HEAVY ION LECTURE SERIES

KANSAS STATE UNIVERSITY

1972-73

VOLUME II

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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 is 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 interest and support of Dr. C. E. Hathaway, Head, Department of Physics, and Dr. J. C. Legg, Director, Nuclear Science Laboratory, is gratefully acknowledged.

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Dr. H. Terry Fortune

Dr. Fortune, of the University of Pennsylvania, is noted for his extensive work in heavy-ion induced reactions, and the interpretation of multi-particle, multi-hole transfer processes.

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WHY SHOULD ANYONE STUDY HEAVY-ION REACTIONS?

Before talking about heavy ions, it is necessary to define what you mean by heavy ions, and different people think of different things when you talk about heavy ions. If you ask a chemist what is a typical heavy ion with which to do experiments, he'll probably think of bromine or iron or something like that. If you ask a physicist, he normally thinks of something like ^{16}O . Middleton has suggested that all things we physicists call heavy ions shouldn't be considered heavy ions, that that terminology should be given to things like sulphur and above.

Question: I don't know whether you are aware of it or not, but by an Act of Congress, heavy ion is $A > 4$.

Answer: Great.

Since there are no stable nuclei with $A = 5$, that of course is my definition; we shall consider lithium and heavier elements to be heavy ions. Middleton has suggested that protons through alphas be considered particles, lithium through oxygen are something called light ions, and sulphur on up are heavy ions. But we will consider lithium and anything heavier to be heavy ions. Before you can talk about what sorts of experiments one can do with heavy ions, or indeed whether it is worthwhile to do anything with heavy ions, it is important to consider how heavy ions differ from light ions - what are their distinguishing characteristics. There are several that obviously come to mind; the heavier ions have a higher charge and a higher mass. The fact that their charge is higher means that with most conventional ways to accelerate particles you can get higher energies. The fact that their mass is greater means that for a given fixed energy you have more angular momentum (more linear momentum, therefore more angular momentum).

The fact that you have both a higher energy and a higher mass means you get extra angular momentum in two ways, and that turns out

to be a very important feature of heavy ions. Also, something that should not be forgotten is that a heavy ion possesses more nucleons than a light ion, and you ought to be able to use that feature. Finally, the nucleons are in a shell other than the 1s shell, and that is extremely important for any heavy-ion reaction. Up to the alpha particle, where all particles are in the 1s shell, the motions within the projectile are very similar. All these effects should have observable characteristics, and it should be possible to use them to advantage.

There are two kinds of experiments that one can do with heavy ions. These are the kind that you can also do with light ions, and the kind that you cannot do with light ions. I will spend very little time talking about experiments with heavy ions that you could also do with light projectiles, but I will mention a few in passing. The experiments that you can do with heavy ions that you cannot do with light ions break into two broad categories of special interest and several other smaller areas. The first is the population of high angular momentum states using the fact that the heavy ion brings in a lot of angular momentum, and the second is the study of states made up of multi-particle excitations. You should be able to populate states by transferring large chunks of nuclear matter, if you have large chunks of nuclear matter available, and we will consider that second type of experiment first.

For the time being we will consider that ^{16}O is a good closed shell nucleus that fills the following shell model orbits:

The ground state of ^{16}O has the $1s$ and $1p$ orbits filled and all the orbits above that empty. If this is indeed the case, then if you add a particle to ^{16}O to make ^{17}O , you would expect to make only single-particle states. [I'll discuss a few features of light-ion reactions to begin with just to say how they would relate to heavy-ion reactions.] If you do a reaction such as $^{16}\text{O}(d,p)^{17}\text{O}$, you would expect that it is a direct transfer of a neutron to make states in ^{17}O that are a single neutron outside ^{16}O . You would expect to make a state by putting the neutron into the $1d_{5/2}$ orbital, or perhaps higher. The energy levels of ^{17}O are shown in Fig. 1.

Fig. 1

It is clear already that something is wrong; ^{17}O is not that simple, but there are states, namely the $5/2^+$ ground state, the $1/2^+$ first-excited state and the $3/2^+$ state at 5.081 MeV, that are largely single particle in character. If you indeed do $^{16}\text{O}(d,p)$, these three states dominate the spectrum, so you can study some of the states in

^{17}O by a reaction that transfers a single particle to ^{16}O . The other states are more complicated, and we will come back to them a little bit later.

You can also study multi-particle excitation by transferring more than one particle. For the sake of simplicity, we will talk about an ^{16}O final nucleus in all the different ways you can get to it. The ^{16}O ground state measured relative to an ^{16}O ground state is a zero-particle, zero-hole state and is zero plus (0^+Op-Oh). That's a closed 1s and p shell, and all other shells empty. You would expect ^{16}O to have low-lying states that correspond to taking the nucleon from the 1p shell and putting it up into the sd shell; those would be (1p-1h) states. If you take the particle from the $p_{1/2}$ and put it into the $d_{5/2}$, you get two states; couple spin 1/2 to spin 5/2 and you get spins 2 and 3, both negative parity. You can make another doublet of states by putting the particle into the $2s_{1/2}$ orbital; those are 0^- and 1^- , and so it goes. These (1p-1h) states are designated as:

$$(1d_{5/2})(1p_{1/2})^{-1}_{2^-, 3^-}$$

$$(2s_{1/2})(1p_{1/2})^{-1}_{0^-, 1^-}$$

Other sets of states could have particles in the $d_{3/2}$ orbit, several MeV higher, and the hole in the $p_{3/2}$ rather than the $p_{1/2}$ orbital.

How would you populate these states? Well, the thing they have in common is the hole in the $p_{1/2}$ shell. If you take ^{15}N , which in the simplest model is just a hole in the $p_{1/2}$ shell, and add a proton to it, say in a (d,n) or (^3He ,d) reaction, to make ^{16}O , then in a similar way to what we saw here, you make a certain set of states. What states do you make? Well, if you put in a proton, the proton clearly can only go to an orbit that has a vacancy. If it goes into the $1p_{1/2}$ shell, then that closes the $p_{1/2}$ shell and you make a (Op-Oh) state.

If you put the proton into another orbit, say the $d_{5/2}$ or $s_{1/2}$, you can make a (1p-1h) state. There is no way (if ^{15}N really is a hole in the p shell) to directly add a proton to ^{15}N and make a state in which

there is more than 1 particle in these orbits, since you added only one particle. So in that simple picture (1p-1h) states are the only states that you can make. Indeed, if you do a (d,n) or ($^3\text{He},d$) reaction, you find that the spectrum is dominated by the ground state and a certain set of negative-parity states.

E_{ex} (MeV)	J^π	Configuration	l	S
0	0^+	0p-0h		
6.05	0^+	4p-4h		
6.13	3^-	1p-1h	2	0.7
6.92	2^+	4p-4h		
7.12	1^-	1p-1h	0	0.4
8.87	2^-	1p-1h	2	0.9
9.60	1^-		0	0.02
9.85	2^+			
10.35	4^+	4p-4h		
10.95	0^-	1p-1h	0	1.8
11.08	3^+			
11.10	4^+			

Fig. 2

These are the low-lying states of ^{16}O . The ground state is the (0p-0h) state. And largely from a study of (d,n) and ($^3\text{He},d$) reactions, the low-lying (1p-1h) states are known to be those at 6.13, 7.12, 8.87, and 10.95 MeV. The 3^- and 2^- at 6.13 and 8.87 MeV would be the doublet corresponding to the $d_{5/2}, p_{1/2}$ hole, and the 1^- and 0^- at 7.12 and 10.95 MeV would be the $(s_{1/2}, p_{1/2})^{-1}$ configuration.

You might ask whether that is a quantitative statement or a qualitative statement. For example, there are two 1^- states at 7.12 and 9.60 MeV. Why is one considered to be a (1p-1h) state and the other one not? The quantity measured in a reaction like this (proton transfer) is the spectroscopic factor, which is a measure of the probability that the state looks like the target in its ground state and the transferred

particle coupled to it. The spectroscopic factors for these states¹⁾ are given in the column labeled S. Also, you see that if you make a state of this sort, you are starting with a target that's a $p_{1/2}$ hole and adding a $d_{5/2}$ proton to that. You add the proton into an $\ell = 2$ orbit, and indeed that's what is seen for the 3^- state; the spectroscopic factor is 0.7. The 2^- is also seen with $\ell = 2$; the spectroscopic factor is 0.9. The 0^- and 1^- states correspond to the transfer of $\ell = 0$ protons, and that's seen for the states at 7.12, 9.60, and 10.95 MeV.

The 1^- state could also be made by adding a $d_{3/2}$ proton. If it were, that would be $\ell = 2$, and you would see an $\ell = 2$ component in the angular distribution for this process. The absence of any $\ell = 2$ tells you that this is largely a $(2s_{1/2}, p_{1/2})^{-1}$ configuration. The spectroscopic factor for the 1^- is 0.4 and for the 0^- is 1.8. These numbers are large; I haven't told you how to scale this, but you can see that the spectroscopic factor for the 1^- state at 9.60 MeV is 0.02. The 7.12-MeV state is 20 times as good a single particle state as the 1^- state at 9.60 MeV, which must be of some other configuration. So, the (lp-lh) states in ^{16}O are identified by adding a particle to a nucleus that is a hole in ^{16}O , namely, adding a proton to ^{15}N . You notice that to make (lp-lh) states we must transfer one particle, but we are not restricted to making (lp-lh) states if we transfer one particle. We can make (lp-lh) and (0p-0h).

How would you make (2p-2h) states in ^{16}O ? Well, you do that by starting with some nucleus that has 2 holes in ^{16}O ; there are two possibilities. The two holes we will assume are in the $p_{1/2}$ shell, $(2p_{1/2})^{-2}$, and they can couple to spin 0 or 1, with positive parity. The 0^+ state must have isospin 1; the 1^+ must have isospin 0. The 0^+ $T = 1$ state is the ^{14}C ground state. The 1^+ $T = 0$ is the ^{14}N ground state. So there are two sets of (2p-2h) states depending on the isospin of the two holes. You would make those two sets of states by completely different reactions. You would make a set of states that had $T = 1$ for the holes by making a reaction in ^{14}C . You can make (2p-2h) states with $T(\text{holes}) = 0$ by adding two particles to ^{14}N , as for example the reaction $^{14}\text{N}(^3\text{He}, p)^{16}\text{O}$.

Suppose you add two particles to ^{14}N , which is, in the simplest

picture:

You can put both of the particles into the $p_{1/2}$ orbit, or one there and one into these empty s or d orbits, or both into higher orbits. Thus, you can add p^2 or $p(sd)$, or $(sd)^2$. If you add both particles to the p vacancies, you make the ground state, (Op-Oh). If you put one particle in $p_{1/2}$ and one particle in the $d_{5/2}$, you make the (lp-lh) states. If you put both particles in the sd shell, you make the (2p-2h) states. So you have added two particles to the 2 hole nucleus, and you make (Op-Oh), (lp-lh), and (2p-2h) states. It should then be obvious that if you see a set of states strong in the $^{14}\text{N}(^3\text{He},p)^{16}\text{O}$ reaction and not strong in the $^{15}\text{N}(d,n)^{16}\text{O}$ reaction, then that set of states is (2p-2h) to correspond to the two-particle excitations, since the states of these two configurations would not be populated in both reactions. You can also make (2p-2h) states by picking up two particles from ^{18}O . You can make (lp-lh) states by picking up a particle out of ^{17}O .

So, for the sake of studying nuclear structure, if you are interested in the shell model configuration for the states, you do it by transferring nucleons, one or more. To study (np-nh) states in a nucleus whose ground state is a closed shell (Op-Oh), you must transfer at least n particles. If you transfer fewer than that, you cannot make such a state. The configuration forbids the population of that state in a reaction that transfers fewer than n particles. You may also transfer more than n particles and still make the state, but that's an extra complication. You should transfer only as many particles as you need to make the state in question.

To make a (4p-4h) state, you must start with a nucleus that has 4 holes in it and add 4 particles to it, or you must start with a nucleus that has 4 particles outside of a closed shell and pick up 4 particles. For example, you would either add an alpha particle to ^{12}C (do a reaction that adds an alpha particle to ^{12}C) or do a reaction that picks an alpha particle up from ^{20}Ne . Fig. 3 is a spectrum from the

Fig. 3

$^{12}\text{C}(^6\text{Li},d)^{16}\text{O}$ reaction, which adds an alpha particle to ^{12}C , from Bethge and others.²⁾ The (4p-4h) states I alluded to are the 6.13-MeV 0^+ , the 6.92-MeV 2^+ , the 10.35-MeV state, and two 6^+ states at higher energies. Those are the states that dominate the spectrum; those are the (4p-4h) states in ^{16}O .

Fig. 4 shows a better resolution spectrum in the same reaction.

Fig. 4

You can see that one should not deduce too much from poor resolution data; that strong state at 6 MeV turns out to be half from the 0^+ state and half from the 3^- . This is very good indication of the fact that you make not only (4p-4h) states, but other states as well. But still the dominant states of any given spin are the (4p-4h) states. If the other states had a very simple configuration, then they could also be strongly populated, but most of these states have other pieces in their configuration. Again, these states are populated strongly in 4-particle transfer for two reasons. One is the fact that the 4-particle transfer reaction is a direct transfer of four nucleons, and second, the configuration of the states is largely (4p-4h). Both features must be present. The reaction mechanism must be such that it directly transfers the number of nucleons you are interested in, and the set of states must have a simple configuration so that there is a large probability for transferring those 4 nucleons to make that state rather than that strength being spread over a large number of states. This works well in ^{16}O and works to some extent in the other nearby nuclei, some of which are depicted on the next slide.

I have given this talk to a class of sophomores and engaged in audience participation - we'll see how you do. Fig. 5 shows ^{16}O with a few simple states labeled. The ground state of ^{16}O is a (0p-0h) state. The ^{20}Ne ground state is (4p-0h) measured relative to ^{16}O (4 particles outside of ^{16}O). In the simple model of ^{17}O , the ground state is (1p-0h) (1 particle outside of ^{16}O). These (4p-4h) states in ^{16}O are 4 holes in ^{20}Ne . They are a set of ^{20}Ne states with an alpha particle removed. Let's go to the extreme left and look at ^{19}F . The ^{19}F ground state is 3 particles outside of ^{16}O .

There is a set of low-lying negative parity states in ^{19}F that seems to be to a large extent a proton, p-shell hole in ^{20}Ne . If you take the 0^+ state of ^{20}Ne and remove from it a $p_{1/2}$ proton, you make a $1/2^-$ state. If you take the 2^+ state of ^{20}Ne and remove from that a $p_{1/2}$ proton (difficult to do experimentally, but easy to talk about) you make two states; you couple $p_{1/2}$ to 2^+ to get two states of spin and parity $3/2^-$ and $5/2^-$. Similarly, you can couple a $p_{1/2}$ proton hole to a 4^+ state of ^{20}Ne and get two states, $7/2^-$ and $9/2^-$, and so it goes. If you

Fig. 5

pull 3 holes out of ^{20}Ne , that puts you in the nucleus ^{17}O . The 3 holes in the $p_{1/2}$ shell must couple to spin and parity $1/2^-$ (since there is now 1 particle in $p_{1/2}$). So, you get (4p-3h) states with the same J^π values in ^{19}F . There is some indication that these states are known in ^{17}O . Now comes the audience participation bit.

To make this set of (4p-1h) states in ^{19}F , what sort of experiment would you do? Well, everyone knows the answer so I won't embarrass anybody; take a 1-hole nucleus and add four particles to it. A good 1-hole nucleus is ^{15}N , so you take ^{15}N and do a reaction that adds 4 particles to it, such as ($^6\text{Li}, d$) or ($^7\text{Li}, t$), which are alpha particle transfer. Just as you make the (4p-4h) states in ^{16}O by taking the nucleus that has 4 holes, namely ^{12}C , and adding 4 particles to it, you

make (4p-0h) states in ^{20}Ne by taking the nucleus of 0 holes, namely ^{16}O , and adding 4 particles. To populate or selectively pick out (4p-3h) states in ^{17}O you take a nucleus that corresponded to 3 holes in ^{16}O , namely ^{13}C , and add 4 particles to that. Indeed, those are the features seen in the simple alpha transfer reactions.

One would expect that if this analogy can be carried further, you might even be able to see states involving excitation of more particles. The ^{24}Mg nucleus is 8 nucleons outside of ^{16}O . That can be looked at as just 4 particles, an alpha particle coupled to states in ^{20}Ne . By analogy there should exist in ^{20}Ne a set of states that corresponds to an alpha hole in ^{24}Mg . If the alpha which you take from ^{24}Mg to make that set of states comes from the sd shell, you get the ground state band in ^{20}Ne . If it comes from the p-shell, there should be a set of states that has 8 particles in the sd-shell, 4 holes in the p-shell, and they should be roughly at 8 or 9 MeV. The evidence for them being (8p-4h) states we'll see in the next couple of days.

There are known to be states in ^{23}Na that correspond to states in ^{24}Mg with $p_{1/2}$ proton holes, and by analogy with the ^{17}O case, one would expect to see a similar thing in ^{21}Ne . Now the sophomores really shone. An (8p-4h) state in ^{20}Ne can be made in exactly two ways: by taking an 8-particle nucleus, namely ^{24}Mg , and pulling 4 particles out of it, or by taking a 4h nucleus, namely ^{12}C , and adding 8 nucleons to it. So, if such states exist in ^{20}Ne , and if there is in nature a reaction that transfers 8 nucleons, then you should be able to make this set of states by taking ^{12}C and doing the reaction that adds 8 nucleons to it, and similarly for these other sets of states.

I went through all this boring stuff just because we'll need it for the next several days. That's one supreme justification for doing heavy-ion reactions, to try to transfer a large number of nucleons in a direct way to look for states of such a simple configuration. (Even though in some models that would be a very complicated configuration, in this kind of picture it is a very simple configuration.)

There also exist in ^{20}Ne states that are not (8p-4h) states, but say (6p-2h) states, where you have elevated 2 particles from the p-shell into the sd-shell rather than 4. The (6p-2h) states in ^{20}Ne

could still be made by transferring 8 nucleons to ^{12}C if you put 2 of the nucleons in the p-shell and 6 in the sd-shell instead of putting all 8 in the sd-shell. You could make a (6p-2h) state by taking a 6-particle nucleus, for example ^{22}Ne , and pulling 2 neutrons out of that to make a (6p-2h) state. You could not make an (8p-4h) state by pulling 2 neutrons out of ^{22}Ne since an (8p-4h) state has 4 holes in it. To first order the ^{22}Ne nucleus has no holes in the ground state, and if you are removing only 2 nucleons, you cannot make an (8p-4h) state by pulling 2 neutrons out of ^{22}Ne .

But it is not enough to show that states are strongly populated in an 8-particle transfer reaction in order to identify them as (8p-4h) states. You must have other evidence in the same way that we discussed here. Two-particle transfer reactions on ^{14}N make also the (0p-0h) and (1p-1h) states, not just the (2p-2h) states. In order to see that there are (2p-2h) states, you would have to do a reaction with some other target, such as do one-particle transfer or do pickups to those states. And that concludes the justification for doing heavy-ion reactions to study multi-particle excitation.

The other important aspect of heavy-ion reactions is a look at high spin states. The heavy ion comes in with a lot of angular momentum since it has a lot of linear momentum. Let's take a hypothetical heavy-ion collision (namely $^{16}\text{O} + ^{12}\text{C}$) and a typical bombarding energy for the ^{12}C of 36 MeV. That corresponds to a center-of-mass energy of

$$E_{\text{cm}} = \frac{16}{28} 36 \approx 20.7$$

and a reduced mass of

$$\mu = \frac{16 \times 12}{28} \approx 7$$

The linear momentum in the collision, or rather, the momentum wave number, is just:

$$k = \sqrt{\frac{2\mu E}{\hbar^2}} \approx \sqrt{\frac{\mu E}{20.7}} \approx \sqrt{\frac{7 \times 20.7}{20.7}} = \sqrt{7} \approx 2.5 \text{ fm}^{-1}$$

(Now you see why 36 MeV lab is a typical energy!) The angular momentum for a grazing collision in such a process is just the wave number times the radius of the nucleus. The wave number k is 2.5 fm^{-1} , and for the radius we can use almost anything from 4 to 6 fm. So l is about 10 - 15.

$$l_{\text{graz}} = kR ;$$

for $R = 4 \text{ or } 5 \text{ or } 6 \text{ f}$,

$$l_{\text{graz}} \sim 10 - 15 .$$

So, in a collision of a 36-MeV carbon beam on an ^{16}O target, you would expect to have available to you enough angular momentum to make states up to spin 10, 12, or 15. This reaction would be ideally suited for a study of such high-spin states. One might, for example, look for states of high spin in ^{24}Mg by doing a reaction like $^{16}\text{O}(^{12}\text{C}, \alpha)^{24}\text{Mg}$. The alpha particle carries out a small amount of angular momentum, and ^{12}C brings in a lot, so you would expect to have left in your residual nucleus at high excitation energies a lot of angular momentum to populate high-spin states. All this makes sense only if such high-spin states are expected to exist.

If we consider the nucleus ^{20}Ne to be just 4 nucleons outside ^{16}O , and those 4 nucleons are all in the sd-shell, then what spins can we have? Well, the easiest way to figure that out is just to add up the magnetic projections by putting in all the particles that the Pauli principle will allow. The available orbits are $d_{5/2}$, $s_{1/2}$ and $d_{3/2}$. The largest magnetic projection is 5/2; you can put two particles there to get a maximum projection 5 - that's a neutron and a proton. The next projection is 3/2, and you can put 2 in there, giving a total projection of 3. With 4 particles, the maximum projection of spin you can get is 8, and therefore an $(sd)^4$ configuration would give you at most spin 8. So, you should be able to make in ^{20}Ne states of spin up to 8.

<u>Number of Nucleons</u>	<u>m</u>	<u>M</u>
2	5/2	5
2	3/2	3
		8

In ^{24}Mg (if it is considered an $(sd)^8$ nucleus) you have 2 particles in $m = 5/2$, to give you spin 5, 2 in the $d_{3/2}$ to give you spin 3, but you can put 2 more in the $m = 3/2$ coming from the $d_{5/2}$. That gives you another spin of 3 and gives 6 particles; you need 2 more. There are no $m = 5/2$ or $3/2$ vacancies left, so they must go to $m = 1/2$, and that's a total of 12.

<u>Number of Nucleons</u>	<u>m</u>	<u>M</u>
2	5/2	5
2	3/2	3
2	3/2	3
2	1/2	1
		12

So, within the sd shell in ^{24}Mg you would expect states to exist with angular momentum up to and including spin 12. In ^{20}Ne there should be states of higher spin than 8, but they are not of an $(sd)^4$ configuration. They have to come by putting particles in the $f_{7/2}$ shell or by elevating particles outside of the closed ^{16}O core, and the same is true in ^{24}Mg . States of spin higher than 12 have to be made by some configuration other than $(sd)^8$.

There is an important distinction between this set of states in ^{20}Ne and in ^{24}Mg . These states in ^{20}Ne are all hypothetically of $(sd)^4$ character, so if you take ^{16}O plus an alpha particle, that alpha particle, if it goes into the sd shell, carries in 8 quanta of excitation. (The sd shell is the second major shell in the harmonic oscillator; each nucleon therefore carries two quanta of excitation.) Four nucleons carry 8 quanta, so by adding an alpha particle to ^{16}O , you can carry in as much as 8 units of angular momentum. You can make all these states that have natural parity by adding an alpha particle to ^{16}O . If you did such a reaction, let's say $^{16}\text{O}(^6\text{Li}, d)^{20}\text{Ne}$, you would expect to make all the natural parity states of this configuration. That reaction has been done, and those states are all known.

In ^{24}Mg you need all eight of these nucleons to make these high spin states, so you take ^{20}Ne and try to play the same game. But you cannot make states of spin from 0 to 12 by adding an alpha particle to

^{20}Ne , because the ^{20}Ne ground state has 4 of those sd nucleons already coupled to spin 0, and the alpha particle can carry only 8 units of angular momentum if it goes into the sd shell. So this reaction makes the same spins in ^{24}Mg as in ^{20}Ne . In an alpha particle transfer reaction leading to ^{24}Mg , the highest spin you could make would be spin 8, and yet we see by a very simple argument that states of higher spin should exist.

These states are all unknown, and there has been quite a bit of investigation to look for the 10^+ and 12^+ states in ^{24}Mg . To make such states, you can look at it two ways. To make a spin 10 state by direct transfer of nucleons, you must transfer more than 4, since transfer of 4 makes only up to spin 8; so you would transfer 6 particles or 8 particles to try to make these things. Or, you can make these states in a reaction that does not obey the selection rules of the direct reaction.

These arguments that restrict the spin to ≤ 8 are valid only if the reaction is a direct transfer of 4 particles into the sd shell. If on the other hand the mechanism is one in which the target and the projectile amalgamate to form a compound system that lives for a while and then decays, these selection rules do not hold. All angular momenta are allowed, within limits. For example, in the $^{16}\text{O} + ^{12}\text{C}$ reaction, you make states of spin, say 12, in the ^{28}Si compound system, and then that state decays by emitting an alpha particle. That alpha could conceivably come out with 0 angular momentum, leaving the spin-12 state. So a reaction of this sort could make high spin states in two ways; by directly transferring 8 particles coupled to spin up to 12, or by a compound mechanism in which the reaction goes through a state of ^{28}Si of high spin, which then alpha decays to give you the state in ^{24}Mg . On the other hand, the $^{20}\text{Ne}(^6\text{Li},d)$ reaction will not make a spin-10 state, whatever the mechanism, because at typical energies the $^6\text{Li}-^{20}\text{Ne}$ system does not have enough angular momentum to make a state of spin 10, even if it's made in a compound reaction. So again, a heavy-ion process is useful to look for high spin states.

Question: Do you assume that the alpha particle comes out with zero angular momentum?

Answer: No, I don't. It should come out with many different

values.

Question: Not in this case?

Answer: O.K., fine. If you put in a typical bombarding energy and the Q value, the deuteron is coming out with 2 or 3 MeV of energy (at the region of ^{24}Mg you are interested in), so it has 0 - 2 units of angular momentum. The lithium plus neon at tandem energies carries in about 4 to 6 units, but if you have enough bombarding energy, you could conceivably bring in 8 units of angular momentum with the lithium; the alpha carries about 4, and in a compound mechanism they could couple to spin 12. In a direct reaction they would still couple to low spin. But the higher the energy, the less likely the process is to be compound. At least that is the way the argument usually goes. In fact, in $(^6\text{Li}, d)$ reactions no high-spin states have been seen that violate the direct reaction selection rules.

You notice I am making a distinction here between lithium and a heavy ion, too. In the present context I am not calling lithium a heavy ion. Both of these sets of experiments I have talked about are possible only with heavy ions. You cannot transfer 8 nucleons from a projectile unless you have at least 8 nucleons in the projectile. There are a lot of experiments that can be done with heavy ions that can also be done with light ions, but they are easier with light ions. You all know that there is a lot of single nucleon transfer that has been done with heavy ions. You bombard a target with ^{16}O and look at ^{15}N coming out; you've transferred a proton to the target. That's just like a (d, n) reaction in most of the important aspects. There are differences that arise from the fact that the proton from the ^{16}O came not from the 1s shell, but from the 1p shell, so it is useful to study those processes.

There are other reactions that are very hard to do with light particles, but can be done easily with heavy ions. One such example is 2 proton stripping; the analogous light ion reaction would be $(^3\text{He}, n)$. That is a difficult experiment to do just because of the difficulty in detecting the neutron. There are a lot of heavy-ion reactions that will allow you to do that. Probably the simplest would be $(^{16}\text{O}, ^{14}\text{C})$, and there have been a lot of studies of reactions of this type. Very little

is known about the 2-proton configurations of excited states of nuclei, and that information is obtainable in this way. A lot of information is known about the neutron configurations from the (d,p) reactions, but ($^3\text{He},n$) is very hard to do. There are two labs doing such reactions now, but they are very tedious and the results are only marginally worthwhile. An even harder experiment to do with light ions is 2-proton pickup. That would be analogous to the ($n,^3\text{He}$) reaction, and that is very difficult. There are a lot of ways to do that with heavy-ion reactions.

There are certain criteria that a heavy-ion reaction should satisfy in order to be useful. (1) The outgoing particle must be stable (it must be particle stable). Otherwise it separates before it gets to your detector. (There are few exceptions. We all saw the magnificent article by Robson³⁾ in which he says there are a few cases for ^8Be as an outgoing particle.) (2) The beam of the projectile must be available. (3) The structure must be simple. The (d,p) reaction would not be a good neutron transfer reaction if the deuteron were not a proton and a neutron, but had some other structure to it. (4) Just because of experimental difficulties, you would prefer that the outgoing particle come out in its ground state. If it comes out in the excited state, well, you know how it goes. You measure the excitation energy of the residual nucleus while looking at the laboratory energy of the outgoing particle. If the outgoing particle can also be excited, as well as the residual nucleus, then you don't know how that energy is shared between the residual nucleus and the outgoing particle. So the outgoing particle ideally should have only a ground state. That's the case with protons and deuterons and ^3He 's and alphas, but for other particles there should be no low-lying states. (5) Finally, the reaction should have a favorable Q value. What is meant by favorable Q value depends on what sort of reaction you are talking about. The (d,p) reactions with a Q value of -10 MeV could not be done with a deuteron energy less than 10 MeV, so it is obvious in that case, but for a heavy ion with a large Coulomb barrier, it isn't so obvious what is meant. Heavy-ion reactions are very sensitive to the Q value, and that dependence is quite different depending on what the reaction is.

We'll illustrate all these features with the 2-proton pickup reaction. Let's take nuclei that are heavy ions but also reasonably light

and run them through the above test to see whether they meet the requirements or not. The first projectile on the top of that list will be ${}^6\text{Li}$. If you use ${}^6\text{Li}$ as a projectile in the 2-proton pickup reaction, what one is talking about is ${}^8\text{B}$ coming out, which is particle stable, so it satisfies the first criterion. ${}^6\text{Li}$ is an easily available projectile, so that's o.k. The structure of the outgoing particle, as a decomposition into ${}^6\text{Li}$ plus 2 protons, is horrible for this particular case, so (3) is not o.k. The nucleus ${}^8\text{B}$ has no excited states that are bound, so (4) is o.k. And, the Q value for typical energies is very negative, and hence unfavorable.

The next reaction one might think about is $({}^7\text{Li}, {}^9\text{B})$. That fails for reason (1), so we don't need to talk about it any further. The next one would be $({}^8\text{Be}, {}^{10}\text{C})$ - that meets all the criteria except (2), but (2) is enough to kill it. For $({}^9\text{Be}, {}^{11}\text{C})$, it's possible, since ${}^9\text{Be}$ is stable; but it is not a very easy beam to get. For the $({}^{10}\text{B}, {}^{12}\text{N})$ reaction I guess ${}^{12}\text{N}$ is stable. The spin of 3^+ on ${}^{10}\text{B}$ and the nonzero spin in ${}^{12}\text{N}$ is already enough to make that one not worth considering for reasons like (3). The $({}^{11}\text{B}, {}^{13}\text{N})$ reaction has possibilities. Boron beams have been obtainable from cyclotrons for a long time and are now becoming available for the tandem with sources of the type that Middleton has developed. A boron beam is available, so (2) is o.k.; (1) is o.k. since ${}^{13}\text{N}$, in its ground state anyway, is stable. (3) is not o.k. Look at Cohen and Kurath's wave functions.⁴⁾ The structure between the two-particle configuration in ${}^{13}\text{N}$ decomposed into the ${}^{11}\text{B}$ ground state is not very good. The outgoing particle should have no low-lying states, and in ${}^{13}\text{N}$ only the ground state is stable. So point (4) is o.k. But, the Q value is not very favorable, and on and on.

The $({}^{12}\text{C}, {}^{14}\text{O})$ reaction meets all the criteria except the favorable Q value, and $({}^{13}\text{C}, {}^{15}\text{O})$ meets all the criteria that $({}^{12}\text{C}, {}^{14}\text{O})$ does, including the simple structure. The ${}^{15}\text{O}$ nucleus has a spectroscopic factor of 1.0 for the state ${}^{13}\text{C}$ ground state plus two protons. The Q value, though not completely favorable, is much more favorable than for $({}^{12}\text{C}, {}^{14}\text{O})$. So that has possibilities as a good 2-proton pickup reaction. The best possible 2-proton pickup reaction is $({}^{14}\text{C}, {}^{16}\text{O})$. It has not yet been studied since ${}^{14}\text{C}$ is radioactive. So is tritium, but it has been accelerated, so somebody someday may accelerate ${}^{14}\text{C}$. That would be an

ideal 2-proton pickup reaction.

The only other one I will mention is ($^{18}\text{O}, ^{20}\text{Ne}$). Of all the 2-proton pickup reactions that have up to now been studied, that one has the most favorable Q value under typical operating conditions at tandem energies, but it fails to be of much interest because of structure. If you take ^{18}O and add 2 protons to it to make ^{20}Ne , you have got to add a lot of 2 proton configurations to make the ^{20}Ne ground state. Decomposition of the ^{20}Ne ground state into ^{18}O plus 2 protons is not simple at all. The ^{20}Ne nucleus is highly deformed; ^{18}O is roughly spherical. Also, ^{20}Ne has a low-lying state at 1.6 MeV which turns out to be more strongly populated than the ground state in most reactions. So, of all the possibilities up to now that have been studied, ($^{13}\text{C}, ^{15}\text{O}$) turns out to have the most possibilities as a 2-proton pickup reaction. The Q value is not favorable with tandem energies; that will become less of a problem as ^{13}C beams of higher energy are produced.

All these things are logical and common sense, and I am just taking a little time to spell them out. In the next few talks I will discuss many of these reactions, tell you the nuclear structure information that one can get from such reactions, discuss which reactions are favorable from a direct reaction point of view, which reactions are largely of a compound nucleus nature, and what the important differences are. Both types of reactions can yield valuable information regarding nuclear structure. Then, in the last talk, I will discuss a few simple experiments, just to show you some of the things going on at Penn.

Question: I think I heard some noise about a ^{14}C beam at Los Alamos.

Answer: They keep saying that.

Question: They certainly can handle the radioactivity problem since they run tritium now.

Answer: Neither the cost nor the radioactive source is the problem, but the buildup on slits seems to be a big worry.

Question: Isn't there a small difference between tritium and ^{14}C radioactivity?

Answer: Tritium is going to become readily available with this

Middleton type source. You can run about 500 hours with the same amount of tritium that would run only an hour in a duoplasmatron source, so you can run for 500 hours on an amount of tritium that if you took it all into your system, there would be no health hazard.

Question: I am curious what the other slides show.

Answer: Oh, yes. Let me show them just for fun. All this stuff I have been talking about is completely worthless if in a given reaction you populate all the states. You might think if you transfer as many nucleons as 8 or 10 or 12, since an 8 nucleon transfer will make (8p-8h), (7p-7h), etc., you can make all the states. I just want to show you that isn't the case. These heavy-ion reactions are extremely selective. Fig. 6 is an example of residual states in ^{20}Ne . You see a high

as shown in Fig. 7, at a large number of energies. There is a

Fig. 7

"desert" in there; there is really nothing strongly excited.

Question: Do you get much ⁸Be out of the previous reaction?

Answer: Yes.

Question: You can also drop off 4 and get the ⁸Be coming out.

Answer: Yes, there is a large probability for that to happen.

Those cross sections are almost as large as the ones seen here.

Question: And it populates those states?

Answer: Yes.

Question: What happens with oxygen on carbon? Do you get carbon out and then populate those same (8p-4h) states?

Answer: The answer is that there is no obvious selectivity in $(^{16}_0\text{Be})$. It doesn't selectively excite 8-particle states

Question: How about $(^{16}\text{O}, ^{12}\text{C})$? Does it make only $(4p-4h)$?

Answer: Yes, (^{16}O , ^{12}C) does transfer four particles. This

reaction is largely selective because it's a direct reaction. The next slide (Fig. 8) shows you another case which is also highly selective

Fig. 8

for a completely different reason, as we will see in the next couple of days. All the selectivity observed in this reaction can be explained by purely statistical arguments in the compound nucleus.

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MECHANISMS OF HEAVY-ION REACTIONS: DIRECT OR COMPOUND

The purpose of this talk is to consider reaction mechanisms for a number of different heavy-ion reactions. I'll assume that there are only two dominant mechanisms: a direct transfer of nucleons, and a compound nucleus mechanism. Then the question is, for a given reaction to a given final state, which mechanism dominates? In order to study reaction mechanisms, it is necessary to do the reaction to a state whose structure is known so that you can unravel the mechanism, just as to study the structure of a given state it is necessary to populate that state with a reaction whose mechanism is known. We'll start with the lightest heavy-ion projectile and work our way up.

With lithium projectiles, there are a number of different reactions one could think of that might be direct. The most obvious one is alpha transfer. Fig. 1 is a spectrum from the $^{16}\text{O}(^6\text{Li},d)^{20}\text{Ne}$ reaction.¹⁾

Fig. 1

If this is a direct reaction, then the states populated should be those states that are of the structure ^{16}O plus an alpha particle. It is a highly selective reaction; there are many states that are strong, many that are weak. Compare the strength of the 10.26-MeV 5^- state with the

strength of the 8.45-MeV 5^- state, two states of the same spin and parity, at about the same excitation energy. Yet the strength is quite different, so the reaction is sensitive to the structure. Selection rules say that the spin and the parity of the state must be $(-1)^J$, where J is the spin of the state, since the alpha particle has zero spin. The J transferred must be equal to the l transferred, and it has been argued on this basis that these data, since the 6^- state is so weak, indicate this is a direct reaction. It does not populate unnatural parity states. However, the 6^- state is not of the same structure as the 5^- state. This set of states has only 1^- , 3^- , 5^- , 7^- and so on; the 6^- belongs to the set of states beginning with the 2^- , 3^- , 4^- , 5^- , 6^- , and this state is weak not because of its unnatural parity, but because it belongs to a structure that is not selectively excited in this reaction. That state is really no weaker than the 5^- .

Question: How does the structure of the 10.26 come in? Why is that so wide?

Answer: Oh, I should have mentioned that. Above 4.6 MeV or so, all the states are unbound to alpha particle emission, and so they have natural widths. The resolution here is good enough to see a natural width in many of these states.

Question: What is the resolution of the spectrometer?

Answer: The resolution is about 20 keV, and the widths of some of these states are a few hundred kilovolts.

Fig. 2 shows another alpha transfer reaction on the same nucleus at a little lower incident energy. Again you see that the 7.16-MeV 3^- state is very strong, and also the 5.78-MeV 1^- state, while the other negative parity states are very weak. Even though this alpha transfer reaction is direct to the strong states, the argument is that the absence of population of the unnatural parity states is for structure reasons and not just because it is a direct reaction. The 8.45-MeV 5^- , although it is natural parity, is also not populated in a direct way in this reaction.

The structure of these two sets of negative parity states is such that one should be populated strongly in alpha stripping, the other

Fig. 2

one strongly in alpha particle pickup. That is indeed what is observed.²⁾ These states that are strong in $^{16}\text{O}(^6\text{Li},d)$ are weak in the $^{24}\text{Mg}(d,^6\text{Li})$ reaction; these that are weak in $^{16}\text{O}(^6\text{Li},d)$ are strong in alpha pickup on ^{24}Mg . The unnatural parity states in the pickup reaction are weaker than the natural parity states, but they are 20 to 30% of the cross section of the natural parity states in that band. They seem to be populated by a mechanism in which the alpha pickup goes to one of these states that is allowed, followed by a de-excitation to the other state of that same structure. The mechanism is not compound because if it were, the relative excitation to these unnatural parity states would be the same in pickup and stripping. The fact that the population of the unnatural parity states depends upon the structure of the state means it is not a simple compound mechanism. For the strong states, the $(^6\text{Li},d)$ and $(^7\text{Li},t)$ reactions are good alpha transfer; for the weak states they're not.

It is possible to analyze angular distributions of these reactions using distorted wave calculations, and some examples are shown in Fig. 3. These are $^{24}\text{Mg}(d,^6\text{Li})$ angular distributions;³⁾ the curves are sort of the typical fits one gets if they are calculated with the parameters out of somebody's desk drawer. They are not fantastically good, but you see the difference in the ℓ value; there is a difference in

Fig. 3

shape in the angular distribution. The magnitude of the cross section gives you the spectroscopic factor, or the probability that those states have a structure of ^{24}Mg ground state minus an alpha particle.

For states reached in $^{16}\text{O}(^6\text{Li}, d)$ that are unbound with respect to alpha particle emission, we write:

You are transferring an alpha to a state in ^{20}Ne , and that state then decays by emitting an alpha particle. The width of that state is almost all alpha width. The gamma width is extremely small compared to the alpha width, so the state has a Γ_α which can be compared to an alpha

particle single-particle width to get another value of $S_\alpha = \Gamma_\alpha / \Gamma_\alpha^{s.p.}$. The alpha spectroscopic factor is the ratio of the alpha particle width of a given state to the alpha particle width a state would have if it were purely an alpha particle state.

Question: Those angular distributions - you're not claiming that you can tell an l value from them.

Answer: No, what I am saying is that there is a difference in the three curves which is caused by the difference in l values. I would not assign l values on the basis of the comparison of the data with the theory, but these are already known.

Question: Since the fits are so underwhelming in that angular distribution, how much faith do you put in their absolute cross section?

Answer: This is not an especially good case, but it is the only slide I have. But, I would think that the fit is not so bad. The agreement or disagreement is 30% or so at the stripping peak, so there is 30-50% uncertainty in spectroscopic factor, but that's typical. I'll show you though that there is quite a bit of sensitivity to the optical model parameters.

Question: This is what I was wondering. If the theoretical curves don't fit the shape, how can the absolute magnitude be correct?

Answer: I'll come to that, but first I want to just finish this point - you have two ways of getting the alpha spectroscopic factor, one from the transfer reaction and one from the width of the state, and those two quantities are the same quantity. If for all the states in ^{20}Ne , for which the $(^6\text{Li}, d)$ cross section is strong, the two numbers agree for all the states within 30%, you are doing the analysis correctly. The two techniques, which depend on quite different techniques of getting the number, do agree within 30%.

Question: Are you using the Vincent prescription⁴⁾ for stripping to unbound states?

Answer: Right, just applying it to alpha particle stripping instead of (d, p) or something.

Question: Doesn't the theoretical angular distribution offset the spectroscopic value?

Answer: Yes. Let me have the next slide (Fig. 4). The shapes

Fig. 4

of the angular distributions are very sensitive to what parameters one would use, especially for low l values. This is one of the worst cases I could find. The $l = 0$ has a shift of about 5° for two different sets of optical model parameters which give roughly the same elastic scattering predictions, so in this case one would not believe the spectroscopic factor extracted. However, as the curves are presently drawn, the spectroscopic factor from the two distorted wave calculations is the same. There is no good reason why it should be. If one does not believe spectroscopic information extracted from poor fits, then these numbers are not reliable. It is also the case that for the weak states the alpha spectroscopic factors deduced assuming a one-step alpha transfer

disagree with those extracted from the width of the state. Those extracted from the alpha transfer reaction are always larger, implying there is some other mechanism contributing to the weak states that is not present for the strong states. Given these alpha spectroscopic factors then, one can deduce the structure of the various final states, and we'll talk about that tomorrow.

Another set of reactions one can do with ${}^6\text{Li}$ is $({}^6\text{Li}, {}^3\text{He})$ or $({}^6\text{Li}, t)$, which, if it is direct, should be direct three-particle transfer. Fig. 5 is a spectrum of the ${}^{16}\text{O}({}^6\text{Li}, {}^3\text{He})$ reaction. The states

spectroscopic factors which agree within about 20% for all those states.⁵⁾ This was the first real evidence that it was the second $7/2^+$ state at 5.5 MeV in ^{19}F rather than the first one (at about 4.4 MeV) that was the member of the ground state band. So, we have 6 members of this rotational band and their spectroscopic factors agree within 20%, assuming that they are simple three-particle clusters outside of ^{16}O . Fig. 6 is the elastic

Fig. 6

scattering excitation function, which is relatively smooth. The solid and dashed lines are two different predictions using two different sets of optical model parameters,⁴⁾ both of which give the same shape for the angular distributions; they also fit the general overall slope of the excitation functions. Fig. 7 shows excitation functions for the three-particle transfer to four of the states in ^{19}Ne (which is the analog of ^{19}F). Data were taken with solid state detectors rather than a spectrograph, so not all states were resolved, but we know from the previous spectrum that for the unresolved doublets it is the ground state band member that is strong and the other states are very weak.

The solid and dashed curves are distorted wave predictions using for the ^6Li potentials the two sets of potentials⁶⁾ shown in the previous

Fig. 7

slide. For the triton parameters it's customary to use V_0 of around 170 MeV and r_0 of about 1.14. The normalization of the theory to the data is the same for all the states, again showing that the spectroscopic factors are about the same. There is very little structure in the data, indicating that it is largely a direct process. This poor fit to the $1/2^+$ state for one of the parameter sets is the same feature that you saw in the $l = 0$ angular distribution in the previous slide. This is an $l = 0$ transfer. One set of parameters fits the forward angles and the other one does not. For the other l values the agreement is rather good. Notice that these data gradually change with energy, a feature that is present in the calculations.

If the $({}^6\text{Li}, t)$ reaction is a good three-particle transfer, then you might expect $({}^7\text{Li}, \alpha)$ to be; i.e., if $({}^6\text{Li}, {}^3\text{He})$ transfers a triton; you might expect $({}^7\text{Li}, \alpha)$ to also transfer a triton. We studied $({}^7\text{Li}, \alpha)$

on a number of nuclei, and there is no obvious selectivity. The cross sections fluctuate rapidly with energy, and the results are completely different from the (${}^6\text{Li}, {}^3\text{He}$) results, so (${}^7\text{Li}, \alpha$) at our energies does not appear to be a good triton transfer reaction. Fig. 8 shows some

Fig. 8

data⁷⁾ from the University of Iowa for (${}^7\text{Li}, \alpha$) on ${}^{12}\text{C}$. The fluctuations are 200 keV in width at 14 MeV. We carried this data up to 24 MeV, and the fluctuations still continue, so even around 24 MeV the (${}^7\text{Li}, \alpha$) reaction is largely non-direct. The cross sections fluctuate with energy, and the strong states are not those you expect on the basis of their structure. So, (${}^7\text{Li}, \alpha$) is not a good triton transfer reaction, at least at these energies.

Question: Is there some reason for that?

Answer: Well, we like to think it is because the compound nucleus prefers to emit an alpha particle rather than a ${}^3\text{He}$. Protons and alphas are frequently emitted from compound nuclei.

Question: From the structure of the particle you might say (${}^7\text{Li}, \alpha$) would be a better transfer reaction than (${}^6\text{Li}, {}^3\text{He}$).

Answer: Yes, you would expect that, especially on the basis

of binding energy effects. But the structure is the same. If ${}^6\text{Li}$ is 100% $\alpha + d$, it is also 100% triton plus ${}^3\text{He}$. Those two configurations are not orthogonal just because they have a different number of particles in their decomposition. The structure of ${}^6\text{Li}$ as a triton and ${}^3\text{He}$ is all right, but the separation energy in ${}^6\text{Li}$ for triton and ${}^3\text{He}$ is about 12 MeV, whereas $\alpha + d$ is about 2 MeV. So, you would expect better kinematic matching for $({}^7\text{Li}, \alpha)$ say than for $({}^6\text{Li}, {}^3\text{He})$, but that turns out not to be the case.

Question: Are the cross sections here a lot larger?

Answer: $({}^7\text{Li}, \alpha)$ cross sections are an order of magnitude larger than the $({}^6\text{Li}, {}^3\text{He})$. So, a direct component of the same magnitude would not show up in the $({}^7\text{Li}, \alpha)$.

The fact that the $({}^6\text{Li}, t)$ and the $({}^6\text{Li}, {}^3\text{He})$ reactions are direct three-particle transfer means that if you do those reactions on a $T = 0$ target and a self-conjugate nucleus, you make states in $T = 1/2$ nuclei that are mirrors of each other. The reactions are highly selective, so a state that is strong in one nucleus and strong in another nucleus will obviously be a mirror pair. In many cases there are states very close together in $T = 1/2$ nuclei, and you don't know which states go together.

We have identified many mirror states using the two reactions $({}^6\text{Li}, t)$ and $({}^6\text{Li}, {}^3\text{He})$. The nucleus ${}^{19}\text{F}$ was very well studied, but not ${}^{19}\text{Ne}$. We made mirror identifications for all the states^{8,9)} up to 5 MeV in ${}^{19}\text{Ne}$, identifying a state in ${}^{19}\text{Ne}$ with the corresponding state of ${}^{19}\text{F}$. Fig. 9 shows a spectrum for the reaction ${}^{16}\text{O}({}^6\text{Li}, {}^3\text{He}){}^{19}\text{Ne}$ in which we found a new state in ${}^{19}\text{Ne}$ at 4.59 MeV; these are spectra at different angles. At one angle you wouldn't believe the state is there, but with data from all the angles it is clearly present. The strength of this state in the three-particle transfer reaction is such that in the mirror reaction $({}^6\text{Li}, {}^3\text{He})$ leading to ${}^{19}\text{F}$, there is only one place that the mirror of this state can be. Fig. 10 shows data for ${}^{19}\text{F}$ for the mirror of this state in ${}^{19}\text{Ne}$. There are only two places in this region of excitation where a state of the right magnitude of cross section could hide, and it's in these two doublets at about 4.0 and 4.6 MeV. Based on the fact that the lower group of states has a much larger cross section

Fig. 9

Fig. 10

than the corresponding mirror state in ^{19}Ne , we concluded that most of this strength had to be due to the other state and that the mirror of the new state in ^{19}Ne is in this doublet at 4 MeV. At the Washington meeting a month ago a group at Maryland¹⁰⁾ reported the γ decay of this doublet and of the new state in ^{19}Ne . They find that the other member of this doublet γ decays just like the new state in ^{19}Ne , so the mirror is in that region.

So, alpha transfer is direct with lithium beams. Three-particle transfer is direct as long as you don't look at $(^7\text{Li}, \alpha)$. It would be interesting to find a 6-particle transfer reaction. None has ever been observed; $(^7\text{Li}, p)$ offers a good possibility for transfer of 6 nucleons. It would be transfer of a ^6He , and would be $T = 1$ transfer. We have looked at a number of nuclei for a direct component to the $(^7\text{Li}, p)$ reaction with not good results. Fig. 11 is a spectrum¹¹⁾ measured on ^{14}N ;

Fig. 11

the lack of selectivity should be striking. You see every state known in ^{20}F up to 6.1 MeV and two new states that had never been seen before; there is no obvious correlation between the structure of the states and their cross sections. There is a very obvious correspondence between the strength and the spin. Fig. 12 shows the angular distributions for some of these states. They are roughly symmetric about 90° ; the scale here goes from 0 to 180° . They are not completely symmetric, but nearly so, and that is true for the other states not shown here. The largest

Fig. 12

cross section on here is $10 \mu\text{b/sr}$, so it is a very weak process. Angular distributions that are symmetric or nearly symmetric about 90° are indicative of compound nucleus processes. If the mechanism is compound, since the proton goes out with a very high energy, you would expect very little dependence on the outgoing proton energy. If the angular distributions are symmetric, the total cross sections integrated from 0 to 180° should be proportional to $2J + 1$, where J is the spin of the state.

Fig. 13 shows a graph of σ_{TOT} vs. $2J + 1$ for all of the states of known spin. These are all the positive parity states. The error bars are purely statistical, so the straight line indicates that there is very good agreement with the proportionality hypothesis. The proportionality between σ_{TOT} and $2J + 1$ is different for positive parity states and negative parity states. It has been observed before,¹²⁾ and I am not sure I understand the reason, but I would guess it has something to do with the density of states. It has been observed in a number of compound nucleus reactions that the magnitude of the cross section depends on the parity of the state for states of the same spin. The fact that this reaction is purely compound does not mean it is useless. It obeys the $2J + 1$ rule nicely, so it means that it can be used

Fig. 13

to assign spins in certain cases.

Fig. 14 shows a plot of σ_{TOT} vs. excitation energy for a

Fig. 14

number of low-lying states. You notice that three 1^+ states, which are separated by a large excitation energy, have the same total cross section. The bands drawn on here are obtained from the straight lines on the previous slide plus or minus one standard deviation. For the 0^- , 1^+ , 1^- , and 2^+ bands, all the states of known spin lie within the error bars of these bands. One member of this doublet at 1.8 MeV (at $\sigma_T = 90$), was already known to be 5^+ . On the basis of these data, we suggested that the other was a 2^- state. In the same session at Washington¹³⁾ this state was definitely assigned spin parity of 2^- . So again that agrees. The only funny state is the one at 2.865 MeV. It lies in between the cross section you would expect for any spin and may indicate that that state is a doublet. We have made guesses for some of the spins of the other states. If it is known the spin is either 2 or 4 say, then this would very definitely tell you the spin if you knew the parity, but one cannot tell the difference say between a 5^- and a 6^+ , or even sometimes between a 4^+ and a 5^+ .

Fig. 15 is a set of angular distributions from a ($^7\text{Li}, p$)

Fig. 15

reaction¹⁴⁾ on ^{13}C leading to ^{19}O . There is a big controversy in the literature concerning the spins of many of the low-lying states in ^{19}O , so we did this reaction to try to pin down some of those things, expecting it to be a compound nuclear reaction. We got the data and the spins did not agree with those in the latest compilation by Fay Selove.¹⁵⁾ We looked at (t,p) data from Middleton¹⁶⁾ that he took at Aldermaston and analyzed it with a distorted wave code using two particle wave functions from Wildenthal,¹⁷⁾ and the assignments from the (t,p) analysis agreed with the results we got from this study. Those results are shown in Fig. 16, where the cross section is plotted as a

Fig. 16

function of $2J + 1$. Three of these states, and I don't recall at the moment which three, are new spin assignments. With the old spin assignments the function would be very far off the data. Fig. 17 shows a plot of this cross section divided by $2J + 1$, as a function of excitation

Fig. 17

energy. The agreement is rather good, but not as good as the ^{14}N case.

There is one state at about 3.2 MeV where the (t,p) reaction did not yield an unambiguous spin. The two possibilities are 1/2 and 3/2, and on the basis of these data, we assign 3/2 to the spin of that state. The state is about 7 or 8 standard deviations away from the solid line, if the spin is 1/2. The spin 1/2 assignment came from an $\ell = 0$ (d,p) angular distribution without any forward angles. At the forward angles the state was obscured by impurities. There is a later $\ell = 2$ assignment,¹⁸⁾ which agrees with $j = 3/2$.

The other lithium-induced reaction that one might expect to be direct is $(^6\text{Li},\alpha)$. The $(^6\text{Li},\alpha)$ reaction has a positive Q-value; the outgoing particle has less mass than the incoming particle, so there is very good momentum and angular momentum match, and you might expect that to be a very good two-particle transfer reaction. If it is, the two particles that are transferred are a deuteron ($S = 1$, $T = 0$). The

only other comparable way to study such a transfer is (α, d), which transfers a deuteron but has a very bad momentum match due to the very negative Q-value. If (${}^6\text{Li}, \alpha$) were direct deuteron transfer, it would be an interesting reaction to study. We have looked at it on a number of light nuclei, and at our energies the process is not largely one of two-particle transfer. The selectivity is not what one expects from the two-particle structure in the states; the excitation functions fluctuate whereas selectivity is not simple.

So that concludes the discussion of the reactions you would expect to be able to study lithium projectiles. Alpha transfer is largely direct; three-particle transfer with ${}^6\text{Li}$ projectiles is largely direct. Six-particle transfer cannot be studied with a lithium projectile, and neither can five-particle transfer. The things I have said about (${}^7\text{Li}, p$) also hold for (${}^6\text{Li}, p$), and (${}^6\text{Li}, \alpha$) is not a good two-particle transfer reaction at tandem energies. It might be if one had 50 MeV lithium instead of 20 or 30 MeV.

I'll turn now to (${}^{12}\text{C}, \alpha$) reactions. Fig. 18 shows a spectrum

Fig. 18

of ${}^{12}\text{C}({}^{12}\text{C}, \alpha){}^{20}\text{Ne}$ that I showed you yesterday, at 30 MeV. This reaction

is clearly selective. It was studied 10 or 12 years ago to the low-lying states, analyzed in a statistical way, and it was shown that the cross section was a largely compound nucleus process at those energies. All the angular distributions and excitation functions were consistent with the dominant mechanism being one of compound nucleus formation and decay. Such a statistical argument would not explain the selectivity in this reaction. Fig. 19 shows that the selectivity continues as you

Fig. 19

change the energy. The same states that dominated at one energy tend to dominate over a wide range of bombarding energies. That cannot be explained in a statistical way. There is some gross energy dependence in this process. At low energies, low-spin states dominate. At high energies, high-spin states dominate, and that can be seen from the next series of slides.

Fig. 20 shows a spectrum obtained at 18 MeV and at $1\frac{1}{2}$ degrees.

Fig. 20

The strongest state in the spectrum is the 0^+ state at 7.20 MeV. The next strongest states are 1^- and 2^+ . The high-spin states of 6^+ and 4^+ are very weak. You see also that the 7.20-MeV 0^+ state is much stronger than the 6.72-MeV 0^+ state. The 7.83-MeV 2^+ state is also much stronger than the 7.42-MeV 2^+ state. These strong states are the ones we referred to yesterday as the (8p-4h) states. They should be selectively excited in this reaction. The weaker states are not of such simple configurations.

