation was equivalent to the SCA, or that at least we got the same formula that we got from the semi-classical approximation when we used a straight-line trajectory. That makes good sense. The plane wave Born approximation ignores completely the interaction between the projectile and the nucleus. That is not to say that there is no deflection in the Born approximation; by all means there is deflection. In fact, the Born approximation provides the transition amplitude for a momentum change. Still, as it turns out, for heavy particles there is a predominance of forward inelastic scattering, because if it collides with an electron, even with a bound electron, a heavy charged particle is going to have so much momentum that it is going to go in the forward direction, so the deflection that comes from the inelastic process itself is going to be small. The deflection that comes from the elastic interaction with the nucleus Z_2 is certainly important, but only at low velocities. It is usually not going to be very significant. Bang and Hanstvedt¹⁾

have set up the SCA formalism, using Kepler orbits rather than straight line trajectories, but very little has so far been done with this formalism. One could also try, in the Born approximation, replacing the plane wave by a distorted Coulomb wave. In either case, the mathematics becomes quite complex. Apparently these are corrections of only a few percent and are important only if the projectile has low velocity.²⁾

Question: What does low velocity mean here? Does it mean that the nuclei do not get very close together?

Answer: The regime where the deflections are appreciable, for protons say, begins around 100 keV, something of that order of magnitude, quite low. Of course for heavy ions it would be more important. As Z_1 goes up, you begin to worry more about that, definitely. In any case, the last word has not yet been said in that field.

When you do take repulsion into account, it appears to diminish the cross section from what it was in the comparable situation for a straight-line trajectory. I really don't have more to say about that. I know how to set up the formulas, but we haven't done any really good evaluating.

Anyway, to the extent we isolate the Z_1 -dependence that comes from interaction between the incident charged particle and the atomic electron, it is all Z_1^2 . This has been known for a very long time; stopping-power formulas all have Z_1^2 in front. Stopping power, of course, is a venerable subject in physics; it goes back to Thomson and Bohr in the early part of the century, and was given a quantum mechanical basis by Bethe²⁾ in the early thirties. I am not concerned about the so-called nuclear contribution (although that's important for heavy ions), which is the contribution from the elastic collision between a heavy ion and a recoiling atom, but I am referring to the energy loss that arises from inelastic energy transfer to the electrons in the atom. The energy loss is given by simply taking the cross section for going from the initial state of the atom, n , to some state n' , and then multiplying that by the energy transfer and summing over

all n' :

$$\Delta E \sim \sum_{n'} \sigma_{n'n} (E_{n'} - E_n) .$$

That is proportional to the mean energy loss per encounter. Bethe's calculation of the stopping power, $-dE/dx$, uses the plane wave Born approximation for $\sigma_{n'n}$, so everything is proportional to Z_1^2 . The famous Bethe-Bloch formula has the form

$$-dE/dx = (4\pi e^4 n Z_2 / m v^2) Z_1^2 \log(2m v^2 / I_{av}) .$$

For nonrelativistic particles, that is essentially it. Here m is the mass of the electron, v is the velocity of the incident charged particle, n is the number of atoms per unit volume of the target material, and I_{av} is some kind of an average ionization potential. I_{av} is supposed to be independent of Z_1 and of the energy; that is the important thing about it, but it does depend on what atom you are talking about. There are simple rules about how I_{av} depends on Z_2 , but none of them are really reliable. The simplest relation is that I_{av} is supposed to be proportional to Z_2 ; $I \approx 11.5 Z_2$ eV. Experimentalists know these relations better than I do.

This is all right for energy loss to most of the electrons, but it really does not accurately represent the energy loss to the inner electrons. So-called shell corrections are made to take into account the fact that the inner-shell electrons are much more tightly bound than average, and one calculates their contributions to this formula separately. That is an interesting subject; we need to know inner-shell inelastic cross sections in order to determine if the calculations of these inner-shell corrections to the stopping power are correct. Hans Bichsel at the University of Washington has become the world's repository of maximum information on this subject, and you can look up what he says about it. It's a very tricky and difficult sort of thing, but important if you really want to know about stopping power.

I really don't want to go into the Bethe calculation in any great depth since it is quite familiar and can be found in many textbooks, but I do want to point out Bloch's contribution to the Bethe-Bloch formula. Bloch wrote a paper in which he made some statements about how I_{av} behaves, and he made some statistical atom (Thomas-Fermi) calculations.³⁾ But I want to stress that Felix Bloch wrote two papers, not just this paper that people usually refer to in connection with the Bethe-Bloch formula, but he gave an independent derivation of the stopping-power formula,⁴⁾ just a little bit after Bethe. He wrote this in Copenhagen under the influence of Niels Bohr, who through his whole life was concerned about stopping problems. Bohr wrote a number of very important papers, including his review paper in 1948,⁵⁾ which has a number of interesting insights in it. Bloch's paper in *Annalen der Physik* is well worth reading. It is in German, so it might take a little effort, but it is a beautiful paper. It was published in 1933, and I think it contains many ideas that must have been brand new. It is a long paper, quite complicated in many ways, and it is devoted to an effort to calculate the stopping-power expression, but not in the plane wave Born approximation; Bethe had already done that in his paper.²⁾

Bohr had made a completely classical calculation in 1913.⁶⁾ Bloch carried out a semi-classical calculation, an SCA type of approach, for the so-called distant collisions where the charged particle sweeps by the atom on the outside. He treated these by a time-dependent straight-line approach. The interaction then induces, through time-dependent perturbation theory, transitions in the atom, as in the SCA theory of Bang and Hansteen. For the close collisions, where the charged particle really goes right through the middle of the atom, he used essentially a quantum version of the binary encounter approximation. He used an impulse approximation; he treated the collision between the charged particle and the electron accurately as a collision with a free electron (Rutherford scattering), and then he averaged this over the momentum distribution of the electron in the atom. Then he

patched the two parts together. That is always the difficulty, piecing them together to make one formula out of it. The various versions of the Bethe-Bloch formulas that you find in the literature differ because different authors use different patching techniques.

When Bloch was all done, he had a formula that was very much like what I wrote before, but not identical. It starts out like Bethe's formula but there are additional terms as follows:

$$-dE/dx = (4\pi e^4 n Z_2 / m v^2) Z_1^2 \times \{ \log(2mv^2/I) + \psi(1) - R_e \psi(1 + i Z_1 e^2 / \hbar v) \} .$$

ψ is the logarithmic derivative of the gamma function. It has a complex argument here; $R_e \psi$ denotes the real part of ψ . Bloch pointed out immediately that if $Z_1 e^2 / \hbar v$ is much less than one, and that was the Born approximation criterion, then the two ψ terms just cancel, and you get Bethe's result. If $Z_1 e^2 / \hbar v$ is much greater than one, it turns out that you can make an asymptotic expansion, and you obtain Bohr's classical result,

$$-dE/dx = (4\pi e^4 n Z_2 / m v^2) Z_1^2 \log(1.123 m v^3 / Z_1 e^2 \omega) ,$$

where $\omega = 1_{av} / \hbar$ is some average oscillation frequency of the electrons in the atom.

The burden of all this is just to say that in the theory of stopping power there is a place where there is something other than the Z_1^2 dependence. This departure from a Z_1^2 law is rather difficult to track down. It comes, in fact, from patching the two regimes of distant and close collisions together. If you make a power series expansion of the terms involving ψ , by consulting the standard mathematical references, you find that the corrections are all even powers of Z_1 ; there are no odd powers:

$$-dE/dx = (4\pi e^4 n Z_2 / m v^2) Z_1^2 \\ \times [\log(2mv^2/I) + a Z_1^2 + b Z_1^4 + \dots]$$

I don't think people ever used this except for a short time after the second world war when there were questions about the stopping power of fission fragments, and people thought maybe this was the thing to use for fission fragments. I think they very quickly realized that this was not really all that reliable, because there is electron capture and loss, and that wasn't included here at all. All of this merits much more discussion than we've had, but I am not prepared for that.

On the other hand, in the past ten years or so it has become apparent that there are, in fact, experimental deviations from the Z_1^2 law of stopping power. These were first pointed out by Barkas⁷⁾ quite some time ago. He found that the stopping powers for Σ^+ and Σ^- hyperons were not the same; the ranges were not the same. It turns out they are not each other's antiparticle, so their masses are not identical. But even after that is taken into account, there is still a difference in range. They are essentially the same particle, but one has a plus charge and one has a minus charge; if you have a Z_1^2 stopping law, then their ranges should be the same. That makes good sense. Remember, the Rutherford scattering cross section doesn't depend on whether it's positive or negative, whether it's repulsion or attraction. That's always a surprising, fantastic thing, that even though the orbits are so different, when you finally get the cross sections they don't know the difference between repulsion and attraction; they just depend on Z_1^2 in the nonrelativistic case. It appears that Fermi made some remarks about this,⁸⁾ suggesting that if instead of using the Rutherford scattering cross section, which does go like Z_1^2 , you use the Mott scattering expression, which is the relativistic scattering cross section that you should be using, since the velocities are high, then there are corrections involving odd powers of Z_1 . So he suggested that somehow this was a relativistic effect. Actually, that turns out not to be the principal

explanation, because the range differences occur at the very ends of the ranges of these particles, and they are quite slow there.

More recently, people have found that π^+ and π^- have different ranges by a few per cent. The π^+ has the longer range. Then there were some Danish experiments by Andersen et al. comparing protons and alpha particles. On a Z_1^2 basis you expect a factor of 4 difference. In the region of 1 to 5 MeV for protons and alpha particles on Al, it was found that the alpha particle had a stopping power greater than 4 times the stopping power of the proton by again a few per cent, depending on the energy.

The nice experiment to do would be to compare the stopping powers of protons and antiprotons. I have been plugging for that; it shouldn't be impossible to do. The suggestion then is that there must be an odd power of Z_1 correction, so that you could have a difference between + and -; otherwise, you wouldn't have that.

Question: I'm not sure whether it is relevant, but there were some beautiful experiments by Allison⁹⁾ and his group in the late fifties, where they did the experiment demanding no charge exchange by doing it in a field, something like that. They got a very substantial difference in the stopping power between protons and neutral hydrogen, for example, more than a few percent.

Answer: This is relevant, but hard to interpret in detail. I think it is very important. The comparison of cross sections for protons and neutral hydrogen gives you an idea to what extent the electron might be regarded as just shielding. Clearly that's touchy, treacherous business.

Let me give you a quick rundown of what has happened recently. The stopping-power problem has become interesting to those who are looking in the cosmic radiation for super-heavy nuclei. If one uses some kind of stopping power criterion, track structure or anything like that, to estimate what Z_1 is, he had better know if there is some Z_1^3 contribution, because it is going to be very large for a super-heavy nucleus. If the

stopping power has a term that goes like Z_1^3 ,

$$-dE/dx = (4\pi e^4 n Z_2 / m v^2) [Z_1^2 \log(2m v^2 / I) + O(Z_1^3)] ,$$

then these people need to know about it. Of course again the charge exchange problems come in; such ions would have to be at very high velocities to avoid charge exchange and where one wouldn't be confused by other effects. Therefore, one needs to look at the relativistic situation, and there probably Fermi's suggestion is right. Several papers have been published on the relativistic Z_1^3 correction.^{8,10)}

Since people have seen departures from the Z_1^2 law in inner-shell excitation,¹¹⁾ it seemed pertinent to take a look at this problem and try to determine where this all comes from. The origin of the departure from the Z_1^2 law in Bloch's theory is entirely in what I would call the gentle collisions. These are collisions that are either taking place way outside the atom, so that there can't be much change of momentum or energy of the incident particle -- all so-called distant collisions are by definition gentle -- or they are among the close collisions. The close collisions can be very hard, of course, if you go right through the atom; the particle can turn back 180°. But among these close collisions there are gentle ones with small deflection. In Bloch's treatment, it is only the small deflection ones that contribute to this departure from the Z_1^2 law. That suggests that, in calculations of the inelastic cross sections for inner-shell excitation and ionization, one should pay particular attention to these gentle collisions, and see whether he has treated them right or whether he can do better.

Many of us have been thinking about this for a long time. The obvious first thing to do is to try to do better perturbation theory, that is, go to higher order perturbation theory, and see what it does. It was clear that one would get departures from the Z_1^2 law; that's easy to see. It was also

clear that the calculations were going to be not only difficult, but also ambiguous, because when you go from first-order to second-order perturbation theory, you have constantly to sum over intermediate states, and that is very hard to do correctly. Many people tried their hand at it, but nobody knew who had the right answer, because everybody had made some approximations that were not easy to keep track of. However, an unambiguous and very interesting contribution to this subject was made recently by Ashley, Ritchie, and Brandt.¹²⁾ They went back to Bohr's 1912-13 treatment of these problems, and carried out an entirely classical calculation; it has no quantum mechanics in it initially at all. They put the charged particle on a straight-line trajectory and let it sweep by an atom, and they did a good dynamical calculation for the distant collisions. They went to the next approximation beyond Bohr's approximation. Like Bohr, they approximated the atom by an isotropic harmonic oscillator; instead of having a real atom, they had a charged particle in an isotropic harmonic oscillator well. This means in this model there is no ionization; the particles can never get away; they can only be excited. Of course it's this model that led to the whole notion of oscillator strength for atoms, so it wasn't all that farfetched, and it was manageable.

I shall describe a calculation using a very similar model that K. W. Hill and I have done.¹³⁾ This calculation does the same thing quantum mechanically. It is a SCA type perturbation theory. You again have a particle moving on a straight-line track, sweeping by an atom, and again the atom is represented by an isotropic harmonic oscillator. I think you will see in a moment why that helps in the computation, and one can also hope to learn something about real atoms. This leads to a Z_1^3 correction; this is a polarization effect, which is presumably important at lowish velocities. It dies out at high velocities. I'm not going to claim anything fantastic for this, but I think it deserves to be looked at, because it is interesting what physical features lead to this Z_1^3 correction, and I think that one can begin to understand it.

The idea is this. We are still confining ourselves to the so-called distant collisions, the regime of particles that move by outside the atom, so to speak. It is hard to know where "outside" is for a harmonic oscillator, because it goes to infinity; there are very highly excited bound states. But I think we can agree on the size of an atom, and I am going to look only at impact parameters that are greater than the size of the atom. So we're ignoring the close collisions, at least temporarily and make a multipole expansion, just as Bloch did for the distant collisions. Here is our old friend again, the interaction between the charged particle that's moving as a function of time and the electron:

(Monopole)

$$V = -Z_1 e^2 / |\vec{R}(t) - \vec{r}| = -Z_1 e^2 / R$$

(Dipole)

(Quadrupole)

$$-Z_1 e^2 (\vec{r} \cdot \vec{R}(t)) / R^3 - \frac{1}{2} Z_1 e^2 \{ 3(\vec{r} \cdot \vec{R}(t))^2 / R^5 - r^2 / R^3 \} + \dots ,$$

where $r \ll R$. We omit octupole and higher terms.

When using this expansion to calculate transition probabilities, Bloch couldn't possibly have gotten any odd powers of Z_1 , because he stopped with the dipole term. You have to have quadrupole contributions before you can hope to get anything like an odd power in Z_1 , since these come, in fact, from the cross terms between dipole and quadrupole contributions. Schematically, in perturbation theory the transition amplitude appears as:

$$T = (Z_1 e^2 / \hbar \omega) \left[\underset{E1}{r/R^2} + \underset{E2}{r^2/R^3} + \underset{E3}{r^3/R^4} + \dots \right] \\ + (Z_1^2 e^4 / \hbar^2 \omega^2) \left[\underset{E1E1}{r^2/R^4} + \underset{E1E2}{r^3/R^5} + \underset{E2E2}{r^4/R^6} + \dots \right] .$$

This is entirely schematic; I haven't written down any quantum

mechanics at all. In actuality, all terms have matrix elements attached to them. The second line represents the second-order matrix elements. These describe a situation where the interaction acts twice; you have the dipole interaction twice, then you have once a dipole and once a quadrupole, then you have two quadrupoles, and so on. We have two concurrent expansions here, the multipole expansion and a perturbation expansion. They go together, but one has to organize them, and one has a bookkeeping problem.

Eventually we have to take the square of the absolute value of the entire transition amplitude. Evidently, we are going to get Z_1^2 terms from all of the terms in the first row. The second row terms, by themselves squared, are going to give Z_1^4 terms. In between, from the cross terms, we are going to get terms involving Z_1^3 . So there is every reason to expect Z_1^3 terms in the cross section.

If we confine ourselves to dipole and quadrupole interactions for distant collisions, the dynamics of the system is governed by the Hamiltonian:

$$H = H_1(t) + V_2(t) \quad ,$$

$$\text{where} \quad H_1(t) = H_0 + V_1(t) = p^2/2m + \frac{1}{2}m\omega^2 r^2 - Z_1 e^2 \vec{r} \cdot \vec{R}(t) / [R(t)]^3.$$

$$\text{and} \quad V_2(t) = -\frac{1}{2} Z_1 e^2 [3(\vec{r} \cdot \vec{R}(t))^2 / R^5 - r^2 / R^3] \quad .$$

$H_1(t)$ describes an isotropic harmonic oscillator with a dipole interaction, which is the leading term of the perturbation. The advantage of using the model of an isotropic harmonic oscillator, instead of a real atom, is that then one can treat the dipole term, not in perturbation theory, but exactly, and this is the only system for which one can do that. Then one can treat the quadrupole term $V_2(t)$ as a first-order perturbation, and second-order perturbation theory is thereby avoided.

The key feature of $H_1(t)$ is that the dipole term is linear in the electron variables x , y , and z , each term having a

coefficient that is time dependent. The time dependence is due to the motion of the incident particle. Thus, one has here an isotropic harmonic oscillator that is forced. The force is constant in space over the whole extent of the harmonic oscillator, but time dependent. This problem can be done rigorously, both classically and quantum mechanically. It is the paradigm of time-dependent quantum mechanics; it is the most complex problem that can be solved exactly. In order to work in quantum mechanics, I rewrite $H_1(t)$ in terms of raising and lowering operators as follows:

$$H_1(t) = H_0 + \vec{f}(t) \cdot \vec{a} + \vec{f}^*(t) \cdot \vec{a}^\dagger ,$$

where $H_0 = \hbar\omega(\vec{a}^\dagger \cdot \vec{a} + 3/2)$

and $\vec{f}(t) = (-Z_1 e^2 / \{R(t)\}^3) \sqrt{\hbar/2m\omega} \vec{R}(t) .$

We can solve the time-dependent Schrödinger equation for $H_1(t)$ exactly and obtain the time-development operator. This is not too hard to do, but it's lengthy.

The key quantity that enters all of it is the Fourier transform of the time-dependent perturbation:

$$\vec{g}(\omega, t_2, t_1) = \int_{t_1}^{t_2} \exp(-i\omega t') \vec{f}(t') dt' .$$

Just like any harmonic oscillator, classically as well as quantum mechanically, this harmonic oscillator, when it is perturbed, in the long run responds only to the Fourier component that has just the right frequency and resonates with it; it ignores all the other frequency components of the time-dependent perturbation that hits it.

So, we treat the dipole problem exactly, and then we introduce, as a first-order perturbation, the quadrupole interaction. The quadrupole interaction is written out formally as follows:

$$V_2(t) = \sum Q_{jk}(t) (a_j^\dagger a_k^\dagger + a_j a_k + 2a_j^\dagger a_k) + \sum Q_{jj}(t);$$

$$Q_{jk}(t) = Q_{kj}(t) = Q_{jk}^*(t) \quad .$$

Since it's of the quadrupole type, we have terms x^2 , y^2 , z^2 , xy , yz , and xz , all quadratic in the displacements. When you translate that into raising and lowering operators, you get the expression I have written. A dipole term can take you only from one oscillator level to the next one; a quadrupole term can take you two steps up, two steps down, or one step up and back down again to where you came from. The time-dependent coefficients again depend on the motion of the incident particle. No commitment has yet been made, incidentally, about whether to use a straight-line trajectory or a deflected trajectory. It could be done in terms of a deflected trajectory if you wanted to invest the time.

Now, we want to calculate the energy loss. We are putting a particle on a track with a certain impact parameter, and describing the particle motion classically, but we are letting it perturb the atom quantum mechanically as a time-dependent perturbation, the atom being an isotropic harmonic oscillator. That's the picture. Now, suppose that this atom is initially in its ground state, the lowest harmonic oscillator state: $\psi(-\infty) = |000\rangle$. What is the average energy transfer after the collision is over? In other words, what is the expectation value of the energy in the final state after the collision has occurred? Here is the answer to behold:

$$\begin{aligned} \langle +\infty | E_0 | +\infty \rangle &= (3/2)\hbar\omega + \hbar\omega |\vec{g}(\omega)|^2 \\ &+ 2i\omega \int_{-\infty}^{\infty} \sum Q_{jk}(t) [g_j(\omega)e^{i\omega t} + g_j^*(\omega)e^{-i\omega t}] \\ &[g_k(\omega, t, -\infty)e^{-i\omega t} - g_k^*(\omega, t, -\infty)e^{i\omega t}] dt \quad , \end{aligned}$$

where $\vec{g}(\omega) = \vec{g}(\omega, +\infty, -\infty)$. It's the initial energy, of course, plus a dipole term. The first line compares the initial energy and what you would have if you had just the dipole term, and this expression is exact and not just valid in first-order perturbation theory. The next lines are the result of lowest-order perturbation

theory applied to the quadrupole interaction.

The first line of this formula, pertaining to the dipole interaction, is exactly the same result that you would get if you did this calculation classically. There are $\hbar$'s here, but actually they are going to cancel out, so the result really does not depend on $\hbar$. That is very well known, of course.

Let me indicate the final results of all this and you refer for details to the literature.¹³⁾ If a certain time reversal symmetry condition is satisfied, one has a strikingly simple conclusion.

$$\text{If} \quad \Sigma Q_{jk}(-t) g_j^*(\omega) g_k^*(\omega) = \Sigma Q_{jk}(t) g_j(\omega) g_k(\omega),$$

holds for the trajectory, then

$$|<+\infty|-\infty>|^2 = \exp\{-\Delta E_0/\hbar\omega\} [1 - (\Delta E - \Delta E_0)/\hbar\omega],$$

$$\text{where} \quad \Delta E = <+\infty|H_0|+\infty> - (3/2)\hbar\omega$$

$$\text{and} \quad \Delta E_0 = \hbar\omega |g(\omega)|^2.$$

This is easy to explain. $|<+\infty|-\infty>|^2$ is the probability that after the collision our harmonic oscillator, which initially was in its ground state, will still be in the ground state, i.e. that it will not have been excited at all. Thus, $1 - |<+\infty|-\infty>|^2$ is the total excitation probability. ΔE is the average energy transfer in the presence of the quadrupole term. ΔE_0 is what that would be if there were only a dipole interaction.

If you make an expansion of this exponential, because the average energy loss is small compared to $\hbar\omega$, then you find that the excitation probability becomes approximately $1 - \Delta E/\hbar\omega$. In other words, it's as if the transition had been only to the first excited state, because that is the energy jump $\hbar\omega$. The energy loss is simply the probability that excitation will occur times $\hbar\omega$, and so it looks as if only transitions to the first excited state are important.

Question: Could you also get a simple closed form answer if, instead of using a forced harmonic oscillator as the unperturbed system, you use second-order perturbation theory, and use the unperturbed harmonic oscillator as your zero order Hamiltonian?

Answer: Yes, we did that. If the dipole as well as the quadrupole interaction is treated in perturbation theory, these are the transitions that are relevant:

The oscillator ground state is the $1s$ state. Then there is a p state and the second excited level is what nuclear physicists call the s - d shell. The second-order transitions that contribute are indicated in the figure. Either a dipole excitation takes you up to the first excited state, and then you have a quadrupole interaction causing no further excitation, or you go by quadrupole interaction from $1s$ to $1d$, and then you go back down to the first excited state by an electric dipole transition. These are the only important contributions to this excitation process, and they give a Z_1^3 contribution to the probabilities and to the cross section.

Now one can check if the two methods of calculation - treating the dipole interaction exactly or by perturbation theory - give the same answer. That's useful to do, because the exact calculation for the harmonic oscillator is a curiosity, and it is not generalizable to an atom. The reason that you can do it exactly is that an isotropic harmonic oscillator has a marvelous

property. The potential is proportional to r^2 . It is separable in spherical polar coordinates, of course, but it is also separable in Cartesian coordinates, and it is also separable in cylindrical coordinates. There is no other system like it, and that is why you can do so much with it exactly. But if you want to learn how to translate these results to real atoms, you better do the perturbation theory, because that is what you are going to be dependent on for real atoms. We have done it both ways. It's a fairly lengthy calculation, so it is nice to do it in two quite independent ways and see that they agree. One hopes that the perturbation theory way of doing it will be generalizable to real atoms. The difficulty is that even in perturbation theory the harmonic oscillator is distinguished in that the selection rules are much simpler than for any other system, so we are still not anywhere near doing it for real atoms.

The result of perturbation theory for the energy loss ΔE is identically the same as the result obtained in the purely classical calculations by Ashley, Ritchie, and Brandt. However, they cannot derive probabilities; they can get only the energy loss, because the energy loss is meaningful classically, but the transition probability is not. So the quantum calculation supplements their work by providing also an expression for the probability.

An important advantage of the method which treats the dipole interaction exactly, instead of by perturbation theory, is of course that the calculation is then appropriate for heavy ions (arbitrary Z_1). The strength of the interaction appears in the exponential $e^{-\Delta E_0/\hbar\omega}$, and this shows the saturation of the excitation probabilities for large values of Z_1 .

Detailed numerical evaluation then depends on how you adjust this oscillator model to real atoms. Ashley, Ritchie, and Brandt have done this in a certain way, and they feel that they have been able to explain the departures from the Z_1^2 law, the Z_1^3 contributions to the energy loss. The Z_1^3 effect in the data on inner-shell excitation has also been interpreted as arising

from this polarization effect. Our purpose here has been mainly to show what the physics of it is, that it comes from the quadrupole interactions in addition to the dipole processes, and that it is a cross term.

Question: Do you have any idea what the dependency of these Z_1^3 effects on the energy of the projectile is?

Answer: The Z_1^3 effect decreases as the velocity increases, but the question is, with what power? It decreases as some fairly high power of the velocity. Being a polarization effect, you expect it to decrease as the particle gets swifter. The specific law of decrease depends on how you apply the model to the atom. There are different ways of doing that, of trying to go from a harmonic oscillator to a real atom, and it depends very much on what you do there.

Question: How do you determine the oscillator strength?

Answer: Ashley, Ritchie, and Brandt use a statistical atom approach, and it gives them a distribution of oscillator strength. Then they treat the atom as an ensemble of harmonic oscillators with appropriate weights. We have been thinking about doing something fancier, but I don't know if we will be able to do it. We thought, in this age of group theory that we might exploit the known connection between the isotropic harmonic oscillator and the hydrogen atom. This is a problem that you can give your quantum mechanics students. If you look at the bound state wave functions for a hydrogen atom and the wave functions for an isotropic harmonic oscillator in spherical coordinates, they both involve associated Laguerre polynomials. The arguments are different; in one case it's r and in the other case it's r^2 . But there is a direct connection between the two, and you can transform the Schrödinger equation for the hydrogen problem into the harmonic oscillator equation. This is the starting point for some work by K. W. Hill.

Comment: There are probably several other ways you can come up with Z^3 -dependent terms. One of them is to use the concept

of final state polarization, where the electron essentially goes off and to some extent tends to be associated with the projectile. That is fundamentally a high energy type of thing, but if you are interested in stopping power, it seems to me that is something you should think about.

Reply: When Jim McGuire says "final state polarization," I think what he means in relation to our discussion is this: In all my lectures so far, I have always assumed that all the projectile does is interact with the atom, and of course that is not right. The projectile itself has energy levels. Even if it is a proton, the proton has hydrogenic levels that go with it. We have completely neglected these, and Jim's remark correctly points out that this is not quite right. In the treatment that I was describing, the level structure of the projectile would appear in a very complicated way. The set of states used is complete, and these features are somewhere; but they are not visible in any primary way. Rather, they appear at the tail end of an expansion, and therefore are completely dropped from sight. Now, when Jim says "final state polarization," I believe he means that somehow that part of the system should be emphasized and more properly taken into account. If that is done, for instance, there is a chance for the electron to be captured, or nearly captured, or somehow to be interacted with in a dynamical way other than just to be ionized and ejected. In other words, the projectile plays a greater role than just tickling the electron; it also attracts it to itself.

When you go to heavy ions, high Z_1 's, then you are going to have to worry about that sort of thing much more. The point of view that says that all the incident particle does is to perturb from the outside is then not adequate. I agree.

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MULTIPLE EXCITATION AND OTHER HIGHER ORDER PROCESSES

I hope that this talk can be open ended. I would like very much to generate discussion of some of the experiments that have been done here at K.S.U. I want to talk mostly about such things as multiple ionization, multiple excitation, and about small impact parameters. But before we do that, I want to go back just for a moment and discuss something that I read this morning in one of the numerous preprints that you have given me about the Z_1 dependence of inner shell vacancy production.

Yesterday I talked about energy loss because that is what motivated the question about the Z_1^3 behavior originally. It is historically interesting, and it is also interesting to many people in a practical way. But your concern is much more directly with the question of whether the inelastic cross sections for exciting and ionizing inner shells of atoms behave as Z_1^2 or not in the Coulomb excitation regime. (We are assuming that that is where we are; we are not talking about the quasi-molecular excitation regime, where the situation could, of course, be quite different.)

I think some of us working in this field have, in between the periods of exultation, occasionally wondered where we are going, what is all of this about, are we really learning something coherent about these atomic processes. I think that the answer is definitely yes. Don't be discouraged by the fact that we are just nibbling away at bits and pieces; it is a large complicated field, and you cannot expect a really simple, coherent picture. We need a lot more data to get anywhere, to make things hang together, and it will take a lot of effort. Some progress has been made, and a lot of progress can yet be made. I know that sometimes one feels all right, so there's another datum, so what? But I do think that we are getting increased understanding. Model calculations, like the one I outlined yesterday, are just crutches;

and obviously eventually they have to be replaced. The spirit behind them is to acknowledge that you can't do everything well in these calculations; you cannot have the best of all possible worlds and treat the collision mechanism as well as the atom properly. You can't do all those things simultaneously, so you have to make choices, and you have to attack the problems in many different ways.

The calculation I was outlining was quite deliberately one in which we abandoned hope of describing an atom anywhere near correctly, so in this regard one shouldn't expect too much. On the other hand, we were at least doing the perturbation theory for distant collisions right. We ignored close collisions, and maybe the distant collisions aren't all that important. Brandt et al.¹⁾ say they are not, but this is difficult to prove. We don't really know, and therefore you have to put ifs and buts in there. I know that some experimentalists don't like that, because it means that the calculations are not immediately applicable to interpretation of data which do not distinguish between distant and close collisions. If you have a collision in which the distant collisions are important, then the kind of multipole expansion that I indicated is certainly going to be valid. But remember we are doing two expansions. One is the multipole expansion; the other is the perturbation theory expansion, and for perturbation theory to be valid, the interaction must not be too strong:

$$0.16 Z_1 \sqrt{\frac{M \text{ amu}}{E \text{ MeV}}} \approx \frac{Z_1 e^2}{4\pi v} \ll 1 .$$

That gives you a feeling for the situation. With heavy ions you may be in a region where you are getting pretty close to unity. For protons, on the other hand, Z_1 is just one, and then you are in very good shape over a considerable range of velocities.

Question: Are we talking about an expansion in terms of $Z_1 v_0/v$, and the higher order terms are being neglected in first-order perturbation theory? Or are we just talking about something

in perturbation theory that is not a power expansion of this parameter, but with higher order terms that perhaps do not go up with this parameter but can dominate over the first-order terms?

