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MASTER

The (p,n) Charge Exchange Reaction

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In discussing our early experiments on the charge-exchange reaction I intend to emphasize three points. First, why we did the experiments, second, how we did them, and third what was the resultant data. I will only briefly discuss any conclusions.

Well, to begin with--how did we ever get into this business? Cal Wong and I were interested in the problem of inelastic neutron scattering. Since there are essentially no clean monoenergetic neutron sources for neutron energies between 8 and 14 MeV we decided this problem could best be solved in an indirect way. Since neutron reaction cross sections were easily measured--if we could obtain an estimate of the spectral shape of neutrons emitted from highly excited compound nuclear states we felt we would have the problem licked.

If one reads the literature one finds that (p,n) experiments had been done earlier at 22 MeV by Cohen on thick targets and by Gugelot at 18 MeV on thin targets. Both experiments were done with nuclear emulsions with coarse energy resolution. Both experiments clearly indicated the statistical nature of the (p,n) reaction. So we set out to measure more carefully the neutron emission spectra from the (p,n) reaction, from which we would deduce level density parameters and then infer the statistical portion of the neutron inelastic scattering cross sections.

In Fig. 1 we see the geometry used. The accelerator was the 96" cyclotron of the Lawrence type--no field focusing, it was variable energy with a maximum available proton energy of 14 MeV--(frequency limited). We were using time of flight techniques for our neutron spectroscopy with post acceleration sweeping. The geometry for the neutron spectra

1b

1a

measurements is shown on the bottom of the figure, and the geometry for first angular distribution measurements is shown at the top of the figure. The lead lining of the beam pipe was to cause us problems later.

In January of 1961 we began an experiment on a few self supporting metal targets at 10.8 MeV proton energy and were not the least surprised to see a new peak in the neutron spectrum coming from our vanadium target. With the then current instrumentation peaks came and went quite regularly. After carefully checking our electronics we were somewhat more concerned. After retuning the cyclotron and auxilliary beam sweeping equipment we were quite perplexed. Changing the target material to niobium made everything better--the peak went away and was therefore obviously due to an impurity in the vanadium target. We were quite happy at the resolution of this problem and went on our way. The next running period was in April. We did not have time to get another vanadium target but concluded this impurity wouldn't hurt us too much because it was such a nice sharp peak it could be subtracted out and would not interfere with our statistical studies. Our run in April went to higher energies and the peak was becoming so prominent as to be bothersome.

In Fig. 2 we see the neutron emission spectra plotted for several bombarding energies. After the run we felt this contaminant was really so bothersome that we would have to identify it and eliminate it from future targets. We had done several experiments on light nuclei and knew that the largest (p,n) cross sections were the result of mirror transitions. So we set out to calculate the Z (charge) of the nucleus for which our measured Q value would give the Coulomb energy. When the answer came back that it was the Z of the target, the interpretation of the data was

immediately obvious. We concluded that the incident proton gives up energy to the target nucleus as it rolls up the Coulomb barrier. Once inside the nucleus it exchanges its charge via an effective two-body force with one of the neutrons corresponding to an unfilled proton orbital and the resultant neutron is emitted. This reaction requires no change in nuclear configuration and hence the Q-value for this reaction is the Coulomb displacement energy--that is the energy required to deposit one unit of charge on the nucleus.

Our simple description clearly seemed to require some sort of direct reaction mechanism as compared to a compound nuclear or statistical process. In looking at Fig. 2, the thing to note is that the relative magnitude of the peak, to the continuum background grows as the bombarding energy increases. These plots are displaced so that you can see the individual curves more clearly. On an absolute basis the peak cross section does not change with bombarding energy, which was consistent with our hypothesis of a direct reaction mechanism for the production of the peak, while the background drops a factor of four which was consistent with a compound nuclear mechanism.