Fig. 21 is a spectrum at 25 MeV at $3\frac{3}{4}$ degrees. I haven't shown you much of the spectrum, but the strongest state is the 2^+ state; it is much stronger than the 3^- and much stronger than the 0^+ . Again the selectivity is obvious. The 2^+ state at 7.83 MeV is stronger than the one at 7.42 MeV. The 0^+ at 7.20 MeV is stronger than the one at 6.72 MeV.

Fig. 22 is taken at 33 MeV, at which energy the 4^+ dominates over all the other states including high spin states ($6^+, 5^-$). This gross energy dependence is displayed in Fig. 23, which shows excitation functions for the 0^+ , 2^+ , and 4^+ states. These states should be populated in a direct eight-particle transfer reaction because they have a structure consistent with such a direct transfer. And you see that the region of maximum cross section shifts to higher bombarding energies for higher spins. We have taken data at Argonne now all the way up to 48 MeV, and all the cross sections fluctuate, which must be explained before one can

Fig. 21

Fig. 22

Fig. 23

claim this is a direct reaction. The gross energy dependence is the large sort of envelope, which can be explained with a very simple argument, the essence of which is depicted in Fig. 24.

These are transmission coefficients calculated in an optical model potential;¹⁹⁾ the carbon-carbon potential was taken from fitting elastic scattering data, and the $(\alpha + {}^{20}\text{Ne})$ is a global alpha particle potential. The transmission coefficient is plotted vs. L ; the L -value corresponding to grazing incidence occurs where the transmission coefficient is 0.5. Remember that I said that around 27 MeV and for a Q -value corresponding to $E_x = 7.8$ MeV, the carbon can bring in two units more orbital angular momentum at grazing incidence than the alpha can carry out. So, at 0° where the angular momentum must be co-linear (since the k -vectors are co-linear) you would expect a 2^+ state to be more strongly

Fig. 24

excited than at any other energy. At lower or higher energy, the cross section should go down. Somewhere around 33 MeV, and at a slightly more negative Q-value the carbon brings in 4 more units of orbital angular momentum than the alpha carries out. For a state of negative Q-value, there is no bombarding energy for which the carbon brings in the same amount of angular momentum that the alpha carries out. It is true that as you do go down in energy, these two curves get closer together, so the 0^+ state should begin to be selectively excited for low bombarding energy. This is described in a little more detail in Fig. 25, which shows why these things happen.

The grazing L-value plotted as a function of the center-of-mass energy has a different slope for the two channels, so as you change the excitation energy of the states you are looking at, you are moving the " $\alpha + {}^{20}\text{Ne}$ " curve up and down. If you change the bombarding energy,

Fig. 25

you move the " $^{12}\text{C} + ^{12}\text{C}$ " curve. So, you get different differences in the grazing L as you change either the excitation energy or the bombarding energy.

In Fig. 26 we have taken the difference in the incoming

Fig. 26

L-value and the outgoing L-value that correspond to grazing incidence and plotted those as a function of excitation energy for a wide range of bombarding energy. Remember that at 0° in a classical one-step transfer of eight particles, if the transfer takes place at a point, this is the only J that can be populated at 0° . Quantum mechanically, this would be the state whose cross section is largest at forward angles, at this excitation energy and this bombarding energy. For example, take a 2^+ state, at around 7 or 8 MeV of excitation, and you see that that state should be strongest somewhere between 25 and 30 MeV of bombarding energy. A 4^+ state at around 9 or 9.5 MeV should be strongest around 35 to 40 MeV. That's what is observed. A 0^+ state at 7 MeV would be excited most strongly at an energy below 15 MeV, but by the time you get that low, you are below the Coulomb barrier, and all bets are off. This vertical bar on each curve is the excitation energy for which the alpha cannot make it out through the Coulomb barrier. These arguments are purely kinematical, just matching of angular momentum, but they do explain the gross energy dependence in the cross section. Those arguments are not what one would expect if the mechanism were compound. There the L-values can add in any way to give you the final J. It is only in a direct reaction that they add collinearly at 0° .

Question: Is there any correlation between (for any of those states) what you see there and what you see in elastic?

Answer: The bumps you mean.

Question: Yes, you know the resonances.

Answer: I'll come to that - the answer is no.

Fig. 27 shows the angular distributions for these three states that are 8-particle, 4-hole (8p-4h). The 0^+ state has a strong forward peak; the 2^+ state is forward peaked but less strongly. The 4^+ state is peaked at a non-zero angle, just what you would expect for any direct reaction. These shapes do not change drastically as you change the bombarding energy; these shapes are not observed for other states of the same spin and parity that are not of this configuration, as shown in Fig. 28.

Fig. 27

Fig. 28

These are angular distributions for two 2^+ states at very nearly the same excitation energy and at two bombarding energies. Although the magnitude changes, the shape of the angular distribution stays roughly constant and is a forward-peaked angular distribution for the 7.83-MeV state, which is the $(8p-4h)$ state. For the 7.42-MeV state,

the angular distribution is highly oscillatory and changes rapidly with energy.

Fig. 29 is the excitation function for the 7.83-MeV 2^+ state

Fig. 29

again, just demonstrating that if one does a simple-minded distorted wave calculation, the gross energy dependence is reasonably well reproduced.²⁰⁾ The fine structure is not; the gross structure is what you would expect from simple kinematics. Whatever a distorted wave calculation does right, it certainly gets the kinematics right, i.e., the matching of the ingoing and outgoing momentum. So, the gross features are correctly accounted for by DWBA. The fine structure, the oscillations, are not what one would expect from a direct reaction. A Phys. Rev. Letter by Noble,²¹⁾ which contended that these oscillations could be obtained from a direct reaction calculation, is not true; that calculation is now known to be in error.

Fig. 30 shows the excitation functions in smaller energy steps for some of the low-lying states. These states have been looked at before with this reaction, and analyzed with a statistical compound nucleus model and shown to be purely compound.²²⁾ The 7.83-MeV state is the

Fig. 30

(8p-4h) state. The type of fluctuation one sees for this state is about the same width and about the same spacing as for the low-lying states. The difference is that the cross section for the 7.83-MeV state has underlying it a very broad slowly-changing component that is absent for the other states. The peak-to-valley ratios for it are small compared to these peak-to-valley ratios for the other states, so there is a much larger nonfluctuating component in 7.83-MeV cross section than in the others.

In fact those statistical model calculations I referred to, which were done by Eric Vogt,²²⁾ predicted that the cross section for the 1.63-MeV 2^+ state, averaged over energy, should be 4 times that for the 7.83-MeV 2^+ state. In actual fact the ratio is 2.5 in the other direction. The average cross section for the 1.63-MeV state is about 4 mb/sr, so the fluctuating part of the cross section to the 7.83-MeV

state should be about 1 mb/sr on the average. Or, 8 mb/sr of the 7.83-MeV state cross section would correspond to a direct reaction, if Vogt's statistical model is correct.

The claim now is that for the 7.83-MeV state and other states of the simple (8p-4h) structure there is a large direct component to the cross section, with the same fluctuating compound nucleus cross section that is present for all the states. There is no reason why the compound contribution should be absent just because the direct component is there. You should have the same compound contribution independent of the structure of the state. The direct component that is implied by the structure of the state then just adds to the compound contribution, and they interfere at certain energies to give you rapid oscillations, but in general the oscillations are similar to those you get for the low-lying states. The conclusion is that although the ($^{12}\text{C}, \alpha$) reaction is direct 8-particle transfer, it is direct 8-particle transfer only to selected states, those states whose configuration allows direct 8-particle transfer to take place. For most of the states in ^{20}Ne , the reaction is simply compound.

We have looked elsewhere for multi-nucleon transfer, such as 8-nucleon transfer, 12-nucleon transfer, and one such search is shown in Fig. 31. This is a spectrum of $^{16}\text{O}(^{12}\text{C}, \alpha)$, and it is selective;

Fig. 31

certain states stick out above the general background of all the other states. There are two possibilities for this reaction. Putting that in a diagrammatic way, the ^{12}C and the ^{16}O come in, and either 12 particles get transferred across and leave an α and ^{24}Mg , with the α particle from the ^{16}O , or one has an 8-particle transfer with the alpha particle coming out of the ^{12}C .

If direct 12-particle transfer takes place, then the alpha that comes out would be going in the direction of the ^{16}O . So, it is possible to look for direct 8- or 12-particle transfer reactions just by looking at the shape of the angular distribution and whether or not it is symmetric about 90° . Considering the reaction in this manner, the 8-particle transfer would be forward peaked, and the 12-particle transfer would be backward peaked. Twelve-particle transfer would populate (12p-4h) states, since we are starting with 4 holes; 8-particle transfer would populate 8-particle states. The 12-particle transfer could also populate (12p-2h) and (8p-0h) states. The direct transfer of 12 particles can populate sets of states that direct 8-particle transfer cannot, though there is a set of states that should be populated strongly in both. So the question immediately arises, is this reaction compound or direct? Or neither?

Fig. 32 shows angular distributions for these three states that are strongly excited, and they are forward peaked, as long as one doesn't look past 90° . They look roughly like what one would expect for the direct reaction. (Does everyone know why we were allowed to look only at $\theta \leq 90^\circ$ for the other reaction? In the $^{12}\text{C}(^{12}\text{C},\alpha)$ reaction we have two identical particles in the incoming channels, so those angular distributions are guaranteed to be symmetrical about 90° .) So these angular distributions may or may not be symmetric; they do have a hint of a forward peak, which a direct reaction would have, but any compound

Fig. 32

reaction for a state of high spin will be forward peaked, but it will be backward peaked as well. It is known that the cross sections for all these states fluctuate rapidly with energy from data taken at Penn,²³⁾ Yale²⁴⁾ and Argonne.²⁵⁾

Fig. 33 shows data taken by the Argonne group.²⁵⁾ Oscillations are rapid for all these states, and widths are a few hundred kilovolts, so there is a large non-direct component. Is there any direct component? If one averages over a sufficient number of these oscillations, and if the mechanism is largely compound, the angular distribution should be symmetric about 90° . If there is a direct component, it will be slowly varying in energy, so averaging over energy does not wash out any of the direct component. If there is a direct 8-particle transfer, the angular distributions would be forward-peaked. If it is direct 12-particle transfer, after you average over energy you would have a backward peaked angular distribution.

Fig. 34 shows energy-averaged spectra taken at forward and backward angles. They were actually taken by interchanging target and projectile. The ($^{12}\text{C}, \alpha$) reaction is a forward-angle spectrum, and the ($^{16}\text{O}, \alpha$) reaction is a backward angle spectrum. These spectra were taken by varying the bombarding energy over a 1.5 MeV range, so these are the energy averaged spectra with an energy-averaged width of 1.5 MeV, yet the resolution is about 40 keV. Good resolution was maintained by changing magnetic fields in the spectrograph every time the beam energy

Fig. 33

Fig. 34

was changed. We have a state at 9.28 MeV in the ($^{16}\text{O}, \alpha$) reaction (a backward angle spectrum) with a cross section much larger than for ($^{12}\text{C}, \alpha$), while for most of the states, the cross sections are comparable. So, we thought we had found a (12p-4h) state. It is backward peaked, just what you would expect for 12-particle transfer, but we didn't go to press - we took more data.

Fig. 35 shows the angular distributions you get after you

Fig. 35

extend the energy averaging over a wider range (the lines are to guide the eye). As far as one can see with the naked eye, the angular distribution for the 9.28-MeV state, as well as for all the others, is roughly symmetric about 90° . There are slight deviations, but nothing large enough that one would want to say there is a direct component to any of these angular distributions. They are all essentially symmetric about

90° , so in this case there is no evidence for direct multi-nucleon transfer.

If a mechanism is purely compound, then one can calculate the angular distribution one would expect for energy-averaged data simply with a Hauser-Feshbach²⁶⁾ technique. One describes the incident and exit channels with the optical model, describes the decay statistically, and puts in some level density formula for the density of states in the compound system. Greenwood *et al.*²⁵⁾ at Argonne and Stockstad²⁷⁾ at Yale have done Hauser-Feshbach calculations, and Stockstad's results are shown in Fig. 36. You can see that the calculations go through the

Fig. 36

general trend of the data, although they do not correctly describe it in all its detail. The two different curves are simply weak and strong absorbing potentials. Although there are many parameters in the Hauser-Feshbach calculations, all those parameters were fixed by doing

calculations for the same reaction to the ground state, 1.37-MeV 2^+ , and 4.12-MeV 4^+ states in ^{24}Mg . Then all those parameters (the level density formula, the spin cutoff parameter and all the alpha cutoff parameters) were held fixed. One would conclude that the Hauser-Feshbach is not in bad agreement with the data. Stockstad assures me that now that he has got the data, he could fit it much better, so it is largely a statistical compound nucleus process, with no obvious direct component.

It is now easy to understand why those other states are forward peaked. If they have high spin, and I'll tell you tomorrow that they do, the angular distribution should look like $1/\sin\theta$.²⁸⁾ Fig. 37

Fig. 37

shows the angular distributions for those same three states I showed you earlier (Fig. 23) with the $1/\sin\theta$ curve drawn in, and they roughly follow $1/\sin\theta$, so the forward peaking is consistent with the compound picture. At backward angles one would then expect the curve to go up symmetrically and be sort of U shaped. If we get a little higher beam energy, we'll take the back-angle data.

The other strong prediction of the Hauser-Feshbach calculations is that if you make these states in another way that goes through the same compound system, the cross sections will be quite different. This incident channel is ($^{16}\text{O} + ^{12}\text{C}$). The incident channel that is

most different from that one and still makes the same compound system is ($^{14}\text{N} + ^{14}\text{N}$), two odd-odd nuclei, very loosely bound compared to carbon and oxygen. The predictions were that that reaction should have cross sections down two or three orders of magnitude from the $^{16}\text{O}(^{12}\text{C},\alpha)$ reaction. The spectrum is shown in Fig. 38, and indeed the cross sections

Fig. 38

are very small. There is no state here where the cross section is as large as 1% of the cross section observed in the $^{16}\text{O}(^{12}\text{C},\alpha)$ reaction. Furthermore, the Hauser-Feshbach calculations predict that if you go up in bombarding energy, this strength seems to disappear. It moves to higher excitation energies, and that is shown in Fig. 39.

Fig. 39

This is the same compound nucleus energy as that for the spectrum I showed you from $^{16}\text{O}(^{12}\text{C},\alpha)$, and there is no hint of any state at all anywhere. That is qualitatively what you would expect from a compound nucleus picture, although the cross section observed is less than that predicted by the Hauser-Feshbach. So, there is no obvious direct component to the $^{16}\text{O}(^{12}\text{C},\alpha)$ reaction. There are a number of possibilities for there being no direct reaction. One is that the (12p-4h) states are not in the region we have looked at, but maybe are higher in excitation. If they are elsewhere, then we don't know whether they are populated directly or not. We remember that in the ^{20}Ne case, only very few states were populated directly, those with the simple structure, so maybe we haven't found those states yet. There is no evidence as to where they should be experimentally. Theoretically they are expected to be around 6 or 8 MeV. It may be that such states exist, but the strength is spread out over a number of different states, so no one state has most of that structure.

The explanation that I prefer is the one which compares the magnitude you would expect from a direct reaction with what you would expect from a compound reaction. If you remember the ^{20}Ne case, the cross section for the 7.83-MeV 2^+ state was 8 times as large as what you would expect from a compound mechanism. In this case the magnitudes are comparable. The compound reaction is even slightly larger than the cross section you would expect for the direct reaction, so the direct reaction may be simply swamped by a compound contribution. If that's the case, then by going to a much higher bombarding energy, the direct component will begin to stick out. The reason it's the case here is it would be lower in energy relative to the Coulomb barrier than we were in the carbon-carbon case. If we could go to 50 or 60 MeV, then the direct component, which drops off very slowly with energy, would begin to emerge from the compound component, which goes like an exponential decrease with energy. I think that is a more likely explanation, that the compound contribution swamps the direct component at these energies, and at higher energy we should begin to see a direct component if there are any such states around.

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NUCLEAR STRUCTURE INFORMATION FROM HEAVY-ION REACTION STUDIES

I am now going to try to convince you that one can learn some things about nuclear structure from heavy-ion reactions, and I will talk mostly about nuclei around ^{20}Ne . Those nuclei have the property of being highly deformed. The spherical shell model does not give a good description of those nuclei, and one can use a Nilsson diagram.¹⁾

Fig. 1

The fact that these nuclei are deformed means that the degeneracy in the M substates is split, and in particular there is a $1/2^-$ substate which comes down from the $f_{7/2}$ very sharply at large deformation. For a δ of about 0.4, the $1/2^-$, $1/2^+$, $3/2^+$, and $1/2^-$ orbits are all about equally spaced. The $1/2^-$ Nilsson orbit from the $p_{1/2}$ is the orbit that is being filled as you go from ^{12}C to ^{16}O . (See Fig. 2) Underneath

these is a ^{12}C , not drawn. The ^{20}Ne ground state fills all orbits up

Fig. 2

through the $1/2^+$, and so it goes.

You can make excited states in ^{20}Ne a number of ways. You can promote the four $1/2^+$ particles up to the $3/2^+$ orbit and make a 0^+ state. That's a second 0^+ state above the ground state, and it is a four-particle state, an $(sd)^4$ state. These four particles are still in the sd shell. You can instead excite the 4 $1/2^-$ particles to the $3/2^+$ orbit, and that has eight particles in the sd shell and four holes in the p shell. So that is our old friend, the $(8p-4h)$ state. Or, you could excite either 2 or 4 of the $1/2^+$ particles up to the next $1/2^-$ orbit, and you would have a state which is $(sd)^2(fp)^2$ or $(fp)^4$. In Fig. 3 are shown states in ^{20}Ne . The

Ground State of ^{24}Mg

0^+ Band-Heads in ^{20}Ne

Fig. 3

6.72-MeV state is an excited sd^4 state, the 7.20-MeV is the (8p-4h) state, and the 8.6-MeV is a state that would be a four-particle state, but with the four particles in the fp shell.

Because of the tight binding of an alpha particle, excitations of this type are preferred over those with particles distributed in all of these orbits. The internal binding of the alpha is about 20 MeV. If you take an ^{16}O core, and do alpha transfer on it, you would expect to make all of these states except the 7.20-MeV state.

The ground state of ^{24}Mg in the Nilsson model is shown at the top of Fig. 3. If you picked up an alpha particle from ^{24}Mg , you could make the ^{20}Ne ground state by picking up the $3/2^+$ particles. You could make the 6.72-MeV state by picking up the $1/2^+$ particles, and you could make the 7.20-MeV state by picking up the $1/2^-$ particles. But you could not make the 8.6-MeV state because it has four particles in an orbit which is unfilled in ^{24}Mg .

You can make negative parity states in the same way by promoting one particle at a time, and those are shown in Fig. 4. There are two

Fig. 4

simple ways to make negative parity states in ^{20}Ne . One is to promote a particle from the p shell up to the lowest levels in the sd shell.

The minimum spin you can get is the K value, and the K value is obtained by algebraically adding the K values for the two orbits. If we add $3/2$ and $1/2$, we can get K values of 1 and 2. It turns out that the lowest band head is the one with $K^\pi = 2^-$. This is the state at 4.97 MeV in ^{20}Ne . That's a five-particle one-hole state. We can also make a negative parity state by taking one of the particles in the sd shell and promoting it to the fp shell. That is a four-particle state; but is a four-particle state with particles distributed over two major shells. The lowest sd-shell orbit has $K^\pi = 1/2^+$. The lowest fp-shell orbit has $K^\pi = 1/2^-$, so the lowest band you get has a K^π of 0^- . A rotational band with K of 0^- has only odd spin states, 1^- , 3^- , 5^- and so on. The 1^- state is at 5.78 MeV. The 4.97-MeV band has all spins from 2 on up until you get to the cutoff that is determined by the shell model. Again, if you take an ^{16}O core and transfer an alpha to it, you can make the 5.78-MeV state because it contains the undisturbed ^{16}O core. You cannot make the states in the 2^- band by transferring an alpha to ^{16}O because those states do not contain an ^{16}O core.

At the top of Fig. 4 again is the ^{24}Mg ground state. You can make states in the 2^- band by picking up an alpha if you make it up by picking up three particles from the $3/2^+$ orbit and one from the $1/2^-$, but you cannot make the 5.78-MeV state that way since it has a particle in an orbit that is unfilled in the ^{24}Mg ground state.

Question: What is the sort of magnitude of overlap for ^{20}Ne and an alpha particle? You talked about them as being alphas where you have four single nucleons in two different Nilsson orbitals.

Answer: It is embarrassingly close to one. It is surprising that if you have 4 particles distributed on two shells like this that you can get such a large overlap with an alpha particle. The experimental numbers for S_α are between .7 and 1. That is not what one was led to believe many years ago by the cluster model people, but the people who do shell model calculations and then transform to the center of mass of the alpha relative to a core get overlaps close to 1. These are calculations of McGrory²⁾ and Arima.³⁾ It is very surprising, but it turns out to be required by the data. That data existed when they did the

calculations. In fact, some of the data are shown in Fig. 5. On the

Fig. 5

left are the low-lying states of the 4.97-MeV band in ^{20}Ne , 2⁻ through 7⁻, and the 9⁻ in this rotational band is now known at 17.14 MeV. The 1⁻, 3⁻, 5⁻ band has a 7⁻ state at 15.4. Remember that these 0⁻ band states all have four particles outside the ^{16}O core, and all these 2⁻ band members have a hole in the ^{16}O core. You should make the 0⁻ band members by transferring an alpha to ^{16}O , and the 2⁻ band members by picking up an alpha from ^{24}Mg .

Fig. 6 is a spectrum of $^{16}\text{O}(^6\text{Li},d)$ in which you transfer an alpha particle to ^{16}O . The states cross hatched diagonally downward to the right are the 1⁻, 3⁻, 5⁻ and 7⁻ states in the 0⁻ band. Those are (sd)³ fp states, four particles outside ^{16}O . The other negative parity states, the 2⁻, 3⁻, 4⁻, and 5⁻, are very much weaker in alpha transfer. We saw the same thing yesterday on a different slide. The 10.30-MeV 5⁻ state is very much more strongly populated than the 8.46-MeV 5⁻ state. The 7.17-MeV 3⁻ is very much more strongly populated than the 5.63-MeV 3⁻. If instead of alpha stripping from ^{16}O you do alpha pickup on ^{24}Mg , you get the spectrum shown in Fig. 7.

Fig. 6

Fig. 7

Question: Don't all the spin states occur for the $K = 2$ bands?

Answer: Right, for all K values except 0 you get all spins from K and above. For $K = 0^+$, you get even spins, and for $K = 0^-$, you get odd spins. It is a parity and isospin structure, and that selection rule comes out rigorously only if you ignore the spins of the nucleons. If you put in the spin-orbit force, you get the other members of this band, but shifted up by 10 MeV or so.

Question: Are the energy rules the same for this $K = 2$ band?

Answer: They all go as $J(J + 1)$. In fact, you see this splitting is roughly the same size. They are both about 3 MeV.

In Fig. 7 are data from Princeton,⁴⁾ so the resolution is not quite as good. Remember that in the stripping spectrum, the 5.97-MeV 3^- state was 10 times as strong as the 6.5-MeV 3^- state. In the pickup reaction shown here, they are reversed.

You also see that the unnatural parity state (the 5-MeV 2^- state), which cannot be populated in a direct alpha particle pickup, has about 20 or 30% of the strength of the 3^- state. The conclusion is that that state is reached by a two-step process where you make either the 1^- or the 3^- state in this band and then go by an E2 excitation to the 2^- .

These negative parity states that we have been talking about involve the excitation of only one particle. The particle band is an fp shell particle outside an sd^3 configuration and is therefore a particle outside ^{19}F . If you do $^{19}\text{F}(^3\text{He}, d)$ to make ^{20}Ne , you make those members of that band that can be made by the selection rule. You do not make the other band. If you do a particle pickup reaction on ^{21}Ne , you make those states that are five-particle one-hole by picking the particle out of the p shell, so that is consistent. The embarrassing thing is that these states are much purer than any shell model calculation (done to date) says they should be. This ratio of particle states to hole states was 10 to 1; the ratio in the other reactions was 10 to 1. Most shell model calculations get ratios of 3 to 2 or 3 to 1, so the states are purer than any shell model calculations would suggest.

The next slide (Fig. 8) shows the individual states comprising

$K^\pi = 0^+$ Rotational Bands in ^{20}Ne

Fig. 8

these various positive parity bands. The ground state rotational band is four particles in the sd shell in the lowest available orbit, and the states through spin 8 are all known. There is an excited $(sd)^4$ band starting at 6.7 MeV, and the 6^+ state is now known. At 7.20 MeV is the $(8p-4h)$ band, and beginning at about 8.3 MeV is a band composed of at least two and maybe four particles in the fp shell. The alpha particle widths of all these states are too large to consist of nucleons only in the sd shell. To explain the large widths you need to have particles at a larger radius than you would get if they were in the sd shell, so these states must have at least two particles in orbits above the sd shell, either two or four. These states should be excited in alpha transfer on ^{16}O , and the others should not be. That is indeed what is observed.

For the $(^6\text{Li},d)$ reactions, spectroscopic factors have been extracted; those have been compared with the spectroscopic factors you get from the alpha particle widths of the states, and those are shown in Fig. 9. The solid bars are for negative parity states - 1^- , 3^- , 5^- , and 7^- states - that should be four nucleons outside ^{16}O , and the alpha particle

Fig. 9

spectroscopic factors are between 1/2 and 1 for all those states. No single state other than those has a spectroscopic factor as large as 10%, so the purity is at least as large as 10 to 1, or at least 90% of the wave function is this simple configuration.

The spectroscopic factors for pickup go the other way, with those for the 0^- band about 10 times smaller than the others. Fig. 10 shows the results for the positive parity states. The members of the excited $(sd)^4$ band and the states that consist of either two or four particles in the fp shell have large reduced alpha widths. The $(8p-4h)$ states should have very little alpha strength, and indeed they do. We'll come back later to the reason why these spectroscopic factors are so large for the excited $(sd)^4$ band.

Fig. 11 summarizes many of the known properties of these positive parity states. The first two 0^+ states we are calling $(sd)^4$ states. Some of the evidence for that is given in this figure. Shell model calculations⁵⁾ that assume a closed ^{16}O core with four nucleons in the sd shell do predict two low-lying 0^+ states, the ground state, and a state at 6.8 MeV. Those two states are predicted to have

Fig. 10

^{20}Ne

J^π	Excitation Energy		Γ_α (keV)	Γ/Γ_{sp}	$S(^3\text{He}, d)$		$\sigma_{exp}(p, t)/\sigma_{DWBA}$	
	Exp.	Th.			Exp.	Th.	35.1 (MeV)	42.4 (MeV)
0^+	0.0	0.0	-	-	0.63	0.88	1.14	1.03
2^+	1.63	1.46	-	-	1.26	1.00		
0^+	6.72	6.81	19	0.38	0.94	1.02	0.80	1.20
0^+	7.20	-	4	0.015	<0.06	-		
2^+	7.43	8.43	8	0.10	0.32	0.19		
2^+	7.84	-	2	0.007	<0.05	-		
4^+	9.03	-	3	0.030	-	-		
4^+	9.99	10.0	120	-	-	-		

Fig. 11

spectroscopic factors in $(^3\text{He}, d)$ of .88 and 1.02. These two states (g.s. and 6.72 MeV) have been observed⁶⁾ to have spectroscopic factors very close to those in single-particle transfer, whereas the other 0^+ state here (7.20 MeV) is extremely weak in that reaction. So, these first two 0^+ states are to be identified as $(sd)^4$ configuration states.

Similarly, there are two 2^+ states predicted by the shell model at 1.46 and 8.43 MeV. We have identified the states at 1.63 and 7.43 MeV with these two 2^+ states, largely on the basis of the single-particle transfer spectroscopic factors and the alpha-particle widths. The two 0^+ states at 6.72 and 7.20 MeV have quite different alpha-particle reduced widths, and so do these two 2^+ states. One is reasonably well described as an alpha particle outside an ^{16}O core; the other one is not.

Furthermore, two-particle transfer has been done to these two 0^+ states. Two-particle transfer is extremely sensitive to the number of terms in the wave function and the sign of those various terms, because everything adds coherently. Taking the shell model wave functions for these two states and analyzing the $^{22}\text{Ne}(p,t)$ data,⁷⁾ you get very good agreement. If the agreement were perfect, all the ratios $\sigma_{\text{exp}}/\sigma_{\text{th}}$ for (p,t) would be unity, but the deviations are about 20% at worst. So, even in two-particle transfers, these 0^+ states, assuming they are purely (sd)⁴ states, agree with the data to within 20%, and these other states are not excited in pickup from ^{22}Ne .

These states at 7.20, 7.84, and 9.03 MeV are the ones we looked at yesterday that are the (8p-4h) states. The next few figures are alpha-particle spectra from the $^{12}\text{C}(^{12}\text{C},\alpha)^{20}\text{Ne}$ reaction.

Fig. 12 shows a spectrum at 18 MeV. The 7.20-MeV 0^+ (8p-4h)

Fig. 12

state is stronger than the 6.72-MeV state by a factor of roughly 3 at this energy. This 7.20-MeV state should be excited in 8-particle

transfer; the 6.72-MeV state can be excited in 8-particle transfer as well, but it is a different type of 8-particle transfer.

Fig. 13 shows the 25-MeV spectrum of the two 2^+ and the two

Fig. 13

0^+ states. The $(8p-4h)$ states at 7.83 and 7.195 MeV are much stronger than the $(sd)^4$ states. Fig. 14 is a 33-MeV spectrum in which the 9.03-MeV 4^+ is prominent. The cross section to that state is about 30 or 40 mb/sr, an extremely large cross section.

The enhancement of one of these 2^+ states over the other one extends over a wide range of bombarding energies, as shown in Fig. 15. These data cover the range from 22 to 35 MeV. We have extended this data to 51 MeV at Argonne,⁸⁾ and the ratio of the cross section is roughly 5 to 1 over the full range. Similar ratios are obtained for the two 0^+ states at 6.72 and 7.20 MeV and the two 4^+ states at 9.03

Fig. 14

Fig. 15

and 9.99 MeV. One set of states is selectively excited because of its structure; the other set of states is weakly populated, again because of its structure. This enhancement seen here is for data at one angle; the enhancement is found in the total cross sections as well, and that is shown in Fig. 16.

Fig. 16

Again, the ratio of the average cross sections is about 5 to 1, so this enhancement is not a phenomenon peculiar to one energy or to one angle.

These states whose structure is suggested to be (8p-4h) are strongly and selectively excited in a reaction that transfers eight particles to a ^{12}C core (eight particles to a four-hole core), and that is confirming evidence that these are (8p-4h) states. It has been suggested that they could be (6p-2h) as well as (8p-4h). It is certainly true that a transfer of eight particles can make either (8p-4h) states or (6p-2h) states. McGrory and Wildenthal have done calculations⁹⁾ for core-excited states in which they include $1p_{1/2}$, $2s_{1/2}$, and $1d_{5/2}$ orbits, but not the $d_{3/2}$ or the $p_{3/2}$ orbits. In that calculation, the next 0^+ state that they get that does not appear in (sd)⁴ calculations is a

(6p-2h) state and not an (8p-4h) state. But, it has been known for some time that if you do calculations with shell-model wave functions in which you include only one member of a spin orbit pair, such as the $p_{1/2}$ and not the $p_{3/2}$, or the $d_{5/2}$ and not the $d_{3/2}$, you do not get the alpha correlations, i.e., the four-particle correlations. The alpha particle is coupled to spin 0. If you take only one member of the spin orbit pair, you will not get that correlation. Four $p_{1/2}$ nucleons do not make an alpha particle. (There is a recent paper by Dieter Kurath which points that out for the 1p shell and the fp shell.)¹⁰⁾

The selectivity in eight-particle transfer reactions is seen in another eight-particle transfer reaction shown in Fig. 17. These are

Fig. 17

spectra from the $^{12}\text{C}(^{10}\text{B},d)$ reaction. Again, the (8p-4h) state at 7.83 MeV is stronger than the four particle state at 7.42 MeV. The angular distributions for these two states are shown in Fig. 18.

The enhancement here is about a factor of 2 1/2 or 3; the enhancement in the $(^{12}\text{C},\alpha)$ reaction was more like 5 to 1 instead of 3 to 1. The cross sections here are a few tens of $\mu\text{b}/\text{sr}$. The cross section in the $(^{12}\text{C},\alpha)$ was around 5 or 10 mb/sr . These cross sections are down

Fig. 18

by a factor of 100 from the ($^{12}\text{C},\alpha$), and yet the selectivity is still present. The selectivity seems to be due to the structure of the states. The enhancement of the 7.83-MeV state over the 7.42-MeV state continues for a range of bombarding energies below and above 20 MeV for several MeV.

We have seen 0^+ , 2^+ , and 4^+ members of the (8p-4h) rotational band. That band should have states with spins up to 12^+ . You know that eight particles in the sd shell can couple to spin 12, and 4 $p_{1/2}$ holes have to couple with spin 0, so you could make an (8p-4h) state that has spin 12. We found the first three states very quickly, the 0^+ , 2^+ , and 4^+ , and there became a great controversy over where the 6^+ state was. Eric Vogt made a suggestion¹¹⁾ which is depicted in Fig. 19. His suggestion was that this 0^+ , 2^+ , and 4^+ set of states at 7.2, 7.8 and 9.0 MeV that we had identified as (8p-4h) really had the lowest 8^+ state in the same rotational band. This 8^+ state at about 12 MeV had previously been identified as the 8^+ member of the ground state rotational band. He claimed, on the basis of what turned out to be incorrect evidence, that this 8^+ state had a very small α particle reduced width and therefore should be in the upper band. If that is correct, then somewhere close

Fig. 19

to the dashed line connecting the 4^+ and 8^+ states there should be a 6^+ state. The 6^+ state should have the same configuration as these other states, and therefore should be strong in $^{12}\text{C}(^{12}\text{C},\alpha)$. It would be an $(8p-4h)$ 6^+ state. It should be strong, that is, if one could pick the bombarding energy correctly. Well, the next slide (Fig. 20) reminds

Fig. 20

you of something we saw yesterday - a plot of J_{favored} vs excitation

Fig. 23

state is about 25 times stronger than the 12.14-MeV state, so the 12.59 is a four-particle state, and the 12.14 is an $(8p-4h)$ state. The degree of mixing is surprisingly small since they are so close together, but the same was true for the 4^+ 's and the 2^+ 's and the 0^+ 's. These states remain remarkably pure even though they are very close together.

Fig. 24 again shows these alpha particle reduced widths. The open bars are the fp shell states; the black ones are the $(8p-4h)$ states; the cross-hatched are the excited $(sd)^4$ states. In a simple shell model of ^{20}Ne , the lowest $(sd)^4$ states are the states in the ground state rotational band. In any shell model calculation that has been done to date, that set of states exhausts more than 90% of the alpha particle $(sd)^4$ strength. In SU3, shell model, and cluster model calculations, that first set of $(sd)^4$ states has all of the alpha particle spectroscopic strength.

The 6.72-MeV band, if it is an $(sd)^4$ band, should have no alpha strength, and yet the alpha spectroscopic factors are large, about .3 to .4. That is very hard to understand and contradicts all the other evidence, which says these are $(sd)^4$ states. If there is a nearby state that

Fig. 24

has the same spin and has a very large alpha spectroscopic factor, and this state started out before mixing with a small alpha spectroscopic factor and then mixed with that state, then it would gain width in the process. Well, the 8.3-MeV state has at least two particles in the fp shell; the 6.72-MeV state is an $(sd)^4$ state. The basis state before mixing is a state that is an $(sd)^4$ state predicted to have a large spectroscopic factor in single particle transfer. This 8.3-MeV state, if it has two particles in the fp shell, could not possibly have any strength in single particle transfer on ^{19}F because ^{19}F has the fp shell completely empty. But if the two mix, the 8.3-MeV state would gain single-particle transfer strength from mixing with the 6.72-MeV state.

Fig. 25 shows a single-particle transfer spectrum. The broad 0^+ state at 8.3 MeV is seen with a considerable amount of strength. If you integrate the area under that peak, it is 1/4 of the area of the 6.72-MeV 0^+ peak. The angular distribution for the two states in this reaction are identical. They have an $l = 0$ stripping angular distribution. The 8.3-MeV state would have no strength if it were indeed a state that had two nucleons or four nucleons in the fp shell, so these results are consistent with the idea that the two states mix.

Fig. 25

Let me repeat that argument. We are assuming that we have two states, ϕ_1 and ϕ_2 , two basis states before mixing. The ϕ_1 state is an $(sd)^4$ state; it has a large proton spectroscopic factor and a small alpha particle spectroscopic factor from all the shell model calculations. The other basis state, ϕ_2 , has either two or four particles in the fp shell. It has zero S_p and a large S_α .

ϕ_1	$(sd)^4$	S_p large	$S_\alpha = 0.03$ (theory)
ϕ_2	$(sd)^2(fp)^2$	$S_p = 0$	$S_\alpha = 0.71$ (theory)
	or		
	$(fp)^4$		

Then we make the two states at 6.72 and 8.3 by mixing these two states.

$$\begin{aligned}\psi(6.72) &= \alpha\phi_1 + \beta\phi_2 \\ \psi(8.3) &= -\beta\phi_1 + \alpha\phi_2\end{aligned}$$

$$\frac{S_p(6.72)}{S_p(8.3)} = \frac{\alpha^2}{\beta^2} = 4 \text{ experimentally.}$$

Then, since ϕ_2 has zero proton spectroscopic factor, the proton spectroscopic

factors would be in the ratio α^2/β^2 . That is 4 to 1 experimentally so that α^2 is .8 and β^2 is .2.

The alpha spectroscopic factors for the 6.72 and 8.3-MeV states (don't be confused by the fact that I've got an alpha particle and a coefficient called α) are given by:

$$S_{\alpha}(6.72) = |\alpha\sqrt{S_{\alpha_1}} + \beta\sqrt{S_{\alpha_2}}|^2 = 0.29$$

$$S_{\alpha}(8.3) = |-\beta\sqrt{S_{\alpha_1}} + \alpha\sqrt{S_{\alpha_2}}|^2 = 0.48$$

The shell model calculations that McGrory²⁾ did on an $(sd)^4$ basis say that S_{α} for ϕ_1 is about .03. Mixing two states like this conserves total alpha strength. Experimentally $S_{\alpha}(6.72)$ is 0.34, $S_{\alpha}(8.3)$ is 0.43. The sum of those two is .77. After mixing we have the same total alpha strength. So, to conserve the alpha strength, S_{α_2} must be .74. The theoretical number for S_{α_2} is 0.71. $S_{\alpha}(6.72)$ comes out to be 0.29, and $S_{\alpha}(8.3)$ is 0.48, using the value of α and β obtained from $^{19}\text{F}(^3\text{He},d)$ and choosing the sign of α and β to make the mixing go the right way. This is a remarkable agreement with the experimental numbers of 0.34 and 0.43. So, it seems that indeed the 6.72-MeV state should have very little strength before mixing. It gets the alpha strength it does have by mixing with the 8.3-MeV state. After this work appeared in print, Ichimura et al.¹³⁾ did a calculation to predict the alpha particle spectroscopic factor for that configuration, and the number they get is 0.71, too good to be true.

Question: This was after the fact? They already had the numbers?

Answer: Well, they claimed to have the number before the thing appeared in print, and it is quite possible, because actually they gave me the number after I had submitted the article but before it appeared. So, unless he was the referee, he didn't have the number. But the agreement is remarkable. So, it does look like the basis state is $sd^2\text{-}fp^2$ and not fp^4 , because an $(fp)^4$ state has quite a different S_{α} . If you were to redo this now, taking the two theoretical numbers, you would still get roughly the

same type agreement, and you can explain the ($^3\text{He},d$) results. Also, it is interesting to note that if you do the same thing for the 2^+ states - the 7.42-MeV 2^+ state and a broad 7.83-MeV 2^+ are both seen with $l = 2$ ($^3\text{He},d$) angular distributions - if you get the ratio of the spectroscopic factors there and assume the mixing is the same (the two spectroscopic factors tell you the mixing is about the same), then if you apply the same argument for the alpha strengths, you can explain the alpha widths of the two 2^+ states.

Question: Why is the 8.36 much broader?

Answer: Because its alpha spectroscopic factor is so large.

Question: The alpha channel has a lot more energy for it?

Answer: The proton channel is still closed, so a large proton spectroscopic factor still gives you no width. The alpha channel opens at 4.6 MeV.

Question: The differences in the natural widths of the 6.73 and 8.3 are just due to the energies?

Answer: Well, a little bit because of different spectroscopic factors, but largely the difference in the energy.

Question: Well, they are not very different. I am confused on it.

Answer: I think the width of the 6.72 is about 4 to 8 keV, and the width of the 8.3 is about 600 keV; the single-particle alpha width is roughly the same ratio, so it is largely the energy dependence. As shown in Fig. 11, the alpha width of the 6.72-MeV state is 19 keV, and the single-particle alpha width is between 50 and 90 keV, depending on what you use for a radius. For the 8.3-MeV state, it is about 600 keV, so the difference is largely the penetrability due to the energy.

We have used these reactions and other similar reactions to identify essentially all the low-lying states in ^{20}Ne and place them in the rotation bands. Those identifications are shown in Fig. 26. The shell model says the ground state rotational band should cut off at spin 8. No state has been observed with a gamma decay to the 8^+ state, indicating that if a 10^+ state exists, it is much higher in that band. The 2^- negative parity band should be accompanied by a 1^- band about 5 MeV further up. There is a 1^- band there, which should be a (5p-1h)

Fig. 26

band. It is rather important that these alpha reduced widths do turn out to be different for different bands and roughly equal within the band, because once states become unbound, you cannot observe even very strong E2 transitions between them. If they can particle decay, they will. They won't gamma decay very much of the time, but we would claim that you can use the alpha spectroscopic factors as an indicator of the fact that two states have the same intrinsic structure and therefore should be placed in a rotational band, even though you will never observe gamma decay between them.

Question: Any speculation on your mixing matrix elements or the mixing force that will mix your 2, 4 particle states, skipping over the (8p-4h)?

Answer: Yes. You notice that the $(sd)^4$ and the sd^2-fp^2 differ only in the core by 2 nucleons and therefore can mix with a two-body force. For either one of those to mix with an (8p-4h) band, they have to change the quantum numbers of at least four particles, and I think that the fact that that mixing is not observed between the (8p-4h) and either

of the other two is again another strong indication that that is an (8p-4h) state and not (6p-2h). If it were (6p-2h), it would mix with an (sd)⁴. The magnitude of the matrix element you need comes out to be a few hundred kilovolts to explain the mixing between the two that do mix. The absence of mixing in the other two says that the matrix element is less than 5 keV. I think that some bands don't mix because you would have to change too many particles for them to mix. There is just no four-body force.

Fig. 27 is a plot of the separation energies in these two

Fig. 27

rotational bands. Everything above spin 8 is wrong, so don't look at that. The ²⁰Ne ground state rotational band turns over at spin 6 instead of spin 8 just because of the truncation of spin. The spacing is very similar within the (8p-4h) band to that of the ground state band, but the energies are a factor of 2 less. This means that the moment of inertia is twice as large for the excited band as it is for the ground state band. This is again consistent with it being an eight-particle excitation instead of a four-particle excitation. If you like to think of such things, it means the deformation for the (8p-4h) band is much

larger than for the ground state band. If you care to go a little further, you can say that these are fission isomers in ^{20}Ne . They are states in the second minimum in a double hump fission barrier. They are fission isomers in the sense that their alpha decay lifetimes are much longer than those of the other states.

Fig. 28 shows a comparison with shell model calculations. The

Fig. 28

spacing in these two bands is very strange; one band goes 0, 2 and then it jumps up to the 4, and the other goes 0, 2, and jumps down to the 4. They deviate from the expected straight line. Both of those features are explained in the calculations of McGrory and Wildenthal,⁹⁾ in spite of the fact that their core-excited band is largely (6p-2h).⁹⁾ The fact that one band is (sd)⁴ and the other is core-excited gives you different kinking in the two bands. You see the agreement is much better with the four-particle band than it is in the core-excited band, and hopefully that is because there is not enough alpha particle correlations in the calculation, because they ignored the $p_{3/2}$ and the $d_{3/2}$.

I'll titillate you a little bit by showing you the next slide. These are the observed and predicted energies of the first two rotational bands in ^{24}Mg . In the 0^+ ground state band and the 2^+ band

Fig. 29

all states are known up to spin 8. The 9^+ , 10^+ numbers have been predicted to lie around 16 to 18 MeV by Akiyama, et al.¹⁴⁾, and we set out to look for those using the argument I presented the first day that states of such spin should be strongly excited in the $^{16}\text{O}(^{12}\text{C},\alpha)$ reaction. The results are shown in Fig. 30.

These high spin states are an important test of the model, since all the models give you the energies of the low-lying states correctly. When you get up to the high spin states, the various models differ. So, it is important to find these 9^+ , 10^+ spins, and we thought that we had found some of them. A group of states at about 16 MeV are strongly excited. The only one of those whose spin was already known is an 8^+ state. We thought surely one of these three states was a 10^+ state in the ground state rotational band. It turns out that that is not the case. Every state that is strongly excited in this reaction is 8^+ , as has been shown by Zuremühle's group at Penn.¹⁵⁾ The 10^+ state has not been found, and from studying this reaction we know that it is not below about 19.5 MeV. Akiyama, et al. had originally predicted it

Fig. 30

to be around 17 MeV.¹⁴⁾ He has now redone the calculation and says the state is around 20 MeV, which turns out to be where the shell model guys put it all along, and he now agrees that his prediction of where the 10^+ should be is similar to results of the shell-model calculations. So, the 10^+ is no longer a test between the two models, but the 12^+ will be - until it is found, and then everybody will agree.

These states are made in the $^{16}\text{O}(^{12}\text{C}, \alpha)$ reaction, and then they alpha decay. If you detect the first alpha particle at 0° , and in coincidence with that measure the angular distribution of the decay alpha particle, if the alpha decay goes to the 0^+ ground state of ^{20}Ne , then the angular distribution of the second alpha particle is just P_2^2 . (The state has spin J.) This method has been used to assign many spins and was the method used to assign the spin of the 14.14-MeV state. Unfortunately, all of these states around 16 MeV that are strongly excited decay to the first 2^+ state in ^{20}Ne and not to the ground state, so the angular distribution is washed out. It is true that the ratio of the angular correlation at 180° to that at 90° depends on the spin, but only by a small amount.

Balamuth, et al.¹⁶⁾ measured the correlation for those three states around 16 MeV this way and were able to say that the spins were between 7 and 11, with 7 and 10 unlikely. Spin 11 is possible, but very

7

unlikely. Then they went back and did some calculations that provide some things that you probably knew before, but I didn't know about them. The 1.63-MeV 2^+ state in ^{20}Ne gamma decays to the ground state. If you detect the first alpha particle at 0° in coincidence with this gamma ray at fixed angle and in coincidence with the second alpha particle, and plot the angular correlation of that second alpha particle, you get a highly structured shape. It is nothing simple, but can be calculated, and is quite different for different spins. This is a triple correlation measuring two alpha particles and a gamma ray in coincidence.

Question: What is the count rate?

Answer: About 10 or 12 per day. They have about 60 counts, but they get all the angles they want because they have a position-sensitive detector, which is in reality about 8 or 10 slices, and they use that to get the angular distribution of the alpha, obtaining all the angles at once.

Most of these highly excited states, if they have high spin, decay not to the ground state by alpha emission but to excited states. You therefore can never use the simple correlation technique to assign spins because they go to 2^+ states, so their technique is going to be extremely powerful to assign spins of these states. The reason they decay to the 2^+ states can be seen on this graph; it is very obvious, I think. The alpha emission will vary rapidly as a function of both l -value and excitation energy (see Fig. 31), and the variation is so rapid that if you change J by two units, you gain more in alpha width than if you change energy by 1.63 MeV. So, if a state can decay to the ground state by spin J , it can also decay to the 2^+ state of ^{20}Ne by spin $J-2$, which is 1.6 MeV away. It will always go to the 2^+ state if it has high spin.

That is shown for specific states in the next few slides. Fig. 32 shows the measured branching ratio for the ground state to the 2^+ for the state at 14.14 MeV. That's the hatched line, measured at Yale¹⁷⁾, and the dashed line measured at Penn.¹⁶⁾ This is the theoretically expected branching ratio for various spins, calculating alpha particle widths with the Woods-Saxon potential. You see that although you could not

Fig. 31

assign a spin on the basis of such a comparison, the branching ratio is what you expect for spin of 8 or 6 or 10. Clearly it is not what you would expect for low spin. These calculations assume that the intrinsic structures of the 0^+ and 2^+ states in ^{20}Ne are the same, and therefore the spectroscopic factors are the same. The branching ratios are determined only by penetrabilities. The same arguments would apply for the other states around 16 MeV, shown in Fig. 33 for the 16.55-MeV state. At the time these calculations were done, this spin was not known. It is now known to be 8^+ , and you see the experiment and theory agree roughly for spin 8, but would not agree for low spin.

Fig. 34 shows a 6^+ state, and the agreement is reasonable for spin 6. There is a fly in the ointment - the state at 16.84 MeV shown in Fig. 35 does not agree for any spin. The comparison of the 0^+ , 2^+

Fig. 32

Fig. 33

Fig. 34

Fig. 35

branching ratio tells you the spin should be around 6 or so. The 4^+ , 2^+ branching ratio disagrees horribly for all spins, so either the assumption that the 4^+ state of ^{20}Ne has the same intrinsic structure as the 2^+ and 0^+ is incorrect, or this is not a single state. Zurmühle's group have already shown that there is an 8^+ state here.¹⁵⁾ Levine at Brookhaven has shown there is a 6^+ state there¹⁸⁾ and Zurmühle's group think there is also a 7^- there within the resolution of the experiment.¹⁵⁾ These branching ratios came from Zurmühle and are certainly within the resolution of the Yale people, so there is either a doublet or a triplet there, and that may be why these disagree. It could also be that the

intrinsic structure is not the same. These alpha decays are taking place through a very small component in a wave function. Even if the 0^+ , 2^+ , and 4^+ members of the ground state band of ^{20}Ne have the same intrinsic structure to within 95%, if the alpha decay takes place through the other 5%, you wouldn't expect the branching ratio to be given just by penetrabilities. But, for most of the states in ^{24}Mg and those in ^{20}Ne , the alpha decays do go like you would expect from penetrabilities assuming that the spectroscopic factors within a band are roughly the same. The disagreements are like 30, 40 or 50%. Here it is about a factor of 5.

Question: I think I remember that the high-energy states in ^{24}Mg were 0, 2, and 4^+ spins.

Answer: No, those are three states a little higher up. There is a triplet of states, one of which has been assigned 2^+ , one of which has been assigned 4^+ , and one of which has been decreed by Bromley as 0^+ . I think the first is at 18.6 MeV. In fact, when these three strong states were seen at Penn, Bromley and Arima and a few other guys said that those are carbon-carbon molecular states, just like the three already known, and here are three more. But it was very clear from the branching ratios immediately that they were not low spin. They had to be high spin, and that has now been confirmed.

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SELECTED SHORT SUBJECTS

Yesterday we talked about the $^{16}\text{O}(^{12}\text{C},\alpha)$ reaction as a possible eight-particle transfer reaction. Moving on, the only other good case that looked favorable for doing eight-particle transfer was eight-particle transfer to ^{40}Ca . ^{40}Ca supposedly fills all of the sd shells in the ground state. There is then a low-lying excited state in which you take four particles from the sd shell and promote them up to the fp shell, and that would be a (4p-4h) state.

Fig. 1

In ^{40}Ca there is a 0^+ state at 3.39 MeV, and then up a little higher there is another 0^+ that is thought to be an (8p-8h) state. The ground state is spherical; each of the excited 0^+ states has rotational bands built on it.

If these really are states of (4p-4h) and (8p-8h) character, they should be populated strongly in an eight-particle transfer reaction. The reaction one tries is $^{32}\text{S}(^{12}\text{C},\alpha)^{40}\text{Ca}$. If this reaction goes by direct transfer of eight particles, you would expect certain ratios for population of the various states by it. In particular, you would expect the ratio of the cross section for these two excited 0^+ states to be the same as the ratio of the 2^+ and 4^+ states of these bands.

Fig. 2 shows the spectrum of this reaction.¹⁾ The target here was a windowless gas cell containing H_2S . (Only in Philadelphia can you bleed H_2S into the atmosphere without anybody noticing it.)

Alpha-particle spectrum from the $^{32}\text{S}(^{12}\text{C}, \alpha)^{40}\text{Ca}$ reaction at a bombarding energy of 30 MeV. The excitation energies shown were calculated from the 7.5 and 10° spectra of the present study and may include uncertainties as large as ±15 keV. The shaded group corresponds to the $^{32}\text{S}(^{12}\text{C}, \alpha)$ transition to the 4.12 MeV 4^+ level of ^{40}Ca . Other α -particle groups resulting from impurities were either too weak to be identified or occur at high excitations where there is a large density of states. Previously suggested [11-13] $4p-4h$ and $8p-8h$ states are labeled.

Fig. 2

This is a differentially pumped gas cell with no entrance window. The ground state has four counts in it; it corresponds to 0.3 $\mu\text{b/sr}$. The 0^+ , 2^+ , and 4^+ states thought to be the (4p-4h) band, and the 0^+ , 2^+ , and 4^+ members of the (8p-8h) band are indicated. The (8p-4h) 4^+ state is stronger than the (4p-4h) 4^+ state, and similarly for the 2^+ and 0^+ states. These other states contain candidates for the 6^+ states in these two bands, but their spins await determination.

Fig. 3 is simply a list of the cross sections observed for all these states. The smallest is 0.3 $\mu\text{b/sr}$ and the largest is 40 $\mu\text{b/sr}$.

Fig. 4 focuses on the 0^+ , 2^+ , and 4^+ members of these excited core bands. If you look at these states not in terms of nucleon particles and holes but alpha particles and holes, the (4p-4h) state is a 1-alpha, 1-alpha hole state, and the (8p-8h) state is a 2-alpha, 2-alpha hole state. It is rather simple to figure out what the cross section ought to be relative to these states. When you go to the (0p-0h) ground state, you are starting with a target that has 2-alpha holes in the ground state. To do this 2-alpha transfer on ^{32}S to make the ^{40}Ca ground state, you must put those two alpha particles in the two vacancies you have. There is just one way to do that; there is only one way to put

Results of the $^{32}\text{S}, ^{12}\text{C}, \alpha^{40}\text{Ca}$ reaction study at
 $E(^{12}\text{C}) = 30 \text{ MeV}$.

Excitation energy Present a) Literature b)	J^π	c.m. cross section ($\mu\text{b}/\text{sr}$) measured at $\Theta_{\text{LAB}} = 7.6^\circ$
0.0	0 ⁺	~0.31
3.35	0 ⁺	3.78
3.70 (weak)	3 ⁻	~0.91
3.91	2 ⁺	6.96
4.5 (weak)	5 ⁻	1.04
5.18	0 ⁺	5.41
Weak	2 ⁺	Weak
5.26	4 ⁺	6.84
5.61	4 ⁻	11.0
	2 ⁺	
	1 ⁻	
5.90	2 ⁻	3.51
	3 ⁺	
6.3	3 ⁻	3.51
	4 ⁺	12.34
6.54	4 ⁺	
	3 ⁻	
	(2) ⁻	Weak
	2 ⁺	16.0
6.91	(3) ⁻	
	1 ⁻	
Weak	-	Weak
7.27		8.17
7.98		40.3
8.11		20.7
8.32		11.8
8.59	Many	27.3
8.79	Levels	15.7

a) Uncertainty is $\pm 25 \text{ keV}$.

b) Ref. 1 and refs. therein.

Fig. 3

two things into two vacancies. That cross section is proportional to 1.

To make the (4p-4h) state, you are putting one alpha into the sd shell and one into the fp shell. There are two vacancies in the sd shell, and you are putting one particle there; there are two ways to do that. You are putting one particle into the fp shell. We don't know how many vacancies there are in the fp shell; we'll just call that number N. So the number of ways to make the (4p-4h) state is 2N. To make the (8p-8h) state, you put both particles in the fp shell. The number of vacancies in the fp shell is N; the number of ways you can put two things into N vacancies is just $N(N-1)/2$.

That is the way the cross section should go for transfer of two alphas to (0p-0h), (4p-4h) and (8p-8h) states. You would expect the cross section for the 3.39 MeV state, relative to the ground state,

Center-of-mass cross sections at $\theta_L = 7.5^\circ$ for the $^{12}\text{C}(^{12}\text{C}, n)^{24}\text{Mg}$ reaction in suggested multi-particle - multi-hole states

	0^+	2^+	4^+
0p-0h			
$E_x(\text{MeV})$	0.0	-	-
$\sigma(\mu\text{b}/\text{sr})$	0.31 ± 0.15	-	-
1p-1h			
$E_x(\text{MeV})$	3.353	3.904	5.275
$\sigma(\mu\text{b}/\text{sr})$	3.5 ± 0.5	7.0 ± 0.8	6.8 ± 0.8
2p-2h			
$E_x(\text{MeV})$	5.212	5.828	6.541
$\sigma(\mu\text{b}/\text{sr})$	5.1 ± 0.6	$< 11.0 \pm 0.9 \text{ a)}$ ~ 2.0	$< 12.3 \pm 1.0 \text{ b)}$ ~ 4.0
$\frac{\sigma(1p-1h)}{\sigma(0p-0h)}$	12 ± 6	-	-
$\frac{\sigma(2p-2h)}{\sigma(0p-0h)}$	1.42 ± 0.25	$< 1.58 \pm 0.3 \text{ a)}$ ~ 0.4	$< 1.90 \pm 0.2 \text{ b)}$ ~ 0.6

a) Cross section listed for the 5.828 MeV state includes a small contribution from the 5.614 MeV 4^+ state.
b) Cross section listed for the 6.541 MeV state includes contributions from the 6.509 and 6.579 MeV states.