Answer: It is not so simple. The quality of a perturbation approximation depends on more than just one parameter, but if there is one, it is $Z_1 e^2 / \hbar v$. Certainly the second-order Born approximation is going to have this, squared, in front of it, but it is going to have a lot of other things that come from the summation over intermediate states and so on. It is not easy to specify it all in terms of one parameter because the charge of the electron, e , comes into all of these atomic calculations in more than one way. If this were nuclear physics, such as Coulomb excitation of the nucleus, it would be simpler, because in Coulomb excitation of the nucleus the interaction that is responsible for the excitation is proportional to the charge of the electron, but the forces that hold the nucleus together are dominated by a totally different process involving the nuclear coupling constant. In the atomic case, e comes in from the interaction between the incident charged particle and the atomic electron, but it also comes in from the binding of the electron to the nucleus. That is why all these numerical inequalities are so confusing, often contradictory, and probably should not be paid too much attention. And that is why your question does not have such a straightforward, simple answer. The answer is both yes and no.

To indicate the difficulty in estimating how good an approximation really is, let me show once more the formula for the transition amplitude in perturbation theory:

$$T = (Z_1 e^2 / \hbar \omega) \left[\underset{E1}{r/R^2} + \underset{E2}{r^2/R^3} + \underset{E3}{r^3/R^4} + \dots \right] \\ + (Z_1^2 e^4 / \hbar^2 \omega^2) \left[\underset{E1E1}{r^2/R^4} + \underset{E1E2}{r^3/R^5} + \underset{E2E2}{r^4/R^6} + \dots \right]$$

To get probabilities and cross sections, you have to take the

absolute value squared of this whole expression. You can see that there is going to be a very large number of all sorts of terms when you do that. But at this point, whether you do perturbation theory or whether you treat the electric dipole part rigorously, as you can for the harmonic oscillator, the selection rules come to your aid, at least for the harmonic oscillator, and even for real atoms, many terms vanish. Remember that the expression I have written is symbolic; the matrix elements do not appear explicitly. As you will remember in quantum mechanics you have a matrix element from the initial state to an intermediate state, from the intermediate state to the final state, and then you have to divide by an energy denominator and sum over all intermediate states. In the formula for T the energy denominator is already out in front; it's $\hbar\omega$ for the harmonic oscillator, so it is simple.

When I square this expression, there are a large number of terms:

$$|\langle T \rangle|^2 = [Z_1^2 e^4 / (\hbar\omega)^2] (r^2/R^4) + \dots + [Z_1^3 e^6 / (\hbar\omega)^3] (r^4/R^7) + \dots$$

The leading term is the dipole term. Then there are a lot of terms that don't play any role. For instance, there is a term that is the product of E_1 and E_2 . Obviously that doesn't contribute at all, because both kinds of transition can't lead from the same initial state to the same final state. The dipole transition does involve a change in parity, and the quadrupole transition doesn't, so you can't have both. I have written the leading term involving Z_1^3 . This is the leading correction term in the cross section to the Z_1^3 law as long as distant collisions are the important processes.

We can compare the Z_1^3 contribution term with the dominant term and see whether this makes any sense. Of course, I don't know anything about the numbers that multiply these things, but the ratio of the correction term to the dominant term is

$$\epsilon = (Z_1^2 e^2 / \hbar\omega) (r^2/R^3) .$$

The question is, how large is that? I think that not terribly much can be said for this reason. We know $\hbar\omega$; that's the atomic energy difference, whatever it is, something like three kilovolts in argon. Little r is sure to be the size of the atom, so that we can estimate. But we don't know at all what R is. We know that it must be greater than the size of the atom if the whole calculation is to make sense, but we have no way of knowing what we should use as a sort of typical value for it. I tried to get an idea by saying, well, the only quantities it can possibly depend on are the constants $\hbar$, e , m , the mass of the electron, and the velocity of the incident particle. On the other hand, it can't depend on the mass of the particle, because remember the particle is assumed not to react appreciably to the collision. Then I ask what length can we make out of these quantities? Well, quite a few different ones, since we have a certain flexibility. Let me omit things that are not interesting and focus only on the interesting questions, such as how the ratio ϵ depends on Z_1 , Z_2 , and v . Z_2 is going to come in through $\hbar\omega$, because that tells you what the atomic energy differences are. I write my conclusion down with a certain amount of diffidence:

$$\epsilon \approx Z_1 Z_2^{2-3\gamma} / v^{3-3\gamma} .$$

Here γ is an arbitrary number; it could be anything. If $\hbar$, m , and e are inserted, this will then be dimensionally all right. Of course, one doesn't know what γ is. If you knew what γ is, then you could predict from this how the Z_1^3 contribution term is going to depend on Z_2 and the incident particle's velocity. I don't know what γ is, but I propose that it is somewhere between $1/2$ and $2/3$. That doesn't give a very strong velocity dependence. I think that that prediction agrees with what Brandt¹⁾ says in his paper. But this is rather a stab-in-the-dark; it has not been checked sufficiently by experiment.

I did look at Jim Macdonald's data;²⁾ in fact, I got them from Jim McGuire's paper.³⁾ The data in question are those

shown in Fig. 1 for $z = 6, 7, 8,$ and 9 . (The z in this plot is

Fig. 1

what I have been calling Z_1 in my talks; it is the projectile charge.) These data were taken using fully stripped projectile ions. If there is an important Z_1^3 effect here, then you would expect a straight line on this plot. The data look to be pretty straight. Not completely, it bends up a bit, but I think a straight line would not be such a terrible approximation. It seems to me that these data might be indicative of a Z_1^3 -type contribution.

Question (Lewis Cocke): It seems to me that if you assume that r is much smaller than R , then your result is not applicable to an inner-shell process.

Merzbacher: Why not?

Cocke: Because we know, for example with Jim Macdonald's data, that the excitation takes place right in the region where R is of a size comparable to r .

Merzbacher: How do you know that?

Cocke: We have done the experiment.

Merzbacher: How do you determine where the collision takes place?

Cocke: From the impact parameter dependence.

Merzbacher: How do you know that? Do you look at the scattered projectile at the same time?

Cocke: Yes.

Merzbacher: You haven't shown that to me!

Cocke: That's just been done within the last month.

Merzbacher: You mean you do a coincidence experiment of the x rays with the scattered ions?

Cocke: Yes, and it does show that about half the cross section is due to impact parameters within the K-shell radius, roughly speaking.

Merzbacher: That is really not surprising and doesn't obviate my arguments about the Z_1^3 contribution. The only assumption I was making was that the Z_1^3 part comes primarily from the distant collisions. Obviously the close collisions are terribly important. But it could be that the bulk of the cross section comes from the close collisions, and yet that the distant collisions are principally responsible for the Z_1^3 part. You can't tell about that from the coincidence experiment.

However, I cannot demonstrate that the close collisions are not also contributing. In fact, I am hopeful that they will contribute to the Z_1^3 correction; our calculations in no way say that the close collisions don't give a Z_1^3 term also. I just don't know how to calculate that. It turns out that is very much harder to calculate. I don't think we know yet how the cross sections go for close collisions, what the contributions there will be.

Question (James Macdonald): This is our point; it cannot be going like Z_1^3 coming from hydrogen, because of Brandt's data, with helium. It just doesn't scale right.

Merzbacher: The question is whether it is a Z_1^3 behavior at all or not. Without my using a ruler, it looked to me on Fig. 1 as if that could be true. The data can be represented, I claim, by $\sigma(Z_1) = Z_1^2(A + BZ_1)$. In fact, the ratio is roughly right; that is, it not only has the right slope but also the right intercept. That is the important thing. This requires really two things, and it looks to me that it might fit. The ratio of B to A is far larger than what Brandt et al. would predict, I grant you that. I estimated that it was something like 40%, rather than a few percent.

Macdonald: I think the point that we would make is that if you include with these data the data taken at the same velocities by Brandt's group with alpha particles and with lithium, and then scale them all, then you have the wrong coefficient.

Merzbacher: I believe that.

You are clearly in a region where one has to be very wary of perturbation theory. Your Z_1 's are so large that they no longer satisfy our velocity criterion very well. Still, a particle that goes by an atom is bound to polarize the atom while it hits it. Maybe you see something else beyond that, but at least such an effect has to be there. That's not to say that there aren't other mechanisms. I have no more to say about Z_1^3 effects now.

One of the very nice things about inner-shell excitation is that it takes place near the nucleus, in a very confined region. It offers the possibility to say something about processes happening at the origin, or near the origin. For example, there is a small but fairly mature literature on inner-shell ionization by radioactive substances.⁴⁾ Consider a nucleus, for example polonium. ^{210}Po emits alpha particles. The alpha particle plows right through the atom, and in doing so it has a good chance of knocking out an inner-shell electron, a K-shell electron, an L-shell electron, and an M-shell electron. This is reflected, I suppose, in a small change in the energy of the alpha particle. It is also reflected in the fact that occasionally there is an x ray coming out afterwards; people have seen x rays following alpha emission.

The same thing happens in beta decay, but the process is different because beta decay is very rapid, so there the process is mostly so-called shake-off. That is, the nuclear charge changes, and then it is in the readjustment of the atom that the emission takes place. In the case of alpha particles, which are slow, the alpha particle leaves the atom and goes through the K shell and interacts with it through the Coulomb interaction and then you see the x rays. This is very closely related to experiments with projectiles, but it has one very special feature. It selects out the zero impact parameter processes. The alpha particles come from the nucleus, so they are confined to zero impact parameter. You don't have projectiles that just come glancing by.

It has been delightful in the past couple of years to see the many experiments on x-ray satellites, where one also has the same sort of thing. The satellites of a K x-ray line, for example, require the simultaneous production of a K-shell vacancy and an L-shell vacancy. For this to happen the projectile must have been near enough to the nucleus not only to excite the L shell--that's not near at all; that's a big thing-- but it must have had an impact parameter small enough so as to also excite the K shell. So you are looking at L-shell excitation, but specifically and selectively only at those impact parameters which are also compatible with K-shell excitation. This requirement limits you to a narrow range of admissible impact parameters and is comparable to requiring coincidences with the angular deflection of the incident particle, provided you assume that the deflection is due only to Rutherford scattering by the nucleus.

From experiments⁵⁾ we have therefore been learning about $P_L(b)$, the probability for knocking out an L-shell electron as a function of the impact parameter b , but confined only to those values of b that are equal to or less than the K-shell radius for you can't be very far outside the shell and still be terribly effective in removing an electron from it. Then the question is, how can you compare this with theory. You can study $P_L(b)$ as a function of Z_2 , as a function of velocity, and as a function of Z_1 .

All of this clearly is nicely phrased in impact parameter language, but you don't have to phrase it that way. If you want to be strictly quantum mechanical, you might ask what the Born approximation says. The Born approximation doesn't talk about impact parameters, and in the Born approximation you would have to make a full-fledged second-order perturbation calculation to have an analogous statement. That hasn't been done yet.

It's not so easy; even in a complementarity way you can see why. Small impact parameter, b , in the classical picture, means large momentum transfer, q , in the quantal picture. If you remember when, in Talk #2, I showed how to go from the impact parameter description to the Born approximation, you might remember that there was a Fourier reciprocity linking the form factor, which is a function of q , to the transition amplitude as a function of impact parameter. This circumstance may make it simply not very convenient to think in Born approximation terms. Anyway, there are going to be the usual problems of second-order perturbation theory, if q is used.

Clearly, therefore, what recommends itself to us at this point is the so-called semi-classical approximation, SCA. There has recently been a paper by Hansteen and Mosebekk⁶⁾ in Physical Review Letters on this, and very similar ideas have been developed by various groups. In the semi-classical approximation, for a given impact parameter, you can put the particle on a track and calculate the ionization probability for the K shell and for the L shell. It would be nice to see theoretical curves for the joint K- and L-shell excitation probability as a function of the projectile energy and the target Z_2 . The trouble is that to do these calculations well is quite complicated, and the computer programs are fairly long. Most of these calculations have been done with a straight-line trajectory, without worrying about Coulomb deflection. However, Hansteen has taken the deflection partially into account.

Let us consider the probability for exciting both, say, one K-shell electron and one L-shell electron. This is something

that people at K.S.U. have been concerned with, and I would like to get into a discussion of it. Let me say first how I see this problem in terms of perturbation theory, following clean ground rules. The projectile is incident with velocity v , on a track with an impact parameter b . We are asking, in terms of first-order perturbation theory, which is what Hansteen⁶⁾ does and what we do, what is the probability for exciting the various transitions? Then we add them all up. That's what I understand the problem to be in first-order perturbation theory. For instance, in the K-shell case, just as an example, $P_K(b)$ is the probability that one K-shell electron will have been ionized in a collision with impact parameter b and that nothing else has happened. The theory does not contemplate that any other electron has also been excited. I know that I am using the notation $P_K(b)$ for something that I don't think some of you mean by P_K . The cross section for K-shell excitation, alone, is then

$$\sigma_K = 4\pi \int_0^{\infty} P_K(b) b db .$$

The factor 2 in 4π comes about because there are two electrons in the K shell. I think we all agree on this. This is the total ionization cross section for the K shell.

Now the question is, how about $\sigma_{1K,1L}$? What is the cross section for simultaneously emitting one K electron and one L electron? We will refrain from making fine, detailed distinctions between 2s and 2p electrons, although they are actually important and can be handled.

How should we do the calculation of ejecting both electrons in perturbation theory? This requires second-order perturbation theory, because the operator that describes the interaction is $e^2/|\vec{R}-\vec{r}_1|$ ($\vec{r}_1$ locates the 1th electron and $\vec{R}$ the projectile.), which is a one-particle operator. Let's make another ground rule: These are atoms that are represented by product wave functions. It is assumed that there are no correlations; the electrons do not hang together. They do really, but we'll neglect that and describe

the atom by uncorrelated product wave functions. For such states, in first-order perturbation theory, a one-particle operator cannot simultaneously excite two electrons, so you have to go to second-order perturbation theory. You have to go through an intermediate state, and then in the second step the second electron is excited. And you have to sum over all the intermediate states. But when you do that, the result is trivial. Because of closure, everything collapses very neatly, and you find, in fact, that in second-order perturbation theory you simply get the product of the two probabilities, $P_K(b) P_L(b)$, as the joint probability of exciting one K-shell and one L-shell electron. But note that this is true only in lowest-order perturbation theory.

Question: Won't there be residual electron-electron interaction?

Answer: Yes, but I'm declaring them off limits.

Question: That is the probability that a K-shell electron has been ejected and an L-shell electron has been ejected, and what else?

Answer: Nothing else; nothing else has happened.

Question: Don't you need to include the probability that none of those other things is happening?

Answer: That's the point I am trying to make; the way we have defined P_K and P_L , you don't need that. In fact, you shouldn't have that; there should not be any factors like $1 - P_K$ and $1 - P_L$. I well understand that people have written down formulas that look different. Just to comfort those who are wondering who's right and who's wrong, it turns out that it doesn't make much difference as long as the probabilities are small compared to one. If the probabilities are comparable to one, then it makes a great deal of difference. However, if the probabilities are comparable to one, perturbation theory is not applicable at all. I think that's where we are going to come together again in spite of our differences.

It is the nature of perturbation theory that you make a

transition from one state to another state. The thing that you calculate is a transition amplitude, and when you square it, you obtain the probability for making a transition from say the ground state to an excited state. Suppose we have eight electrons in the L shell and two in the K shell. Then we take one electron and excite it.

For uncorrelated atomic states, perturbation theory gives the probability of exciting the electron and nothing else happening. That is the important thing. Thus, P_L is the probability that an electron is excited and nothing else happens. The product $P_K P_L$ is the probability that a K-shell electron and an L-shell electron are yanked up, and nothing else happens.

Question: It's a question then of the theoretical definition of P_L ?

Answer: Yes, that's exactly right. I would argue that it's simply this product, and then, of course, the whole thing has to be integrated from 0 to infinity;

$$\sigma_{LK,LL} = 32\pi \int_0^\infty P_K(b) b \, db \quad .$$

We have to multiply by $16 \times 2\pi$ to take the number of available K-shell and L-shell electrons into account.

Question: The whole question about which is right hinges then on the definition of P_L ?

Answer: I am saying that with time-dependent perturbation theory it would be illogical to use anything else. Now,

Hansteen⁶⁾ did use $1 - P_L$ in his theoretical expression. Strangely, he put it in for the L shell only and not for the K shell. He writes something like this: $P_K P_L (1 - P_L)^7$. By that he means that one L-shell electron is removed under the condition that the other seven are not removed, but he does not include a factor of $(1 - P_K)$. Presumably, he didn't do that because he knew that P_K is tiny. But then he argues in the next paragraph that P_L is also tiny, and forgets about that too, in fact, as I recall. It is true that P_L is always much larger than P_K . There is no question about that, but P_L need not be much less than unity.

Question: I would like to see you now do the triple ionization case of one K-shell and two L-shell excitations because that's a little more interesting. Do you put just the number of particles, or do you put some coefficient?

Answer: In triple ionization, we consider $\sigma_{1K,2L}$. We shall assume that the atom is being good and that the wave functions are "frozen" and don't change. I don't regard that as a side issue, actually, but we are assuming it.

Consistency demands that under these simplifying assumptions, the triple ionization cross section is written as:

$$\sigma_{1K,2L} = 112\pi \int P_K(b) [P_L(b)]^2 b db .$$

The coefficient in front is $2 \times (\frac{8 \times 7}{2})$ to account for the number of available electrons. If the identity of the electrons is respected and determinantal wave functions are used, the same result is obtained, owing to the normalization of the state.

Question: I think maybe you're starting to get into real trouble, from the experimental point of view, because those P's are not small.

Answer: I quite agree. If they are small, then this would be the way to do it. But suppose that they get bigger. Then I have to say that perturbation theory no longer can work. This approach certainly does break down then.

Question: Before you move on, could you tell me again

how you define that P_L ?

Answer: P_L is just what quantum mechanics says it is. It is the square of the transition amplitude, which says it's the probability for exciting or ionizing an L-shell electron, with nothing else changing. The other approach⁶⁾ is to say that P_L is the probability of removing an L-shell electron, not requiring that nothing else must happen, but in fact saying "no matter what else happens." When you throw dice, you say the probability for getting a three on a particular die is one-sixth, no matter how the other dice fall. Similarly, you may say that $P_L(b)$ is the probability that the particle has come with impact parameter b , has ejected an L-shell electron, and has done anything else that it is likely to do. Then, at that point, you declare a statistical independence hypothesis. You say, well, the probability for knocking out one and only one electron must be $8 P_L(b) (1-P_L(b))^7$. I have absolutely no issue with that, but I wanted to point out that there is this difference of interpreting P_L . In terms of the experiment, there is no question, that's fine, and it makes good sense to try to represent the data by a binomial probability distribution. But when you now ask how to theoretically predict the values of the $P_L(b)$'s so obtained, it is important to realize that these are not the same as the probabilities calculated from simple perturbation theory, especially when these probabilities begin to be of the order of 50% or more, because then it will in fact make a difference, and I think one ought to be careful about that.

Question: Could you get a relationship between these viewpoints by summing the first kind of expression?

Answer: Let me say something about that. Suppose that these P_L were not small compared to unity. Suppose that somebody has been able to calculate the P_L 's without perturbation theory. These are the probabilities for exciting an L-electron without anything else happening. They are not the P_L 's that appear in the binomial distribution. I won't put b in explicitly as an argument,

because this is all at a fixed impact parameter, b . We have P_K and P_L , and then there is P_{KL} , which is the probability that one K-shell electron has been removed and one L-shell electron has been removed, and nothing else has happened, which is not subsumed in P_K and P_L . Now let me write down an identity connecting these probabilities. The best way of connecting them is by adding the probabilities up to unity. What can happen? Either the K-shell electron is excited, or the L-shell electron is excited, or both of them are excited. The latter is not included here in the first two possibilities, whereas in the other definition of the probabilities, leading to the binomial distribution, that is already included. That's where the big difference lies. The fourth possibility is that nothing has happened, neither the K shell nor the L shell has been excited. Thus, we have

$$2P_K + 8P_L + 16P_{KL} + (1 - 2P_K - 16P_{KL})(1 - 8P_L - 16P_{KL}) = 1.$$

For simplicity, I am neglecting excitation of higher shells or any other multiple excitations. Suitable corrections can be made. But the L shell can be excited either alone or together with the K shell, and the K shell can be excited either alone or together with the L shell. The sum for all possibilities has to add up to 1 by unitarity. This includes all possibilities: the K shell excited, the L shell excited, both of them excited, neither of them excited. Now you just work this out and obtain a relationship that gives P_{KL} as a quadratic equation in terms of P_K and P_L :

$$16P_{KL}^2 + (2P_K + 8P_L - 1)P_{KL} + P_KP_L = 0.$$

That you can solve. Now, let us make two assumptions that are usually valid. First, that P_K is much less than P_L , and second, that P_{KL} is much less than unity: $P_K \ll P_L$, $P_{KL} \ll 1$. P_L can be large, but P_{KL} , the double excitation probability, is small compared to unity. With that assumption you can forget about P_K in the parenthesis. Furthermore, you can neglect P_{KL}^2 compared to

the other terms. You then have that $P_{KL} \approx P_K P_L / (1 - 8P_L)$. That's as a function of b . Therefore, I would argue that one should have

$$\sigma_{1K,1L} = 32\pi \int_0^\infty [P_K(b) P_L(b) / (1 - 8P_L(b))] b \, db \approx \frac{8P_L(0)}{1-8P_L(0)} \sigma_K$$

which is a result different from that obtained by other authors.⁷⁾

Question: Let me argue the following assumption, which may or may not be true. The assumption that I would like to make is that $P_L(b)$ is reasonably flat over the region where P_K is non-zero. If that's true... (interrupted)

Answer: That's quite true. Since all our integrands contain P_K , the only thing that counts in the integral is that part of the impact parameter domain where P_K is other than zero, and over that domain P_L is pretty flat. I think in all theories that is quite true. Here is a sketch of the situation:

So it really is not $P_L(b)$ one measures in multiple excitation of K and L shells; it is really effectively only $P_L(0)$. That is why it relates to the alpha decay measurements, which are also concerned by requiring that the x ray follows the alpha decay with zero impact parameter.

Let me finally report on some work that we have been doing at Chapel Hill. J. T. Wu has gone back again to the old oscillator model of the atom.⁸⁾ That works so nicely that one is

attached to it. He has taken the three-dimensional isotropic harmonic oscillator, and he has calculated some excitation probabilities. You can place the electron initially in the ground state, or you can put it in an excited level, say in the L shell, if you wish. The calculation has that flexibility; it could be in any initial state. Wu has done the calculation in perturbation theory, but he has avoided a multipole expansion, taking the correct, the full Coulomb force. To that extent it is a good calculation, because it doesn't make any approximation about the interaction. Remember those modified Bessel functions K_0 that we had in Talk #2; the calculation is based on their matrix elements. Wu has calculated both $P_n(0)$, n labeling the initial state and the 0 meaning zero impact parameter, and the integral of $P_n(b) b db$ over all impact parameters; $\sigma = 2\pi \int_0^\infty P_n(b) b db$. As soon as we have numerical results for these two quantities, we can compare the model with experiment. We hope to learn from this model how much contribution there is from the close collisions and whether this assumption that only the distant collisions contribute is right.

Question (Richard): You talked about the case of alpha decay causing multiple ionization. In the case of fission, we don't see any atomic x rays at all.

Merzbacher: I asked somebody about that, and I understood that was because internal conversion masks the effect. Fission seems certainly similar to the quasi-molecular collisions of two atoms. Frans Saris must have talked to you about molecular x rays and so forth. And fission is just the opposite; the two ions are coming apart, so you would think you would see atomic x rays. In fact, there has been a calculation of that by Noble.⁹⁾

Richard: I don't understand his discussion at all.

Merzbacher: I don't either.

Richard: In his paper he claims to agree with the experiment, but we don't claim to see any atomic x rays at all.

Merzbacher: Whether atomic x rays are detected in the aftermath of fission or not, connections between atomic physics and

nuclear physics are definitely worth thinking about. For example, S. M. Shafroth at TUNL shoots protons at lead, and he sees not only the K-shell x ray from lead, but also the K-shell x ray from bismuth, the next element up. You might think that there is a (p,n) reaction that produces bismuth, and in the process, as the proton goes into the nucleus with zero impact parameter, it has a probability of exciting a K-shell electron. It probably isn't that, it turns out; again it is probably internal conversion from the bismuth, and not really Coulomb excitation.

There are other possibilities, real nuclear possibilities. You might expect to see the effects of inner-shell excitation on nuclear reaction cross sections. Let me give you an example. Suppose you had a very narrow nuclear resonance, say in (p,p) scattering or in a (p,n) reaction. Or let's take a (p, γ) reaction with a very narrow Breit-Wigner resonance. Every once in a while a proton that approaches the nucleus will enroute ionize the K shell of that atom and therefore arrive at the position of the nucleus not with the energy you thought it had, but it will have given up some of its energy to the K-shell electron. It will therefore no longer resonate. Conversely, if you set your energy a little higher, you catch some protons that by losing energy to the K-shell electrons then arrive just at the right energy to resonate. So you would expect a skewing of the shape of the resonance. You might think this effect will be very small. We have only estimated this, but it turns out that if the resonance is the order of a few electron volts in width, if the resolution of your detection equipment is say of the order of 50 to 100 electron volts, then you might begin to see something of that sort. So, there is one example of a remarriage of atomic physics and nuclear physics, and I'll end on that.

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Professor Donald Robson

Professor Robson of Florida State University and a recent recipient of the Tom W. Bonner Prize in Nuclear Physics is well known for his understanding and interpretation of isobaric analog resonances, and for his contribution to phenomenological descriptions of heavy-ion nuclear scattering.

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HEAVY-ION REACTIONS - ARE THEY WORTHWHILE?

I am certainly impressed by the enormous amount of friendliness I have had so far. Not one person has said to me "you are wrong," which is rather nice. There are two conclusions you can get from that; either I haven't said anything worthwhile, or everything I have said is right.

I would like to start with what I label slide 0 (Fig. 1)

COLLOQUIUM LAWS

1. Be "Entertaining"

E = Many Clever Caricatures

2. Be "Finite" in Time

F = Much appause

3. Be "well-balanced" in content

Prejudice $\otimes$ Clarity $\otimes$ Truth = const.

Fig. 1

because every projection operator always shows it first whether you want him to or not. This is devised so that you can get focused without losing whatever punch you may have for your real first slide.

Talking here is a little difficult for a theorist; it is much better to show a lot of data and impress you with what is going on in the field. What I want to do is to start talking about

one of the myths of heavy-ion reactions, the production of so-called super-heavy nuclei.¹⁾ If you go to meetings or talk to the NSF or the AEC, almost everyone will ask you what you think the probability that you can make super-heavy nuclei is if you were given a machine of this or that type. I am rather annoyed at being asked that question, because I don't think that heavy-ion physics should depend upon whether super-heavy nuclei exist or not. So, first of all I would like to talk about super-heavy nuclei a little and indicate what they are so we can get them out of the way and talk about what people are really doing in the field rather than what they are dreaming about.

The first real slide (Fig. 2) is a diagram that was

Fig. 2

published by Seaborg and Bloom²⁾ showing the so-called islands of stability of nuclei. The diagram shows regions of known or predicted nuclear stabilities as land masses in a sea of instability representing various types of decay. The grid lines show the well-known magic numbers of protons or neutrons that give rise to exceptional stability. The high mountain at 82 protons and 126 neutrons represents doubly magic lead, a relatively dull subject. The flat island at 114 protons and 184 neutrons (or 196) is the new doubly magic region that people have predicted theoretically. The

new magic number is 114. I am not going to derive where the number 114 comes from; it is just based upon the shell model. Corresponding to that is a neutron magic number of about 184 and another one of about 196. In that region it is expected there are two islands or one island of stability. Some people are theoretically predicting this and being rather confident about it, yet when you ask them what lifetime such a system would have, they can tell you no more than within 6 orders of magnitude. That is not terribly convincing if you are trying to build a new machine that might exceed 25 million dollars. After all, maybe this super-heavy island is in fact submerged. If that was the reason you built your machine, you may have a physicist who ends up getting drowned with the island.

The relation of heavy-ion reactions to the production of super-heavy nuclei is fairly obvious if we define a heavy ion to have a mass greater than an alpha-particle and realize the heaviest nucleus currently available as a target is about mass 250. We have to provide enough mass to hop over the gap of instability to the super-heavy island. Consequently, it seems the only way in which we can get to that island is to use something like heavy ions. There are some other suggestions that I won't go into here.

One reaction has already been tried at Berkeley; that is to use a curium target, which has a mass of 248 and a proton number of 96, and then add to this a rare gas and ask what you can make. One of the things you hope to make is (284, 114). We don't know what to call it yet because we have not found it.

You get 4 neutrons boiled off. This is a failure so far. You never measure a zero cross section; you only measure no cross section, which means you don't get any counts. The argument was, well, that is not surprising because the number of neutrons is 170, and that is not magic. So why are you looking for it in the first place? I don't know; the Berkeley people tried it because it was

something they could do. Anyway, it didn't work. To my knowledge, that is the only attempt in this country to look for super-heavy nuclei. There have been attempts overseas that have also failed. The present experimental evidence for super-heavy element formation is regarded as zero.

What some people would like to do, so they claim, is to take a heavy nucleus like uranium and add to it another one and see what you can get out of that.

You have enough mass to make the element ${}_{114}^{298}\text{?}$. The number of neutrons is now right because it is 184, and that is where it is predicted to be stable. You get the super-heavy nucleus plus something else that turns out to be reasonably stable, that is ${}_{70}^{174}\text{Yb}$ (ytterbium) (One thing about giving these talks; you learn to pronounce the names of nuclei that you have never heard of before.), again plus 4 neutrons. The problem is that you need about 2 GeV with uranium, and that is currently not available. So the argument is we need a facility if we are going to discover things like that. The reason the energy needed is so large is that for uranium on uranium you have two huge charged liquid drops, and when you work out the Coulomb barrier you actually require about 1.5 GeV or so, and people thought well, we ought to allow some slop in order to do some other experiments when we are finished.

Fortunately, I managed to persuade my wife to draw some slides in which some of this thinking is illustrated. The next slide (Fig. 3) is a cartoon labeled Super-Heavy Politics. At the top is the sort of thing you sometimes see at a meeting involving people who really believe we should be thinking about this in detail; this is what I call the optimistic approach. The reference to superconducting is because if we do construct a facility, the most likely sort of machine that will be developed is a superconducting Linac. The pessimistic point is related to the sort of

Fig. 3

scheme that people have been proposing that most likely would work. I tend toward a pessimistic view of using super-heavy nuclei as an argument for building better heavy-ion facilities. My point about being pessimistic is that even if you discovered one super-heavy nucleus, you might question afterwards if it was worthwhile. After all, when you've got it, what are you going to do with it?

The next slide (Fig. 4) indicates one of the problems we have in talking about heavy-ion facilities. (I notice that where the man is pointing isn't too far from where I am standing.) There are, in fact, two major national labs, Argonne and Oak Ridge, that seem at present to be regarded by the NSF (and perhaps by the AEC) as the most reasonable places to put such a facility. That is the propaganda that goes around, and I think there is, in fact, a good chance that a facility will be built in the next five years at one of these two places. We are expecting, of course, that it will be at Oak Ridge. Seeing where Tallahassee is, you can figure out why

we think that.

HI FACILITY LOCATION

Fig. 4

At meetings, when I talk about this as a person who might be interested in the theory of heavy ions, I am sometimes surprised by the attacks that are received from what I call normal nuclear physicists who tend to scatter protons, deuterons, tritium and so on and do what they say is useful spectroscopy. I agree that what they do is useful (I didn't use the word worthwhile.), and I really believe they have a number of points to make about how heavy ion physics affects their welfare. The sources of the financial pot for nuclear physics have become smaller and smaller relative to the number of people we keep pouring into the field. We have to make decisions now as to which way the priorities will go. The report that came from the Wenner Panel, echoed then in the Bromley Report, said that a heavy-ion facility should be built at all costs, or at any cost, or some such words; the light-ion physicists would have to suffer in order to let a heavy-ion facility be built. The outcry against was enormous, as you might guess, because there

are a large number of nuclear physicists who are still happily and usefully scattering protons and deuterons, and so forth.

There is a battle now, which I will call HI versus LI for heavy ions versus light ions. This battle is one that is based entirely on the dollar, but that is not the way the argument is put forward. This is the correct argument, because one field will suffer if the other field gets funded; there is no doubt about it unless something radically changes. But the argument used by the LI people is that heavy ions have not produced anything particularly useful so far, and they have, so why should we pursue a new field when we already have a field that is doing well? Or at least if you do want to pursue this field, don't put so much money into it. Let it prove itself first before you fund it.

