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THE NUCLEAR PHOTOELECTRIC EFFECT AND REMARKS ON HIGHER MULTIPOLE TRANSITIONS

A Personal History

by

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This Symposium on the History of Nuclear Physics concentrates on a time when physicists were switching their major attention from the then recently conquered field of atomic physics to nuclear physics. Our gathering is probably unique in having a large fraction of speakers who had already made important contributions in atomic physics before turning their attention to the nucleus. In this transition the nuclear photoelectric effect has played a role which might be worth recounting.

For some personal reminiscences connected with its discovery and investigation I have to go back more than forty years. However, as we all know, memory is selective. It could hardly be otherwise. "Total recall" probably can play a role only when the "resolving power," both spatial and temporal, is unimportant, e.g., when we are concerned solely with numbers. Different observers will remember the same event differently--this might be called, after the Japanese story known to many of us from the famous movie, the "Rashomon effect." This is especially true in the history of science because here different "observers" are often separated both in space and time. While reproducibility plays an important role in science, history of science has to emphasize consistency. Thus, by using many different sources,

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the historian of science can develop a more or less balanced picture, though hardly ever a complete one. The memories of one scientist, subjective as they necessarily are, sometimes telescoping different events into one, and favoring "vivid" ones which he has often "replayed" in memory or explicit recollections, can form only a part of the raw material used by the historian. In preparing these remarks, I have not attempted to make more than sporadic use of the published literature and of my own notes, unlike the systematic use a historian of science would consider necessary for a fairly well rounded picture.

With these provisos, let me come to my story. In the early thirties, while I was a student at the University of Berlin, I got interested in nuclear physics, partly through a stimulating course given by Lise Meitner. She was very enthusiastic about her field, and I still remember how excited she was when reporting the discovery of the neutron in a colloquium. She got so carried away that she spoke of the collision of a neutron with a "brass nucleus." This is not such a bad mistake after all because the "brass nucleus" has fewer "isotopes," namely seven, than some other well-known nuclei like tin, which has ten.

From time to time the Nazi students rioted, and one day in March 1933 they had brought all lectures to a halt. This unexpected free time permitted me to go to the library, where I came across a short note in a popular scientific journal which reported from the United States that G. N. Lewis had separated 1 cc of heavy water. The news that a rare isotope--discovered only a year earlier by Urey, Brickwedde and Murphy--had been isolated in such a large amount impressed me very much. I immediately asked myself: To what use could heavy hydrogen be put? I jotted down a few ideas, one of them being:

look for photodisintegration of the heavy hydrogen nucleus (later called the deuteron). Such an effect might explicitly check whether the deuteron is "made" of a proton and a neutron. Remember that only a few years earlier the possibility was still taken seriously that nuclei were made of protons and electrons. The best meaning that can be given to such a naive phrase as "something is made of certain constituents" is that a theory of a process in which these constituents are postulated to play a role leads to agreement with experiment. Photodisintegration of the deuteron can be considered the nuclear equivalent of the spectroscopy of the hydrogen atom, though nothing like as rich in detail and precision, in fact a rather poor relative as far as its "spectrum" is concerned, which in this case has no discrete lines.

In May 1933 I left Berlin for Cambridge, England, where Rutherford (see Fig. 1) had accepted me as a research student at the Cavendish Laboratory. As Herman Feshbach told you in his introduction, I had intended to start a thesis with Schroedinger, but during, or soon after, our first serious discussion we both decided to leave, and it was probably he who recommended me to Rutherford. I started theoretical work with R. H. Fowler in October 1933. In those days one didn't yet feel the hot breath of competition, certainly not at the Cavendish. You could have an idea that you thought important, you could sit on it for a year or so without being afraid of losing it, and if you did lose it nevertheless, you said to yourself, "Now I can go on to the next idea."

In April 1934 I was writing a paper on the role of spin in nuclear reactions ("On the Probability of Artificial Nuclear Transformations and Its Connection with the Vector Model of the Nucleus," Proc. Cambridge Phil. Soc. 30, 561, 1934). I had to know the energy released in some of the simple reactions with lithium isotopes which had been studied by Cockcroft and Walton. Somebody at the Cavendish--I believe it was Mott, though I am not quite sure--

told me that Chadwick (Fig. 2) would know the best values for the masses of these isotopes.