Fig. 4

to be $2N$, and you would expect the cross section for the 5.21-MeV state, relative to the 3.35-MeV state, to be $(N-1)/4$. You would expect this latter ratio to hold for the 0^+ , 2^+ , and 4^+ states in the two bands. The ratio of cross sections for the 3.35-MeV state and the ground state is 12 ± 6 . The ratios for the 0^+ , 2^+ , and 4^+ states are also shown in Fig. 4. The ratio is 1.4 for the 0^+ states. It is around 1.6, with a larger error, for the 2^+ states, and around 1.8, with an even larger error, for the 4^+ states. Within the errors, these three numbers are all equal, and they are all larger than 1, as you would expect. In fact, if you take this stuff seriously, 12 is equal to $2N$, which gives $N = 6$. The next ratio should be $(6-1)/4 = 5/4$ or 1.25, and experimentally it comes out about 1.4, so there is very good agreement between the ratios in these two bands and the ratio we expect. Also, $N = 6$ is what you would get in the simplest model of these states where you include the $f_{7/2}$ and the $p_{3/2}$ shell. You can put 16 particles in the $f_{7/2}$ shell and 8 in the $p_{3/2}$, for a total of 24, which is 6 alpha particles. So it does look like these states are populated by something like direct eight-particle transfer. Statistical calculations would suggest that the ground state would be the strongest 0^+ state if it were a compound reaction.

Question: In the cross sections here, how can you be sure your transmission coefficients are not fouling you up?

Answer: For the direct transfer you mean? The ground state is far enough away that it could be messed up by as much as a factor of 2. The other states are close enough together that you don't have to worry about them. You are comparing two states that differ by at most 2 MeV; even though the states cover more energy than that, the two 0^+ 's are about 2 MeV apart, and the two 2^+ 's are about 2 MeV apart, etc. You are never comparing a 0^+ with a 2^+ or anything like that. So the answer is that this is not a problem except for the ground state. I have done the calculations with a distorted-wave code, and depending on how you play the game, you can get a factor of 2 difference for the ground state, but our number is uncertain by a factor of 2 anyway just because there are almost no counts in the ground state. It is also completely different from what you would expect for a compound reaction - the 3^- state is very weak, the 5^- is very weak. The ground state is the weakest state in the spectrum; it should be the strongest if it is compound.

The next thing to discuss is in Fig. 5 and concerns the one thing that we are absolutely sure of, the fact that we do not understand it. Almost all of these reactions have been roughly symmetric about 90° in their angular distributions. When we bombarded ^{12}C with ^{13}C and looked at alpha particles coming out, almost all of the states had the same cross section at forward and backward angles, except the two states at 3.66 and 3.89 MeV. One of those is very strong at forward angles, and the other is very strong at backward angles.

Fig. 6 shows the angular distributions in detail. One is very forward peaked, and one is very backward peaked. For the other states the angular distributions are all essentially symmetric about 90° , which is what you would expect for a compound nucleus cross section. These data were taken at 30-MeV lab energy, and as can be seen in Fig. 7, it is a very fortuitous choice of energy. These are excitation functions for all the low-lying states in the $^{12}\text{C}(^{13}\text{C},\alpha)$ reaction at $3\frac{3}{4}^\circ$, and they all sort of dribble along, except for the 3.89-MeV state, which

Fig. 5

had a very large peak in the excitation function at forward angles at around 30 MeV in the lab.

Fig. 8 shows data for that state and data for the other state at the conjugate angle. This is the excitation function for the 3.89 at forward angles over this energy region and for the 3.66 at back angles, and the structure is almost identical for the two states. The 3.66-MeV state, which has a resonance or a bump at backward angles, does not have such a bump at forward angles. The naive assumption would be that whatever the mechanism is that makes the 3.89-MeV state strong at forward angles, it makes the 3.66-MeV state strong at backward angles. Fig. 9 shows the two angular distributions, one of them reversed relative to the other. Where they are strong, they are almost identical in nature. At back angles their cross sections are in the ratio of $2J + 1$. The 3.89 is the $5/2^-$ state; the 3.66 is the $3/2^-$.

The angular distributions when added look very much like the angular distribution to the 2^+ (8p-4h) state in ^{20}Ne , and that

Fig. 6

Fig. 7

Fig. 8

Fig. 9

comparison is in Fig. 10. The lower curve is the sum of the two angular distributions for the two states we just looked at. There is

Fig. 10

absolutely no reason whatsoever to assume that when you add those two you are going to get something symmetric about 90° , which you do essentially. The shapes are roughly the same. The $^{12}\text{C} + ^{13}\text{C}$ cross section is down about a factor of 4 from that for $^{12}\text{C} + ^{12}\text{C}$.

It is known that the structure of these states is largely holes in the $1p$ shell. In fact, the wave functions of those states are something like 1 p -hole in the ^{22}Ne first excited state and 3 $p_{1/2}$ -holes in the ^{24}Mg 2^+ state, and that exhausts essentially all the structure of those two states.

$$\psi = \alpha \, ^{22}\text{Ne}(2+)p_{1/2}^{-1} + \beta \, ^{24}\text{Mg}(2+)p_{1/2}^{-3}, \quad \alpha^2 + \beta^2 \approx 1.$$

Single-particle transfer, three-particle transfer, and Wildenthal's

calculations²⁾ all agree that β^2 is about 0.3 and α^2 is about 0.7. It is the β component that we would excite in a direct eight-particle transfer reaction since we are starting out with a ^{13}C target which has the three holes. So if this picture is right, then this state at 3.66 will be about 1/3 as strong as the corresponding state was in $^{12}\text{C}(^{12}\text{C},\alpha)$ when we started with a ^{12}C target, and it is roughly a factor of 1/4. I don't have any idea if that is the correct explanation or not. It certainly has nothing whatsoever to say about the fact that one of the angular distributions is forward peaked and one is backward peaked. That is very difficult to understand.

Once you have these data it is possible to explain them in a number of different ways. You could say these are quasi-molecular states, and they are mixed parity or something. You can also say that you have both ^9Be transfer and ^8Be transfer going on, schematically like this:

If you assume that ^8Be is transferred in its ground state and its 2^+ state with comparable intensity, and if ^9Be is transferred in its ground state and in the four states you get by coupling the $p_{3/2}$ particle with a 2^+ state of ^8Be , and assume the cross section depends only on kinematics and $2J + 1$, then the cross section for ^9Be transfer is identical to that for ^8Be transfer except you get one angular distribution backward peaked and one forward peaked. I don't believe it for a minute!

There are other possibilities, but there is no firm explanation for these data. It is possible to explain the bumps in the excitation functions and the sum of the angular distributions a number of different ways. None of those really says anything about why one state should be forward peaked and one backward peaked. They can both have the same shape and all the other explanations would still hold. Both of these mechanisms are present for both states. It is not the case that one state is formed by ^9Be transfer and the other one by ^8Be transfer.

The structure already tells you that.

Question: How do you know these states must be coupled? Why do you have to couple one strongly in the forward direction and one strongly in the backward direction? What makes you pick these two states out of the entire spectrum?

Answer: These were the only two whose angular distributions were not symmetric about 90° , except for two others up around 6 or 7 MeV, which appear to be the $7/2^-$ and $9/2^-$ states of this structure. We did the reaction expecting everything to be symmetric, and we saw two states that weren't, so then we started looking at it further.

Question: Were you able to do the angular distribution for the $1/2^-$ state?

Answer: It is covered by an impurity, but to the extent we can extract it, it's symmetric, but it should be since there is only one state there. If you couple $p_{1/2}$ to the ground state, you only get a single state and not two states. The angular distribution is not as complete, but it is roughly symmetric.

Question: Are these only coupled at this one energy, or do they couple throughout?

Answer: No, at other energies their angular distributions are symmetric if you go several MeV away.

Question: Must this be some kind of resonance process?

Answer: Well, that's one explanation, that we are sitting on one of these quasi-molecular type resonances. You can really get carried away with this stuff. If you take the Siemssen molecule³⁾ in $^{12}\text{C} + ^{16}\text{O}$, you probably know that Debevec, Körner and Schiffer have now supposedly seen that resonance in $^{12}\text{C} + ^{17}\text{O}$.⁴⁾ In this case its energy is shifted by the amount you would expect just from the fact that the radius is different. If you take that energy and the radius from the fact that this is ^{13}C instead of ^{16}O , we expect it at 14.7 MeV - and that's where it lies. I don't believe any of that! It's fun to play the games.

There are lots of explanations, none of which is satisfying. It is certainly not a general compound nucleus phenomenon that is

statistical in nature. It is not the usual compound nucleus state that has lots of open decay channels. We have made it a number of different ways, as shown in Fig. 11. These are three spectra from three different

Fig. 11

reactions, all going through the same compound system at the same compound system energy. You notice that the 3.89-MeV state is not selectively excited in either of the other two processes. The overall relative cross sections are about the same in all three reactions, except for the states at 3.89 and 6.65 MeV. There are two other states that are strong at backward angles. The only thing that is different about the three spectra is the level of cross section. The ^{14}N cross section is down an order of magnitude from those for ^{13}C and ^{16}O , but there is nothing strange in either of these two reactions concerning

the 3.89-MeV state. Its angular distribution is symmetric about 90° . It has no sharp, large bump in the excitation function, so it is not a simple compound nucleus state that resonates whatever channel you feed it with; that is just not the case.

Question: It could be a penetrability problem in the entrance channel. The Coulomb barrier differs.

Answer: That is the only difference in the two, and they are not very different, not even a factor of 2. I don't think it is a simple compound nucleus thing. These states are not selectively excited because they are high spin or anything; they are low spin. They are of the opposite parity from all the other nearby states, and you might expect them to be strong for that reason, but you would not expect them to be asymmetric in their angular distributions.

Question: It could be a single compound state if it has structure - you need carbons plus a neutron.

Answer: I need two states, and they must be opposite parity, and they must mix to get asymmetric angular distributions. You need a carbon-carbon-neutron molecule, and you need two states of the same spin and opposite parity or something like that, and they must mix. I don't understand it!

Fig. 12 shows the work we did to try to pin down high spin states in ^{22}Na . This is sort of an advertisement. We got energies for 129 states in ^{22}Na .⁵⁾ Warburton had energies for about 60 of these states.⁶⁾

Fig. 13 is a slightly better spectrum obtained the other way, with a ^{12}C beam and ^{14}N target, and the same set of states (they are not all labeled). These states are strong because they have high spin; all the states that are known to have high spin turn out to be strong. We guessed on the basis of the strengths what some of the other high spin states might be, and I think an excellent set of experiments would be to do this reaction and look at gamma rays in coincidence with the alphas to see how these strong states can decay. The state at 8.6 MeV is thought to be the 8^+ member of the ground state band, and I can give you

Fig. 12

Fig. 13

the other energies if you are interested.

Question: These are positive Q reactions?

Answer: The Q-value to the ground state is about 6 MeV.

Question: Have you done any cases where you could do the light particle when the residual nucleus is stable and do the complete inverse?

Answer: Well, ^{20}Ne for example: We have done $^{12}\text{C}(^{12}\text{C},\alpha)$; we have not done the $(\alpha, ^{20}\text{Ne})$ to compare the other way around, but it has got to be equal because of time reversal to the ground state, but the excited states are not so obvious. The ground state cross sections would have to be equal.

Question: But you could study the excitation function.

Answer: Right. We haven't, but it could be done.

Question: That's what would be of interest.

Answer: Right. We have done it the forward way around, but not the backward. We have done lots of these reactions where we make the same kind of system in more than one way. This is another such example. You see a wealth of states in all these reactions, if you do them with high resolution. Up to 10, 15, 20 MeV of excitation, you still see strong states.

Fig. 14 is another example. There are two very strong states

Fig. 14

at 10 and 11 MeV and an extremely strong state at almost 14 MeV. Those states must be either very high spin or possess some simple configuration, or they wouldn't stand out, so again an excellent set of experiments would be to look for the gamma decay of some of these high-lying states and see if they gamma decay or alpha decay. If they are high spin, they should still gamma decay even though they are unbound, and if they are unusual configurations like multi-particle-multi-hole, those configurations will strongly hinder their alpha decay. It is clear that their alpha decay widths are very small because they have no natural width within the limits of our resolution, which is about 20 keV, so we can say that all these states are less than 20 keV in width. The single-particle alpha width for low-spin states up here is about 3 MeV, so they don't have much alpha width.

Fig. 15 is a similar spectrum on ^{20}Ne . Again there are very

Fig. 15

strong, sharp states at very high excitation energies. This is a low-statistics run, so it doesn't look so good.

Fig. 16 shows states up to 10 or 11 MeV in ^{40}Ca , still showing up strongly. The purpose of this set of slides is simply to suggest that heavy-ion reactions, if for no other reason, are an

Fig. 16

excellent way of making states with properties you would like to study; you form the state with a heavy-ion reaction, and then observe its decay some other way. There is no other reaction that makes states with such large cross sections up at those excitation energies, not even resonance reactions. These large cross sections are very definitely an alpha-particle type phenomenon. As you go away from projectiles and targets that are $4n$ type nuclei (alpha-particle type nuclei) the cross sections go down drastically, and that's demonstrated in Fig. 17.

These are spectra with ^{12}C and ^{13}C on each other. In the $^{12}\text{C}(^{12}\text{C}, \alpha)$ reaction, the cross section to a typical state is 7.5 mb/sr. The $^{12}\text{C}(^{13}\text{C}, \alpha)$ typical cross section is .27 mb/sr (down a factor of about 30), and the $^{13}\text{C}(^{13}\text{C}, \alpha)$ is down another factor of 20 for typical cross sections to discrete states. That is not true for the background. The low-energy background is due both to making discrete final states that are too broad to resolve and to multi-particle breakup of the beam. Those cross sections are about the same in all three processes. (It doesn't look like it from the scale because they differ in the collected charge for the run, but it is the case.) These background cross sections are about the same; it is the cross sections to discrete states that go down as you go away from alpha-particle-type nuclei.

Fig. 17

That's probably a consequence both of the structure and of the Q-value and the number of open channels and so on. The cross section goes down exponentially as the energy increases because the number of open channels increases exponentially, and there are a lot more open channels in ^{22}Ne than there are in ^{20}Ne .

In fact, the cross section in ^{22}Ne is so small that the ^6He channel competes favorably with the alpha channel, which is shown in Fig. 18 for ^6He 's coming out when we bombard ^{13}C . The cross sections here are favorable to those observed for an individual state in the $(^{13}\text{C}, \alpha)$ reaction. Nobody would believe this is a direct reaction, so most of the $^{13}\text{C}(^{13}\text{C}, \alpha)$ is probably simply compound, but it shows that the arguments that nuclei don't like to emit certain types of particles are probably not valid. Compound systems will emit anything that has a favorable Q-value, even ^6He 's or ^8Be 's or ^{14}C 's.

I'll turn now to another argument concerning nuclear structure.

Fig. 18

In Fig. 19 are shown all the ways you can make low-lying states in ^{19}F .

^{21}Ne		^{23}Ne	
$\frac{3}{2}^+ [211]$	—	—	—
$\frac{1}{2}^+ [220]$	—	—	—
$\frac{1}{2}^- [101]$	—	—	—
$K^\pi = 3/2^+$			
<hr/>			
^{19}F			
$\frac{3}{2}^+ [211]$	—	—	—
$\frac{1}{2}^+ [220]$	—	—	—
$\frac{1}{2}^- [101]$	—	—	—
3p-0h	4p-1h	5p-2h	7p-4h
$K^\pi = 1/2^+$	$K^\pi = 1/2^-$	$K^\pi = 3/2^+$	

Fig. 19

You remember in the Nilsson model⁷⁾ that the filled $1/2^-$ shell is the closed ^{16}O core. You make a ^{19}F ground state by adding three particles to the next orbit. That is then a (3p-0h) state; the K^π is $1/2^+$, and

you get a rotational band built on that. That band is now well established. You make a very low-lying negative parity band by putting one particle from the $1/2^-$ orbit up to the $1/2^+$, and that is the $K = 1/2^-$ band. It is a (4p-1h) band, and the energy is roughly degenerate with the ground state simply because even though you lose energy when you have to move one particle from $1/2^-$ to $1/2^+$, you gain energy from the fact that the pairing is stronger in the $1/2^+$ orbit than it is in the $1/2^-$ orbit. Two nucleons in the $1/2^+$ orbit pair together better than two nucleons in the $1/2^-$ orbit, and it brings the state back down so that the $1/2^-$ and the $1/2^+$ states are almost degenerate.

There are two ways to make additional low-lying positive parity states. One is to promote two particles from the 1p shell. The other is to promote 4, so one is the (5p-2h) state and the other is the (7p-4h) state. There are a lot of additional low-lying states known in ^{19}F , and it is of interest to determine which configuration they belong to.

On top of Fig. 19 are the ^{21}Ne and ^{23}Na ground states in a simple Nilsson model. You can see that the ^{19}F (7p-4h) states, if they exist, are just an alpha particle hole from the p shell in the ^{23}Na ground state. On the other hand, the ^{19}F (5p-2h) states can be made from ^{21}Ne by picking up two particles from the 1p shell. If you do alpha pickup in ^{23}Na , you can make the (7p-4h) state by picking up the alpha from the 1p shell. You can make the (5p-2h) state by picking up four particles, two from the $1/2^-$ and two from the $3/2^+$. If you do two-particle pickup on ^{21}Ne , you can make the (5p-2h) states by picking up two particles from the p shell. Two-particle pickup on ^{21}Ne cannot possibly make (7p-4h) states, so again, it is a case where four-particle pickup will make both kinds of states, two-particle pickup only one kind of state.

Additionally, if the (5p-2h) states are simply these two p-shell holes in the ground state of ^{21}Ne , then you would expect that the spectroscopic amplitude for picking up two particles from ^{21}Ne to make that state would be the same as that amplitude for picking up two particles from ^{16}O to make ^{14}N , since it is the same two particles.

Fig. 20 shows which states these are physically. The ground state band is $1/2^+$. The $1/2^-$ negative parity band is an alpha particle

Fig. 20

outside of ^{15}N , and it is strong in alpha-particle transfer to ^{15}N . It is the $3/2^+$ band whose structure we were going to say something about. Two-particle transfer reactions have been done leading to these states, and the cross sections are weak.⁸⁾ They are roughly 20% of what you would expect if the spectroscopic amplitudes were the same as those for two-particle pickup on ^{16}O , so if you take that argument seriously, this set of states would have about 20% of the five-particle two-hole component. We then went to Princeton and did $^{23}\text{Na}(d, ^6\text{Li})$ four-particle pickup to try to make these states. These four states in the $3/2^+$ band are strong in alpha-particle pickup. Unfortunately, they would be strong if they were either (5p-2h) or (7p-4h), but you would expect the (7p-4h) component would be stronger. One gets no help by looking at shell model wave functions. McGrory's wave functions, which include $p_{1/2}$, $d_{5/2}$, and $s_{1/2}$, have a set of states here that have holes in the p shell, and they are largely five-particle two-hole,⁹⁾ but

again he has no alpha clustering in his model, just because he's ignored the $p_{3/2}$ and the $d_{3/2}$. Nobody has yet done shell model calculations in this region that include all the p shell and all the sd shell. I would bet, if I were a betting man, that these are (7p-4h) states.

There's a little hint that that might be the case in Fig. 21,

Fig. 21

but nobody is obligated to believe that. These are the energies measured from the lowest member of the band as a function of $J(J+1)$ for the $3/2^+$ band in ^{19}F and the ground state rotational bands of ^{23}Na and ^{21}Ne . The energies are very similar in all three cases. The kinking is in the same direction in all three nuclei. Energies in ^{19}F are much closer to those in ^{23}Na than to those in ^{21}Ne , so if you believe any of this, it would slightly favor these being simply weak-coupling holes in ^{23}Na rather than in ^{21}Ne . There is no other reaction that one can think of doing that would answer the question of which their configuration is. There are no direct five-particle transfer or seven-particle transfer reactions yet known. If there were, those would be good reactions to do, but at present the evidence favors these being (7p-4h) states. But they could still be (5p-2h) states. It is known they are not just excited (sd)³ states, because no shell model calculation that includes only particles in the sd shell has any other states down low, so these are core-excited states.

The fact that they are not seen in any single-particle or two-particle stripping reactions also indicates they are core-excited states.

You remember yesterday we discussed ^{20}Ne and the ground state band, which has spin 0^+ , 2^+ , 4^+ , 6^+ , 8^+ . Vogt says the 8^+ state is not in the ground-state band.¹⁰⁾ The $B(E2)$ for the $8^+ \rightarrow 6^+$ transition is about 7.5 Weisskopf units, which is, within experimental error, equal to the value you would expect from SU3 if these states were all in the same SU3 classification and were all in the ground-state band. So it became important to determine whether or not the 8^+ was in the ground state band or in what Vogt called his super-band (it was our (8p-4h) band).

His evidence that this 8^+ is not the ground-state band comes from the alpha width. The measured alpha width for the 6^+ state is $110 \text{ eV} \pm \text{about } 30 \text{ eV}$, and for the 8^+ state was $35 \pm 10 \text{ eV}$. Vogt calculated the alpha-particle spectroscopic factors from these widths using single-particle widths obtained from Coulomb penetrabilities at some fixed radius. He modified the width for the fact that the nucleus really has a diffuse surface by making a correction to this width. He made the same correction for all l values. He never put in the fact that there was a potential inside the nucleus. If you do that,¹¹⁾ you get quite different results from what he had. You get single-particle widths of about 520 eV and 350 eV for states at those two energies, so that the predicted spectroscopic factor, which is just the ratio $\Gamma_\alpha / \Gamma_{sp} = S_\alpha$, is 0.21 for the 6^+ state and 0.10 for the 8^+ state. The other members of the ground-state band (0, 2, and 4) all have spectroscopic factors of .2 (to one significant digit). So the 6^+ agrees with that, and the spectroscopic factor for the 8^+ state differs only by a factor of 2 from that.

ALPHA-PARTICLE REDUCED-WIDTHS FOR LOWEST 6^+ AND 8^+ STATES IN ^{20}Ne

J^π	$\Gamma_\alpha \text{ (eV)}$	$\Gamma_{sp} \text{ (eV)}$	S_α	$S_\alpha (^6\text{Li}, d)$
6^+	110 ± 30	520	0.21	0.22
8^+	35 ± 10	350	0.10	0.15

It has been known for quite some time that Coulomb penetrabilities are not good enough to get spectroscopic factors for unbound states. If you do that, and then do a transfer reaction to the same state in which you transfer the particle with respect to which the state is unbound, and compare the two spectroscopic factors, you get good results if you use Woods-Saxon potentials, poor results if you simply use Coulomb penetrability. So, with no other evidence, I would believe these numbers and not believe Vogt's numbers. But we don't have to believe these numbers; we have S_α from the $({}^6\text{Li}, d)$ reaction on ${}^{16}\text{O}$, and they are .22 and .15. These two should be the same measured quantity. These two numbers agree very well. This is probably the worse disagreement we've seen for states with reasonably large spectroscopic factors, but the difference is in the same direction I mentioned yesterday. Namely, if the two disagree, spectroscopic factors in $({}^6\text{Li}, d)$ are always larger than calculated from the level width, indicating perhaps some other mechanism is contributing to the $({}^6\text{Li}, d)$ cross section. The upshot of all of this is that the alpha particle spectroscopic factors for the 6^+ and 8^+ states are the same and are the same as those for the other members of the ground state band, and they are equal to the spectroscopic factor you would expect from SU3, namely .2, so we do think this 8^+ state is in the ground state band. I think that is rather confirming evidence.

Arima and Yoshida¹²⁾ pointed out in a Physics Letter that you could make these two numbers equal by decreasing the radius slightly for the 8^+ state. You don't have to decrease it much to cause a factor of 2 in the width. They made an argument about something called "anti-centrifugal stretching" which says you would expect the nuclei to get smaller as you get larger spins, which is contrary to intuition, and then they used that to say, well, since that is what you would expect, there is every reason to decrease the radius of the Wood-Saxon potential for the 8^+ relative to the 6^+ . Then Lee and Cusson¹³⁾ did a Hartree-Fock calculation for this rotational band in ${}^{20}\text{Ne}$. They calculated the root-mean-square radius for all the states, and they got the results that the 8^+ had a root-mean-square radius of .3 Fermi less than that for the 6^+ . The reason for that has nothing to do with the radius of the potential; it has to do with the fact that the 8^+ is not where it ought to be. If

you plot the energies as a function of $J(J+1)$, they don't lie on a straight line. The 8^+ is lower than it should be. If you remember yesterday, the rotational band bent over for the 8^+ , and it is that result that makes it have a small radius, not anything else.

In fact, if you take a Woods-Saxon potential, keep the radius parameter constant for all the spins, calculate the wave function, and then calculate the mean square radius, it turns out to be .3 Fermi smaller for the 8^+ than it is for the 6^+ , using the same radius parameter for all the states. So, the anti-centrifugal stretching is what you would expect from a simple cluster model calculation of these states in ^{20}Ne .¹⁴⁾ It is nothing strange at all, but it doesn't mean you must decrease the radius of the potential for the high spin states. The anti-centrifugal stretching is there if you keep the radius the same, so there is no justification for changing the radius of the potential for different spins.

Question: You calculate the states and the potential for the same phases?

Answer: We vary the well depth to make the energy come out right. It is not the same potential. You've got to vary something to make the energy right. It is well known that in ^{20}Ne the energies go roughly like $J(J+1)$, but it is not the kinetic energy that goes like that. It is not a true rotator where the energy comes from kinetic energy; it is all potential energy. The potential energy is a function of J and goes like $J(J+1)$, except the 8^+ is not quite at the right excitation energy. If you keep the radius constant to make the 8^+ come out at the right energy, the well depth is not going to be in a straight line with $J(J+1)$; it deviates slightly. That's enough to make the radius smaller. To bind the 8^+ more than it would be if it were on a straight line, you must make the well depth deeper, which pulls it in, so that the radius is smaller.

Question: Why are you sure you should be varying the well depth and not the radius?

Answer: Because I think the radius is a much more physical quantity than the well depth. It is already known that the well depth

is different for each state, roughly like $J(J+1)$, but this agrees with what one normally does for stripping to bound states. You keep the radius fixed and vary the well depth to make the energy come out right rather than varying the radius.

Question: Is there a physical argument why the kinetic energy should go like $J(J+1)$?

Answer: Well, there is a term in the potential energy that it has been argued should go like $L(L+1)$ in the quadrupole-quadrupole force, I think, and there is essentially no rotational kinetic energy for any of these states. It is all in that $\vec{Q} \cdot \vec{Q}$ term or whatever.

Question: That is not really a different thing.

Answer: It gives you the same energies, but it's a quite different physical reason for the energies being different. It means that if you are going to describe these states as eigenstates of some Hamiltonian with a given potential, that potential has to be different for each L value.

Question: This is just the shell model approach to the rotational band?

Answer: Right.

Well, that's the conclusion to that short subject. Fig. 22

Fig. 1. $^{12}\text{C}(^6\text{Li},d)^{16}\text{O}$. Deuteron spectra obtained at 20° and 40° .

Fig. 22

goes back to something we saw the first day, the two 4^+ states in ^{16}O at 10.36 and 11.10 MeV. This 10.36-MeV state has 100 times as much alpha particle spectroscopic factor as the 11.10-MeV state, and yet they are almost comparable in this reaction. If the process were one with simple alpha particle transfer, the 10.36-MeV state would be 100 times stronger. They are the same spin and parity, and very nearly the same excitation energy, yet they differ in strength by only a factor of 2. That difference is roughly constant for a number of different alpha transfer reactions and a number of different bombarding energies. One is shown in Fig. 23. This is $(^7\text{Li}, t)$, and here the ratios are more like 3 to 1

$^{12}\text{C}(^7\text{Li}, t)^{16}\text{O}$. Triton spectrum at 22° for an excitation energy in ^{16}O from 0 up to 11.2 MeV. ^{20}Ne levels are excited by $(^7\text{Li}, t)$ in ^{16}O contaminations of the target.

Fig. 23

instead of 2 to 1, but it is never as different as 100 to 1. Why is that? The mechanism responsible for populating this state is clearly not a compound-nucleus mechanism since the energy dependence is not at all what you would expect for a compound-nucleus process. I think it is very easy to see what is causing the problem. These states are all (4p-4h) states, composed of the four holes that make ^{12}C and the four particles that make ^{20}Ne . The ^{12}C nucleus has an 0^+ ground state, a 2^+ at 4.43 MeV, and the 4^+ is at about 14 MeV. The ^{20}Ne nucleus has a 2^+ at 1.63, 4^+ at 4.25 and so on. (Fig. 24) If you want to make states in ^{16}O that are (4p-4h) states, you can make a state that is what I will

Fig. 24

call 0×0 , in which the four particles of the ^{20}Ne ground state are the 4 particles, and the 4 holes are the ^{12}C ground-state four holes. Then make the 2^+ state, which is 0×2 , and it should be at 1.63 MeV above this. You can then make another 2^+ state up here that would be 2×0 , which would be the four holes coupled to 2^+ and the four particles coupled to 0^+ .

You can then make 4^+ states; the " 0×4 " is the ^{12}C ground state coupled to the 4^+ state of ^{20}Ne . You can make another 4^+ state by coupling the 2^+ of ^{12}C to the 2^+ of ^{20}Ne , and that would be 2×2 .

In actual fact, at that energy you can make a triplet of states. If you couple 2^+ to 2^+ , you also get a 2^+ and a 0^+ . The important thing is that you can make two low-lying 4^+ states. One of them is 0×4 , and the other is 2×2 . Looked at simply, the 0×4 state is an $L = 4$ alpha particle outside of the ^{12}C ground state. This 2×2 is an $L = 2$ alpha particle outside of the ^{12}C 2^+ state, so without mixing this 2×2 state would have no spectroscopic factor for decay to the ^{12}C ground state. The decay of the 0×4 would go strongly, but it turns out it is

energetically forbidden, so it doesn't go. The 0^+4 state would have a very large spectroscopic factor. This spectroscopic factor is known, and it's within about 20% of the spectroscopic factor for the 4^+ state of ^{20}Ne , so these states do not mix very strongly.

If you do inelastic scattering on ^{12}C with a heavy ion like ^6Li , you make the 2^+ state of ^{12}C very, very strongly. If you've got ^{12}C in its 2^+ state, it is very easy to make the 2×2 state by transferring an alpha particle to the 2^+ . That is a two-step process in which you inelastically scatter and then strip an $L=2$ alpha particle to the excited state. So that seems to be the mechanism responsible for making this state. Tamura, who has now got a magic distorted wave code that does everything except stripping to unbound states (he assures me that he is going to put that in), is going to do the calculations for this process. My guess is that it will turn out that with the same spectroscopic factor for transitions to the two 4^+ states, you can fit those cross sections. We shall see eventually.

Question: If the two-step process is included, the possible inverse has to be also.

Answer: The inverse is in ^{20}Ne , and you don't have any ^{20}Ne around.

We saw yesterday that the $^{12}\text{C}(^{10}\text{B},d)^{20}\text{Ne}$ reaction (Fig. 25) had

Fig. 25

many of the properties of a direct transfer reaction at energies around 20 MeV, for direct transfer of two alphas. At higher bombarding energies the selectivity seems all to be one of high spin. All the states seen strongly are those of high spin, and integrated cross sections are proportional to $2J+1$, so something strange has happened when we go to higher bombarding energies. The mechanism for making these states seems to be one of simple compound-nucleus formation. These states seem to be selectively excited because they have high spin, not because they are multi-particle, multi-hole.

Why aren't the multi-particle, multi-hole states strongly excited? Well, there aren't any in this region. Hopefully that is the reason why. We are also way outside the angular-momentum window for making low-spin, multi-particle, multi-hole states. If that argument is any good, then somewhere at high excitation we should see a high-spin state that is the $(8p-4h)$ 8^+ state, so we must look further up to see what happens. It is a little perplexing to see only high spin states sticking up and nothing else, but I think it is understandable. There are no $(8p-4h)$ states in this region.

People have looked for other 8 particle transfer reactions. In particular, Fig. 26 shows one such reaction that had been previously

Fig. 26

thought to be an eight particle transfer reaction, namely $(^{14}\text{N}, ^6\text{Li})$. These data are from Oxford¹⁵⁾, and they are the $^{12}\text{C}(^{14}\text{N}, ^6\text{Li})$ reaction to states in ^{20}Ne . All the angular distributions are symmetric about 90° ; they all have angular distributions that go like $1/\sin\theta$, and they all have total cross sections that are roughly proportional to $2J+1$. So it looks like this reaction has no features that are direct eight-particle transfer. It is all compound. The low-energy data, if you look at it carefully, is clearly not direct either because it doesn't selectively excite the eight particle - four hole states. It selectively excites all states.

The $(^{16}\text{O}, ^8\text{Be})$ is another candidate for an eight particle transfer reaction. These data (Fig. 27) are from Florida State.¹⁶⁾ The

Fig. 27

selectivity observed in this reaction, if any, is one of Q-values and high spin, and not one of structure. These (8p-4h) states are not selectively excited. The excitation functions also suggest, as shown in Fig. 28, that it is not a direct 8-particle transfer reaction. They oscillate rather rapidly. But you remember that the $(^{12}\text{C}, \alpha)$ reaction also oscillated, so that itself is not evidence that it is not a direct reaction, but the lack of selectivity seems to indicate that this is

Fig. 28

not direct eight-particle transfer.

We have done a number of other heavy-ion reactions just to see what happens. One such case is shown in Fig. 29. This is $^{12}\text{C}(^{12}\text{C},p)^{17}\text{O}$. Again, this is an excellent way to make states whose decay you would like to study. This reaction is extremely favorable because you bring in a tremendous amount of angular momentum and carry out almost none. At all these energies the proton carries out about one unit of angular momentum or two, and the carbon brings in 8, 10, or 12. In fact, you can see that very clearly in this spectrum taken at 39 MeV. There is no state strongly excited up until state number 20, which I think is the $13/2^+$ or $11/2^+$. The reason is that at energies below this, the proton simply cannot carry out enough angular momentum to make those states. These spins are too low; the ^{12}C brings in a lot of angular momentum; the proton cannot carry out any, so those low-lying states aren't formed. As you go down in bombarding energy, say at 30 MeV, you then begin to make those states.

Question: What is the size of the cross section you have there?

Answer: These are about 100 $\mu\text{b}/\text{sr}$. I guess some of the strong states are close to a millibarn, but cross sections are down roughly a factor of 5 to 10 from the alpha channel. You see this in all these (heavy-ion,p) reactions.

The cell is about 1 1/2 inches in diameter, very small. The outgoing reaction products go out through a Parylene window over the forward quadrant and the back quadrant. That stuff is very uniform. For those of you who have done gas cell experiments, you can appreciate the fact that with 10-MeV alphas we have 20-keV resolution through this material. Then the beam exit window depends on what the reaction is that we are doing.

Fig. 30

also have an outgoing slit system that is just a tube with a nickel window on it; the beam then goes on to the Faraday cup. Then we have another exit port. It contains the Faraday cup sitting on the gas cell isolated with a ceramic piece. So, those are the differentially pumped gas cells. We also have a rotating gas cell and a gas cell with closed window for experiments with protons and deuterons. I could go on and on; I hadn't planned to, but I realize I could have talked for two more hours with the slides I brought with me. But this is a very convenient place to stop. I'll simply say again that I have enjoyed this very much. Thank you.

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Dr. Garcia, of the University of Arizona, has been active in the theoretical interpretation and understanding of phenomena involving ionization and excitation of atoms induced by collision processes.

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POINT CHARGED PARTICLES: BORN AND BEA APPROACHES

Today I would like to discuss the binary encounter approximation and its use in computation of inner-shell vacancy production. The binary encounter method was originated by J. J. Thomson in 1912;¹⁾ in fact, J. J. Thomson's calculations were one of the bases for the rejection of classical mechanics as applicable to the microscopic world, because everyone said, "Look, he has calculated this cross section classically, and the numbers that we get experimentally are very different." That was one of the pieces of evidence offered for the rejection of the classical analysis of the microscopic world. In 1965 Gryzinski²⁾ resuscitated this approximation. He put in a very important ingredient that Thomson had neglected, the motion of the particle in the target atom. I think if Gryzinski had done the 1912 calculation, it might have been a few years longer before quantum mechanics actually came about, because Gryzinski's results for hydrogen, which is the test case that one uses, were truly amazing when he first presented them at the Quebec conference in 1965.

The basis of the method is stated on the first slide (Fig. 1). One visualizes a target atom electron with a vector velocity $\vec{v}_2$ and a projectile with velocity $\vec{v}_1$. Subscripts 1 and 2 will consistently refer to projectile and target particles, respectively. What one does is assume that the exchange of energy is the only important ingredient in the interaction between these two particles. This is probably more true in the case of an ionization than it is in an excitation. As a matter of fact, the binary encounter method is notably unsuccessful at predicting excitation cross sections as opposed to ionization cross sections. Then one needs a cross section for the energy transfer between the two particles, each moving with their own velocity, and then you must sum over all energy transfers permitted for the process that you have in mind. We still have the orientation of vector $\vec{v}_2$ to worry about; surely we know something about that. We know, for instance, that in closed shells these orientations are isotropically distributed. In

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$$\sigma(\underline{v}_1, \underline{v}_2) = \int d(\Delta E) \sigma_{\Delta E}(\underline{v}_1, \underline{v}_2).$$

$$v_1 \sigma_{\Delta E}^{\text{eff}} = \frac{1}{4\pi} \int d\Omega_2 |\underline{v}_1 - v_2 \underline{n}_2| \sigma_{\Delta E}(\underline{v}_1, \underline{v}_2).$$

$$\sigma_{\text{ion}} = \int_{13.6}^{E_1} d(\Delta E) \sigma_{\Delta E}^{\text{eff}}$$

Fig. 1

fact, whether it's a closed shell or not, one can show that the orientation of this velocity vector is quantum mechanically isotropic. We can average over the direction of the vector $\vec{v}_2$, but if we try to do that, we are in trouble because the answer is infinite. The divergence of the Coulomb cross section guarantees that answer will blow up. So what you say is, "What I really want to calculate is v_1 times sigma." If we do that, then that answer is finite. And then, having averaged over all of those orientations, we must sum over all the possible values of the energy transfer to obtain the total cross section for the particular event. The case that I have on the slide is the ionization of a hydrogen atom; you must transfer at least the binding energy of the electron on a hydrogen atom, and the maximum energy transfer is the total energy of the incident projectile.

You may think that is all very well and good, but I haven't told you how to compute the cross section. If we do it classically, Rutherford told us all about that, so let's proceed with this calculation on a classical basis. Then you tell me, "But this is a quantum

mechanical system; what are you doing talking classical language?" That point is a bit more ticklish, and that is just the point I hope to elucidate at least a little bit.

What we are doing here in effect is adding the squares of the amplitudes for each particular energy transfer process. Quantum mechanically we should add the amplitudes and then square, so there may be some interference going on that we haven't accounted for, and this result may, in fact, not be a good approximation. It turns out, as we will see in just a moment, that adding the squares of amplitudes for the particular case of ionization is a very reasonable approximation. For other situations, such as excitation or charge transfer, the adding of the amplitudes and then squaring becomes extremely important. There are at least two ways in which one can approach a justification for this via quantum mechanical calculations. One is to proceed from the viewpoint of the impulse approximation, and the other is directly through the Born approximation. Let me first talk about how this procedure fits in, if it does, with estimates based on the Born approximation.

As you know, the Born approximation to the amplitude for an ionization can be written fairly simply. The electron that is ionized starts from some initial state ψ_0 and has a resultant momentum κ . M is the mass of the projectile (particle 1). We have an interaction between particles 1 and 2 that we describe by a function $U(r_{12})$. The incident particle comes in with a momentum $\vec{k}$, so its wave function is simply a plane wave. (The normalization of that wave function is included in the factor in front of the integral.) In the final situation, after the interaction, the scattered projectile has a momentum $\vec{k}'$ and the target electron, which is now in the continuum, has a momentum $\vec{\kappa}$, as indicated above. The expression for the amplitude then is:

$$f_{\text{ox}} = \frac{-m}{4\pi} \int e^{i\vec{k} \cdot \vec{r}_1} \psi_0(\vec{r}_2) U(r_{12}) e^{i\vec{k}' \cdot \vec{r}_1} \psi_{\kappa}(\vec{r}_2) d^3r_1 d^3r_2 .$$

It is convenient to rearrange this expression. We introduce the momentum transfer $\vec{q}$, which is the difference between $\vec{k}$ and $\vec{k}'$. The rewritten expression

$$F_{0\kappa} = \frac{-m}{4\pi} \int U(r_{12}) e^{i\vec{q} \cdot \vec{r}_{12}} d^3r_{12} \underbrace{\int e^{i\vec{q} \cdot \vec{r}_2} \psi_0(\vec{r}_2) \psi_\kappa(\vec{r}_2) d^3r_2}_I.$$

The integral I in this expression we will represent as follows:

$$I \equiv (2\pi)^{3/2} \epsilon_{0\kappa}(q).$$

This is all standard stuff; anyone who has read Schiff, for example, recognizes exactly what I am doing here.

The quantities $|\epsilon_{0\kappa}(q)|^2$ are the generalized oscillator strengths. The interaction $U(r_{12})$ is just $1/r_{12}$. The result of all this is that we can write the differential cross section, in the Born approximation, for a momentum transfer q in the following form:

$$d\sigma = \frac{4\pi}{E_1} \frac{dq}{q^3} |\epsilon_{0\kappa}(q)|^2 \kappa^2 d\kappa d\Omega_2.$$

On the other hand, if we use the classical procedure to calculate the same cross section, we obtain

$$d\sigma_{Cl} = \frac{2\pi}{E_1} \frac{dq}{q^4} \frac{d\Delta E}{v_2}.$$

This is obtained by straightforward use of the Rutherford cross section, averaged over angles. There isn't anything very surprising about that result.

When we look at these cross sections, we see that we can make a connection between the two relations. We still have to do the integral over Ω_2 in the first formula. If I let the generalized oscillator strength be simply a delta function,

$$|\epsilon_{0\kappa}(q)|^2 = \frac{1}{4\pi v_2^2} \delta(|\vec{q} - \vec{\kappa}| - v_2),$$

and then integrate over Ω_2 , I get exactly the classical expression. So there is some limit in which the Born approximation becomes the classical

expression. What limit is that? Let's examine what goes into the generalized oscillator strength. What goes into it is essentially the Fourier transform of an overlap of wave functions. Suppose for the moment we let ψ_{κ} be a plane wave instead of a proper Coulomb wave. Then it becomes simply the Fourier transformer of the wave function describing the initial state. In that limit, the generalized strength looks as follows:

$$|e_{\text{OK}}(q)|_{\text{PW}} = \frac{8}{\pi^2} \frac{Z^5/n^5}{[|\vec{q} - \vec{\kappa}|^2 + Z^2/n^2]^4}.$$

In writing this, I have had to decide what to use for an initial state. I have kept it general and used a state of principal quantum number n . You say, "Well, that is cheating because the wave function for the ground state may give this, but the wave functions for all excited states, even of hydrogen, look different." But it is an interesting miracle of the Coulomb force that, so long as it's absolutely a pure Coulomb force, the sum over all l for a given n of the momentum space wave functions squared always produces exactly the result that I have written down here. So, if we stay with pure Coulomb cases, we have not approximated at all, thus far.

It is fairly obvious that in the limit as n approaches infinity we have

$$\lim_{n \rightarrow \infty} |e_{\text{OK}}(q)|_{\text{PW}} \rightarrow 0 \quad \text{so long as } |\vec{q} - \vec{\kappa}| \neq 0.$$

Then, of course, the integral over momentum space is always 1 since we have a normalized wave function. So the integral is 1, and the function itself 1, and the function itself is zero everywhere. That's exactly the requisite for it being a delta function, so in the limit of n approaching infinity we get the classical result back. Now you sit back and chuckle to yourself and say, "This silly man; we know that for continuum states the quantum mechanical and classical cross sections are identical, so why is he deriving the identity that we all understood to begin with?" The point is that n doesn't have to be very large before this result is

a very good approximation. In fact, for n as small as 3 or 4 we already have essentially an identity in the two cross sections.

Let me demonstrate that by using electron impact ionization cross sections as computed via the classical method and via the full Born approximation.³⁾ The next slide shows the results for electron

Fig. 2

impact on hydrogen. The dashed line is the Born result, and the solid line is the binary encounter result. It differs from the Born approximation by no more than 15% anywhere. By the time you get to $n = 4$, the differences are everywhere less than a few percent. I could not plot the differences between the Born and classical results for $n = 5$. Both computations are available, but I couldn't show it to you on a graph like this; to the accuracy that either calculation is done, there is no

difference. So, $n = 5$ is the infinite limit here, if you like.

Question: You have illustrated that under the conditions where your Coulomb wave function goes to a plane wave, the two calculations should be similar. But that doesn't happen until you get to extremely high velocities. The velocity ratio for that to happen, the ratio of the incident velocity to the orbiting electron velocity, has to be something like... (interrupted)

Answer: But the difference that you want is the momentum transfer compared to the outgoing momentum. It's that quantity that contributes massively to the cross section. In fact, I'll show you in just a moment that the ejected electron distributions peak at zero energy, but still contribute considerably at energies that are non-zero. For the peaking at zero energy, this approximation is terrible. Detailed comparisons have been made between the classical and Born expressions for just the situation that I have here, and they show that it is really a very bad approximation. But a fair fraction of those contributions are from regions in which the ejected electron energy is non-zero, and as soon as it is non-zero, as soon as it has any reasonable value, then the words that I said here lead to reasonable agreement between the two, amazing agreement I think.

You might say, "All right, maybe we get some sort of accidental cancellations of bad results. Show some differential cross sections directly; don't show us the integral of all this over all of these variables." On the next slide^{3a)} I have such a plot, giving comparisons with the Born approximation and data as well. This is for 100 keV proton impact on helium. It shows the ejected electron distribution, or differential cross section, as a function of ejected electron energy at a particular proton bombarding energy. The points indicated by x's are binary encounter results in which hydrogenic velocity distributions have been used for the speed distribution of the target electron. The dashed curve results from using a delta function speed distribution corresponding to the virial-theorem result for the Coulomb force:

$$f(v_2) = \delta\left(\sqrt{\frac{2u}{m_e}} - v_2\right) .$$

Fig. 3

That means simply a mean velocity is being used for the velocity of the bound electron. Again, if you use the full hydrogenic velocity distribution, the results look very much like the Born approximation. The data agree with both of those moderately well. There is a false cutoff of the dashed curve. That arises because classically a heavy charged particle cannot transfer a large fraction of its energy to a lighter, slower one, or quantum mechanically for that matter. This slide indicates that if there are fortuitous cancellations, they occur at a level deeper than first differential.

I want now to talk about the reasons for doing all of this. We do have what we consider reasonable quantum mechanical approximations. Why go to something for which you have to stand up and give apologies? There are several reasons, and I think not insignificant reasons. The first thing is that it is very simple. You can write down these expressions in a few lines; you don't need a computer and you don't

need any fancy analysis to it; you just crank out the numbers. It's very simple. That's really not a reason, but it does affect how people think of it. Experimentalists have direct access to this, whereas to do some of the fancier quantum mechanical calculations they either have to spend a lot of time themselves or ask a theorist friend to crank it out for them. More important than that, I think this provides a simple picture, as simple a picture as the simplest quantum mechanical approximation, and the orientation of that picture is slightly different.

With the aid of Fig. 4 let me discuss some fundamental

Born Approximation

$$\sigma \sim \frac{2\pi}{k} |V_{fi}|^2 \rho_f$$

$$V_{fi} = \int d\vec{r}_1 d\vec{r}_2 e^{-i\vec{k}'_1 \cdot \vec{r}_1} \psi_f^*(\vec{r}_2) \frac{e^2}{r_{12}} e^{i\vec{k}_1 \cdot \vec{r}_1} \psi_i(\vec{r}_2)$$

$$E_f - E_i = \frac{\hbar^2}{2m} (k_1^2 - k_1'^2)$$

Impulse Approximation

$$\sigma = \int f(k_2) d\vec{k}_2 \sigma_{AE}(\vec{k}_1, \vec{k}_2)$$

$$\sigma_{AE}(\vec{k}_1, \vec{k}_2) \sim \left| \int \psi_{\vec{k}'}^c(\vec{r})^* \frac{e^2}{r} \psi_{\vec{k}}^c(\vec{r}) d\vec{r} \right|^2$$

$$\vec{k} = \vec{k}_1 - \vec{k}_2, \vec{k}' = \vec{k}_1' - \vec{k}_2, \Delta E = \frac{\hbar^2}{2m} (k_1^2 - k_1'^2)$$

Fig. 4

differences between the two. In a Born approximation you are required to think of the effect of the projectile as being simply a small perturbation on the atomic system. You essentially use the Golden Rule and calculate the matrix element V_{fi} , the same one I wrote on the board, with your viewpoint being that mostly nothing happens to this system, but for a very, very small fraction of the interactions between say a proton and an atom, the proton tickles the atom, and you calculate

it on the basis of perturbation of the atomic state of the system. In the binary encounter approximation, you say, "That's not how I want to think about it; I want to think about it as being fundamentally an interaction between the incident particle and the bound electron. I am not going to worry about all that other stuff in the atom. I am going to worry only about how this incident projectile affects a given target electron." You sum over all possible modes of interaction between those two particles, conserving energy and momentum exactly. In the Born case you require only energy conservation for the entire interaction. In the classical case you require momentum and energy conservation exactly between the incident particle and the target electron, so you neglect recoil. That is one of the first things that enters into the picture. So this is really a different picture. The same picture can be built quantum mechanically, and it is called the impulse approximation versus the Born approximation, I don't think there is any clear understanding as to under what circumstances which should be preferable. So I guess I am not prepared to discuss that issue with you.

One of the nicest features of the binary encounter approach is that it yields scaling laws.^{4,5)} All we have built into it is the velocity distribution of the bound electrons and the Coulomb cross section. The scaling laws are a direct result. What you find is that the product of the binding energy squared times the cross section, say for K-shell ionization, divided by the projectile charge squared is a universal function of only the ratio of the projectile energy to the binding energy of that electron and a quantity λ , which is the projectile mass in electron mass units.

$$\frac{u^2 \sigma_K}{Z_1^2} = f(E_1/\lambda u, \frac{1}{\lambda}) \quad , \quad \lambda = \frac{m(\text{projectile})}{m_e} \quad .$$

You can calculate this once and then apply it to any system you choose thereafter.

The Born approximation actually has similar scaling rules.⁶⁾ In the Born approximation one introduces a parameter θ , which is

essentially a measure of how well the screened hydrogenic functions commonly used in the Born approximation reproduce the actual binding energy in the particular atomic system, and a parameter η , which is essentially a measure of the incident particle energy in units of the hydrogenic binding energy. The expressions for these parameters are

$$\theta_s = \frac{u_s}{(Z_s^2/n^2)} \quad \text{and} \quad \eta_s = \frac{1}{Z_s^2} \left(\frac{Mv}{e} \right)^2$$

where we are referring to the s th shell and Z_s is an effective charge (to account for screening). It turns out in the Born approximation that what one plots when making comparisons with experiment is the cross section multiplied by the effective Z^4 and divided by the projectile Z^2 versus η ;

$$\frac{Z_s^4 \sigma_s}{Z_1^2} \quad \text{vs} \quad \eta_s$$

If one simply multiplies by θ^2 , one obtains just

$$\theta^2 \left(\frac{Z_s^4}{Z_1^2} \right) = \frac{u_s^2}{Z_1^2} ;$$

and if one divides eta by theta, one obtains

$$\frac{\eta}{\theta} = \frac{E}{\lambda u}$$

You see that the Born scaling parameters and the binary encounter scaling parameters are similar. I must note that neither theory requires any parameters. These scaling parameters are simply convenient handles for scaling from one atom to another. One can use *ab initio* numbers in both of these theories; they are not parametric fits.

Fig. 5⁷⁾ shows a comparison of the impulse and Born

Fig. 5

approximations. Recall that λ is the projectile mass in electron mass units and u is the target electron binding energy. These are all ionization results for K shells. I would like for you to pay a bit of attention to the order in which these curves occur. Curve B is the Born approximation with $\theta = 1$; in that case the binding energy is accurately reproduced by the hydrogenic wave function you use. Curves C, D, and E are for decreasing values of the parameter θ . Whatever the choice of θ here, the Born estimates are all below the binary encounter values.

I would now like to very quickly go through some comparisons of numerical results with experiment, particularly for inner shells. This will be primarily for point charge particles, or those particles

that somebody thinks are pure point charges when they interact; I guess we could discuss that at some length. They are heavy particles, heavier than protons. The observed variable is the emitted x ray. In order to convert that into the vacancy production cross section, you have to use the fluorescence yield, that is, the relative number of times that a vacancy, when produced, will result in an x ray. For comparison purposes I have used theoretical values of the fluorescence yields, partly because the experimental values have large spreads, but mostly because this provides more direct comparison of the experimentally determined quantity with theory, avoiding the larger uncertainties associated with experimentally determined fluorescence yields.

Fig. 6⁷⁾ shows results for protons on K shells. I don't want

Fig. 6

to take the time to describe what all these symbols mean; there are many elements represented, all the way from beryllium to gold, with projectile energies ranging from 20 keV to 160 MeV. All those results are condensed onto this graph. It's a log-log plot, as theorists are wont to do - that hides all trash that you would otherwise see if you plotted it using linear scales. The dashed curve is the Born approximation; the solid curve is the binary encounter result. The open triangles at the peak are for 160 MeV protons. They may be showing relativistic effects. Don't worry about them. Also don't worry about the open circles with a vertical bar; they are for tantalum, which may also be showing electron-related relativistic effects. No relativistic effects have been calculated here, so those two are unfair comparisons. The rest of the data seems to lie near the curve, but somewhat below.

Fig. 7⁷⁾ shows exactly the same thing for L shells. The

Fig. 7

solid curve is the binary encounter result. The agreement for both L shells and K shells, except for a few points in Fig. 6 that were dragging their feet, considerably down, seems to be somewhere between 30% and a factor of 2. That's the kind of numbers that seem to arise from these comparisons.

The disagreement between the Born curve and the binary encounter curve in Fig. 6 is, I think, quite understandable. The classical theory has something that I haven't told you about; namely, we have built into that one the effect of the repulsion of the nucleus on the cross section. The Born curve here is just a plane wave Born curve. Corrections can be made; instead of using plane waves for the projectile, you use Coulomb waves, bent in the field of the nucleus. Bang and Hansteen⁸⁾ have done that, and it brings the two results pretty much into agreement to the left of the peak.

Up at the right of the peak is another disagreement; this disagreement makes these curves become parallel with about the spacing shown as the energy of the projectile increases. This disagreement is fundamental in the sense that the Born approximation takes the recoil of the nucleus into account. Remember I mentioned that at one stage; the Born approximation adequately provides for recoil of the nucleus, whereas the binary encounter approximation requires exact momentum and energy conservation only in the interaction of the target electron and the incident projectile. There is no recoil of the nucleus built into it, and this deviation is symptomatic of that. If one permitted a spread in momentum transferred in the classical approximation, one could pick that up as well, but frankly it is not worth the effort.

Fig. 8⁵⁾ shows a comparison in which the Born calculation has been corrected by Bang and Hansteen for repulsion of the nucleus. The Bang and Hansteen results are triangles, the plane wave Born results are squares connected by a dashed line, and the binary encounter results are on the solid curve. The experimental points are open circles.

Question: I'm not clear about the difference in the effects for nuclear repulsion and for nuclear recoil.

Answer: Recoil is something that affects momentum transfer.

Fig. 8

It is certainly not allowed for in the classical calculation. That was explicitly ruled out because we conserve momentum and energy in the interaction between the projectile and the bound electron. The Born procedure, when carried through exactly, even with plane waves used, does allow the nucleus to recoil. But now if the projectile's path is bent, I would not use plane waves; I would use Coulomb deflected waves. That deflection has two effects. First, the Born approximation as I have written it would have the projectile come in and go straight on through, as in the dashed line.

If we allow repulsion, then we have a trajectory that looks something like the solid line, so that the distance at the time of interaction with this electronic shell is slightly greater than for the initial impact parameter. Also, the velocity of the projectile is changed; at the turning point its velocity is a minimum. Those are the two major effects that Bang and Hansteen built into their model. The same things, of course, can be done classically, and in fact have been done and are contained in the solid curve here and in all other curves that I have shown you thus far except for the electron impact results.

All of the comparisons that I have shown you have had logarithmic scales for either one or both axes. Experimentalists always say, "Well, you can hide all sorts of discrepancies if you do that; all differences become minuscule." For that reason, I have chosen to show you an alpha particle comparison with linear scales on both axes. This is shown in Fig. 9.⁹⁾ Both of these scales are now linear. The measurements here differ from the measurements on the last slide in that these are only thin foil measurements, so the stopping power didn't have to be used to reduce the data. I didn't talk about that, but you have probably heard all about stopping powers in the last few weeks, so I won't belabor it right now. You can see that all of these points are within 20% of the theoretical curve. In fact, more heartening than that is they are very well scaled by this parameterization. Even if you forgot about the theoretical curve, the fact that all those cross sections for all those different elements do group so nicely if you just plot them in this way would be pleasing.