I would like to make two comments: (1) Heavy-ion physics is an infant in comparison to light-ion physics in the technology that has been developed. (2) The comparisons between heavy ions and light ions are performed on light-ion terms. A light-ion spectroscopist says, "Can you give me information from heavy-ion experiments better than I can get it with light ions?" I think the answer to that in many cases is no. Heavy-ion methodology in fact does not generally beat out light-ion methodology. This is not too surprising because light ions are also a lot simpler in many cases than heavy ions. On the other hand, the sort of information you can get from heavy ions may not even exist for light ions.

I think it is useful to draw a parallel with the 1930's. In the 1930's there were some very excellent physicists who spent five years investigating proton-proton and neutron-proton scattering; so long, in fact, that I forget their names. This was regarded as the fundamental thing to do in nuclear physics; measure the nucleon-nucleon force as accurately as you possibly can. Also in the 1930's it was suggested that we should look at things like protons on ^{12}C or protons on nitrogen. The technology and accuracy of information involved in determining the basic nucleon-nucleon interaction from p-p and n-p scattering certainly exceeded that for proton scattering from nuclei like ^{12}C in the 1930's. But we still

don't know very much more about the basic nucleon-nucleon interaction than nuclear physicists did 30 years ago, while light-ion scattering from nuclei has given us tremendous information about the properties of nuclei. Where I draw the parallel is that in the 1970's people are telling us that protons on ^{12}C is more fundamental and provides more useful information than trying to do ^{12}C on ^{12}C , which I have decided is the experimentalist's dream. My point is simply that the information we will get from heavy ions on heavy ions is of a different type than that obtained via light-ion reactions, just as the information one got from $p-^{12}\text{C}$ was about the properties of nuclei as opposed to the fundamental nucleon-nucleon force. I don't think even a light-ion physicist would argue that such a parallel is unreasonable.

In the remainder of the talk today I want to give an overview of the kinds of things that occur in experiments with heavy ions on heavy ions, complex nuclei on complex nuclei, that contain information that you cannot get with other techniques. In order to classify these, I have another one of these nice slides (Fig. 5). There are three basic types of HI reactions I wish to

Fig. 5

discuss. The first is where you imagine all that happens is that the two ions run past each other and shake hands; all that will happen is that you might get a warm glow and a little bit of energy as you pass by. This is referred to as elastic or inelastic scattering. In other words, the only thing that happens is that his eyebrows may go up, or something internally may change in the structure of one of these two, but they will not change their basic character. The second type of process, which is actually the one in which spectroscopists are really interested, is where you break off a small part of one of the ions and form a different nucleus, but not terribly different in the sense that normally the mass transfer is relatively small. (Although this leg is getting awfully big recently in some of the work that has been done, I am sure that when Dr. Fortune comes by, he will be talking about very large legs taken off.) We call such a process a transfer reaction. The third process is spallation, or the production of many fragments.

In my remaining two talks I intend to talk about the first process, which is of the type

A' differs from A or B' from B only in that it has a different energy, or it can have the same energy, in which case it would be purely elastic scattering. The second type of process can be represented as

There are only two final products, but they are different from the initial reactants. This is nothing more than chemistry, and chemical reactions are taught in the same way. The third case, spallation, can be represented as

depending on how many final products you have. I will talk about the three cases in the following order: Case one will be my second talk, and case two will be my third talk. This means I need to cover case three today. So we start with spallation in order to insure that I talk about it.

Due to time limitations, I will pick out only two or three examples of each type of heavy-ion reaction. The important example I chose of spallation, which will clarify my point that you can get new information, is the case of a heavy-ion projectile where several neutrons are pulled off,

You start with nucleus A and finish with nucleus B. What is important here is that you can form some "exotic" new nuclei, much like the super-heavy element arguments. If we refer to Fig. 2, we have a long island of stability and (perhaps) a little island sticking off the end. It may be just as interesting to go perpendicularly off the long island as to go off the end. After all, it may turn out that the super-heavy island isn't there, and it may turn out that there is something in another direction where you can already do the experiment, so why not start looking? In fact, there is one successful experiment on exotic nuclei that I will mention. That is

This was achieved by Ward and collaborators³⁾ and is listed in the Oak Ridge proposal for the new machine because they have some additional ideas about forming small, complicated isotopes. For example, they suggest that they could do

Because one could think up various combinations of this type, they

claim it should be possible to look at a whole set of nuclei we have never even heard of, and perhaps to map out the set of nuclei and their properties as we improve the technology of investigating gamma rays; we could really begin to do spectroscopy on nuclei we don't even know about. Heavy ions could allow us to really spread our wings a little around the island of stability, to find out something about what is going on on either side.

The second example of spallation reactions that I want to discuss relates to the fact that heavy ions carry in large angular momentum even at energies like 50 MeV. If we consider a reaction $A(HI, Xn)B^*$, the neutrons produced have such a small mass that they are ineffective in carrying off angular momentum. Consequently, conservation of angular momentum leads to a large amount of angular momentum being deposited in the residual nucleus B. In deformed nuclei such as Yb it has been known for many years that the low-lying levels follow a rotational pattern. The nuclei look something like the following if you are a classical physicist:

You simply imagine that there is an internal axis Z' and that this can rotate with a frequency ω to produce angular momentum J . The idea is that if you have a state of the nucleus like this, then you can form a rotational band. We know the energy; it is supposed to go something like

$$E(J) = \frac{\hbar^2}{2} J(J + 1) \quad .$$

The dominant deformation is believed to be an axially symmetric

quadrupole shape, and in even-even nuclei a cascade of electric quadrupole γ -radiations is observed to connect successive members of the rotational band:

That is what one was supposed to see according to the simple-minded collective model of this situation. As J went up to 6^+ , people found a need to make this $E(J) = AJ(J+1) + BJ^2(J+1)^2$, because if you start to make something that has any possibility of stretching rotate rapidly, then A will change slightly. Consequently, there ought to be higher order terms. Indeed, this formula fit very well.

The importance of the experiments with heavy ions is that one can reach values like $J = 22$.⁴⁾ This is nice, because it means you can test rotating the nucleus very fast to see what happens on the inside of it. Indeed, rather remarkable things happen on the inside. The reason why this sort of reaction is particularly nice is because if you have a large heavy ion like oxygen, which is about 16 times bigger than a nucleon, and you bombard another nucleus with it at an impact parameter b , then you have a large angular momentum $L = Mvb$. For a constant energy, the angular momentum is proportional to the square root of the mass, but it does rise rather rapidly with the mass of the incoming particle. Heavy ions bring in large amounts of angular momentum for a given fixed energy compared to say a neutron with the same energy moving with the same sort of impact parameter. When you send a heavy ion in and pour out only 3 or 4 neutrons, you have a change of mass of the target nucleus of about 12 units. It is not possible for the

neutrons to carry off the large angular momentum very easily, so most of the angular momentum is put into the internal state of B. That is why we can form a very high spin state in the residual nucleus. Angular momentum has to be conserved, and the only place you can put it is into B.

The next slide (Fig. 6) shows $2\mathcal{I}/\hbar^2$ plotted against

Fig. 6

$(\hbar\omega)^2$. The data are taken from Theibberger et al.⁴⁾ It is a little hard when you read this article to figure out where those things come from. What they do is to use the law $E(J) = \frac{\hbar^2}{2\mathcal{I}} J(J+1)$ and let $\mathcal{I}$ vary as a function of J . They calculate the moment of inertia by taking the energy separation between two successive members of the rotational band and calculating what the effective moment of inertia is for that energy separation based upon a $J(J+1)$ rule; that is

$$E(J) - E(J-2) = \frac{\hbar^2}{2\mathcal{I}(J)} [J(J+1) - (J-2)(J-1)] .$$

The frequency is then obtained classically by taking the gradient of the energy with respect to the angular momentum:

$$\omega = \Delta E / (\Delta I \hbar)$$

$$\text{so that } (\hbar\omega)^2 \approx \left[\frac{E(J) - E(J-2)}{\sqrt{J(J+1)} - \sqrt{(J-2)(J-1)}} \right]^2 .$$

The points line up in a very nice straight line that corresponds to the picture one had for the 0, 2, 4, 6 type spectrum. All of a sudden, near $J = 14$, there is a surprising change in the character of the moment of inertia. I am not going to claim that people know the reason for that, but the fact one could investigate the phenomenon in this way is very interesting. It allows you to talk about the effect of the Coriolis force on the internal structure of the nucleus by simply spinning it more and more rapidly. They observed the cascade gammas all the way from $J = 22$, so they really had a lot of fun, I am sure, looking at the connecting gammas in the sequence.

The argument put forth to explain this, though I am not sure it really explains it, is based upon a phase transition around $J = 14$.⁵⁾ The argument is that below $J = 14$ the short range pairing interaction between nucleons is stronger than the Coriolis interaction, which tends to break up the pairs. The paired phase is loosely referred to as the superconducting phase because BCS theory is used to describe the lowest configurations. For $J > J_{cr}$ it is surmised that the pairs are broken up and a normal phase occurs. As yet there is insufficient data to draw any definite conclusion about phase changes, but clearly the rotational band does not follow any simple pattern based on rotational models of conventional type. The moment of inertia near $J = 22$ is still somewhat less than the rigid body value calculated for the spheroid, so nuclei do not behave as classical rigid bodies even at $J = 22$.

I would now like to move on to the second process, transfer reactions. There are only two areas I want to talk about in detail, (1) Sub-Coulomb Tunneling and (2) Large Mass Transfers. In the first place, when you use heavy ions you have a large Coulomb barrier. If you are willing to use them with slow velocities, it clearly becomes a Coulomb problem because all the

long-range force in the problem is Coulomb repulsion; they never get close enough to know that they are nuclei. You can carry this game far enough until they are just close enough to start talking to each other. The next slide (Fig. 7) is the original approach

Fig. 7

to this that was due to Breit and co-workers.⁶⁾ In this you imagine the transfer of a neutron, which has no charge, under the Coulomb barrier via a simple tunneling mechanism. The nuclei never get close enough to really see a nuclear force, but they have tails which overlap just enough for the neutron to tunnel through. In this way, Breit could predict fairly easily this sort of cross section behavior that rises very rapidly as you go up the Coulomb barrier. Indeed, the theory is quite successful in predicting the main features at these low energies. It didn't take long before it became obvious that if you could have one neutron transfer, you should also have two neutron transfer. Then, people realized that if you think about superconductors and you want to take a pair across, what about the same situation you have in solid state physics; one heavy ion here and another heavy ion there and at the

distance of closest approach one stops and says, "I have a junction."

My next slide (Fig. 8) illustrates the attempt that

JOSEPHSON STATIC JUNCTION

NUCLEAR JOSEPHSON JUNCTION

Fig. 8

nuclear physicists have made to talk about a Josephson effect in nuclear physics in the case where a pair of nucleons are transferred.⁷⁾ The ideology is well known in solid state physics. One usually has a static junction in which one takes a superconductor and another superconductor and puts an oxide layer between and applies a small voltage. One finds that the current transferred across the junction is AC and DC. Depending on the particular things you do, in this situation you can get one or the other of these or both. The AC current was apparently unexpected until Josephson put forth his ideas. The argument is that nuclei behave like superconductors in the ground state if they are pairing type nuclei such as Sn, which is believed to be described by BCS theory. So, what you do is to take two Sn isotopes and hook them together by a heavy-ion reaction, which possesses the Coulomb barrier as

effectively something keeping them apart. For a pair to go across they have to tunnel through that barrier in the same way as the pairs tunnel through the oxide layer in the Josephson junction. There is one important difference that is rather obvious; the junction is time dependent here because the two heavy ions do not sit there for you. They are going on their Rutherford orbits, and whether or not the pair makes it depends upon how long they stay in close proximity. Well, we know tunneling transfer does take place in heavy ions. The question then is, if a pair does make a transfer, which means the time interval involved must be something comparable to the collision time, how will this affect the superconductivity and the possible AC current.

This has been analyzed by Hara and Dietrich⁸⁾ in Germany, and their conclusion is that they don't know. They had two possibilities. You would certainly have a DC current if the ions went past pretty fast and had a separation distance that altered rapidly with time (collision time $\tau \ll$ pair transfer time), because even if you had an AC current you would average it away. If the time of collision is very long compared to the pair transfer time, which roughly depends on the difference in the Fermi energies of these two, this would be a static junction. Their claim is you would see an alternating nucleon pair transfer current. It seems most likely that the first situation is to be expected since collision times are $\tau \lesssim 10^{-22}$ sec. whereas pair transfer times are

$$t_{\text{pair}} \sim \frac{\hbar}{\Delta E_{F1}} \frac{6}{\Delta E_{F1} (\text{MeV})} \times 10^{22} \text{ sec}$$

which yields

$$\frac{\tau}{t_{\text{pair}}} \sim \frac{\Delta E_{F1} (\text{MeV})}{6} \lesssim 1 \text{ unless } \Delta E_{F1} \text{ is unreasonably large.}$$

If you allow too many MeV of excitation, the two neutrons are not strongly enough bound to remain with the residual ion. They will happily go off by themselves. So it seems likely that what will happen is you will see a DC current, which may have some of

Josephson's characteristics because there is a large probability of pairing. The DC current might be quite large, but I think that is all you would get out of it. No experiments have been performed yet.

The other type of transfer reaction I want to talk about is large mass transfers ($\Delta m > 3$). This is also quite fascinating. You cannot transfer more particles than you start with. If you start with three particles, you cannot transfer four. Therefore, if you want to transfer four nucleons from one nucleus to another, the only way to do it is to begin with a particle that has more than four nucleons. Lithium is the first of these that was looked at. Send a lithium ion in, and you see a deuteron coming off:

This corresponds to an alpha particle going across. People have thought up a good name for such four particle structure, quartets. (I am sure there is a string attached to that somewhere.) This is really nothing more than the Pauli principle, and I don't think you really need a new name for that. The point is the following. Most nuclear physicists now believe in independent particle motion, at least as a first starting point. You start with independent particle motion and then add the residual interactions to it. Suppose you define an orbit for a nucleon with A_ω as some quantum number that defines the projection of the angular momentum of that nucleon on a certain symmetry axis. Then it is always possible to put just four nucleons into that orbit, because the Pauli principle allows two neutrons with spin up and spin down, and two protons, with spin up and spin down. That makes four nucleons, and that makes an α -like structure. No matter what nuclear model you use, you have that possibility built in by the Pauli principle.

The question is, in two reactions like

do you really select final states of the type $(A + \alpha)$, corresponding to adding four nucleons in one orbit in the final nucleus? It has been argued from Saclay⁹⁾ recently that this is indeed true. This four nucleon particle is what people call quartets. They don't care whether the nucleons move relative to each other in s-state motions, which is what an alpha particle does. An alpha particle acts in the shell model as $1s^4$. They don't tell you about what center that takes place. It is a little hard to figure out, but the idea is that you have four nucleons and if you look at the relative motion of any two, there will be zero relative angular momentum. The quartets are regarded as now having the possibility of not having just relative s-state motion, but something else, their only character being zero total spin and isospin ($S=0$, $T=0$), which is more a group theoretical argument than it is a dynamical picture of what is going on. I will talk about that later and try to show that Saclay is the most contradictory lab I have ever seen. One time they want it to be an alpha particle; the next minute they want it to be a quartet.

The next slide (Fig. 9) shows that the states populated in these four-nucleon transfer reactions are relatively few compared to the number of levels known in the residual nucleus via other reactions. The heavy ion reaction is indeed selective. Unfortunately, the assumption that the states are therefore quartet states in B^* is fallacious, although the slide was used as positive evidence for the existence of quartet states in Ni nuclei, formed by $^{54}\text{Fe}(^{16}\text{O}, ^{12}\text{C})^{58}\text{Ni}$, in which 4 nucleons are transferred. Indeed, if you look at the energies of the outgoing ^{12}C , which are shown by these peaks and correspond to discrete states in ^{58}Ni , they seem to represent only a few levels that are selected in this reaction. The same experiment was also done with $(^7\text{Li}, t)$ with the same initial and final large nuclei, Fe and Ni. This reaction is also selective, and some of the states of the residual nucleus are the same for the two reactions, at least within the resolution of the experiment. The Saclay people said "See, there it is," and they are right where they were predicted to be according to the

Fig. 9

calculations of the Saclay theorists. The excitation energy is about 6 MeV, and the prediction of where these states would be is at about 6 MeV excitation in the final nucleus. The argument is, since you see states there they are quartet states. Today I am not going to talk about why they are not; I am simply going to tell you that they are not. As far as we can see, there is no reason to believe that they are quartet states. I will be talking more on this subject in the last lecture.

Nevertheless, the reaction is selective in that it only populates a few states. I think that feature is important. Here we see something that is extremely selective; if you look at the level density deduced from other mechanisms, you find that there are probably a hundred levels here, but you only select half a dozen with any strength. This is exciting. It suggests that certain features of the problem must be simple. Simple results in any experiment are always marvelous to obtain, particularly in a complicated field like nuclear physics. I'll talk about this in

the third lecture, so I don't want to get too involved in it here.

My final area in surveying heavy-ion reactions is elastic and inelastic scattering. Much of the early interest in heavy-ion scattering arose because of the idea of nuclear molecules. The idea of the nuclear molecule is that two heavy ions come together like this,

and you can think of the system as essentially just rotating around in that structure and forget about everything else. That is, it does this for a while and then it decays. It comes together, goes around a few times, and goes out, and indeed, I think there is some evidence for this.

The next slide (Fig. 10) shows early data on this,¹⁰⁾ and shows you the danger of making arguments based upon insufficient data. In these experiments they did ^{16}O on ^{16}O and ^{12}C on ^{12}C , and what they measured were differential cross sections at a particular angle as a function of the incident energy of the particle. The dotted line is Mott scattering, which is the Rutherford scattering for identical bosons in this case, and we see that the experimental curve deviates very smoothly in the oxygen case and has a rather resonant behavior in the case of the carbon. At the time these data were taken, people said C on C makes a molecule but O on O doesn't. They said O on O is like He on He; it's sort of too much Pauli principle effect to really have any chance of sticking

Fig. 10

together, because it is doubly magic, but C on C isn't magic. In fact, it likes to sort of deform a little bit, and you can imagine that you can polarize the two carbons and make some states in the system by sharing nucleons between them. This would lead to nuclear molecular states involving a two center type shell model for carbon.

Well, they didn't do any calculations because, after all, they had data that shows there is a difference. But they did get better machines a little later, and the next slide (Fig. 11) shows what happens when you continue the O on O data to higher energies.¹¹⁾ It is oscillating very happily up and down with beautiful resonant structure, if you like to call it that. The standard model for this is to assume a complex potential and see how well you do with a molecular type potential of some form. You have to make it complex because oxygen on oxygen at these energies can break up in other ways than just oxygen and oxygen. The dotted curves correspond to a conventional optical potential in which you assume the same absorption process no matter what the impact parameter is; you use a constant imaginary potential to describe the refractive index of the problem corresponding to absorption. If, however, you allow

Fig. 11

the higher orbits to be not so well absorbed, in other words to behave more like the molecule, then in fact you are able to fit this data somewhat better, but still not perfectly. If you go through a detailed analysis of all this, various angles and various energies, you are forced always to use a model in which the absorption for high partial waves is quite small relative to what it is for low partial waves. This suggests that if nuclear molecules exist in this case, they exist only when the two oxygens are relatively far apart. Then they just rotate around one another and the reason why they might do that is because out there it is very hard for them to do anything else. They have not approached close enough to change states into anything else, whereas with the low partial waves, like an s wave, you can imagine it is like two cars coming together. You don't really want to ask for the comfort of the driver, which is what elastic scattering potentials are doing here.

Inelastic scattering, in which you excite one of the incoming heavy ions, has some new features. One of them is Coulomb excitation; namely, just the Coulomb field in the motion of two ions past each other can excite states in one or the other or both of the two nuclei. In fact, there is no doubt that Coulomb excitation will be done exclusively with heavy ions. As a nice feature, you think you understand the force in that case. You can investigate nuclear transitions via Coulomb excitation, even multiple Coulomb excitation in fact. If you use beams like bromine, which are now being used to do Coulomb excitation in places like Purdue, you can examine inelastic transitions by a pure Coulomb field, which people believe are analyzable. With light ions you simply, in general, don't get enough Coulomb effect to get very much multiple Coulomb excitation. Clearly the amount of Coulomb excitation ought to depend upon the charge you put into the system. Heavy ions carry much more charge.

A second feature which is quite new to use is the fact that you can excite not only the incoming particle and the target individually, but also together. So, for example, you can start with two carbons and excite them both to their 2^+ state, their first state, and then watch this in the way it decays:

This allows to look at some entirely new modes of nuclear motion. At present the data is still too sparse for us to really know what is going on.

I want to spend just two or three minutes here at the end talking about something I learned while I was at Princeton. Most of you may not have heard that that machine was shut down. It used to be called the Princeton Penn Accelerator, but changed its name to the Princeton Particle Accelerator when the University of Pennsylvania found it didn't have any money. It was shut down a few months ago for lack of funds. However, before they retired, they developed a very interesting facility, a 7 GeV ^{14}N beam. This

is a high energy heavy-ion beam. They investigated in particular something which was something of a desperation move--cancer therapy with heavy ions. Their initial results, which were discussed in Princeton's seminars, were successful in showing that heavy ions of this type are better than any other type of radiation therapy currently available. They did admit that π mesons may compete, but there was no π meson facility available with sufficient intensities at that time. Of course Los Alamos is due to start a π meson facility fairly soon.

The argument is fairly simple. If you have a human body, and you have a tumor in the middle of it somewhere, the idea is you bombard and you stop the heavy ion around about the tumor. Stopping there does radiation damage to the tumor cell and kills it, stops it from multiplying and growing. The surface, however, is also affected in any radiation treatment. After all, you always get radiation damage when you have any sort of charged particle passing through matter. The important thing about a heavy ion is that it does most of its damage at the end of its path rather than at the beginning. You don't do radiation therapy in one burst. If you do, you are likely to kill the patient on that one run. You may kill the tumor, but you will also give him sufficient radiation burns that he no longer cares. So it's done by a differential technique. You have a burst and you kill so many healthy cells and so many tumor cells. The advantage in the process is that you kill fewer healthy cells than you kill tumor cells. Moreover, the healthy cells recover after a certain time interval. You can kill so many every so often and get away with it. But the tumor cells do not recover at the same rate. Consequently, by successive bursts you can slowly but surely kill the tumor. The only advantage of heavy ions is that the ratio of tumor cells killed to healthy cells killed is bigger than for most of the conventional radiation treatments such as x rays, protons, or what have you.

A second thing that they looked into was meson production, because maybe after all the Los Alamos people aren't right and it isn't the best thing to do to make a high energy beam of protons

and produce pions. Maybe you should use ^{14}N and make your mesons that way. The argument is that perhaps (and this remains a perhaps as far as I know) when you have ^{14}N you can imagine that there are in effect always some of the nucleons essentially behind one another, which means that when I hit a target of some sort and produce a pion with the first one, then the next one that comes up may also hit that pion in the same direction and transfer its energy to the pion. In this case the pion can gain more than the energy per nucleon associated with the ^{14}N motion. You are also gaining because of Fermi motion in general, but that tends to go both ways, so you only spread your distribution. The idea was that perhaps you could produce pions of a higher energy than you would have expected with a single collision. At the time of shut-down they had observed π -production at forward angles, but I don't know if there were any π 's with energies much higher than that expected from single collisions.

The third thing they were looking for was smoke rings. The idea was from John Wheeler, and he had some calculations based on classical physics. If you take a big nucleus and a little ^{14}N , then the thing to do is to knock the middle out of the big nucleus all the way through.

The nucleus will supposedly become a rotating toroid. This, however, seems to neglect the fact there are correlations between one part and another part of the nucleus, and it doesn't seem obvious

to me why you should expect a toroid. But calculations performed recently by Wong¹²⁾ suggest that toroidal nuclei are stable. At least they are about as stable as a super heavy element.

I would like to conclude with the statement, even if heavy-ion reactions are not worthwhile, they certainly are interesting.

FOOTNOTES

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ELASTIC AND INELASTIC SCATTERING OF HEAVY IONS

Being the first speaker has both advantages and disadvantages. I can't assume you know very much; by the time you get to the eighth speaker, he'll assume that you know everything and that he can talk about anything. I do want to start fairly basically and cover quickly some of the ideologies that have developed through the years, particularly with regard to elastic scattering. I am going to talk about elastic scattering for most of the time, because there is very little inelastic scattering data now available, so there is not so much to talk about there.

The quantum mechanical description of elastic scattering in its simplest form (i.e. spinless particles) is, as we are aware, a simple sum of two terms:

$$F(\theta) = F_{\text{coul}}(\theta) + F_{\text{nuc}}(\theta) \quad .$$

With heavy ions we are particularly forced to use Coulomb terms. Usually we then call the remaining term nuclear, but really it isn't purely nuclear. Rather, it is the additional effect of the nuclear field in the presence of the Coulomb field. The Coulomb result is something that you can find in most textbooks. I will write it down in terms of fairly standard variables, the Sommerfeld parameter η , a wave number k , the usual Rutherford factor, and a phase term, whose presence represents the difference between the classical and quantum mechanical expressions.

$$F_{\text{coul}}(\theta) = \frac{-\eta}{2k} \text{Cosec}^2\left(\frac{\theta}{2}\right) \exp \left[-2i\eta \ln \sin\left(\frac{\theta}{2}\right)\right]$$

The nuclear part is then a fairly simple thing to calculate in terms of partial waves.

$$F_{\text{nuc}}(\theta) = \frac{1}{k} \sum_{l=0}^{\infty} (2l+1) e^{2i\omega_l} \left[e^{\frac{2i\delta_l^{\text{nuc}}}{2l+1}} - 1 \right] P_l(\cos \theta)$$

$$\omega_l = \eta_l - \eta_0, \quad \eta_l \equiv \arg \Gamma(l+1+i\eta)$$

This is a very old expression that you can find in books like Schiff, so I won't dwell on it. The usual angular momentum weighting factor of $2l+1$ is multiplied by the Coulomb phase factor $e^{2i\omega_l}$ for each partial wave (l is the angular momentum quantum number). The nuclear phase shift, δ_l^{nuc} , goes to zero for a fairly small value of l , in comparison with the Coulomb phase, which never goes to zero for any value of $l \neq 0$. The Coulomb phase corresponds to a point charge until you reach atomic radii at which stage the Coulomb field is turned off via screening.

The nuclear phase shift is complex. In other words, we can rewrite it as

$$\delta_l^{\text{nuc}} = \lambda_l + i\mu_l$$

It has two parts to it, and the only thing we are sure of in that expression is that the imaginary part must be greater than zero, corresponding to the fact that you must have absorption, if there is something else going on besides simple elastic scattering of particles. The cross section for spinless particles is given by $\frac{d\sigma}{d\Omega} = |F(\theta)|^2$. Since most of the calculations that people do for heavy ions do not put any spin dependence into the models, this is relatively general as far as the present levels of analysis of heavy ions is concerned. Of course, if the colliding particles are identical, you must take appropriate linear combinations of the scattering amplitudes, depending upon whether the wave function is symmetric or antisymmetric: $F(\theta) \pm F(\pi-\theta)$. Note that there is symmetry about $\theta = 90^\circ$ in the center of mass system cross sections for identical particles.

The earliest models for this were called strong absorption models. It was believed that when you splatter together two complex particles, the target somehow or other presents a black hole. That is, if the projectile goes within a certain area surrounding the target, it is no longer elastically scattered. If you think about

it, what that simply says is that at a certain distance from the target center there is a sharp change. You can call that distance the impact parameter b . If you want to go over to quantum mechanics, all you have to do is talk about the angular momentum corresponding to that impact parameter, which I said last time was something like momentum times the impact parameter. You would say that for l less than this critical value you want $e^{2i\delta_l^{\text{nuc}}}$ to be zero.

$$L = m v b$$

$$\delta_l^{\text{nuc}} = \lambda_l + i\mu_l = \begin{cases} i\infty & \text{for } l < L \rightarrow e^{2i\delta_l^{\text{nuc}}} = 0 \\ 0 & \text{for } l > L \rightarrow e^{2i\delta_l^{\text{nuc}}} = 1 \end{cases}$$

In other words, for $l < L$ the projectile is completely absorbed; there is no probability of having a scattered wave. (If $F_{\text{coul}}(\theta)$ is expanded in a series of Legendre polynomials, one sees that $F_{\text{nuc}}(\theta) = -F_{\text{coul}}(\theta)$ when $e^{2i\delta_l^{\text{nuc}}} = 0$. See Schiff, Quantum Mechanics (3d ed.), p. 143.) That is about as simple as you can get. This model took many names; often it was called the Blair sharp cutoff model.¹⁾ There are also more parametric forms of the phase shift δ_l^{nuc} , usually referred to now as diffraction models. I might remind you of the smooth cutoff models put out by McIntyre et al.²⁾ We rewrite $e^{2i\delta_l^{\text{nuc}}}$, which we call S_l , in the form

$$e^{2i\delta_l^{\text{nuc}}} \equiv S_l = A_l e^{2i\lambda_l},$$

$$A_l = \{1 + e^{(L-l)/\Delta l}\}^{-1}$$

$$\lambda_l = \lambda_0 \{1 + e^{(l-L')/\Delta l'}\}^{-1}$$

This gives a smooth variation, which is now called a Woods-Saxon function. I have never quite understood why people call it that;

it is really a Fermi distribution function. What they are trying to do is give a diffuse surface to the nucleus in this way. In all you have five parameters here with which to fit the data. I can't find anywhere the reason for using this. There is no justification given other than that it is a flexible parametric form that you can vary until you fit the data.

Early scattering work on heavy ions was done at fairly small angles and from fairly complicated nuclei; these types of models were sufficient to fit the data. But they didn't tell us very much. All they told us was that the nucleus looked like a black hole, which wasn't terribly interesting. More recently there has been an attempt to be popular, which is common among nuclear physicists these days who drag something from somewhere else, and this attempt has been to use Regge Poles to describe the elastic scattering of complex particles. This actually started with a number of papers, I think about five years ago, that were more or less ignored. Then McVoy³⁾ in this country realized that he could analyze data very simply with Regge Poles. He simply adopted the idea that this S_ℓ could be rewritten in a different form from any previous ones:

$$S_\ell = \{1 + e^{-i\alpha_e(L-\ell)/\Delta L}\}^{-1} \left\{1 + \frac{1D(\ell)}{\ell - L_0 - \frac{1}{2}\Gamma(\ell)}\right\}$$

$$D(\ell) = D_0 \{1 + e^{(\ell-L)/\Delta L}\}^{-1}$$

$$\Gamma(\ell) = \Gamma_0 \{1 + e^{(\ell-L)/\Delta L}\}^{-1}$$

Again, it is purely a parameterization model. He has something that could in fact be regarded as a pole if you imagine that the angular momentum can be complex: $S_\ell = \infty$ (where $\ell = L_0 + \frac{1}{2}\Gamma(\ell)$); this is the Regge Pole. One is able to fit data, but this is based purely on parameterization. There is no dynamics. I want to stress this; there is no real dynamics in anything I have said so far. All people want to do is take a lot of data and reduce it to a fairly small number of parameters and consider how they vary to see if

anything makes sense. I think with the type of data you have to begin with, that is about all you can hope to get out of this type of analysis.

The number of parameters in the Regge Pole model gets quite large, and again you start to question the significance of any parametric model that cannot be directly related to any dynamics. I am, of course, anti-Regge Pole theory, because there are absolutely no structure calculations in nuclear physics that show we should expect Regge Poles. The ideology comes, as far as I know, purely by analogy with the high energy situation. If structure theorists had always predicted Regge Poles in nuclei, then I would think this is a reasonable model to use. But as far as I know we have no structure theorists saying that we should have Regge Poles. There doesn't seem to be any necessity to go into the complex plane. It is hard enough in the real plane without going into the complex one.