These data were first submitted for publication in July and we received a very prompt note informing us that our manuscript did not contain any particularly new or interesting data worthy of rapid publication. The second referee, who turned out to be Denys Wilkinson, recommended a few changes but did indeed think the work was worth prompt publication.

We were not completely satisfied that we had really shown that the reaction was proceeding via a direct process and so we attempted to measure the angular distribution of this neutron group. The results are shown in

Figure 3. What we had hoped to see was a strong forward peaking of the angular distribution and indeed we did. We have since remeasured this angular distribution and have found that the decrease in cross section at small angles is spurious--it was caused by neutron absorption in the lead liner in the beam pipe.

Having satisfied ourselves as to the reaction mechanism we next wanted to find out just how far into the heavy nuclei this phenomena would persist. After all, in vanadium all the valence nucleons were in the same $f_{7/2}$ shell and even though it was generally thought that isospin would not play any major role above $A = 40$, it was argued that isospin would be a useless concept only after the protons and neutrons were filling different shells. We desperately wanted more energy so we could reach heavier nuclei.

Since our cyclotron was frequency limited at higher energies, we drove the dee liners out to their limits and kept right on going until the bearings bound. This reduced the capacity of the dees sufficiently so that we could get another few hundred kilohertz in frequency and an energy of 14.8 MeV. With this increase in energy we were able to see analog states in nuclei up to niobium. These measurements are shown in Fig. 4. This meant that we had now observed analog states in nuclei well beyond the $f_{7/2}$ shell and indeed these states were still narrow when unstable against particle emission. This narrow width phenomena was one which we were not to understand. The progress to this point was discussed at the 1961 Thanksgiving meeting of the APS in Chicago.

We were still fascinated by this same question--for how heavy a nucleus will this phenomena persist. We desperately wanted more energy--and there was not an easy way to get it. The next modification of the

cyclotron required major surgery. We rebuilt the r.f. cavity shrinking it to the point where the r.f. coupling loop could see only a small portion of the resonant cavity. We then gradually raised the dee voltage to about 50% above design specifications and prayed for beam. Through the efforts of the late Bob Jopson, who was responsible for most of the cyclotron modifications and the operations staff our prayers were indeed answered and we had 18 MeV protons.

We also had a new challenge. Ernie Rost had written and suggested that on the basis of his shell model calculations it would be reasonable to expect to see analogs of excited states of even-even nuclei especially around the $f_{7/2}$ shell. Fig. 5 shows the time of flight spectrum for the odd-even nuclei vanadium and cobalt. One notes the absence of structure other than the analog state. In Fig. 6 we see the corresponding spectra for the even-even nuclei, iron and nickel. The peaks marked 1 are indeed the expected analogs of the first excited state of the target nuclei. Although these are not the most convincing spectra, they were the first we observed. The cleanest and prettiest spectra were subsequently measured for selenium. We will return to the discussion of analogs of excited states later.

Given more energy we were able to extend our Coulomb measurements up through praseodymium. This is shown in Fig. 7. Although we were not able to make a good width measurement it was clear that the phenomena had persisted even though the final state was unstable against particle emission by many MeV.

These forays to high energy with our cyclotron were not without cost. With the whole r.f. system pushed to the limit and with many copper surfaces

carrying current densities close to the limits catastrophic failures were common. A normal sequence was two weeks to modify the cyclotron and obtain beam--two weeks of operation terminated by a major system failure, followed by two weeks to month of down time while the cyclotron was repaired and returned to its normal mode of operation for a years rest.

Returning to the summer of 1961 when Tony Lane heard of our results he immediately suggested that the (p,n) reaction could be understood in terms of the Heisenberg or charge-exchange part of the two-body force. This led to his inclusion of an isospin dependent term in the optical model which could not only explain the proton potential anomaly, that is the fact that protons and neutrons required different potentials, but also led to specific predictions for the magnitude and angular distribution of the (p,n) reaction. By 1962, Satchler, using distorted wave Born approximation calculations had been able to obtain a qualitative fit to the angular distribution measurement shown previously. Although we would like to have measured more angular distributions our experience had shown that operating the detector inside the pit was not very promising because of the background neutrons resulting from high energy protons on collimators and beam stoppers.