Note that in going from protons to alpha particles we have changed the Z of the projectile. I asked you to remember that for protons the data lay generally below the binary encounter curve. You see on this slide that at the peak the data are generally above the curve, not by a lot, but a little bit. Fig. 10⁷⁾ shows you what happens with a further increase in the Z of the projectile. You see what I am talking about; this effect is creeping further and further up. The deviation is now a factor of 2 at the top where the agreement was best for protons. The deviation down here on the tail looks more drastic than that for

Fig. 9

protons. The alpha particle curve that I showed you didn't go down far enough or it would have shown it too. These deviations are the source of great current interest, as I am sure you are all aware. I would guess they are probably not fundamental deviations (in terms of fundamental interactions), but are more associated with the fluorescence yields. I think it would be very interesting to see corresponding Auger measurements for these events or to examine the x-ray spectra very, very carefully. I guess I remain unconvinced by this evidence, except that what one sees here is exactly what has been seen by Watson *et al.*¹⁰⁾ and by Brandt *et al.*¹¹⁾ in their work on the Z -dependence of the ionization cross sections for various projectiles. If one plots the deviation of the cross section from the theory, this trend that I have

Fig. 10

shown you here is displayed very clearly by experiments in which say protons, deuterons, and alpha particles are all compared at the same velocity. The comparisons can, I think, be made much more accurately in that case.

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5792 STRUCTURED PROJECTILES: THEORETICAL MODELING

I would like to begin by discussing the differences in the production of inner-shell vacancies for ions heavier than alpha particles compared to protons and alpha particles. If we look at Fig. 1,¹⁾ it becomes immediately obvious that there are gross qualitative

Fig. 1

differences. These cross section measurements are for production of vacancies in carbon, as determined by carbon x rays. The cross section σ_I has been determined by use of the fluorescence yield, which you may or may not wish to believe. That number could change these results by

open, up-pointing triangles. The remaining points are other miscellaneous observations. I have not included any gas target x-ray data here because of the fluorescence yield problem, which we will have to talk about in just a little while. I have included, however, some Auger data taken with gaseous targets. What we should have here is something that is in some not too unreasonable way related to the total cross section for vacancy production. I have divided by the full Z of the projectile squared. As you can see, there are still discrepancies of two orders of magnitude at low energies. There is not that much discrepancy at high energy.

Question: In some of these low energy things where you have projectiles heavier than the target atoms, how much of an effect will recoil corrections make?

Answer: So long as the projectile mass is less than or equal to the target atom mass, the corrections are of the order of 10% or less. For projectiles that are heavier than the target atom, most of the target x rays result from the projectiles hitting a target atom, then the target atom recoiling and hitting other target atoms, which then produce the x rays. For that process there are serious corrections to be made. According to Sigmund and Taubjerg,³⁾ who have done an analysis of this problem, the corrections can be a factor of 2, perhaps even a factor of 5, but not an order of magnitude for anything they have calculated. So, indeed, that problem arises and affects these points quite seriously. On the other hand, it will not affect the general conclusion that these are orders of magnitude bigger at low velocities than the corresponding point charge particle results.

By using the full projectile Z here, I have most likely divided by too large a number, and so, consequently, these points should be even further up the line than they are now. The effect is at least as big as I show here, probably bigger. Exactly analogous results occur for the L shell, as can be seen on the next slide.²⁾ The argon on argon results here are Auger electron measurements for a gaseous target. They are absolutely hugely larger than the corresponding point charge particle

Fig. 3

analysis would have you believe.

One of the things that plagues any discussion such as this is the problem of fluorescence yields. Most of the data - in this case with the only exception being the argon on argon data for the gaseous phase - comes from x-ray cross sections, which can be directly experimentally determined, but which have to be divided by fluorescence yields in order to be put onto this plot. Let me just show you how serious that problem can be. Fig. 4²⁾ shows the L-shell fluorescence yield of argon as determined experimentally by looking at argon on argon collisions. One has measurements of both the total Auger cross section and the x-ray cross section, and for small fluorescence yields, which these clearly are, just the ratio suffices to give you the fluorescence yield. The Auger measurements are a composite, some of them made by Ogurtsov⁴⁾ in Russia and some of them by Cacak et al.⁵⁾ at Nebraska. The x-ray measurements are those of Saris and Onderdelinden.⁶⁾ You can see that the fluorescence yield changes by a factor of 20 over a very short energy range.

Fig. 4

Question: What's the dashed line?

Answer: The dashed line is a theoretical value of the argon neutral atom ground state fluorescence yield. That is, you take a neutral atom, remove a 2p electron, and calculate what the fluorescence yield ought to be. The calculation was done by Walters and Bhalla.⁷⁾ The cross represents a measurement taken using 125 keV protons on argon.⁶⁾ It really shouldn't be plotted at that position; we should plot keV per amu in order to do a fair comparison, and that would move it off of the graph.

One thing one can do about fluorescence yields is think about the processes that are occurring. The x-ray rate can change if the number of electrons in the state that is going to decay to a lower state

changes. If you only want an estimate of how they are changing, then it changes linearly with that number as a rough approximation. The Auger rate depends upon two electrons doing something, one of them falling into the hole and the other being promoted upward and outward. So that depends not on the number but on $n(n-1)$. If you are comparing with the neutral atom yield, then the Auger rate for n electrons in a particular subshell as compared to the whole shell containing n_0 electrons would give you the ratio

$$\frac{A_n}{A_{n_0}} = \frac{n(n-1)}{n_0(n_0-1)} .$$

For x rays, the same comparison would give you

$$\frac{x_n}{x_{n_0}} = \frac{n}{n_0} .$$

Since the fluorescence yield is simply the fraction of vacancies that are filled by x-ray emission, the x-ray cross section is the fluorescence yield times the total vacancy production cross section,

$$\sigma_X = \omega \sigma_I .$$

The Auger cross section then is what is left,

$$\sigma_A = (1-\omega)\sigma_I .$$

In the so-called statistical approximation, one can estimate by simply saying that if you have a shell that has lost a few electrons, then scale down your x-ray rate and scale down your Auger rate according to the ratios given above, and then see how the fluorescence yield has changed. This was first done by Larkins⁸⁾ for the case of argon; he just sequentially stripped the M shell of argon. The results of that are shown on Fig. 5.^{2,9)} The results are normalized to the fluorescence

Fig. 5

yield for the neutral atom, so the ratio is 1 when there are no vacancies. For argon you see it is a very steep curve; this is the difficulty that I was referring to earlier. On the other hand, if you do the same thing for copper⁹⁾ you get a variation that looks quite different.

The reason for the difference between the two cases is quite clear. Removal of outer 3p electrons from Ar has a strong affect on Auger rates, but does not affect x-ray rates since the principal L x-ray transition is the 3s→2p transition. In Cu, however, the 3d level is populated and the principal L x ray involves the 3d→2p transition; thus, as one removes 3d electrons from Cu both the x-ray and the Auger transition rates decrease at about the same rate, and the ratio, the fluorescence yield, is relatively unchanged. This continues to be the case

until a large number of 3d electrons has been removed. As one then continues to remove 3d electrons the Auger transitions involving 3p electrons become relatively more important and the Auger rate decreases at a smaller rate than the x-ray rate, i.e., the fluorescence yield drops. This continues until all the 3d electrons are removed. Once all the 3d electrons have been removed, we begin to strip 3p electrons, and so we have returned to a situation similar to that of Ar; the fluorescence yield increases with increasing outer-shell ionizations.

Of course this is an extremely crude procedure for determining the fluorescence yield; it only gives an estimate of what can happen. The only reason one would do such a thing is to see where real trouble is expected. The correct thing to do is to go ahead and calculate the x-ray rates with the given number of vacancies that you expect on the basis of spectral information, and then use the actual calculated rates in order to determine the total vacancy production cross section. I don't care to elaborate on that subject, because you have one of the world's leading experts sitting in residence here, and he can tell you all about that.

I would like to try to account for some of the differences that we saw in those slides for heavy-ion processes as compared to point charge particle processes. I think the differences are fairly obvious. For most of the processes that we were looking at here where the maximum deviations occurred, the velocity of the projectile was small compared to the velocity of the electrons in the given shell. In that case, as the atoms approach each other you can see the electrons adjusting to the presence of the new force center, and you actually obtain Pauli exclusion effects, which cause the electrons not to stay in the shells that they are originally in. So the explanation of those data qualitatively is not really very hard to come by. The question is whether we can do it quantitatively.

If we want to do it quantitatively, we have to solve the following very simple equation:

$$H\psi = i \frac{\partial \psi}{\partial t} .$$

It's a first order differential equation, and all we have to do is solve it. Everyone can solve a first order differential equation. In writing the Hamiltonian in question here, let's call one of the atoms A and the other one B. There will be the kinetic energies of the nuclei; there will be the interaction between the nuclei; there are electrons associated with each nucleus, so there will be the interactions of nucleus A with all the electrons and of nucleus B with all the electrons. Then there are the interactions of the electrons with one another and the kinetic energies of the electrons. We can write this Hamiltonian symbolically as

$$H = T_A + T_B + T_e + V_{A,B} + V_{A,e} + V_{B,e} + V_{e,e} .$$

One needs a boundary condition for such a problem. One way of writing that boundary condition is the following. Let's first break up this Hamiltonian into two pieces, one associated with each atom:

$$H = T_A + H_A + T_B + H_B + V_{A,B} + V_{e_A,e_B} + V_{A,e_B} + V_{B,e_A} .$$

I have made obvious associations of the kinetic energies of the electrons and the interactions of each nucleus with those electrons on it initially. H_A is just the standard atomic Hamiltonian for atom A; H_B is the atomic Hamiltonian for atom B.

All of the interactions that I have written to the right of the arrow go to zero as the distance between A and B goes to infinity, so our initial condition can be written by ignoring those terms and solving the Schrödinger equation for remaining part of the Hamiltonian. That's not too difficult if I do it in the center-of-mass system. If the center-of-mass momentum of atom A is $\vec{k}$, then I can write the following initial condition for the problem:

$$\psi(t_0) = A(\exp(i\vec{k} \cdot \vec{r}_A) \phi_A \exp(-i\vec{k} \cdot \vec{r}_B) \phi_B) G_A(\vec{R} - \vec{R}_I) .$$

This is only one representation, one approach that one can use, but it will do. This A here is the antisymmetrization operator required to

antisymmetrize all of the electrons; they are already antisymmetrized within each of the wave functions ϕ_A and ϕ_B corresponding to atom A and atom B, respectively. This is a big plague; this full antisymmetrization creates immense difficulties in carrying out a calculation. The function $G_A(\vec{R}-\vec{R}_I)$ describes a wave packet in the coordinate $\vec{R} = \vec{r}_A - \vec{r}_B$ centered at position $\vec{R}_I$ and having an initial width Δ that is large compared to the de Broglie wavelength of the relative motion. We assume that the two atoms are well separated initially, separated such that no exchange effects are significant.

Now we are all done; we have just a simple first order differential equation and an initial condition, so we can go home. Well, not quite! What I would like to do first is outline the kinds of developments of these wave functions in which we might be interested for given types of collision problems. Since I can't solve the whole problem, and there is no hope of ever solving the whole problem, and there is probably no interest in all the details of a full solution to the problem, what we do is try to break up the problem into regimes that we can hope to understand. Suppose we transform this wave function into momentum space. Let me make the following crude but perhaps helpful plot for this. Let's look at the z component of momentum associated with each atom; I will plot time along the horizontal axis and p_z along the vertical axis. Suppose we forget for the moment the full antisymmetrization; the wave functions for atoms A and B are already antisymmetrized within themselves. For atom A the relative velocity in the center of mass is related to the vector $\vec{k}$ by the relation $\mu\vec{v} = \hbar\vec{k}$, where μ is the reduced mass. Associated with each electron is a momentum about center A, and the center A itself has a relative momentum of this much. Initially, at times that are very large and negative, we might find the A electrons having a wave packet that we represent as indicated in the following diagram:

The B electrons have a corresponding wave packet. As a function of time, if nothing else happened, if these fellows just rode in together and out again, the atoms might trace paths that look like what I have drawn. When we get down to the distance of closest approach, we may or may not have momentum space overlap of the wave functions, depending on what the nuclear velocities are at the distance of closest approach, if we wish to use classical language.

What I am saying is, assume for the moment that I turn off interactions and just let these guys ride together. It is just Coulomb scattering of the two nuclei, and I don't let the electrons do anything except ride along for the moment. Let me just develop this qualitative picture. The momentum space wave functions for the electrons don't overlap unless the nuclear momenta get small. If there is no significant overlap, there is small probability of exchange. I can forget about that particular part of the operator; I can forget about full antisymmetrization. Then I can treat the problem on a Born basis. So for sufficiently high velocities and for large impact parameters, I can probably get away

with a Born description of these collisions. If I want to look at small impact parameters, that won't work. When I look at small impact parameters, I am going to start getting large overlaps, and exchange will be important.

The Born description here differs from the Born description we discussed in the previous talk substantially in that in addition to the projectile-nucleus interaction with the electrons, there will be electron-electron interactions, even in the case of no overlap. There will also appear energy dependent overlap integrals. No such calculation has been done for inner-shell excitation. Calculations that have been done have all been done for small velocities. Under those circumstances, the expression that we have here in terms of separated atom states might not be the best possible representation.

I guess one more comment is in order before I leave this momentum representation. It turns out that the condition for strong overlap of the wave packet, which we were just talking about, is $\frac{E}{\lambda u} < 1$. Here λ is the reduced mass in electron mass units; $\lambda = \mu/m_e$. Let me explain what I mean by that. If the impact parameter is such that the relative velocity of the nuclei at the distance of closest approach is such that these statically assumed electronic wave functions will overlap strongly, then the energy must be related to the binding energy in this fashion. Even under those circumstances, you can still have substantial overlap in position space. This gives a crude but perhaps useful classification of how to think about the problem and what kinds of approximations to make to proceed further in trying to wrestle with that messy thing. If we have $\lambda u_K < E$, then we are in the domain where we might be able to ignore this antisymmetrization operator and use the Born approximation, except for the very small impact parameter collisions that bring you close in.

You probably wonder how big λ is. For argon, λ is almost exactly 4×10^4 , so this energy would be about 8 MeV for argon. If λ times the outer shell binding energy is greater than the center of mass energy of the collision, $\lambda u_1 > E$, then nowhere in momentum space will you have lack of overlap; there is almost complete overlap in that case. This simply says your starting point in momentum space is such that the

momentum spread of the wave function about each center is much larger than the corresponding momentum associated with each electron. The momentum of an electron is $(m_e/\mu)\hbar\vec{k}$ if it is being carried with the nucleus. Here where you have full overlap, there are no circumstances where you can ignore the antisymmetrization operator. You have to do a full molecular problem. That is, you think of this as a molecule, not a set of separated atoms. In between these two conditions, heaven knows what you should do. The best approach appears to be to use more and more extended molecular considerations in the energy range in between those two, though no calculations exist, so how you are going to do that I cannot say.

Calculations do exist for energies such that $\lambda u_1 > E$, and that's what I would like to spend the remaining time on. These calculations proceed essentially from the remark I just made. You have to do the full antisymmetrization. In that case, it is much better to break up the Hamiltonian in the following way:

$$H = H_{\text{nuc}} + H_e, \quad \text{where} \quad H_{\text{nuc}} = T_A + T_B.$$

We write the wave function for the system as a product of nuclear and electronic parts:

$$\psi = \chi_N(\vec{R}) \psi_e(\vec{r}, \vec{R}).$$

Here $\vec{R}$ is the nuclear relative coordinate and $\vec{r}$ represents the electronic coordinates. If $\vec{R}$ were fixed, the electronic part of this function would satisfy the equation

$$H_e \psi_e(\vec{r}, \vec{R}) = E_e(\vec{R}) \psi_e(\vec{r}, \vec{R}).$$

I have now put all the electron-electron interactions, the electron interactions with both nuclei, and the kinetic energy of the electrons into H_e . Also included is $V_{A,B}$, which need not be included, but it's sort of free for nothing anyhow. The so-called Born-Oppenheimer approximation consists of using ψ_e as obtained from the above equation and

determining χ_N from

$$[H_{\text{nuc}} + E_e(R)] \chi_N = i\hbar \frac{\partial \chi_N}{\partial t} .$$

In using this approximation, the action of H_{nuc} on $\psi_e(\vec{r}, \vec{R})$ has been neglected. The stationary states defined by the ψ_e are considered to be perturbed by H_{nuc} , causing electronic transitions.

One obtains ψ_e for each position by solving the problem for fixed R , and thus obtains the energy as a parametric function of R . One then lets the system move from position to position in a classical fashion; that's probably not too bad an approximation. We included $V_{A,B}$ in H_e , but that provides a very inconvenient display because that goes as $1/R$, and all the displays would be dominated by that term. So, after calculating it that way, one then takes $V_{A,B}$ out, and the result is called the correlation diagram.

On Fig. 6¹⁰⁾ I have a correlation diagram for argon on argon. In doing this kind of thing, the better calculations use a Hartree-Fock approximation. That approximation gives you the binding energy of each electron directly. What is plotted here is not really the total energy, which would be $E_e(R)$, but the Hartree-Fock eigenvalues, which one commonly refers to as the binding energies of the electrons involved. The calculations in this particular case were done only to about an R of 0.5 in atomic units; the rest is essentially drawn freehand. You know where the end point has to be, because when R goes to zero you have a united atom situation. In the particular case of argon, the united atom is krypton. One can calculate outward from the $R = 0$ end point using perturbation theory to estimate the effect of separation of the two centers. I know of no decent calculations in the intermediate region, but if one does this perturbation theory calculation coming outward and uses Hartree-Fock going inward, there is only a short gap, and you can almost convince yourself that there aren't going to be any kinks in between. The Hartree-Fock procedure for molecules breaks down as R becomes small, and we are stuck with the situation of extrapolating between two approximations for these curves.

Let me mention the notation used for this diagram. The

Fig. 6

molecular notation for a level is taken from the united atom state from which it originates. You use the n, l designation from the united atom and a Greek letter σ, π, δ , etc. representing the projection of the angular momentum on the internuclear axis. At the far right are the n, l designations for the separated atom levels. The spin and the projection of the angular momentum on the internuclear axis are the only good quantum numbers, aside from parity.

The correlation diagram doesn't really show what you get from a Hartree-Fock calculation, not quite. There are crossings of levels that in a fully rigorous theory would not be permitted. For instance, the crossings of the $3s\sigma$ and $3d\sigma$ levels. As you know, if two states of the same symmetry approach each other when you are solving a problem

like this using full antisymmetrization, those two states cannot cross; there is an avoided crossing. In this representation that avoided crossing has simply been connected across the crossing. This is called a diabatic representation. The term diabatic was originated by Lichten.¹¹⁾ It is exactly at such crossings that electrons can jump from one orbital to another. That's the probability that you would calculate using H_{nuc} as a perturbation.

There is really not much more to what has been done. This, of course, is a gross simplification. A particular calculation that has been done is the coupling of the $2p\sigma$ and $2p\pi$ levels for neon-neon collisions at very small internuclear distances.¹²⁾ There have also been various calculations appropriate to any number of the crossings of the $4f\sigma$ level with other orbitals. One is interested in the probability of an electron initially riding on one of the lower orbitals jumping into another one and ending in a highly excited final state, leaving an inner-shell vacancy in the corresponding atom as the two atoms separate.

It is a rather simple matter, once you decide to use this rather crude representation of the total wave function, to isolate one particular orbital and say that that's what life is really all about. At this stage you ignore the antisymmetrization because you can't deal with that orbital in any other way. So once you are willing to accept these very serious approximations in the problem, then the calculations proceed fairly simply, and the results, I think, are moderately encouraging. I will go through some of the details in the next talk.

Question: How valid is the molecular orbital picture given in a correlation diagram? You have a collision, not a static situation. How legitimate is it to draw lines for levels?

Answer: If I let these guys move, then I am not entitled to draw lines anymore; I should use a broader and broader brush depending on what the velocity is. I will talk about that spread and its effect on transitions tomorrow.

After some considerable amount of interpretation, one can say that in at least one example, the example of the $4f\sigma$ that I pointed out that is rising very rapidly, it is changing so extremely rapidly as a

function of position that if you give the atom any velocity at all, almost any velocity, then you should be drawing that level almost as broad as the diagram. It mixes with every state in sight, and what happens is that it dumps entirely. One sees in collisions above a certain energy that the probability of creating two vacancies, even at what we ordinarily consider very low laboratory velocities, is unity. In such a case the methods that I started to outline here won't work.

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HOMONUCLEAR QUASI-MOLECULE PICTURE FOR ION-ATOM COLLISIONS

At the end of the previous talk we were in the midst of a characterization of current theoretical models for low energy ion-atom inner-shell vacancy production. You will recall we said that low energy means the following:

$$\frac{E}{\lambda u_K} = \left(\frac{v_0}{v_K} \right)^2 \ll 1$$

Here λ is the projectile mass in electron mass units, v_0 is the speed of the projectile, and v_K is the mean speed of a K electron in the orbit having binding energy u_K , if you characterize that binding energy as $\frac{1}{2} m_e v_K^2$. The criterion for high energy was that this same quantity be larger than 1. Under those circumstances we decided we might be able to forget about exchange effects and use a Born description except for small impact parameters; for small impact parameters exchange effects are always important and may, all by themselves, cause multiple vacancy production.

Using the K shell gives a limit that is not appropriate for all energy levels in the atoms. When we consider the least bound energy levels, low energy, which some people might characterize as the rigor mortis limit, can be expressed by the condition

$$\frac{E}{\lambda u_I} < 1$$

Here u_I is the outermost electron ionization energy. If this condition is met, all electrons may adjust adiabatically and molecular considerations are appropriate. The quantity λu_I is about 500 keV for argon-argon collisions; the quantity λu_K is about 8 MeV for the same collision.

We said that if we go down to the rigor mortis limit, we then need full antisymmetrization, and the best way to do that in a systematic fashion is to build a quasi-molecule out of the collision partners. We

are assured by this criterion that the electrons can adjust to the presence of the other center as the two molecules come together. We decided that under those circumstances we might be able to write the total wave function for the system as something that describes the positions of the nuclei times something that contains all the information about what the electrons are doing, which of course will depend on the positions of the nuclei:

$$\Psi = \chi_N(\vec{R}) \psi_e(\vec{r}, \vec{R}) .$$

This is to be done for fixed R in such a way that $H_{\text{electronic}}$, which is the total Hamiltonian minus the kinetic energy of the nuclei ($H_e = H - T_A - T_B$), has ψ_e as an eigenstate,

$$H_e \psi_e = E_e(R) \psi_e .$$

We then have the following equation to be obeyed by χ :

$$[T_A + T_B - E_e(R)]\chi(R) = i\hbar \frac{\partial \chi}{\partial t} .$$

For these situations, χ will follow the classical trajectory. That is, if we build a small wave packet we can follow the entire collision in terms of classical mechanics, so long as the electronic potential does not change dramatically during the course of the collision, that is, so long as the electrons stick around. Even if they get excited, the change in $E(R)$ will be small in terms of its effects on the nuclear motion. What we can glean from this is a definite position as a function of time and of impact parameter, if we choose to characterize it in that way:

$$R = R(b, t) .$$

This just locates the peak of the wave function associated with the nuclear motion. The classical equations are going to be well obeyed.

As I suggested, what one then does with the electronic problem is use Hartree-Fock analysis in order to find the eigenvalues. That

means you are using a single configuration description of the electronic states, so at this point you neglect configuration interaction. When you use the Hartree-Fock procedure, you end up with single electron orbital Hartree-Fock equations:

$$H_{HF} \phi_i = \epsilon_i(R) \phi_i$$

The ϵ_i are the things that I displayed on the correlation diagram I showed you previously. ϵ_i is a function of R ; the Hartree-Fock Hamiltonian contains the distance between the two centers so the whole problem remains a function of R . In this equation, of course, we are treating the problem statically.

Let me say a few more words about the correlation diagrams that result, as shown in Fig. 1.¹⁾ You can figure out what states in

Fig. 1

the separated atoms limit connect with what states in the united atom at zero internuclear separation. Once we have brought it down to this stage, the problem is simply a two center, one electron problem. Under those circumstances, we know that the projection of the angular momentum on the internuclear axis and the parity are good quantum numbers, and of course the spin, which we are going to ignore throughout this discussion. In fact, the way to connect these orbitals is that the number of radial nodes, which is $n-l-1$, has to be the same in the united atom and separated atoms limits.²⁾ This just results from the approximate Coulomb two center nature of the problem. Now 1s and 2p levels both have zero radial nodes, so we can make the connections shown at the bottom of the correlation diagram. Similarly, a 2s level, having 1 radial node, can connect with 2s or with 3p levels, and so on up the line. I guess we could discuss at some length the reasons for the splitting that goes on here. If you think of it very simply, it's just the electric field of one center causing a perturbation on the other. There are four electrons that you are starting out with in the 1s states of the separated atoms. According to the Pauli principle, only two of them can end up in the 1s state of the united atom. The other two have to go somewhere else. They go to the next higher state having zero radial nodes, and similarly for each of these. It is not very complicated; Barat and Lichten²⁾ have discussed the rules for making these connections in some detail. You can draw the connections for the diagram without doing any calculations. You know, for example, that the separated atom 2p has to connect to the lowest empty, non-filled, zero radial node state in the united atom. You don't know how it's going to go there; it may rise and bump around and so on, but it will get there eventually.

Question: In practice, what sort of wave function do you start with? Do you use a two-center basis on the far right?

Answer: Yes, over here...(interrupted)

Question: Then as R changes that two center basis eventually mimics a single center object?

Answer: You might use one-electron functions of the form

$$\psi(r) = X(\xi) Y(\eta) e^{im\phi},$$

where $\xi \equiv (r_A + r_B)/R$, $\eta \equiv (r_A - r_B)/R$, ϕ is the azimuthal angle measured about the internuclear axis, and r_A and r_B are the distances of the electron from each of the two nuclei. You'll need more and more basis functions as you come in in order to correctly describe the situation. That's why the Hartree-Fock calculations break down as R decreases.

What I would like to do now is concentrate on one of these orbitals and see why it is that I get K-shell vacancies in say a neon⁺-neon collision. All homonuclear collisions will have essentially this correlation diagram; the energy scale will just be squished or stretched. In the case of neon, the 2p shell is the last shell that is filled. It is completely filled in a one configuration description of neon. For a neon⁺-neon collision, there is one vacancy in the total system. Let's consider how you can create a 1s vacancy at low velocities, where you would think that energetics would forbid such a thing. A 1s electron for either collision partner rides in at fairly low levels here and then rides out. The possible levels (1s and 2p σ) do not cross or connect with any levels containing vacancies to which we could excite that electron except near the united atom limit, where the 2p σ orbital comes into contact with the 2p π orbital. The essence of the game is that if you have a vacancy in the 2p π orbital, then there is some probability that an electron will jump from the 2p σ orbital into that vacancy, and that the electron will then ride out in the higher orbital and the vacancy will ride out in the 2p σ orbital, ending in a 1s separated atom level.

Calculations have been done for proton-hydrogen atom collisions^{3,4)} in which the electronic problem is treated not neglecting configuration interactions, together with the assumption that the nuclei move classically. The calculations were performed in the following way. You have an electronic wave function $\psi_e(\vec{r}, \vec{R})$, and you let it be a function of time through its explicit dependence on R . You then describe the various channels that you want to open up in the calculation by simply saying that this wave function is a linear combination of the several electronic

states. This leads to the equation

$$H_e \psi_e = i\hbar \frac{\partial \psi_e}{\partial t} ,$$

where

$$\psi_e = C_1(R) \exp\left[-\frac{i}{\hbar} \int_{-\infty}^t \epsilon_1(R) dt\right] \phi_1 + C_2(R) \exp\left[-\frac{i}{\hbar} \int_{-\infty}^t \epsilon_2(R) dt\right] \phi_2 .$$

In the proton-hydrogen atom problem, the calculation was carried out at this level without neglect of configuration interaction. You simply say, "All right, I want to see how the electron goes to the 2p state of hydrogen, so I will ask for the probability of the electron being in the 2p state on one of the atoms in the final situation." Initially you have the electron in the 1s state on the atom.

You have to worry about how to represent $\partial\psi/\partial t$. The partial of ψ with respect to t is characterized by what enters into $\dot{R}$. Using the chain rule, we find the following:

$$\frac{\partial \psi}{\partial t} = \dot{R} \frac{\partial \psi}{\partial R} + \frac{1}{\hbar} \dot{\theta} L_z \psi ,$$

where θ is the polar angle in the plane of the motion. It was possible to carry out this calculation explicitly since we know how to compute molecular wave functions for the hydrogen molecule ion.

These calculations were lifted almost intact by Briggs and Macek⁵⁾ and applied to the production of inner-shell vacancies in neon⁺-neon collisions. In order to do that, you have to introduce another set of assumptions, another approximation; that is the neglect of electron correlation. You obtain the one-electron wave functions from the Hartree-Fock equation, $H_{HF} \phi_i = \epsilon_i(R) \phi_i$. You then translate the time dependent equation to this case by saying that $H_{HF} \phi = i\hbar \partial\phi/\partial t$. You describe ϕ in terms of a linear combination of the two orbitals whose mixing you want to consider, the same sort of linear combination as written above.

The partial derivative of ϕ with respect to time is written as

before:

$$\frac{\partial \phi}{\partial t} = \dot{R} \frac{\partial \phi}{\partial R} + \frac{1}{\hbar} \dot{\theta} L_{\perp} \phi .$$

The term on the right hand side here consists of two pieces. The first term has the same symmetry as whatever one-electron wave function you are putting in there, so this term will connect only states of the same symmetry and the same projection of angular momentum on the internuclear axis. The operator $L_{\perp}$ is the electronic angular momentum component perpendicular to the plane of the motion, so the second term connects states for which the angular momentum projection on the internuclear axis changes by 1. Thus, the second term can connect σ and π states, the particular case in which we are interested, and the first term can not. The physical origin of these terms is fairly obvious; the first is due to the radial motion, and the second is due to the rotation of the internuclear axis.

If you connect states having the same projection of angular momentum via the first term, then you get connections between a certain class of states. If you use the second term, you get connections between a different class of states. If we first consider the first kind of connection, then the equations become the following:

$$\frac{dC_1}{dt} = V \exp \left(\frac{-i}{\hbar} \int_{-\infty}^t \Delta \epsilon dt \right) C_2$$

and

$$\frac{dC_2}{dt} = V^* \exp \left(\frac{+i}{\hbar} \int_{-\infty}^t \Delta \epsilon dt \right) C_1 .$$

Here $\Delta \epsilon$ is the difference in the orbital energies and V is the matrix element

$$V = \left(\phi_1, \dot{R} \frac{\partial \phi_2}{\partial R} \right) .$$

This is just the set of equations that characterizes the Landau-Zener

approximation.⁶⁾ It's a two-state approximation for application to many electron systems; it neglects correlations and neglects configuration interaction. If you are willing to do all that neglecting, you can solve this set of equations exactly to the extent that the V 's are constants in time, that is, not functions of R . That's what Landau and Zener did, but that doesn't address itself to the problem that I want to talk about here, so I am going to set this aside for now.

For the particular case of $2p\sigma$, $2p\pi$ system that I want to talk about, the connection is via the second term, and the equations are the following:

$$\frac{dC_1}{dt} = \frac{i\dot{\theta}}{\hbar} f(R) \exp\left\{\frac{i}{\hbar} \int_{-\infty}^t \Delta \epsilon dt\right\} C_2$$

and

$$\frac{dC_2}{dt} = \frac{-i\dot{\theta}}{\hbar} f^*(R) \exp\left\{\frac{-i}{\hbar} \int_{-\infty}^t \Delta \epsilon dt\right\} C_1 .$$

Here $f(R)$ is the matrix element

$$f(R) = (\phi_1, L_z \phi_2) .$$

We know $\dot{\theta}$ because we are assuming a classical trajectory. For a classical trajectory we know that angular momentum is conserved, and that tells us that the initial velocity times the impact parameter times the mass is the same as $mR^2\dot{\theta}$, for a planar description of the trajectory, $mv_0 b = mR^2\dot{\theta}$. That tells us further that $\dot{\theta}$ is simply given by $\dot{\theta} = v_0 b/R^2$. We know R as a function of time from the classical solution, so we put that into the equations for C_1 and C_2 and turn the mathematical crank.

Briggs and Macek computed two different solutions, one using a straight line trajectory and the other using a Coulomb deflected trajectory. It turns out the shape of the trajectory makes a substantial difference in the answer, as you might suspect. The result of that computation for the Coulomb deflected trajectory is shown on the next slide (Fig. 2).⁷⁾ The theoretical results are comparing with experimental

Fig. 2

Auger data of Cacak et al.⁸⁾ The Auger data are the large open circles; the Briggs and Macek calculations are the black dots.

I guess I haven't told you how to extract the cross section from all this; let's think about that for a moment. What you need to get for a cross section is the probability of vacancy production as a function of impact parameter, $P(b)$. You then have the following relation:

$$\sigma = \int_0^{\infty} P(b) 2\pi b db \quad .$$

In terms of the above model, if we use the initial conditions $C_1 = 1$ and $C_2 = 0$, the probability that after the collision an electron has been promoted to the $2p\pi$ orbital is $|C_2(t \rightarrow \infty)|^2$.

In the standard discussion of adiabatic orbitals one says these two orbitals cross, and there is a probability p that at that crossing the electron will jump from one orbital to the other. This

means there is a probability $(1-p)$ that it will continue without jumping. The representative point for the electron then moves along the appropriate orbital to the distance of closest approach and starts back out. On the way back out you again encounter the crossing (We assume for simplicity that we are concerned with a single crossing.), and again there is a probability p that at the crossing the electron will jump. The relations can be shown more clearly on the following diagram:

The net answer is that the probability of changing orbitals is given by the expression $P(b) = 2p(1-p)$. In the Landau-Zener approximation, one obtains an expression for p of the following form:

$$p = \exp \{-y/v_x\} \quad ,$$

where v_x is the radial velocity of relative motion at the level crossing and

$$y = \frac{2\pi|V|^2}{\left. \frac{d\Delta\epsilon}{dR} \right|_{R_x}} \quad ,$$

In fact, this is too simple minded a discussion. You really have to worry about how many electrons and how many vacancies you

started with, and do a statistical counting of the various possibilities.

Let's return now to consideration of Fig. 2. You see that the Briggs and Macek results are within a factor of 2 of the data everywhere. The data don't go far enough to see whether that is going to remain so; you don't expect it to remain so forever because of the assumptions that have been made here concerning the adiabaticity and the ability of the electrons to follow those orbitals.

I've shown some other model dependent curves on Fig. 2. The Landau-Zener curve that you see there is, for this purpose, simply a two-parameter fit to that data, nothing more, because we know very well that the particular matrix element involved here cannot couple the two states that we are talking about. The Kessel model,⁸⁾ I think, is worth a bit of discussion. In the limit where this state can couple to many other states, somehow, the probability p ought to be a constant so long as you have gone in far enough that those other states are available for coupling. What the Kessel model consists of is simply saying that $P(b)$ is some constant, C , for b less than some particular value b_m and is equal to zero for larger values of b . Here b_m is the impact parameter such that the distance of closest approach just equals the level crossing radius R_x . The fact that you must use Coulomb deflected trajectories introduces an energy dependence into the cross section. The final expression for the cross section is

$$\sigma = C\pi b_m^2 = C\pi R_x^2 [1 - V(R_x)/E] ,$$

where E is the ion energy in the CM system and $V(R)$ is the potential energy. This is just simply putting Coulomb deflection into the problem and relating b_m to R_x , nothing more. This says there is a threshold, and that is what you see the curve doing in Fig. 2.

The Briggs and Macek results are the only explicit calculations that exist for inner-shell vacancy production. There are lots of parametric fits running around, but, as my nuclear theorist friends tell me, give me three parameters and I can fit the world. I guess I personally wouldn't trust any results based on nice lookingness of data using parametric fits. Let me just show you an example of that.

Fig. 3 shows results found by Der et al.⁹⁾ for the production of carbon

Fig. 3

K-shell vacancies by various projectiles. The line is a fit done on a Landau-Zener basis. That is, you write

$$P(b) = 2ap(1-p) \text{ for } b < b_m,$$

$$P(b) = 0 \text{ for } b > b_m,$$

and

$$p = \exp(-y/v_x),$$

and treat aR_x^2 and y as fitting parameters in the resultant expression for the cross section.¹⁰⁾ You can apparently successfully parameterize

carbon K-shell vacancy production via the Landau-Zener formalism. On the other hand, because the K shell is always riding in on low orbitals, K-shell vacancy production must involve rotational coupling, but the Landau-Zener formalism involves only radial momentum coupling. The fit is great, but I don't know what that means except that with two parameters you do very well for functions that are as kind as all total cross section curves are. Cross section curves in atomic physics, as we have been talking about in the last few days, go up and come down; they don't do much else. So it is pretty easy to parameterize them with two parameters, especially when one parameter is in an exponent. I think that is all the information we are getting from that fit. On the other hand, the picture that we have been talking about here, about looking at a correlation diagram and watching the representative point ride in and out, has provided a large amount of qualitative information that helps sort out some of the existing data.

Fig. 4¹⁾ shows the results of some inelastic energy loss

Fig. 4

measurements made by Kessel and Everhart.¹¹⁾ They measured the average number of electrons ejected from the Ar^+-Ar system as a function of distance of closest approach; the distance of closest approach was found by means of a classical orbit using classical analysis in accordance with what we have said previously. You see at about 0.5 atomic units a rise by more than $2\frac{1}{2}$ in the number of ejected electrons. In terms of the correlation diagram (Fig. 1), that rise has been interpreted in terms of the many crossings that the 4f orbital in the Ar^+-Ar system undergoes right at just a little under 0.5 atomic units. You can see that it crosses with orbitals such that both of our terms, separately, can couple the 4f level to various states. Furthermore, the 4f orbital is rising very, very steeply. If you make a simple estimate of the adjustment time for the electrons starting out in the 2p shell in argon, how quickly they can adjust to changes in the potential presented to them (This is simply the mean radius divided by the mean velocity sort of time.), you will find that for most of the collisions that we were just looking at, the electrons cannot adjust to such a rapid change. As I suggested previously, one ought to paint this orbital with a very broad brush. It can really couple not only at the explicit crossing points, but it just sort of smears upward, and the probability of losing everything that was in that 4f orbital becomes unity. This means that something like the Kessel model should work very well for that orbital. I don't have a slide of it, but the Auger measurements that have been made for Ar-Ar collisions show the cross section coming up at a threshold and then going completely flat, very much in accordance with what the Kessel model would tell you. So this lack of adiabaticity, I think, if this interpretation is correct, is well displayed in that particular case.

There are some more crossings for orbitals that originate in the separated atom 2p shell of argon, for example the 3d σ and 3d π orbitals that are trying to creep over here to the 3d united atom level. These other crossings occur a little further in, at 0.2 or 0.3 atomic units. Right about there you see a similar qualitatively dramatic rise in the number of ejected electrons. On Fig. 5⁷⁾ I show average inelastic energy loss calculations for Ar^+-Ar collisions compared with

Fig. 5

experiment. The energy losses are, in fact, triple-valued in the region we are talking about, but I am not going to discuss that right now. They show a rapid rise around 0.5 a.u. and another rapid rise around 0.2 a.u. One can account for those energy losses by simply assuming some vacancies are produced in the p shell of argon. If you assume 1 argon vacancy, you get energies that are consistent with the measured energy loss quantity around 0.45 a.u.; if you take 2 vacancies, then you are up here at the plateau in the center of the diagram. By making strictly *ad hoc* assumption of a few vacancies in the 2p shell of argon, you can account for the measured energy losses, and the assumed number of vacancies in the 2p shell can be accounted for by counting the possible number of electrons lost to crossings in those orbitals. That's not theory, however. It is just a series of qualitative connections from the correlation diagram to the data, with no explicit calculations of the numbers expected to be lost, except in the case of the 4f_o orbital, where one could get C equal to two since there are two electrons in there. Since no further theoretical treatment

exists, maybe this is a good time to stop.

Question: Isn't the Landau-Zener theory really for the adiabatic case where you have an avoided crossing, and it gives the probability of jumping across the crossing?

Answer: Yes, but that can be looked at two ways. If you look at it from the adiabatic viewpoint, then it is the matrix element V_{12} that prevents the crossing but does the perturbing. If you look at it from the diabatic viewpoint, the matrix element determines the probability of going through the crossing.

Felix Smith¹²⁾ has, I think, laid out this problem as well as anybody in terms of describing what adiabatic means and what diabatic means. In fact, there have been some cross section calculations based on his rigorous definition of the term diabatic.¹³⁾ The calculations are over a short range, and the data are not of great quality, but one can say there is good agreement for those calculations based on a much more rigorous definition of diabatic than I have presented here.

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DIABATIC MODELS AND SPECTRAL FEATURES

I would like to talk a little bit further about the use of correlation diagrams for explaining gross phenomena and then talk about spectra. Let's look at the first slide (Fig. 1),¹⁾ which is our old

Fig. 1

friend, the Ar-Ar correlation diagram. Recall that we said we have to connect states with the same number of radial nodes in the separated atoms and united atom limits. We talked about various phenomena that occur because of the forcing, due to the Pauli exclusion principle, of some electrons into rapidly rising orbitals, some of which were

adiabatic, such as the cases we talked about in neon, and some of which were highly nonadiabatic, so we could use something like the Kessel model with constant probability of dumping a level that's rising very rapidly toward the continuum.

In the non-homonuclear case, the establishment of correlations is considerably more difficult. No really definitive calculations have been done. There have been many calculations of non-homonuclear diatomic molecules on the Hartree-Fock basis, but in those cases you don't get the 100% unique correlations and identifications that you get for the homonuclear cases. Nonetheless, some people opine that in a collision you establish single particle orbitals, but there is no complete full adjustment. The correlation energies are always small, and there is, in fact, a lack of adjustment of correlation energies. In fact, the two-center Coulomb problem is the dominant situation, in which case you can still establish exactly the sort of correlations that we talked about before. The rules, though it's more complicated to come by them in this case, remain the same. You still connect levels with a given number of radial nodes in the separated atoms limit with levels having the same number of radial nodes in the united atom limit. This is not an obvious statement. The only real theoretical justification for it that exists is based on one-electron hydrogen excited state calculations by some Russians.²⁾ In the one-electron problem, these correlations are exactly established; that says if you are willing to live with a one-electron picture, then the correlations that we talked about in connection with the homonuclear case still occur. But there are marked differences now, because in the separated atoms limit you are starting out with two atoms having different binding energies for their different shells and subshells. You might have, for example, something like the following diagram:

The details depend on what kinds of atoms you have.

If we adhere to the idea that we have to connect states with equal numbers of radial nodes, then the two $1s_A$ electrons simply map down to the $1s$ level of the united atom. The $1s_B$ electrons simply map to the next zero radial node case and similarly on up the diagram. The $m = 0$ part of the $2p_A$ level has to map on up to the $3d$ united atom level, which is the lowest unfilled zero radial node level. Remember that we are labeling the molecular orbitals with the n, l notation for the united atom limit plus a Greek letter indicating the projection of the orbital angular momentum on the internuclear axis. Let me not make any more connections.

If we were to follow the representative points for various electrons in to the distance of closest approach for this collision, we would expect that atom A's $1s$ electrons would get a free ride in and a free ride back out. The $1s\sigma$ orbital doesn't connect to anything, unless the slope is very large and you get nonadiabatic effects. If atom A's $2p$ shell is full, so that the $2p\pi$ orbital has a full complement of 4 electrons, then the $1s$ shell of atom B also gets a free ride in

and a free ride back out. Under those circumstances, the first action you would expect to see is the possibility of creating holes in the 2p shell of atom A.

This model has been used to qualitatively explain rapid rises in total cross section observed as a function of target Z for both gases and solids. The gas work was done by Saris and Onderdelinden,³⁾ and the solid work was first done by Specht,⁴⁾ but repeated with a cleaner situation. Specht did it using fission fragments, and you don't really know what the A is; you have a range of Z's to work with, so it is a muddy situation. The clean experiment was done by Kavanagh *et al.*⁵⁾ for copper projectiles bombarding various target materials. The target materials were chosen such that the projectile 2p level moved across first the 1s, then the 2p, and then the 3d level of the target. If the 2pA level were below the 1sB, then the 2pA correlation would have simply gone to the united atom 2p. The 2pA electrons would have gotten a free ride in and back out in the same way that the 1sA electrons did. I have actually drawn a situation where it is very likely you would produce a 2p vacancy in atom A.

Fig. 2⁵⁾ shows the data obtained for the total cross section

Fig. 2

for production of copper L x rays as a function of target Z. It is a messy slide. The solid line is drawn through the data points just to guide the eye. In order to keep the energy levels straight and be able to think about which levels are crossing with which, the binding energies of the K, L, M, and N shells for the target are indicated on the slide. The horizontal dashed lines represent the binding energies of the $2p_{1/2}$ and $2p_{3/2}$ levels of copper. The other dashed curves represent target atom binding energies as a function of target Z. You can see there is a variation of three orders of magnitude in the cross section as you go from Z equals 12 up to Z equals 30. Z equals 29 is the copper-copper situation. You will recall that in the homonuclear case two electrons from the separated atoms 2p level must ride up in the 4f orbital that goes very near the continuum, so there is a very high probability of producing copper L x rays under those circumstances. There is a corresponding rise associated with the copper L shell going below the 1s level of the target atom. You see also a marked increase in the neighborhood of Z = 62 or 63.

When the 2p matches the 2p, you get a high probability of vacancy production. When the 2p matches the 3d (You have to go all the way up to the 3d here before you get another zero radial node situation after the 2p. It is not just simply level matching; it is level matching with the correct number of radial nodes.), again you have a high probability of ejection of those 2p electrons. There is a dramatic change; it's several orders of magnitude from peak to valley, indicating that these ideas have at least some qualitative validity. Saris³⁾ has similar data for various projectiles on argon gas targets, and again the matchings occur where you expect them to occur on the basis of the correlation diagrams.

One might be able to understand the declines with increasing Z in several ways, none of which are completely satisfying. One factor affecting the decrease is simply the decrease in the sizes of the shells. As Z increases, each of the shells becomes more and more compact, so you expect some decrease of the maxima. That decrease would probably not be as abrupt as the decrease shown by the data. Barat and Lichten⁶⁾ have attempted an analytical construction of the L-L matching

peak around $Z = 29$ in which they use the Kessel model and a Landau-Zener type probability function to describe level mixing. (These were discussed in the previous talk.) They can parameterize the fall-off in terms of distances associated with the appropriate shells, but it is just a parameterization; there is no serious theoretical justification for it. The non-sudden part of the rise beginning around $Z = 50$ might be viewed in several ways. It might come from nonadiabatic contributions. More likely, it appears because these assignments are not 100%. When we do the correct Hartree-Fock molecular calculation, we find there is some probability that a given level connects several ways, just the normal expansion of states sort of idea. For that reason, these levels might not be expected to be sharp. All of these are qualitative notions; I don't wish to say that I am speaking about theory in any sense of the word. The data do show more than qualitative agreement with the concept of level matching discussed here, I think. Three orders of magnitude is a very significant thing.

Question: Could I ask about the energy of the incoming particle? If you increase the energy of the copper ion, what effect does that have on this curve?

Answer: The experiment has not been done sufficiently extensively to really investigate the energy dependence. What apparently does happen, as the next slide will show you, is a considerable filling in of those valleys as the energy increases.

Fig. 3⁵) shows data that involves the K shell-L shell matching. This is not quite the same experiment; this is for a copper target being bombarded by various projectiles, but the effect is still there. The cross section is for Cu L x-ray production. The various curves are for different ion energies per atomic mass unit (i.e. fixed ion velocities): triangles, 1.0 keV/amu; squares, 2.0 keV/amu; open circles, 3.0 keV/amu; crosses, 5.0 keV/amu; crossed circles, 10 keV/amu; and half-filled circles, 50 keV/amu. From the squares to the crossed circles the energy has changed by a factor of about 5. The valleys do get filled in very substantially.

Fig. 3

In Fig. 2 note that the first peak is reached not quite where the L-shell energies match, but slightly to higher Z than that. This might be because the copper projectile is traveling in the solid and therefore finds itself stripped, so the binding energies for the electrons listed here are slightly higher, and therefore the correct Z to match with is slightly larger. This notion has not been made quantitative. In Fig. 3, however, you see that the peak is quite accurately at the position of the level matching.

If I had shown you the spectra associated with these curves, you would have seen that near the peaks both target and projectile x rays are produced. Cu L x rays overwhelmingly dominate on the high- Z sides of the peaks, and collision-partner x rays dominate the spectra on the low- Z sides of the peaks. If I had shown you a corresponding plot for the x rays from the target atoms, it would have had the dips inversely to these curves.

Fig. 4⁷⁾ shows results for the argon-carbon system, and

Fig. 4

Fig. 5 shows corresponding correlation diagrams. Consider first the upper correlation diagram, which would also be appropriate for Ar^+-C and C^+-Ar . We have the 2s and 2p for argon with about a 40 volt separation, and between them is the 1s for carbon. Now 40 volts is a pretty tight tolerance for these ideas to be holding. If we adhere to this picture, we expect the argon 1s and argon 2s to get free rides. The carbon 1s correlates via the 2p σ orbital, and the argon 2p correlates via the steeply rising 3d σ orbital to the united atom 3d level. This should imply a high probability of vacancy production for the argon 2p, but not for the carbon 1s. We have squeezed the tolerance quite a bit; we now ask ourselves if there are any carbon x rays in collisions

FIG. 3. Diabatic molecular-orbitals diagrams for the Ar-C collision. For simplicity only inner shells are included.

Fig. 5

of carbon with argon. The answer from Fig. 4 is perhaps some, but certainly not many. The carbon x ray should come at the position indicated by the results shown for H^+-CH_4 collisions. There may be some carbon x rays there, but what you do see is that there are many, many more argon x rays, so argon is predominantly getting the vacancy, which is in agreement with what the correlation diagram shows. That's a very tight tolerance; these are 80 keV collisions. You would expect at these energies perhaps a beginning breakdown of adiabaticity, certainly for the outer shells. I think this is rather interesting confirmation of this model when a very tight squeeze has been put on it.

On the other hand, suppose you run argon ions into graphite. You know that if you run ions into a solid, there may be a stripping

of the ions so you have a higher ionization state in the collision. If the argon is sufficiently ionized as it enters the collision ($n \geq 3$), then the correlation diagram will look like the lower part of Fig. 5. In such a case, we expect vacancy production in the carbon 1s to be favored.

The next slide (Fig. 6)⁸⁾ shows data that corresponds to that

Fig. 6

situation. The position of the carbon line is indicated on the top curve by the results shown for $H^+ \rightarrow C$ collisions. We saw no evidence for the carbon line in the gas; here we see a rather smeared spectrum. We see some x rays, but we don't know what element is putting them out. Note the scale; it's backwards here, the way it comes out of the spectrometer. What we do know is that carbon all by itself is incapable of emitting x rays at the smaller photon energies shown here, so one has to suspect that argon is doing the emitting of the bulk of

those x rays. It is reasonable to assume that the feature in the neighborhood of 280 eV, which is the normal position for the carbon line, is in fact due to carbon x rays. We see that, as a function of projectile energy, whatever is there is growing quite rapidly. There are a couple of things we can say about that. One is that argon is much heavier than carbon, and we expect substantial recoil. Therefore, some of those x rays are probably simply from carbon-carbon collisions where a carbon atom has recoiled and collided with another carbon atom. That certainly happens. However, from the calculations of Sigmund and Taulbjerg,⁹⁾ that effect is not expected to be able to account for all of the intensity at these energies. At higher energies, it should certainly account for a majority of the carbon x rays seen, but at the lowest energy here, there should be essentially no contribution from that source. Of course, you have to have faith here in the calculation associated with recoil, and it is not completely clear that that analysis has been correctly done. I would say that one should hold this slightly in abeyance and see, when the experiment has been extremely carefully analyzed, whether it still holds up.

Fig. 7¹⁰⁾ shows x rays produced when 90 keV sulfur projectiles are incident on CH₄ gas. There is very good production of sulfur x rays. If we should look at 277 eV, which isn't on the slide, we would find there are no carbon x rays. The situation remains the same as for argon; there is a huge sulfur line, but essentially no evidence for carbon emission.

Fig. 8¹⁰⁾ is for protons on argon. The largest peak is at the position of the normal argon L-shell line. The little bump at the left of that peak is interpreted as being a radiative Auger process in the argon atom, whereby an x ray is emitted simultaneously with the motion of an electron, either to a bound state or into the continuum. The resolution in this case was only 10 eV, so there was no chance to check to see what fraction went into the continuum and what fraction went into bound states. The bump at higher energy is from a 3d+2p transition. The energy of the x ray in that case is higher than the binding energy of the 2p electron, which you might think is rather odd, but it is just associated with screening effects. It's not very

Fig. 7

exciting business. The magnitude of the effect has surprised some people, however.

If we now look again at Fig. 4, we see the collision of argon with argon done with exactly the same resolution, the same instrumental setup. We see that a radiative Auger process does not occur in this case. I don't have an explanation for that, but I wanted to point it out; it isn't there. The width of the main line is hardly different, so it isn't just a masking of the situation; it is a fact that radiative Auger process is not occurring, or is not occurring with as much probability in this case as it was in the previous case.

I would now like to change subjects altogether and talk a bit about spectrometric investigations of the so-called molecular line that has been observed using lithium-drifted silicon detectors.¹¹⁾

Fig. 8

Argon is the projectile in the majority of the experiments. Various targets have been used. Silicon was the first case reported, but various other targets have also been used. In the neighborhood of 1 keV, where no radiation from either argon or silicon (or carbon, depending on the target) is expected, there occurs a very well defined bump. This bump has been attributed to a $3d+2p$ transition in the united atom argon-argon system, krypton, occurring while the target and projectile are still in the process of collision and therefore very close to each other. The shape of the bump is expected to yield some of the details concerning that process. In the case of the argon-argon system, the united atom being krypton, the radiative lifetime is shorter than the collision time, so the probability of emission of such an x ray appears eminently likely. I think several laboratories have begun extensive investigation of these emissions; they represent a brand new phenomenon in investigation of ion-atom collisions.

Gons and Bickel at the University of Arizona have undertaken

high resolution spectrometric measurements in the region of 1 keV. The next slide (Fig. 9)¹²⁾ shows results they have obtained. They are using

Fig. 9

a lead stearate spectrometer, and the biggest bane in the game is that you get all sorts of orders coming right in the region in which you are interested. The arrow indicates the 1 keV region. They also have made observations using a lithium-drifted silicon detector, and in that case they find the bump right where everybody else finds it, right at 1 keV.

Fig. 9 represents about 20 hours data gathering. The bump in the 1 keV region does not appear to be present. However, there is a lot of noise. Furthermore, there are windows on spectrometers, and windows eat up low energy photons like mad; photons between the K-absorption edge of oxygen and about 900 eV are severely attenuated. It isn't a cut off; it's sort of gentle; it's a one over E cubed slope in the region of 900 eV. One can see lines very accurately, but not a broad feature at the expected position.

Question: Are you looking for something sharp?

Answer: No, we are looking for a big broad bump.

Question: Well, isn't it probably in there?

Answer: Yes, that's just the comment I made. The noise and that silicon line could just mask the bump.

The same observations were made with a carbon target. Again, there is a very huge tail rising here that could mask the effect. What these observations tell me is that one didn't learn anything by spending all that time with the spectrometer.

Question: Did you say that the same experiment was done with a silicon detector, and you do see a bump?

Answer: That's right, exactly the same bump as has been reported, with very much the reported intensities.

Question: Isn't the window in the silicon detector much worse than the window in the spectrometer?

Answer: There are various windows. I show you here only one run, but there were various windows used in the spectrometer in order to try to see if the problem were window attenuation. In fact, most extrapolations of this bump that account for window efficiency have it growing very rapidly at lower photon energies. So if there were photons there, one might very well have expected to see them in the spectrometer run, but there are no very large phenomena of that type present. On the other hand, estimates of the phenomenon say that there should be intensity of say one part in 10^7 of the corresponding carbon K α line, and 1 part in 10^7 is pretty hard to come by considering the amount of noise that is around in these runs.

Windows are very interesting creatures. I didn't have time to make a slide of this particular picture, but I'll show it to you anyway (Fig. 10). This is a lithium-drifted silicon detector run in which a very thick window was placed in front of the detector and protons were shot into aluminum. If one puts just a normal window there, then one sees the aluminum line and no bump on the high energy side; it is very

Fig. 10

clean. If one puts a thick window there, one sees an apparent bump. There are some discrete lines in there, but those are identifiable as impurity lines that show up even when the window is absent. So this is a manufactured bump, and the only thing that has been done to manufacture it is to place the window such that certain radiation is cut out.

I think it is very easy to understand the manufacturing of such a bump. Let's assume that the line has a Lorentzian shape, and that you have a detector function, including the absorber put in front of it, that looks like the dashed line in the following sketch. Then the net spectrum ought to look like the lower line in the sketch.