This leads us to an old standby, the optical model, which I would like to explore a little today. I think that when people talk about the optical model they often view it in much the same style of parameterization as this and, therefore, completely ignore what can really go into an optical model style of fitting. The attempt of the optical model is to introduce an effective Hamiltonian that in principle can be calculated from a more fundamental (many-body) Hamiltonian. In the conventional optical model the effective Hamiltonian is written $H_{\text{eff}}(R) = T_{\text{opt}}(R) + U_{\text{opt}}(R)$, where the coordinate R is the separation between the centers of mass of the two heavy ions and T_{opt} and U_{opt} are the kinetic and (complex) potential energy terms, respectively. This is the basis of the old independent particle model, but it is a completely different scheme here because we are not talking about an effective Hamiltonian for each nucleon; we are talking about the relative motion of two large objects separated by a distance R . People try to find an effective potential for these two large objects and to calculate the elastic scattering from this effective potential, making sure it fits the data by adjusting the parameters.

What I want to do is to try and indicate some of the

features this model possesses if you develop a correct expression for the effective Hamiltonian operator instead of just taking the phenomenological approach and saying let's take a potential and fit it to data and see what happens. There may be properties of H_{eff} that should be built-in that are peculiar to heavy ions but with which we are not used to dealing. After all, when this was done for the nucleon it was just regarded as a simple extension of Hartree-Fock theory for the bound problem up into the continuum. If we have an independent particle model for the bound state problem, there is no reason why we couldn't have one also for the free state problem. In the present case it is a somewhat different problem, as you can see.

For heavy ions, $U_{\text{opt}}(R)$ is usually given a Woods-Saxon shape:

$$U_{\text{opt}}(R) = [1 + e^{(R-R_0)/a}]^{-1} (V + iW)$$

That is again a four-parameter model. It is about the same number of parameters as we had in the phase-shift models, but we intend later on to have some of these parameters relate to physical concepts from the dynamic theory. It also has the added advantage of producing an elastic scattering wave function. This can be used for other purposes later because it gives you roughly the average motion of the particle (in principle at least) and if something else happens you can treat that as a perturbation.

There have been some attempts to calculate this in a very direct fashion by thinking back to the old argument that if you have a nucleon moving at a certain energy interacting with a nucleus, then it has some sort of optical potential. What is done in this sort of model is to imagine that all you have to do in order to get from the single nucleon case to the heavy-ion case is to add up all of the single nucleon contributions. You define an effective potential that is given by simply integrating over the probability of finding a nucleon at a given point in the nucleus, which is usually given by looking at the electron scattering or

some such thing that measures the distribution of the probability of a nucleon within the nucleus, multiplied by the potential for a single nucleon:

$$V(\vec{R}) = \int \rho(\vec{r}_n) v_n(\vec{r}_n - \vec{R}) d^3\vec{r}_n$$

$\rho(\vec{r}_n)$ - probability density for finding a nucleon at $\vec{r}_n$.

$v_n(\vec{r}_n - \vec{R})$ - nucleon-nucleus interaction potential.

This is called a "folding procedure," and I am afraid the only thing I can say is that it is about to fold unless more careful work on the subject is performed.

One has to think about what goes into a folding procedure and why it should be regarded as useful. Remember that most of the experiments I am talking about are done near the Coulomb barrier. They are done at low kinetic energies in general; we don't have facilities for fast heavy ions at present. But the concept on which the folding procedure is based, which is really a perturbation series, doesn't make sense if you have two slowly moving heavy ions that come together. However, if you remember back in the history of nuclear physics, the Breit-Wigner formula was first developed by perturbation theory, and, if you went to second order, it did indeed give you a very nice Lorentz shape. The only problem was when you calculated the widths, they came out ridiculous because

you used perturbation theory. But the form of the formula was right. So what people are now doing, and I have to question this somewhat, is to put an effective operator in for the nucleon-nucleus potential. But then we haven't made terribly much progress, because we have just replaced one effective operator that we were trying to calculate by another one. The only place where this appears to have some validity is when you have two large objects that just start to overlap. The interaction is actually quite weak if they are not overlapping very much. Also, the Pauli principle should have appeared somewhere, but it doesn't. We distinguished particles on one object from particles on the other object. This leads us to the question, what happens if they overlap, because then we can't distinguish which nucleon came from which object. Not having proper antisymmetry with respect to exchange of nucleons is a defect that is not negligible in understanding the scattering.

Question: Why couldn't you antisymmetrize the folding procedure if you wanted to? Answer: You could. The question is how many nucleons are you going to take into account; how many orbits do you have to worry about. Then you have to have a structure model that goes back to the many-body problem. I am sure you will hear later on in this series about the models proposed by Greiner and others that say instead of regarding the colliding objects as separated nuclei, the only thing you can really do is to imagine that each is in a potential and do Hartree-Fock theory with a two-center problem. Then you calculate the potential energy of the system as a function of separation; in this way you can derive the potential. There are a number of problems there that I won't go into, because that is going to be part of his talk.

I would now like to move to the general properties of an optical model type operator. The Hamiltonian that produces elastic scattering can be derived in a formal way, and you can find some rather interesting properties for it. I will follow a fairly conventional nuclear theory here, which is the Feshbach⁴⁾ type

approach. The solution for the total Hamiltonian for the $(A_1 + A_2)$ nucleon problem is separated via projection operators into an elastic part and a non-elastic part:

$$\Psi = \underset{\text{elastic}}{P\Psi} + \underset{\text{non-elastic}}{Q\Psi}.$$

The idea is to find only the elastic solution and treat the non-elastic one only in some formal way. After all, if you are measuring elastic scattering, the only thing on which you have information at the end is $P\Psi$. Not only are you not sensitive to $Q\Psi$, but you are sensitive to what happens to $P\Psi$ only at infinity, because you make measurements only when the scattered objects get to the detector. We impose the requirements

$$P\Psi \underset{R \rightarrow \infty}{\sim} [\Psi_{\text{inc}} + F_{\text{el}}(\theta) \frac{e^{ik_{\text{el}} R}}{R}] \Psi_{\text{intrinsic}},$$

$$Q\Psi \underset{R_c \rightarrow \infty}{\sim} F_c(\theta_c) \frac{e^{ik_c R_c}}{R_c} \Psi_c, \text{ intrinsic}, \text{ all } c \neq \text{elastic}$$

$Q\Psi$ includes all the possible modes of scattering other than elastic scattering. R is the separation distance of the two heavy ions that go apart; $\Psi_{\text{intrinsic}}$ describes the internal states of the two heavy ions. I'll speak only about 2-body decays; in other words, I am dealing with low energies where I am going to assume that there are no 3-body decays of the type where I can have, say, a proton, a neutron, and an alpha particle all coming out.

Feshbach developed his theory by assuming the almost impossible task that he could construct mathematical objects P and Q that satisfy these conditions. If you think about it as a many-body problem, you will find that is very hard to find P and Q ; but, from a formal point of view, you can assume it is done and not worry about how difficult it is to actually do it. There are models where you can do this and see that the general properties

I write down are valid, but I don't want to go into a general theory of nuclear reactions in order just to talk about the optical model. Since P and Q are orthogonal projectors we have $PQ = QP = 0$. We also want $P + Q = 1$, $P^2 = P$, $Q^2 = Q$. We now have enough algebra to do the rest of the problem. I am not going to do the algebra; I am going to write down the answer. You start from $(H-E)\Psi = 0$ and put in $P + Q$: $(H-E)(P+Q)\Psi = 0$. You then multiply this expression from the left separated by P and by Q . This gives the two equations $P(H-E)(P+Q)\Psi = 0$ and $Q(H-E)(P+Q)\Psi = 0$. These two coupled equations can be solved formally to yield

$$(H^{\text{eff}} - E)P\Psi = 0 \quad \text{with}$$

$$H^{\text{eff}} = PHP + PHQ \frac{1}{E - QHQ + i\epsilon} QHP \quad .$$

H^{eff} represents the effective operator for elastic scattering alone. When I solve for $Q\Psi$ it turns out that it contains the coupling between channels that must exist if we are going to have anything different from elastic scattering:

$$(QHQ - E)Q\Psi = QHP(P\Psi) \quad .$$

The boundary conditions allow me to divide through and rewrite this as

$$Q\Psi = \lim_{\epsilon \rightarrow 0^+} (QHQ - E + i\epsilon)^{-1} QHP P\Psi \quad .$$

The reason why there is no leading term is because $Q\Psi$ by definition has no incident wave; it is purely an outgoing scattered wave describing all channels that weren't incident. I have to make sure we have an outgoing spherical wave, and that is why I put in the $+ i\epsilon$. That is just standard scattering theory for producing an outgoing spherical wave.

We are now in a good position to discuss the significance of what we have. The first term of H^{eff} , PHP , is like a self-energy

term; it doesn't have any reference to what happens in the remainder of the problem. It causes one to imagine all that can happen is that the two ions can overlap, and that they can have an energy and nothing else, and therefore you can calculate the potential energy of relative motion, forgetting about all of the other channels or all of the other many-body features of the problem. This, I should say, is roughly the folding procedure. In the second term of H^{eff} we have something that is clearly complex, and it should be complex because it allows us to imagine what happens as the particle comes in. It goes into channel Q, it moves around in that state, and then it comes back into the elastic channel. This term has two parts, a real part and an imaginary part, which we write as $V + iW$.

Let us now introduce the eigenstates of the operator QHQ :

$$(QHQ - E')|\alpha, E'\rangle = 0 \quad , \quad |\alpha, E'\rangle_{R_C \rightarrow \infty} \text{ scattering, } E' > E_{\text{threshold}}.$$

$$|\alpha, E'\rangle \rightarrow 0, \quad E' < E_{\text{threshold}}.$$

We can then write

$$V + iW = \sum_{\alpha} \int_{E'} \frac{[PHQ|\alpha, E'\rangle dE' \langle E', \alpha| QHP]}{E - E' + i\epsilon}.$$

We now use Cauchy's theorem to split this into a principal value integral minus another term (the so-called "on-shell" contribution):

$$V + iW = \sum_{\alpha} \int \frac{[PHQ|\alpha, E'\rangle dE' \langle E', \alpha| QHP]}{E - E'} - i\pi \sum_{\alpha} [PHQ|\alpha, E\rangle \langle \alpha, E| QHP]$$

This is possible because the numerator of the integrand is a positive definite object, so there is no problem. The imaginary part comes entirely from the second term, and that says if you have absorption it must come from

$$W = -\pi \sum_{\alpha} PHQ|\alpha, E\rangle \langle \alpha, E| QHP.$$

The real part satisfies the simple dispersion relationship

$$V = \frac{-1}{\pi} \oint \frac{W(E')}{E-E'} dE' .$$

This is a standard sort of dispersion relation that tells you how the real part V relates to the absorptive part W .

Roughly speaking, if W is approximately constant, V will be essentially zero. When E' is less than the energy E of the bombarding particle for the calculation, $-\frac{1}{\pi} \frac{W(E')}{(E-E')}$ is negative and goes down to minus infinity as you approach E . For E' greater than E we have essentially inversion symmetry, if W isn't varying. Thus, we would get essentially zero for V because the principal value integral is given by taking the area A minus the area B and letting the distance δ approach small values.

However, phenomenologically we know that normally the absorption grows roughly linearly, and V is clearly not equal to zero for a simple linear dependence of W on E' .

Well, I haven't said terribly much so far except that we have an absorptive potential, W , that can be simply written down in terms of states that are solutions characterized by boundary

conditions corresponding to the exit channels of the problem. They don't know anything about P at all; they only know about Q. A more rigorous treatment of the optical potential should include an average over energy to eliminate "fluctuation." At high energies and for complex nuclei this is only a minor alteration to the present discussion.

The first slide (Fig. 1) reminds you of what I have just

OPTICAL OPERATORS

$$H_{el} = PHP + PHQ \frac{1}{E - QHQ + i\eta} QHP$$

$$W = \text{Im. } H_{el}$$

$$= -\pi \sum_0^{\infty} PHQ |a, E\rangle \langle a, E| QPH \left. \begin{matrix} E > \epsilon \\ E < \epsilon \end{matrix} \right\}$$

$$PHQ |a, E\rangle \longrightarrow 0 \text{ for } J > J_c$$

$$\therefore W \longrightarrow 0 \text{ for } J > J_c$$

Fig. 1

said. The elastic scattering operator H_{el} is the sum of these two terms; the absorption comes from the imaginary part of H_{el} , which has a very simple form. If you are below the threshold for any exit channel other than elastic scattering, then W has to be zero, because when I did the integral over energy I wouldn't have had a point where E could equal E' , so there wouldn't be any absorption. However, above threshold we have an expression of the type shown that includes all possible states of energy E that are eigenstates of the fictitious Hamiltonian QHQ .

An important point I want to make is the following. Generally speaking, PHQ is a nuclear interaction; it is something

that couples the incoming particle to different types of motion; the Rutherford term won't do it. The only thing you have left is a short range type of force. (I ignore Coulomb excitation for the time being.) This would be essentially a nuclear force. Consequently, the overlap between the interaction that couples the two channels together and the state must go to zero as the angular momentum increases in the problem. These states can be decomposed into various angular momenta, and the overlap would go to zero as the angular momentum of the state increased because eventually the state is pushed out of the nucleus. There is an angular momentum barrier that grows and grows with J , and you will find this overlap must go to zero for some critical value of J . So we derive a rule here that the absorption will go to zero beyond a certain impact parameter. People have, of course, told us that will happen, because if you go far enough away, you just never have any interaction, and that corresponds to a large enough impact parameter. But there is a difference here. The angular momentum we are talking about is the angular momentum available in the exit channels only. It doesn't know anything about the entrance channel. These are states that have components solely from the part of the operator that relates only to exit channels; they don't know that they had an entrance. Consequently, the vanishing of the absorption relates only to the angular momentum of the exit channel and not to the angular momentum in the entrance channel.

Another property that I want to make very clear is that this is what we call a nonlocal operator. In other words, we integrate over something so it is an integral operator. What we mean is that whereas people usually say the potential is just a simple function of the distance apart, it really isn't. It is an operator that is a function of distance apart, but it also depends upon where it came from as an integral that operates on whatever follows it:

$$U(\vec{R}) = \int d^3\vec{R}' U(\vec{R}, \vec{R}')$$

This means that it doesn't just depend on where it is now, but also

on where it could be in the same coordinates with respect to what is following it. Nonlocal potentials have been found necessary in nuclear physics in order to remove some of the energy dependence of these operators. When you replace a nonlocal potential by a local potential, what you intend to do is make it velocity-dependent. When I said earlier that the optical potential in nuclear physics is energy-dependent, I should have said the equivalent potential version of it is.

Question: Couldn't the potential be both nonlocal and energy dependent? Answer: Yes, of course it could. The expression I wrote down for W in fact depends on the energy of the exit channels even though it is a nonlocal operator. It can be energy-dependent, and it must be energy-dependent in some way because it must be zero below the threshold of the first exit channel, and it is finite above the threshold. But much of the energy-dependence of the real part of the potential appears to vanish when you use a nonlocal potential, or at least it is heavily reduced.

There are two types of nonlocal potential with which people are probably familiar if they do theoretical physics. One type has a separable kernel of the form

$$U(\vec{R}, \vec{R}') = v(\vec{R})v(\vec{R}') \quad .$$

That is a rather extreme nonlocal potential that is used predominantly at present as a way of treating the three-body problem. The other nonlocal potential that has been used in nuclear physics is the so-called Perey-Buck⁵⁾ form:

$$U(\vec{R}, \vec{R}') = U\left(\frac{1}{2} |\vec{R} + \vec{R}'|\right) H(|\vec{R} - \vec{R}'|) \quad ,$$

$$\text{where } H(|\vec{R} - \vec{R}'|) = e^{-\beta^2 |\vec{R} - \vec{R}'|^2} \quad (\text{Gaussian})$$

and the factor U is given the same form as used in local models as a function of the average radial variable $p \equiv \frac{1}{2} |\vec{R} + \vec{R}'|$:

$$U(p) = -(V+iW_I)F_S(p) + iW_D F_D(p) ,$$

where F_S , F_D are the Woods-Saxon form factor and its derivative, respectively. This, as far as I know, is a prescription. I can't find any reason to assume this is a good form. What they are doing is saying it is almost local, but there is a nonlocality depending upon $|\vec{R}-\vec{R}'|$. The function $H(|\vec{R}-\vec{R}'|)$ is taken to be a gaussian, and if the gaussian is very sharp the expression will reduce back to the usual local potential. The object $U(\frac{1}{2}|\vec{R}+\vec{R}'|)$ is given a Woods-Saxon shape of normal local form. To my knowledge this is not solved exactly. They tried to solve this in a simple-minded way by using things like WKB. What they found was if you had a wave function that was a solution for the local potential that just matched very nicely at large distances somewhere, then the wave function for the equivalent nonlocal potential would show much less amplitude inside the nucleus. So it damps the wave function internally to use a nonlocal potential. That is, to my knowledge, specific to this particular form; I have never seen it proved as a general theorem. But nonlocalities may actually damp a wave function, so I think there is some work to be done, particularly for heavy ions, because we know that the absorption in heavy ions is very large, and therefore the presence of nonlocalities in heavy ions appears to be much more likely than it is in light ions. That is not so surprising when you have two big objects. It doesn't just depend on where the centers are, it is going to depend on integrating around in the sphere they can occupy.

A feature that I already alluded to is that the absorption should vanish as a function of angular momentum. This is something we tried to investigate in a number of cases.⁶⁾ Let me show you some typical fits to data where one assumes one of two things; either one lets the absorption simply go to zero by the fact that the elastic wave function doesn't overlap with it, or else you force it to zero before that. The description we used for the potential is as follows. (Fig. 2) We split it into real and imaginary parts, and we assumed that the angular momentum dependence of the real part is small. There is no reason to assume

EFFECTIVE POTENTIALS

$$V \rightarrow U(r, E, J^2) + i W(r, E, J^2)$$

$$U(J) \approx U$$

$$W(J) \approx W \left[1 + e^{(J - J_c)/\Delta J} \right]^{-1}$$

Fig. 2

that, other than the fact that there is no prescription for it. The absorption turns out to be the major factor anyhow when you do the calculations. We gave the absorption potential the usual Woods-Saxon cutoff as a function of J , and we are back to the same thing the strong absorption people did with the phase shifts, except we do it with the operator, and believe that is reasonable from what we have been talking about. The parameter J_c is supposed to be typical of the maximum angular momentum that can be carried out in the exit channels.

The fit obtained with this sort of model is shown in the next slide. (Fig. 3) This is for the case $^{16}\text{O} + ^{11}\text{B}$. With a standard optical model, which is just a simple $V+iW$ with no angular momentum dependence in the absorption, you simply do not get two things. You don't get enough oscillation, and you don't get the large rise in the differential cross section at large scattering angles that seems to dominate almost all heavy-ion elastic scattering. In almost every experiment the cross section rises above where it would be if you believed in the simple optical model. The cross section is given as the ratio to the Rutherford

Fig. 3

cross section. The reason it drops below Rutherford is due primarily to absorption.

The next slide (Fig. 4) is another fit, this time for the

Fig. 4

case $^{12}\text{C} \rightarrow ^{10}\text{B}$. Note these are very light nuclei that we are considering with almost equal masses in each case. Again you get the same effect, the curve for the simple optical model goes steadily

down, and the data comes back up. The only difference between the two curves that oscillate is the presence of a spin-dependent force in the problem. It appears necessary to put one in if you want to describe the data more accurately, but I am not going to consider that in detail here.

The next slide (Fig. 5) is the case of ^{12}C on ^{19}F . The

Fig. 5

same sort of effect is observed. I might point out that the vertical scale covers four orders of magnitude, so you can get some idea of how these cross sections vary and perhaps why there is no data in the center.

The next slide (Fig. 6) is the case that was quoted by Austern⁷⁾ as evidence for a process that I want to talk about in the next few minutes. That is that ^{16}O scattering on ^{12}C may transfer four nucleons across and form ^{16}O again and leave behind ^{12}C . In other words, you can imagine that the reaction goes $^{16}\text{O}(^{12}\text{C}, ^{16}\text{O})^{12}\text{C}$, or you can imagine that it goes $^{16}\text{O}(^{12}\text{C}, ^{12}\text{C})^{16}\text{O}$. In the first case you say, ah ha, I picked up four nucleons and I lost four nucleons, and the sum total is correct. But you can't distinguish between these two except kinematically the first should happen at back angles and the second should happen at forward

Fig. 6

angles. Consequently, their argument was you would get a very large backward peak. The calculations shown in Fig. 6 were done by just turning the absorption off at a critical value. Don't worry about the fact that the critical angular momentum is 11.12; that doesn't mean I didn't use discrete values. This is just a parameter telling me the mean value of the angular momentum that is critical. What they failed to tell us in the transfer theory article is that this point is 1/10th of a barn and when you look at the cross sections for four nucleon transfer, they typically are a millibarn or 5 millibarns, but they are not 100 millibarns. So the explanation of exchanging an alpha particle between the incoming particle and the target seems very unlikely, and I managed to persuade the author of the original work to agree with me (at least temporarily!) on that particular issue.

I would now like to talk a little bit about elastic transfer. The particular type of case that people have thought about is when the two heavy ions are almost the same mass, e.g. $^{12}\text{C} + ^{13}\text{C}$. Those people who have thought about things like protons plus hydrogen atoms will see the comparison. In that case all I have to do is transfer an electron and I have the same undistinguishable final state. Here what they are saying is that

^{13}C can be viewed as ^{12}C plus a neutron, and single nucleon transfer should be allowed in this pattern:

It is rather nice in this theory that it is very simple to do calculations. All you have to do is to do molecular theory, because you have two large atoms and one particle that does the binding for you. You can use molecular orbital theory to calculate the effective potential that this particle generates in running around these two identical cores. The actual potential it generates turns out to have a part that comes from the expectation value of the orbit itself, much in the way I did the folding procedure, plus an exchange term corresponding to the fact I can interchange the cores. Since the cores are identical, the problem has a nice symmetry and you get a + or a - with a phase depending upon the total spin of the cores, which in this case is zero.

The model⁸⁾ used is a direct adaptation of molecular orbital theory and uses a "two-state" approximation.

$$\begin{array}{ccccccc} \vec{R}_{A'A} & & \vec{r}_{Ac} & & \vec{R}_{AA'} & & \vec{r}_{A'C} \\ \downarrow & & \downarrow & & \downarrow & & \downarrow \\ \psi(\vec{r}, \vec{R}) = F(\vec{R}) \phi_B(\vec{r} + \frac{1}{2} \vec{R}) + (-)^S F(-\vec{R}) \phi_B(\vec{r} - \frac{1}{2} \vec{R}) \end{array}$$

in which $\phi_B(\vec{r} \pm \vec{R}/2)$ represents orbital motion about A (+) or A' (-)

$$B = A + c \\ \text{or } A' + c$$

respectively. It is assumed to be an eigenstate of the unperturbed system $A + c = B$,

i.e. $H_B \phi_B = E_B \phi_B$, where

$$H_B = -\frac{\hbar^2}{2m} \nabla_{Ac}^2 + V_{ac}$$

The phase $(-)^S$ assures the correct spatial symmetry of the solution $\Psi(\vec{r}, \vec{R})$ under the interchange of the coordinate labels for the identical cores, $A \nleftrightarrow A'$. The total solution (neglecting spin forces) takes a product form

$$\phi = \Psi(\vec{r}, \vec{R}) \chi_{Sv}$$

where

$$\chi_{Sv} = \{ \chi_{S_{AA}} \oplus \chi_{S_{A'A'}} \}_{Sv}$$

is the vector coupled total spin of the two cases. Since the interchange operator $P_{AA'}$ has eigenvalues

$$P_{AA'} \phi = (\pm) \phi,$$

corresponding to A, A' being bosons (+) or fermions (-), and

$$P_{AA'} \chi_{Sv} = (-)^{2S_{AA'}} \chi_{Sv} = (\pm) (-)^{-S} \chi_{Sv},$$

The $(-)^S$ in the expression for $\Psi(\vec{r}, \vec{R})$ does indeed specify the symmetry of the spatial function which must be used to obtain the overall desired symmetry. Using

$$H = \frac{\hbar^2}{2M} \nabla_R^2 + U_{AA'}(R) + V_C(R) \\ - \frac{\hbar^2}{2m} \nabla_r^2 + V_B(\vec{r} + \frac{1}{2} \vec{R}) + V_B(\vec{r} - \frac{1}{2} \vec{R})$$

and the "two state function" yields the coupled equations

$$\begin{aligned} & \left[-\frac{\hbar^2}{2M} \nabla_R^2 + U_{AA'}(R) + V_C(R) + E_B + K(\vec{R}) - E \right] F(\pm \vec{R}) \\ & = (-)^{S+1} J(\vec{R}) F(\pm \vec{R}) \quad , \end{aligned}$$

or with

$$g_{\pm} = \frac{1}{2} [F(\vec{R}) \pm F(-\vec{R})] \rightarrow \begin{cases} F(R) = \sum_{\ell} F_{\ell} P_{\ell}(\cos \theta) \quad , \\ \text{even partial wave for } + \\ \text{odd partial waves for } - \end{cases}$$

$$\left[-\frac{\hbar^2}{2M} \nabla_R^2 + U_{\pm}(\vec{R}) + E_B - E \right] g_{\pm}(\vec{R}) = 0 \quad ,$$

where

$$U_{\pm}(\vec{R}) = \underbrace{U_{AA'}(\vec{R}) + K(\vec{R})}_{U_{\text{opt}}(\vec{R})} \pm (-)^S J(\vec{R}) \quad ;$$

i.e., a parity-dependent potential with a phase depending on the core exchange properties.

In the above we have set

$$K(\vec{R}) = \int \phi_B^*(\vec{r}_{Ac}) v_B(\vec{r}_{A'c}) \phi_B(\vec{r}_{Ac}) d^3 r_{Ac} = \text{folded potential for added nucleon,}$$

and

$$J(\vec{R}) = \int \phi_B^*(\vec{r}_{Ac}) v_B(\vec{r}_{A'c}) \phi_B(\vec{r}_{A'c}) d^3 r_{Ac} \quad .$$

↘ orbit exchange

What this does is give you a parity dependent potential; U for even partial waves is not equal to U for odd partial waves. This is rather nice in fitting data, because what you realize when you look at backward angles is that the scattering amplitude is of the form $F(\theta) = \sum (2\ell+1) A_{\ell} P_{\ell}(\cos\theta)$, and $P_{\ell}(\cos\theta)$ has the feature of being $(-1)^{\ell}$ at 180° so that if the A_{ℓ} is roughly a constant, you

get nothing at the backward angle. If you could differentiate between odd and even parity, make these A_2 somewhat different, clearly you would get a cross section at backward angles, and that is exactly what we get.

More important, however, is the fact that one can compare cases like $^{12}\text{C} + ^{13}\text{C}$ with cases like $^{13}\text{C} + ^{14}\text{C}$. Von Oertzen^{8,9)} has calculated a few examples using approximations to evaluate the exchange term. His results for $^{12}\text{C} + ^{13}\text{C}$ and $^{13}\text{C} + ^{14}\text{C}$ show good qualitative agreement with experimental results. The exchange term has opposite sign for these two cases because in one case you have two identical bosons and in the other case you have two identical fermions. In other words, in one case the exchange term will give a plus, and in the other case it will give a minus. Consequently, when you do a cross section calculation, the ordinary optical model drops very smoothly, as it should. The exchange term is oscillatory, the two cases having opposite phases. The data that are presently being done do indeed show this characteristic in the cases that have been looked at.

I find people who do this suddenly decide that they want to be more adventuresome and look at cases like $^{14}\text{N} + ^{14}\text{C}$ compared with $^{14}\text{N} + ^{12}\text{C}$.¹⁰⁾ I don't understand the logic in this case. You may as well argue that these two behave identically anyway because ^{14}C has the same characteristics as ^{12}C except for having two more neutrons, so they would look identical except for a very slight change in the radius. So you expect to see the same sort of cross section. When they look at these two, they do see the same sort of cross section, but it is argued that the exchange process here is similar to the exchange process in the preceding case. You imagine that a deuteron is transferred in the case $^{14}\text{N} + ^{12}\text{C}$. This is regarding the ^{14}N as $(d + ^{12}\text{C})$. The only way you can make the exchange process in the $^{14}\text{N} + ^{14}\text{C}$ case identical is to start thinking of it as a proton and a neutron in a 1^+ state that goes over one way while two neutrons in a 0^+ state are transferred the other way. In other words, you go crazy, because you are asking effectively for four nucleons being transferred to

become just two nucleons transferred, and that doesn't make sense. I think this is very unlikely; the model is being pushed too far. But, you know, when you have an idea that works once it is nice to try to make it work for everything.

I feel that we have reached an impasse in elastic scattering; in order to understand elastic scattering you have to understand what is not there. It is not sufficient to look at just the elastic channel to understand elastic scattering when there are a million other channels that are coupled to this elastic channel. In order to understand the way in which the absorption behaves you have to look at those channels and understand them. I think the way in which heavy-ion physics is now moving is to look at channels that are not elastic so that you can understand the whole process. The reason for not doing this earlier was that the only data available were for elastic scattering and therefore you wanted a model that pertains to elastic scattering. As soon as you have enough data that pertains to non-elastic scattering, you should start using that information in order to understand how the heavy-ion reaction proceeds. In the next talk we will discuss some of that.

FOOTNOTES

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TRANSFER REACTIONS WITH HEAVY IONS

251-276

I have a lot to cover today, so I may not be so explanatory about things. I'd like to get over some of the ideas even if it is only a matter of expressing some comments rather than trying to prove anything to you. I want to cover inelastic scattering, and then move into the transfer problem, solely within the framework of direct reaction theory. In particular, I will specialize to the finite range distorted wave theory. We are developing codes in that area, so I can, for a change, talk about something we are doing as well as what other people are doing. Finally, I want to talk about the quartet situation in relation to a large number of experiments. I will briefly describe each one and show there are some contradictions in that area.

Let's start with inelastic scattering. There are almost no data, so I won't show any. That makes life easy. The theory of inelastic scattering for heavy ions is poorly developed. In the case of collective excitations, e.g. rotational or vibrational excitations, the approaches under study have been extrapolated from light ion physics. In light ion physics the conventional optical potential is generalized to allow for collective excitations by using the form shown in Fig. 1. Here R describes the deformed or vibrating surfaces of the target nucleus and $\vec{\xi}$ represents the collective coordinates of the target nucleus. The ideology for light ions is that all you have to do is describe the surface of the nucleus and the collective excitations. For light ions that seems very reasonable, because I bring in a light nucleon and I can sort of examine every point on the target surface with the nucleon acting like a point particle.

In the heavy-ion situation you have two large things coming together, both of which can have a shape. No one bothers, as far as I know, to use a deformed shape for a nucleon projectile. But certainly if you are going to put carbon onto carbon, you have to have a theory that allows deformations of both nuclei. There is nothing special any more about the target if you are doing

GENERALISED OPTICAL MODEL

$$U = \frac{V + iW}{1 + e^{(r-R)/a}}$$

LI's

$$R = R_0 \left(1 + \sum_{LM} \alpha_{LM} Y_{LM}(\theta, \phi) \right)$$

$L=2$ QUADRUPOLE, $L=3$ OCTUPOLE

ROTATIONS

$$\alpha_{LM} \rightarrow \hat{Q}_{LM} Y_{LM}^* \left(\frac{\hat{r}}{R} \right)$$

VIBRATIONS

$\alpha_{LM} \rightarrow$ phonon creation op.

HI's

COLLECTIVE VARIABLES

$$\alpha_{L'M'}(\vec{z}_A) \quad , \quad \alpha_{L''M''}(\vec{z}_B)$$

IRREDUCIBLE SET

$$\left[\alpha_{L'M'}(\vec{z}_A) \otimes \alpha_{L''M''}(\vec{z}_B) \right]_{LM} \equiv \alpha_{LM}^{L'L''}(\vec{z}_A, \vec{z}_B)$$

$$R = R_0 \left(1 + \sum_{L'L''LM} \alpha_{LM}^{L'L''}(\vec{z}_A, \vec{z}_B) Y_{LM}(\theta, \phi) \right)$$

NOTES

(1) MUTUAL EXCITATION ALLOWED

(2) $\alpha_{LM}^{L'L''00}(\text{HI's}) \Rightarrow \alpha_{LM}(\vec{z}_A)$ of LI's

heavy ion on heavy ion. I think one of the difficulties with heavy-ion analyses for collective excitations so far is that they have not been done properly because the target has been treated as though it alone gets collectively excited, and everything about the projectile has been forgotten. But clearly one should include the possibility that the projectile has collective variables associated with it, just as the target does.