I went to see Chadwick to ask him about the masses and after he had enlightened me I suddenly found the courage to tell him of my idea that the photodisintegration of the "diplon"--as the heavy hydrogen nucleus was then still called by Rutherford--might be worth trying, using the highest energy γ rays then available. These were the homogeneous γ rays of radiothorium, which in equilibrium contains a daughter product, thorium C", that emits a 2.62-MeV γ ray. This γ ray had been discovered by Ellis, and a source of a few millicuries of radiothorium was available at the Cavendish. Thus, the photonuclear experiment would not have been easy in many places but it happened to be relatively easy at the Cavendish. Chadwick, gentleman that he was, listened politely, but seemed to catch fire only when the point was brought out that one could determine the mass of the neutron rather accurately from a measurement of the "photoproton" energy. Perhaps his interest was aroused because the question of the correct neutron mass was a hotly debated one at that time. It was then not even certain whether the neutron was heavier or lighter than the proton. Our conversation ended with no explicit commitment from Chadwick.

About six weeks later, when I showed him a "Letter" concerned with a then premature subject, later known as "Delayed Neutrons," which I wanted to send to Nature ("Spontaneous Emission of Neutrons by Artifically Produced Radioactive Bodies," Nature 134, 25, 1934), he asked: "Were you the one who suggested the photodisintegration of the diplon to me?" When I answered yes, he said, "Well, it works--for the first time last night. Would you like to work on it with me?" Since I realized by then that I enjoyed pursuing questions which could be answered by experiment, I was glad to accept this

chance to change from theory to experiment. I went to see Fowler to ask his permission, and he immediately agreed. Things were very informal at the Cavendish in those days. Experiment and theory were not yet as separated as they are today in many places, and, anyhow, the overall official supervisor of the research students was Rutherford, who I suppose was consulted. I joined Chadwick in his own research room. Since he was pretty busy as Assistant Director of Research concerned with the progress of all experimental research students, he allowed me quite a bit of leeway in continuing the further experiments which we undertook. In these experiments I was helped by Chadwick's very capable assistant, H. Nutt, who is known from a line in a Cavendish song: "N for Nutt who discovered the neutron." This help was, of course, invaluable to a greenhorn in experimentation. Ever since, though, I have tried to remain a crypto-theorist, as Herman Feshbach realized.

After about six weeks of intensive work, we started to write a "Note" to Nature. (See Paper No. 1 in a collection of papers on "Photonuclear Reactions," edited by E. G. Fuller and E. Hayward, Benchmark Papers in Nuclear Physics, Vol. 2, Dowden, Hutchinson & Ross, Inc., 1976. I shall refer several times to this large and very useful collection.)

By observing the photoprotons from the reaction

$$D + \gamma \rightarrow H + n \qquad (1)$$

we could measure the approximate energy release as well as the cross section. We used a sensitive ionization chamber filled with D_2 gas. Since the photoneutron carries away approximately the same energy as the photoproton, we could write, in suitable units, using Einstein's two famous equations for

the photoelectric effect and for the equivalence of mass and energy:

$$m_n = m_D - m_H + E_\gamma - 2\,E_p\ . \qquad (2)$$

We calibrated the ionization chamber roughly with α particles and found $E_p \approx 250$ keV. This led to a neutron mass $m_n = 1.008$, definitely larger than that of the proton. Chadwick, Feather and Bretscher (Proc. Roy. Soc. London Ser. A 163, 366, 1937) measured the range of the photoprotons in a cloud chamber containing deuterium and obtained a more accurate Q value. The best values on the present-day mass scale ($^{12}C \equiv 12$ mass units) are $m_n = 1.008665$ and $m_H = 1.007825$. I remember being quite shocked when it dawned on me that the neutron, an "elementary particle," as I had by that time already learned to speak of it, might decay by β emission with a half-life that I could roughly estimate from the existing systematics for the β decay of complex nuclei to be about half an hour or shorter, since the neutron was a simpler system. Of course, we are by now quite used to the idea that elementary particles can decay. I believe the first one for which the lifetime was explicitly measured was the μ meson, and the half-life of the neutron is now known to be 10.6 min. Since the development of reactors, the spectrum has been measured by modern techniques with high precision, and the neutron does indeed exhibit a nice continuous β spectrum with an end point of 780 keV, in agreement with the mass difference.