It is very easy to produce a bump of this type from just the tail of the Lorentzian. I have made some estimates of how much of a tail the carbon K α line could produce in the region of 1 keV. If you look at the 300 kV proton on carbon data, such that you are not getting any bremsstrahlung and there is not very much recoil, you can account for almost all of the bump produced by protons on carbon at 300 kV via just the tail of the carbon line, even though that carbon line is at 277 eV, and you are only looking at one part in 10^7 of the total output of the carbon atom under those circumstances. That raises the question as to whether in fact this entire phenomenon might not be just arising from the tail of some line. Of course that can be answered relatively easily by knowing what your instrumental efficiency curve is and then calculating the expected intensities on such a basis.

You can take the window off, but you still have a dead layer in the detector, and that dead layer acts as an effective window. To get rid of that, you would have to take the dead layer off, and then the detector wouldn't work. The tail of the Lorentzian goes down as $1/E^2$, and so one has to very carefully see what the absorption of that dead layer is and then see what the expected results in the spectrum might be.

Question: I think that all of us who are working in this area are generally agreed that some form of spectral continuum is what we are looking for, and that it is formed into the shape of a bump by attenuation. It is an interesting notion that it is the Lorentzian tail of the carbon K line. I am intrigued by the Lorentzian peak shape as a theoretical way of describing the line. I had always been under the impression that it was a way of describing the shape of a line only up to some cutoff. Does the theoretical development of the Lorentzian line shape go smoothly without any cutoff?

Answer: This is just the Breit-Wigner formula, and there are no cutoffs associated with that.

Question: Isn't there an experimental way to study this by just sending protons into carbon and looking at what you can see?

Answer: Yes; here is exactly the graph you are asking for

(Fig. 11). This is using a solid state detector for protons on carbon;

Fig. 11

the energies are indicated on the graph. The solid line is the calculated expected Lorentzian tail, normalized to the data. The dashed line is what is actually observed. The proton energy was 300 keV in this case. The peaks are from impurity x rays that are observed; the carbon sample that was used had these impurities in it.

Question: So long as you are working at the center of a nice, well-behaved line, you've got a well-defined width; it stays constant across the energy. If you work in the tail, or if you are close to a threshold, the width of your Lorentzian shape changes as your energy changes, because the probability of emission changes.

Answer: Let me see if I am reading you right. The standard way of writing this out is $f(E) = K / [(E - E_0)^2 + (\Gamma/2)^2]$, where K is linear in Γ . Γ is a function of E .

Question: Don't you have to worry about that when you are calculating those shapes?

Answer: When you are 10^6 line widths away from the line center, you are in bad shape. It is not clear you should be using this whole expression at all; the theory has never been pushed hard enough to test

that. The only experimental test that I know of is the index of refraction where the transmission is occurring some 10^4 to 10^5 line widths from the line position. There it seems to work moderately well as is, without any changes. But I am not willing to make any strong statements.

Question: If this is the source of that bump, it essentially doesn't matter whether the line shape is Lorentzen or some equivalent shape. You have a line with a very long tail, and the detector has almost a sharp cutoff. One thing should surely be true; the width of the observed bump ought to change with the intensity. If you wait a little longer and get more counts, then the half height should go progressively further and further out.

Answer: Right, until the cross section peaks. All these checks ought to be done. I am just raising some questions and ideas rather than proposing any final solutions to the problem.

Footnote added in proof: The contribution to the molecular-orbital band due to line width discussed here has been theoretically investigated by J. S. Briggs in J. Phys. B 7, 47 (1974) for N^+-N collisions. He finds that the line width contribution is a significant fraction, but less than 1/2 of the signal expected in the wavelength region associated with the quasi-molecular band region. Thus, this phenomenon may play a role for homonuclear collisions, but the proton projectile signal is probably due to either bremsstrahlung or recoil-induced homonuclear quasi-molecular bands.

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Professor Walter Greiner

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THE TWO-CENTER SHELL MODEL

Thank you very much Professor Legg and thank you Professor Hathaway for this kind invitation. It is a great pleasure for me that I can talk to you this week and give a kind of introduction to various things we have been doing in recent years. The aspects I am talking about are both of nuclear physics character and of electrodynamic character, which fit together in a particular way in heavy-ion physics, and today I am talking about the nuclear physics aspect.

The problem of the two-center shell model emerged out of the following considerations. When one wants to study fission, one wants to have the process that the nucleus deforms and finally goes over into two fragments. On the other hand, when one studies heavy ions, one has just the opposite situation. You come from two separate fragments; you eventually amalgamate them, and end up in one nucleus or the system dissipates again into two fragments. The model available for describing such a process until a few years ago was essentially only the Nilsson model,¹⁾ and I assume that most of you have heard about the Nilsson model.

You have the nucleon essentially moving in a common shell model, and you distort it in such a way that the equipotentials of this shell model are conformal ellipsoids. Such a deformed three-dimensional oscillator we call 'Nilsson model' if it is suitably modified by LS forces and other terms, which I shall describe later. This model was very, very successful in low-energy nuclear physics since it could explain spin sequences and deformations and many things of low-energy nuclear structure. For fission and heavy-ion physics it is not suitable, as you see immediately, because when you increase the deformation the Nilsson model becomes unphysical and produces a needle-like nucleus: It doesn't squeeze in and it doesn't make fission. It is asymptotically wrong, and therefore in heavy-ion physics and in fission physics you need to have a shell model with the proper asymptotic behavior. The aim of my talk today is to describe to you such a shell model in a clear-cut way and to discuss a few of its applications. Today mostly on the fission aspect, tomorrow

mostly on the heavy-ion scattering and molecular aspect.

Now, what is wrong with this model if we go over to such a long Nilsson ellipsoid? If you assume that you calculate the single particle energies in such a model as a function of the deformation (I call the deformation beta, whatever it means) then you easily can think that the potential energy of a nucleus of mass A is a function of this collective variable beta, and would be something like the sum of the single particle energies.

$$V(\beta) \sim \sum \epsilon_i(\beta)$$

You perhaps get a function which looks like this.

It has a certain minimum and a certain maximum, if you are lucky. But then it goes up to infinity. Why does it go up to infinity? Well, because when you put a particle in such a long needle ellipsoid, you immediately see that the zero point energies rise, tending to go to infinity. You always have this zero point energy in the system; this is what makes the energy go to infinity for large deformations.

Basically the model is asymptotically wrong, but to deduce something useful from it was the main goal of Strutinsky.²⁾ Strutinsky said, now let's try to deduce from the Nilsson model something which is right, and so he defines the so-called shell corrections, which are nothing else than taking the sum of the single particle energies as a function of β , and subtracting from it a kind of average of single particle energies like this:

$$\text{shell corr} = \sum^f E_1(\beta) - \sum^f \overline{E_1(\beta)} \quad .$$

The sum is always taken up to the Fermi surface. In this way you see that you somehow subtract the infinities out of the model. And then, you deduce something which asymptotically goes to zero, and therefore this is a useable quantity.

However, the question is then, where do you cut it off in order to calculate a fission barrier, or the correct potential energy curve? Now in principle, the idea was the following - that you should add the shell correction on a curve which I call $V_{LD}(\beta)$. That is, you construct as a sequence of your deformation,

$$V(\beta) = V_{LD}(\beta) + \text{shell corr}(\beta) \quad .$$

The potential $V_{LD}(\beta)$ is from the liquid drop model, which by definition is an empirical model and which we know has built in the proper binding energies as well for a separated fragment as for the original fragments. When you have such a liquid drop model and you calculate the surface and the Coulomb energy as a function of the deformation, you could immediately make the suggestion that perhaps the proper potential which has to be used is that of the liquid drop model plus shell correction as a function of β .

Now, if you do this you run again into trouble, within the Nilsson model, because you can only do it consistently, of course, if you use the same type of underlying potentials for the liquid drop model and for the shell corrections. In other words, when you want to be consistent you should use shell corrections in the Nilsson model, and at the same time calculate the liquid drop energy for such equipotential surfaces which you use in your shell correction calculations, namely such conformal ellipses. If you do this however, you see that you have not solved this difficulty, because when β goes to infinity, the surface of the stretched ellipsoid also goes to infinity, and the surface energy rises to infinity. This does not mean that Strutinsky is basically wrong or something like that; it only means that the Nilsson model is wrong. The Strutinsky procedure is very useable to calculate potential energy surfaces in a semi-quantitative mixed microscopic and macroscopic way, which contain proper

asymptotic behavior and binding energies.

So, we have to look for a new model, and the new model is constructed in the following way. Many of these works started with Holzer, Mosel, Maruhn and others.³⁾ We start by going from one sphere into a double oscillator kind of model.

I just connect two oscillators, and there's a join in the middle. I stop the potential so to speak, and there is a kind of a sharp barrier in the middle. Later on we will round the spike off and see that this doesn't really change things very much. When you go to larger separations, you finally end up with two complete separate fragments. If I draw here again the oscillators, the picture looks like this:

It is again a double oscillator, but with a high barrier.

In the earlier drawing it is still joined. Here it is quite

separated through a big barrier, and so we have the smooth transition from one into two fragments. When you set up such a Hamiltonian, first for a double oscillator, this is rather simple. If you use cylindrical coordinates along this Z axis, you can separate the angular degree of freedom, and also the azimuthal degree of freedom, ρ , and you can solve this all analytically. I call the distance between the minima of the two nuclides, $2Z_0$, so Z_0 is just the distance from the minimum to the barrier.

If you solve this, you get a differential equation which is nearly like the usual differential equation for a single oscillator, which we all know. The single oscillator has always the principal quantum number n , and then other quantum numbers, and the solution is a hypergeometric function

$${}_1F_1(-n(Z_0); (Z-Z_1)^2) \quad ,$$

where Z_1 are the coordinates of the internuclear centers. However, now we have to match the solutions from the left- and the right-hand side, and whereas normally this has been an integer number, we find out now that n is a function of Z_0 , the distance between the two ions. And $n(Z_0)$ is the solution of a transcendental equation, just arising from the matching of two hypergeometric functions in the left- and the right-hand well.

So, what I want to say is, without going into mathematical details, one can solve these things all analytically. (Analytically except for finding the zeros of a transcendental equation, which is of course a very simple thing.) If we do this, we get the suitable hypergeometric functions, and with them one easily can calculate all sorts of matrix elements. So this is a very handy way to describe the physics of fission.

Let me show you now a few of the differences between the Nilsson Model and the Two-Center-Model just discussed. In Fig. 1 you see the asymptotics of the Nilsson model, and you see here a cut through the potential along the symmetry axis. Of course the oscillator along the Z -axis opens widely, whereas the oscillator in the perpendicular axis, which is aligned with ρ , becomes steeper and steeper, and the zero point energy rises up.

Now when one does this double oscillator model which I introduced

Fig. 1

here, one can generalize it to three cases which can also be solved analytically. (See Fig. 2) Case a) is a breakup of a sphere into two equal

Fig. 2

spheres. Case b) is a breakup of a deformed nucleus into two equally deformed nuclei. You can also solve analytically the wave functions and the energies for a breakup of one deformed nucleus into two unequally

deformed nuclei (case c). As we have different types of volumes here, we have differentially large nuclei. But it is essential that they have the same rho extension. The oscillator, in other words, in the three dimensions must always be the same in the rho direction, otherwise you cannot solve it analytically.

In Fig. 3 I show the separation into two fragments and the

Fig. 3

oscillator potential. The upper potential is along the symmetry axis, Z , and you see how the barrier develops, and if you take this as a Fermi surface, you see the separated fragments developing. In the lower one, which is taken in the rho direction, the oscillator changes until it reaches a proper value, which is typical for the ion fragment $A/2$, if A was the original nucleon number, and from there on it stays that way. So, the width never squeezes together to zero, and the zero point energy never can rise to infinity.

When one solves this, one gets the eigenvalues, and also the wave functions. In Fig. 4 I show the eigenvalues as a function of Z_0 , in Fermis. Remember that Z_0 is half the distance between the two separate nuclei. You see that at $Z_0 = 0$, you get this original oscillator spacing, as it must be, because here you have the original volume and a single

Fig. 4

oscillator. Then you see that these states move apart, and you end up here asymptotically, with new oscillator shells showing bigger spacings. These new oscillator shells correspond to the fragment $A/2$. Now since we have two fragments $A/2$ in a symmetric process, you end up here with twice as many states everywhere. Where you had one K shell at $Z_0 \approx 0$, you have two degenerate K shells at large Z_0 .

There is a double degeneracy because of this particular symmetry, and we can look at the wave functions in Fig. 5. You see here for each

Fig. 5

nonspherical oscillator separation there is large level separation. On the right we focus on how the wave functions develop on the lower oscillator states, and you see at the top is a Gaussian function, and then there comes the next mode of the oscillator function.

Now, when you go to larger separations, you see how the oscillator changes. The Gaussian function squeezes in, and the deformation of the next function is not very easily seen, but when you go to large deformations, like at the bottom, you see that the squeezing in has become so large that you get now two wave functions, one completely symmetric here around this original center, and the second wave function is completely equivalent to it, just with a different parity. This shows how parity with respect to the original center comes into the game in the breakup of these two oscillator states.

Now there is one problem. Normally when we construct a model for fission, we require that there should be no compressibility of nuclear matter during fission, because the compressibility of nuclear matter is very high, about 100 MeV (it may be 150 MeV) or something like that. A small compressibility of nuclear matter would immediately mean that we have tremendous energy fluctuations in these deformation channel theories. Since fission is assumed to be an adiabatic process, there should be no compressibility during the deformation into two fragments.

Now how can you assure that in a theoretical model? You can do it in the following way: Usually the standard assumption is made, that nuclear forces are short range. If nuclear forces are short range, you can argue that the matter will essentially follow the equipotential surfaces, (I draw here a short-range matter distribution around an equipotential).

When you believe that, then you can assure that nuclear matter is not compressed by requesting that every equipotential, V_1 , V_2 , or V_3 go over into V_1 , V_2 , and V_3 , in the deformed system, and similarly in the two-center model, of course. If every equipotential conserves volume, then we have, under these assumptions, just stated that the system is incompressible. So you see that we now have to focus on a kind of volume conservation in this deformation process, and that is a degree of freedom one didn't think about, namely, how to deform the various equipotentials on that way out.

Now, this can be done easily in the Nilsson model, as the nuclear physicists of you know, by a common factor, which you take out. With one common factor you can assume that every equipotential is conserved in volume during the deformation process. This is not any longer the case in the double oscillator model. It is just a particular property due to the typical ellipsoidal deformations of the Nilsson model, and nothing else. It is a geometric effect. Now what can we do here in the two-center model? Well, we tried three types of volume conservation.

The first one was the following: Let us conserve the volume of the equipotential which is equivalent to the Fermi surface in the original nucleus. Now this we call surface volume conservation. The second method works with a statistical argument (I don't go into these details because they hold us up tremendously and don't bring much insight.) You can calculate how many particles you roughly can bring into such an oscillator potential, between potential V and $V + \Delta V$, and you weigh now every factor which you get out for the various equipotentials by the number of particles you can put in, and then you average this. We call this the average volume conservation.

Now the third method is an exact method, namely, you conserve the volume of every equipotential you can construct. In this way however, you change your potential; it is no longer a double oscillator potential, but something different. But this is the exact method of volume conservation, and I will show you what the deviations in the potentials are, and what the consequences of these three methods are. The first and second method can be handled extremely easily; the third method is extremely complicated with a lot of pure numerics. Let us take a look now at the

consequences.

In Fig. 6 I show you how the potential looks for the double

Fig. 6

oscillator and the three types of volume conservation. The solid line is the so-called surface volume conservation and is a crude double oscillator shape. The dotted curve is for average volume conservation. It is again a double oscillator potential, but with different curvatures everywhere. Last comes the exact volume conservation method; the dashed curve looks nearly like a double oscillator potential. It is not however. Of course it has to go through the middle point, because this by definition was volume conserving. But you see these deviations, however, are weak. So this encouraged us to accept the following procedure that you use a volume conservation of the method 1 or 2, and take into account the small deviations from the exact volume conservation by diagonalization procedures.

Now, we also studied the consequences, and this is even more important, of the three methods of volume conservation, for these so-called shell corrections of Strutinsky, which are shown in Fig. 7. These are for ^{236}U , and here you see how it separates into two fragments. This is just a pictorial way for looking on the abscissa, and you see that the three

Fig. 7

methods of volume conservation for the shell corrections agree, and don't differ except at a point which is something like the secondary or shape isomeric minimum, and there they differ only by about .5 MeV. So then we have the main conclusion that for shell corrections, the three types of volume conservation are equivalent. The shell corrections are nearly independent of the particular method of volume conservation which we apply, and therefore we choose of course the simplest one, and this is always the surface volume conservation.

We now run into the following problem: What we have considered up to now was all double oscillator, and double oscillator is a nice thing to have to play with, but it is not a shell model. It becomes a shell model in that moment when we add $\vec{l} \cdot \vec{s}$ forces, and the so-called l^2 -terms (l is the angular momentum operator), $V = V + \kappa \vec{l} \cdot \vec{s} + \mu l^2$. You know that in oscillator-type shell models, one always had to use these two types of forces in order to make it in agreement with the single-particle levels known very well over the whole periodic table.

Now, we have run here into two problems. The first problem is the following: The angular momentum operator is always defined as $\vec{r} \times \vec{p}$

where $\vec{r}$ is a vector out of the center and $\vec{p}$ is the corresponding momentum. But what do we do when we have originally one center and then go over into two centers? I draw it here once more.

$$V(r) \sim r^2 \quad \vec{\nabla} V \sim \vec{r}$$

$$\vec{L} = \vec{r} \times \vec{p} = (\vec{\nabla} V) \times \vec{p}$$

See, we have this sequence which we have to follow up. For one center $\vec{r}$ is clear; for two centers we would like to have that it is this vector $\vec{\nabla} V$ respectively around the centers. Now for an oscillator potential, which is of course proportional to r^2 , we notice immediately that the gradient of the potential is proportional to r , and therefore it would be suggestive of two vectors to replace this angular momentum operator introduced in that way by these gradient cross $\vec{p}$ terms.

Now actually, from a basic point of view, if you like, these are the real fundamental terms, and you know them from atomic physics quite well, from the Thomas vector and Thomas potentials. They are included there precisely in the same way, and only are reformulated as $\vec{L}$ -s force because in the r^{-1} potentials this reduces precisely then to the angular momentum operator. Now you see that if we do this, then we will have automatically assured what we want, namely that our gradient of the potential measures always this angular momentum around the individual centers, except of course at the center point, which is undefined. Now if you later on round these things off anyway, then even that center point is defined and everything is all right. So this solves the first problem.

The second problem is of this character: We know that these kappa values in the $\vec{L}$ -s terms are A-dependent. In the rare earth region, the spin-orbit force is a different strength than say in the actinide region. Now when we go from a system A into two systems A/2, we have to assure that we have originally the kappa values corresponding to A, and then the kappa kappa value corresponding to A/2. Nobody really knows what the proper interpolation between these two is and how that changes, and if you are in such a situation, you take the simplest way possible, and let the rest be to further workers in the field. So we interpolated linearly during

fission from kappa for A into kappa for A/2. And the same is of course also true for mu. So, during fission, we assured that asymptotically the real shell structure of the final fragments comes out in this two-center shell model, which sometimes shows up, especially in higher shells, in shell inversions and things like that.

I should say one more word how these things are treated. These l -s forces and l^2 terms are used within these basis wave functions; their matrix elements are calculated and diagonalized. The matrix elements can exactly analytically be calculated, so the whole numerics which we are entering is just a simple matrix diagonalization, and goes very fast and is very straightforward. In Fig. 8 you see the original shell model of the

Fig. 8

$p_{1/2}$, $p_{3/2}$ states, etc.; and you can follow it way out in Z_0 . You see a

larger spacing emerging at the proper l - s splitting for the shell models of the final fragments. We have the $1s_{1/2}$ shell for $Z_0 = 0$ going to a $2s_{1/2}$ shell in the double oscillator, because we have two different fragments. It is the same for $p_{3/2}$ and $p_{1/2}$, etc. We always get twice as many at the right hand side. The shell spacing you may also recognize is rather characteristic for the nuclei.

We now can calculate the shell corrections, as well as the liquid drop energies, with an asymptotically exact model, self-consistently if you want. Both do not go to infinity, but they go to their proper values which they asymptotically would have according to the shapes of the model. By calculating the liquid drop corresponding to these two-center shapes, and adding on top of it the shell corrections calculated with the same model within this Strutinsky method, we obtain potential surfaces.

In Figs. 9-12 I'll show you a sequence of potential surfaces. Fig. 9 is for ^{226}Ra . If we didn't have the two-center degree of freedom

Fig. 9

in it, we would have gotten only the potential around the $d = 0$ line. This is precisely the old Nilsson model, and you see the various equipotentials now in the way of a landscape. The deepest potential is for

$d = 0$, $l = 0$, for a spherical nucleus; it tells us it's a spherical nucleus of nearly no deformation. And then you see that as you go to greater elongation you come to a saddle point with a very shallow, straight isomeric minimum (I wouldn't like to say anything about it), but then you climb up the hill, about 10 MeV, and from there on you climb through a double hump. This is the Nilsson mountain, as we call it, but the actual nucleus decides, when it comes to that barrier, which is the fission barrier, that it wants to go on out that way. Now, from the fission barrier on, the two center degrees of freedom are important, and this steep valley at the lower right is a Coulomb valley.

In Fig. 10 for ^{232}Th we see this even more distinct. Again you

Fig. 10

have the same thing, a first center, a kind of an isomeric shape minimum, and then you climb up to a saddle point; the nucleus goes out into the Coulomb valley, and again there is Mt. Nilsson.

Fig. 11 shows ^{236}U . The only difference you may notice is that the minimum in the spherical ground state becomes more deformed and deeper, the isomeric minimum becomes well pronounced now; it is a kind of a second minimum out there where the nucleus can sit in the isomeric deformed shape,

with a higher barrier to the second minimum. The saddle point is nearly perpendicular to the Z_0 direction.

What we learn from this is essentially that these nuclei are proceeding along the Nilsson axis (see Fig. 13) up to the saddle point of

Fig. 13

fission, and then go out, nearly perpendicularly, in the two-center degree of freedom. And that's why the Nilsson model can be used for calculations up to the saddle point, and from there on it's no more valid. Now, I should mention that these potential surface calculations were developed at home in Frankfurt and done by Mosel and Harold Schmidt⁴⁾ in Oak Ridge.

Question: Are these calculations for the mass breaking up into equal fragments?

Answer: Yes. These were all equal fragments. I come to unequal fragments right now.

The next generalization you of course can think of is this.

Ordinarily, you do not have symmetric fission. You have mostly the nucleus dividing up into two unequal fragments. Now the question is, how do you handle this? It has been done by Mr. Maruhn,⁵⁾ and here I show you what we have done. In Fig. 14 we have the typical shapes of the isomeric two-

Fig. 14

center shell, to which we come now. We allow for deformations in both fragments and the deformation is always defined as the ratio of the minor to the major axis of the ellipsoids. We allow different separations z_1 and z_2 , and, most importantly, we allow different volumes - volume defined as what is to the left and what is to the right in this squeezing in. So if the nuclear matter is homogeneously distributed, these volumes correspond directly to the isomeric mass splitting.

Now for the potential in all these previous calculations; there were sharp-edged barriers all around this oscillator. In here we have to use a little bit more complicated function, because in the ρ direction, we have to match potentials at every point out in a smooth way, and this

you cannot achieve with an inverse oscillator; you have to use a more complicated function.

I should say one more word on how to resolve it. I mentioned before that we can get analytical solutions for the asymmetric breakup in such a case, where we have fragment 1 and fragment 2, but with the same oscillator in the rho direction. Now, in reality, however, the lighter nucleus fragment is smaller, even in the rho direction, than the big fragment. We proceeded in the following way, that we used these analytic solutions always as a basis, and diagonalized these differences with l -s forces and l^2 terms introduced in the same way as before. This is mathematically more difficult than before, but it can be done.

Fig. 15 shows how equipotentials of such a nucleus look. This

Fig. 15

is roughly the surface of a nucleus, and you see, then you have equipotentials which are belonging to both, and then you have the individual circles at a certain distance developing between the two nuclei, and we have a barrier in between, as you can readily see from that picture. In Fig. 16 are just a few shapes which can occur. These are of course artificial shapes, but they are all contained in these various parameterizations. Fig. 17 is to show that all types of shapes are possible. You can construct practically any shape you want with such a model.

Fig. 16

Fig. 17

Now, in Fig. 18, I show some results for the wave functions for two fragments which are now no more symmetric. They do not come out as degenerate as before. Before, when we had equal fragments, we always had parity conservation with respect to the origin. Now, if this center A_1 is

Fig. 18

larger off from the origin than A_2 , of course, this parity conservation does not hold anymore. You have, so to say, a direct parity violation with respect to the center, as it must be, in such a nonsymmetric state. Therefore the wave functions come out either to the left side or to the right side, and this is fine. In the other case, in the symmetric model, if you want to have the wave function sitting in the left or the right center, you have always to take the plus-minus combinations of the two-center shell model wave functions.

I show now, in Fig. 19, the level sequences as a function of the separation between the two centers, in the asymmetric two-center shell model. On the right hand side we do not get one shell model, but two; one is referred to the light fragment by the upper index L, and the other one by the H's. You see that the light fragment has a larger spacing than the heavy fragment, as it must be, since the oscillator frequencies, and therefore the spacing of the shell model frequencies, are inversely proportional to $A^{1/3}$ (ω is always proportional to $A^{-1/3}$). The light fragment with the small A has to have the larger oscillator frequency, hence the larger spacing. But you see now how the separation takes place, and when the asymmetry goes to zero, these two lines merge. The $3/2$ heavy

Fig. 19

and the 3/2 light lines always merge.

In Fig. 20 are the upper shells and you see now it becomes

Fig. 20

rather involved. However, there is one important thing which you see by looking at the picture in the following way. We have the shell structure of the combined fragments on the left. The shell structure of combined final fragment reaches in obviously very far, to the saddle point. You see indications that the asymmetric fission will be mostly determined by the shell structure of the final fragments.

I would like to discuss now a most recent development, namely, the calculation of the mass distribution in asymmetric fission. Think about the following situation. You have the main fission coordinate. (I call it now, the two-center degree of freedom). The path looks roughly like this: ground state, a kind of isomeric state, and then a saddle, and then we go downhill in the Coulomb barrier.

This is now a one-dimensional representation of the curved path which I showed in the potential diagrams.

Now how can one create asymmetric fission most easily? In principle, such a potential is now dependent on many, many variables; there is first this R , there is second the deformation of fragment 1, let's call it β_1 , and for fragment 2, β_2 . There is a barrier height in between the two fragments, which you also can vary, and finally there is the asymmetry which we described. There is also a difference $A_1 - A_2$. These are all parameters on which the potential depends:

$$V = V(R\beta_1\beta_2EA_1A_2) \quad (R = Z_0 \text{ when } A_1 = A_2)$$

Now we did a calculation along the following lines. Put A approximately zero in the first step, and then look in this four-dimensional plane for the minimal path. The length along that minimal path, so to say, along the geodetic, we call R. It is the minimal one of all the potential in terms of these four coordinates. Now, this is the R plotted above, and so this is the potential along this minimal path.

We now treat this asymmetry as a dynamic collective coordinate, which we define as $\xi = (A_1 - A_2) / (A_1 + A_2)$, so $\xi = 0$ means symmetric fission, $\xi \neq 0$ asymmetric fission, or other breakup. For every R, we get now a potential $V(R)$, but also a coupling potential of this form. I'll write it in this way

$$V(R) + C(R\xi/2)(\xi - \xi_0)^2$$

where ξ is the minimal path, so to speak. For symmetric fission, ξ_0 is 0; for asymmetric fission, something else. But you get out of it the curvature of this oscillator (out of the V-R plane).

Now, we have the potential, but we don't have the masses. Now, with the masses, it is a different story, but of course as you define a potential surface you also can define a kinetic energy surface of kinetic energy operator, and you end up with the following type of masses,

$$\frac{1}{2} B_{RR} \dot{R}^2 + B_{R\xi} \dot{R} \dot{\xi} + \frac{1}{2} B_{\xi\xi} (R, \xi) \dot{\xi}^2$$

where there are mixed terms with coefficients which may depend on R and ξ .

How does one calculate these masses? These masses are calculated within the same two-center shell model, which I described and for which the potentials are constructed, by using the so-called cranking model or cranking formulas which were first developed by Inglis.⁶⁾ We of course put in pairing and all these fancy things. A mass B_{xy} for example, for the two degrees of freedom x and y, is of the following type:

$$B_{xy}(x, y) = \frac{\sum \langle 0 | \frac{\partial}{\partial x} | u \rangle \langle u | \frac{\partial}{\partial y} | 0 \rangle}{(E_0 - E_n)^2}$$

It's the sum which you obtain going from the ground state scattering by the operator $\partial/\partial y$ into an intermediate state, n , and then going from intermediate state n , scattering by the operator $\partial/\partial x$ into the final state, and then divided by an energy denominator $(E_0 + E_n)^2$.

I may transform this into a kind of particle coordinate. These masses are of course a function of their coordinates themselves. They may vary from point to point, but can be calculated numerically. To see what coordinate-dependent masses mean, you can imagine the following example: If you would have a sand wagon, it's a function of the coordinate, that is whether you are in Kansas City, where it's loaded, or on the way, where it's full, or in Manhattan where you unload it, or on the way back to Kansas City where it is empty. You see, the mass of the wagon is not the same at each place and time, and this would be a typical model of a coordinate-dependent mass. In a many-particle problem, which fluctuates and makes motion, the number of particles which may participate in the motion very well depends on the coordinates. This is immediately plausible, but very few people recognize this.

In Fig. 21 we show calculations of these masses as a function of R . You see that for B_{RR} for various values of deformation, these masses are very, very large, and asymptotically merge into precisely the reduced mass. Now since that must be so, you see that the model is asymptotically quite correct, but it is very essential that the masses in the interior are very large. This is a very interesting thing; it is a nuclear Landau-Zehner effect, if you want. Always when you have level crossing, which you have in the interior, you somehow can get a very strong repulsion, and this is precisely the virtual Landau-Zehner effect in nuclear physics.

I come back now to the problem of asymmetric fission. You see, what we find here is that the kinetic energy as well as the potential energy is a function of R and ξ (Fig. 22). One can calculate the mass distribution in the following way. We assume that there is adiabaticity, that the velocity in the R direction is so small that you can neglect terms in $\dot{R}$. It is a common belief that fission goes like that. We do not know how good that is; we have to find it out, and we will. If this is true, then these $\dot{R}$ -terms vanish, and at every point R , you have essentially oscillation, now in a dynamical sense, because it is a particle fluctuation

Fig. 21

Fig. 22

back and forth from the one center to the other.

That is what this ξ coordinate describes in a dynamic way, and now, if I assume this is adiabatic, then of course at every point R I can search for the lowest wave function in the ξ oscillator. And, let's call the ground state in the ξ oscillator $\phi(R)$. At every R there is in principle a different ξ . Then, of course, the quantity $|\phi(R, \xi)|^2$, for any given R , gives you just the mass distribution as a function of ξ . This idea excites me when I think about it, because it is a very elegant trick. You get the whole mass distribution in one elegant way out of it; you just calculate one wave function; you will see how it works and in what I show is no parameter fitted whatsoever.

These are the calculations of Maruhn.⁵⁾ (Fig. 23) Here is the

Fig. 23

potential, shown coming up from the spherical point up to the saddle point.

The saddle point is at about 5.0 on the vertical axis and in the middle of the horizontal direction, but you see you can go now two paths. This surface is always symmetrical around ξ_0 . The asymmetric fission barrier is above the saddle point, of course.

In Fig. 24 I show the final results. The ξ mass at the top

Fig. 24

oscillates wildly. This is a double oscillator, a cut through the oscillator in the final states just before fission of the fragments. At the bottom is the wave function squared, together with the experimental points. Nothing has been normalized, and nothing has been fitted. And it is a logarithmic scale, which shows that the maxima are at the right place. For symmetric fission we get a yield which is too small by a factor of 10^5 , in the maxima of the yield curve we get too much by a factor of 2 or 3 and at strong asymmetry we get too small rates again.

Exactly what we miss for symmetry and strong asymmetry, we overshoot around the maxima. If we add a little variation from adiabaticity, by exciting higher ξ oscillations, we get wave functions which grow out in the ξ direction. Then the tails of this are filling out the minima, and asymmetric fission can be directly attributed to the deviations from adiabaticity.

Now, this is not the end of the story. The work from here has to go on in the following way: One has to find out how, for example, does a third peak develop which is observed, especially at higher energies. What this would mean is that you excite ξ oscillator states, ξ states which have eventually a maximum in the middle, which can easily be done, and then you will observe that they build up, in a dynamical energy-dependent way, to fill the minimum in the middle.

And finally, for very high energy, of course, we come to a symmetric distribution again. That is the one thing, but you can turn the crank around. When you come in with a certain ξ function in heavy-ion scattering, you may study, in a time-dependent way now, what develops after scattering. You may calculate in a very simple and rather straightforward way the whole mass distributions of observed fragmentations in a heavy-ion collision in a very simple and rather elegant way.

Now let me finally summarize, that you have seen that this two-center model is a rather lucid one; it can easily be used. I should mention that there are different groups who have followed immediately different paths. For example, J. R. Nix⁷⁾ of Los Alamos, uses two square wells, which he solves numerically and joins together. There is also some Russian work by a man named Pashkevich⁸⁾ along lines which are similar to those of Nix, and it is like nuclear shell models of the traditional sense. You could have nuclear shell models with oscillators, and you could have nuclear shell models more realistic with Woods-Saxon potentials. The advantage of the oscillators was always that they were most handy and easily used in large calculations. And I think this is also the big advantage of it, and an application of the whole thing on the inverse processes, not in fission but on heavy-ion scattering, which I will talk to you about tomorrow. Thank you very much.

Question: I am interested in the time of all these things. I

was always under the impression that fission took 10^{-20} seconds, something like that.

Answer: This is right, what you say; fission is a process which takes place on the order of 10^{-19} seconds or so; what you mean by adiabaticity is essentially the following: when you solve the energy in the R mode in the two-center shell model as a function of R, this internal excitation energy is sometimes very small compared to it.

Question: Your R is a distance over a potential surface really. Z_0 is the distance between the nuclei.

Answer: Yes, that's right. Asymptotically the R is equal to Z_0 . For small distances R is more like a deformation coordinate. If you would have Euclidian geometry throughout, then R would simply be the length of the arc lengths of this curve, but since you have varying mass, you have to weight it with different factors at different points, and it is a little bit different from that.

Question: You said that at higher energies you got too much symmetric fission from the calculation, but as you go up in energy, you actually do get more symmetric fission.

Answer: First, this experimental data, which I showed for uranium, are not, as it turned out, ground state fission. Our calculation was ground state fission. It was calculated as if we had ground state fission; that is we are always sitting in the deepest ξ oscillator state, but the experiments are done with an excitation energy of the order of about 4 MeV; that is, you have so much excitation energy in your nuclear system, it may couple in the ξ oscillator.

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THE TWO-CENTER SHELL MODEL AND HEAVY-ION SCATTERING

I would like to continue my lecture from yesterday by applying the two-center shell model to heavy-ion scattering. It is clear from what I said yesterday that this model is very suitable to describe heavy-ion scattering, especially the shell structure during the collision of two ions. One might think at first that everything has been said; that heavy-ion scattering is just the inverse of fission, but if you think a little bit about it, you recognize that it is not so. The reason is that in fission, by definition, we assume that the processes are rather adiabatic. We have seen toward the end of my last lecture that there might be indications of deviations from adiabaticity, especially at higher excitation energy. However, in heavy-ion scattering, when you bombard two heavy ions, you are faced with the fact that you shoot, with rather high energies usually, two heavy ions on each other, and you might get local compression. Especially at high energy you would expect that local compression takes place, and in some way we smear out over a larger piece of overlap or so, and it is one of the important problems, I think, to try to deduce from

heavy-ion scattering, some basic nuclear quantities which we do not know from other sources, something like the nuclear compressibility, κ .

Of course, in order that we are able to do something like this perhaps we need to know a lot about the collision process and of what is going to happen in such collisions. So, let me start now with my first experiment and discuss this type of topic. I will come back to this

question of compressibility in a heavy-ion collision, at least what one can say theoretically about it and see indications of.

There were experiments done, very early, by the Bromley group¹⁾ about 10 years ago as they scattered carbon on carbon and observed resonances at the Coulomb barrier which they interpreted as intermediate molecules occurring in heavy-ion scattering. These measurements were continued, when higher energies were available again by Bromley and his collaborators²⁾ about 5 or 6 years ago, at relatively high energy. What they measured is typically always something like this, a ^{12}C - ^{12}C or ^{16}O - ^{16}O scattering experiment. They found the Rutherford cross section for small energies, and then as soon as they came across the Coulomb barrier in energy, they found a decrease of the cross section by a factor of 30 to 50, and then typical sharp resonances, and more carefully they observed even a fine structure on these resonances. The width of the main resonances was of the order of 2 to 5 MeV, and the fine structure widths were typically of the order of about 0.3 to 0.5 MeV.

Well, it will also be one of the goals in this lecture to try to explain and make clear what these resonances are. The basic idea behind this is the existence of intermediate nuclear molecules, where the two ions do not lose their identity, amalgamate and make a compound nucleus, but rather stick together in a surface type of a reaction and form a molecule. This will be the main explanation for these things, and I will come to the details.

You see at the top of Fig. 1 the ^{16}O - ^{16}O excitation function for elastic scattering. There is the typical behavior I described, the Rutherford cross section at low energy (note the logarithmic scale), then a fast decrease with energy to the main structure. On top of the main structure is this intermediate structure; you see it much more pronounced in a non-logarithmic scale (see Fig. 2). And in the carbon-carbon collision you see the same thing. You have also low-energy Rutherford scattering, but the intermediate structure is now much more dominant; it is stronger somehow. You cannot distinguish too well between the main and the intermediate structure. That is always true if the coupling somehow becomes too strong, which you would expect in carbon, because it is softer than ^{16}O . Now in nitrogen on nitrogen on the other hand, you see again the same thing, but here only main structure; there does not exist intermediate structure,

Fig. 1

Fig. 2

and we will have to talk about that. Finally, at the bottom of Fig. 1 there are experiments from the Seattle group³⁾ for ^{18}O plus ^{18}O . However, there the cross sections are so small that I wouldn't like to say anything

about it.

Fig. 2 is the data on a linear scale for the $^{16}_0-^{16}_0$ excitation function. You see now the main structure and the intermediate structure rather well, and I will come back to that.

Now, what we are obviously dealing with here is the problem of calculating a potential between the two ions, which has to have a real and of course also an imaginary part. These potentials in elastic scattering will be responsible for shape resonances, and these resonances here look somehow like shape resonances. Now the question I try to deal with is how to calculate the real potential, and then I will come to the imaginary potential.

Yesterday we also calculated the real part of a potential, for fission, and we did it in the following way: construct a liquid drop model, calculate your liquid drop energy, and add onto that the shell corrections within the two-center shell model. Now, we can in principle accept the same procedure, but there is one difference, and that is we have to incorporate now in our theory the possibility of having compression in the system. This is not the case in the normal liquid drop model.

The model which contains nuclear compressibility is called the extended liquid drop model,⁴⁾ and it is the following. We define the energy functionals, that is, energy as a function of density distribution ρ , and this consists first of a part which is proportional to A . The entire expression looks like this:

$$E(\rho) = A_k + \frac{C}{2} \frac{(\rho - \rho_0)^2}{\rho_0} + \int \phi \frac{e^{-\mu r}}{r} \rho + \text{Coul} + \text{Symm} \quad .$$

We have a term in the expression for deviations of density from the normal density ρ_0 ; ρ_0 is the matter density which you would have in infinite nuclear matter. It is, roughly speaking, the density of nuclei to which all the nuclei seem to converge around the origin or away from the surface area. If we have deviations either from that density, either too low or too big, then we get compression in the system, which will give rise to dramatic energy change. The constant C is unknown. It is a constant which characterizes compression in nuclear matter, and it is related to the compressibility.

Now, in this energy function there is a kind of Yukawa term; I just write it as an integral. There is also a Coulomb term; I just write Coulomb plus symmetry energy. You all know that from ordinary textbooks where one justifies a kind of an infinite nuclear matter calculation. One can derive now from such an expression a differential equation for ρ , which one can solve. One can in fact derive the whole mass formula from such an expression, and see that the binding energy variation with the proton and neutron number comes out rather correctly, and all that. I don't want to go into these details; I just want to mention that the constants which are entering here, except for C , are all determined rather well from all kinds of properties you can imagine. And it even leads to the charge distribution of individual nuclei, and that leads to Woods-Saxon type distributions.

Now, after having defined such an energy functional, and fixed the constants from binding energies, radii and so on, we have only that C remaining, and we can calculate with any C value we wish to assume. We can calculate the potential for density distributions, for example. When we let two nuclei collide, like this

in the middle we just add the density. We have the normal densities ρ_0 , and in the middle we have $2\rho_0$. Then we see that we get a lot of compression in the system, and in this way we get ρ as a function of R . We can plug it into such a formula and get E as a function of R , and this means we get the potential.

Now, we have to add shell corrections. We don't want to have local compression in the system, but overall compression. This additional

matter, which normally would fill out to the edges, is distributed over all the volume.

So we are open to make different assumptions here. I will discuss this later on in connection with the two-center model. We have made an assumption how this compression shall go. Let's calculate now shell corrections.

Question: You say that C is a constant?

Answer: Yes.

Question: By that you mean, it still must depend on A , to some extent?

Answer: That's what we want to find out. At first we do not know anything about C , and in principle you are right, it may depend on the shell structure very strongly. But the question is, how big is the magnitude of C altogether. There is no experimental information on it. There are only three theoretical papers of Brueckner⁵⁾ on C , and all three give different numbers varying between 100 and 300 MeV. But in principle you are completely right. This C is in principle very likely a function of shell structure. That is of A , of N and Z .

Now, how do we incorporate compression effects in the two-center shell model? As I said, we can calculate things like this, and this I would like to show you now in Fig. 3. We get two extremes. The one we call the sudden approximation, the approximation when nuclear matter overlaps such that the two densities add. In such a case you get a potential as a function of R which is a Coulomb potential, then a binding part, but then soon the compression dominates and you get this strong, soft core

Fig. 3

potential. It goes up to about 200 MeV if you assume a compressibility of the order of about 100.

The other extreme case is the adiabatic approximation, for which you assume that this motion is so slow that at every instant this compression will not take place, but the nucleus expands such that it readjusts to the normal density ρ_0 . You have volume conservation all the way, and you are led to such potentials shown on the right in Fig. 3. These are for the $^{16}\text{O}-^{16}\text{O}$ case, and would be the typical potential for ^{32}S , at its fission barrier. Now in a heavy-ion collision, probably neither case is correct. That is, we have neither a sudden nor an adiabatic potential, but perhaps something in between.

In Fig. 4 now you see once more in a picturesque way the compression of two densities. These are two ^{16}O nuclei. These are their normal density distributions which you can calculate out of such a model, and you may also compare them with various values of radii and surface thickness. Now as they come together, these edges overlap and fill up the distance in between, and this is where things attract, where you get the binding part of the potential. But then when they overlap too much in this sudden approximation the compression becomes so large that you get really much more density compared with ρ_0 and this leads to the strong compression, to the strong soft core.

Fig. 4

I come now to the point of the two-center model; how do we discuss compression in the two-center model? Now compression in the sense discussed here means that we leave the radii of the two ^{16}O nuclei unchanged while they collide. Adiabatic approximation means we blow the radii up such that at every distance the volume is conserved. Now the radii on the other hand are directly related to the oscillator constants we used yesterday, which are proportional to $A^{-1/3}$. And this determines the radius of the nucleus. Now if during the collision we leave the oscillator constant ω the same, then what we get is a shell model of ^{32}S but compressed, because when you let $^{16}\text{O} + ^{16}\text{O}$ collide, you have 32 nucleons in the system, but compressed to a smaller volume because the oscillator constants belong to ^{16}O .

And what will happen then is schematically shown in Fig. 5, namely that these energy levels all move upwards. In a qualitative way now you see how a compression shifts the levels. In a quantitative way you see this in Fig. 6 for the difference between compression, or a sudden

Fig. 5

Fig. 6

approximation, and the adiabatic approximation in the two-center shell model. In the middle we have the two ^{16}O nuclei separated, and on the right they collide to form a ^{32}S according to the adiabatic assumption. Adiabatic assumption means the volume is at every distance the same. This combined sphere has the same volume as the two separate spheres together. And you see we get this shell structure out of what we had yesterday

already; this is the adiabatic two-center shell structure. We have the ^{32}S shell model, and on the right the two ^{16}O shell models.

However, let me go to the left side, and let the oscillator constant be the same during the collision, symbolically shown by that shadowed area. You see that when the oscillator remains constant, clearly all these levels rise. Of course, it doesn't come out to be the ^{32}S shell model, but a shell model where you have the particles of ^{32}S fitted into an ^{16}O shell model. This rise of the levels here is the compression effect in the shell model.

And now I come back to your question. Is the compressibility shell-dependent or not? If you believe the shell models strongly, you can calculate the compression effect right away from this model and you get shell-dependent compressibilities. We take the opposite point of view, at the moment. We assume a common compressibility all the time and add onto it shell corrections which are calculated as I discussed yesterday, namely the sum over the individual single-particle energies minus the average sum.

$$\text{Shell Correction} = \sum_i^f \epsilon_i - \sum_i^f \bar{\epsilon}_i .$$

The shell correction is the sum of the ϵ_i to the Fermi surface minus the sum over the average $\bar{\epsilon}_i$. We can either calculate in the sudden or the adiabatic approximation. In both cases, either we sum all the single-particle energies and average over them afterwards, or we sum all these average single particle energies. So we get shell corrections again in the sudden and adiabatic approximations in that way, and we add them on the corresponding sudden or adiabatic potentials.

Fig. 7 once more shows the energy function which I described earlier, only with more details. Fig. 8 shows you some calculations of the real part of the optical potential, where we have added the Coulomb force. The Coulomb force is rounded off at the nuclear surface.

These are adiabatic and sudden cases for various systems: calcium on calcium, sulfur on sulfur, silicon on silicon. Recognize that in the case of the sudden approximation there exists really no binding anymore for the calcium-calcium system. That is, in the sudden approximation, if this would hold through all the periodic table, we would get

$$\begin{aligned}
 E[\rho] = & W_0 A + \frac{C}{2\rho_0} \int (\rho - \rho_0)^2 d\tau \\
 & + \frac{V}{8\pi} \int \rho(r_1) \frac{e^{-|r_1 - r_2|/\mu}}{|r_1 - r_2|} (\rho(r_1) - \rho(r_2)) d\tau_1 d\tau_2 \\
 & + \frac{1}{2} \left(\frac{eZ}{A} \right)^2 \int \frac{\rho(r_1) \rho(r_2)}{|r_1 - r_2|} d\tau_1 d\tau_2
 \end{aligned}$$

$$E/A = W_0 + \frac{C}{2} \cdot \frac{\rho_0}{\rho} \left(\frac{\rho}{\rho_0} - 1 \right)^2$$

Fig. 7

Fig. 8

from a certain system, as in the area of calcium, no binding part anymore in the potential. Now, I come to the question I raised. Namely, how sudden, how adiabatic is it?

To say something about this, we did the following type of calculation. Let us assume we have the following model: We describe the compression of the nucleus by R , the radius of the individual two ions. If this radius, which I call Z - (I called it Z_0 yesterday, I call it Z now) - increases according to volume conservation we have the adiabatic approximation; if it doesn't blow up, but stays constant on the radius of ^{16}O , then we have the sudden approximation. (See Fig. 8.)

And like yesterday we can calculate the potential energy, in both cases. We can calculate the masses in the Z -direction, B_{ZZ} , (the factor of $\dot{Z}^2$), which is a function of course of Z and R in general. We can calculate the mixed mass B_{ZR} , and we can calculate the compression mass, the mass connected with the compressional motion. All of these terms and matrix elements here are functions of Z , R , and $\dot{R}^2$. And then we calculate the adiabatic potential (all this in the adiabatic approximation for the moment) as a function of Z .

We calculate also the compression potential, in the following sense, that we say, well, here we have our adiabatic potential as a function of Z , but, when we would take our two ions and bring them very, very slowly together, then we would see how they come along the adiabatic potential. Now, there is however a compression in the system, and if I assume that there is a compression of a certain quantity, then I would have such a term to add. A constant $C(Z)$, with a kind of a curvature, which I again show here,

$$V(Z,R) = \frac{1}{2}B_{ZZ}(Z,R)\dot{Z}^2 + \frac{1}{2}B_{ZR}(Z,R)\dot{Z}\dot{R} + \frac{1}{2}B_{RR}(Z,R)\dot{R}^2 \\ + V_{\text{adiab.}}(R,Z) + \frac{1}{2}C(Z)(R-R_0)^2$$

in the R -dimension so to speak (R_0 is always the adiabatic radius). It is a quantity defined as a function of Z , but if I change R from this adiabatic radius R_0 , I have additional compression in the system. And these things of course you can precisely calculate, as I said. Namely, this is

the total potential, $V(R,Z)$ but just depicted in a peculiar way that the physics becomes more transparent, and that it consists of the adiabatic potential plus at any position in Z an expansion in Z around the adiabatic potential in the compression coordinate.

Question: Is R a radius....?

Answer: R is the radius of the individual ion, and Z is the distance between the two ions.

Question: Why is the compression proportional to the radius squared?

Answer: It is proportional to $(R-R_0)^2$ because it has a minimum at the adiabatic radius R_0 , and when you decompress or compress your system, in either way which you deviate from the adiabatic value you raise the energy. You also raise the energy by decompression, not only by compression. That is a stability condition, so to say, always for stable minimum.

Question: Before we had variable densities....

Answer: Here it is the same thing. Either you can make ρ smaller than ρ_0 , or larger than ρ_0 , and you get the same amount of energy. Around any equilibrium state you may expand in a quadratic way; otherwise you have no equilibrium.

Question: So in other words you're just doing a Taylor series expansion?

Answer: You may call it that, but it's completely sufficient; it's so good that it's completely sufficient.

Let me repeat what has been done, because this is only the calculation of a Hamiltonian. If we call the first three terms the kinetic energy T , and the last two the potential energy, we can also write a Lagrangian $= T-V$. And we may treat now the following problem, in a classical way first, because the classical physics tells us a lot. In a classical way we let the two ions collide at a given energy on the adiabatic potential and see how much the compression mode is excited along the ion path by setting up now the Lagrangian equations. We get two coupled Lagrangian equations; one for the Z motion, and one for the R motion.

If we solve this system of two equations, we get $Z(t)$ and $R(t)$.

I can eliminate t , and I get, for example, R as $R(Z)$. But when I solve this equation at a given energy, I can always say that at every energy, when I am at a distance Z apart in the dynamical model, where I let the ions collide along the real potential, I get a certain amount of compression. If for example there would come out R equals R_0 , then I have no compression at all in it. If there something else would come out, then I would have compression in the system.

So I think at least the motivation is clear and also the calculation, perhaps not the details, but they are not so essential. So these things were solved, and the results I'll show you now in the following slides. Let me first say this. We can, for example, set $R = R(Z)$ for given incident energies, in that expression, and then we get the potential compression energy. However, there is also a kinetic compression energy, because we have terms as a function of $\dot{R}(Z)$. And then we get additional compressional energies in the system, which are due to the kinetic energy.

This is the same procedure as we do in quantum mechanics when we define effective potentials. Effective potentials are always defined by eliminating channels. If I have a system of, say, fifty coupled channels and I would like to have an effective potential of only one of these channels, then I must eliminate the other ones, and this leads me precisely to the idea of the optical model. The same thing is done here. I have eliminated, so to say, the compression channel by attributing at every distance Z , which describes the elastic channel, the corresponding compression coordinate to get this equation. Now, classically this can be done very simply, but quantum mechanically it leads to very, very complicated integro-differential equations. That's why we did it classically first, to get some insight how the quantum mechanics problem may be solved.

But now let's come to this potential compression energy, calculated in that way. You see in Fig. 9 the two ^{16}O ions. The adiabatic potential is at the bottom, and now if I let the two ions collide with 30 MeV, then you see that the potential is not changed very much. This compression mode is very small. But when we go to 300 MeV, it is already more appreciable, and at 3000 MeV it's the dominant part of the potential. So you may say that compression probably doesn't play a role, at least not for the small energies we are dealing with at the present time of 30-40 MeV.

Fig. 9

But this is a mistake, because it considered, as I pointed out very clearly, only the potential compression energy. We will see how the situation changes immediately if I take the total compression energy, which consists of kinetic and potential energy. Now how can we do that? We can do it in such a way that I say, I know now my function $Z(t)$. Let us insert also $\dot{R}(Z)$ everywhere, and then you see I get a functional of the form, $\frac{1}{2}B_{ZZ}(Z)\dot{Z}^2$, which is a sum of all these terms. It is a function of Z now. We can then make a transformation on this in a way, that there is only the reduced mass μ everywhere. This is now an effective mass, which comes from the elimination of the compression channel. And I can transform this such that I get $\frac{1}{2}\mu\dot{\tilde{Z}}^2$, where $\tilde{Z}$ is a real coordinate describing the distance between the two centers in an Euclidian way, if I have a constant mass. This requires the transformation which one has to do between Z and $\tilde{Z}$.

The main purpose which we should remember by looking now at Fig. 10 is that we also take into account in that transformation the total compression energy, consisting of kinetic and potential energy. The adiabatic potential is the dashed curve. I am now going to 30, 60, or 90

Fig. 10

MeV, rather low energies, which we are dealing with all the time. You see that we get these shallow soft core potentials. There occurs a sharp edge but it is only for calculated reasons. I always told our draftsman to blur that point and no questions would arise!

Question: Do you have an imaginary part to that potential?

Answer: I will come to that.

So what we learn out of that is that we get for different energies obviously different soft core potentials. And this is again what we expect from this, the elimination of the compression channel. The effective potentials are equivalent to energy-dependent potentials, and here we have them in a direct way, energy-dependent soft-core potentials. This explains as well the observation which Bromley probably here discussed quite extensively, that these potentials in heavy-ion scattering which fitted the main resonances are of a shallow nature. Now this seems to suggest that they are.

The next question is of course, has this soft core a practical meaning? Is it in principle observable or not? I will come back to this question later when we discuss the physics of these experiments.

I would like to describe now the calculations⁶⁾ leading to the explanation, or part of the explanation, of these experiments. The real part of the optical potential is now clearly a soft core type of a potential. In order to proceed, I need to know W now. The imaginary potential in such a system is quantitatively described in such a way, that you say, if I had a molecular state set up, no matter how, this molecular state may decay after some time into its compound states. If it decays into the compound states, it's lost for the elastic channel. Now this means that the imaginary potential must be proportional to the transition probability for going from the molecular channel, which I call ϕ_ℓ now, into all these compound channels, which I call C .

$$W_\ell(E, \ell) = 2\pi | \langle C | V | \phi_\ell \rangle |^2 \rho_\ell(E)$$

The matrix element squared is multiplied by the density of the states $\rho_\ell(E)$. The density $\rho_\ell(E)$ is the density of states of the compound system at a certain distance R or Z between the two ions, and the molecular channel, the channel in which the identity of the ions more or less is confirmed, decays into the compound system.

Now this obviously leads to a potential which is dependent on E and ℓ at the same time, an energy and angular momentum-dependent optical potential. The known energy and angular momentum dependence comes from the density of states; it also comes a little bit from the matrix elements. I will now just discuss the main dependence on the density of states. I should mention here that the first and important proposal of an angular momentum-dependent imaginary part in heavy-ion reactions was to Robson, Eck, Richter, and Chatwin.⁷⁾ They recognized in an empirical way that strong angular dependence is necessary to explain these data.

Now, let us think a little bit about this energy and angular momentum dependence of this imaginary potential. And for this, we make the following plot.

I plot on the abscissa $l(l+1)$ for special reasons, and here on the ordinate I plot E . So W is defined as a function of R in the E - l plane. Now, we all know that there exists a more or less straight line, which is called the Yrast level. The Yrast level is defined in such a way that for a given energy E you have the maximum angular momentum in the system which is possible. Higher angular momentum than that Yrast angular momentum is not possible. That is, we can say from the beginning that in this shadowed area, in the lower part of the plane, the imaginary potential is exactly zero.

There comes now another important aspect. In a strip parallel to that Yrast level, there are only a few states. These states are mostly of molecular nature, in the sense that these are states, for example, where the two ions rotate at large separation, or where one of the ions is highly excited and they rotate in a molecular way, or both ions are excited in a molecular way. All these low density states, these molecular type of configurations, lie close to the Yrast level. This Yrast level is a minima line, it's a zero line, and what I shadow in this strip is an area of low density of states where you cannot make a statistical argument.

You have too few states, and they are all more or less - that is an assumption - of molecular nature. And there exists in the upper left a line where $\rho_E(E)$ has a maximum.

So when you come to treat such a system, you can only treat it by an imaginary potential in the strip around the Yrast level and with a statistical argument in the upper left area. All channels which lie in the lower right you have to treat explicitly. That is, you treat them by explicit channel coupling to the elastic channel. The elastic channel is one of them. To make this more quantitative, I show Fig. 11. Here is once

Fig. 11

more the $E-l$ plane shown for the carbon-carbon system. You see the Yrast line. It is a continuation of the angular momentum states in magnesium 24, extrapolated. One doesn't know whether this bends over a little bit or what it does at high angular momenta. In ignorance of that knowledge we just extrapolate linearly. There would be a kind of a strip, so to speak, next to the Yrast line where a low density zone is. There is a kind of a

molecular band indicated, like that, which we'll discuss later on when we survey molecular bands observed in experiments by Bromley.