The suggestion I will make now, and not go past this point today, is that one should form irreducible cross products of the collective variables for each of the two systems. This is indicated in Fig. 2. This is just a Clebsch-Gordan type product, which leads to a new set of collective variables that depend upon collective variables in each of the two ions. You can define the radius of the system now, which after all can look like a dumbbell if, instead of looking at just one deformation, you have both deformations built-in. One thing this does is to allow for the possibility of mutual excitation, because I have a collective variable for each ion. Clearly I can get cross products where I have say a quadrupole here and a quadrupole here. I can, therefore, have a collective excitation on both nuclei, and consequently I can allow for mutual excitation. The conventional models people are using, where you forget about the collective excitation of one of the ions, only allow for the corresponding L being zero for the projectile. Here we allow for rather more possibilities.

Question: What is the meaning of R in this expression?

Answer: This R is the separation between the centers of mass of the two particles, the same as it was before, and of course elastic scattering will still be given primarily by the leading term.

That is all I am going to say about inelastic scattering, except that if you are doing Coulomb excitation, some of this ideology will also hold. As far as I know, it is not now properly treated. People who do Coulomb excitations still think of their incident beam as something very different from their target. That doesn't seem correct to me. If I use bromine on nickel, or

something like that, then why should I think that bromine is different from nickel; it can also be Coulomb excited. In heavy-ion work you find that multiple Coulomb excitation and multiple nuclear excitation are quite likely; consequently, this type of process should not be ignored. Even if it doesn't take place physically, it should take place virtually and be an important contribution to what is going on.

Now, let me move on to the problem of transfer reactions, which I think are most interesting to heavy-ion spectroscopists right now. I will start with the Finite Range Distorted Wave Born Approximation, FRDWBA. We consider reactions of the type $C_2(A_1, C_1)A_2$, as shown in Fig. 3. It looks complicated, so I will go a little slowly to begin with in order to get the point across. What you start with is a reaction in which you imagine that a certain amount of mass is going to be transferred from the incident particle, which I will call A_1 , to the target, C_2 . C_1 and C_2 will be regarded as essentially inert objects. In fact, the only factor that we will put in the equation is to describe the elastic scattering of C_2 by A_1 . That wave function appears here as the object that is called the initial heavy-ion elastic scattering relative motion, and is a solution of the Schrödinger equation for elastic scattering in the initial state. This is a standard distorted wave Born approximation, after which a certain amount of expansion has been done such that the transferred mass is simply given an orbit initially about C_1 and finally about C_2 . This is a molecular type situation where you imagine you can get from C_1 over to C_2 , and you are going to treat that probability to first order. So this is a first-order perturbation in that sense.

This is the current form of the theory. There actually are other interactions that should go in here, but I won't discuss them today because they are a little harder to treat. This is very similar to the things people are trying to do with an electron, a proton, and a proton in atomic physics, although normally people would try to do a little better than first-order perturbation theory in that case. For complex atoms where you transfer one electron, this sort of picture is about all people have done so far.

Also, for heavy ions we have not really gone beyond this point. In fact, only one group so far has managed to perform this six-dimensional integral for heavy ions.¹⁾

The coordinates here are a little hard to see. The $\vec{r}_1$ describes the bound orbit about C_1 that has orbital angular momentum l_1 and total angular momentum, including the spin of the transferred particle, of j_1 . The final orbit about C_2 has angular momenta of l_2 and j_2 and is described by a coordinate $\vec{r}_2$ about C_2 . The initial relative motion of A_1 and C_2 is described by $\vec{r}_1$, which runs from the center of mass of A_1 . Similarly, the coordinate $\vec{r}_p$ describes the final motion of A_2 relative to C_1 . Finally, there is a change in the coordinate system in such a way that you are left with six dimensions over which to integrate. Each function in the integrand is a function of a different coordinate. You must realize that these are numerical functions and have no analytic structure, so you can't start simplifying by doing transformations in coordinates. Some actual coordinate relationships are listed at the bottom of Fig. 3. The initial separation between the two heavy ions coming together can be written in terms of the initial and final bound orbit coordinates $\vec{r}_2$ and $\vec{r}_1$ as shown. Usually the transferred mass is quite small; you are doing one nucleon or two nucleon transfer with ions like ^{16}O , so the correction factor $(m_p/m_{a1})\vec{r}_1$ is small. But it turns out that it is more important than people first imagined, even though it is small. We will get back to that point later. Similarly, $\vec{r}_p$ can be expressed in terms of $\vec{R}$ and $\vec{r}_2$. These really are just linear relationships you can read directly from the diagram. If I should ignore the two correction terms, $\vec{r}_1$ and $\vec{r}_p$ would both equal $\vec{R}$; that is called the no recoil approximation. One just imagines that the cores have an infinite mass compared to the transferred particle. In atomic physics that mass ratio is one part in 2000 or more, but in nuclear physics it is more like one in 10, which is not so insignificant.

The standard approach to this in light ions is to say why bother with the six dimensions; why not convert to a three-dimensional integral by using the fact that nuclear forces are short range and by using the zero range approximation to the problem.

(Fig. 4) You take the interaction causing the transition, multiply it by the orbit that the transferred particle is in initially, and replace that by a delta function. However, there is a selection rule that says you can do this only for $l_1 = 0$. Of course the light ions, which are protons, deuterons, tritiums, and alphas, all have internal s-states, so this is a reasonable thing to do, or at least it is a start. The six-dimensional integral goes down to three-dimensional since the delta function allows one to do the integral over $\vec{r}_1$ immediately. The zero range approximation has been applied to heavy ions. In that case, the difference between the incoming angular momentum of the heavy ions and the final angular momentum of the heavy ions is just given by the orbital angular momentum with which the transferred particle goes into the final nucleus. It is a very selective situation that the transferred angular momentum is completely determined by whatever the final orbit is. That is why it has been a spectroscopic tool, because the transferred angular momentum is defined entirely by that of the final orbit. It has nothing to do with the initial angular momentum, because it happens to be zero, and so it is a very simple angular momentum coupling.

In heavy ions, say with oxygen, carbon or lithium beams, one encounters the fact that l_1 is not equal to zero, and this won't work as an approximation simply because $\phi(\vec{r}_1) = 0$ at $\vec{r}_1 = 0$ for all $l \neq 0$. Only s-wave functions can be finite at the origin because that is a head-on situation. Anything else has an impact parameter classically and quantum mechanically. So you are forced to do something about the finite range of the situation. The thing people did first was to realize that heavy-ions are massive and that they could get away with the approximation $\vec{r}_1 \approx \vec{r}_F = \vec{R}$ if $m_p/m_{a1}, m_p/m_{a2} \ll 1$. That reduces the integral I to the form shown in the lower part of Fig. 4. The way it is done is to expand $\phi_{l_2 j_2}^*(\vec{R} + \vec{r}_1)$ in terms of a set of basis states and do this as a sum of separate integrals. The selection rules you obtain are shown at the bottom of Fig. 4. There L represents the transferred angular momentum. The current codes in this area are those written by Tobocman and Schmittroth²⁾ and by Buttle and Goldfarb.³⁾

FRDWBA

CO-ORDINATES

6-D INTEGRAL

$$\begin{aligned}
 I = & \int d^3 r_i d^3 r_f \underbrace{\chi^{(-)}(\vec{r}_f)}_{\text{Final HI's elastic wave fn.}} \underbrace{\phi_{l_2 j_2}^*(\vec{r}_2)}_{\text{Final bound orbit of p}} \\
 & \times \underbrace{V_{pC_2}(\vec{r}_i)}_{\text{stripping inter'n.}} \underbrace{\phi_{l_1 j_1}(\vec{r}_1)}_{\text{initial bound orbit of p}} \underbrace{\chi^{(+)}(\vec{r}_i)}_{\text{initial HI's elastic scatt. relative motion}}
 \end{aligned}$$

$$\vec{r}_i = \vec{r}_2 - \frac{m_{C_1}}{m_{a_1}} \vec{r}_1 = \vec{R} + \frac{m_p}{m_{a_1}} \vec{r}_1$$

$$\vec{r}_f = \frac{m_{C_2}}{m_{a_2}} \vec{r}_2 - \vec{r}_1 = \vec{R} - \frac{m_p}{m_{a_2}} \vec{r}_2$$

ZERO RANGE

$$V \phi_{\ell_1 j_1} \rightarrow K \delta(\vec{r}_1) \quad \boxed{\ell_1 = 0}$$

$$6D \rightarrow 3D \text{ "easy"}$$

$$\vec{L} = \vec{L}_f - \vec{L}_i = \vec{\ell}_2 \text{ ONLY}$$

FR. NO RECOIL

$$\vec{r}_I \approx \vec{r}_F \approx \vec{R} \quad \frac{m_p}{m_e} \ll 1$$

$$I = \int d^3 \vec{R} \chi^{(-)*}(\vec{R}) F_{\ell_1 j_1, \ell_2 j_2}^L(\vec{R}) \chi^{(+)}(\vec{R})$$

$$F_{\ell_1 j_1, \ell_2 j_2}^L = \int d^3 \vec{r}_1 \phi_{\ell_2 j_2}^*(\vec{R} + \vec{r}_1) V(\vec{r}_1) \phi_{\ell_1 j_1}(\vec{r}_1)$$

$$\ell_1 + \ell_2 + L = \text{even}$$

$$|\ell_1 - \ell_2| \leq L \leq |\ell_1 + \ell_2|$$

$$|j_1 - j_2| \leq L \leq |j_1 + j_2|$$

$$\underline{\text{EXACT}} \quad \ell_1 + \ell_2 + L = \text{odd and even}$$

The only essential difference between Tobocman and Schmittroth and Buttle and Goldfarb is that the representation of the final state orbit is different in the two cases. Buttle and Goldfarb assume that the interaction takes place entirely outside the nucleus, in which case you can use the asymptotic form for the radial dependence, which is just a spherical Hankel function that has nice transformation properties. Clearly that works only around the orbital Coulomb barrier, where you are really outside the range of the nuclear force. Tobocman and Schmittroth construct oscillator expansions of $\phi_{l_2 j_2}^*(\vec{R} + \vec{r}_1)$ and use the fact that oscillators have rather nice transformation properties by Moshinsky brackets, so their calculations are more general than the Buttle and Goldfarb technique.

What is of vital importance here is that the selection rules in the no-recoil approximation are quite clear cut. The selection rules on L are no longer as stringent as they were in the case of the zero range approximation because in this case I can have a range of L values. What is important is that in the no-recoil approximation there is a parity rule. The L of the transferred particle must be such that the parity of the final orbit times the parity of the transfer is the parity of the initial orbit conserved. You lose this, however, in the exact finite range case where you don't make these approximations; you find that there is no parity selection rule on the angular momentum of the transferred particle.

I'll come back to an example of what happens when you use these selection rules later. Right now I want to talk about how one does exact finite range DWBA. A method for light ions was developed initially by Austern, Drisco, Satchler and others.⁴⁾ Their suggestion was to take the initial orbit times the interaction times the final orbit, namely the bound state part of the problem, and try to rewrite it in terms of the coordinates of the free motions of the ions, as indicated at the top of Fig. 5. Then you just have to do the integral over these coordinates. They do the angular integrals analytically, using some rather nice transformations for spherical harmonics of this type. The only problem

in this technique (and even for light ions this is a problem) is that you need a radial step of about .1 Fermi to do the numerical integrals. For direct reactions the typical range of the integrals is 20 Fermis. That means you have 200 values of r_I and 200 values of r_F , making an array of 40,000 points in all. You can start to see why it takes a long while.

This technique has been used recently at Saclay;¹⁾ a man named Perrenoud initiated the program and a man named DeVries exploits it. They now have a code of this type going for heavy-ions and claim that it runs in the order of 5 minutes, but they use an IBM 360-91, which is about the biggest computer you can get right now for this sort of work. They have also made some restrictions in their theory that they don't tell you about. I won't go into these now, since I don't think this is really going to be the way in which you do these calculations. The accuracy of their technique for heavy ions is not really tested, and it would appear likely that it is not as accurate as they would like to think. At least the number they quote as 5 minutes is the case which is the "typical" one, as we all say when we give talks; it is probably the best one they have ever had. The really typical time is probably 30 minutes or more, and when you have 30 minutes on an IBM 360-91 and you convert that into an IBM 360-50, the time goes up to around 20 hours for something this tough.

In Tallahassee we have also written codes of this type for light ions and found that indeed they can take 5 hours for a calculation, and it is no fun. You run all night, and then when you get an answer you are not sure it is right. We have been trying to improve on this by using a different technique.⁵⁾ The approach used is to expand every distorted wave into a set of Bessel functions of the same angular momentum, as indicated in the lower part of Fig. 5. We have to do that only over a finite range $0 \leq r_I \leq R_{\max}$, with $R_{\max}$ being a value where the form factor product $\phi^* V \phi$ is essentially zero. If we did it over an infinite range, we would be doing what high energy physicists call the momentum representation. The way we are doing it, it is a momentum

representation, but we do it within a box that is the same size as the range of this kernel. These are bound states, and beyond a certain point $\phi^* V \phi$ goes to zero, so we only need to fit it over a finite range. The important thing about the Bessel function expansion is that it is multiplied by an angular function of the same angular momentum, and the product $j_L Y_{LM}$ can be rewritten as the integral of the angular projection in the momentum space times a plane wave, as shown in Fig. 5. Now the nice thing about this expression is that it is extremely easy to transform because the $\vec{r}_1$ and $\vec{r}_2$ and the $\vec{r}_I$ and $\vec{r}_F$ are linearly related;

$$e^{-i\vec{k}_n \cdot \vec{r}_F} e^{i\vec{k}_n \cdot \vec{r}_I} = e^{-i\vec{q}_{nn'} \cdot \vec{r}_1} e^{i\vec{Q}_{nn'} \cdot \vec{r}_2}$$

with $\vec{q}_{nn'} = \frac{m_{c1}}{m_{a1}} \vec{k}_n - \vec{k}_n$ and $\vec{Q}_{nn'} = \vec{k}_n - \frac{m_{c2}}{m_{a2}} \vec{k}_n$.

The six-dimensional integral separates into two parts involving separate coordinates:

$$I(\vec{k}_n, \vec{k}_n') = \int e^{-i\vec{q}_{nn'} \cdot \vec{r}_1} V(r_1) \phi_{\ell_1 j_1}(\vec{r}_1) d\vec{r}_1 \\ \times \int e^{i\vec{Q}_{nn'} \cdot \vec{r}_2} \phi_{\ell_2 j_2}^*(\vec{r}_2) d\vec{r}_2.$$

The major difference between this expression and the technique described earlier is that we have a sum over the number of momentum points instead of the number of radial points, which is what we'd expect, and we find we need only about 15 momentum points ($n, n' \sim 15$) instead of 200 radial points. In fact, that number 200 I quoted earlier is really typical only for light ions; for heavy ions, that number may well go up to 500. As I said, I don't think the accuracy of the Saclay calculations has really been tested. We have tested our technique for $^{12}\text{C}({}^6\text{Li}, d){}^{16}\text{O}_{6.06}^*$, and we find that we need about 200 points in momentum space to achieve

2% accuracy in the cross section. We have compared our calculations to the Japanese code of this type that came out recently,⁶⁾ which was devised for the (${}^6\text{Li}, d$) reaction, and the time it takes is five times the zero range in that case, which is really quite fast. And we have only a CDC 6500, which is slower by at least a factor of 5 than the 360-91, but the time it took for the (${}^6\text{Li}, d$) reaction was only 2 minutes, which is getting down to something reasonable. I might add that we also have spin-orbit distortions, which none of the other codes have. In other words, we have repeated all that the code DWUCK can do except we have finite range, which we hope will be of significance.

Let me give you an example that was in a recent issue of Physical Review Letters.⁷⁾ This example has to do with what I call comparative spectroscopy; I think this is what heavy-ion people are going to be doing in order to remove theorists from their problems. In other words, if they can take ratios instead of having to do things absolutely, they don't need a theorist quite so much. The title of the article is "j Dependence of Heavy-Ion-Induced Reactions." Some of the considerations involved are indicated in Fig. 6. We know that the difference between the ${}^{16}\text{O}$ and ${}^{15}\text{N}$ ground states has to come from filling the $p_{1/2}$ orbit. Consequently, the initial orbit of the neutron has $\ell_1 = 1$ and total angular momentum $j_1 = 1/2$. I am going to consider the case where ℓ_2 is not equal to zero; that is typical of the final states in ${}^{209}\text{Pb}$. You would imagine this is a pretty good situation to pick because ${}^{16}\text{O}$ and ${}^{208}\text{Pb}$ are doubly magic, and the idea of a single initial orbit and a single final orbit is a good one. For the case (${}^{12}\text{C}, {}^{11}\text{B}$) we have a total angular momentum j_1 of $3/2$ and $\ell_1 = 1$. The significance is that, for light ions, if you look at the probability of exciting the final state with its spin up relative to its orbit compared with having its spin down, you get a rather small effect; the only place you get large effects is usually at some particularly large angle. This was called j-dependence in the old days by people doing light-ion scattering. In the heavy-ion case, if you look at the selection rules I talked about earlier for the no recoil approximation (Fig. 4), you find for (${}^{16}\text{O}, {}^{15}\text{N}$) that L must

EXACT FRDWBA

$$\phi^* V \phi = \sum_{\alpha\beta} K_{\alpha\beta}(r_I, r_F) Y_{\alpha}(\hat{r}_I) Y_{\beta}(\hat{r}_F)$$

$$\left. \begin{array}{l} \Delta r_I \\ \Delta r_F \end{array} \right\} \lesssim .1 F \quad R_{\text{MAX}} \sim 20 F$$

→ 40,000 pts in r_I, r_F

$$f_{L_I}(r_I) = \sum_{n_I} A_{n_I} j_{L_I}(n_I r_I) \quad 0 \leq r_I \leq R_M$$

FOURIER - BESSEL EXPANSION

$$j_L Y_{LM} \Rightarrow \int d\hat{k} Y_{LM}^*(\hat{k}) \underbrace{e^{i\vec{k} \cdot \vec{r}}}_{\text{EASY TRANSFORMATION OF CO-ORDS.}}$$

$N_I \sim 15 \rightarrow 200$ pts in momentum space.

Charlton - Robson code $\sim 20 \times$ faster (4x DWUCK)
(MERCURY)

j-dependence (Kovar et al, Oct '72 P.R.L)

$$\ell_1 = 1 \quad j_1 = \frac{1}{2} \quad \boxed{\ell_2 \neq 0}$$

$$j_2 = \ell_2 + \frac{1}{2} \quad L = \ell_2 + 1$$

$$j_2 = \ell_2 - \frac{1}{2} \quad L = \ell_2 - 1$$

$$\ell_1 = 1 \quad j_1 = \frac{3}{2} \quad \boxed{\ell_2 \neq 0}$$

$$\left. \begin{array}{l} j_2 = \ell_2 + \frac{1}{2} \\ j_2 = \ell_2 - \frac{1}{2} \end{array} \right\} L = \ell_2 \pm 1$$

DWBA $\sigma_{L+2} \approx 10 \sigma_L \rightarrow$ STRONG j-dependence in HI's
(NO RECOIL)

RESULT

$$\text{e.g. } \frac{\sigma(f_{\frac{1}{2}})}{\sigma(f_{\frac{5}{2}})} \sim 4$$

$$\frac{\sigma(f_{\frac{1}{2}})}{\sigma(f_{\frac{5}{2}})} \sim 2$$

EXACT DWBA ? $L = \ell_2$ also allowed in all cases

equal $\ell_2 + 1$ when $j_2 = \ell_2 + 1/2$ and $\ell_2 - 1$ when $j_2 = \ell_2 - 1/2$. The selection rules give you only these specific values; they are unique and they are different. The significance of that is that when you do the DWBA calculation, in this approximation at any rate, the cross section for $L = \ell_2 + 1$ is 10 times larger than the cross section for $L = \ell_2 - 1$. In the carbon case this is not true because with $j_1 = 3/2$ the selection rules are less restrictive; you have more slop in your vector addition, and consequently you can get both L values for both J values. Since you can have the same L 's for both j_2 values, you don't have the same selectivity that you have in the oxygen case. The result they find experimentally for the oxygen case is that the cross section ratio $\sigma(j_2 = \ell_2 + \frac{1}{2}) / \sigma(j_2 = \ell_2 - \frac{1}{2})$ is about 4, and it is relatively independent of the orbit into which you go. This "agrees" with what the DWBA calculations say. In the carbon case the same ratio is about 2. You can actually get a ratio other than 1 because the amounts of each L for a given j_2 are not necessarily the same, but you can have both L -values there, and therefore you don't get a factor of 10.

In the oxygen case the ratio of 4 is a little hard to explain in detail unless you go over to an exact distorted wave theory. The authors claim to have looked at this, but they don't have any calculations. I don't know how you do that, but in any event the general argument is fairly simple. If you allow that L can also be equal to ℓ_2 because in the exact finite range case you don't have a parity rule, the average distance between L transfers is no longer 2 units but one and a half. Consequently, this ratio will come down, but whether it comes down to 4 is a good question. You don't have quite as strong a selection rule as the calculations suggest, but it is quite different from light-ion spectroscopy, and I think it will be successful simply because we do have ratios of this type each at a different angle in the transfer. This is the first paper I've seen where there are some really sound arguments for using heavy ions to see what is happening and where there is a fairly clear cut argument about what is going on. In the past heavy ions have been nasty, not telling us very

much about the spectroscopy of any specific state.

Now I want to go over to the quartet problem, where we have some of this nastiness. I don't want to do what I fear the Saclay group does, which is to tell you what they believe rather than what they know. I used to work with a man in England by the name of Tony Lane; I was a young theorist then, in fact, it was about 7 years ago. Young theorists have a natural attitude of believing everything they do, and so naturally I believed everything I did. One day he stopped me short when he said, "I know you believe it, but do you know it?" Indeed, it is very hard to get theorists these days to tell the difference between what they believe and what they know, and there is a vast difference, I can assure you. The Saclay group has, perhaps, fallen into this error. They have a very brilliant theorist named Gillet who has predicted quartet structure in nuclei. I explained a little about this in the first lecture. What you think you can do is have some sort of single-particle energy-level diagram (Let's make it self-conjugate so we have the same numbers of neutrons and protons.), and you fill out the lowest levels.

Then a quartet state is a matter of putting four particles into the next higher levels, as shown. It is that simple. These states have $S = 0$ and $T = 0$.

Fig. 7 shows some experiments from which we can hope to extract facts about quartet states. You can see that I have a lot of experiments, so I can only spend a couple of minutes on each one. The initial experiment, or rather the initial type of experiment

since there were actually several targets used, is shown on line 1 of Fig. 7. This is the favorite experiment that has been used by the Saclay group⁸⁾ to predict the existence of quartet states. The data for this experiment are shown in Fig. 8. Note that you hardly see the ground state of ^{58}Ni . There are some highly populated states of ^{58}Ni at about 6 MeV of excitation in the residual nucleus. The quartet states should be between 3 and 8 MeV, so it looks awfully good; you do see such structure between 3 MeV. The point is you have to get two protons excited to the same level with the last two neutrons that are with the neutron excess in nickel before you can really start putting four nucleons together. That takes an energy of between 3 and 8 MeV, depending on which orbit you put them into. There is more than one orbit you can put them into, in general.

Another experiment I can show that has been done very recently is $^{54}\text{Fe}(^7\text{Li}, t)^{58}\text{Ni}$.^{*9)} Data for this experiment are shown at the bottom of Fig. 8. The data are for quite low energies you will notice, 16 MeV, which was necessary in order to make outgoing tritium match momentum-wise relative to its Coulomb barrier with the incident ^7Li . You have to go down towards the Coulomb barrier in order to get these two to match initially and finally in the orbits and get a reasonable cross section. That is the argument. I don't know if it is true, but indeed experimentally the energy must be dropped way down in order to see this sort of spectrum. Again you see a few states between 3 and 8 MeV, which might be quartet states. So the argument is this is really going by an alpha transfer mechanism, and everybody is happy.

About a year and a half ago I was concerned about this, because I had been told that the probability that oxygen would look like a ^{12}C ground state plus an alpha particle was very small. Why should ^{16}O like to transfer an alpha particle to ^{54}Fe , leaving ^{12}C behind, if this probability is not large? Indeed, what is predicted is that ^{16}O likes to look like the first excited state of ^{12}C plus an alpha particle. Therefore, we might expect to see the reaction of line 2 in Fig. 7, namely that an alpha particle in ^{16}O

is captured by ^{54}Fe , but it doesn't come from the situation where the core is ^{12}C in a ground state; the core is ^{12}C in its first excited state. You expect to observe these in your spectrum in the form of the whole nickel spectrum shifted by 4.43 MeV, which is the excitation of the ^{12}C excited state. The problem is that you don't see this with any magnitude, certainly not in this experiment. You can tell the difference between $^{12}\text{C}^*$ and ^{12}C , believe me. The reason you can tell the difference is because when $^{12}\text{C}^*$ gamma emits, it is Doppler shifted, depending on which direction the gamma is emitted. So you spread your peaks by about 300 kilovolts, which should be observable with the resolution used, ~ 300 kilovolts. You should see some peaks twice as wide as those moved up by 4.43 MeV. But in hardly any of the data for ($^{16}\text{O}, ^{12}\text{C}$) reactions do you see any $^{12}\text{C}^*$ production.

Two things were argued by the Saclay people concerning this result. First of all, for a ^{12}C core the alpha particle moves around in ^{16}O with $\ell = 0$, and an $\ell = 0$ state has a long tail because naturally it doesn't have an angular momentum barrier. Moreover, the binding energy of the alpha particle relative to ^{12}C is more than it is relative to the first excited state of ^{12}C , and again it tends to make the tail longer. The argument is if you have a long tail, you can go better than if you have a short tail. Well, that at least sounded like an explanation of the effect. The other argument was that it isn't really an alpha particle. What is wrong with that argument is that it is an exactly opposite argument, because if it isn't an alpha particle it has to be an excited state of the alpha particle, 20 MeV away. The tail of that one would be extremely short, but you at least have two guns in case one fails.

One of them did in fact fail recently.¹⁰⁾ The experiment is line 3 of Fig. 7. Here $^{12}\text{C}^*$ is seen quite prolifically, more often than the ground state. Consequently, this says sometimes it goes. You can't really argue then that it can't go in the earlier case just due to its tail.

I have already mentioned the experiment shown on line 4. Here it is pretty hard to imagine that ^7Li has a large component

QUARTETS - FACTS

of an alpha particle in an excited state. If a quartet state is an alpha particle that doesn't have relative s-wave motion, then why do you see the same states here as you do for the reaction of line 1? It is a little hard to believe. There are certainly some contradictions to the idea that a quartet isn't necessarily an alpha particle, because you tend to see the same thing for reactions 1 and 4. You have to realize when you say you see the same thing that the resolution is 250 kilovolts, and the level spacing is more like 10 to 20 kilovolts. Consequently, you don't know which state is which anymore. All you know is that both of these reactions are selective; whether they produce the same states of $^{58}\text{Ni}^*$ or not isn't entirely clear.

Well, some answers to that have come up recently. The experiment of line 5 in Fig. 7, which is rather damaging to the quartet state idea, was done at Saclay,¹¹⁾ but it was done under the influence of George Morrison, who comes from Argonne where the prevalent feelings are that these are not quartet states. Some data are shown at the top of Fig. 8. You see many states, but the point they brought out was that if you really start lining up the states that are strong in the inelastic alpha scattering with those that you see in the transfer reaction, the correlation is as good as it was between the ($^7\text{Li}, t$) and the ($^{16}\text{O}, ^{12}\text{C}$) reactions. There you start to get into difficulties, because the states that are seen in both reactions are 3^- states, and to make a 3^- state when you bombard a 0^+ target involves putting particles into orbits that aren't in a quartet configuration. So, the evidence here is that you are exciting states apparently in reaction 1 as well as reaction 5 that are not typical of a quartet structure.

Why 3^- states needs to be answered. The reason why the 3^- states are important in the (α, α') reaction is simply because they are in the vibrational region, and there is a collective octupole state in the region that is easily excited in elastic scattering. Not only that, the collective octupole is somewhat fractionated over several states so that you see a number of 3^- states; at least that is the explanation put forth.

Well, now let's look at some more Saclay data that also suggests that life is not easy.¹²⁾ Reaction 6 in Fig. 7 is the same reaction as line 1, but the experiment was done by looking at the gamma ray spectrum in coincidence with particular ^{12}C groups. In this way they found that the state seen at about 4.5 MeV excitation is a 3^- state. There is a state at 3.5 MeV that is 0^+ . All they can tell you about the remaining states is that there are many states excited in this reaction under each one of the peaks shown in Fig. 8. With the gamma rays you can see that because you have high resolution, and you can see the splittings. So they are not single states, and the Saclay group has said well, maybe they shouldn't be because these states have to mix with the surrounding states. The next question to come up, of course, is to ask whether the states have the same spin and parity, which would be typical of a quartet state that dissolved into its neighboring states, or whether they all have different spins and parities and there is really nothing selective about the reaction. That is not known.

Another type of reaction considered by the Saclay group involves, for example, calcium isotopes bombarded with oxygen and carbon going to titanium. (Line 7 in Fig. 7). In this case what they said is that when you excite states in ^{40}Ca you have no blocking effects because there is no interference from any excess neutrons. What they found when they did this reaction was that the selectivity on the number of strong states went down as they went up in the mass number of the target.¹³⁾ Correspondingly, they are simply adding more and more excess neutrons, which impedes the formation of quartet states. At least that was the argument. However, the reaction of line 8 has recently been studied by an Argonne, Rochester combination.¹⁴⁾ This reaction also provides a good alpha transfer mechanism. They are doing just 40 and 42, and 42 looks very similar to 40, so they do not seem to see any blocking in this case, at least nowhere near the amount that was seen for the reaction of line 7. So one of the main arguments used by the Saclay group to describe the way in which quartets exist in nuclear isotopes seems to have in this case some difficulty or contradiction experimentally.

In Tallahassee we have been pushing an alternative idea, and that is to do $^{58}\text{Ni} + ^{12}\text{C}$ going to $^8\text{Be} + ^{62}\text{Zn}^*$. (Line 9 in Fig. 7) The ^8Be breaks up; it is an unstable nucleus. The only way it can break up is into two alpha particles, so what is done experimentally is simply to put two detectors very close to each other—you typically use about 3° between them—and ask for coincidence between two alpha particles with the same energy or within a certain energy range. It is a very easy experiment because you don't have any problems with the first excited state of ^8Be , which turns out to break up in such a way that the alpha particles have a relatively large angle between their directions of motion. You put two more detectors at large angles and you can detect part of the excited ^8Be way out where there aren't any ground states.

The reason why I was interested in pushing this is because the experiments given on line 10 of Fig. 7 existed. If you look at the cross sections, they say that alpha pick-up out of ^{12}C leaving behind ^8Be is somewhat more likely than alpha pick-up out of ^{16}O . Therefore, this might be a better way of looking for alpha particle transfer than $(^{16}\text{O}, ^{12}\text{C})$. The $^{58}\text{Ni}(^{12}\text{C}, ^8\text{Be})^{62}\text{Zn}^*$ reaction has been done,¹⁵⁾ and one sees fewer states than one sees for reaction 1. Nevertheless, there are a few strong states. What is intriguing, however, is the cross section for this is 20 times smaller than the cross section for reaction 1, which is the $(^{16}\text{O}, ^{12}\text{C})$. This is the wrong way around if the ideas we were just developing about alpha transfer are right, because ^{12}C has an alpha probability that is larger than ^{16}O . I don't think any direct reaction theory is going to be able to explain that factor of 20 because the binding energies are so similar; you're stuck. The experiment exists for $^{58}\text{Ni}(^{16}\text{O}, ^{12}\text{C})^{62}\text{Zn}^*$ also, with the same target and final state. The only difference is $(^{12}\text{C}, ^8\text{Be})$ versus $(^{16}\text{O}, ^{12}\text{C})$. That ratio is still 20, and there doesn't seem to be any explanation at present.¹⁶⁾

We believe $(^{12}\text{C}, ^8\text{Be})$ is a direct reaction because it has good parentage. We are not so sure that $(^{16}\text{O}, ^{12}\text{C})$ could ever be a direct reaction; I would suggest it is not. It is going by some

other process. I do have a suggestion, which I regard as a complete speculation. There are two facts that seem to be emerging. One is (^{16}O , ^{12}C) likes to go, and secondly the states that it likes to excite are octupole states of the residual nucleus. These are not real facts, they are possible facts, because, as I said, nobody has done experiments with high enough resolution to really identify the spin and parity of the states. The suggestion I have is that this reaction goes by some process that carbon can't do so easily. That is, first of all, it goes to ^{16}O in the 3^- state, which it happens to have so nicely sitting there, and then $^{16}\text{O}_{3^-}$ transfers its octupole vibration over to the target via the alpha particle. The alpha particle is the main reason for the 3^- vibration. If you look at the overlap of the two nuclei and you draw your peak-shaped nucleus, the larger part will be ^{12}C and the smaller part will be the alpha particle:

Such a deformation might well be a good one; all you have to do is have an octupole vibration in ^{16}O and then look for an octupole vibration in A. They should be nicely matched. If that is true, I'll be amazed, but it is as good as any other explanation I've seen.