In the summer of 1962 we decided that the way to beat this background problem was to take our neutron collimation and long flight path with us into the pit. This geometry is shown in Fig. 8. By drilling through the concrete walls of the target pit and excavating dirt we were able to install six detectors at a cost of \$6,000. This geometry did not allow sufficient angular definition, so we converted an old pair spectrometer into a bending magnet and using an existing bending magnet

we were able to re-steer the beam through the target at 18° to the old beam line. The combination of geometries allowed us to measure angular distributions in essentially 15° steps. The first angular distributions were measured using 6 detectors simultaneously by splitting an old 400 channel analyzer into four 100 channel subgroups and a 256 channel analyzer into a pair of 128 channel subgroups. Preliminary results of our first angular distribution measurements were presented in February of 1963 at the International Conference on Fast Neutron Physics which was held at Houston. It was at this conference that the T. W. Bonner Nuclear Laboratory at Rice was dedicated. The observed angular distributions along with the DWBA calculations of Satchler, Bassel and Driscoll are shown in Fig. 9. At this point one could now infer from the (p,n) data both the magnitude of the isospin potential and some indication of its radial form factor.

It was during 1963 that our problem in understanding the width of analog states was resolved, not at Livermore--but at Tallahassee, and that ends the historical resume.

There were a few problems that required more attention. Although the population of analogs of excited states was postulated before observed--the model was not able to describe the data. The only correlation we could establish was that wherever there was a large BE2 matrix element, there was also population of analogs of excited states.

Satchler, Drisko and Bassel attempted to fit our first angular distribution measurement of excited analogs by including isospin in the extended optical model--so then we had an isospin dependent deformation parameter. These were DWBA calculation and assuming the isospin dependent

deformation was equal to the normal deformation parameter the calculations underestimated the observed cross section by a factor of 50. One was faced with the prospect of a very large isospin deformation--or finding another reaction mechanism. The latter turned out to represent the better alternative. In Fig.10 we see a comparison of the data with a recent calculation of Brown and Madsen. These are coupled channel results where the ground state, first excited state, and their analogs are explicitly taken into account in the calculation. The normal deformation parameter was adjusted to fit the (p,p') inelastic data and the strength of the isospin potential was adjusted to fit the (p,n) analog cross section. Without using an enhanced isospin deformation one is able to account for the observed cross section and in fact the calculations are rather insensitive to the isospin deformation. It would seem then that a plausible explanation of the observed data is that the reaction mechanism is a two-step process involving inelastic excitation and charge exchange and that the one step process is not important--at least not at these energies.

By 1968 we had changed our instrumentation again and expanded to a 10 detector system in 15° steps and sure enough we still weren't satisfied with the angular definition so we extended our double bend geometry to pick up intermediate angles. We had also begun negotiations for construction of a cyclograaff facility and so were not quite as concerned over the possibility of a career ending catastrophic failure of our cyclotron. And so we pushed it, to what we really felt was the physical limit and thus we obtained the data at 19.7 MeV shown in Fig.11. The machine was not stable enough to try double bending at this energy but an MeV lower at 18.7 one sees a rather complete angular distribution for a number of

$E_p = 18.7 \text{ MeV}$ $E_p = 19.7 \text{ MeV}$

nuclei. The rather large error bars on the 19.7 MeV data are due to statistics which also indicates that we didn't run very long at 19.7 MeV. The solid curves shown in Fig.11 are optical model calculations using the Becchetti-Greenlees global optical parameters.