I should first explain a few things now in the reaction mechanism, and then say how the calculation is done. I come back to the experiment and to the idea of explaining the main and intermediate structure. And the idea goes the following way. If we have such a soft core potential, the compressibility parameter is adjusted such that the first experimentally determined main resonance is explained, and the break of the Coulomb barrier goes in the right place. We have for various angular momenta, of course, different potentials. At large radius of course they go in the same way, like this.

Now, such a potential gives you quasi-bound states - we call them quasi-bound molecules - which lie below the barrier. There also occur states which are still resonances, so to say unbound vibrational states; we call them virtual molecules. The wavy line would be a virtual molecule, and the solid line would be a quasi-bound molecule.

Now what have these things to do with each other? Normally you would think that for a scattering experiment below the Coulomb barrier, you cannot see anything on the inside and that's it. I have to correct

this impression, because there were recent experiments as well in Erlangen by Voit⁸⁾ and in Australia by John Newton,⁹⁾ a pioneer in heavy-ion physics, who measured very carefully about 4 to 6 MeV below the Coulomb barrier, the deviations from Rutherford elastic scattering, and found very sharp resonances there, and in various channels. The penetrability is extremely small, but they see obviously these sharp resonances, and Newton in fact made an analysis, and in order to explain all these things quite well, he found that he has to attribute a soft-core shallow potential like that. Not an adiabatic, not a deep potential, but a soft core, otherwise he cannot explain the sequence of the resonances as an analytic continuation of the resonances we see in the continuum. This is an analysis which is unpublished, but I have a preprint from it, and I think it will appear soon somewhere; I don't know where.

Now, the mechanism goes as follows. When you come across a Coulomb barrier, you may get in resonance with one of these virtual resonances. The virtual resonances here are rather broad; they lead to the main structure. However, when you couple to the system, and allow one of the ions, or perhaps two of the ions to get excited, say to the first 2^+ in the carbon-carbon collision, then you take out of the relative motion, an internal motion energy, and you drop down and you resonate with this quasi-bound molecular state. That is, when you do a coupled-channel calculation of that type, you automatically get these resonances built in whenever you do an excitation function. With these resonances, the total system resonates, and this leads to this intermediate structure. Now of course this tells you immediately that this intermediate structure must also show up in the inelastic cross sections, which you can calculate. I hope to convince you now that this indeed has experimentally been very well verified in the last months, the last half year or so, by Pelte and his collaborators;¹⁰⁾ I will come back to that.

Now let me show this mechanism again (Fig. 12). We call this a double resonance mechanism. I should say this was done prior to these arguments which I gave here with the soft-core shallow potentials. We have done everything in the sudden approximation and all these calculations have to be redone. It will not change the results dramatically, because there is always this parameter of the compressibility, and of the Yukawa

Fig. 12

force in there, which have been adjusted so that the main resonances at the surface come at the proper position. The idea is now that the rotational potential is added on top of the soft-core potential. We come in, resonate in a virtual state (dashed line) which is very broad, and due to the excitation of the individual ion, it drops down, resonates with the quasi-bound (solid line) state, and this gives you the intermediate structure.

Now, in the carbon-carbon case, we coupled into the system the 2^+ and the 3^- states, and also other states, but mainly we coupled one elastic channel, one carbon excited to the 2^+ , and two carbons excited to the 2^+ , simultaneously, into the system. That leads to forty channels.

Question: Are you now saying that the gross structure, the three-MeV structure in oxygen is a virtual resonance?

Answer: That's right.

Question: The 3-MeV broad resonance implies a lifetime of that resonance that is approximately the time for one transit.

Answer: Yes, that's it.

Question: That it just goes by.....

Answer: Not just goes by. It is when it has been depicted in a classical orbital model. "Goes by" in the heavy-ion case means Rutherford scattering. Going through such a virtual resonance means this:

And this resonance structure you can treat of course completely quantum mechanically, so you determine overlaps of the waves, whatever they are, in an accurate way in it.

In Fig. 13 you see at first the case where none of these inelastic

Fig. 13

2^+ channels has been coupled to the system. The dashed curve is the experiment, and the solid line was the fit. There are two constants fitted, the compressibility (it was about a hundred MeV) and μ , the range of Yukawa force with which you put the Coulomb barrier in the right place. And you see then you get this type of thing out. You will say this is not a fit, but people were pleased with it; and this is without the inelastic channels included. It is just the pure elastic scattering, and these wide resonances are precisely all these virtual states.

Question: If you were to look at data at another angle, and this was due to a resonance effect, then you would expect that those structures would come at the same energy, at least in the oxygen-oxygen scattering.

Answer: Well, not only in the $^{16}\text{O}-^{16}\text{O}$. It depends on the coupling of the other channels, because the coupling to the other channels may shift all those angular distribution points.

Question: What I'm saying is it should fit that excitation function as well as the excitation function at another angle.

Answer: It has only been fitted to the excitation function at ninety degrees, here, and at the other angles it has been predicted, and with a coupled channel calculation it agrees rather well, except for very small angles where there are deviations at the higher energies.

Question: Going back a couple of steps, when you introduced your virtual states, would you just define them again for me please?

Answer: Certainly. When you have a potential of this molecular type, then you get bound states which are actually not bound states because there is a small penetrability through the barrier. Therefore they are called quasi-bound states. But when you take an infinitely high oscillator, you would expect another state above them. But due to the fact that it is only a finite high oscillator, and rounded off, this state lies a little bit at a different energy and becomes very broad. And such broad states we normally call resonances, or shape resonances. In order to distinguish these lower states and those upper states, which are actually the same, we call the one virtual, the others quasi-bound molecular states.

Question: Do I understand that you see this shape resonance

because your imaginary potential has an l dependence that allows you to penetrate that far?

Answer: Yes.

Question: So that you only see a certain l -value shape resonance.

Answer: Yes. You see channels which are all lying in this strip of extreme low density. Therefore, these type of channels have to be treated without any imaginary potential. The imaginary potential comes in only for those channels with low l and E which lie over to the left. And this is how the calculation was done. That is the main idea, but it's quite general in the sense that there is a strip parallel to the Yrast level, where you have a low density zone of compound levels, where there can be only molecular states in there. And it's clear that the imaginary potential of states which lie precisely on the Yrast level, as supposedly these main resonances would be, that they have imaginary part zero of course, because they have nothing to do except decay because they are above the Coulomb barrier.

Question: You and John Eck possibly disagree on the specific placement of resonances.

Answer: I don't know even whether we disagree there. I think what this says is just a kind of a philosophical concept that agrees with what Robson and John Eck established in their model.

Eck: The model is not microscopic in any sense.

Greiner: Well, this is not microscopic either, I would say. Well, it would be microscopic in the moment I calculate the $\rho_l(E)$ from the two-center model, which hasn't been done yet. There have been the typical statistical formulas used for it.

What happens to the elastic channel when these channels are coupled into it, that is, one ion excited to the 2^+ or both ions excited to the 2^+ , simultaneously? Take about 4 MeV of energy out of ^{12}C . The improvement you see is in Fig. 14. It introduces even in the elastic channel a great amount of additional structure. Now this does not give all the structure you see in the experiment, but you can't expect it because we have only taken a very few channels into account. You should clearly take much higher excitations as well. But the overall agreement shows that this

Fig. 14

mechanism is correct, and that you also recognize that here at the higher energy you don't get this intermediate structure until you introduce into the theory the excitation of the ions. That is quite clear, because when you go to high energies, if you couple only 2^+ states into the system you cannot reach down anymore; you have to couple higher states, higher internal excitations of the individual molecules into the system.

Now comes the experimental comparison in Fig. 14. All fits were done on the elastic scattering cross sections. The theory predicted (when this was published no experiment was available) the inelastic cross sections. This was one of the few cases where a theory has been done before the experiment was done, namely, the inelastic excitation functions in the carbon-carbon collision, where one of the 2^+ states is excited or both 2^+ states is excited or both 2^+ states are excited. And the predicted cross section is this: a steep rise on the threshold, very sharp resonances, and then going on up. These sharp resonances have to do with the quasi-bound levels. This experiment was done by Pelte in Heidelberg.¹⁰⁾

I also show in Fig. 15 the other excitation function we predicted for Pelte for the case of a single one of the ions excited to the 2^+ state. And you see at the higher energy end it was done by Bromley²⁾ again - Br means Bromley, P means Pelte - . And you see, the magnitude - because it's a logarithmic scale - is a little bit too high for the theory. This is not

Fig. 15

surprising, because we said that in this low-density strip where there are quasi-bound states, we assume no imaginary potential whatsoever. A little bit is of course there, because there would be even more channels than those treated explicitly, and they will immediately damp the system a little bit, so this is not a surprise. But what is surprising is that all these sharp structures, show up here in a semi-quantitative way. And the same is true now for the double 2^+ excitation. Again nothing is fitted. And the theory was published long before the experiment. In Fig. 16 is what the theory says, and what Pelte found.

This is all I have to show. I would like to finish up by saying the following. I do not intend to say that the theory has fantastic agreement; it is not correct; it is not true. But what it shows is that there is a mechanism behind it, and the mechanism is obviously pointing to the right track. And I should say that we were fought very strongly by people who said all the things you observe are fluctuations. When we made a fluctuation analysis on our theory, taking the same energy intervals in experiment and on the theory (on the elastic excitation function and later on also on the inelastic one) we found that even though nothing is calculated

Fig. 16

as a compound reaction, due to the channel coupling it simulates as if you have a thirty percent compound component. That means a fluctuation analysis is not meaningful. But on the other hand you may say, well, when you have enough coupled channels you can get any fluctuation out you want. You see, it's an explanation of the fluctuations. It's not a contradiction, but it's an explanation behind it.

Now, what it suggests is this. The model of the mechanism of this molecular model with these resonating levels has obviously a lot of truth in it. One has to proceed further, namely couple other channels into it, higher excitations on the one hand, on the other hand other molecular channels. For example ^{20}Ne and an alpha particle, which is also a molecular channel, or ^{16}O and ^8Be . It has been measured recently also by Pelte that this channel is quite appreciable. At least you get strong cross sections also for shooting in elastically in the carbon-carbon system and coming out ^{16}O and ^8Be . Of course ^8Be disappears into two alpha particles right away. These other molecular channels all belong in that strip, and have to be taken into account. And when you do this, you introduce more structure, even more than that, and of course I hope then that the

agreement will look better than it is now. We were at least very proud of when this type of agreement of the experiment to the predicted theory came out. Thank you very much.

Question: Are you planning then to couple to the second excited state at 7.7 MeV?

Answer: That's right. And there is a 12-MeV state, and we also need higher angular momenta, you know. Higher angular momenta are also essential because you reach higher partial waves.

Question: If I understand, my intuition would then say that you would suggest an enhancement of the ^{20}Ne alpha channel at the same time as the $^{16}\text{O}-^8\text{Be}$ channel, because of the three-particle...

Answer: Yes. I wouldn't like to get nailed down here, because the point is, it may very well be that when you go to certain final states, say in neon (^{20}Ne can of course be excited to various things) then the coupling is strong, in other cases weak. One has to find out.

Question: There are correlations in some of those channels; there are correlated some cases with the elastic, and sometimes anti-correlated, but there is definitely a relationship.

Answer: That's right.

I think, you see, these things have to be done now, and I think it's very worthwhile to proceed along these lines, and also to see how it goes in other systems, like nitrogen on nitrogen and so on, but the main thing for me personally is to prove that the idea of nuclear molecules in these collisions is correct. And I think these experiments and these agreements are encouraging in that direction. I wouldn't like to say that it's a final proof. I think one has to wait still a year or two. But it's very encouraging that these ideas of nuclear molecules in a heavy-ion collision are correct. This is one of the essential ideas in low-energy heavy-ion scattering. That's much more important than discovering some other level, but that there occur the analogs of electronic molecules in nuclear collisions.

Question: The curve for the nitrogen on nitrogen didn't have

what you term fine structure. Do you just attribute that to inelastic channels?

Answer: There are two reasons. It is good that you ask. Reason number one is that nitrogen has spin one in the ground state, and you have two particles with spin colliding. This increases the number of channels. Second, the binding energy situation in ^{28}Si is such that you would expect that this strip is tremendously small. This means you have a strong imaginary part. In the statistical model, the high level density reaches very far out, even into the vicinity of the Yrast level. And if this happens, of course, all this structure is damped out. The intermediate structure is there, but you never can see it because it is damped out.

Question: At the Argonne meeting there were many people that talked about many computer programs, and they all used the PWBA. But Professor Tamura mentioned his coupled channel calculation, and it seems that from this data you've just shown that coupled channels can reproduce a whole lot more.

Answer: Oh, sure; coupled channel is an exact method; PWBA is a perturbation method, where you calculate just one transition matrix element.

Question: And so it looks like all these programs these people worked on are just...

Answer: Oh, I wouldn't say that. I think that would be unfair to these people who have made so many contributions. PWBA has its value in many areas of physics, especially for surface collisions. As long as you are below a Coulomb barrier, and you have single-particle transfers and things like that, it would be useless to do fancy calculations with coupled channels. PWBA is completely sufficient when you have, so to say, just one switch. But the moment you have the things overlap so strongly that you switch back and forth a few times or so, then PWBA is no good anymore. That's also clear. And this one has to recognize. PWBA, so to say, dies the moment you come across the Coulomb barrier. And there are still some people who make some excuses for that, but I think it dies exponentially as you go.

Question: It somehow looks significant to me that the cross section goes to nearly zero in the oxygen and none of the other cases.

What's the reason for that?

Answer: In the carbon-carbon case, it goes also to zero at higher energies. At least it goes nearly to zero there. However, in the carbon system this whole break of the Coulomb barrier is much wider and most of these main resonances lie already on this edge and this is the difference. In other words, there is a decrease from Rutherford, which we call a Coulomb barrier decrease. And this is due to the softness of the carbon, so to speak, that they can very early do the easy deformations as they approach, which diminishes the Coulomb barrier.

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QUANTUM ELECTRODYNAMIC EFFECTS FOR LIGHT AND HEAVY NUCLEI

One starts ordinarily with the Dirac equation for strong fields, because of the heavy binding energies you expect. The Dirac equation looks like this:

$$\gamma_\mu \left(\frac{\partial}{\partial x_\mu} - A_\mu \right) \psi + m\psi = 0$$

Ordinarily this four vector potential A_μ is composed out of a three-vector, which is the standard vector potential, and the fourth component of it is the standard Coulomb potential, in the appropriate gauge.

$$A_\mu = (\vec{A}, A_0(r))$$

Now, in static problems one usually neglects $\vec{A}$. In a static problem you have a central field, whose motion can be neglected.

You all know the solutions. Consider for example those cases which are connected with the so-called Sommerfeld fine structure formula, namely for a central potential of this type,

$$A_0 = \frac{Ze^2}{r} = Z U(r).$$

In that case one has the following type of solution. First, in the field free case, that is when I have no potential at all, I get a positive energy spectrum above mc^2 , and a negative energy spectrum below $-mc^2$.

And now assume that these lower negative energy solutions somehow are all occupied with electrons, as in the Dirac hole theory.

In field theory we actually do precisely the same thing. Field theoreticians just say it in a little bit more fancy words; they say we define this to be the Dirac hole. But actually what you assume is precisely that these lower states are all occupied. Field theory is precisely equal in nearly all aspects to the old hole theory, it's just that the language is more mathematical and consistent, I would say. Dirac had to assume this, because otherwise there would be empty space; an electron which would be above $+mc^2$ could really fall further and further down, radiating all the time, and this would lead to a radiation catastrophe. Of course, we do not recognize that such a catastrophe exists.

If a Coulomb potential is coupled in, it happens, that a few of these upper continuum states are bound. In fact, not only a few, but infinitely many, for the pure r^{-1} potential. If the potential is not purely r^{-1} , say if you have another term, then it's cut off somewhere, as a function of r . Then it's only a finite number, usually a very few states, which are bound. The Sommerfeld fine structure formula gave as a function of Z , the dependence of the energy levels of the Dirac equation in an analytic form. In the hydrogen case, these states are very close to the $+mc^2$ line; the binding energy is of the order of 13 eV.

But then when you come into the area of Fermium, that is Z equals one hundred, the K binding energies have become of the order of about 100 keV, that is, one-tenth of that gap. Now, when you go further along with the fine structure

formula, at $Z = 137$ all solutions dive below zero energy. And when you look up in textbooks, why these have to be there, you find very, very ridiculous explanations of it. The reason is that the s-wave functions, that is R_{1s} , are proportional to $r^{\sqrt{1-(\alpha Z)^2}}$, alpha being the fine structure formula. You see that if αZ is larger than one, and this happens precisely at 137, then this solution has an imaginary exponent, and it starts to oscillate. In other words the wave function is no more normalizable, and the normalization condition of a wave function is essential for quantizing such a system in first quantization. We will come back to this problem later and see what happened.

It was Wheeler and Werner¹⁾ who very early studied the extension of this problem, which was also investigated by Pieper and myself²⁾ about six years ago. If you make extension of this problem, you don't choose a $1/r$ potential. You cut it off somewhere, at some radius, like this:

If you choose an oscillator or whatever you have for an extended charge distribution, and you choose a radius $r_0 A^{1/3}$, then you can find solutions all the time. But you have just postponed your problem, because at that point something else happens. The binding energy of the s states, and correspondingly of the other states, becomes very large. They become $2mc^2$, and join the lower continuum.

Now, physically speaking, it is the following that is happening. Say you have a hole in the K shell. You have the K-hole somehow continuing its way down with Z .

The state is still to be remembered as a kind of a bound state, and you have suddenly a free bound state wave function lying in the lower continuum. And then out of these occupied levels which you have around, of course, without needing energy, one of them can jump over into that K-hole and can free a positron, create a normal hole which is called a positron, and the positron escapes. That is, you have an energyless creation of electron-positron pairs from that point on.

We will call this point Z critical (Z_{crit}). Z_{crit} is defined in such a way that it is the last proton number where you have still the s state bound. If you add one proton more, you get to the unbound lower continuum. Now, this is precisely where we have to think about this mechanism, how these things go. You can also say it in a little bit more loose language, and consider a K-hole as nothing else but a bound positron. And if these fields become strong enough, this K-hole may become unbound.

I would like to discuss the autoionization of positrons; secondly, I would like to discuss the problem of vacuum polarization. Connected with this are non-linear field effects, that is, the question of how non-linear is electrodynamics. I will discuss various possible experiments to check on this. Of course one wouldn't talk so much about it if this would be just an academic problem, if it were not feasible that one can check it. And finally, if you do not find that these are already speculations enough,

I will speculate on something more.

What is autoionization usually? In atomic physics it was discovered very early, in the early thirties, and formulated by Fano.³⁾ I will make use of Fano's formalism here. It was copied in many ways by nuclear physicists. You have a well, and you have certain bound states in that well, and let's assume you have in the well the Fermi surface:

You will create always two-particle, two-hole states of this type. And it is clear that the total energy of these two-particle, two-hole states lies in the continuum. And it's a bound state configuration because all wave functions which occur are bound.

But degenerate with this bound state which lies up here in the continuum are obviously simple one-particle, one-hole configurations because you can directly go from a bound state into a continuum by a one-particle, one-hole transition. Now ordinarily it is so that the bound state and these continua - there may be one, two, three or more continua - mix with each other, because of residual interactions. It's always true

that when you have embedded a bound state in the continuum these things try to mix because of some interaction you have forgotten. And due to this mixture, this bound state now may decay. It mixes with the continuum, and this is a decaying state. One of the particles in the two-particle, two-hole state jumps down into its hole and kicks out another particle and this is called autoionization. And the same happens in electronic physics.

The formulation as I said for these processes came first from Fano. Fano himself reformulated it in 1961,⁴⁾ and several nuclear physicists copied this method, and use it for problems which are suitable, but just for pedagogical reasons, you will see. One can do the whole problem also exactly.

Now in this autoionization problem you have a bound state embedded in the continuum. We have at Z_{crit} a bound state, and if we increase Z by one unit or more, we get a bound state embedded in the continuum. And we formulate now our problem in the following way. Let me formulate it with the Dirac Hamiltonian $H(Z)$, where $Z = Z_{\text{crit}} + Z'$. If I have this Z' , I can obviously split up my Hamiltonian into an $H_0(Z_{\text{cr}})$ plus an additional charge, Z' , times an excess potential $U(r)$. Now, what is the excess potential? The potential itself is proportional to Z , as you have seen earlier. Now for $U(r)$ I may use a rounded-off $1/r$ potential, and then I can write $H(Z)$ as $H_0(Z_{\text{cr}})$ plus $Z' U(r)$, because the Z essentially enters only in the $U(r)$.

$$Z = Z_{\text{crit}} + Z'$$

$$H(Z) = H_0(Z_{\text{cr}}) + Z' U(r)$$

Now the idea is the following. I do not know the solutions of $H(Z)$, but I do however know the solutions of $H_0(Z)$, because that is still the case when I see the problem quite clearly as bound state, lower continuum, and upper continuum. So, what I propose to do is this: diagonalize the total Hamiltonian, $H(Z)$, in the basis of H_0 . This is a little bit of mathematics I am going to do now, but it's very simple, and it is only for pedagogical reasons that I make a few approximations. Later I will show exact results, and I also will tell how to get exact results.

Now I draw a picture once more to remind you how the situation looks.

I have $-mc^2$, and my bound state near it, shortly before Z_{crit} . And of course I have other s states, and I have an upper s continuum and a lower s continuum for the Dirac equation. I call the solutions ϕ , the unperturbed solutions which I consider now, and this lower continuum ψ_E . I can give exact results of this mixture of bound state and continuum states. In that case, I have a finite number of bound states and upper and lower continuum to treat at the same time. But this exact result is formally very involved, and you do not learn so much about it.

You learn most of it if I restrict myself as a basis just to take this one bound state into account together with this lower continuum. That is a first approximation in the sense that I consider only one bound state and the lower continuum as the basis states, and diagonalize the Hamiltonian neglecting the rest. We could treat the rest also, and I will later show the exact results.

If we do this, we have an eigenstate ϕ of H_0 , and its energy is nearly $-mc^2$. $H_0\phi = E_0\phi$. And then we have $H_0\psi_E = E\psi_E$, where E is less than $-mc^2$. Now, for this one bound state and this continuum, I diagonalize the total Hamiltonian. How do we do this? For that purpose, I need to calculate the following matrix elements: the diagonal one, $\langle\phi|H|\phi\rangle$,

and the off-diagonal one $\langle \psi_{E'} | H | \psi_{E''} \rangle$.

Now let's look at the first matrix element. H is always $H_0 + Z'U(r)$, so we get out that

$$\langle \phi | H | \phi \rangle = E_0 + \Delta E_0.$$

And you see immediately that ΔE_0 is Z' times the expectation value of this function $U(r)$, and is of the order of about 30 keV.

$$E_0 = Z' \langle \phi | U(r) | \phi \rangle \sim 30 \text{ keV}.$$

Now, here in the continuum matrix elements, if I take the H_0 part of the Hamiltonian, because it's an eigenstate of ψ_E , its contribution vanishes. So this gives zero, but the other part is proportional to Z' and gives non-zero matrix elements. So we get a matrix element which I call V_E ; it's dependent upon the energy E . And it is proportional to Z' , to the excess charge.

$$\langle \phi | H | \psi_E \rangle = V_E \sim Z'$$

I now calculate this matrix element for the continuum-continuum interaction. H_0 obviously gives the energy, and because these continuum wave functions are normalized to delta functions, we obviously get out, E' times a delta function, $E' \delta(E' - E'')$ plus something, which is proportional to Z' , and I call it $U_{E'E''}$.

$$\langle \psi_{E'} | H | \psi_{E''} \rangle = E' \delta(E' - E'') + U_{E'E''}$$

Now this $U_{E'E''}$ is a continuum-continuum rearrangement matrix, and it makes life difficult. Usually one makes the argument that one may re-diagonalize the continuum and use such continuum wave functions which contain this in a diagonal form. Nobody can do that. Honest people say just we neglect this! This is what we do, and since I will give another method which leads to the exact solution later on, we easily can afford to do that.

Now this problem can be solved exactly. The solutions that come

out of such a procedure look like the following. Due to the joining of that bound state level into the lower continuum, you rearrange the continuum, and out of the old continuum comes a new continuum, which is distorted, so to speak by the bound state, or bound states, which have entered it. And this old continuum is a combination of that old state ϕ , the bound state, plus an integral over the old continuum, (The E' belongs always to the old continuum.).

$$\psi_E = a(E)\phi + \int b_{EE'} \psi_{E'} dE' .$$

That's nothing else than a completeness relation or expansion of the state. The point is that I can use these coefficients $a(E)$. And what interests us most is, for example, $|a(E)|^2$, which has the following form, involving the continuum matrix element, V_E :

$$a(E)^2 = \frac{|V_E|^2}{[E - (E_0 + \Delta E_0 + F(E))]^2 + \pi^2 |V_E|^4}$$

The $F(E)$ term is a principal value integral, given by

$$F(E) = -P \int_{-mc^2}^{\infty} \frac{|V_{E'}|^2}{E-E'} dE'$$

This is an exact expression, independent of what the V_E or so on are.

Recognize what the $V(E)$ is; the $V(E)$ was this bound-state-continuum matrix element. Is this $V(E)$ a rather smooth function? This integral goes, as you can imagine, over the entire lower continuum. If $V(E)$ is a smooth function then $F(E)$ is approximately zero. It is not exactly zero. And its effect again I will study later. One could worry about what happens to this principal value integral if E goes to that branch cut, if it goes close to $-mc^2$. But again there one can show, and this is really involved and I refer to the literature of my collaborators, Johann Rafelski and Berndt Müller,⁵⁾ that these matrix elements all vanish in the limit where E goes to the branch cut, to $-mc^2$.

X

Question: This term gives you something that looks like the spreading of a width of this resonance state...

Answer: Completely; just a minute, I come to it.

Now you see, when I neglect this $F(E)$ for a moment, then $|a(E)|^2$ resembles a Breit-Wigner formula. If one introduces a width $\Gamma_E = 2\pi|V_E|^2$, then you see that you get

$$a(E) = \frac{\Gamma_E/2\pi}{[E - (E + \Delta E_0)^2] + \Gamma_E^2/4}$$

This width is energy dependent, and in particular because the matrix element V_E is proportional to Z' , the width is proportional to $(Z')^2$ because the width is proportional to the matrix element squared.

Now, how can we interpret these results? We distinguish two cases. Case number one is that we have the K-hole. In the case of the K-hole, this was all single-particle Dirac equation, and to interpret the results we have to come back now to field theory. That is, we fill up that negative sea, and we have the infinite many-body problem again. And in the case of the K-hole, we have the following situation now. The bound state as a function of Z behaves like this, crossing before Z critical:

We have an excess charge in it, and the bound state obviously has distorted the lower continuum. Now where is it lying? The maximum of the bound state distribution lies obviously at the energy $E + \Delta E_0$. And ΔE_0 was $Z' \langle \phi | U(r) | \phi \rangle$. It goes linearly, that is proportional to Z' , diving into the lower continuum. At the same time, it gets a distribution; it is distributed over the lower continuum, as Pat Richard suggested. And it is distributed by this width Γ_E which is proportional to $(Z')^2$. So it's zero for light atoms, and then, since the diving is proportional to Z' , it goes quadratic with the diving distance.

Now if you have a K-hole, and you have these normal continuum states all occupied, and this is a distribution of that hole, then obviously this K-hole width is nothing else than the decay probability for creating electron-positron pairs spontaneously, without energy, in an energy-less creation of electron-positron pairs. I will now write down the field-theoretical formulation or justification. A K-hole state is a state in the s shell where you can have electrons with spin up, or down, and this is identical with the state ϕ . And then you may get, due to the excess matrix elements, two electrons; that is, two electrons are now up in the bound state, but only one positron, $\vec{S}_1 E$ -configuration. This matrix element is

$$\langle a_{1s+}^+ a_{1s+}^+ \vec{S}_1 E | a_{1s+}^+ | 0 \rangle$$

when you calculate in a field theoretical way and put in the annihilation and creation operators. And from this it's clear that we have now a two-electron configuration made out of a one-electron configuration; a two-electron, positron configuration. And that's a creation of an energy-less (because they are of the same energy) electron-positron pair. So, the positron is bound in the p state, then the electron is bound in the p state, and the positron escapes. This width increases quite rapidly, the deeper we go into the lower continuum.

Question: You have a K-hole that has a large negative binding energy. This is filled by one of the negative energy electrons, leaving, I thought, a negative energy positron, a bound positron now. Why does it

escape? What are the energetics here?

Answer: In order to make this more transparent, remember what normal electron-positron pair-production is. It is a pair production in which you excite an electron from down here going up there, and in this way you create an electron hole. And if you have an empty K-shell, you need energy $2mc^2$. That's why one usually says, pair production, $2mc^2$. That's not completely correct, because when you have an atom with a K-hole you need less energy. You can already bring the electron in that bound state. And now this energy becomes less and less, and from Z critical on it's energyless. And from there on it dives. That's so to say a new threshold opens a complete new channel where really nobody, up to our knowledge, has thought about it.

Question: In your model, the electron does not fill that K-hole. You see there's another mode. The electron could come from an upper level.

Answer: You mean an electron from up...

Question: L-shell, say.

Answer: From up here, and make a radiative transition down...

Question: Except that it only happens when the difference in the binding energy is greater than $2mc^2$, rather than when just the binding energy is greater than...

Answer: Then you have a K-hole, of course; these K-holes can never escape. They only make more decays that are fed by an electron from above. That is, you have a p-s transition. This transition probability, or this typical transition time is 10^{-17} sec. And when we discuss the experiments here, I will come back to that question, because this is essential whether a K-hole can be annihilated either by this channel, which is a radiative channel, or by this autoionization channel.

Question: Does it have to be a radiative channel though? Can't the electron come from up above, and you still have this?

Answer: You mean that you have a free electron here, and the electron can come from above.

Question: Let's say you have an electron in the L-shell...

Answer: Then you make such a transition, but that's a radiative transition.

Question: What I'm saying is that if you have enough energy, it

doesn't have to radiate. Why can't it just produce a pair?

Answer: Where does the energy come from?

Question: The energy is coming from the upper electron going down to the K-shell...

Answer: Where does the energy go?

Question: Into a pair?

Answer: No. Because if the electron springs from a level up here down to here, this energy is not sufficient to create a normal pair.

Question: Well that's what I'm saying. If the difference in binding energy is greater than $2mc^2$, then you can...

Answer: If the difference in 2p-1s transition energy is larger than $2mc^2$, you could make a normal pair; you would have an additional process for making a normal pair. But this process is by a factor one-millionth smaller than the process of autoionization. This is clear because here you remove no energy at all. That's a spontaneous process; that goes out right away. I'll give you the precise recipe a little later.

Question: It just seemed like such a natural thing to happen...

Answer: Well, except that it doesn't happen here, because just to make it clear a little better, the 2p-1s transition energies are still of the order of 300 to 500 keV; not yet 1 MeV.

Question: So you go pretty high to see...

Answer: Oh yes, you have to go to Z equals 220, or 240 perhaps or something like that.

This critical number I should say is about 170. One doesn't know it too precisely, by one or two units, because of the non-precise knowledge of the charge distribution of this nucleus.

We come now to the second case, namely K-shell filled. In this case we obviously have the following situation, that when we come across Z_{crit} , then the bound state which you had before Z_{crit} has not joined the lower continuum, and is distributed so that one state more is in the lower continuum, and it is also occupied with electrons. Every state is completely occupied. But this bound state of course is distributed. It has a spreading width as we say in nuclear physics, and is distributed over this lower continuum.

The interpretation of this is quite clear. I will immediately show that this amounts to real vacuum polarization, in contrast to the vacuum polarization we have known up to now, which is virtual vacuum polarization. I will explain this language right away. But one thing is surprising, and one has to understand perhaps this too; namely, that why this gamma width is precisely this hole escape width. Well, this must be like that, because consider the following Gedanken experiment: this is an effect which in principle can be measured, because it says that the K-shell is now never a sharp shell, that the K-shell is distributed energetically, and has obtained a width. Now in order to measure that, you will say, I irradiate with various gamma energies through that resonance. And I even make it a transition like this, absorption from a K-shell into a p-shell, and normally you would see a sharp line, except for the radiative width. In this case you see a broad line because of this spreading. Now it is clear, because your final state is a particle-hole state where the hole, the positron down here, can escape again. And this escape is much more probable than that width. That's why the spreading width must equal the escape width in the first case of our discussion. So, you see the whole thing is quite consistent.

I would like to discuss now the problem of vacuum polarization again. We have learned so far that the lower continuum, due to this joining of the K-shell, is dramatically distorted. The K-shell spreads; it is no longer sharp. The K-hole escapes as a positron, and the width can be differently populated. It corresponds to transition times for $Z = 184$ of the order of about 10^{-19} seconds.

Now comes the next step. Namely, what is this vacuum polarization? Let me first explain why this problem is so important, and what vacuum polarization means. We have learned that this lower continuum is occupied. Now clearly when you have a free field case you have such an occupied lower continuum, and when you apply now an external field, like Coulomb field, you distort all the wave functions. Normally these wave functions are plane waves, in the free field case. I plot now - I have the center here - you distort them a little bit, like that.

I exaggerate. Because the square of the wave function is the probability, you somehow redistribute the charge in those lower states, and this redistribution is a polarization charge. And this polarization charge obviously is the following:

$$\rho_{\text{polarization}}(\mathbf{r}) = -\frac{1}{2}e \left(\int_{-E} \bar{\psi}_{-E} \psi_{-E} dE - \int_{+E} \bar{\psi}_{+E} \psi_{+E} dE \right)$$

where ρ equals the integral over all negative energy states ψ_E , integrated over dE . Of course this first part of the integral is infinite, because you integrate over all these states and you have an infinite charge. That's why one subtracts from it the same quantity for the free field case, and then you get the polarization charge out.

Now, I cannot show this in detail, but it amounts to the same as if you would subtract the unoccupied energy states for the positive charge. So, the charge operator, the so-called proper-charge operator in field theory, is defined as the integral over the negative energy states minus the same integral over the positive energy states, also belonging to $Z(\mathbf{r})$.

You see in the free field case, where Z equals zero, this is precisely zero because you have a vacuum in the upper continuum and plane wave solutions, and the same for the lower continuum, and because of symmetry reasons they cancel.

Now, when you have a Z in the system, they don't cancel precisely anymore; there is something left over. But the integral over ρ_{polariz} is zero. That is, it's a polarization charge. And this is called the vacuum polarization charge. We call it therefore vacuum polarization; the name is clear, because you redistribute the vacuum. And one normally forgets where this comes from. Normally you draw graphs, and when you ask a good graduate student why such a graph here is called vacuum polarization, he usually gives up. But the reason is that the very original definition in fact comes precisely out of that which I just explained.

And now comes the problem. This integral is infinite, still, even though one would think it should be finite. And this leads into the so-called renormalization problems which I do not want to enter. All the renormalizations enter in such a way that one expanded these wave functions, which belong to Z , in a power series in $(\alpha Z)^2$. And one could only renormalize in this expansion. In our case, however, αZ is larger than one, it's 1.3. So you cannot expand anymore, and you have to think about completely new techniques, how to treat field renormalization in these over critical fields. These techniques have been worked out by Reinhard,⁶⁾ who essentially reformulated the Schwinger equations, which one thinks describe the quantum electrodynamics correctly, into a kind of a Hartree-Fock procedure, including the lower and the upper continuum at the same time, and adjusts the bound levels as in standard Hartree-Fock procedure. But this is a very involved thing, and I don't want to talk about it.

Now why does one need to study this? Well, the reason is this, you see. It was in 1968 when Pieper and I said,²⁾ positrons must come out. Not yet in this elegant form as we present it today, but the essential physical ideas were there. But then we were not sure for about a year or two whether this vacuum polarization cannot prevent this diving. In other words, whether this polarization charge is not rearranged in such a way that you get a bending over of the levels. This is a very essential problem. Thus, do the levels dive, or do they bend over, because of that tremendously

strong vacuum polarization? Now, I think I know the answer to this problem, even though I cannot calculate yet this quantity. And the arguments go as follows.

Consider that you have a conductor, and you apply to the conductor an external field. The vacuum polarization just says you will set up a counter field in the vacuum. You are redistributing the charge. Now the maximum of vacuum polarization I can think of is the following, that if I plot here the external field and here the effective electric field, that for small fields they are precisely equal. Clearly, vacuum polarization for the small fields we have in the laboratory doesn't matter at all. But when the fields may become very strong, the strongest effect I can imagine is this, that the effective field and the counter field cancel, and you are led into a kind of a saturation. This is maximum vacuum polarization. It cannot be stronger than this; it cannot be a back-bending curve.

This would be against any physical sense. So, if we agree on that point, let me call such a limiting field E_0 , and if I can show that for such limiting field electrodynamics, that is, electrodynamics which have an upper limit to electric fields in nature built into itself, this diving problem is still there, and then we are out of that question. Then we have solved it.

After this question became clear we came back to these very early considerations of limiting field electrodynamics by Born and Infeld.⁷⁾

Limiting field electrodynamics is an electrodynamics which has no infinite self-energies anymore, like a point charge. Point charges do not exist in such electrodynamics. You will immediately see why. And they studied this in the early thirties, and they studied the following type of Lagrangian. You see, I first write it down and then explain it. E_0 is always the limiting field and E the electric field, and then B is the magnetic field,

$$\mathcal{L} = E_0^2 \left[\sqrt{1 - \frac{(E^2 - B^2)}{E_0^2}} - 1 \right]$$

This Lagrangian is constructed in analogy to the free field Lagrangian in which you have for a relativistic particle,

$$\mathcal{L}_{\text{rel}} = mc^2 \left[\sqrt{1 - \frac{v^2}{c^2}} - 1 \right]$$

where c is the limit to all velocities, as you know. And in the same way E_0 is the limit to all electric fields. Well, if we have static fields, B is zero, and then we are left with such an expression that is actually a field density.

And now when you study this type of Lagrangian, as a special case of a whole class of other limiting field Lagrangians, then you find that the Coulomb potentials are changed. For example, when you write the Lagrangian equation and you solve your point problem, you get a modified Coulomb potential. For a point charge, the Coulomb potential goes like $1/r$, and at that moment when it reaches the limiting field (the gradient becomes equal to E_0) then from there on is linear, like this:

You see, it never becomes infinite like in ordinary electrodynamics. How does one determine E_0 ? Born and Infeld said, well, because now our energies are all finite, let's determine E_0 in this way. We calculate the four-four component of the energy-momentum tensor with this energy, the Hamiltonian, and require that this is equal to mc^2 , the self-energy of the electron.

$$E_0 = \int T_{44} dx^3 = mc^2$$

In this way they found an E_0 of about 10^{18} volts per centimeter. Now, these fields are much too low; (high E_0 field means a small effect; low E_0 field means strong effect.) They are much too low compared to present day physics. We would come in trouble with such E_0 in the Lamb shift, and with high precision measurements of K binding energies of electrons in Fermium, as Mr. Freedman at Argonne⁸⁾ has pointed out, when measuring them very accurately to about 20 eV. And we even more come in trouble in muonic atoms.

But you can turn the process around. You can say, how low can we choose our E_0 that we do not come in trouble with present day physics. And the result of this analysis, which has been done very carefully in his diploma thesis by Mr. Soff⁹⁾ is the following, that you have to choose E_0 larger or equal to about 5×10^{19} volt per centimeter. It is by a factor 50 larger.

Question: What does that do to the constraint on your integral

over T_{44} ?

Answer: Oh, it becomes larger, of course. There must be other energies which are of negative origin, binding energies which pull back to the electron mass in the total fields.

Question: Does that make sense?

Answer: Sure, in weak interactions.

And now, the point is this. When we use such an E_0 strength of this second kind, then for any limiting field, the diving is not prevented, it is only shifted. In the case of Z_{crit} if we would choose a Born-Infeld field, it would be shifted to about $Z = 215$ instead of 170. In the case of choosing a form of field which is not in contradiction with present day physics, the shift is of about one to two units so that 171 or 172 is the diving point.

But I think you also can understand in very simple terms that limiting field Lagrangians like this can shift but cannot prevent diving. The reason is that when you start out with a $1/r$ potential at low Z , and at the moment when you reach the critical E_0 field strength, you end up with deeper potentials of high Z and deeper potentials mean deeper bound states, and that means diving. That is, by increasing Z you dive. And so we learn out of this consideration two things. First, the diving cannot be prevented by vacuum polarization, except in the case if I would construct potentials which couldn't become deeper than $-mc^2$. But such potentials are ridiculous, because they are first not gauge invariant, and second, you already have a 5 MV tandem in the house - right? So this would mean that you could never get in one step a voltage larger than 1 MV, and that's ridiculous. This settles these questions, but of course we learn from this that if we study these various points here, that we also can learn about non-linear behavior of quantum electrodynamics.

I come now to the next to last point, and discuss a possible experiment. Basic to all experiments is the following thing. When we have a heavy-ion scattering between two ions, and arrange the heavy ion energy always such that we just reach the Coulomb barrier, that the ions touch, then the ion velocity is approximately independent of the ions. It is about a twentieth of the velocity of light. Now when you have such deep

binding energies of one MeV, and deeper, then you have an electronic velocity of the order of the velocity of light; it's $0.99c$. This means you have a typical adiabaticity problem in the sense that during a collision time, the electron can circle around about ten to twenty times, and you have a typical situation for superheavy molecules. This now is clear-cut prediction, independent of any quantum electrodynamics or what, it's just simple electronic motion of these molecules. They should set up an intermediate molecule and as you here know better than I do, there are first proofs or hints that this molecule might exist by Mokler and Armbruster¹⁰⁾ and by Saris.¹¹⁾ Mokler and Armbruster picked up this idea in nuclear isomers and Saris I think also. And they seem to have found in the bombardment of iodine on thorium molecular lines which seem to correspond to M-shell transitions of the intermediate molecule. I do not say more about this, because you know it better than I do, and you also know that this is not yet a proof or either a disproof, but one has to proceed and see more lines and get more information about these things, in order to be sure.

Question: In the superheavies, what are these K-shell radii?

Answer: When you have Z of 172 or 184, the maximum of the radial density of the K-shell is roughly three times larger than the compound nucleus radius. If you have the two ions colliding, then the quantitative number in the uranium-uranium case, if they just touch, is about 14 fermis, and the K-shell radius lies at about 22 to 25 fermis. It's just outside.

Question: What happens if you replace that by a muon?

Answer: I'm looking for K-shell ionization; and of course the muonic shells are completely ionized. That was the first idea, to go right away for muons. And there must be a lot of antimuon production up to this point. But the trouble is that because of the large muon mass, as the muon is from the beginning inside the nucleus, the depth of rounded off nuclear potential doesn't change too quickly. And then of course you get this minus 210 MeV, and the diving point in the muonic case is $Z = 2000$! A fantastic idea, but unfortunately we cannot make these targets.

There are three kinds of experiments now. The first one I already mentioned, the gamma-ray transitions, which should be observed in these

heavy-ion collisions. Since the change in the s-shell is much bigger than the $p_{3/2}$ shell, for example, ($p_{1/2}$ just stays constant all the time) you will find that when you do such a collision and you observe gamma rays in coincidence with the scattered heavy ion, you find, for the gamma energy plotted with the cross section:

You always get a sharp peak, which corresponds to a transition at the point of closest approach. The reason is two-fold. First the transition probability is largest there, because the energy is largest. Second, when you fold it with the heavy-ion path at the distance of closest approach, the time where they are closest is longest because the velocity has gone down to zero. And that's why it is much, much sharper, this continuous spectrum, than a simple collision time. A simple collision time would say, well you would spread out the whole thing over all energies. And it would not be sharper by a factor of 10.

The second experiment is of course due to these molecular binding energies. They change considerably when you go to very heavy ions, like uranium on uranium. You get at close distances binding energies of the order of 6 to 8 MeV from all electrons. Most of it comes from the deep shells. The K-shell already has 1.5 MeV binding. Two electrons there make already 3 MeV, and then you add up the other shells, and you immediately see that you have of the order of 6 to 8 MeV. Therefore you get deviations from Rutherford scattering. I would expect them in the order of one percent.

When you have a normal Rutherford scattering and you have now a molecule set up, the molecule tries to prevent the ion going around, and therefore destroys the angular distribution. So this is the second type of experiment, to study Rutherford scattering carefully at energies where you come close enough that this molecule is set up, and you will see deviations.

The third and most important thing is now, of course, the positron escape. In such a heavy-ion collision now, the K vacancies are created in this intermediate molecule. You have a two-center molecule and then you form an ion as we discussed for nuclear physics. The distance between the ions is R , and r is the electron distance, and the potential is $V(R, r)$.

Of course, R is time-dependent because the ions pass by on a hyperbola. This is the electronic potential. If you Fourier expand this whole thing,

$$V(R(t)) = \int V(r, \omega) e^{i\omega t} d\omega$$

you get a whole Fourier spectrum, and let me plot it. For a typical Rutherford scattering such a Fourier spectrum looks like this:

It goes very, very fast, nearly exponentially at low frequencies. And the typical frequency, say electronic frequencies for the unbound system, these small frequencies of the individual centers, lie low. And of course you have also very high frequencies, but with a much smaller amplitude, in the system. And these high frequencies can make direct electron-positron pair production, not due to the nonadiabaticity at closest approach. I am discussing now nonadiabatic effects, very general processes. And, the point is, you see, for the problem to be completely adiabatic you would get a delta function distribution. And then we get a contribution at large ω if it is very nonadiabatic. Adiabaticity means always, what frequencies do we have in the system, with respect to the frequencies we bump in? I told you that now because of these very small amplitudes, you get direct electron-positron pair production. Now these positrons go out right away. And they serve, so to say, on the collision path as a kind of a background all the time. It is, when you put in numbers, of the order 10^{-4} smaller than the autoionization process. But probably that is a very crude estimate and it is even less than that.

Now comes the amusing process. You see, you also create this ionization, because of the larger amplitude with the smaller frequencies here, and this makes K vacancies. Now the problem is, are these fed from above?

These near the continuum are filled right away, and these lower down - fortunately they stay, because the decay time from above is 10^{-17} seconds, and the collision time is 10^{-19} or 10^{-20} seconds, and you see if a K hole is created it stays in the collision. On the way you bump the probability into that K orbit until you come down here, and then the valve opens, and the probability goes out. So, when you study the recoils, the escape opportunity and cross sections come out according to the formulas we derived here, with the Rutherford hyperbola.

Let me mention one more point, which I nearly forgot. And I think this is very essential. I have written down here the vacuum polarization operator. Of course, one can solve this and I do not go into the details. Normally when you solve the continuum down here directly by solving for the phase shifts, you find that you get a regular and an irregular solution. When you are beyond 137 the two independent solutions are regular. That was a long struggle in our institute until we really got that point clear. And then you can solve for the phase shifts directly and actually calculate these wave functions directly, not relying on any approximate scheme like the Fano scheme. And when you do this, you can calculate this integral for $V(R(t))$. And then you find it's no longer zero, but it's now two, or one. That is, you have the K-shell embedded in the continuum, and you get precisely the K- and the P-shell densities and all that out. That is, what I am talking about is now real vacuum polarization, real in the sense that it is real because there is a real K-shell or a real P-shell or whatever shell has dived into the continuum and deformed it. And this is another qualitatively new aspect which occurs. That's not virtual vacuum polarization anymore, which is also present, but there is a real one now.

The quantitative picture is shown now in Fig. 1 as a function of Z ; the s-levels and the p-levels are also shown. You see that the $p_{1/2}$ level dives also quite quickly, but not the $p_{3/2}$; that stays rather constant. And that is why I meant that for example the transition energy is here increased dramatically, and leads to a very sharp distribution. In the case of positron escape, we get it also, because the escape width increases quadratically with diving distance. It's the same type of argument. But you see also that the p-shell dives (the $p_{1/2}$ -shell) at 184. That is, with a uranium on Californium collision, you can also perhaps

Fig. 1

observe that. The 2s shell dives later.

In Fig. 2 I show once more that for an under-critical field, we

Fig. 2

still have the bound states there. For an over-critical field the bound state has joined the continuum and distributed over it. It enriches the continuum states by one state. And that's typical of what we call spreading width in nuclear physics. But elementary particle physics you see, they

have not heard about it. It's very hard to get that word across!

Fig. 3 is the first Dirac two-center calculation, made for

Fig. 3

extended charges. At a distance R from each other the Dirac equation has been solved by Mr. Müller.¹²⁾ It is for the lowest orbitals, for the $1s\sigma$ and $2p_{1/2}\sigma$ shell, and asymptotically these two shells merge together into the double K-shell, for identical ions. And here you see precisely at a distance of about 60 fermis these shells dive. A little closer, the p shells dive and Mr. Müller has set up the code, and I think in about a week or two weeks - I don't know how it takes now to calculate the full spectra, I'm sure you will see the same pictures then as I showed you yesterday and the day before that in the two-center-nuclear model, now for the two-center Dirac model, which is very important for all of your studies you are doing, and these studies here too.

By solving this lower continuum exactly we can now make up for all the mistakes we made on the first graph. (See Fig. 4) And this is linear diving, and when I incorporate this principal value integral we get quadratic deviations like this here, which makes the diving even steeper.

Fig. 5 is this complete resonance deduced out of an exact calculation. You see, it is quite symmetric, and this is exact, and is very sharp and clear-cut resonance deduced out of this exact calculation of the

Fig. 4

Fig. 5

lower continuum.

In Fig. 6 are the wave functions. The density distributions of these shells after diving are deduced from this operator, by inserting here the exact wave functions for the lower continuum. And you see, just before diving we have the s-shell. It is the $Z = 172$ shell, and it's a typical s-shell distribution. Now when you go after diving, say 184, you get a similar distribution still. The embedded K-shell spreads it actually,

Fig. 6

but the integral over it gives again the full K-shell, and it's there, but spread out, and looks like the K-shell.

And you see, also, what is even more amusing, the $2s_{1/2}$ shell, when one goes to $Z = 255$, it also dives. It can be deduced in the same way, and you get the typical $2s_{1/2}$ distribution. One question, why isn't this going to zero? This is a typical relativistic effect due to the upper and lower component of the spin.

And here is the dynamic picture shown (Fig. 7), for before the collision, at closest approach, and after collision. Here you see how the K-shell dives in and out again. The small circle indicates that you have created a hole in the K-shell around that path. Now this hole dives in, and the electrons come over and jump into it, and the positrons go out. At this deepest point most of them come out, because here the escape width is largest and the N-shell hole stays at this distance for the longest time, so to speak. The velocity has decreased to zero at the turning point. And on the way out, of course, still a few positrons come out.

Here is the positron energy in Fig. 8, and here is the differential cross section, that an ion is scattered into a solid angle $\Delta\Omega$. The positron's energy you can readily read. L_0 is the probability of emission K holes present, and $d\sigma_R/dr$ is the Rutherford cross section. These are

Fig. 7

Fig. 8

thorium-thorium, uranium-uranium, and Californium-Californium collisions. They are very sharp peaks, the same as we would expect for the gamma rays as well. And now, if you integrate over all of the Rutherford cross section, then because the Rutherford cross section is so large in forward direction, you get most of the positrons. This would be a coincidence experiment. In a non-coincidence experiment, where you don't ask where the ion is scattered, you would get these distributions out. Now, what is the magnitude? We insert here $1/100$ for L_0 , that is one K-hole in a hundred collisions. That is why I am so eager to get such numbers from you. You get here an integrated cross section, which means essentially the cross section under the peak, of the order of 10^{-29} cm^2 , in the coincidence experiment; for the differential cross section, a maximum of 10^{-27} cm^2 . These are cross sections which one still can lower by a factor of one hundred or so and still have measurable effects. But the K-hole probability is directly proportional to L_0 , and that's very essential. It is why I think your experiments are so essential in this connection because one needs to know these numbers carefully. This number may depend both on Z and on the impact parameter. That is, on the Rutherford scattering angles themselves, and all this has to be deduced in order to refine this calculation.

Well, I should mention one more point, gentlemen, and this is what I said in the beginning. Namely, in the beginning I told you that the fine structure formula ceases to have such a perpendicular behavior, and you don't get solutions for Z of 137. Now when you take an extended nucleus you postpone the problem; we understood very well what was happening here. Now if I do the same thing and decrease this nucleus, successively, and define in that way a point charge, make the nucleus smaller and smaller, what's happening is that this value becomes more and more perpendicular, and here precisely perpendicular. This was so to say already foreshadowed in the fine structure formula, only one didn't understand it. That is, if we could make superheavy point-like nuclei, that would be fantastic wouldn't it, because, you see, you would gain in a small distance, infinitely small distance here, infinite binding energies. And I don't know whether this has some meaning. I am pretty sure that this steep decrease of binding energy, at small energies, has a lot of consequences in the uranium-uranium collision because it may lead to quasi-stable intermediate superheavy nuclei.

molecules. That is, lead to a stabilization of a molecular uranium-uranium system of the order of about milliseconds. The reason is that the $1/r$ potential goes like this, then as soon as the ions overlap the nuclear force helps flattening off, and then the electronic binding does the rest, of about 3-4 MeV. With this type of speculations, I want to finish.

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COULOMB-INDUCED AND TRIPLE FISSION

I thought that I would talk today a little bit on outlooks in heavy-ion physics. That is, not only on Coulomb fission, which I consider as one of the exciting problems which comes up there, but also a little bit on super-heavy elements, and especially on triple fission, another qualitatively new aspect in the physics of very heavy elements. I think this suits better a kind of a farewell speech than if I just stick to one topic alone.

"Coulomb fission" means that during a heavy-ion collision one or both of the nuclei - normally the target nucleus - is split up into two parts because of the high excitation energy induced by the time-dependent Coulomb field.

In the center-of-mass system it always looks like this. Now, this is the process which I want to formulate a little bit more quantitatively today. It was first suggested by Willets, Guth and Tenn¹⁾ a few years ago. Coulomb fission has not been observed up till now, since we only have light ions available and we do not have the energy to bring them close enough together so that they can disrupt each other.

Now, what can happen in such a heavy-ion collision? One has to

think, when one studies such processes, a little bit more about the dynamics of the system. The nuclei may, for example, deform on their orbit. There was this old model, according to which the Coulomb barrier should be raised in heavy-ion collisions compared to the classical figure.

Classically one would assume that the Coulomb barrier E_c is given by the Coulomb potential, evaluated at that point where the nuclei just touch. (For simplicity one may take two spheres with homogenous charge of distribution.) Now, early it was thought, that this barrier is raised in heavy-ion collisions. That was a very essential question because this rising was quite appreciable and it had to do with the maximum energy one gave the heavy-ion machines which are under construction.

The reason why one believed this is the following. While the two ions approach, they may deform, and then of course the energy is lowered everywhere (see the dotted line in the figure).

On the other hand, compared to the spherical case, the ions can come now closer together, because the distance for just touching is less. And therefore you have an effectively higher Coulomb barrier (E_c') than originally.

This conclusion however was wrong. The falseness of this lay in the dynamics, partly, and partly also in the nuclear force. From the dynamical calculation it follows - and I will show this a little bit more detailed - that the vibration which is excited in the target has not time enough to elongate to its full amplitude. It just gets hit and tries just to breathe out, so to speak, and wants to expand into such a dimension, but the elongation at the distance of closest approach is nearly negligible. To make it very definite, when I have for example two deformed nuclei colliding parallel to each other, the Coulomb barrier would be very low. But if they would turn into the crossed position along the heavy-ion path, the Coulomb barrier would be very high.

The question is now: How much do two ions, which come along like this, turn around in a heavy-ion collision, in their approaching path and in the leaving path? And the answer to that problem is: Five degrees, roughly, if you take a typical nucleus. It's not very much.

You may ask then, where the Coulomb excitation is coming from. Coulomb excitation of rotational levels just means a turning around of the nucleus. That's what you measure when you have a nucleus in a 2^+ or 4^+

state. And the mechanism is the following: The nucleus turns around by a few degrees, five to seven degrees, goes by, and then in the final path, while leaving the target nucleus, the energy is all pumped into the system.

Now, how does one study such a scission? If you want to do it in the most fundamental models (the shell models are called fundamental; they are not, in fact, as you have seen), you never can describe it. You calculate for fifteen years, and the problem is out of date. But in such cases you always assume the most quantitative models we have available at present; these are the collective models. So you work out the Coulomb energy, essentially, for the relative system, and then you use the collective Hamiltonian describing the surface vibrations of your individual nuclei in the following way.

$$H_{\text{coll}} = T(R) + V_{\text{Coul}}(R) + H_1(\alpha_1) + H_2(\alpha_2) + H_{\text{Coul int}}(R, \alpha, \dots)$$

where α_1 and α_2 are collective coordinates and H_{int} is the interaction between R and the alphas. This is a collective Hamiltonian, straightforward to calculate.

Now these collective Hamiltonians, say for a vibrating nucleus, introduce standard surface vibrations. Simply expand the nuclear radius in the spherical harmonics $R = R_0(1 + \alpha_{2\mu}^* Y_{2\mu})$, where I have limited myself to quadrupoles. One can consider these expansion parameters to describe the deformation of the shape of the nucleus as generalized coordinates. One deduces from the properties that the radius has to be invariant under rotations of the frame, and so on, that the alphas have to transform under the rotational matrices U_2 . That means that the elementary quanta, again, carry a spin angular momentum two. This is why phonons carry an angular momentum two in this type of model.