FOOTNOTES

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Dr. Frans Saris

Dr. Saris, of the F.O.M. Institute of Atomic and Molecular Physics in Amsterdam, is noted for his experimental work and interpretation of x rays from quasi-molecular systems formed in low-energy ion-atom collisions.

ION-INDUCED X RAYS, THE MOLECULAR ASPECT*

A renaissance has taken place in the field of x rays since ions are used as projectiles to generate x rays rather than electrons or photons. In this series of seminars I would like to display what this renaissance is all about. The first talk will be concerned mainly with the molecular aspect of heavy-ion-atom collisions. In the second talk we will discuss so-called solid-state effects and in the third, a new phenomenon, the molecular x ray will be introduced. The final talk will review the analytical use of ion-induced x rays.

The field of ion-induced x rays is still rather young, yet Coates¹⁾ made the first observation in 1934. Unfortunately, his work was completely ignored by other laboratories for something like 34 years, and ion-induced x-ray emission, particularly heavy-ion induced x-ray emission, had to be rediscovered in something like 1968-69. It came out of a completely different field than that in which Coates was interested. Its rediscovery was initiated in two places, in Connecticut by the group of Everhart, and in Leningrad by the group led by Fedorenko, for reviews see ref. 2,3. These people were studying inelastic ion-atom collisions. What I mean by that is you have an ion coming in, say an Ar ion of kinetic energy E_1 , and it collides with say an Ar atom. The particles leaving the collision site have kinetic energies E_1' and E_2' .

$$E_1 = E_1' + E_2' + Q$$

It is observed that there is kinetic energy transferred into excitation and ionization of the colliding particles, and that is inelastic energy loss. What you need for this experiment is, for instance, to measure the scattering angle and the kinetic energy of the scattered projectile. You then calculate the value of the kinetic energy for a given scattering angle assuming an elastic collision. The difference between the calculated value and the measured value is the inelastic energy loss. If you plot that as a function of minimum internuclear distance (Since you have measured the scattering angle you can apply a screened Coulomb potential and calculate the minimum internuclear distance.), you obtain a curve like that shown in Fig. 1.⁴⁾

Fig. 1

What you see in Fig. 1 is the inelastic energy loss in an $Ar^+ - Ar$ collision plotted as a function of the minimum internuclear distance. The striking thing is that at a well-defined internuclear distance of $0.25 \text{ \AA}$ there is a drastic increase in

inelastic energy loss. The energy loss is only 50 eV for a large range of internuclear distances, then suddenly it rises to more than 600 electron volts. There is a plateau, and then it increases further with decreasing r_0 . It was this experiment particularly that triggered the great interest in the field that we have today. People speculated that these results could be explained by inner-shell excitation occurring for relatively low energy ion-atom collisions, typically a few keV to 100 keV laboratory projectile energies. Because the energy difference of 500 eV is twice the binding energy of an Ar L-shell electron, the speculation was that there are two Ar L-shell electrons removed, one from each of the two particles or two from one of the particles. In fact, systematic studies showed another plateau,⁵⁾ so there was one step of 250 eV and then another step of 250 eV, which gave more evidence for this speculation.

It was suggested that in order to prove that these Ar L-shell excitations occurred, one should search for Auger electrons. If you have an L-shell vacancy produced, this vacancy can decay in two ways, radiative decay or radiationless decay where the energy is emitted in the form of an ejected electron. Indeed, a number of people have observed these Ar L-shell Auger electrons.⁶⁾

At about this time we wondered whether one could also observe the radiative part of the decay, that is, whether we could observe x rays emitted in these ion-atom collisions, in this case Ar L-shell x rays. Experiments to look for these x rays are extremely simple. A block diagram of our apparatus is shown in Fig. 2. All you have to do is fire a beam into a gas cell and

Fig. 2

look at the scattering region with either a proportional counter or a lithium-drifted silicon detector. You can integrate the total beam by measuring the beam current with a Faraday cup. The proportional counter gives you a signal with height proportional to the energy of the emitted x ray. This is analyzed in the pulse-height analyzer. You can control the gate of the scaler with the current integrator, and thus you can do an absolute x-ray emission measurement with such a simple device. The beam enters the gas cell through a slit that is differentially pumped. In the system we use we have differentially pumped all the way down to the scattering center, which is very important in order to keep the charge state that you want.

As I said, this is a very simple way to measure absolute cross sections. I think it is mainly for this reason that the x-ray field has emerged so rapidly. We can study total x-ray emission cross sections in an experiment that is much simpler than inelastic energy loss measurements or Auger electron emission measurements. A typical result for x-ray emission cross sections is shown in Fig. 3.⁷⁾ Here is plotted the Ar L-shell x-ray

Fig. 3

emission cross section versus relative energy of the colliding

particles for a whole set of ions fired onto an Ar target. This is plotted in the center-of-mass system; the energy is relative energy. You see that we indeed observe the Ar L-shell x rays in Ar-Ar collisions. But the most striking thing, which I want to get across here, is the big difference between heavy ions and low mass ions like protons and helium. The heavy ions, even at very low energies like 10 to 30 keV, show cross sections of the order of 10^{-19} to 10^{-20} cm², whereas with proton and helium projectiles we are still down by orders of magnitude in cross section at much higher energies.

I would like to briefly summarize the conclusions to which we came in measuring these x-ray emission cross sections and report what progress we made in understanding ion-atom collisions. So conclusion number 1 was that there is a big difference in excitation or ionization mechanism between heavy ions and protons. This is seen not only for Ar targets and not only for gas targets, but people have observed it in solid targets as well.

Fig. 4 shows results obtained by the Livermore group for

Fig. 4

protons and heavy ions fired onto carbon.⁸⁾ They observe exactly the same thing; the carbon K-shell x-ray emission cross section or ionization cross section for protons is much lower, even at high energies, than the heavy-ion induced x-ray emission cross sections are.

Let's go to the second conclusion, which becomes very apparent when we plot these data a little differently. For a given energy we draw lines through these curves and plot the cross section versus Z number. In Fig. 5^{7,9)} you see the Ar L-shell cross section

Fig. 5

plotted against the Z number of the projectile for a set of energies. There are resonances occurring in this plot, which is conclusion number 2. The resonances occur at those Z numbers where the binding energies of the two colliding particles match. Thus, there is a maximum near $Z = 18$ (Ar on Ar) so the binding energies of the two Ar L-shells match; there is a maximum near $Z = 6, 7$ —carbon, nitrogen—where the binding energy of the carbon K-shell matches with the Ar L-shell binding energy.

This has been seen for different targets. Fig. 6 shows the Livermore data for Cu L-shell x-ray cross section versus Z number when one fires different ions onto a Cu target.¹⁰⁾ Again you see two maxima occurring roughly at positions where there is

Fig. 6

matching of energy levels. One maximum is at $Z = 29$, Cu on Cu, and the other is at $Z = 10$, Ne, where the binding energy of the Ne K-shell matches the binding energy of the Cu L-shell. These resonances also occur when you reverse the collision and fire Cu ions onto different targets. Fig. 7 again shows data obtained by

Fig. 7

the Livermore group.¹⁰⁾ Here we see the maximum for Cu on Cu and another maximum when the target M shell matches the binding energy of the projectile L shell, near $Z = 60$.

Question: Are you sampling a wide enough range of energies to be sure that you are not just velocity matching? Do you have a matching of the velocity of the ion with the velocity of an electron in the shell?

Answer: No, we are way short of a matching of velocities. I think I will come to that soon, but the proton and helium induced ionization does occur when there is a matching of velocity. It is Coulomb interaction, and you saw that we need proton velocities corresponding to energies of several hundred keV in order to get into the region where we have an equal velocity of the orbiting electron and the incoming projectile. Here we have heavy ions in the keV region, so the velocities are orders of magnitude smaller than the orbiting velocities of the electrons.

These results are not peculiar to the low energy region; they also hold for higher energy. They were, in fact, first observed by a German nuclear physicist, Hans Specht, in 1965.¹¹⁾ This work was ignored in the field of ion-atom collisions, first because it was published in German, and second because it was published by a nuclear physicist. Some results reported by Specht are shown in Fig. 8. He observed x-ray emission cross sections for fission fragments colliding with different targets. You have light and heavy fission fragments with energies ranging from 40 to 80 MeV, and you see that there occur collisions in the cross sections that correspond to a matching of the binding energies of the K and L shells of the projectile and the target atom.

I don't have a slide to illustrate this, but the third conclusion is that when you have a collision between particles of unequal Z number, the lower Z inner shell is excited with greater probability than the higher Z . In other words, as the cross section goes down for Ar L with decreasing Z number, the cross section

Fig. 8

goes up for projectile L x-ray emission. I don't have a slide to show you this, but we will see some more data later to give more evidence for this. Thus, the third conclusion is that lower Z is more likely to be excited.

There is a fourth conclusion. All the x-ray lines we observe induced by heavy ions are greatly shifted compared to the lines produced by protons or helium. I think this was first recognized by Pat Richard,¹²⁾ but since I think he will be talking about his work here, I won't show his slides, but rather those of this competitors. Fig. 9 shows results obtained by the Naval Research group.¹³⁾ They fired protons and nitrogen ions onto Al and observed the x rays produced using a crystal spectrometer. You can see that for protons the K α emission is confined to just one line

Fig. 9

plus one satellite line, but if you fire a heavy ion onto the Al you see a wealth of satellite lines. These satellites can all be identified as being due to transitions to the Al K-shell in Al ions that have been ionized one time, two times, three times, four times or five times in the L shell. This shows that with the nitrogen projectiles you are more efficient in ionizing the outer shell than you are with the protons, so the existence of multiple ionization with heavy-ion projectiles is our fourth conclusion.

I have described the four main experimental conclusions to which we came in the past three years of measuring these total x-ray emission cross sections and x-ray spectra. The question now is, how can we explain these results? Is there a model that is able to handle all four of these features? The answer came in 1965 already when Lichten and Fano¹⁴⁾ proposed their molecular, electron-promotion model, which was later extended by Lichten and Barst¹⁵⁾ in 1972. Let us try to visualize what we mean by the molecular model with the aid of Fig. 10. The electrons on the Ar ion and

Fig. 10

the Ar atom don't see one center during the collision; they see two centers. They therefore occupy molecular orbitals rather than atomic orbitals. As the particles move apart, the electrons revert to atomic orbitals, but not necessarily the lowest atomic orbitals. They can be in higher orbitals, which means their orbiting velocity is slower than the velocity with which the particles are emerging. We are left with two highly excited particles that then deexcite by emitting fast electrons in an Auger process or x rays in a radiative decay.

The next question is whether we know something about the orbitals the electrons are in during the collision. I am going to construct for you a correlation diagram that is well known in molecular physics. To begin with, we know the energy levels of the electrons at infinite internuclear distance. Then the energy levels are the energy levels of the separated atoms, so we have the 1s state of atom A and the 1s state of atom B, the 2s and 2p states of atom A and the 2s and 2p states of atom B, and so forth. Let us assume that A is heavier than B so the binding energies in atom A are larger. We also know that if we could push hard enough to have these two atoms form a single atom at zero internuclear distance,

we would have the united atom C, and we know the energy levels of atom C. Some of these levels are shown schematically in Fig. 11.

Fig. 11

We now ask, how can we correlate the energy levels of the separated atoms with the energy levels in the united atom? There are rules that have been worked out by Barut and Lichten.¹⁵⁾ The main argument is that in these quasi-adiabatic collisions the wave functions of the electrons are not allowed to change considerably; the collision goes too fast. What that means in quantum mechanics is that the number of nodal surfaces is not changing. Thus, the radial quantum number, which is the number of ellipsoidal nodal surfaces, must be the same for the united and the separated atoms.¹⁵⁾ In terms of the principal and azimuthal quantum numbers this says that $n-l-1$ is conserved. This is a very simple rule to apply. The quantity $n-l-1$ for a 1s state is zero, so what the rule says is that we should correlate the two lowest 1s levels in the diagram, as we have done in Fig. 11. We can proceed in this way to build up the correlation diagram from the bottom. For a 2s state, $n-l-1$ is one, so we cannot correlate the next two lowest states. However, for a 2p state $n-l-1 = 2-1-1 = 0$, so it is the

united atom 2p state that correlates with the second 1s state of the separated atoms. The 2s state of the united atom is correlated with the lowest 2s state of the separated atoms. For the lowest separated atom 2p state, $n-l-1$ is zero; this cannot correlate with the united atom 3s state because that state has $n-l-1 = 2$, nor with the united atom 3p state, which has $n-l-1 = 1$. The lowest united atom state with which we can correlate is the 3d state, which has $n-l-1 = 0$.

This is a sketch of how one can develop the correlation of levels as a function of internuclear distance going from separated atoms to the united atom. I don't want to work this out in detail any further, but I have a few slides to show you the final results. Fig. 12 shows a fully worked out correlation diagram.¹⁵⁾

Fig. 12

The symbols σ , π , δ , etc. represent respectively 1, 2, 3, etc.

planar nodal surfaces passing through the internuclear axis. This number should also be conserved in the correlation.

There are two very important things to realize. One is that promotion occurs in this correlation diagram. For instance, the principal quantum number of the 2s state of atom B is promoted from 2 to 3 in the united atom limit because it is correlated with the 3p state of the united atom. The principal quantum number can be promoted by more than 1 unit. For example, the 2p σ state of atom B is correlated with the 4f state of the united atom. The second thing that is important to realize is that there are crossings in the correlation diagram. That is the main difference from correlation diagrams in ordinary molecular physics where one is concerned with adiabatic states. There one has avoided crossings; that is, the collision is so slow that you can do the calculation in a very static way. It is found that two different σ states (and correspondingly for π , δ , etc.) cannot intersect as the internuclear distance is changed.¹⁶⁾

Now let's try to explain the four experimental conclusions we listed earlier in terms of such correlation diagrams. Conclusion three, that the lower Z particle is more likely to be excited than the higher Z particle, is very easy to understand. If atom A is heavier than atom B, then the 2p state of atom B is correlated with 4f state of the united atom, so the promotion is larger, from 2 to 4, than it is for the 2p state of atom A. There the correlation is to the 3d shell, and the promotion is only from 2 to 3. The difference it makes is that there are many more crossings occurring in the first case, and that means the electron can find many more exit paths as the collision proceeds and the particles go apart. This is visualized in Fig. 13 for a copper-argon collision.¹⁵⁾ The united atom in this case is silver. I don't have a slide showing this, but the cross section for argon L production is larger than the cross section for copper L production. That is because the 2p state of argon is correlated with the 4f united atom state. Of course many crossings occur where the electron cannot change levels; that is a point that is important. We have to have vacancies in the outer orbitals into which to trap

Fig. 13

the inner shell electron. For instance, the 2p electron of copper cannot go into the 3s shell, even though the levels cross, because the 3s shell in copper is full. In fact, the only way this electron can exit is in the 3d level of copper, and that is also filled in the ground state configuration.

Question: If you are going fast enough that level crossings are allowed, why aren't you going fast enough to destroy the whole meaning of the correlation diagram; how does the electron know that it is supposed to be correlated?

Answer: Because the nuclear velocity is still smaller than the orbiting velocity of the electrons involved. Yet we cannot ignore the nuclear velocity. You will find a detailed

answer to your question in Barat-Lichten.¹⁵⁾

Question: The promotion argument, as I understand it, assumes that the collision essentially reaches $R = 0$. Is this correct?

Answer: No, during a collision the internuclear distance has to become small enough to reach a crossing of two MO's. This critical internuclear distance is roughly equal to the radius of the inner-shell involved. We will come to that soon.

Fig. 14 shows the correlation diagram for aluminum-argon.¹⁵⁾

Fig. 14

The aluminum projectile is lighter than argon, and it is now the $2p$ state of aluminum that correlates with the $4f$ state of the united atom. There are many more crossings occurring for that orbital than for the $2p$ level in argon, so the cross section for argon excitation is down; we are in a valley on the curve of cross section versus Z number. That can be easily understood from the

correlation diagram. The only way you can get the 2p electron of argon out is in the orbital dotted on the diagram. The first crossing wouldn't help us because the 2s state in aluminum is filled. Also the 3s state in argon and the 2p state in aluminum are filled. We have to promote the electron up to the 3d united atom level, which needs, first, a very penetrating collision to get to almost zero internuclear distance and second, high velocity to obtain reasonable rotational coupling between the 3d σ state and the 3d δ state. This explains why aluminum L x rays are observed much more abundantly than argon L x rays.

When we go to lower projectile Z number, for instance carbon on argon as shown in Fig. 15,¹⁵⁾ we see that there is a

Fig. 15

matching of the lower levels. When we go to lower Z number, we have fewer electrons in the outer shell. For instance, in carbon even the 2p shell is not entirely filled. There are 4 vacancies in the carbon 2p shell, and that means we can put an argon 2p

electron into the 2p shell of carbon during the collision. With an aluminum projectile the argon 2p electron had to get out in the aluminum 3d level. Thus, as we decrease Z from aluminum to carbon our cross section for argon L x-ray production goes up again.

Now consider what happens when we go one Z number lower than carbon. Then the projectile 1s level becomes higher than the 2p level of argon. Then the correlation diagram is quite different. The next Z number is boron. It is the 1s level of boron that is correlated with the united atom 3d shell; the 2p shell of argon is correlated directly with the 2p shell of the united atom. There are no crossings; there is no excitation. Such a correlation diagram is shown in Fig. 16.¹⁵⁾ Again, the point of importance here

Fig. 16

is that the 2p shell of argon is correlated with the 2p shell of the united atom, and it is the 1s level of the projectile that

is correlated with the united atom 3d shell. There are no crossings occurring for the argon 2p, and there is no excitation. Thus, the molecular model does not predict argon L x-ray production for boron on argon or lithium on argon or helium on argon or protons on argon. That explains why we have to have a different excitation mechanism with protons and alphas in order to produce vacancies in the L shell. The mechanism is Coulomb excitation, a direct interaction of the incoming nucleus and the 2p shell in the target. We have also indicated an explanation for the resonances shown in Fig. 5. When we go from aluminum to carbon we find that the cross section increases abruptly and then goes down very low as we go beyond carbon. This is simply because we get swapping of level order in the correlation diagram. The same thing occurs as we go from aluminum to chlorine. We get a swapping of level order in the correlation diagram again. It is the 2p of argon that goes to the united atom 4f_o, and that is where the maximum comes from. As we continue to increase Z, it becomes less and less probable to produce a vacancy in the argon L shell because there are more and more electrons in the projectile outer shells, giving fewer exit channels.

If this molecular model explains inner shell excitation, then it is very likely that the process involved is even more important for outer shell excitation, and more efficient too. There are many, many crossings in the outer orbitals where electrons can get trapped, and therefore it is very likely that multiple ionization occurs with heavy ions.

Question: The outer levels will be so broad that you will essentially have a continuum. How can the molecular model apply at all?

Answer: For outer shells, you mean? It depends very much on what outer shells you are talking about and at what velocities. If you consider neon on carbon at a few keV (binding energies of 10 or 15 eV), the energy uncertainties at these low energies are small and you can have well defined states there. But once you

are up to a few hundred keV projectile energy, I agree that the levels are all smeared out; it is essentially continuous.

What I have given were all handwaving, qualitative arguments. The field is now going into more quantitative calculations of cross sections based on this molecular model. One such calculation that I would like to mention is for the Ar-Ar system. For the Ar-Ar system, Lichten^{14b)} has constructed not just a molecular orbital correlation diagram, but a molecular orbital energy level diagram. This is shown in Fig. 17. Here we don't just have

Fig. 17

straight lines between the separated atoms and united atom limits. Rather, it has been estimated how the molecular orbital actually change energy as a function of internuclear distance. Thus, now the horizontal axis has a real physical meaning. There is a very interesting point; the 4fσ orbital goes up very steeply and goes through a great many crossings in the extremely small range of internuclear distances from 0.23 to 0.25 Å. This means that the

probability for ionization of the 2p shell as a function of inter-nuclear distance, is almost a step function. We can assume, since there are so many level crossings here, that the probability for excitation of the 2p shell as a function of distance of closest approach is essentially unity for $r_0 < 0.23 \text{ \AA}$ and is essentially zero for $r_0 > 0.23 \text{ \AA}$. Using a screened Coulomb potential to relate the impact parameter to the distance of closest approach, we can use this excitation probability to calculate the cross section for x-ray emission as a function of energy.¹⁷⁾ The result is shown in Fig. 18. Here you see our experimentally determined cross sections

Fig. 18

for Ar L x-ray emission as a function of energy and the results of calculations for three different distances of closest approach. I would like to draw your attention to the threshold part of this excitation curve. You can see that it is $r_0 = 0.23 \text{ \AA}$ that nicely fits the threshold region, whereas the curves for $r_0 = 0.21 \text{ \AA}$ and $r_0 = 0.25 \text{ \AA}$ are off by more than an order of magnitude. At higher energies the calculated cross sections are lower than the measured data. We will come to some possible reasons for that in a short while.

Question: Fig. 17 shows many crossings for the 4f_o level, but it appears that only two of the crossings would be effective for what we want. Do the crossings with n=3 levels make any contribution to the excitation probability?

Answer: Since you have an ionized projectile, there is a vacancy in the projectile n=3 shell.

Question: If you have a single vacancy in the projectile n=3 shell, is there any prediction from this model that you can test as to whether the x ray will come from the projectile or the target?

Answer: We have to do differential cross section measurements. I think it is about time that we started to do that, but it hasn't been done yet. People have measured the probability for Ne K ionization in collisions of neon⁺ with neon and of neon⁺⁺ with neon,¹⁸⁾ and the probability went up by a factor of 2 for the doubly charged projectile because there are two vacancies rather than 1; there is twice the probability for the electron to get trapped in that shell.

We can reverse the whole line of reasoning. That is, we assume that the probability is dependent only on the distance of closest approach, and from the thresholds that have been measured for projectiles of different Z on Ar we determine the critical internuclear distance for the ionization of the argon L shell. This is shown in Fig. 19, where we have plotted the critical internuclear distance derived from the thresholds of the excitation functions versus the Z number of the projectiles that were fired onto Ar targets.⁷⁾ Points from our x-ray measurements are shown by closed, dark circles. For copper (Z=29) the critical internuclear distance is something like 0.16 Å. For iron (Z=26) it is larger, and for titanium (Z=22) it is even larger. For argon (Z=18) it increases still further. Why is that? I have tried to illustrate the geometrical effect of shell sizes by plotting the sum of the radii of maximum charge density for the 2p electrons of argon and the 2p or 1s electrons of the projectile. The data suggest that for $Z \geq 18$ excitation of the Ar L-shell occurs via

Fig. 19

interaction of the L-shells of the projectile and target atom, while for $6 \leq Z < 18$ the excitation is dominated by a K-L overlap. The 2p shell of copper has a smaller radius than the 2p shell of argon has, so you need not go in so deep to get this 4fs promotion in the argon case as you have to do in the copper case. The points represented by vertical bars are data from inelastic energy loss measurements done by Fastrup and Hermann.¹⁹⁾ They represent the "active r_0 region" where structure is observed in the inelastic energy-loss distribution. They are able to attribute the structure in the inelastic energy loss to excitation of the one of the two colliding particles that has the smaller Z. Thus, for $Z < 18$ the inelastic energy loss data reflect L-shell excitation of the projectile. We see that the trend of the geometrical factor $r_{2pZ} + r_{2pAr}$ is nicely followed; these two lines are almost parallel.

If you decrease the internuclear distance further you can get the rotational coupling of the 3d level that was mentioned earlier. The Ar 2p level is correlated with the 3d of the united atom. (See Fig. 14.) When you fire aluminum onto argon, at large

internuclear distances there is a high probability for aluminum L vacancy production. As you go down in internuclear distance, argon L excitation comes in too. But in this case r_c doesn't follow the shape of the geometrical factor so nicely; the trend of the data is not parallel to $r_{1sZ} + r_{2pAr}$. Apparently there is something else happening here; apparently our model breaks down. What is most likely is that the excitation probability as a function of minimum internuclear distance is not a step function. The probability is also velocity dependent, which is largely because we are talking about a rotational coupling to open the $3d\sigma-3d\pi-3d\delta$ channel. This can only occur if the velocity of our collision is high enough.

This is illustrated in Fig. 20 where cross section data

Fig. 20

for Ar L are plotted as a function of projectile Z for various relative velocities.¹⁵⁾ You see that in the region around Ar-Ar collisions the lines are very close; increasing the velocity doesn't increase the cross section very much. On the other hand, for smaller Z we increase the cross section by orders of magnitude as

we increase the velocity. This velocity dependence is likely due to the rotational coupling that is important in the ionization process. This rotational coupling influence on the excitation probability has recently been worked out further by Briggs and Macek,²⁰⁾ based upon the work by Bates²¹⁾ for the hydrogen-hydrogen system. Larkins²²⁾ has done a Hartree-Fock calculation of the correlation diagram for the neon-neon system (Fig. 21). Production

Fig. 21

of a K-shell vacancy requires, according to the promotion model, coupling between the $2p\sigma$ and $2p\pi$ molecular orbitals. (Note the $2p\pi$ orbital is 1π in Larkins's notation, the first σ orbital. There are other notational differences.) This coupling has been worked out from first principles by Bates for the hydrogen-hydrogen system, and what Briggs and Macek have done is show that this can be very easily scaled up to a neon-neon system. The total cross section for K-shell vacancy production that they calculate is shown in Fig. 22. The data points in this figure are absolute Auger emission cross sections measured by Cacak *et al.*²³⁾ One could also plot x-ray emission cross sections; they would follow the same trend.

Fig. 22

A comparison of Auger cross sections and x-ray emission cross sections gives the fluorescence yields, that is, the relative probability that a vacancy is filled by radiative as opposed to radiationless decay. Before concluding, I would like to spend a few words on this fluorescence yield because some interesting things have come up. In Fig. 23 x-ray emission cross sections¹⁷⁾ and Auger emission cross sections²³⁾ are compared. The x-ray emission cross sections are indicated by crosses, the Auger cross sections are indicated by circles. These emission cross sections are normalized at 50 keV. For the argon case, the Auger cross section curve is a straight line; it follows the theoretical calculation. The x-ray cross section deviates by a factor of 2 at 100 keV. The same thing seems to happen in the neon case, although the error bars in the Auger data are $\sim 100\%$.

What are really plotted here as a function of energy are the Auger cross sections and the x-ray cross sections multiplied by one over the fluorescence yield as determined at the one normalized point. Since the fluorescence yield is something on the order of 10^{-4} , the Auger cross section is essentially the ionization

Fig. 23

cross section.

We suggest that the difference here is due to multiple ionization of the outer shell. Consider a very simple picture of an atom with a vacancy in the L shell. In a radiative decay it needs only one electron from the M shell to fill this vacancy, whereas in a radiationless Auger decay the energy difference is carried away as kinetic energy of a second M-shell electron. If, due to multiple ionization, some of the electrons are missing from the outer shell, we decrease the probability for this 2 electron process compared to fluorescence decay. Thus, the fluorescence yield is increased. So, the fluorescence yield increases as a function of energy because multiple ionization increases as a function of energy.

In Fig. 24 we have comparison for Ar-Ar collisions including Auger cross sections down to the threshold as recently determined by Ogurtsov.²⁴⁾ This gives us fluorescence yields from threshold (8 keV) up to 100 keV. You see that there is an order of magnitude increase of the fluorescence yield, then it levels off. A very recent result by David Burch shows that for 10 MeV

Fig. 24*

projectiles on Ar the fluorescence yield of argon is up by 2 orders of magnitude from this; it is 10^{-1} , so there is a higher data point for 10 MeV.

Chander Bhalla²⁵⁾ has started some calculations that I think are very relevant to attack this problem. Fig. 25 shows the theoretical fluorescence yield for the 2p shell of argon versus the number of 3p electrons missing from the configurations $(1s)^2 (2s)^2 (2p)^5 (3s)^2 (3p)^m (3d)^n$ for $n = 0$ and for $n = 1$. The experimental values are shown for $Ar^+ \rightarrow Ar$ collisions at 50 keV and 100 keV, in addition to the data for protons incident on argon. If we assume a 3d electron to be present, which is very likely since in these

*The circles are ratios of Ar L x-ray cross sections as measured by Saris and Onderdelinden¹⁷⁾ to Auger cross sections as measured by Ogurtsov²⁴⁾ and Cacak, Kessel, and Rudd.²³⁾ The cross represents a value for 125-keV proton bombardment of Ar¹⁷⁾; the dashed horizontal line is a theoretical value for the neutral atom from Walters and Bhalla, Phys. Rev. A 4, 2164 (1971).

Fig. 25

ionizations you get electrons from the 2p or 2s levels removed into the 3d level, we come up with a charge state of 3^+ , 4^+ , or 5^+ in this energy region. The charge states of the ions that have suffered inner shell excitation have been measured by the Connecticut group and the group in Leningrad, and they show that charge states of essentially 3^+ , 4^+ , 5^+ are present in that fraction of the scattered particles that have inner shell vacancies.^{2,3)}

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* The reader is reminded that this is not an official scientific publication but merely a typescript of an oral presentation of lectures, given in an informal way the personal ideas of the speaker about a very rapidly expanding field of research. Much of the work presented here has not been published yet and if published it will appear in a very different form. Therefore the reader is urged not to quote these lectures in the official literature.

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SOLID EFFECTS ON INNER-SHELL IONIZATION

We have seen how the study of x-ray production cross sections has contributed considerably to our present knowledge of ion-atom collisions. We have also seen how the molecular model is able to explain, at least qualitatively, many of our experimental results. Still there are some headaches left, and I have listed some of them:

For example, when we draw the correlation diagram for heavy ions like xenon, krypton, or argon fired onto carbon, we see that the carbon K shell is correlated with a united atom 2p shell, and there is just no crossing. (See Fig. 15 of talk one.) That means there is no way of getting a carbon K-shell electron out. But we see carbon K x rays prominently. The question is, why is that? Also, when we fire oxygen, neon, or argon onto things like silicon or aluminum, we see silicon K or aluminum K x rays. If we consider, for example, the case of oxygen or neon on aluminum, we see immediately that production of Al K x rays is in disagreement with the molecular model. The aluminum K shell is the lowest orbital in the separated atoms limit, and it is correlated with the lowest orbital in the united atom. There is no crossing, so we are in trouble. Where does this excitation come from? An even worse case is argon on titanium. We see argon K and titanium K x rays in such an experiment, and they both shouldn't be there. Since titanium is the heavier of the two, the correlation diagram (Fig. 1) shows its K shell correlated with the lowest united atom level. Also, the argon K shouldn't be promoted. Later it will be shown that these x rays are observed due to various kinds of "solid effects," because we have been using solid rather than gaseous targets.

Fig. 1

The second point I want to discuss is, having these solid targets, can we study particle-solid interactions by looking at x rays; can we study the state projectiles are in as they move through the solid? I see it this way. With the x rays we have eyes to look into the solid. The x rays come out from deep in the solid, and we are able to look inside the solid by looking at our x-ray spectra. Maybe we can get some information on the state of these ions inside the solid, and I think that would be very important. I think of seeing all those beautiful results of Pat Richard where he is able to resolve the various satellite lines that are due to different charge states present in an atom that radiates an x ray. But he has looked so far only at target x rays; my emphasis here will be on the projectile x rays.