14 months ago we shut down our old 90" cyclotron and began disassembly. We had on hand the negative ion cyclotron which was to be the injector for the cyclograaff and were in the process of running acceptance tests on the tandem. The tandem is an EN and is actually the rebuilt Florida State machine. By summer we had cyclograaff beam but no where to go with it. As you can see in Fig.12 it was necessary to clean up and rebuild parts of the old cyclotron pit before we could use the beam. Our first attempts at internal pulsing of the cyclotron beam were not very rewarding and so we installed our old sweeper between the negative-ion cyclotron and the tandem. The facilities were sufficiently complete by fall to run on a part-time basis.

Given a partially complete cyclograaff what does one do? Well, we immediately ran an excitation function of the angular distribution of our old favorite--the vanadium (p,n) analog reaction. The results are shown in Fig.13. We ran in one MeV steps from 16-26 MeV. The data at 19 MeV was somewhat obscured by the target gamma ray overlapping the analog neutron peak and is not shown--the gamma rays were a problem in spite of the fact that we are using proton-electron pulse shape discrimination on our detectors. One notes that there seems to be a very smooth transition in angular shape as a function of bombarding energy.

In Fig.14 we see the integrated cross section plotted as a function of bombarding energy. This is by no means the first excitation function observed. By looking at $\bar{p}$ decay of analog states, excitation functions had been obtained for several nuclei including Zr, Sn, Pb, and Bi. All

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$^{51}\text{V}(p,n)^{51}\text{Cr}$ (Analog)

these indirect measurements indicated that the cross section rose rapidly above threshold and had a broad pronounced peak (with a width of perhaps 8 MeV) before falling to a rather slowly varying energy dependence.

At this time I need to point out there is a suppressed zero in the figure which badly accentuates this effect. Although the magnitude of the optical model calculation is not bad the energy dependence is most certainly wrong. These conclusions do not change with optical parameters as long as one does not change the energy dependence of the imaginary potentials. We shall return to this point later.

We next decided to try and fit the highest energy (p,n) data and then see what happened to the energy dependence of the angular distributions. This is shown in Fig. 15. The geometry was essentially held fixed and only the magnitudes of the real and imaginary part of the isospin potential was allowed to vary. Having fit the 26 MeV data we also obtain a reasonable fit to the 21 MeV data--a span of 5 MeV but then in the next 3 MeV the agreement is poor. The angular structure is clearly given by the calculation but the magnitude of the cross sections is clearly wrong. If you look at the difference between the measurement and calculation at 18 MeV you would note that it is fairly isotropic. Could it be there is a compound nuclear contribution? It isn't very likely but let us consider the problem.

Fig. 16 shows a schematic representation of a measured neutron energy spectrum. To get the analog state cross section one draws a smooth background shown by the dashed curve and I believe this honestly accounts for all the $(T_0 - 1/2)$ background. But what about this dotted curve? This

corresponds to a sort of compound quasi-elastic process. To see that there really should be such an effect we look at the level diagram shown in Fig. 17. We note that all the reaction cross section proceeding through the $T_0 + 1/2$ channel is either damped into the $T_0 - 1/2$ states whence it can neutron decay to T lower states or if not damped it will proton decay to the target nucleus and neutron decay to its analog. The difference in the Coulomb barrier height and the Coulomb displacement energy makes proton decay much more favorable and only for nuclei below $A = 90$ or close to threshold is there any appreciable probability of neutron decay. For vanadium it is possible that there is such a compound contribution but it should be limited to the energy range of perhaps a few MeV above threshold while we see pronounced effects 10 MeV above threshold.

We can slow down the energy dependence of a compound-nuclear contribution by invoking pre-equilibrium emission. Having formed a doorway state there is a small probability of decay to the continuum rather than into more complicated configurations. Using this type of argument we get too slow a cross section variation with energy. Thus we don't seem to be able to fit the data with a pre-equilibrium process.

Hoffman and Coker trying to fit the $\bar{p}$ excitation function have found it necessary to modify the energy dependence of the neutron imaginary potential and have thus reproduced the observed peaking. Whether such a prescription will also be able to fit our data and especially the observed angular distributions is not as yet clear.