You formulate then the intrinsic Hamiltonian. The simplest one is obviously of this type:

$$H_1 = \sum_{\mu} \frac{1}{2} B \dot{\alpha}_{2\mu}^* \dot{\alpha}_{2\mu} + V(\alpha_{2\mu})$$

where $\dot{\alpha}_{2\mu}$ are the generalized velocities. The potential has again to have a very general structure. Because of rotational invariance it must be of

the following form. Since these alphas carry an angular momentum $2\hbar$ you can never construct a rotational invariant potential out of one alpha. You must have at least two; so you couple two of the alphas to an angular momentum zero. This is how you construct rotational invariants.

Explicitly written this would be a Clebsch-Gordon coefficient coupling two alphas to an angular momentum zero. Then you have coefficients like this, where you couple three alphas to zero, and you make a power series expansion, up to how many alphas you want, say α^8 , and so on.

$$V(\alpha_{2\mu}) = \frac{1}{2}C(N,Z) [\alpha_{2\mu} \times \alpha_{2\mu}]^{(0)} + C_3 [\alpha \times \alpha \times \alpha]^{(0)} \\ + \dots + C_6 [\alpha^8]^{(0)} + \dots$$

Now these constants are in general of course dependent on N and Z, and these one can calculate. Usually when one calculates such a collective potential energy surface which I have shown you, one can deduce these constants from there, and has in this way obtained the invariant expansion of these collective potentials. But this relates now, just because of geometrical reasons, to the collective structure itself. And the rest is so to say structure constants, which one has to introduce, and which are determined by deeper models like the shell model.

Now in this way one can always fix this Hamiltonian. One can of course calculate higher terms of the kinetic energy. This is a very important question nowadays, whether it is necessary or not to introduce higher velocities, for example, terms containing four $\dot{\alpha}$'s. By the way, nobody knows how to quantize such systems. What they correspond to is of course a velocity-dependent mass, which can exist. It's clear that systems which vibrate faster may have less mass transport than systems which vibrate low. In the same way as the mass can depend on the elongation, on the coordinate itself, it may depend on the velocities. Questions arise, for example, how to quantize kinetic energy operators and things like that of higher powers than just bilinear forms. These are to my knowledge simply unsolved. They go rather deep into the structure of quantum mechanics or the thinkings of quantum mechanics.

We have studied now the simplest case of all, namely that we

always take for the kinetic energy this bilinear form in $\dot{\alpha}$, and for the potential energy the quadratic form in α . This is a harmonic quadrupole vibrator, which leads to the typical form of the spectrum.

If the potential has a minimum at some strong deformation, one gets the structure of rotational schemes by solving this type of Hamiltonian. I draw a one-dimensional picture now, because I cannot draw it in five dimensions. The α squared potential would be the harmonic oscillator.

And a potential like the solid line would lead to the ground state rotational bands at non-zero deformations. That is the same structure of the Hamiltonian; it's just that the potential is a little different.

Now in the problem which we face here, where we are calculating the deformations of heavy ions when they approach each other, we are less interested in how we calculate these constants B and C. They are for us God-given, and God-given means that we look at the experiment. In the same way, we do it here. We expand the potential into a harmonic paraboloid, and then we transform these five laboratory coordinates $\alpha_{2\mu}$ (μ going from +2 to -2) to the intrinsic frame. One usually introduces the Euler angles, which are three coordinates. Then two are left. These are a_0 and a_2 , if using the Cartesian representation, or β and γ , if using polar coordinates.

Now, in principle this surface is always a function of two coordinates, a_0 and a_2 , and we make such a plot.

A spherical oscillator would be represented by a minimum at the point $a_2 = a_0 = 0$, and a deformed rotator would be characterized by a minimum elsewhere. That turns out when one studies this geometry, and I think you all know this; (if you don't know it, read the Bible, volume one. That's

Eisenberg-Greiner,²⁾ volume one. It's called by my students the Bible.) There this is well explained, and what one needs always is only this sector of sixty degrees.

The behavior of the potential in the rest of this plane is just given by remapping all that. This is again a geometrical property, which is known as the so-called Bohr symmetries³⁾ in the collective Hamiltonian. Now if a deformed nucleus lies on the a_0 axis, it would be a prolate deformed nucleus; if it lies on the 60° line, it would be an oblate deformed nucleus. And in between is a triaxial ellipsoid. At $a_2 = a_0 = 0$, a sphere. So you see, everything is explained; it just depends on the structure of the potential characterizing that particular nucleus.

In order to make life simple, one doesn't take the full potential energy surface, but expands around the minimum into a power series. As you may know, the vibrations perpendicular to the axes are called the γ -vibrations; the vibrations along the axes are called the β -vibrations. The one makes the cigar longer and shorter, the other ones bump the stomach. That's essentially the picture. To describe this microscopically is a hard thing.

Now we know what the picture is behind the models which we use. Of course, there is another degree of freedom being very important. When the two ions approach, they may not change their shape, but instead the protons in one ion, because of the mutual repulsion, drive the protons in the other ion to the most distant corner.

And clearly, every nuclear physicist now knows what it means. It is a giant dipole resonance, which is set up in these individual nuclei just by approaching and flying apart from each other. I do not write the collective Hamiltonians for these, but one can do it in a very similar fashion, and you may read it in a textbook.

Now these modes are shown in Fig. 1 just in a pictorial way. In

Fig. 1

a) and b) we have quadrupole and octupole vibrations. The octupole vibrations are negative parity vibrations. Fig. 1c) shows the giant resonances and Fig. 1d) the rotations in the system when the nuclei approach each other. Everybody would believe that we should skip quantum mechanics as soon as we are convinced that a classical picture holds. And to describe a Coulomb wave for two heavy ions quantum mechanically is a very difficult job. With 500 partial waves in these, this is nearly impossible. But everybody would believe that two heavy ions, when they come to each other, because of their large mass, follow a classical hyperbola, and the quantum mechanical disturbances are extremely minimal. And this is the reason why we studied this type of Hamiltonian with various degrees of freedom first classically, to see in a classical picture how the various excitations and motions and rotations and whatever you may think of develop. Well, what did we do? We derived from the Hamiltonian function the set of coupled equations, and solved them by a hyper Runge-Kutta method.

Coulomb fission now is depicted in Fig. 2. You see the one ion

Fig. 2

coming along its hyperbola. The heavier one on the way is so much deformed by these excitation mechanisms - namely by the mutual excitation due to the Coulomb field - that it finally fissions. I mean that is the process for Coulomb fission. And this is something new for very heavy ions. It is not a fundamental effect, but it's an interesting effect. I always distinguish between good physics and excellent physics. Good physics is such physics which can stay later on in textbooks. Excellent physics is beyond that. Now, this is good physics, in the sense that when you discover this, you haven't discovered a fundamental process, but you have discovered certainly something which will stay in any textbook later on. And I think that is the most important thing we should hunt for, textbook physics. The excellent physics comes by accident, I think.

Now, in order to describe this properly, we make the following assumption for Coulomb fission. Fig. 3 shows a cut through a potential energy surface of a deformed nucleus along the prolate axis. At the minimum is the ground state deformation. To the right we have the fission barrier, and you see that one can approximate the potential by various models. If

Fig. 3

one makes a harmonic oscillator around that minimum, one can describe the ground state rather well. It becomes very questionable if one wants to determine highly excited states in such a way. This is nearly impossible. There are of course rotations in addition; these can be described by the so-called rotation-vibration model.

We will determine the cross sections for Coulomb fission in such a way that we do a classical calculation of the Coulomb excitation. (I also will present quantum mechanical calculations later on.) After having calculated the Coulomb excitation classically we ask at what energy, as a function of the incident energy, this approximate oscillator is so highly excited that the vibrational amplitudes become larger than the amplitude of the fission barrier. If it's larger than this barrier, then we assume that it is going to fission. The idea behind this is to try to find out in this classical (and later semi-classical) picture the amplitudes as a function of the distance or the collision time for every incident energy. At certain energies you never can excite an amplitude larger than a certain fraction of the barrier, and you have to go to quite high incident energies in order to excite the system across the Coulomb barrier. And when you have done that, you have Coulomb fission. I think the assumption is rather clear now.

Before we consider these time-dependent functions, let us have a look at Fig. 4, where for a typical fission barrier of the order of about

Fig. 4

six or seven MeV various centrifugal potentials are shown. These come in because of the rotations and are very important. One may ask then for the critical angular momentum to get binding. This determines where the nucleus would dissociate itself, because of centrifugal forces in the system induced by the enormous rotations. The centrifugal forces would disrupt the nucleus. And you see here from that picture very clearly that this happens around an angular momentum between 50 and 60 $\hbar$.

Now again; we come to another side topic which could well be included in such a speculative talk. The physics of high angular momentum is a typical thing you can only do with heavy ions. The highest angular momenta with light ions we have reached so far are about 20 $\hbar$. Some people believe they have 22 $\hbar$ in their system. But I think with the very heavy ions, for instance in a U-U collision, we should easily get appreciable Coulomb excitation cross sections up to about 40, maybe 50 $\hbar$. These energy dissociation effects, which can directly be studied in such heavy-ion collisions, are very important.

Fig. 5 is just for pedagogical reasons, which are now very

Fig. 5

important in present day Germany, showing a full potential energy surface in such a plane. This is a two degree-of-freedom case, which I described. The potential energy surface consists of sectors of 60° , being all equivalent. The minima repeat themselves always at angles of 120° . Since the potential is symmetrical always along the middle line of a sector, you need only a sector of 30° . In such a potential energy surface there occurs also a second minimum at the origin, and then we have a saddle shape.

This potential energy surface is constructed so that it fits all empirical data known for uranium 236. We know the ground state deformation, the beta and gamma energies, determining the curvatures of the sides and the diagonal Coulomb vibrational energies. And we know the fission barrier. These values essentially determine the main feature of such a surface. This is the rotation-vibration model approximation, which means that you expand your minimum into a hyperboloid, with different axes of course. At low energies it's quite all right, but at higher energies of course it's an

approximation, and one should keep that in mind.

Question: How would you calculate electromagnetic transitions between states of these two minima?

Answer: If you have two minima like this, the question is, how to solve it. In the standard rotation-vibration model of Faessler and myself (see ref. 2) or Bohr-Mottelson model,⁴⁾ depending what version you take, you have only the states expanded in this hyperboloid approximation around such a single minimum. You would have a different one in each minimum. And you would think that you couldn't construct any matrix elements. That's correct, because you have done it wrongly. What you have to do instead, has been worked out by Gneuss,⁵⁾ relying on group-theoretical arguments by Hecht and Weber,⁶⁾ that you diagonalize any potential energy surface in the basis of a five-dimensional harmonic oscillator. You have to use very high basis wave functions, up to about thirty phonons, and this basis allows you to describe as well the rotational states as a mixture with the other minimum. Because you use the same basis everywhere, you can calculate the overlaps in a simple fashion, and there is no problem anymore to calculate matrix elements.

Question: I just thought that there might be something direct in there, since you evidently have beta vibrations that could couple...

Answer: You could do it, but then you don't know how wrong you are, and in this way you are exact. It is a rather straightforward method after the mathematics has been worked out. The group theory in that case is tremendously involved.

Question: How difficult is that to do in practice?

Answer: Such a calculation here? Well, for potential energy surfaces like this here, it takes about five minutes on a Univac 1108, to get the full spectrum.

We are studying now the Coulomb fission of a large deformed nucleus colliding with a spherical projectile (Fig. 6). We also studied cases where two deformed or two spherical nuclei hit each other, but as representative I take just this one case. The collective excitations shown in Fig. 7 have been obtained by setting up the classical Hamiltonian

Fig. 6

Fig. 7

equations and solving them with appropriate initial conditions. Now, look at the top picture. This is just pure Coulomb force now. There is not yet a nuclear attractive force taken into account. The horizontal scale is the time, t_n marks the time of closest approach. The β -vibrations, enlarging and shortening the cigar, are represented by the a_0 -amplitudes. The a_2 -amplitudes are the full lines; they describe the pulsation of the stomach.

Before reaching the turning point, there is no elongation in both amplitudes. The elongation comes after that. This is because the vibrational time is roughly a factor of ten longer than the collision time in such cases. The typical frequencies for these vibrational states are of the order of 1 MeV. These are short frequencies, i.e. a long breathing and oscillation time. Therefore, when the ions approach, they get hit, and then they try to breathe out. They get an initial momentum induced. They try to breathe out, come closest, but still they haven't breathed out as far as they really ought to go according to the momentum they have gained. And then they go apart, and breathe out fully. This is the situation shown at the top in a very clear way for both amplitudes.

Now, when you study the energy which is pumped into the system as a function of time, very little energy is in the system at closest approach. Most of the energy is pumped in afterwards. Of course, the energy is proportional to the square of the amplitude and the velocity in harmonic approximations. And so it's not surprising, after having considered the top picture, to get the next picture down. Here you see indications - and one can make this even stronger - that in the moment of going apart from each other, an appreciable amount of energy is lost by the vibrational degree of freedom, and then you get it out again, and afterwards the primary state has constant energy.

The situation changes considerably if besides the Coulomb force a Yukawa force is taken into account (lower part of Fig. 7). Even if the nuclei do not yet touch, they feel already a Yukawa force, with a range of about 0.8 fm. The Yukawa force counteracts the Coulomb force, since it is attractive and not repulsive like the Coulomb force. (I should mention that the parameters of the Yukawa force are taken from those molecular studies which I presented on the second day here.) The scale is much larger than above now. First a little energy is pumped into the system, but the Yukawa

force doesn't let that breathe out as it wants. It wants to go higher, as shown in the next to bottom picture, but the Yukawa force immediately tries to decrease the amplitudes. Typically there is a time dependence. The same holds for the a_2 -vibration. And when you look now at the excitation energy, with mixed Coulomb and Yukawa forces, then the Coulomb force would like to pump all the 2 to 5 MeV into the system, but the Yukawa force turns it at about 1.8 MeV, and after some time the γ -vibrations reach a constant energy of about 0.7 MeV. It is very instructive to follow this up in various examples. But, we have to remind ourselves that the pure classical theory describes only the mean values, and not the individual levels. Quantum mechanically we have perhaps to refine this a little, but it will tell us essential features.

Now, in Fig. 8 I come to the results concerning Coulomb fission.

Fig. 8

In this way of course one can study the Coulomb barrier. The Coulomb barrier is the energy where for the first time the nuclear matter distributions touch. This depends of course on the initial orientations and on the initial energy of the nuclei. When they are deformed, they touch earlier. And we know that they do not turn around very much.

This actually brings up another interesting goal for the future, which hasn't been stressed very much at all up to now. You can do, in principle, because of these effects, polarized heavy-ion collisions. Usually you would have thought that - having polarized the target and the projectile in the approach - due to Coulomb excitation all the polarization would disappear. Right? But now we learned that the advantage of the polarization would be that you can have a head-on or crossed collision, like this.

When you have an oblate nucleus you make smoke rings, or something like that. And this makes sense, because I told you that when you polarize your target, you really polarize the reaction. Take holmium-165, which you can easily polarize, and shoot the polarized projectile on a polarized target arranged like this:

Then you are sure that you get "stomach-stomach" collisions, because this projectile turns around only by a few degrees. So, this makes a lot of sense to investigate new features in nuclear physics. That's textbook physics again. You can study the stomachs of the nuclei, the smoke rings, and whatever.

Now, as I said, I define the Coulomb barrier as the energy where the matter distribution overlaps for the first time. This is of course also the threshold for the reaction cross section, which is in fact the Rutherford cross section multiplied by the probability of all initial orientations which lead to an overlap of matter distribution.

$$\sigma_R = P \cdot \sigma_{Ruth.}$$

The Coulomb fission cross section is defined as the Rutherford cross section multiplied by the probability $\tilde{P}$,

$$\sigma_{CF} = \tilde{P} \sigma_R$$

that the elongation of the cigar is larger than the fission barrier deformation β_{crit} .

We made the oscillator approximation to elongations which are larger than this critical one and say, that this is Coulomb fission.

The results of our calculations can be seen in Fig. 8. (Pure

classical calculations are always indicated by full lines.) The line at the top is the Rutherford cross section for a xenon-uranium collision. Where reactions set in, is the Coulomb barrier. The reaction cross section first increases with energy and finally becomes equal to the Rutherford cross section. The energy scale is given in the center of mass. The Coulomb barrier in that case is a little bit above 500 MeV. Starting near the left axis is the Coulomb fission cross section. Now, there are two things interesting. First, that the Coulomb fission sets in around 450 MeV. Second, that the uranium fissions in this case. (One can also get xenon fissioning, but this happens at much higher energies.) The upper part of the figure has been calculated without, the lower with the Yukawa force. It diminishes the Coulomb fission, naturally, because it doesn't allow the big amplitudes, as I explained before, in these vibrational modes. You get typical orders of magnitude of about 50 mb/sr for the Coulomb fission cross sections.

These are quite accurate numbers. They are much smaller than the original extremely crude estimates of Wilets, Guth and Tenn,¹⁾ but larger than the wrong estimates of Beyer and Winther.⁷⁾ They published a paper that Coulomb fission cross sections are by a factor of about 10 smaller than these ones which we get out here. Now, this was the classical picture. If you do the semi-classical calculation, which is a more realistic one, of course, then you end up with the dash-dotted lines shown in Fig. 8. Again, the upper half is for Coulomb excitation only, and the lower half for Coulomb and Yukawa excitation. "Semi-classical" means that you treat the orbit classically, but the excitation of the nucleus quantum mechanically. So I think these curves give the insight which we wanted; we treated much more systems, and I have a whole package full of slides about it. But I think this suffices.

There is one more case, Fig. 9. Namely just a heavier projectile, radon on uranium. You see again the Coulomb fission cross sections, calculated with and without Yukawa forces. And you see that the quantum mechanical treatment gives the same order of magnitude. In the lighter fragments where the cross sections were rather weak and the force was rather critical this brought about an essential difference.

I want to leave now the topic of Coulomb fission and just mention

Fig. 9

another interesting aspect. I didn't talk here at all about the superheavy elements, which, if they exist, and I am pretty sure that they do, are extremely interesting and fundamental for the chemical point of view, but also for the continuation of the nuclear shell structure. We want to know whether the next magic numbers really exist and where they set in. But I would like to stress another point which is very interesting textbook physics. And this is the triple fission. The superheavy element calculations have all been done on the basis of binary fission, that is the nuclei break up into two fragments. When you calculate according to the mass formula the binding energy of a breakup of a superheavy element into three fragments, you find that this process is much more exothermic than binary fission. The question is: Does this mean that suddenly the triple fission becomes very important or not, for superheavy elements? This is an essential question, because you might worry about how to discover these events if they are rarely produced, but if you are pretty sure that triple fission is a very important process there, you could easily look for stars in mica or whatever. And events can be identified in this way, because when you

have three fragments they should always lie in a plane because of momentum conservation. And it would help in identifying these processes.

Now, to make that long story short, and just tell the results, these calculations⁸⁾ were done in a three-center liquid drop and three-center shell model, and renormalized as we discussed the days before. There are three modes to be distinguished. The one is the oblate type of breaking up. And the other one is the chain type.

"OBLATE"

"CHAIN"

If you calculate in the liquid drop model the total energy and the barriers, (of course with rounded edges everywhere) as a function of the separation of the fragments, then you find that for $Z = 110 - 114$ this binary mode still has the higher barrier, of the order of about 50 MeV. It's much lower than in uranium, where it's about 100 MeV, and then it decreases very fast. But the triple fission barrier, in a chain type breakup is equal or lower than the binary fission barrier. This suggests strongly that there exists triple fission in superheavy nuclei, at least as strong as binary fission. And if one goes even to the upper island at $Z = 164$ (which is of course a little bit exotic, and I'm not sure whether one can make it at all in the next twenty years), this barrier becomes very low, comparable to the other barrier, so that you would get also the new phenomenon of oblate fission.

I would like to close now. I think I have shown you in this last lecture a kind of an outlook on various new phenomena which you can attack with heavy-ion physics, being all in itself very interesting. They really shed new lights in this topic and may somehow finish up our picture from nuclear physics or perhaps surprise us all. It could also happen that

things come out differently by doing the experiments.

Now, let me say finally a word of thank you, especially to the Chairman and to the Director of the Institute here, but also to the many colleagues which I met. I must say that when I came here I was tremendously surprised because I learned so much from you. Thank you for this and especially also for your fine hospitality.

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Dr. Ivan A. Sellin

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METASTABLE ATOMIC STATES OF HEAVY IONS I: X-RAY EMITTING STATES

I am going to spend most of my time talking about metastable atomic states of heavy ions. I will talk only about elementary systems, that is, those ions containing just a few electrons attached to a rather highly charged nucleus. We will discuss primarily analogs of hydrogen, helium and lithium, but built on nuclei of higher charge.

I want to begin by telling you why I think these are interesting objects to study. What makes these systems a bit different from their low Z counterparts is that the atomic electrons are pulled in to rather small radii compared to their radii in nearly neutral ions. They also move at substantially higher orbital velocities than in nearly neutral ions; all the effects that depend on v/c , of which the most important are the magnetic spin-orbit and spin-spin interactions, are substantially stronger than they are in nearly neutral systems. The electrons also interact much more strongly with each other and with the nucleus by virtue of their reduced separation and their significantly nearer proximity to the nucleus. This means that a good many forbidden de-excitation processes for excited states of these ions, which go on at unmeasurably slow rates in nearly neutral systems, begin to have rates fast enough for one to make good laboratory rate measurements.

Before discussing any of the experiments, I want to show a couple of slides that just give some basic information about atomic energy levels of elementary systems. Fig. 1 shows something about the Z -dependence of various atomic quantities, in particular the energy eigenvalues derived from the Dirac equation for a single electron in the field of a point nucleus. The top line is the exact expression. It is perhaps more familiar in the limit of low Z , where the electronic energy begins with, first, the rest energy of the electron, then the Bohr model energy, Z^2/n^2 Rydbergs, then a term that includes the magnetic fine structure. There are also radiative level shifts, which do not come out of the Dirac equation. Perhaps the most well known is the so-called Lamb Shift, the leading term of which is $\propto Z^4$, but which contains

2. DEPENDENCE OF ATOMIC TERMS

One-Electron Ions

$$\text{Dirac Energy } E = mc^2 \left\{ 1 + \left[\frac{Z\alpha}{n - (j + \frac{1}{2}) + \sqrt{(j + \frac{1}{2})^2 - Z^2\alpha^2}} \right]^2 \right\}^{-1/2},$$

$$\text{For } Z\alpha \ll \frac{Z}{137.036} \ll 1, \quad E \approx mc^2 - \frac{Z^2}{n^2} \text{ Ry} \left[1 + \frac{(Z\alpha)^2}{n} \left(\frac{1}{j + \frac{1}{2}} - \frac{1}{n} \right) \right]$$

fine structure

$$\text{Radiative Shifts } \Delta E(n) \approx \frac{Z^4 \alpha^2 \text{ Ry}}{n^3} \left[\frac{8}{3n} \left(2 \log(Z^{-1} \alpha^{-1}) + \log \left(\frac{Z^2 \text{ Ry}}{K_0} \right) + \frac{19}{30} \right) \right]$$

• Terms of Relative Order $\frac{Z\alpha}{n}, \frac{Z^2 \alpha^2}{n^2}, \dots$

Two-Electron Ions

Typical fine structure splitting

$$^3P_0 - ^3P_1 \approx \frac{(Z-1)^3 \alpha^2}{6n^3} \text{ Ry } (9 - 2)$$

Fig. 1

a good many terms of higher order, proportional to Z^5 , Z^6 , and so on. Clearly if you change from Z near one to something higher, these higher order terms become increasingly significant. That is one reason why there is still interest in measuring radiative level shifts in systems of increasingly high Z .

One other interesting thing is that when Z equals about 137, so that Z times α , the fine structure constant, goes to 1, then the expression for the energy becomes unphysical; the Dirac theory breaks down for $Z\alpha$ greater than or equal to one. One interesting question is, how do systems where Z is greater than 137 behave. A divergence is avoided, even for a single electron system, by realizing that the nucleus is not a point, but is an extended object. That prevents the analytical disaster that occurs in this expression. For a long time this has been thought to be academic in the sense that we don't know of any superheavy elements. But in the very recent past there have been several suggestions by Walter Greiner and his colleagues¹⁾ about how to make, very briefly, atoms with Z greater than 137 by binding together two heavy ions at sufficiently small impact parameters that

their distance of closest approach is less than or of the order of the reduced Compton wave length. The idea is that the Compton wave length is the smallest distance that an electron can be sensitive to; it cannot distinguish the separation of two potential sources, like the Coulomb fields of two nuclei, if they are closer together than λ . So the suggestion has been made that perhaps there is some interesting atomic physics to be done on "superheavy" atoms that are superheavy in the limited sense that for a short time, at least, they can be made to resemble a monopolar Coulomb field in their vicinity.

I will talk just briefly about Greiner's prediction of what happens to the energy levels of an electron in the field of such a double nucleus. In Fig. 2¹⁾ you see the Z of the combined system

Fig. 2

plotted horizontally and the total energy of an electron, say in a 1s state, that finds itself in the vicinity of such an object plotted vertically. An unbound electron would have just the rest energy mc^2 . For low Z atoms there would be additional binding energy of the electron so as to slightly depress its energy level. The interesting thing is that if the combined Z is large enough, it is possible by Greiner's

solution to the two center Dirac equation to make the energy levels of the electron in the field of two nuclei take on a value that is below $-mc^2$, which is the top of the Dirac negative energy continuum. This means an electron can find itself in an energy region that is degenerate with the negative energy electrons in the Dirac sea. Greiner's hypothesis is that if there is a mixing between such electron states and the states in the Dirac sea, this will manifest itself in terms of positron production. The decay probability for positron emission turns out to be proportional to the difference between this effective Z and some Z_{CRITICAL} , squared. The Z_{CRITICAL} turns out to be larger than 137; in fact, it is about 169 according to his recent estimate, because of the smearing out of the potential due to the finite nuclear radius.²⁾ This is the prediction of the usual linear version of the Dirac equation. If there are nonlinear additions we don't know about, that would modify what happens in this region of expected positron production. One of the experiments that surely needs doing is to search for positron emission for quasi-nuclei of sufficiently large charge.

The next systems I want to discuss are the two-electron systems, helium analogs, which are described not by the Dirac equation but by the so-called Breit equation. Fig. 3 shows in schematic form what is called

Helium Atom

Breit Equation in Pauli Approximation

$$WU = (H_0 + H_1 + H_2 + H_3 + H_4 + H_5 + H_6)U$$

H_0 = non-relativistic Hamiltonian

H_1 = relativistic mass variation

H_2 = electronic em field retardation

H_3 = spin-orbit interactions (nuclear and electronic)

H_4 = Dirac electric field terms ($\vec{\alpha} \cdot \vec{E}$)

H_5 = electronic spin-spin interactions

H_6 = interaction with external $\vec{B}$ field

Fig. 3

the Breit equation in the Pauli approximation. There occurs an energy eigenvalue W times some spinor eigenfunction; there is a chain of terms in this Hamiltonian, which are listed on the diagram. Most of them just produce shifts in the energy levels. Of course, the H_0 term produces the energy to lowest order; most of the others just shift that energy in one way or another, with the exception of the terms labeled H_3 and H_5 , the magnetic interactions. The spin-orbit and spin-spin interactions not only shift the energy levels, but they also split levels that are degenerate with respect to the rest of the Hamiltonian. Because it is of some interest to learn about the fine structure splittings for two electron systems, it is useful to talk about the terms H_3 and H_5 separately. In Fig. 4 I have written out what these terms look like

Only H_3 and H_5 lead to *fs* splittings

$$H_3 = \frac{\mu_0}{mc} \left[(\vec{E}_1 \times \vec{p}_1 + \frac{2e}{r_{12}} \vec{r}_{12} \times \vec{p}_2) \cdot \vec{s}_1 + \text{same terms for } 1 \leftrightarrow 2 \right]$$

$$H_5 = \mu_0^2 \left[-\frac{8\pi}{3} \vec{s}_1 \cdot \vec{s}_2 + \frac{1}{r_{12}^3} (\vec{r}_{12} \cdot \vec{s}_1 - \frac{3(\vec{s}_1 \cdot \vec{r}_{12})(\vec{s}_2 \cdot \vec{r}_{12})}{r_{12}^2}) \right]$$

For $1s2p$ 2^3P states

$$\Delta W = \frac{\alpha^2(Z-1)^3}{6n^3} \begin{cases} 9 - Z, & \text{for } J = 0 \rightarrow 1 \\ \frac{10}{3} - 2Z & \text{for } J = 1 \rightarrow 2 \end{cases}$$

H_3 and H_5 connect ortho and para states of the same J . Ortho-para dipole transition probability ($1^1S_0 - 2^3P_1$) scales $\sim Z^{10}$.

Fig. 4

for a two-electron system. They would look very similar for three or more electron systems, except there would be more terms corresponding to all the individual interactions among the electrons. But this is the basic structure.

For a two-electron system there will be two possible spin states, spin zero states in which the electron spins oppose one another, the so-called singlet states, and the states in which the electron spins add; two spin 1/2 electrons will add to spin one, making so-called

triplet states. The simplest, lowest order expressions for the splitting between the J-states that can be made by adding the total orbital angular momentum to the spin in all the different ways that are possible are given here. Also we note that the spin-orbit and spin-spin terms connect the ortho and para states of the same J; in other words, spin is not a good quantum number, and neither is total orbital angular momentum. This violation is rather small for ordinary helium, but it can be quite large if Z is sufficiently large. As you can see by examining this expression, it can even change sign. The ordering of fine structure levels for ordinary helium is completely inverted; that is, instead of having $J = 2$ on top, $J = 1$ in the middle, and $J = 0$ on the bottom, it is just the opposite; $J = 0$ is on top, $J = 1$ is in the middle, and $J = 2$ is on the bottom. These levels do not retain their relative ordering as a function of Z.

This is just the lowest order expression; there are, of course, many correction terms. You can see, for example, that in the lowest order theory the $J = 0$ and $J = 1$ levels are approximately degenerate for $Z = 9$. You can see in Fig. 5³⁾ that the levels become nearly

Fig. 5

degenerate for $Z = 7$, but not quite. This interval turns out to be

something on the order of 6 cm^{-1} , which is quite a bit; but still they are accidentally very close compared to what you would get from just estimating the magnitude on dimensional grounds.

This raises the possibility that it may sometimes be possible to determine the fine structures for different ionic systems provided one can find a place where the levels are sufficiently close to be mixed by some applied field. This kind of thing can be detected by looking for changes in the amount of radiation an excited state emits as a function of the applied field. I have a few slides from an experiment that we did some years ago that illustrate for the simplest system, ordinary hydrogen, the point of how one might hope to do level splitting measurements by using applied fields. Fig. 6⁴⁾ shows the dependence of

Fig. 6

the fine structure splitting on an applied electric field for the $n = 2$ state of hydrogen. If one does not consider the Lamb Shift, which separates the 2s and 2p states in hydrogen, they are exactly degenerate in the Dirac theory. The $j = 1/2$ states and $j = 3/2$ states are separated by roughly 10 GHz at zero field. If one then applies a

field, one finds that the levels shift as a function of the applied field. The 2s state is radiatively metastable; to lowest order it doesn't radiate at all, but the application of the field is sufficient to mix it with the nearby 2p state. The Lamb Shift is too small to see here. If you apply an electric field, the s and p states no longer remain pure, but will be mixed by a Stark matrix element. The resulting state is a superposition that has some p character. That piece of the wave function can then decay to the ground state, and the size of that piece of perturbed wave function essentially determines how fast the state is going to radiate. For example, if the perturbed s wave function looks like an s state plus a small admixture of the nearby p state, $\psi'(s) \approx \psi(s) + a\psi(p)$, then to lowest order one expects the decay rate, the inverse of the lifetime for the perturbed state, to be like an ordinary allowed rate for the p state in going to the ground state times the square of the absolute value of this mixing coefficient, $\gamma(s) = |a|^2 \gamma(p)$. Applying the electric field makes it possible to induce a decay that could not otherwise occur. This is the basic principle of the method; one can examine the decay of metastable states, for which it would be impossible otherwise, by applying a field.

Fig. 7⁴⁾ shows what happens in ordinary hydrogen. These are theoretical predictions for the mean lives of the states as a function of the applied field. At zero field the s state has a very long lifetime and the p state has a lifetime of 1.6 nsec. As the field increases, the two lifetimes approach a common mean value. In the case where one is dealing with more highly excited states, the problem is more complicated, but the characteristic feature is that at large fields the lifetimes of all the levels become comparable to one another; that is just a description of the fact that all these states become thoroughly mixed. It is possible to induce these decays at a rate that depends on the size of the applied field. For example, the zero-field splitting of the $2S_{1/2}$ and $2P_{1/2}$ states, which I represent by the symbol S, is just the ordinary Lamb Shift.

Fig. 7

$$\begin{array}{c}
 2S_{1/2} \\
 \hline
 \updownarrow S \\
 \hline
 2P_{1/2}
 \end{array}
 \quad
 a \sim \left[\frac{(eE a_0)}{S} \right]$$

The coefficient a turns out to be a constant of order one times a Stark matrix element-- E is the electrostatic field, a_0 is the Bohr radius; this numerator has dimensions of energy; all ordinary Stark matrix elements come in this kind of unit. This is the important parameter, dimensionally. Now S , as we have already remarked, grows approximately like the fourth power of Z ; a_0 is just the Bohr radius for hydrogen, and if one applies the same kind of analysis to systems

of higher Z , the radius of the system shrinks and goes in fact like possibly $1/Z$. This means the mixing parameter for a system of higher Z has the structure eEa_0 over roughly Z^5 times what the Lamb Shift is in hydrogen. So all things being equal, a field that will give certain effects in a hydrogen atom needs to grow like Z^5 in order to produce the same effects in systems of higher Z ;

$$a \sim \frac{(eEa_0)}{Z^5 S}.$$

This circumstance is the basis of some recent measurements of Lamb Shifts in one-electron systems of higher Z . The first such experiment was one that we did at the University of Chicago about 7 or 8 years ago.⁵⁾ I want to mention it because I have a slide that describes how it was done, and I don't have any slides for the experiments done on carbon and oxygen at Bell Labs in the last couple of years.⁶⁾ From Fig. 8⁵⁾ you can get an idea of how this kind of experiment is done.

Fig. 8

The more modern version is quite similar to this one. What one does is to take a suitably energetic ion beam, say from a Van de Graaff, pass it through some kind of medium that excites it, in this case a gas

cell (that's also true for the more recent experiments), and then into an electrostatic quench region. The purpose of this quench region is to induce the mixture of s and p states that we have already described. The decay rate of the perturbed state then depends on the ratio of the electric field to the Lamb shift. By measuring this rate in the laboratory, and by measuring the electric field that is required to produce the mixing, it is possible to work backwards and find out what the Lamb shift is.

The way that the rate is measured is by now very familiar to everyone who is acquainted with lifetime measurements using fast beams. One merely tracks the decay of the radiation that metastable systems in the beam emit in flight as a function of the distance from the point of excitation. By moving one of these photon detectors that picks up the Lyman alpha radiation, it is possible to follow the exponential decay in flight of the perturbed state. By finding the relative intensity as a function of position along the beam, and by measuring the beam velocity, it is possible to work backwards and obtain the decay rate of the radiation. That gives one an indirect method of determining the Lamb Shift. It is useful in such experiments to have a so-called pre-quench section to destroy all the metastable atoms before they enter into the region where the basic measurement is performed. It is then possible to measure the background in the absence of metastable atoms.

This was the way that we measured the Li^{2+} Lamb shift. The wavelength of the radiation was around $137 \text{ \AA}$, which is what Lyman alpha turns out to be in Li^{2+} , and the sizes of the quench fields used were roughly 10 kV/cm. In the last year or two, as I have already mentioned, some quite similar experiments have been done at Bell Labs on carbon and oxygen. The method is basically the same; the main difference is that instead of using an applied electrostatic field, they applied a magnetic field. In the rest frame of some ion traveling at a velocity $\vec{v}$, there is an effective motional electric field $\vec{v} \times \vec{B}$. It turns out for magnetic fields on the order of say 10 kilogauss and for beam velocities typically on the order of 10^9 cm/sec that one can get an effective motional electric field on the order of 10^5 volts/cm .

Thus, it is possible in certain cases to induce decays in

systems that decay too slowly for measurement by ordinary means. Fig. 9

Electric Field	Magnetic Field	Motional Electric Field ($V \sim 10^9$ cm/sec)	Motional Magnetic Field ($V \sim 10^9$ cm/sec)	Stark splitting	Lamb splitting
$\sim 10^5$ V/cm			~ 10 G	$\sim 10^{11}$ Hz	
	~ 10 G	$\sim 10^5$ V/cm			$\sim 10^{10}$ Hz for $Z \sim 1$

$$\psi_i = \sum_{M_j} \langle L, J, M_j, b \rangle \langle L, J, M_j \rangle \exp(-i\omega_j t - \gamma_{LJ} t/2)$$

$$\langle L, J, M_j, 0 \rangle = \sum_{M_L, M_S} \langle L, M_L, M_S, 0 \rangle \langle L, M_L, M_S, M_j \rangle$$

In matrix notation, $\psi(0) = C \psi(0)$

$$B(t) = \exp(-i\omega t) B(0) \quad \text{(zero field)}$$

$$B(t) = e^{-i\omega t} e^{+i\omega' t} B(0) \quad \text{(Stark perturbation H')}$$

Intensity $\propto \sum_{\text{final states}} \sum_{\text{polarizations}} |\langle \psi_{\text{final}} | \hat{e}^2 | \psi_i \rangle|^2$

Fig. 9

says something about how general this technique might be if you go to still higher Z . I have listed here the sort of real electric field you might be able to produce fairly conveniently in the laboratory; 100 kV/cm is not an unreasonable electrostatic field to make. If one asks how big a magnetic field one gets as a result of the motion of an ion through such an electrostatic field, the answer for beams moving at say 10^9 cm/sec is an effective magnetic field on the order of 10 gauss. This means one can largely neglect the effect of the motional magnetic field on these ions. The Stark splitting, which is what we have discussed for hydrogen, for doubly ionized lithium, and, very briefly, for carbon and oxygen, turns out to be a number like 10^{11} Hz divided by Z ; it goes down like one over Z . The Lamb shift in ordinary hydrogen is about 10^9 Hz; in Li^{2+} it is roughly 60×10^9 Hz; in oxygen it is roughly 10^{12} Hertz. So one is rapidly losing; one can make this kind of thing work up to roughly say neon; then it starts to become increasingly difficult. As we have already remarked, laboratory magnetic fields permit an effective motional electric field that is of the same order

or larger than the biggest electrostatic field that one can make conveniently, so this is probably a more useful way to obtain an electric field. There will, of course, then be a magnetic field in the rest frame of the ion, and that is going to mix levels of the same parity and different J with each other. The amount of such Zeeman splitting that one gets as a result of this applied field is on the order of 10^{10} Hertz for g factors on the order of unity.

It appears that possible magnetic quench methods have more restricted applicability than electric quench experiments. There might be a few special cases, of which we have already shown one example, the accidental "crossing" between $J = 0$ and $J = 1$ in helium-like ion 2^3P systems near $Z = 7$. There are a few cases where the splitting is anomalously small and may permit one to do a Zeeman level mixing experiment by observing changes in the amount of radiation that some state emits, but I think it has to be true that such cases are rare and that one has to be somewhat lucky to find one. So I think that the method of inducing decays by applied fields is limited to fairly low Z or high n states.

Since we conclude that it is difficult to find very many cases where applied fields will be of enormous help, we next ask about systems that can be investigated without having to perturb them. Are there systems that spontaneously decay fast enough that one can measure the rates of forbidden processes without having to resort to applied fields? We need to consider the metastable lifetimes as functions of Z for systems that decay spontaneously through the violation of some selection rules. The systems that undergo forbidden decays are those for which some matrix element is almost zero, but not quite, because of high order corrections such as relativistic corrections or quantum electrodynamic effects. The reason I think it is interesting to study these things is that it is possible to amplify the departures from the low order theory by going up in Z .

If one can measure any decays at all, one can study the difference between the first order theory and the higher order theory to whatever is the precision of the experiment one plans to do. Two of the first questions to ask are how the ordinary radiative selection rules depend on Z and how common are the higher order radiative processes.

Everyone is quite familiar with the electric dipole selection rules, which are usually applied to atoms and molecules. Less familiar are the radiative selection rules for higher order processes. I have found a very convenient way to tabulate them, which is shown in Fig. 10. We

RADIATIVE SELECTION RULES	
ELECTRIC $\Delta J = 0, \pm 1, \pm 2, \dots \pm K$ $J_1 + J_2 \geq K$ $\Delta m = 0, \pm 1, \pm 2, \dots \pm K$ $(\Delta L = 0, \pm 1, \pm 2, \dots \pm K, \Delta S = 0)$	PARITY CHANGED FOR EVEN K PARITY CHANGED FOR ODD K
MAGNETIC $\Delta J = 0, \pm 1, \pm 2, \dots \pm K$ $J_1 + J_2 \geq K$ $\Delta m = 0, \pm 1, \pm 2, \dots \pm K$ $(\Delta L = 0, \pm 1, \pm 2, \dots \pm K; \Delta S = 0)$	PARITY CHANGED FOR ODD K PARITY CHANGED FOR EVEN K

Fig. 10

have basically two types of radiation, so-called electric radiation and so-called magnetic radiation. Two to the K power is the order of the radiation; $K = 1$ is dipole, $K = 2$ is quadrupole, $K = 3$ is octupole, and so on. J is the total angular momentum of the emitting system. By picking what order one wants to consider, whether it is dipole, quadrupole, or higher, one can decide what the maximum change in the J of the emitting system can be. An interesting point is that the sum of J_1 and J_2 , which are the initial and final total angular momentum of the emitting system before and after it has emitted the photon, have to be greater than or equal to K . The rules on the magnetic quantum number M are similar to the ones for J .

In addition, there are approximate rules, and these are the things that break down increasingly with increasing Z . The total orbital angular momentum change is roughly zero for systems of low Z and

for lower order multipoles, and the change in total electronic spin of the system is roughly zero, but these rules are not exact.

The rules concerning parity are easily calculated as well. For electric multipole radiation, parity is conserved for even values of K , i.e., quadrupole and so on, and parity changes for odd values of K , i.e., dipole, octupole and so on. If one looks at the selection rules for magnetic radiation, these rules change character; for magnetic radiation the parity is conserved for odd values of K . The selection rules on total angular momentum and on the magnetic quantum number are very similar to those for the 2^K -pole electric radiations. The same comment applies about the weaker selection rules on total orbital angular momentum and total spin.

Fig. 11 gives an example that is appropriate to magnetic

EXAMPLE

FOR M2 RADIATION

(1) PARITY CHANGES

(2) $\Delta J = 0, \pm 1, \pm 2$ ($0 \leftrightarrow 0, 0 \leftrightarrow 1, 1/2 \leftrightarrow 1/2$)

(3) $\Delta M = 0, \pm 1, \pm 2$

(4) $\Delta S = 0, \pm 1$

(5) FOR $\Delta S = 0, \Delta L = 0, \pm 1, \pm 2$

FOR $\Delta S = \pm 1, \Delta L = 0, \pm 1$ ($0 \leftrightarrow 0$)

Fig. 11

quadrupole radiation. The higher order multipole radiations become increasingly common as Z increases. For magnetic quadrupole radiation one has the rule that parity must change, the permitted values of ΔJ are 0, 1, and 2, and then one has these additional rules, arising from $J_1 + J_2 \geq K$, that not only are 0 to 0 transitions forbidden, but also 0 to 1 and 1/2 to 1/2 transitions are forbidden. The rules on M are very similar to the rules on J . The rules on spin are $\Delta S = 0$ or ± 1 ,

but the selection rules on the total orbital angular momentum are different. This is an interesting point. The rule depends on whether you are considering radiation for which $\Delta S = 0$ or whether you are considering radiation for which there has been a spin change. For unit spin change, ΔL can only be zero or ± 1 . Only if the spin does not change can ΔL be as much as ± 2 .

What systems can one investigate experimentally? The helium system is one that we have worked on in the past. Remember that the levels of helium are ordinarily classified into singlet and triplet series. There is a whole series of singlet levels, beginning with the ground state and then higher singlet levels having $n = 2$, $n = 3$, and so on. The triplet levels are $1s2s$, $1s2p$, and so on. For a 3P state there are three possible J values, 0, 1, and 2, which form either an ordinary multiplet, or an inverted multiplet, or a partially inverted multiplet, depending upon the value of Z . The question that arises is, how does each of these levels decay? Let's consider first the $J = 1$ level. In ordinary helium, the $(1s2p)^3P_1$ state is formed say in some collision, and will undergo an ordinary allowed dipole transition and wind up in the $(1s2s)^3S_1$ metastable state. But if Z is large enough, it becomes possible for the spin to change from 1 to 0, i.e., for the triplet levels to become mixed with the singlet levels, and for an intercombination photon to be emitted, leaving the system in the 1S_0 ground state. This is a spin forbidden transition; in order to reach the ground state the spin has to change by 1 unit. It turns out that this channel is actually more probable than the ordinary allowed channel for all systems having $Z \geq 7$, so a system prefers to de-excite by changing its spin for all two-electron ions as heavy or heavier than nitrogen.

Why is this the case? We ordinarily think of the singlet and triplet levels as distinct, but the 3P_1 level is not really a pure triplet level. The reason is that there is an admixture of singlet level of the same J . If we are talking about the 3P_1 level, we really ought to label it $(^3)^1P_1$. There is a small piece of the wave function that has singlet P character. The reason is that there are off-diagonal matrix elements of the spin-orbit and spin-spin terms of the Hamiltonian,

which we previously labeled H_3 and H_5 . These terms not only split the fine structure levels, but they also mix states of different spin with each other and states of different orbital angular momentum with each other. The amount of such mixture has the order of $(Z\alpha)^2$. The decay rate for this particular kind of spin-forbidden transition grows approximately like Z^{10} . That is a big number. It means that if you go up to nitrogen or a little heavier, even moving from one element to an adjacent element can change the decay rate by say roughly a factor of 3, and that something that would essentially never occur in ordinary helium, something that takes milliseconds in ordinary helium, takes an order of nanoseconds in a system like oxygen.

Measuring this spin-forbidden decay rate essentially measures the magnetic interactions in a helium-like system. We have done some experiments over the past few years related to them. The apparatus is quite similar to what I have shown you previously. One has a decay-in-flight rig in which ions are excited at one place and the amount of light they emit is measured downstream as a function of separation from that place. Fig. 12⁷⁾ shows apparatus that is useful for that kind of

Fig. 12

work. It is quite similar to the Lamb Shift apparatus that we looked

at earlier, except we built a small flat-crystal Bragg spectrometer to pick out the particular wavelength radiation ($1^1S_0 \sim 2^3P_1$) for the two-electron ions. There is a movable foil target in which ions are excited, and the radiation emitted is measured downstream by the spectrometer as a function of the separation of the viewing region from the target. A plot of relative intensity of the radiation versus this separation distance allows one, through a knowledge of the beam velocity, to work backwards and find the lifetime of the radiation.

The work sometimes takes a long time because Bragg spectrometers are inherently fairly inefficient devices, and the decay curves one gets are frequently statistics or noise limited. Fig. 13⁷⁾ shows

Fig. 13

an example of a decay-in-flight curve that we obtained in this kind of experiment. This is a plot of the log of the radiation intensity versus the separation of the target from the spectrometer viewing region for this spin-flip transition, otherwise known as an intercombination

transition. These particular experimental results are recent ones for an oxygen beam, in which case the wavelength of the radiation is close to 22 Angstroms. Some calculations by Dalgarno and Drake are listed here; the agreement is quite good.

More recently we have done an experiment on the same transition, but this time in fluorine. Because of the Z^{10} scaling, the lifetime gets shorter fairly rapidly, and the radiation is sufficiently hard that it is much easier to do the experiment using a Si(Li) detector. Fig. 14⁸⁾ shows essentially the same apparatus as before, except that

Fig. 14

the Bragg spectrometer has been replaced by a Si(Li) detector. For example, this particular transition in fluorine turns out to have an energy of roughly 722 eV.

The upper curve in Fig. 15⁸⁾ shows a spectrum from a calibrated pulser which was set up to produce a simulated pulse of the same energy as the 722 volt soft x rays. The middle curve is an actual experimental signal. You see marked the centroid calculated between appropriate integration limits. The centroid matches very nicely the center of the calibration spectrum. The bottom curve shows there is

Fig. 15

a small noise tail. If one turns everything off and just measures the electronic noise, one gets the curve on the bottom.

Fig. 16⁸⁾ shows a decay curve for this same intercombination line in the two-electron fluorine ion. As with nearly all experiments, there is something that one does not understand; there is a long sloping background, which probably contains contributions from other states we have not previously thought about. One possibility is that the background arises from some metastable state of the doubly excited helium-like system. The experimental result for the decay rate of the 2^3P_1 state of the fluorine ion from this and similar plots at different beam energies is $1.9 \pm .1 \text{ (nsec)}^{-1}$. The theoretical number for the decay rate is 1.92 (nsec)^{-1} . Apart from the excellent agreement between theory and experiment, I personally find a scaling of $\sim Z^{10}$ unusual, and therefore worthwhile pursuing.

I want to cover just one thing more today, and that is how one expects the lifetimes of various multipole radiations to scale with Z .

Fig. 16

Fig. 17 has that information. Let us recall that in the matrix

2. DEPENDENCE OF RADIATIVE TRANSITION PROBABILITIES

Electric Dipole Approximation

$$\exp(i \vec{k} \cdot \vec{r}) \sim 1 + i \vec{k} \cdot \vec{r} \sim 1 + (Z\alpha) r \quad \text{for } Z\alpha \ll 1$$

$$E1 \text{ Transitions} \quad \gamma \sim \alpha (Z\alpha)^4 \frac{mc^2}{h} \sim 10^9 \text{ sec}^{-1} \cdot Z^4$$

$$M1, E2 \text{ Transitions} \quad \gamma \sim \alpha (Z\alpha)^6 \frac{mc^2}{h} \sim 10^5 \text{ sec}^{-1} \cdot Z^6$$

$$E3 \text{ Transitions} \quad \gamma \sim \alpha (Z\alpha)^8 \frac{mc^2}{h} \sim 10 \text{ sec}^{-1} \cdot Z^8$$

Special Cases

Parity forbidden $2S_{1/2} \rightarrow 1S_{1/2}$ decay of one-electron ions (M1)

$$\gamma \sim \alpha (Z\alpha)^{10} \frac{mc^2}{h} \sim 10^{-4} \text{ sec}^{-1} \cdot Z^{10}$$

Two Photon Emission

$$\text{One-Electron Ions } \gamma \sim \alpha^2 (Z\alpha)^8 \frac{mc^2}{h} \sim 10 \text{ sec}^{-1} \cdot Z^8$$

$$\text{Two-Electron Ions} \quad \sim 10 \text{ sec}^{-1} \cdot (Z-1)^8$$

Fig. 17

elements for transitions of various multipole radiations there is a factor $e^{ik \cdot r}$. The electric dipole approximation consists of replacing that exponential by the quantity one in the matrix element, using the argument that r is a number of the order of atomic dimensions and k is a number of the order of the reciprocal of the wavelength of the radiation, which is usually much larger than any atomic dimension. However, if one represents the correction terms as a function of Z , one gets something the order of 1 plus $iZ\alpha$, where α is the fine structure constant. The bigger Z gets, the larger these correction terms become. That's why the higher multipole radiations become increasingly common with increasing Z .

Let's consider how the various multipole terms scale with Z . Ordinary electric dipole transitions go like $(Z\alpha)^4$ multiplied essentially by α times the rest energy of the electron, divided by $\hbar$ to convert things to frequency units. One gets something the order of $10^9 \text{ Hz} \times Z^4$. For the higher order multipole transitions, that coefficient goes down and the power of Z goes up. Then there are some special cases, about which we will have more to say next time. There are anomalous cases, such as the $2S$ to $1S$ transitions in one-electron ions, which are forbidden not only by virtue of the selection rules that we have discussed, but also have radial matrix elements that vanish except in so far as they are affected by relativistic corrections. One of the more interesting cases is this magnetic dipole decay channel of the $2S$ state of the one-electron ion. That scales like Z^{10} .

There are some other processes that we have not talked about yet, for example, the possibility that in getting rid of the excitation energy the system emits two, or more, photons simultaneously in such a way that the total energy of the photons emitted adds up to the difference in energy between the initial and final states. That process scales like Z^6 . In the next talk I will discuss a few cases in which these decays have actually been seen, and after that move ahead to some of the work that has been going on recently in electron spectroscopy.

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METASTABLE ATOMIC STATES OF HEAVY IONS II: X-RAY AND AUGER EMITTING STATES

I want to take up approximately where we ended last time, so I'll review the previous talk very briefly.

We were talking about metastable states of two electron systems, in particular of helium. Fig. 1¹⁾ shows the energy level

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Fig. 1

diagram for a particular two-electron system, Ar^{+16} . This displays many of the things that we talked about last time. The figure also shows a schematic energy diagram for the hydrogen-like, one-electron system. The hydrogen-like argon ion has a $2^1S_{1/2}$ ground state, and a metastable $2^2S_{1/2}$ excited level. There are two decay modes permitted for radiative deexcitation of this state to the ground state. One of them is the so-called two-photon emission channel. That is, the simultaneous emission of two photons whose combined energies add up to the total energy of the transition. The other transition permitted by the selection rules is a magnetic dipole transition. A single photon

electric dipole transition is forbidden by the selection rules. Which of these two modes of decay is dominant depends entirely upon Z . That is, there is a cross-over point between systems of low charge, where it turns out that two-photon decay dominates, and systems of high charge, where the magnetic dipole decay dominates. For argon, which has $Z = 18$, it turns out that the two-photon mode is still dominant.

The situation for the helium-like system is similar. Once again we have an S ground state and a metastable state at some distance above it. There will be two types of spin states, singlet states and triplet states, depending on how one couples the two electronic spins available. The only probable decay mode of the metastable singlet state turns out to be the $2E1$ transition; $M1$ radiation is not possible. Then we come to some of the states that we talked about last time, the triplet states where the spins line up. Notice the triplet states follow what is known as Hund's rule; states with higher multiplicity in general lie lower than states of lower multiplicity. Hence a triplet state lies below the corresponding singlet state. In addition, there are some metastable P states shown on the diagram. These owe their metastability in part to the fact they belong to the triplet system. There will be three of them, depending on how the spin $S = 1$ couples to $L = 1$; $1 - 1 = 0$, $1 + 1 = 2$, and the intermediate case is $J = 1$.

Last time we talked mostly about the decay of the $J = 1$ level for the helium-like ions of oxygen and fluorine. The intermediate J state prefers to go to the ground state by emitting an electric dipole photon. (This decay mode is not shown on the diagram.) This, however, is accompanied by a change in the spin direction of the electron making the transition. As it goes from $n = 2$ to $n = 1$, the electron must change its spin so that its spin can be paired against the spin of the electron already in the K shell. That's what makes the $J = 1$ level metastable. However, the effect of increasing Z is so strong that for $Z = 18$ it looks very much like an allowed transition, so for argon that is no longer a legitimately metastable level.

Let's consider the other two triplet P levels. We have a $J = 0$ state and a $J = 2$ state. How do they decay? The $J = 0$ state, of course, can decay to the lowest state of the triplet system, which it

could do in ordinary helium as well. It cannot decay to the ground state because of the rule forbidding $J = 0$ to $J = 0$ transitions. For a long time it was believed that the two photon transition from this state to the ground state was dominant as for the hydrogen-like system. Then about 3 or 4 years ago a single line of frequency corresponding to the energy difference from the ground state was discovered in the sun.²⁾ From that day forward it has been believed that it is a magnetic dipole transition. The $J = 2$ level also has the possibility of decaying to the ground state of the triplet system. The selection rules also permit it to decay to the ground state by means of a magnetic quadrupole transition; the quadrupole comes from the fact one can connect a $J = 2$ level to a $J = 0$ level. In fact, only magnetic quadrupole radiation is not forbidden. It turns out that electric multipole radiation is forbidden in all orders, and magnetic multipole radiation is forbidden in every order except the quadrupole order; magnetic octupole and higher are forbidden, and magnetic dipole radiation is forbidden. That's a singular, interesting case.