I would like to start with one experiment and work that

out in some detail to give you a feeling for the kind of problems that are involved. I don't mean experimental problems, but problems determining how to interpret the experimental data. Fig. 2

Fig. 2

shows results for Ar ions fired onto Ti that were taken in Amsterdam.¹⁾ We used a grazing incidence monochromator, (so we had a grating rather than a crystal). This monochromator had a resolution of better than $1 \text{ \AA}$, something in the order of a few electron volts for an x-ray energy of 200 to 300 eV. In this experiment we were looking at the collisions of very low energy argon ions, 35 keV and 100 keV, with a solid titanium target. We see roughly 4 peaks. Peak no. 1 is at 235 eV, peak no. 2 is at 258 eV, and then there is a shoulder, which is peak no. 3, at 280 eV. Then we have a small peak at 220 eV for 35 keV projectiles that almost disappears for 100 keV projectiles. Frank Larkins²⁾ has done calculations on the energy shifts of x-ray lines of argon as one successively ionizes the outer shell. The results of his calculations are shown in Fig. 3. Here are plotted the x-ray energies of several transitions as function of the degree of M-shell ionization.

Fig. 3

We have one line at 235 eV, which is identified as a 3s to 2p transition with one 2p vacancy and 3 to 4 electrons missing in the Ar M shell.

Peak no. 2 is at 258 eV. There is no reasonable way we can identify that peak as a 3s to 2p transition when we have only a single vacancy in the 2p shell. That would mean we have a charge state of 7^+ or more, and for a 35 keV argon ion that is extremely unlikely. Much more likely is the 3s to 2p transition with a doubly ionized argon L shell. Production of a double vacancy in the argon L shell is seen to be a likely process from inelastic loss data, as I will discuss later. For a 3s to 2p transition at 258 eV with a doubly ionized L shell, the charge state of the M shell is seen to be 3^+ or 4^+ .

Peak no. 3 is at 380 eV. This can be identified as a 3d to 2p transition with a single 2p vacancy. (This implies one M-shell electron is excited to a 3d level.) Again we come up with a charge state of 3^+ or 4^+ .

The little peak at 220 eV can be attributed to the 3s to 2p transition with a single 2p vacancy in an ion of charge state 1^+ . I think this is most likely produced because this data took so long to accumulate that we were implanting argon into our samples, and we were seeing Ar-Ar collisions. The 220 eV peak thus is not a projectile x ray, but most likely comes from Ar implanted into the target. We will discuss that further a little later.

So, we see that the spectrum can be interpreted as being due to singly and doubly ionized Ar-L shells with roughly 3 to 4 M-shell electrons missing. The next question is, "Does this represent the typical charge state of an argon ion inside titanium, or is it merely the charge state that is produced in a violent collision between an argon ion and a titanium atom? Is this the equilibrium charge state of the argon projectiles, or is it not?" First, I must point out that the contribution from the 3^+ to 4^+ charge states is enhanced; we have to correct for the relative fluorescence yields. As you ionize the outer shell, the fluorescence yield increases. The fluorescence yield for a 3 to 4 times ionized argon atom is an order of magnitude larger than the fluorescence yield for a neutral argon atom. If you correct for that, it turns out that the most abundant charge state is more likely to be 1^+ than 3^+ to 4^+ . One can do a more careful analysis than I did here; I just wanted to indicate that such a thing should be done in the first place.

Question: Doesn't the efficiency of a grazing incidence spectrometer depend strongly on energy?

Answer: Yes, it is strongly dependent; that is another correction we have to make. In fact, the peaks at higher energies would increase by something like a factor of 2 relative to those at lower energies because the reflectivity of our grating goes down as the x-ray energy goes up.

Let's go on now to the question whether this is a typical charge state for an ion moving inside a solid or not. Let's see what this factor would look like if we were looking at a gas. The

gas to be chosen is, for instance, argon, with Z close to titanium. Fig. 4 shows data from the Livermore group,³⁾ taken with a crystal

Fig. 4

spectrometer, not a grating. They looked at x rays from Ar-Ar collisions for projectile energies of 50 keV, 100 keV, 130 keV, and 330 keV. The most striking thing is that the spectra look completely different from the spectra we saw for argon on titanium. At 50 keV there are two lines, and their relative intensity is completely different from Fig. 2. It is likely that the high energy peak here does not correspond to the strongest peak we saw in the Ar-Ti experiment, because this peak has an energy of 270 eV. It is more likely to be the 3d to 2p transition in excited argon rather than the 3s to 2p transition for argon with a doubly ionized L shell. That doubly ionized peak does not show up clearly before

bombarding energies of 130 and 330 keV. In addition, at low bombarding energies we don't see the large shifts corresponding to a high degree of M-shell ionization. (See Fig. 3.) What I am saying is that the 3^+ and 4^+ charge states are not observed until we are at much higher bombarding energies than with the solid target, so in comparing gas targets with solid targets there are big differences observed in the spectra that reflect differences in charge states.

Now let's do a little calculation; the cross section for argon L production in argon-titanium collisions is of the order of $3 \times 10^{-17} \text{ cm}^2$. We can then calculate the mean free path inside the solid for argon before it suffers a vacancy production in the L shell:

$$\lambda = \frac{1}{n\sigma} = \frac{1}{(6 \times 10^{22} \text{ atoms/cm}^3)(3 \times 10^{-17} \text{ cm}^2)} \approx 60 \text{ \AA}.$$

That is the mean free path for L-shell ionization of the argon projectiles. Thus, argon L-vacancy production inside the titanium is a very likely process for these projectiles.

Let's consider the lifetime of these vacancies. After the vacancy has been created, the lifetime for a neutral argon, according to Chander Bhalla,⁴⁾ is about 4×10^{-15} seconds. For a triply ionized atom, the decay rate goes down; the lifetime is more likely to be around 10^{-14} seconds. The velocity of a 100 keV argon ion is $7 \times 10^{15} \text{ \AA/sec}$, and that means that the distance traveled before decay of the vacancy is $vr = 70 \text{ \AA}$. If we compare this with the mean free path found above, we see that the equilibrium fraction of particles having inner shell vacancies is 1. Our projectiles will suffer another L-shell vacancy at roughly the same time that the first vacancy is decaying.

It is interesting to note that this mean free path for L-vacancy production is long compared to that for ionization of the M shell. The mean free path for M-shell ionization for argon moving inside titanium is typically the lattice constant, about $2 \text{ \AA}$. This means that when an argon projectile moves with an inner-

shell vacancy in it over this $70 \text{ \AA}$ distance, it suffers roughly 35 collisions that ionize the M shell. Therefore, the argon M shell is in its equilibrium charge state as the x ray is radiated, so from our x-ray spectra we indeed obtain the charge state that is typical of an argon projectile inside the solid. That is conclusion no. 1. I would prefer not to call it simply the "charge state"; let us refer to the "configuration" of the projectile.

A further consequence of using solid targets, as we have already seen, is that double vacancies are more likely to be created than with gas targets. Why is that? It is simply because the mean free path is small and the cross section for ionization is large. The second ionizing collision occurs before the first created vacancy has decayed. These things are typical for collisions in solids; that you can successively increase the degree of excitation or ionization of projectiles inside a solid is well known. But it leads us to another conclusion; there is excitation occurring inside the solid that does not occur with a gas target. If we go up to slightly higher energies for argon on titanium, we can see this beautifully. In Fig. 1 we have a correlation diagram for the neutral argon and titanium atoms. If we have a doubly ionized 2p shell in argon, the binding energy goes up by so much that the Ar 2p level is below the 2p level of titanium. That is a thing that can only occur in the solid, not in the gas, because in the gas you don't have a second collision before the first 2p vacancy has decayed, so you will never have this doubly ionized configuration. This correlation diagram is not correct for the solid; we have to swap the correlation. That means the argon 2p is correlated to the 2p in the united atom, which is correlated to the 1s of argon. That introduces the possibility of creating argon K vacancies, and that was one of the headaches mentioned before. Argon K vacancies are produced in these solid collisions, and we do see argon K x rays abundantly.

Fig. 5 shows the argon K x ray for 500 keV argon projectiles bombarding titanium.⁵⁾ In addition to the argon K x rays at 3 keV we have a broad feature arising from what I call molecular

Fig. 5

x rays. They originate when the vacancy decays along the molecular orbital between the separated atoms and united atom limits. We will discuss that further tomorrow, but what it means here is that not only is there excitation occurring inside the solid that doesn't occur in the gas, there is also deexcitation occurring inside the solid that does not occur in the gas.

I would like to work a little further on these points. Let's consider first the charge state configuration of the projectile inside the solid and what we can learn about this configuration from studying our x-ray spectra. In the next few slides I will show you some experimental results that have been produced at various laboratories.

Fig. 6 shows the results of an experiment done by Hans Lutz, Sheldon Datz, and others at Oak Ridge.⁶⁾ They fired iodine ions at 48 MeV onto selenium solid targets and bromine and krypton gas targets. You can see that there are differences in the spectra for the two types of target. There are not large differences in the line widths or in the energies of the x-ray lines. They are very much the same in the gas and in the solid. Why is that?

FIG. 1. X-ray spectra excited by impact of 48-MeV I ions on Se, Br, and Kr targets.

Fig. 6

Consider the iodine L x ray. The lifetime of the iodine L vacancy is so short that it decays almost immediately after it has been created. That is, the particles move inside the solid for 3 or 4 Å at the most before decaying, and that means that the charge state we see is really just the charge state produced during the violent collision where the inner-shell vacancy is produced. That is a different case from the one I discussed before where we had long lifetimes. But still there is an intriguing difference of almost an order of magnitude in the intensity of the iodine L x ray. It is argued⁶⁾ that this is due to the configuration of the iodine projectile inside the solid. Inside the solid you can ionize the iodine M and N shells very easily, and that gives you outer shell vacancies into which to promote an iodine L electron. The probability of L-shell ionization is, therefore, much higher in the solid than in the gas. Thus, it is argued that the difference is due to the difference in charge states for the projectile inside the solid compared to the projectile inside the gas. You will notice, however, that for the iodine M x ray the intensity is higher in the gas than the solid. I'll come back to that shortly.

Question: How are the intensities normalized?

Answer: In this experiment the thickness of the gas and solid targets was the same. You can choose a thin film that has the same thickness as the density of your gas target so the number of atoms involved is exactly the same in the two experiments.

I would now like to discuss the results of some very recent and intriguing experiments of the MIT group, Schnopper, Betz, and co-authors, who fired beams of almost anything at almost everything. They fired beams of fluorine up to uranium into almost any target you can think of. Some of their results are very intriguing, and I would like to show a few of them here that were presented at the Atlanta conference.⁷⁾

Fig. 7 shows an x-ray spectrum for 105 MeV bromine ions

Fig. 7

incident on copper. The data were taken with a lithium drifted silicon detector. We have seen that the characteristic lines in these heavy-ion induced x-ray spectra are significantly shifted. They have studied this in more detail. In Fig. 8 we see the energy shift of the x rays as a function of projectile energy for the bromine L and K lines. There are differences from one target to another, and the energy shift increases as we increase the

Fig. 8

projectile energy.

Let's try to pursue this a little further. At 50 MeV the energy shift for L_{α} is about 160 electron volts. That can be related to a charge state of something in the order of 21^{+} . For 100 MeV bromine on copper they measured the cross section for bromine L x-ray production and obtained $2 \times 10^{-17} \text{ cm}^2$. That means that the mean free path for bromine L-vacancy production inside copper is $100 \text{ \AA}$ and not more. We know also the vacancy lifetime from Chander Bhalla's work.⁴⁾ We know the velocity of the bromine from the bombarding energy, and this gives us a distance for the bromine to travel before the vacancy decays of $vr = 100 \text{ \AA}$ too. This means that for the bromine, just as for argon on titanium, an L-shell x-ray spectrum represents a typical state for the bromine ion inside the copper. That means that the charge state we see of roughly 21^{+} is a typical charge state for bromine inside copper at these energies. If we increase the energy from 50 MeV to 100 MeV, the shift is 230 electron volts, and the charge state is 25^{+} .

It is interesting to compare this with the charge states

that have been measured previously. Moak et al.⁸⁾ have measured the equilibrium charge states shown in Fig. 9. Here we have the

Fig. 9

most probable charge state for ^{79}Br after passing through various solid (upper group) and gaseous (lower group) targets plotted versus the bromine ion velocity and energy. For an energy of 50 MeV the charge state for a solid target is something like 18^+ ; that is close to the charge state we came up with when we analyzed the x-ray energy shifts. If we increase the energy to 100 MeV, the charge state is close to 22^+ , which agrees with our energy shift results.

Bromine in a charge state 25^+ is stripped down to the L shell. The L shell is full, but all the other electrons are removed. If that is the charge state inside the solid, that would mean the bromine L light would be out, because there is simply no electron left to decay into the bromine L vacancy. You can check that very easily by sending a bromine beam through a foil. If the charge state is 25^+ so that all the outer electrons are removed, and that is a typical charge state inside the solid, then it doesn't matter what the thickness of the target is, you will not see an increase of your yield as a function of thickness. Normally,

if you measure a thick target yield, you see a dependence on target thickness. This experiment has been done by Schnopper and Betz,⁷⁾ and indeed they find there is very little difference in intensity for the bromine L line whether they send their bromine beam into a thin copper foil or a thick copper foil. The same holds for chlorine. At these high energies you can strip chlorine down to almost 17^+ . That would mean the chlorine K light would be out, and that is what they see. When they increase the thickness of their sample, the chlorine K intensity does not increase. I think these are strong arguments for our conclusions about the charge states from x-ray spectra.

I would like now to spend a few words again on the excitation inside a solid that does not occur with a gas target. It was realized by Sigmund and Taulbjerg⁹⁾ about a year ago that the recoil effect is extremely important in solids. What I mean by the recoil effect is that the beam projectiles produce, per projectile, typically several hundred recoiling target atoms. We know from the results shown in talk one that a symmetric collision is more likely to produce x rays than an asymmetric collision; the cross sections oscillated with Z number and showed a maximum for a symmetric collision. Thus, it is very likely that these recoiling atoms will produce x rays in collisions with other target atoms. Sigmund and Taulbjerg did some calculations using experimental results on x-ray yields obtained by Kahn and co-workers.¹⁰⁾ Figs. 10 and 11 show the calculated carbon K x-ray production cross section, $\sigma_X(E)$, as a function of the energy of the incident ion for various projectiles. $L(E)$ is the cross section obtained neglecting recoil effects. $R(E)$ is the contribution to the total x-ray yield cross section arising from recoil effects. In carbon, the recoil effect is not very large compared to the cross section for the direct effect. That is what you expect, because this is already a symmetric collision. When you go up to nitrogen, the recoil effect is still not very significant; a molecular model still explains the carbon K ionization very well. That is true for oxygen also. For neon things are more difficult. If you have a neon 1^+ ion, there is only one vacancy to which you can promote the carbon K electron.

Fig. 10

Fig. 11

Production of a carbon K vacancy is considerably less probable than with the lighter ions. We see that the recoil effect is significant. Things become worse for krypton and xenon. Essentially everything can be explained by the recoil effect; you don't

need the direct effect. That relieves us from part of the first headache listed at the start of the talk. It is the recoil effect that is producing this carbon K x ray, and it is not a contradiction of the molecular model.

For argon projectiles this is not so. The argon on carbon case is very interesting, and I would like to show the beautiful results that have been obtained by the Livermore group on that system.¹¹⁾ Fig. 12 shows the x-ray production from Ar-C collisions

Fig. 12

in gas targets: (a) 90 keV Ar^+ and protons incident on CH_4 , (b) 80 keV C^+ on Ar, and (c) 80 keV Ar^+ on Ar. When you fire argon onto a methane gas target, you see essentially the same spectrum as for $\text{Ar}^+ - \text{Ar}$ gas and $\text{C}^+ - \text{Ar}$ gas collisions. There are two argon L lines; one is from the 3s to 2p transition; the other is from the 3d to 2p transition. There is no carbon K, as was checked by firing protons onto methane. The carbon K x ray is not present.

in the argon induced x-ray spectra.

In the upper part of Fig. 13 we have the correlation

FIG. 3. Diabatic molecular-orbitals diagrams for the Ar-C collision. For simplicity only inner shells are included.

Fig. 13

diagram for the argon-methane collision with two neutral particles.¹¹⁾ The $2p$ shell of argon is correlated with the united atom $3d$ level and the $1s$ of carbon is correlated with the united atom $2p$ level. As you can see, there are no crossings for the carbon $1s$, and that means there is no way you can get that carbon K electron out. So you don't expect carbon K x rays, and they don't show up. Things are considerably different in a solid, because as the particle enters the solid, ionization of the outer shell occurs and what is probably much more important, we get the effects of singly or doubly ionized L shells in our projectile beam. That means that the $2p$ level of argon is pushed down relative to the $1s$ level of carbon, and we have a swapping of the correlation, as shown in the lower part of Fig. 13. In the solid it is the carbon $1s$ that is correlated with the united atom $3d$ level, and we expect

the carbon K x ray to show up.

Fig. 14 shows the x-ray spectrum for argon on graphite,¹¹⁾

FIG. 2. X-ray spectra for 90-keV Ar^+ and protons incident on a graphite target.

Fig. 14

and indeed there are carbon K x rays. The two shoulders at the lower energy are very similar to the peaks shown in Fig. 2. One is from the 3s to 2p transition in argon singly ionized in the L shell, the other is from the 3s to 2p transition in argon with a double vacancy in the L shell. I think this all ties in nicely with the molecular model, if one takes solid effects into account.

For argon on silicon and argon on aluminum there is another point to make. Fig. 15 is the correlation diagram for an argon-silicon system.⁵⁾ You see that the 2p level of argon is correlated with the 2p level in the united atom, which is also correlated with the 1s level of silicon. Now we have seen that in the solid a large fraction of our beam can have 2p vacancies as an equilibrium state. That means that during argon-silicon collisions inside the solid we can have promotion of the silicon 1s into an argon 2p vacancy. That explains why we see silicon K x rays. Silicon and aluminum are very similar in this respect since the correlation diagrams are the same, so it also explains why we see aluminum K x rays. Joe Macek¹²⁾ recently calculated

Fig. 15

the equilibrium fraction of beam particles with 2p ionization for this particular system. He came up with a cross section for ionization of the silicon 1s shell that fits remarkably well with the experimental results.

I would like to finish with a slide that is well known to most of you. Fig. 16 shows another type of excitation going on in solids, which was measured at Kansas State.¹³⁾ It is the formation of metastable, highly stripped chlorine ions as you send a chlorine beam through a carbon foil, and at remarkably low energies. I don't want to discuss how they are produced; I just want to mention that I think the production of these weird metastable states in highly stripped ions is a very intriguing consequence of the use of solid targets. These results clearly indicate a fruitful line of research to be followed in the future.

Fig. 16

X-ray energy spectrum from a 45-MeV chlorine beam viewed 2 cm downstream from the exciting foil (open circles). For comparison, an x-ray spectrum produced by proton bombardment of NaCl is shown (closed circles), displaying x-ray energies characteristic of removal of a single 1s electron from neutral chlorine.

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X-RAY STUDIES OF THE CHARGE STATE OF IONS IN A SOLID

In the two previous talks we have seen how successful the molecular model is in predicting and explaining x-ray results. Because of this, about a year ago, we started to wonder whether we could actually observe these molecular states that Lichten predicted to exist during the collision. We thought it quite a different thing to have a model that explains what happens after the collision is over than to show that these molecular levels really exist as intermediate states during the collision. In order to see the existence of these states, you have to observe a transition during the collision, that is, a radiative or Auger transition between two of those molecular orbitals.

A slide frequently shown during these talks is the energy level diagram for argon on argon.¹⁾ (Fig. 1) The question now is,

Fig. 1

can we observe radiative transitions between these molecular

orbitals that are said to exist during an argon-argon collision? Well, the first thing we have to have in order to observe such a transition is a vacancy in one of the lower orbitals. For the moment, let's assume that we can do the experiment where our projectile comes in with a vacancy in one of the inner shells, say the L shell. Let's assume that that is possible; I'll explain how in a moment. During the argon-argon collision this vacancy may follow any of a number of molecular orbitals. Can the vacancy decay during a collision? Let's make a hand-waving argument about the lifetimes. The lifetime of a 2p vacancy in argon is typically 4×10^{-15} seconds, whereas the lifetime of a 2p vacancy in krypton (that's the united atom of argon + argon) is about 5×10^{-16} seconds, roughly an order of magnitude shorter.^{2,3)} So as the collision proceeds, the vacancy lifetime decreases by an order of magnitude. It is interesting to note that the collision time is of the same order of magnitude. The collision time relevant here, I would say, is the time the two particles spend at an internuclear distance smaller than the radius of the L shell, where all these transient molecular orbitals exist. That is something like 0.25 Å. A 100 keV argon ion has a velocity of something in the order 7×10^{15} Å/sec. This means that the collision time is roughly 0.7×10^{-16} seconds, of the order of the lifetime of the vacancy in the krypton L shell. It is perhaps even more important to note that the fluorescence yield, that is the probability for the vacancy to be filled by radiative transitions compared to the probability for radiationless transitions, is also changing drastically as the internuclear distance changes from infinity to zero. Chander Bhalla⁴⁾ has calculated the fluorescence yields, and for argon L it is two orders of magnitude smaller than for krypton L. So, as the collision proceeds and we push the vacancy along the 2p orbital to smaller internuclear distances, the probability for the vacancy to be filled by a radiative decay increases by two orders of magnitude, making the observation of the x-ray photon more likely.

If it is so likely, what are we expecting? What should our spectra look like? The most likely transition is the 3d to 2p.

The vacancy can decay at any time during the collision, so we don't expect a very sharp line but a broad distribution. Most of the collisions will not be very violent collisions, that is, we won't go to very small internuclear distance. So we expect to see the argon L line in our spectra. The most likely thing to occur is that the vacancy does not decay during the collision, but just partly moves in and goes out, and decays when the particles are far apart. So we will see argon L x rays. If the vacancy decays during the collision, we will see a broad band. If we increase the impact energy so that the internuclear distance during the collision is decreased then the transition energy from 3d to 2p becomes larger. So we expect that this band will extend to higher photon energy. Eventually it could reach the krypton L x-ray energy. If the vacancy can be pushed down all the way to the krypton L level, then it can go out in two ways, along the 2p π orbital, or along the 2p σ orbital ending up in the 1s level of argon. That means we will see argon K x rays.

If the vacancy moves out along the 2p σ orbital, and if it can decay along this orbital, we are increasing the x-ray energy in the band. The transition energy from 3d to 2p σ is larger than from 3d to 2p π . This implies that the region beyond the Kr L energy will be more or less filled in too.

I have not explained how we get the starting condition.

Obviously with a lifetime of 4×10^{-15} seconds for the vacancy in the 2p shell you cannot use an ion source, produce an L-shell vacancy, and bring it down along the beam line for a collision in the target chamber. The vacancy will decay long before the target chamber is reached. What we decided to do was to use a solid target. A solid target will help us in two ways. First, as we discussed in the previous talk, in a solid the fraction of beam particles having L-shell vacancies can be extremely large, something in the order of 10% to 100%, depending on the kind of target. Second, we want to observe argon-argon collisions, because that is the system with which we are most familiar, and also because we think symmetric collisions are most likely to show such x rays. We would like to have a solid argon target. The easiest way to produce it is to implant argon into a solid target. One can easily produce a concentration of something like 10% of argon by bombarding with low energies and high doses. If you use, say, a 100 keV argon beam of 10^{17} ions/cm², a saturation dose of the order of 10% total concentration of argon inside a Si lattice is obtained. A collision with an implanted argon particle becomes very likely.

The first experiment with implanted solid targets, specifically silicon, was done in Amsterdam.⁵⁾ The next slide (Fig. 2) shows the result we obtained with a lithium-drifted silicon detector where we replaced the beryllium window with a mylar window in order to be able to see very low energy x rays. What you see is the argon L x ray and at higher energies a broad band that looks very much like what we expected. We studied this "molecular x ray" as a function of energy, Fig. 3. We have put the beryllium window on the detector again to avoid pile-up problems from the argon L x rays. Thus, the argon L part of the spectrum is not visible, and the shape of the molecular band on the low energy side is essentially determined by the absorption in the window and nothing else. What we wanted to see was a shift of the high energy side of the molecular band as a function of energy. That was very hard to do with the silicon target because the silicon K x ray came in. At 150 keV there is already a considerable contribution from

Fig. 2

silicon K x rays in the spectrum; at 200 keV the two peaks are of almost equal weight, and at 240 keV silicon K is significantly the larger. It was encouraging that the silicon K x ray appeared because, considering the correlation diagram, the fact that silicon K x rays appear indicates that indeed a large fraction of beam particles have L-shell vacancies, which are then pushed down into the 1s orbital of silicon in argon-silicon collisions. This proves that the assumed starting condition inside the solid was really there. Another thing we saw in this experiment was the dose dependence of the molecular band. This is expected because argon-argon collisions are needed inside the solid, so the intensity should be (argon) dose dependent.

However we were not too happy with these implanted targets. A little bit more clever than using implantation would be to use a KCl target. The potassium atom is one Z number higher than argon, and the chlorine atom is one Z number lower than argon,

Fig. 3

so we should observe the same band, and it should not be dose dependent. I have learned now that one can be even more clever and use a solid argon target, which is what Jim Macdonald has done.⁶⁾ His spectra and our spectra look very much the same, but for some reason I have my own slides here. Fig. 4 shows the argon on KCl results.⁷⁾ Again there is the molecular band; there is no dose dependence for this band. We can now see this band shifting in energy when we go up from 100 keV bombarding energy to 250 keV bombarding energy; the position of the high energy side changes by at least 250 eV. As we increase the energy further to 400 keV, there is not much change in the position of the high energy cut off. You see that we are just a little beyond the 1.6 keV krypton L x-ray energy.

Fig. 4

Since we did not use a solid argon target, there is a slight difference in the correlation diagram. (Fig. 5) The levels of chlorine and argon are pretty close, and at these energies there is considerable mixing. We get 2p vacancies formed, and they follow the 2p π orbitals. At something like 250 to 300 keV, we can push the vacancy down into the 1s separated atom orbital, but in the argon-chlorine collision that is more likely to be chlorine than argon, according to the molecular model. Since there is considerable mixing, you would expect argon K as well as chlorine K; we didn't see as much argon K as chlorine K. In Fig. 6 you see the chlorine K peak at 2.6 keV, whereas the argon K is at 3 keV. We couldn't distinguish it from the K α /K β structure of chlorine K. Apparently the molecular model is working pretty well at these energies. This figure also gives you an idea of the intensity of the molecular band compared to the characteristic

x-ray line.

So far we have confined the discussion to the symmetric system argon-argon in either argon implanted targets or solid argon targets. But one could wonder why not use asymmetric systems, where also sometimes a 2p vacancy can go down along one of the inner orbitals. It would seem likely that similar molecular x rays would be observed for asymmetric systems. Well, they are. I think of the work done by Jim Macdonald for things like argon on silicon, aluminum, and carbon.⁶⁾ We have done particularly argon on titanium. Fig. 7 shows the correlation diagram for argon on titanium

Fig. 7

that we discussed in talk No. 2. I mentioned then that the cross section for argon 2p vacancy production in argon-titanium collisions is so large that the fraction of beam particles having two L-shell vacancies is one. That means that the vacancy level is not a neutral atom level, but it is mixed with the 2p level of

titanium. That allows us to bring the vacancy down along the molecular orbital leading to the 2p united atom level, and so it allows us to observe the 3d to 2p molecular transition in the argon titanium system. It also allows us to bring down the vacancy further to the 1s orbital in argon.

Indeed, in the argon-titanium system we saw bands similar to our earlier molecular x rays, but at higher photon energies. The system is heavier for argon on titanium than for argon on argon. (The united atom is zirconium, which is heavier than krypton.), so the 3d to 2p transition energy is larger.

Fig. 8 shows the band for 200 keV argon on titanium. As

Fig. 8

we increase the bombarding energy further to 1 MeV, there is no significant change in the shape of the high energy cut off. We also have argon K x rays coming in, and that is why the background is larger.

Fig. 9 shows argon K x rays at 3 keV. The broad struc-

Fig. 9

ture in the low energy region, covering x-ray energies from something like 1 to 2 keV, is much, much broader than our detector resolution. It is very close to the zirconium L characteristic x ray.

Question: Is the low energy edge of that molecular x ray still determined by the detector?

Answer: Yes. In this experiment we had a 50 micron beryllium window. If you take the absorption function of the window, the transmission drops by more than an order of magnitude over an energy region from 1.5 to 1 keV.

Question: Why do you have so few counts in Fig. 8?

Answer: That is because in this experiment we wanted to avoid implantation of argon in the titanium, so we couldn't afford high beam currents and long counting times, and cross sections are small.

Question: What causes the strange peak shapes in Fig. 8?

Answer: What this reflects is the transmission of the filter in addition to the shapes of the molecular orbitals. I don't

know what the shape is, exactly, because I don't know the efficiency of my detector as a function of photon energy very well. I could do a correction for transmission knowing that my beryllium window is 50 microns thick, but I don't know, for instance, what is on the surface of the detector and beryllium window. There are certainly sputtered particles if the detector has been used for a long time in collision experiments. You cannot really do a good correction for the shape.

If these bands represent molecular x rays, then we can wonder what kind of interesting things can be done with such molecular x rays. I think there are three significant things to discuss here. First, there is the recoil effect. We might wonder whether the recoil effect, I mentioned in talk No. 2, which is a very efficient process in creating characteristic x rays, is not also a very efficient way to produce molecular x rays. Second, can we learn something about the molecular orbital energies as a function of internuclear distance by observing these molecular x-ray bands more carefully. Third, if we go up in Z , from argon on argon to say iodine on tellurium or iodine on gold, we can go well into the super-heavy region. That is, maybe we could study energy levels of super-heavy quasi atoms. I would like to discuss these three points in the next part of this talk.

Let me begin with the recoil effect. We have chosen to study the systems obtained by bombarding neon, nitrogen, and argon ions onto titanium, vanadium, manganese, cobalt, copper, nickel, and germanium targets.⁸⁾ I am going to show you just the major features of these experiments. If we use nitrogen and neon on titanium, the recoil effect in producing titanium K x rays could be more important than any direct effect. The recoil effect to which I am referring is that we produce recoiling titanium atoms. We only need titanium atoms at a few keV to produce L-shell vacancies. The threshold for L-shell vacancy production in titanium-titanium collisions is somewhere around 10 keV, so if we have a sufficiently high-energy recoil produced, then it is very likely that there are titanium L-shell vacancies present. Then these 2p

vacancies can give rise to a molecular band. It is the same story as for argon on argon, but now for recoiling titanium inside titanium targets. We expect that at low energy all we will see in the spectrum is a broad band. The titanium L is not visible because it is too low in energy to be seen in our detector; the titanium K is not produced because our recoil energy is not yet high enough to penetrate to small internuclear distances and get the vacancy down into the 1s orbital. As we increase the energy of our projectile, and hence the mean energy of our recoils, the high energy cut-off will shift further, and eventually it will shift to the united atom energy. Simultaneously, we should also see titanium K x rays coming in, because the vacancy can now leave the system via the titanium 1s orbital. So when the molecular x ray saturates, that is when the high energy cut-off doesn't shift any further and we apparently have reached the united atom region, we should observe titanium K x rays also.

Fig. 10 shows the result when we draw all our experimental

Fig. 10

results, one over the other, all normalized at channel 50, which

is 1 keV. The first line is for 300 keV nitrogen on titanium; the others are for 400 keV, 500 keV, 600 keV, etc. It is visible in this figure that it really saturates at a few channel numbers. This is for projectile energies ranging from 300 keV to 2 MeV. By the time the MO band has reached the Ru-L x-ray energy, titanium K x rays come in, and as we increase the projectile energy, their intensity grows further and further. Since we are observing titanium-titanium collisions, we can send in a beam for a long time. We don't need to worry about implantations, particularly not with nitrogen and also not very much with neon. This means we can produce much better statistics here and get a better line shape for these bands.