The next diagram (Fig. 2) is a repeat; I show it just to

Z DEPENDENCE OF RADIATIVE TRANSITION PROBABILITIES

Electric Dipole Approximation

$$\exp(i \vec{k} \cdot \vec{r}) \sim 1 + i k r_n \cdot \vec{r} \sim 1 + i Z a \quad \text{for } Z a \ll 1$$

$$E1 \text{ Transitions} \quad \gamma \sim \alpha (Z a)^4 \frac{m c^2}{h} \sim 10^3 \text{ sec}^{-1} \cdot Z^4$$

$$M1, E2 \text{ Transitions} \quad \gamma \sim \alpha (Z a)^6 \frac{m c^2}{h} \sim 10^5 \text{ sec}^{-1} \cdot Z^6$$

$$E3 \text{ Transitions} \quad \gamma \sim \alpha (Z a)^8 \frac{m c^2}{h} \sim 10 \text{ sec}^{-1} \cdot Z^8$$

Special Cases:

Parity forbidden $2S_{1/2} - 1S_{1/2}$ decay of one-electron ions (M1)

$$\gamma \sim \alpha (Z a)^{10} \frac{m c^2}{h} \sim 10^{-4} \text{ sec}^{-1} \cdot Z^{10}$$

Two Photon Emission

$$\text{One-Electron Ions } \gamma \sim \alpha^2 (Z a)^6 \frac{m c^2}{h} \sim 10 \text{ sec}^{-1} \cdot Z^6$$

$$\text{Two-Electron Ions} \quad \sim 10 \text{ sec}^{-1} \cdot (Z a)^6$$

Fig. 2

remind you of how these things scale with Z . The approximate magnitudes

of the various radiative transition probabilities are shown. For electric dipole transitions the scaling rule is Z^4 . That comes from the truncation of the exponential in the radiative transition matrix element at 1. All of the higher multipoles come from further terms in the expansion of this exponential. These terms have approximate magnitude $(Z\alpha)^{2n}$ relative to the leading term. Thus, the electric dipole transitions vary like Z^4 , the magnetic dipole and electric quadrupole like Z^6 ; and the electric octupole like Z^8 . Then there are these special cases, many of which we have just seen on the previous slide. There is the magnetic dipole decay of a $2S_{1/2}$ to a $1S_{1/2}$ state. It turns out that this is forbidden not only by angular wave function selection rules, but also by the fact that nonrelativistically the $2S$ and $1S$ radial matrix elements are exactly zero. Only in the relativistic corrections does one find the radial matrix elements slightly non-zero. The upshot of this is that the scaling with Z involves a particularly high power, just as for the intercombination line in helium. The scaling goes like Z^{10} . It turns out that the indicated numerical factor, which is just a dimensional factor that comes from calculating the relevant power of α and the electron rest energy over $\hbar$, is quite a bit off. The reason for this has to do with the need to calculate relativistically the rather subtle non-vanishing of the radial matrix elements. The two-photon emission is down by an extra factor of α , and the scaling rule there is Z^6 . The leading α in front of each one of these expressions indicates that one has a single photon. The α^2 indicates two photons.

For two-electron ions, the scaling rule is very nearly the same except that the nuclear charge is screened by the extra electron. The screened charge ought not to be exactly $Z - 1$, but rather Z minus some screening constant. In addition to that, there is another interesting phenomenon. The two-electron ions have two electrons in their ground state K shell, either of which could be involved in the decay. It turns out that introduces an extra factor of 2 in the transition probability expression in the limit of large Z compared with the corresponding two-photon transition for a one-electron ion. So these don't approach precisely the same limit for large Z . They approach limits that are a

factor of two apart, essentially because of the Pauli principle.

Last time we talked about intercombination line lifetime measurements and their Z^{10} scaling. That's another case that scales at a large power of Z . I have slides illustrating some of the other processes we have just discussed. I borrowed these from Dr. Richard Marrus, who has been studying a number of these metastable-state decays for the one- and two-electron argon ions at the Berkeley HILAC. Last time I showed you one of our own Si(Li) detector spectra for the fluorine ion $2^3P - 1^1S$ line. Let me just remind you what that looks like (Fig. 3).³⁾

Fig. 3

The top spectrum is a calibration spectrum. Fig. 4 shows the lifetime curve associated with the fluorine ion intercombination line.³⁾

Fig. 5 is a diagram of the Berkeley experiment.¹⁾ You can see that in principle it is quite similar to the setup we have already described; note the steering magnet, movable target, and detectors with respect to which that target is moved for the purpose of making lifetime measurements. One difference is the use of two detectors because for

Fig. 4

Fig. 5

two-photon decay studies one wants to have two detectors that can perform a sum, or perhaps detect them in coincidence, to make sure that they are genuinely emitted at the same time. Another difference is the presence of a stripping foil upstream from the steering magnet, which allowed them to bend either fully-stripped ions or hydrogen-like ions into their experiment, one species at a time. By adjusting this foil thickness so that it was thinner than required to achieve charge-state decay equilibrium, it turned out they were able to optimize production of the one-electron or two-electron charge states for the purpose of maintaining charge state purity in their experiments; they could have some control over whether they had primarily one- or two-electron ions in their experiment. Otherwise, the experimental arrangement is very much the standard kind that we have discussed earlier.

There is a difference in the spectrum obtained, as we see in Fig. 6.¹⁾ The spectrum looks somewhat similar to the one we just saw

Fig. 6

for the fluorine intercombination line. The principal difference is that in addition to the line coming from the metastable state of interest, there is a two-photon continuum. The spectrum of the two photons that sum to the total energy of the transition has to be symmetrical; there is no reason for one photon to have a greater share

of the energy than the other. So if one takes half the energy of an $n = 2$ to $n = 1$ transition, then the frequency spectrum of the photons has to be symmetric on each side. The noise was obviously problematic, but they were able, by means of doing a coincidence check, to show that there were coincidences where the sum energy of the photons added up to the energy of the transition. That's the origin of the two-photon continuum.

Fig. 7 is a decay curve,¹⁾ a plot of the logarithm of the

Fig. 7

radiation intensity versus target-detector separation. From this one can extract an exponential lifetime. This particular decay curve is due to the hydrogen-like argon ion, which the diagram notes decays primarily by two-photon emission. The predominance of two-photon emission here is nearly complete, but if one went from $Z = 18$ to about $Z = 45$, one would find the two-photon decay rate and the magnetic dipole decay rate for this transition to be comparable. For Z greater than this, the magnetic dipole radiation will dominate.

Fig. 8 shows a similar decay curve, this time for the metastable singlet state of helium-like argon.¹⁾ The origin of the background is not really known. There have been some suggestions about what

Fig. 8

it is, but its origin is as yet not established.

In Fig. 9 we have curves for some triplet state decays.¹⁾

Fig. 9

In our experiments, we had been measuring the intercombination line from the $J = 1$ level for somewhat lower Z . The Berkeley experiments examined the magnetic dipole decay of the 3S state and the magnetic

quadrupole decay of the $J = 2$ level of the 3P state. Fig. 9 shows the resolution of these decays into their respective exponential components. There is a rather small amount of the longer-lived exponential present, so the uncertainty associated with that measurement is fairly large. There is a substantial piece of the M2 part of the decay present, so a pretty good determination of that lifetime is possible, as you can see from this slide.

Fig. 10 gives a comparison of experimental and theoretical

THEORETICAL AND EXPERIMENTAL LIFETIMES (10^{-9} sec)

	Experiment	Theory
$\text{Ar}^{+17} 2s$	3.54(25)	3.46
$\text{Ar}^{+16} 2^1S_0$	2.3(3)	2.35
2^3S_1	170(30)	212
2^3P_2	1.7(3)	1.49

Fig. 10

results.¹⁾ The theory is from various people. As you can see, for the most part the agreement is fairly good. The longest lived decay, the one where we just saw that not much of the decay curve was on the plot of radiation intensity versus distance, shows perhaps a discrepancy, but it is really hard to say. The quoted error limits suggest that, but I am not sure that the uncertainty is not somewhat larger than quoted. The 2^3P_2 lifetime is a little bit surprising; it seems to be a bit far from the theory. But otherwise things are in pretty good shape. Apparently one understands how to calculate these forbidden decay rates fairly well, at least for Z as high as in argon.

I would like to say one thing more about that magnetic quadrupole transition. It turns out that the analogous line in other ions is frequently observed in spectra of the sun and of other stars. This comment isn't restricted to the quadrupole line; it is certainly true of the intercombination line in the helium-like ion as well, which is

observed in the spectrum of the sun for all elements from carbon through iron. It is one of the most intense features of the solar spectrum in the wave length region where these lines lie. There is something special about the magnetic quadrupole radiation, which I alluded to earlier. The M2 transition is unique. This is the subject of Fig. 11. I mentioned

$s^1S_0 - sp^3P_2$ TERMS

ALL ELECTRIC MULTIPOLES FORBIDDEN
(E1 SINCE $\Delta J=2$, E2 BY PARITY, E23 BY $0 \leftrightarrow 2$ RULES)

M1 FORBIDDEN ($\Delta J=2$)

M ≥ 3 FORBIDDEN ($0 \leftrightarrow 2$ RULES)

ONLY M2 IS ALLOWED

Fig. 11

before that radiative deexcitation is forbidden to all orders for electric multipoles. The reason for that is quite clear. Furthermore, this is not just for the 3P_2 to 1S_0 type of transition in helium-like ions; every ion in any charge state that has an excited 3P_2 level cannot decay to a lower lying 1S_0 ground state, except by magnetic quadrupole radiation. All electric multipole radiation is forbidden: electric dipole radiation since an angular momentum change of two is required, electric quadrupole radiation because of the parity, and all electric multipoles greater than or equal to 3 because of the $J_1 + J_2 \geq K$ rule for a 2^K -pole transition. In this application it means $J = 0$ and $J = 2$ cannot be coupled radiatively. The situation is the same for the higher magnetic multipoles. Magnetic dipole radiation is forbidden because of the total change in angular momentum. The upshot is that the level just can't radiate except in the single order of magnetic quadrupole radiation.

What I am going to discuss next is the spectroscopy of

electrons emitted from metastable states of some of these ions that do not decay very rapidly radiatively because of selection rules, but which decay instead by emitting electrons. Emission of electrons as opposed to the emission of photons certainly is a permissible way to reduce the excitation energy of the system. The strongest interactions that cause excited states of ions to emit electrons are the Coulomb interactions. The interelectron Coulomb repulsion terms permit some electrons to reduce their energy and others to be ionized from the system. This phenomenon is known as autoionization. It is sometimes forbidden by a different set of selection rules, analogous to the optical selection rules, but having somewhat different structure. That is the topic I want to discuss next. In many cases these states cannot be seen radiatively; they can be seen to decay only in the autoionization decay channels.

The electron experiments represent novel variations of many things we have discussed earlier. One of the characteristic features of the optical work is an optical spectrometer, or at least a detector that has some discrimination against the wavelengths of unwanted radiations. Needs are similar in studying autoionization electrons emitted by excited ions. One needs a spectrometer to do that. The type of spectrometer we use is schematically indicated in Fig. 12,⁴⁾ and is a

Fig. 12

cylindrical capacitor device. These cylindrical electrodes are symmetrical about the direction of the entering beam, which can be excited in a movable foil target and then allowed to decay in flight as it passes on its way to some beam dump further down the line. The beam emits all kinds of photons as well as a lot of autoionization electrons. If it happens to emit electrons while it is passing by the small region viewed by two annular slits that restrict entrance into the region between the cylindrical electrodes, the electrons that make it through the slits find themselves in an electrostatic field between these electrodes. That electrostatic field is adjusted in such a way as to bend electrons of a certain energy so that they pass through exit analyzer slits and are detected.

The way the experiments are done is to excite the beam in some kind of movable foil, to sample the electrons emitted by the beam from the small region viewed by the spectrometer, and to count them as a function of their emission energy using some detector at the exit of the device. The reason for the design is to get some appreciable fraction of the 2π geometry available from the annular structure of the entrance slits. This particular unit is a small device. You can see how small in Fig. 13, which is actually a photograph. The chamber is about 30" in diameter; the spectrometer is the device clamped between the two insulating plates seen in the photograph.

The analyzer can be calibrated with elastically scattered electrons from a target gas leaked into the region near its entrance. In Fig. 14 we show a fairly symmetric looking resolution function, with resolution of a fraction of a percent,⁵⁾ determined mainly by the overall length of the device and the widths of the entrance and exit slits, which at the moment are a few thousandths of an inch each.

Fig. 15 is devoted to explaining schematically what goes on in autoionization. I chose helium as an example because that is the simplest case I can imagine. Suppose one has a helium-like, two-electron system. There will be a bound state, the ground state of the system, and then the first excited state, a one-particle excited state where one electron is in the $n = 1$ level and the other has $n = 2$. There will be a sequence of higher one-electron excited states, which are not shown in

Fig. 13

the diagram, up to a series limit above which the running electron is no longer bound to the system. These are the one-electron continuum states. Now there is a possibility that not just one of the electrons, but rather both of them are excited, say up to the situation where both of them are in a $2s$ state. They interact of course, but never mind the interaction; for the purposes of this argument, it doesn't matter. If both of them are excited, one has roughly twice the excitation energy. That means that the total electronic energy of the system lies a larger distance above the ground state of the system than the one-electron ionization limit does, so that this level is adjacent to some continuum states. If there is any interaction that couples the electrons in this state to the one-electron continuum states, then the system can auto-ionize, one electron being ejected into the continuum and other electron falling down into a lower level, for instance the ground state. Usually this coupling is quite strong; it's just the Coulomb repulsion between the two electrons. The rate is calculated essentially by the Golden

Fig. 14

Fig. 15

Rule. One has $2\pi/\hbar$ times some matrix element squared times some density of states. The interaction operator is sandwiched between the initial and final wave functions that describe the system. The normal interaction is just the Coulomb repulsion between the electrons, and the rates one typically gets tend to fall in the range 10^{12} to 10^{15} sec^{-1} . So, by and large, autoionization lifetimes are short compared to radiative lifetimes, except for the radiative lifetimes of systems of very high Z . There is also an approximate rule for scaling of the autoionization lifetimes versus principal quantum number n , which is given on the last line of this diagram.

So far we have described ordinary permitted autoionization. There is an interesting inverse process that comes up frequently in astrophysics. The atmospheres of stars tend to be low density plasmas with a lot of fairly highly ionized atoms immersed in an electron gas. Fig. 16 describes a process called dielectronic recombination that works

Dielectronic Recombination

$$\begin{aligned}
 e(E, \ell') + X^{+I}(i) &\rightarrow X^{+(I-1)}(J, n, \ell) \\
 &\quad \uparrow \\
 &\quad \text{Doubly Excited} \\
 X^{+(I-1)}(j, n, \ell) &\rightarrow X^{+(I-1)}(k, n, \ell) + h\nu \\
 W(j, n, \ell) A_E(j, n, \ell \rightarrow i, E, \ell') &= \\
 = W(i) \frac{16\pi m}{h^3} \sigma(i, E, \ell' \rightarrow j, n, \ell) E dE
 \end{aligned}$$

Fig. 16

just the opposite way from autoionization. Imagine a collision where some electron bumps into an ion in a charge state that is labeled $+I$ in Fig. 16. It is possible for it to stick if some other electron already bound to the ion is bumped up to a higher level. So the process is that an electron hits an ion, and a bound electron is bumped up; energy conservation then permits the incident electron to stick. If the reaction were reversed, one would have just a doubly excited system

reducing its excitation, changing its charge by one unit and emitting a free electron of some total energy E and angular momentum ℓ' . The recombination process turns out to be important in the study of ionization equilibrium in stellar atmospheres. It's not directly relevant to the present talk, but I wanted to mention the connection.

There are selection rules for the permitted transitions in Coulomb autoionization, and then there are violations of these selection rules, just as in the optical case we have already discussed. Fig. 17

AUTOIONIZATION SELECTION RULES (LS COUPLING)

	ΔL	ΔS	ΔJ	PARITY
COULOMB	0	0	0	CONSERVED
SPIN-ORBIT	0, ± 1	0, ± 1	0	CONSERVED
SPIN-SPIN	0, $\pm 1, \pm 2$	0, $\pm 1, \pm 2$	0	CONSERVED

EXAMPLE: $(1s\ 2s\ 2p)\ ^4P_{3/2}^o \rightarrow (1s^2)\ ^1S_0 + k\ ^2F_{3/2}^o$

TO CONSERVE PARITY, L MUST BE ODD

TO CONSERVE J , $L = 2, 3$

HENCE, $k\ ^2F_{3/2}^o$ IS THE FINAL STATE;

ONLY THE SPIN-SPIN INTERACTION IS EFFECTIVE

Fig. 17

shows some of these selection rules. These are autoionization selection rules in the LS coupling limit. The top line indicates the permitted Coulomb processes. The total orbital angular momentum of the electrons in the system cannot change. The interactions are Coulomb interactions, so they will not flip any spins; the total electronic spin of the system must be conserved. The total angular momentum of the system, including its total spin and orbital contributions, cannot change, and because the process is electromagnetic, parity is conserved. These last two columns represent required conditions for all the interactions. We have to conserve the total angular momentum of the system, including the angular momentum of the emitted electron, and we have to

conserve the parity of the system, including the parity associated with the outgoing electron. Most helium-like autoionization states decay by permitted channels.

There start to be an abundant supply of metastable states for systems containing three or more electrons. Probably the simplest example is the one given in Fig. 17. Imagine a system having some excitation energy, a $(1s, 2s, 2p)$ state. That's a configuration of a three-electron system. It looks like it is singly excited, but it turns out there is enough energy in the system to expel one of these electrons and permit the other to go to the K shell, and that is what is required here, that the state of the system be adjacent to some ionization continuum. One important thing about this configuration is the K hole in it. Another important thing is that the state is a quartet state, having spin $3/2$. That means the spins of all the three electrons are parallel to each other; we are talking about the state of maximum spin. That turns out to have something to do with the metastability of the system. Spin $3/2$ can be added to the orbital angular momentum $L = 1$ in three different ways; one can form $J = 1/2, 3/2$ and $5/2$ states. It turns out that the state with maximum J and maximum spin is most metastable of all.

The excitation energy is available for autoionization provided one of these electrons can reach the ground state. To see if it is possible to make that excitation energy available, one has to worry about the violations of the selection rules we have listed. In particular, it is clear that Coulomb autoionization can't happen at all for the following reasons. The outgoing electron, the free electron that is emitted, must be emitted in an angular momentum state having odd L , because there is no angular momentum in the ground state of the adjacent helium-like ion; the outgoing electron carries it all. The emitted electron is in a doublet state because the free electron has just two possible spin directions. The system starts out in an odd orbital angular momentum state; it therefore must end in one, so L must be odd. On the other hand, if you are talking about a $J = 5/2$ state, since J must also be conserved, that restricts the possible L values of the emitted electron to two values, 2 or 3. Three is the only odd number, so the

emitted electron must go out in an F state, and it must have $J = 5/2$ total angular momentum. So $^2F_{5/2}$ is the only possible final state permitted by conservation of total angular momentum. That means that there has been a change in spin of one unit, going from a quartet to a doublet, and a change in total orbital momentum of two units. You can see by examining this chart (Fig. 17) that not only is Coulomb autoionization forbidden, but spin-orbit autoionization processes are forbidden as well. The only way that the system can autoionize is through the spin-spin interactions among the three electrons. If one is able to measure the rate for a process like this, one would isolate the contribution of the spin-spin interactions for these systems. This changes as a function of Z ; the further up the periodic table, the stronger these interactions become. There is a predicted scaling with Z that is of fundamental interest to test. Of course, that's what we have been trying to do.

Concerning the other possible J states, the $J = 1/2$ and $J = 3/2$ states, it turns out that they also cannot autoionize by the Coulomb interaction, but that the spin-orbit interactions can make autoionization happen. Thus, $J = 3/2$ and $J = 1/2$ states will decay somewhat faster than a $J = 5/2$ state, but not as fast as a state in which Coulomb autoionization is permitted. We have done a number of experiments over the past couple of years on many of these three-electron ion states for several different Z . This is not the only important state. There are other states one can imagine, for example $(1s, 2s, 3s)$ and so on. The important considerations have been, do the spins line up, and is the total angular momentum of the system large? If so, the system is likely to be metastable. There are three types of problem one can study. One is, what are the energies of these states; that is the spectroscopic aspect of the problem. The second type of problem is, what is the lifetime of the state; that's what I have talked about mostly for the radiative processes. How fast do metastable states decay? How well can the violations of the selection rules be calculated? The third type of thing one can study is, how commonly are these states formed? Are they formed a big percentage of the time, a negligibly small percentage of the time, or somewhere in between? Are they rare, exotic, or are they common? I will discuss these questions more or less in order.

(1s, 2s, 2p) state of maximum spin and maximum J, which shows up at the right energy. There has been no normalization of the energy scale here; the comparison between theory and where the electron actually occurs is a good one. The other bumps turned out to be associated mostly with higher excited states of the three electron system that are partially metastable against autoionization because of high spin. They are somewhat radiatively unstable because many of these higher excited states can decay to the lowest state of spin 3/2 by ordinary electric dipole processes. However, radiative processes generally have small transition probabilities compared to Auger processes, and for that reason one can look upon them as metastable at least from the Auger point of view.

What we will do next is look at these peaks in more detail, that is, under conditions of higher resolution. Fig. 19 covers some

EXAMPLES OF NOTATION

Fig. 19

matters of notation. All those labels that you saw in the earlier slide, and which you will see on subsequent slides, label states in this way. Quartet states mean spin 3/2; S or P means the total orbital angular momentum of the state is 0 or 1 respectively; e or o refers to the parity being even or odd. The 1, 2, and 3 refer to the ordering of the states energetically; that is, for a ${}^4S^e$ symmetry, the (1s, 2s, 3s) state lies lowest, the (1s, 2s, 4s) state lies next lowest, and so on up the line. The notation for ${}^4P^e$ is similar; 1s2p² is the lowest state of this symmetry, the next one in the series is 1s2p3p, and so on.

Fig. 20 shows the largest feature from the coarse spectrum

Fig. 20

under circumstances of higher resolution.⁶⁾ We see the peak from the $(1s, 2s, 2p)$ state, and an adjacent bump that turns out to come from the $(1s, 2p^2)$ state. That state is less metastable than the first one. For one thing, an electric dipole photon can couple the $(1s, 2p^2)$ state to the $(1s, 2s, 2p)$ state, so the $(1s, 2p^2)$ survives a shorter time. I should point out that a spectrum like this is taken with the viewing slit at about 2 cm distance from the foil target; at that position a lot of the $(1s, 2p^2)$ has already decayed, presumably by radiative processes. Then there are some more features here that do not fit in the quartet term scheme.

You may have noticed that the electron energy scale here is different from that in Fig. 18. The reason is that the electrons emitted by these ions are emitted from moving systems; the ions have some velocity in the laboratory. The kinematics require that the total laboratory

electron energy has to be related back to what the energy of emission was in the rest frame of the emitter; that is the scale in Fig. 20. This is the type of figure one would want to look at to compare with theoretical structure calculations. You see a slight mismatch on the energy. It turns out that the precision of our energy determination is of the same order as the precision of the variational principle that was used to calculate the positions of these states.⁷⁾

The energy of the emitted electron in the rest frame of the emitter is determined by taking the total electronic energy of the initial state and subtracting from that the energy of the ion that is left over after the electron is emitted. The initial state is a three-electron excited state and the final state is a helium-like ground state ion plus a free electron, so the energy of the emitted electron is just the difference in energy between the helium-like ion and the total electronic energy of the excited three-electron state.

Fig. 21 shows another example of an oxygen spectrum and a

Fig. 21

fluorine spectrum.⁸⁾ All the same states reoccur for the latter, of course at somewhat different energies. Fig. 22 shows some of the higher excited states,^{5,8)} the ones in the smaller bump in the first spectrum that we looked at. One sees a substantial amount of unresolved

Fig. 22

autoionizing activity. Some of the activity is near where states have been theoretically calculated, other features are not. There are two "series limits." One can talk about $(1s, 2s, 3s)$, $(1s, 2s, 4s)$, $(1s, 2s, 5s)$, and so on. Eventually one has $1s, 2s$ plus a "free" electron. If the last electron were just barely tied on, were almost not bound, we would then expect the emitted electron energy for the state to correspond to the first ionization potential of the helium-like system. That's what the "series limit" tick marks represent; the triplet states of helium-like ions as determined from spectroscopic tables are located at these positions. The amount of Auger activity drops off just as one approaches these series limits; the analysis works very nicely.

There is one high energy feature in these spectra that is prominent compared to the bumps from all the other higher excited states, for which there is an interesting reason. We think it is quite likely this feature should be assigned to a $(1s, 2p, 3p)$ state.⁸⁾ It is possible for that state to emit an electric dipole photon and reach the $(1s, 2s, 2p)$ state. For the first state the parity is even, and for the second state the parity is odd, so E1 rules are obeyed. The

6

jumping electron changes orbital angular momentum by one unit, and the selection rules on L and J are easily obeyed. However, there is a 2s electron in the final state that was not there in the initial state, and there is a very significant difference in shielding for the 2p electron *vis-à-vis* the 2s. This difference in shielding is enough to reduce the dipole emission probability for this transition by a significant amount; there is an anomalously small matrix element for this process because of the radial adjustment, the extra shielding correction. We are pretty sure that is responsible for the survival of this particular excited state at some 2 cm distance from the point of excitation over and above the other states.

Question: How have you determined experimentally that you are observing electron emission only from three-electron systems, rather than possibly some of these bumps arising from four-electron or five-electron systems?

Answer: Four- and five-electron system emission is indeed possible; I haven't come to that yet. In fact, that is very likely because there are some features in the spectrum that cannot be accounted for from known three-electron states. The only handle we have on that is comparison of spectral features that appear with theoretical calculations of what the state energies are. So the information we have is spectroscopic in character; if we see a peak that can't be assigned to the three-electron system, it is likely it comes from some other charge state.

Question: Do you have information on the yield variation with energy?

Answer: Yes, we have some information of that kind. Doing this kind of experiment is a bit difficult because of the tremendous spray of electrons ejected from the target, more or less in the forward direction. The higher the beam energy goes, the more fierce this problem becomes. Several electrons per ion are ejected, perhaps even as many as ~ 10 per incident ion, that have nothing to do with these processes. If these leak into the spectrometer, it really gives a serious background problem. Our ability to raise the beam energy up to where we would like

to have it in order to explore exactly what you are asking about has been somewhat limited by this.

These states recur in ions further up the periodic table. For instance, Fig. 23 shows the (1s, 2s, 2p) feature for a three-electron

Fig. 23

chlorine beam.⁹⁾ It appears right where we expect to find it. By the time we reach this Z, there is another type of effect that occurs. Notice the energy scale here. Fig. 24 gives an experimental value for the total electronic energy of the state as determined essentially from the data in Fig. 23. We also did a similar experiment on Ar^{15+} , so we know something about the energy of that system also. What is interesting is that the electrons are becoming sufficiently relativistic that already about 75 volts of the binding energy of the K-shell electron is the contribution from the relativistic mass shift of the electron. This number is fairly large compared to what we think is the experimental uncertainty, a factor of three larger anyhow, so it is possible to think about measuring some of the relativistic corrections to atomic quartet binding energies. There are, of course, compensating

$4p_{5/2}$ State Energies

Expt. Value (keV)	Relativistic Ke ⁻ mass Effect (eV)	Relativistic Energy (Total) (keV)
Cl ¹⁴⁺ -5.658 ± .024	-75	-5.679
Ar ¹⁵⁺ -6.369 ± .033	-95	-6.387

Fig. 24

relativistic effects that cancel a good deal of this; these are just the leading terms.

I think I will stop here; next time we'll take up the topic of how commonly these states are formed in collisions.

Question: Could you compare your experimental relative intensities for these lines with calculations?

Answer: All of the spectra we have looked at so far share one thing in common; almost all of the activity that one sees is associated with metastable states. It has been very hard to make the type of comparisons you are asking about because there is almost no theoretical lifetime information for any of these states; the lifetime information that is known is experimental in character. There are two ways for us to do a better job of identifying states. One is to do a better job of measuring state energies through better resolution and so on. The other would be to utilize theoretical lifetime information, but there is almost none. There are a great many radiative lifetime calculations in the literature, but there are very few metastable state lifetime calculations for any of these Auger processes.

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Fig. 1

number of spectral features near the three-electron states in fluorine and in oxygen.¹⁾ Under conditions of higher resolution this has been split into a number of states. These are reviewed on the next two slides. Fig. 2 shows the larger group split into a $(1s, 2s, 2p)$ state, with spins aligned, a $(1s, 2p^2)$ state, with spins aligned, and then

Fig. 2

some previously undescribed features, which I want to talk about more today.²⁾ These spectra were accumulated at target-spectrometer separations on the order of 2 cm, so that from the point of view of auto-ionization, the states are fairly metastable. Fig. 3 shows the high

Fig. 3

energy groups involving at least one electron in an $n = 3$ or higher level.^{2,3)} There are some states associated with $n = 3, 4$, or 5 , and then a whole unresolved mixture of states up to the ionization limit established by the electron being at the limit of binding to a $(1s, 2s)$ core or a $(1s, 2p)$ core, with the spins aligned.

Now we turn to the question of how commonly these states are formed. Are these states exotic or not? Fig. 4 gives information

Fig. 4

on the yield, the total number of these ions that are formed per incident ion.⁴⁾ The horizontal axis expresses the percentage of the ions that were incident on the target that emerged in this particular kind of excited state. The $(1s, 2s, 2p) 4p_{5/2}$ state is a fairly exotic state. The spins are aligned; J is maximum. This yields the largest peak on the previous spectra that you have examined. From the horizontal scale

one sees that for appropriate beam energy, nearly 1% of the total number of incident ions emerge in that state. We regard this as fairly remarkable. Of course there are experimental uncertainties associated with this measurement, but even so it seems a true statement that roughly 1% of the time that happens for both oxygen and fluorine. Since these are only the very longest lived metastable states, the conclusion is that the formation of states of high angular momentum and high spin is common. After all, even the metastable ones account for something like 1% of the total beam.

For comparison purposes, there are some dashed curves in Fig. 4 that represent equilibrium fractions; the charge states of the beam approach an equilibrium with respect to each other. The lithium-like, three-electron beam component is labeled ϕ_5 for the case of oxygen and ϕ_6 for the case of fluorine. The other curves are, respectively, the helium-like fraction and the beryllium-like fraction. You can see that the yield curves follow the lithium-like fraction fairly well. A model for the formation of the states can then be guessed. One possible model, the one that appeals to me most, is a sequential process involving first formation of a metastable helium-like core in a triplet state, followed by addition of the last electron to make a quartet state. It appears that the peaks occur at somewhat lower energy than the maximum of the three-electron charge state equilibrium fraction curve. One might imagine that the peaks should actually fall in between the lithium-like charge state and the helium-like charge state, inasmuch as the total amount of electronic energy of the excited states lies in between the three-electron system ground state and the two-electron system ground state. The solid curve doesn't do that; it seems to peak on the lower ion energy side a little bit, but for the most part it follows the corresponding equilibrium charge state distribution in shape quite well. That's the story for the three-electron ions. We'll talk a bit more later about systems with more electrons.

Question: Did you do the experiment also with argon?

Answer: We have done experiments like this on both argon and chlorine beams. The data have not been analyzed as carefully as this,

but the numbers are of the same general order, on the order of a substantial fraction of 1% yield for this particular state.

In more recent times, we have grown interested in how much autoionizing activity one will get if the metastability requirement is relaxed. That is, we have looked at some of the short-lived states using smaller target-spectrometer separations. We have done this in two ways. At the lowest energies, we have managed to put the target right in the field of view of the spectrometer. At somewhat higher energies, we have to back off a little bit to keep from getting swamped by the forward spray of free electrons ejected by the passage of the ion through the target foil. Fig. 5 shows an unresolved spectrum.⁵⁾

Fig. 5

It displays many of the features we have already seen, but with the target directly in view. The height of the $4p^0(1)$ state on the previous slide is represented approximately by the height of this small vertical bar. It appears here as a slight shoulder on the nearby $(1s, 2p^2)4p^e(1)$ state. One sees that the shape of the higher energy part of the spectrum is different from what it was; the entire region

between the low energy group and the high energy group has been filled in by a very considerable number of as yet unidentified states. Many of them can be assigned, it turns out, to short-lived states of the three-electron system. The feature labeled B here is not aligned with the peak. The position of B was taken from the earlier slides. The states near B are not associable with the three-electron system according to recent theoretical calculations.⁶⁾ So those states remain quite ambiguous; they don't seem to fit into a three-electron term scheme at all, and their origin is unknown.

The spectrum in Fig. 6 was obtained at somewhat higher energy

Fig. 6

by backing the foil target out of the viewing region a small amount, perhaps on the order of 0.020 inch.⁷⁾ Once again we see a substantial amount of additional activity. Our old friends are on the left. State A now is quite precisely located; it turns out to agree to within about 0.2 electron volt with a metastable state of the three-electron system of the form $(1s, 2p^2)^2P^e$, doublet as opposed to quartet. This state has a certain limited metastability because its decay is inhibited by

the selection rules on total angular momentum and on parity, but it is not as strongly inhibited as the quartet states, because of the lower spin, so it is a shorter-lived state. We have been able to follow the decay of this state to some extent, and to get crude lifetime information on it. State B is still with us, and it remains a puzzle; there are no states with which B can be tied at this time in either the three- or four-electron systems, at least none that have been calculated. There is so much activity in the higher energy group that it is rather difficult to resolve. We have taken many more of these close-up spectra recently, but I have no slides available at this time.

It is evident that one can make lifetime measurements by the decay in flight method just as one does in the optical case. We spent substantial time measuring metastable state lifetimes for the specific purpose of measuring the strength of the spin-spin interaction in these ions. I have a number of examples of decay curves, which I will run through rather quickly.

Question: Before we leave the subject of looking at the target, did you look at low energies to see the target activity?

Answer: You mean the number of electrons from the target? It gets progressively more difficult to do that because the spectrum of the splatter from the target is shaped like this:

In fact, we had to lower the beam energy down so this tail would be small, and moving to the left to look at the low energy electrons gets us into the tail.

We have collected a variety of lifetime data over the past couple of years. We measured the decay of the $(1s, 2s, 2p)^4P_{5/2}$ state,

the lowest quartet state. This is shown in Fig. 7.¹⁾ Up close there

Fig. 7

is evidence of cascading, and also, probably even more so, the presence of the other J states. The $J = 3/2$ and $J = 1/2$ states, which can decay by the spin-orbit interaction as well as the spin-spin interaction, contribute to the deviation of this lifetime curve from the tail into which it eventually gathers itself. Fig. 8 shows similar data, except for an oxygen beam rather than for a fluorine beam.¹⁾

Fig. 9 shows the same kind of thing, except this is for the principal feature of the higher energy group.²⁾ Remember, I told you in the previous talk why the lifetime of that state is larger. If one asks how the (1s, 2p, 3p) state decays to the (1s, 2s, 2p) state radiatively, the crude answer is the 3p electron falls into a 2s hole. That changes the shielding of the 2p electron that is already there

Fig. 8

Fig. 9

by a significant amount, so one can view the process as something like a two-electron transition. It is not really a two-electron transition, but both electrons have to adjust, so this particular state is long-lived in terms of what one would expect considering the energy available for the optical transition. The lifetime is determined also by the optical decay channel, but shows up in the autoionizing decay channel, so one can learn about optical lifetimes in the Auger channel as well.

Fig. 10⁸⁾ shows a decay curve for Cl^{14+} , three-electron Cl.

Fig. 10

The same state occurs as for highly stripped fluorine ions. Fig. 11 shows the same thing for argon ions.⁹⁾ Argon ions don't come out of tandem Van de Graaffs, of course. We have to do this experiment at the Oak Ridge cyclotron. That is all the lifetime data; I don't want to dwell on it since you have seen so many of these curves.

Fig. 12 summarizes the lifetime information obtained from these experiments.¹⁰⁾ The experimental values are all our values. In the column containing theoretical values, as I noted last time, there is a difficulty because there are no values. You do see here some semi-empirical scaling rules suggested by workers at the Columbia

Fig. 11

$$\gamma(4P_{5/2}) \sim 10^5 (Z - 1.8)^3 \text{ sec}^{-1}$$

$$\gamma(4P_{3/2}) \sim 10^4 (Z - .4)^6 \text{ sec}^{-1}$$

$$\gamma(4P_{1/2}) \sim 10^5 (Z - 1)^6 \text{ sec}^{-1}$$

$4P_{5/2}$ Lifetime	Expt. (nsec)	Theory (nsec)
O^{5+}	25 ± 3	$31 \pm ?$
F^{6+}	15 ± 1	?
Cl^{14+}	$.91 \pm .04$	?
Ar^{15+}	$.66 \pm .04$	?

Fig. 12

Radiation Laboratory in connection with some of their work on these states in systems of low Z .¹¹⁾

It is perhaps well to note at this point, because some of the states decay only by the spin-spin interaction and some decay by both the spin-spin and spin-orbit interactions, that the states are differentially metastable; that is, the maximum J states decay at different rates than the minimum J states do. This makes it possible to dream about schemes for making polarized ions. There are some suggestions of this type for making polarized lithium beams that were made by the Columbia group.¹¹⁾ This scheme has not been realized, and it is quite complicated, but I wanted to mention that the potentiality is apparently there. I don't happen to know whether one might think about polarized heavy ions, but in principle differential metastability of this kind always raises the possibility.

Fig. 13 illustrates the semi-empirical scaling rule as a

Fig. 13

function of Z for long-lived states.¹¹⁾ The data are from experiments both by the Columbia group¹¹⁾ and, using a different method, by some Soviet investigators,¹²⁾ who instead of looking at the electrons

emitted by the beam measured just the change in charge of the lithium-like beam component as a function of flight-path distance from a target that they had in their beam line. The Soviet measurements were not consistent as a function of beam energy, so their data did not permit the extraction of a unique lifetime. I think that identifying the state by electron spectroscopy gives a more powerful handle on measuring the state lifetimes.

There are increasing departures from this simple scaling rule with increasing Z . This opens the possibility of relativistic effects that might serve to explain this discrepancy. Lew Cocke pointed one out to me some months ago, which I thought was very interesting.¹³⁾ The idea is that a $(1s, 2s, 2p)$ state, a quartet state, looks quite similar to a $(1s, 2p)$ triplet state of a helium-like ion with the exception that there is another electron attached whose spin is parallel to these. This helium-like ion, the radiative decay mode of which we spent time discussing in the previous talks, has the possibility for the $J = 2$ part to go to the ground state by means of a magnetic quadrupole transition. This is, in fact, a very sizeable decay mode for ions of say $Z = 18$, which were the ions in question in the Berkeley experiments on this transition. One can imagine a similar situation here, in which one of the electrons goes to the ground state, again by a magnetic quadrupole radiative transition. The details change slightly because of the presence of the extra electron, but as I understand it the radial matrix elements for the process are very similar, and so the rates are very much the same. It seems quite likely; I buy the explanation completely; the reason for at least part of this discrepancy is the presence of this magnetic quadrupole decay channel. However, there doesn't appear to be enough of it to explain why the data fall this badly off the curve. The semi-empirical rule is however not more than an educated guess. There remains a task for someone with theoretical talents who is interested to take on.

I want to discuss one further thing before leaving this particular subject. So far we have talked only about three-electron systems. How about metastable states of systems with other numbers of electrons? There are, as I have already mentioned, metastable autoionizing states of helium-like, two-electron ions, but not very many, so the direction

to look is at systems with larger numbers of electrons. We ask what metastable states are formed in systems with larger numbers of electrons and how commonly they are formed. What general principles might we use that are not specific to the three-electron system that might govern where these states would be likely to occur? The next slide (Fig. 14)¹⁴⁾ is basically an energy level diagram, just to get us used

Fig. 14

to thinking about the relative orientations of some of the levels we have discussed so far. This diagram shows the situation normalized to the ground state of the three-electron ion; for oxygen that is O⁵⁺. The ionization potential of that ion, separating it from the ground state of the helium-like ion, is about 138 volts. The excited (1s, 2s, 2p) 4p⁰ state of the three-electron oxygen ion lies above the helium-like ground state to which it decays, and the difference between

the excitation energy of the three-electron ion and the ionization potential of the two-electron ion is the energy of the electron that is emitted. There are a series of such states converging on a series limit, either 2^3S or 2^3P of the He-like ion. Higher quartet states of the three-electron system than that can decay to the triplet states of the helium-like ion and so are not metastable. Thus, the metastable states tend to lie as discrete states adjacent to the continuum of the two-electron ion until they reach the first excitation potential of the two-electron ion, and then they stop. The thing to look for, then, is levels between the ground state of the adjacent ion one charge unit removed and the first excited state of higher spin of that adjacent ion.

What about other electronically similar systems? Let's look at a candidate. One possibility is to look at the other alkalis, which tend to have the same kind of structure; the ground state is the closed shell plus one electron, the idea being that if one excites one of the electrons from the full shell up to a higher shell, it is possible to align the spins of these two electrons and also to align these spins with the spin of the hole that is left in the no longer closed core. It is possible, then, to think about building quartet states by coupling the spins of a valence electron, an electron excited from the core, and the hole that that excited electron leaves. One would expect to be able to put the excited electron into a number of states up to about the first excitation potential of the adjacent charge state ion.

One of the most likely possibilities, it seemed to us, was that an ion with eleven electrons, which is therefore like sodium, having inert gas structure plus a single additional electron, might show the same kind of phenomenon. We had accessible to us at our tandem a chlorine beam at the right sort of energy range to produce eleven-electron ions abundantly. It turns out that energy is about 5 MeV. The spectroscopic tables gave us a good deal of information about the location of the ground state of the eleven-electron ion and of the adjacent ten-electron ion to which such autoionizing states might decay. The energy level diagram (Fig. 15)¹⁵⁾ is schematically very similar to the three-electron energy level diagram that we have seen before. One expects first the ground state of the eleven-electron ion, then the ground state

Fig. 15'

of the ten-electron ion, and then some possible metastable states above the ground state of the noble gas ion up to some series limit formed by the first excitation potential of the noble gas ion. We found at least one such state, which is labeled here. Actually we found many, but the most probable one occurred at about this level, relatively closer to the ionization limit than was the case for metastable states of the three-electron system.

Fig. 16 shows the electron spectrum accumulated for an equilibrium distribution of chlorine ions that had passed through foils of sufficient thickness to produce equilibrium, in which the beam energy was chosen to maximize production of charge states near the sodium-like charge state.^{3,16)} Here is the series limit that was indicated in Fig. 15. We have already noted in that diagram the occurrence of the most prominent feature of the spectrum. There are a lot of other features

Fig. 16

about which I cannot comment very much, because there are no theories of where these states should lie or what their lifetimes ought to be. Data like this is completely missing from spectroscopic tables. It is obvious that there are a tremendous number of states. They differ from the lithium-like spectrum in two ways. First of all, some of the more prominent features are up near the ionization limit. We think it is likely they are associated with the eleven-electron ion for several reasons. One is the choice of beam energy; another is the appearance of a series limit in the data right where one would expect to find one. Thirdly, because noble gases generally have a large binding energy, the energies of the emitted electrons are larger than one would find if one examined nearby charge states for which a different sort of ion would be left over. Because a noble gas ion is tightly bound, the energies of Auger electrons that are associated with the formation of that ion

tend to be large, and for adjacent charge state ions, the activity would be at mostly lower energies.

The other difference is that, in contrast to earlier lifetime results in which the lifetimes tended to be of the order of nanoseconds or a few tens of nanoseconds, these states are significantly longer lived. In fact, they are so long lived the apparatus wouldn't accommodate them. We measured decay curves out to a substantial fraction of a meter away from the target, perhaps half a meter. The decay curves continued to fall, but had not yet reached the traditional straight-line decay curve on a semi-logarithmic plot. All we could do was set lower limits; the lifetimes are somewhat in excess of 40 nsec for all of these states, so a lot of these states are significantly longer lived than anything we've seen previously. There must be some reason for this great stability. So far, we don't know what it is. The theory has not been done.

That's all I want to say about the subject of autoionization. I am going to pass on next to a somewhat different topic, so I want to pause at this point and ask if you have any observations you would like to make or questions to ask.

Question: Can you suggest how the states are formed? What is the excitation process?

Answer: Specifically the eleven-electron system? Really, the only specific idea I have concerns the three-electron system; I already mentioned the formation of a metastable helium-like core and then the addition of an electron by subsequent collision. For this case, I would make roughly the same sort of guess, that one makes a noble gas core, and then knocks out an electron, and when the system captures another electron to replace the one that was knocked out, it captures it in a state of the wrong spin, so it can't descend into the noble gas core right away. I am talking about a multiple collision situation. In a solid target, we have no information, of course, about the detailed nature of any of the ions as they are going through the target. The addition of the last electron may well occur at or near the last atomic layers, on the way out.

Question: Did you look at the Auger electrons very close to the place where the beam leaves the foil?

Answer: There were two examples of that already. Perhaps you remember the oxygen spectra at 2 MeV and at 5 MeV with the foil either in view or just backed out of view (Figs. 5 and 6). That is about the only sort of information that we have.

Question: You simply move the foil real close to....

Answer: Right, right up into the field of view of the spectrometer, while trying to collimate as many of the electrons from other processes as possible out of the way. It is not easy to do, because there are dozens of electrons per incident ion that are scattered near the forward direction.

Question: Can you use this technique to look at the target Auger electrons?

Answer: Our attempts to do that have thus far not been very successful. For one thing, we haven't tried to look at solid targets in this sense, because of the expectation that electrons emitted in those solids would lose a lot of energy on the way out and would have their energy distribution smeared. We tried it once using a gas target, and our experience then was there was too much scattering off the gas near the entrance region to the spectrometer. It's a problem of having to pump that region very strongly.

Question: It seems to me that maybe ten- and twelve-electron systems might also have metastable states.

Answer: I agree about the twelve-electron systems. How about the ten?

Question: Particle-hole excitations should....

Answer: Should form a triplet state; that's true. That's a distinct possibility. However, let me remind you of two things, first, the series limit in the data, and second, the large energy of the electrons that emerge. If one examines this region in the periodic table and asks with what energy an electron is likely to be emitted when the residual nucleus is strongly bound, the amount of available energy is larger than if you start with a neon-like system and leave behind a fluorine-like system.

Question: Doesn't that mean that other series limits will lie off to the left, at lower energy?

Answer: Yes, right. A good many of the series limits lie completely off the scale; in fact, that is true for the single-electron-excited spectra of all adjacent charge states.

Question: So seeing that particular series limit does not rule out other series limits lower down.

Answer: It does not rule that out at all; quite right. They may be there. It also does not rule out multiple excitations where two or more electrons in the system are excited. Then all these energetic arguments are invalid.

I want to spend what time I have left talking about where, apart from inside beam tubes and accelerators, metastable ions like this exist in nature. They are rare on earth, because we live in a rather cold environment. Not many ions we commonly encounter are more than singly or doubly ionized, as any manufacturer of ion sources will be quick to attest. However, these are rather common constituents of stars, in particular of our own star, the sun. I have brought along some slides that fall in the realm of applications of this kind of thing to solar astronomy. Most of these slides were very kindly loaned to me by Leon Heroux of Air Force Cambridge Research Laboratories. I thought that I would just discuss a little bit what ions occur and how prominent the lines that they emit are in the spectra of stellar atmospheres.

Fig. 17 shows the solar soft x-ray spectrum in the region of some of the wavelengths emitted by ions we have been discussing.¹⁷⁾ For instance, we discussed previously measurement of the lifetime of the intercombination line in the helium-like oxygen ion. In this spectrum it is one of the stronger lines. It has comparable intensity to the lines from the 2p to 1s transition in the helium-like oxygen ion, the ordinary allowed transition. That raises an interesting point; how can a forbidden line possibly have a comparable intensity to that of an allowed line? The answer is that the population of ions in the solar atmosphere reaches some equilibrium level that depends on how fast the

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Fig. 17

states are created by electron impact and how fast they decay by radiative or collision processes. If the electron density in the plasma is low, as it is in the solar corona and in the solar chromosphere, then the situation is best described as one in which electrons pump ions up by collisions which fall back down by radiative processes, and if they fall down slowly by radiative processes, then quite a substantial population of ions can be formed. So the comparability of the intensities of these lines comes from two things. Although it's not shown here, these same lines appear in solar x-ray spectra up to at least iron, so these are the most distinctive features of the solar soft x-ray spectrum in this particular wavelength region. The intensities of these lines are prominent quantities in theories that seek to determine what the electron density and the temperature are in various regions of the solar atmosphere. For the purpose of orientation, let me say that in the solar atmosphere the temperature of the electrons in the plasma tends to run in the region between 10^4 and 10^7 degrees Kelvin, except in say a solar flare. The photosphere has an optical temperature around $6,000^\circ\text{K}$. As one goes out in the corona--the corona, of course, extends all the way out to the earth and beyond--

the electron temperature of the corona gets very hot compared to this. The electron densities tend to lie in the range between 10^8 per cubic cm and 10^{11} per cubic cm. Those are the physical conditions under which these lines occur.

These ions actually form a very minor part of the content of the solar atmosphere. The most abundant element, of course, is hydrogen. If one normalizes the ion abundances to something weak like oxygen, the results in Fig. 18 are obtained.¹⁸⁾ Here is a resume of some ion

TABLE 2
FRACTIONAL ABUNDANCE OF THE IONS REPRESENTED
IN THE PRESENT SPECTRUM*

Ion	ELECTRON TEMPERATURE (10^4 °K)									
	1	2	3	4	5	6	7	8	9	10
O VII.....	1.00	0.56	0.13	0.02	0.007	0.003	0.001			
O VIII.....	0.007	.36	.34	.17	.08	.05	.03	0.02	0.01	
O IX.....		.08	.53	.81	.91	.95	.97	.98	.99	1.00
Ne IX.....	0.87	.97	.81	.52	.34	.12	.05	.03	.02	0.01
Ne X.....		.02	.17	.37	.40	.32	.23	.16	.11	0.09
Ne XI.....			.01	.10	.25	.55	.71	.81	.87	0.90
Fe XVII.....		.015	.31	.50	.58	.58	.54	.46	.38	0.28
Fe XVIII.....			.005	.03	.08	.14	.21	.27	.29	0.29
Ni XIX.....		.001	.10	.24	.35	.44	.48	.48	.44	0.40
Fe XVII.....		0.030	0.31	0.44	0.46	0.46	0.43	0.37	0.30	0.22

* O VII-Fe XVII data from J. W. Allen and A. K. Dupree (1967), *Harvard College Obs. Report, No. 14*; Fe XVIII and Ni XIX data from C. Jordan (unpublished).

Fig. 18

abundances from a spectrum that was reported a few years ago. These are data based on interpretation of spectral line intensities. You can see what ions occur in the solar corona and approximately how the ion abundances vary with electron temperature.

Let me say just a word about how intensities are related to populations, transition probabilities, and that kind of thing. One can write a rate equation if one accepts the idea that line intensities are determined by a balance between electrons pumping states and their decaying back down by radiative processes. Such a rate equation, following the work of Gabriel *et al.*,¹⁷⁾ is

$$N_u [A_{mg} + C_{mg} N_e + \sum_u C_{mu} N_e] = N_g N_e [C_{gm} + \sum_u C_{gu} R_{um}]$$

Suppose we are looking at some metastable ion; I use a subscript m to represent a metastable system. Its number density is N_m ; that's the number of metastable ions in the particular state per unit volume. The rate at which these states are being destroyed comes from two types of terms. The first term is the rate at which they spontaneously decay. Many of the lifetime measurements we talked about the last few days enter in this term, which is the A-value for the transition of the metastable system to the ground state. There are, of course, other ways of destroying the ions. There are collisional destruction rates in which the ion is bumped from the metastable state down to the ground state; that is in the coefficient C_{mg} . This is generally assumed to take place by means of electron impact, so this term contains a factor of the electron density N_e , the number of electrons per unit volume. Finally we have the possibility that we have collisional excitation to higher states, including continuum states, so we have a sum over upper states (u is for upper.) of C_{mu} times N_e . In the solar atmosphere the electrons and the ions have the same temperature but vastly different velocities. It turns out that the electrons are the dominant objects in determining ion population by means of collisions. Proton-ion collisions are significantly less important, and heavy-ion collisions are of almost no importance because the density of heavy ions of any sort is very small.

We must balance this rate at which metastable systems are destroyed against the rate at which they are created. The dominant source of excitation is again the electrons, so the number of electrons per unit volume is important. Also, what they are striking is important. In a first approximation, all of the ions are pretty much in their ground states, so we would expect the rate to be proportional to the density of ground state ions, N_g . Then we have two types of terms. We have direct population of the metastable state by collisions, i.e., bumping an ion from the ground state up to the metastable state; that occurs with some rate coefficient C_{gm} . Also we have the possibility of populating this state from above by populating an upper state that lies above the metastable level by means of collisions from the ground state. This involves a coefficient C_{gu} times some branching ratio R_{um} that describes

what fraction of the time an ion in the upper state decays back down through the metastable channel. That's the basic kind of rate equation that is used in this kind of work.

The overall intensity of some allowed line involving an upper state that is populated by means of electron impact is commonly used to measure the electron temperature. The intensity of some transition involving a metastable system is given by the number of metastable systems that occur, as determined by this kind of rate equation, times the transition probability from that metastable level to the ground state:

$I_m = N_m A_{m \rightarrow g}$. The electron temperature determines what the magnitudes of these C coefficients are. We have in the square bracket on the left a term that depends on the transition probability and some terms that depend on the electron density. If one varies the electron density by looking at some region of the solar corona and comparing it with another region, one effectively alters the importance of the collision terms on the left in the rate equation compared to the transition probability term. That in turn alters the number of metastable systems, which figures into the intensity equation, so if one looks at intensity variations for one of these forbidden lines, one then measures something about the effect that the electron collisions have in determining the equilibrium value of the number of metastable systems per unit volume. Much of our information about electron density in stellar atmospheres comes from combining knowledge of these A values with astronomical observations of how these intensities vary. For example, if there is a solar flare event, sometimes these intensities can vary quite remarkably. One can gauge the temperature in the flare and the electron density in the flare by examining the variations of the intensities of some of the lines that originate from these metastable levels.

I think I will finish my talk by showing a few slides that illustrate other astrophysical applications. Fig. 19 shows some more spectral data about what sort of ions occur in the sun and at what wavelengths the radiations fall.^{19,20)} One can see that there are a great many lithium-like, helium-like, and beryllium-like lines in the sun. Fig. 20 shows a different spectral region. One sees more high charge state spectral features from sundry sorts of ions.

Fig. 19

Fig. 20

Fig. 22

of very prominent iron lines, also nickel lines, oxygen lines, and

sulphur lines.

Fig. 23 shows a term scheme appropriate for lithium-like

Fig. 23

ions.^{19,20)} One has a $2s$ ground state (The $1s^2$ isn't shown.) and a sequence of excited lines associated with s , p , and d states. The commonest transitions are indicated by the arrows. A great many of these transitions are seen in observations of the sun. There are few metastable states in the doublet spectrum of lithium-like ions. While the resonance lines can be related to electron temperature, there are no metastable states in lithium-like ions that are useful for making electron density measurements. On the other hand, for the beryllium-like ions there are such states. (Fig. 24) It is possible to take one of the electrons in the $n = 2$ level and flip its spin to make 3P levels. These metastable states figure in the rate equations, which allows one to connect intensity variations to electron densities.

Fig. 25 gives examples of excitation rate coefficients for electron impact as a function of electron temperature for sundry lines

Fig. 24

of various lithium-like ions.²⁰⁾ They are all calculated cross sections. One gets from this some qualitative information about what sort of collision rates to expect, which are averages of collision cross sections times velocity. These are all states that yield allowed radiations. Fig. 26 shows a similar situation for beryllium-like ions.

Fig. 26

We now come to predicted ratios of line intensity as a function of temperature for various lithium-like ions as shown in Fig. 27.¹⁹⁾ It is possible to calculate the intensity of the 2p to 2s transition. Things are quite steeply dependent on the electron temperature; this is the basis of temperature measurement in the solar corona. Because of the steepness of the variation that occurs with temperature, one can gauge quite accurately what the temperature is in this range. In Fig. 28 a similar situation occurs for a beryllium-like ion, O^{+4} . The experimental result is from a rocket measurement.

In Fig. 29 the bars indicate the range of temperatures where emissions of the various photons that are listed here are important.²⁰⁾ The lengths of these bars are determined as follows. If 50% of the

Fig. 27

Fig. 28

Fig. 29

maximum line strength ever observed is still present in a certain temperature range, then the bar is shown. Beyond the limits of the bar, the intensity has grown too weak; it falls below the 50% level. This allows one to get some feeling for the temperature ranges in which the lithium-like oxygen lines, the lithium-like neon lines, and the lithium-like magnesium lines are important.

Fig. 30 has, in rather peculiar units, the measured photon flux, photons per cm^2 per sec.²⁰⁾ It is scaled in another way with temperature, but that's not really important here. It gives an idea of how the lithium-like oxygen lines vary as a function of temperature. One can see that the lithium-like oxygen lines are not really a very good measure of the solar temperature, because some of them peak over a rather wide range of temperature, like the 3p to 2s transition, for instance. But some of the other lithium-like ions are satisfactory, as shown in Fig. 31.²⁰⁾ This diagram is for a lithium-like magnesium transition. The precision of a temperature measurement based on this line is considerably better because of the more narrow photon flux versus electron temperature plot.

Fig. 32 shows the temperature ranges in which the beryllium-like oxygen ion is important. These bars are narrower than the bars

Fig. 30

Fig. 31

Fig. 32

on the previous diagram for the lithium-like ions.

Fig. 33 is a plot of the photons per cm^2 per sec in the

Fig. 33

emitting region as a function of temperature for this particular state of the beryllium-like ion. The region is really quite narrow, so this

transition turns out to be particularly useful for measuring the temperature of the solar plasma at a particular point. People like Heroux et al. do such things regularly; they fly rockets to look at specific regions of the sun or of the corona, and by gauging the intensity as best they can, they measure the temperature there.

The only moral to this particular astrophysical story is that some A values and intensities are of interest to measure for a specific applied purpose. Since the electron density does vary from point to point in the sun, and since A for a particular type of transition also will vary over a considerable range depending on how forbidden the transition is, a good many of these metastable state lifetimes are useful numbers to measure for solar physics purposes. It turns out that a good many of the lines in the solar spectrum cannot be identified; many of them have to do with states of ionization of heavy ions like iron and nickel.

That finishes what I wanted to say about astrophysical application. I want to thank you very much for inviting me to speak in your lecture series. I enjoyed my visit here immensely.

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