We have analyzed the data and plotted the end points of the MO bands as a function of energy. We have also plotted the excitation function of titanium K. (Fig. 11) At the bottom of the

Fig. 11

figure you see the x-ray energy and point of our band plotted versus projectile energy in the lab system for nitrogen projectiles. You see that once the lines start to saturate, when the end point doesn't shift position any longer, that is when the excitation of the titanium K comes in.

This excitation begins at lower energies for neon than for nitrogen projectiles. That is because the maximum energy transfer from neon to titanium is larger than for nitrogen to titanium. The maximum energy transfer is⁹⁾

$$T_m = \frac{4m_1 m_2}{(m_1 + m_2)^2} T_o .$$

You can calculate the maximum energy transfer for nitrogen and for neon, and it turns out that the titanium recoil energy at which titanium K excitation comes in is the same for the two. Fig. 12

Fig. 12

shows the K x-ray excitation functions for a number of targets for nitrogen (O) and neon (X) projectiles. The titanium K excitation comes in at projectile energies of about 600 keV for neon and 700 keV for nitrogen. The maximum energy transfer for neon on titanium is roughly 0.8 times the energy of the neon incoming beam; that means that our titanium recoil energy at 600 keV projectile energy is 500 keV. For nitrogen, the maximum energy transfer is 0.7 times the beam energy; 0.7 times 700 keV is also 500 keV. So, what we see here is really the excitation function for creating K-shell vacancies in recoil induced titanium-titanium collisions.

Question: Is the united atom K x-ray energy for titanium-titanium precisely what you get from the energy where your band saturates?

Answer: No, it is slightly above. The band saturates at 3.2 keV, and the united atom energy is 3.0 keV.

Question: Can you quantitatively account for that by integrating the recoils?

Answer: No. The reason will be discussed later.

Let me now consider the second point. It should be noted that the highest energy molecular x ray is radiated during the most violent collision between two titanium atoms. The most violent collision is when we have the maximum energy transfer from the projectiles, and if this titanium recoil has a head-on collision with another titanium atom. If we make that assumption, we can calculate the internuclear distance at which this high energy x ray was emitted. We can then plot the x-ray energy of the end point as a function not of E_0 , but of R_0 , the minimum internuclear distance of the two colliding titanium atoms. That should give us the shape of the $2p\pi$ molecular orbital, because the outer orbitals are not changing shape with R_0 . We are at such small internuclear distances that we are an order of magnitude inside the radius of the M shell. That means we have already formed the united atom M shell, and that energy level doesn't change, so what we see here is the

shape of the $2p\pi$ orbital. I have plotted that in the next slide.
 (Fig. 13) What you see here is the energy level diagram of

Fig. 13

titanium + titanium going to ruthenium in the united atom. I have used the same data points you saw in the earlier slide to construct a plot of the x-ray end points as a function of minimum internuclear distance. I have also sketched the molecular orbitals as they should show in a correlation diagram. You see that the data give us a shape that is not unreasonable. What I have tried to get across here is that it looks very much like we can match the shapes of our molecular orbitals as a function of R_0 using these data, and people can thus compare experimental results with computer calculations of the shapes of these orbitals.

Finally, I would like to talk about the super-heavies. It was the Jülich group, Mokler, Stein, and Armbruster, who did an

experiment with iodine on gold.¹⁰⁾ Their results are shown on the next slide. (Fig. 14) For 57-MeV iodine projectiles on gold, with

Fig. 14

no filter, they see gold M and L x rays and iodine L x rays. There is something of a broad feature around 8 keV, but it could not be studied in detail because of the enormous intensity of these characteristic lines. What they did was use aluminum filters to reduce these intensities. For the 11 MeV and 25 MeV iodine on gold you can see the broad band in the spectrum at around 8 keV. I think that the shape at low energies, the fact that you see the broad band intensity go down, is due primarily to the aluminum filter. The interesting thing is that predictions show that the x-ray energy of the M x ray in the super-heavy quasiautom iodine plus gold, which is $Z = 132$, is somewhere around 8 keV, and that is where this band tails down to.

One must be very careful here. First, you must wonder whether the correlation diagram allows you to push vacancies from

the iodine shell into, for instance, the M shell of the united atom. In this case, that is true. If you draw the correlation diagram, iodine L vacancies can end up in the M shell of the united atom of $Z = 132$.⁹⁾ But second, we have seen that recoil effects are enormous. One could wonder whether this is not a much higher Z number x ray, namely the M x ray from gold + gold, with $Z = 158$. Since we do not have a very well defined cutoff, and since all these energy levels in the super-heavies are not very well known, it is hard to identify this as being due specifically to $Z = 132$.

The interest in doing this whole thing of the super-heavies, apart from the word super-heavy, arises from an intriguing part of physics proposed by Walter Greiner.¹¹⁾ It is predicted that nonlinear electrodynamic effects will show up in the binding energies of super-heavies. If one plots the energy eigen-value of the 1s electron obtained from the Dirac equation as a function of Z number, at some Z number in the order of 170, the binding energy dives into the negative continuum. That is if one uses electrostatics based on Maxwell's equations (dashed line).

If one uses the nonlinear electrodynamics of Born and Infeld,¹²⁾ the dive into the negative continuum occurs around $Z = 215$ (solid line). The question is, can we determine the binding energy for these high Z numbers? If so, then we can tell how important these nonlinear effects are. It is also predicted that if one brings in a K-shell vacancy in a collision of, for instance, lead on

lead, and if that vacancy dives into the negative continuum, then one should expect spontaneous positron creation.

I think those experiments look rather intriguing. On the other hand, I think it is going to be a tough job for many, many reasons. One is the fact that it is very hard to get K-shell vacancies in these very heavy atoms, particularly things like gold and lead. Another thing is that, if this means we will have to go up to extremely high energies to produce K-shell vacancies, then our collision times will be so short that a collision becomes highly non-adiabatic. That means we cannot determine the energy level with great accuracy, and that means collision experiments won't help us very much in determining these energy levels.

Let me conclude by briefly summarizing. First, we have seen that one can indeed observe transitions between molecular orbitals during collisions of ions and atoms. Second, the molecular x rays can help us in better understanding collision processes, such as transfer of vacancies from one orbital to another, as well as recoil induced effects. Third, a preliminary analysis of our data shows that one can get quantitative information on the shape of the molecular orbitals. One can map the molecular orbital on the energy level diagram and make a comparison with computer calculations. Fourth, there may be interesting prospects related to super-heavy elements.

Question: Do you understand how Briggs and Macek were able to map one threshold energy into another threshold energy?

Answer: Yes, I think I do. You can show that the two important things are the velocity and the energy difference between the two orbitals. The energy difference of the two orbitals scales by Z^2 , and the velocity was scaled by Z . I think the arguments they give for that are correct.

Question: Most of the things you talked about have involved L-shell vacancy promotion. But I have a question about vacancies in K shells that you carry into the collision. I think in this case that the molecule description is not so necessary, but in fact what you begin to do is just provide a system where

you suppress the energy level of the K shell towards the united atom limit. Do you feel there is anything in that?

Answer: Well, let's say that the whole business of K shells is still within the molecular model, but much more complicated. The complication I see from our data is that the lines are much broader. I think I understand why that is. In the case of the L shells we can push the two nuclei inside the radius of the K shell and the screening of the K electrons provides us with essentially a united atom L shell. Thus, the L-shell electrons can form a configuration of the united atom, whereas the K electrons cannot do that so easily because they still see the two centers; they still have molecular wave functions rather than atomic wave functions. What that means is that, for the L shells, we will have a straight line as a function of internuclear distance on the energy level diagram after some internuclear distance. If we allow the vacancy to decay in this region where the energy doesn't change, we get a reasonably well defined x-ray energy. This is an adiabatic process. That is not true for the K shell, because the two nuclei must come really very close to be seen by the electrons as one nucleus. The K-shell energy levels will change shape down to very, very small internuclear distances, typically of the order of $10^{-3} \text{ \AA}$ or smaller. Since we have to have much higher energies to observe these K vacancies in the united atom limit, we will go through a distance $10^{-3} \text{ \AA}$ in a time of the order of 10^{-21} seconds. That implies an energy uncertainty of several keV.

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THE ANALYTICAL USE OF ION-INDUCED X RAYS

The increased interest in ion-induced x rays has certainly been inspired by their possible use as an analytical tool, better than Röntgen's electron-induced x rays. Let us briefly recall the differences between the "old x rays" and the "new x rays" with the aid of slide 1, which originates from the cover of the Scientific

Fig. 1

American of 1896. Rather than looking at the x rays generated by the electrons striking the anode, we are here interested in x rays emitted by the cathode under ion bombardment. Röntgen's rays were mainly the bremsstrahlung continuum x rays, which are virtually absent in ion-induced x rays. The characteristic radiation, generated by ions impinging the cathode, is detected by a Si(Li) detector rather than the fluorescence screen of the figure. We have also replaced these coils by Van de Graaff accelerators which with such a system one can do absolute cross-section measurements.

The next slide (Fig. 2) shows a typical example.¹⁾ You have seen this in an earlier talk. I don't think I need to go over

Fig. 2

the details again, but let me point out once again that if we plot the cross section for x-ray production versus bombarding energy, there are two regimes. There is the regime of heavy ions, for which we reach high cross sections already at very low energies, and there is the regime of low mass particles, protons and helium, where we have to go out to much higher energies to reach eventually the same cross sections.

We know that for heavy ions we don't need very high energies to produce a large intensity of x rays. We have seen that that is because in heavy ion-atom collisions one forms a quasi-molecule in which one has to have overlap of inner-shell electron levels to get an inner-shell electron promoted to a higher orbital. That means that we have geometrical cross sections at energies where the inner shells overlap, and that is at relatively low energies. It also means that if we don't have an overlap of inner-shell electron levels, if there is no crossing where we can promote the electrons into higher orbitals, then there is no excitation of

those inner shells. That means that we have a selective process for ionization, which is very useful if one wants to do an analysis of trace elements in a substrate. A big difficulty is the large intensity of x rays of the substrate. This effect allows you to selectively ionize the inner shells of the trace elements and reduce the intensities from the substrate.

The person who first did this, and who has developed the technique most, is Jim Cairns at Harwell. I am going to show you two of his results.²⁾ The next slide (Fig. 3) will help me to

Fig. 3. The production of characteristic X-rays from antimony and silicon by Kr^+ ions of increasing energy

Fig. 3

explain a bit further what I mean by selective ionization. The interest here was antimony as an impurity in silicon. When one bombards antimony with krypton ions, the antimony M characteristic x ray comes in at very low energies, around 70 keV, and already at 100 keV the excitation function starts to level off. We have a very high yield of antimony M x rays, whereas the silicon K x rays are still not seen in the spectrum. They come in beyond something like 170-180 keV. So if we do the analysis with a 100 keV krypton beam, although we bombard a silicon sample, we don't see the silicon

sample, we don't see the silicon K x rays in the spectrum. We see only the x rays of the impurity, antimony.

That situation is ideal if you are interested in studying the profile of antimony implanted in silicon, which is what Jim has done. He sent a beam of krypton into his sample, measured the yield of antimony M x rays, stripped off a layer of a certain thickness, a few tenths of angstroms, sent the beam in again, measured the antimony M x rays, and so forth. You get a curve that you then take the derivative of and that gives you the implantation profile of the antimony in silicon. The results are shown in the next slide. (Fig. 4) This shows the observed x-ray yield as a

Fig. 4. Antimony distribution profile of three antimony-implanted silicon specimens.

Fig. 4

function of depth. You see that we have curves that peak at a mean depth of about 400 Å. I believe this was for 100 keV antimony implanted in silicon. It is important to note the low doses. For the lower curve, the total dose implanted in the sample was 3×10^{14} ions per cm². If you realize that $\sim 3 \times 10^{15}$ atoms per cm² is one monolayer, so one has implanted one-tenth of a monolayer distributed over a depth of 1000 Å. We are able to observe the profile of that

small amount of antimony with this technique.

Another example, using heavy ions at quite different energies, is one that was done at Oak Ridge. I think most of you know about the hassle that is going on about radioactive waste disposal in salt mines. People have claimed that plutonium oxide diffuses over the surface of a salt crystal (see ref. 3.). It was done using an electron microprobe and measuring the signal of the plutonium x ray as a function of distance, annealing the sample, and again measuring the plutonium signal as a function of distance. But the trouble was with the peak-to-background ratio, the sensitivity, which was not very high.

A similar experiment was done by Saltmarsh and Van der Woude³⁾ in Oak Ridge using 20 MeV oxygen ions. A 160 Å layer of lanthanum oxide, which is the analog of plutonium oxide, was deposited on half of a target of salt; the other part was masked. Then the mask was taken away and the sample was exposed to a 20 MeV oxygen beam. The yield of lanthanum L x rays was then measured as a function of distance across the surface. The spectrum in the part where the lanthanum was deposited is seen in Fig. 5. We see

Fig. 5

the chlorine K x ray of the salt substrate. In order to reduce

this yield, they put in a filter to reduce the intensity of these lines and get rid of pulse pile-up problems. The interesting lines are the lanthanum $L\alpha$, $L\beta$, and $L\gamma$ lines. There is an impurity of calcium in the salt, and there is an iron peak visible also. There is also background; I'll come to that a little later. But obviously one can measure this intensity as a function of distance across the surface. They did this experiment for an unannealed sample and for a sample that was annealed at 250°C for two weeks. They expected a diffusion of 2 mm in total if the diffusion coefficient was $10^{-8} \text{ cm}^2/\text{sec}$; their spatial resolution was much better than 2 mm.

The results are shown in the next slide. (Fig. 6)

Fig. 6

Plotted here is the lanthanum thickness as determined by the x-ray yield versus distance. The original boundary where the mask was put apparently has not changed after annealing. The trouble is to determine where really the cutoff is since there is a background. You see indicated on the figure the claimed spatial resolution; 2 mm is two divisions, and it looks like they are correct in saying that since their yield cuts out sharply at the position where the mask was, there is no diffusion seen.

These examples are the only two cases I know of where

people have really done a significant job in analysis using heavy-ion-induced x rays.

There are serious troubles with heavy ions as a means for analysis, and one of these troubles is the recoil effect; another is charge state effects. If we use a thick target, the beam is stopped in the target and changes charge state all the way along the penetration depth. We have seen in the earlier talks that the charge state greatly affects the probability for excitation as well as the probability for radiative decay. What I am saying is that the probability for exciting the inner shell, which is dependent on the charge state, is thus dependent on the depth as well as on the energy of the beam. The fluorescence yields are also dependent on the charge state, in a manner that we won't get a handle on until we have done much more research. Thus, heavy-ion-induced x rays are not really in a state where we can do an absolute analysis. We can do a relative analysis as Jim Cairns has done,²⁾ although one has to be careful there too that by stripping layers from the sample and changing the distribution of antimony atoms in the silicon you don't change the charge state of your incoming beam. (I think that is not very likely for these low doses.)

This is one of the main reasons why people have so far used proton and helium induced x-ray analysis rather than heavy-ion-induced x-ray analysis. For protons and helium you need much higher energies; you need Van de Graaff accelerators. On the other hand, in these days when we have budget problems if you can keep the machine running doing an analysis, that's not so bad either. The big advantage using proton or helium induced x rays is that we have a quantitative knowledge of the cross sections. They fall on the universal curve shown in the next slide (Fig. 7).⁴⁾ Plotted vertically is the cross section multiplied by the binding energy of the particular K shell squared and divided by the square of the Z number of the projectile. Plotted horizontally is the energy of the incoming projectile divided by the K-shell binding energy and by a quantity λ , which is the ratio of the projectile mass to the proton mass. Cross sections plotted in this manner

Fig. 7

fall on a universal curve. For any given case, we can predict within something like 25-30% what the cross section is for every particular energy. We can do a quantitative analysis within that accuracy, which is, I think, compared to other techniques, still rather good.

It is interesting to compare these cross sections with Rutherford backscattering cross sections, because Rutherford backscattering is being used these days, using the same accelerators, for analysis of samples, particularly thin films. At various laboratories people now use both Rutherford backscattering and nuclear reactions as a means for trace element analysis in thin films. Let us compare the cross sections. In Fig. 8 you see the x-ray production cross section, σ_K , compared to the Rutherford backscattering cross section, σ_{RS} .⁵⁾ For x-ray production the effective cross section is the ionization cross section multiplied

Fig. 8

by the fluorescence yield. The cross sections are plotted as a function of Z number. For the x-ray generation we have taken 2 MeV protons, which you can get from a single stage Van de Graaff. For Rutherford backscattering you would rather use 1 MeV helium ions, which are also available from the same accelerator. Let's compare those two. We see that the x-ray production cross section in the region of Z below 30 is 4 orders of magnitude larger than the Rutherford backscattering cross section, which on its part is larger than the x-ray cross section for much higher Z numbers. If we were able to increase the energy of the beam, we could extend the region where the x-ray cross section is larger than the Rutherford cross section until something like $Z = 50$ (dashed line). I think we could increase this region further using the L and M characteristic x rays, but the trouble there is that we don't know the cross sections as well as we do the K x rays. On the other hand, we don't gain very much either, so I think the Rutherford backscattering really is the better technique for large Z because it is a sensitive technique and accurate technique. One problem with the Rutherford technique is the energy resolution.

X rays from adjacent elements are easily resolved by present day lithium drifted silicon detectors. This is not so for the back-scattering from two adjacent elements where you use a surface barrier detector. We need not talk about nuclear cross sections because they are much, much smaller. We have to penetrate into the atom much deeper than when we only have to ionize the K or the L shell or bounce back in an elastic collision.

Let us do a very simple calculation. Suppose we have a typical cross section of 10^{-19} cm^2 for producing a characteristic x ray of the trace element we want to detect on a substrate. What is the number of particles we can see in a reasonable experiment? It is given by the smallest number of counts we want to use in the experiment - let's say that we will be satisfied with 100 counts in total for this analysis - divided by the cross section, which is 10^{-19} cm^2 , divided by the total amount of beam. If we have a beam of say 10 microamps, and we send in the beam for an hour, we have about 10^{16} beam particles, say on a mm^2 . We have to divide this by the efficiency of the silicon detector. In the region we are talking about here the detector efficiency is 1. Also, if you do a good job in designing your apparatus, you can have a solid angle of something on the order of 10^{-2} . This means that the number of particles you can detect is 10^9 atoms per cm^2 . This is unbelievable; 10^{-6} of a monolayer! Well, it is also rather naive, because it's not really the cross section that determines the lower limit of sensitivity of our technique, but the signal to background. But I liked to do this calculation to show you that if we were able to reduce the backgrounds in our x-ray spectra, if we were able to increase our peak-to-background ratios, we could go much, much better than 1000th of a monolayer, which is presently the limit of sensitivity.

We see that the backgrounds are really important, but very little has been done to study these backgrounds. In Chalk River we have done some work on it in relation to the molecular x ray, because we wanted to show that the molecular x ray was not bremsstrahlung or some other background process. Fig. 9 shows a sketch of the kinds of backgrounds produced by various means of

Fig. 9

x-ray generation. You can generate x rays with other x rays. If you use 20 keV x rays and plot the number of x rays emitted versus energy, you will have a peak in your spectrum due to Compton scattering in the region shown in the top diagram. On the lower energy side the sensitivity will be very low because the photo-ionization cross section is low. In the region where the photo-ionization cross section is large, you will have a large background. Using 20 keV electrons, you will have a bremsstrahlung spectrum that will look roughly like the center diagram. In the region where the ionization cross section is large, the background of bremsstrahlung is rather high. Using 40 MeV alpha particles, the projectile bremsstrahlung goes down to 20 keV, but peaks at much lower energies, as shown in the lower diagram. In that region where the interest is here, where the cross sections are really high for 40 MeV alphas, the background is much lower than these other backgrounds. Naively one would say, since the highest energy

projectile bremsstrahlung is 20 keV, the spectrum should be very clean beyond that point, but that's not the case.

At the University of Colorado people have done some very relevant studies.⁶⁾ I would like to show you what the backgrounds in their proton and helium induced x-ray spectra look like, for 25 MeV alphas or 4.3 MeV protons, and for the three samples that were used in the two cases. The shapes of the background, the peak-to-background ratios, are considerably different. They not only differ from one projectile to another, they also differ from one sample to another, although the thicknesses of the samples were the same. They were able to relate the intensity of the background to the intensity of the calcium peak. (One has to realize that again there was a filter used to reduce the low-energy intensity, because calcium was the major impurity in this water sample.) The claim is that the background photons are produced by calcium K electrons that have been knocked out by the projectiles and that cause bremsstrahlung in the substrate. One can transfer more energy to bound electrons than to free electrons. This empirical analysis also tells us that one should use 4.3 MeV protons rather than 25 MeV alphas, because peak-to-background ratios are much larger, and therefore the sensitivity is much larger. It is also obvious that we see a large number of trace elements in these water samples, and an absolute analysis of these trace elements can be done. That's what they are doing in an interdisciplinary study with physicists, biologists, chemists, and environmentalists in Boulder. They have not done only water samples; one can also do air pollution research for instance.

Application of these techniques to air pollution studies were done for the first time in Sweden. People at Lund University⁷⁾ analyzed a thin carbon foil, made sure there were no impurities seen in the x-ray spectrum of the carbon foil first, and then they put the foil outdoors for 24 hours and looked for the number of impurities that were accumulated on the surface. Their beautiful results are shown in Fig. 10. For instance, for titanium, this peak corresponded to a total amount of titanium of 10^{-9} grams.

Fig. 10

An extremely small amount of titanium was accumulated on the surface, but one can easily detect it as a function of those parameters environmentalists want to vary.

Biological samples can also be studied. Fig. 11 shows results obtained again in Boulder.⁶⁾ The accumulation of molybdenum in blood was of interest. If you feed laboratory animals with food that contains molybdenum, you observe accumulation of the molybdenum in the blood of these animals. The trace elements in plants have also been studied (Fig. 12).⁶⁾ A large number of metals are found in these plant samples. Skin tissues of people who have suffered from an ill-functioning of the kidney and who use the artificial kidney have been studied (Fig. 13).⁶⁾ The lower curve is from skin tissue of a normal person; the upper curve is from skin tissue of a person who has been treated by a dialysis machine. Apparently a tin impurity is building up in the body of the patient. On the other hand, bromine is drained away. This is really significant information for people in medicine. In this particular case they have been able to relate this to nervous diseases in these patients.

Fig. 11

Fig. 12

Fig. 13

So far I have talked about uses of the beam as a means of producing characteristic x rays only. The beam is sent onto a sample, and you get characteristic x rays; you don't really make use of the fact that the projectiles are ions other than in enhancing the peak-to-background ratio. The next part of this talk will be devoted to a more elaborate way of using the beam that has been studied in the field of particle-solid interactions for 10 years now. We can study crystals by making use of the channeling phenomenon. The next slide (Fig. 14) shows how a projectile will see the target as it approaches a single crystal. As we align the beam along one of the low index directions of the crystal, the beam particles can go very, very deep into the crystal without suffering from small impact parameter collisions. Such particles can be found at much greater depths than particles that have been bombarded into the sample in a random direction. If the beam is sent in a low index direction, all those processes that require a small impact parameter are reduced. For instance, the Rutherford backscattering yield is reduced. If one measures the intensity of a small impact parameter process of interest, as a function of

Fig. 14

angle with respect to the channel of low index direction, one observed a minimum for the aligned direction. You can plot, for example, backscattering yield, or x-ray yield, or nuclear reaction yield; all those processes require a small impact parameter collision.

The channeling phenomenon can be used to study damage in a crystal because if we move or replace lattice atoms due to radiation damage, the channeling doesn't work; the minimum fills in. You can, in fact, actually study the lattice site of impurities that are brought into the sample. Let me explain that using the next slide. (Fig. 15). Consider this black atom, implanted in a substitutional position in the lattice. We fire the beam into an open direction, the $\langle 110 \rangle$ direction for instance, and the beam will channel through and will not suffer from any violent collision with this atom. This means that we don't see backscattering from this atom. We will have a normal channeling effect on the yield from the substitutional impurity. This is not so for the other types of impurity shown. We can see those impurities, at least for certain directions, so we can, by measuring the yield and observing the reduction in yield in the open directions, pin down the lattice site of impurities.

○ - silicon lattice atoms

	Directional Effect			Impurity Location
	<111>	<110>	<100>	
●	yes	yes	yes	substitutional
▲	yes	no	yes	tetrahedral interstitial
X	no	no	no	off lattice site

Fig. 15

Making use of this technique, people have studied a large number of impurities in silicon, and also some in germanium and in metals. The big problem, though, is that by making use of Rutherford scattering one can study only those impurities that are heavier in mass than the substrate is. The reason is that if the impurity is heavier in mass, the proton cannot transfer as much energy to that object and will come out of the crystal at higher energy than if it had been scattered from a substrate atom. We can then resolve the impurity peak from those protons that have been backscattered by the substrate.

The next slide (Fig. 16) shows typical results for a silicon sample that has been implanted with antimony. The antimony apparently is sitting on silicon lattice sites for the larger part, because we observe for both antimony and silicon a large channeled minimum dip. On the other hand, by looking at

Fig. 16

the backscattering yield for a sample implanted with gold atoms, we learn that the gold is not sitting on lattice sites because we don't observe this channeling minimum with the gold impurity.

We wanted to show that x rays can be used for those cases where a lower mass impurity implanted in a heavier substrate is of interest, like for instance the germanium semiconductor. We have tried to locate the lattice site for phosphorus or sulfur impurities in germanium. That was an experiment we did at Chalk River recently. Before we could do a proper job, we had to establish empirically what the best beam was for this particular case; that means we had to establish peak-to-background ratios for the phosphorus and the sulfur signals in germanium.

We used a beryllium filter to reduce the germanium L yield by 6 orders of magnitude. We bombarded 0.5 MeV protons on

germanium; Fig. 17 is a spectrum of a germanium sample.⁸⁾ The small

Fig. 17

peak is the signal we get from 6×10^{14} phosphorus atoms per cm^2 implanted in a germanium crystal, and the peak-to-background ratio for 0.5 MeV protons on phosphorus in the germanium is $\sim 2\frac{1}{2}$. For helium this is slightly better, but only slightly better, at 1 MeV; it's a peak-to-background ratio of 3. At 1.5 MeV it's 2. For 1 MeV protons it becomes much, much worse. The background goes up so much that the peak-to-background ratio is only 0.1, so it is an advantage to work at lower energies. It is also an advantage to use protons rather than helium, first because the cross sections are higher, so bombarding times are much smaller, and also the projectile is lower in mass and consequently produces less damage in the crystal. You don't want to damage the crystal because that greatly affects the lattice site of the impurity. So, we used 0.5 MeV protons for this analysis.

The whole analysis was done with a total number of protons

of 10^{16} . We always checked the damage by looking at the backscattering yield. We didn't see a significant increase of the minimum yield, which told us that there was no significant radiation damage during the analysis. If you look at the backscattering yield and compare it with the germanium K and L x-ray yields, there is a difference in shape of the x-ray channel-dip compared to the backscattering channel-dip. The next slide (Fig. 18)⁸⁾ shows the

Fig. 18

signal if we scan across the $\langle 110 \rangle$ channel in germanium. The curve represents the channeling minimum for 0.5 MeV protons backscattered from the germanium sample. The channeling minimum is also observed with the germanium L and germanium K x-rays. You see that there is a difference, not so much in widths, but a considerable difference in the yields. For backscattering the minimum is 5%; for x rays that is 15%.

One can understand that because the backscatter spectrum for the random case looks like the top curve in Fig. 19.⁸⁾ For the channeling case, it looks like the bottom curve. You see that if one plots the backscattering yield at lower energies (that means for protons that have been backscattered from greater depth), this yield is not as low as it is for protons that have been backscattered at zero depth. That is simply because there is

Fig. 19

dechanneling occurring. As the particles move into the channel, they are scattered out of the channel by electrons and by surface impurities and by lattice vibrations, and therefore eventually are lost from the channeled fraction of the beam. With backscattering we can discriminate; we know where the protons come from because we have the energy spectrum. With the x-ray yield we cannot. The x rays are averaged out over a large depth, typically in this case $5,000 \text{ \AA}$ or so. That means the x-ray channeling minimum is an average over the channeling minimum for backscattering from the first $5000 \text{ \AA}$; thus it is a higher minimum yield than the backscattering from the surface. Backscattering gives the more significant number for us, because 40 keV phosphorus ions in germanium don't penetrate further than typically $250 \text{ \AA}$. We have to compare the phosphorus signal to the signals from protons backscattered from this depth, and not from a depth of $5000 \text{ \AA}$.

For the case of sulfur implanted in germanium we have made a scan through the $\langle 110 \rangle$ channel of the germanium sample, as you see in Fig. 20. The channeling minimum for the sulfur K comes

Fig. 20

down to a minimum yield of something like 0.43. If one analyzes this qualitatively, it tells us that a large fraction of the sulfur is sitting on substitutional lattice sites, but there is a fraction that is sitting on nonsubstitutional sites, and that causes this yield to increase. A scan through the $\langle 111 \rangle$ channel gave us a lower yield than for the $\langle 110 \rangle$. You see in Fig. 21 that the sulfur K yield is 0.35 for the $\langle 111 \rangle$. That's strange, because the $\langle 110 \rangle$ is the most open direction. Apparently the fraction of sulfur atoms not sitting in the $\langle 110 \rangle$ row is certainly not sitting outside the $\langle 111 \rangle$; they must be aligned along the $\langle 111 \rangle$, because we have a greater reduction in yield along the $\langle 111 \rangle$ direction.

The next slide (Fig. 22) shows a sketch of the diamond lattice. We have seen that a large fraction of the atoms are substitutional along the $\langle 110 \rangle$ row, but some are displaced outside that row. We have also seen that they are not so much displaced along the $\langle 111 \rangle$ row, so it looks like they are along this diagonal line 1-7-C-D-4, outside the $\langle 110 \rangle$. In order to find where

Fig. 21

Fig. 22

they are, we have analyzed the yields for the <100> direction, which showed a minimum yield higher than the <111>, but not as high as the <110>. That analysis with more quantitative data will

appear in a paper.⁸⁾ We were able to get a rather detailed picture of the site of the sulfur impurity in the germanium lattice.

For phosphorus, things were different.⁸⁾ In that case we found that there was no significant difference between the channeling minimum for backscattering and the channeling minimum for the phosphorus K, so the phosphorus is essentially 100% substitutionally implanted in the germanium. This was true for low doses of phosphorus, say 8×10^{14} per cm^2 . We also did a high dose, 3×10^{15} per cm^2 sulfur and phosphorus in germanium, and there were two things that were rather interesting compared to the low dose. First we analyzed without annealing; we implanted the samples, then put them into the target chamber and measured the x-ray signal and the backscattering signal. It told us that there was saturated disorder; that means we didn't observe any channeling minimum. Then we put the samples into an oven, annealed them at 400°C , and put them back into the target chamber. We found that we had lost a large fraction of our sulfur and phosphorus, because our signal had gone down. What had happened, we assume, is that the phosphorus and the sulfur precipitated at these high doses and diffused out to the surface. We etched the samples before we put them into the target chamber, and in the process we etched away the sulfur and phosphorus. The yield, we were left with, corresponded to a total amount of phosphorus or sulfur less than 1×10^{15} per cm^2 , roughly the same as we had implanted with the lower dose. We had lost more than 60% of the impurity. Still, after annealing we did not find the same lattice sites for the phosphorus or the sulfur. It appeared that the yields were nowhere near the minimum yields we found with the low doses. We found much higher yields, typically only a 55 to 60% reduction for the channeling direction. That means that there is a large fraction non-substitutional for both phosphorus and sulfur implants in germanium.

These results are similar to what has been observed with silicon. They are also similar to what people have observed so far for group 3 and group 5 implants in germanium for heavier

masses.

Let me summarize. We have seen that the high efficiency and the high sensitivity of the ion-induced x-ray technique are ideal for trace element analysis of very many samples outside physics: biological samples, air samples, water samples. One can also do a detailed solid state study, as I have shown here, making use of the channeling phenomenon. On the other hand, we need much more research on the background yields that are observed with these x-ray spectra, for two reasons. First, because I hope I have shown you that these non-characteristic x rays are interesting physics, and second because they are rather disturbing in the analyses.

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