Bethe and Peierls followed the γ-D experiments by a detailed and very successful theory of the deuteron and the photoelectric effect (Paper No. 2 in Fuller and Hayward's book.) It is perhaps fortunate that quantum mechanics was developed at a time when electromagnetic interactions were considered to be the only forces on a microscopic scale. Now one could assume that quantum

mechanics would be valid in the new situation found in nuclei and thus one had to guess only about the new forces, not about quantum mechanics at the same time. I believe it would have been very hard for theory to make a double step simultaneously, and it was fortunate that we didn't know any particles other than protons and electrons at the time quantum mechanics was created. It is sometimes better if a discovery is postponed: a strange lesson to give anybody! The discovery of the neutron, as we heard from Hans Bethe, removed a major difficulty, permitting Heisenberg, Majorana and Wigner to extend the use of quantum mechanics to the behavior of protons and neutrons in the nucleus. Bethe and Peierls made the simple assumption that the forces between protons and neutrons were of very short range and pretty strong. They did not need to know the details--this was the beauty of their theory--but were able to calculate the cross section with only one parameter, the binding energy of the deuteron. Refinements of this theory have been made by many physicists, and we especially miss here today one who has perservered in this field over many years, but was unable to attend this symposium: Gregory Breit.

To test the electric dipole character of the photodisintegration of the deuteron further we made a rough measurement of the angular distribution of the photoneutrons (Fig. 3). In our simple arangement we used a cylinder (C) filled with heavy water, and we had two positions for our γ-ray source, A or B, at equal distances from C, a lead block to absorb γ rays, a paraffin block to slow down the neutrons, and a boron counter inside the paraffin to detect the slow neutrons. This was one of the early angular distributions studied in nuclear reaction physics, but since the angles were very poorly defined we could not expect any very large effect. We found twice the intensity at position B as at position A. This means that most photoneutrons are emitted at right angles to the γ ray, which agrees with expectations for an electric dipole transition.

To get the heavy water was quite a business. I went to Rutherford and asked his permission to use heavy water, and he wrote a note to Oliphant, which I kept as a memento (Fig. 4). This was a good fraction of the world's supply of heavy water at that time, and it was carefully handled. Since our experimental cross section and angular distribution were in rough agreement with the theory, we could conclude that the deuteron was really "made" of protons and neutrons. Today we know this is only a good approximation, since mesons also play a role. The cross section for photodisintegration of the deuteron has by now been pursued up into billions of electron volts of γ-ray energy, and it is an interesting curve. Figure 5 shows a printout from the photonuclear data bank being maintained at the Bureau of Standards under the direction of Everett Fuller, one of the editors of the Benchmark book on "Photonuclear Reactions." The curve is a theoretical one, based on effective range theory with a slight correction for the photomagnetic effect near threshold, which I shall come to shortly. Note the logarithmic energy scale. Anyone with a one-track mind, pursuing the cross section to higher and higher energies, would have found an interesting bump, which was actually found only after the much bigger meson resonance had been discovered. The photomeson production leads indirectly to a resonance also in the "direct" photodisintegration cross section: Sometimes it would have paid to have a one-track mind!

An interesting speculation on the possible existence of "slow" neutrons turned up while we were writing our "Note." Some of those present today may remember part of this story.

Our approximate experimental cross section permitted us to estimate the inverse cross section

$$H + n \rightarrow D + \gamma . \qquad (3)$$

In fact, the inverse reaction for ~1-MeV neutrons would just correspond to the same center-of-mass energy as the one we studied in reaction (1), and we could therefore safely make a thermodynamic argument for the cross section expected for reaction (3) for 1-MeV neutrons. A little earlier D. E. Lea at the Cavendish had carried out experiments in which he had observed γ rays emitted when fast neutrons impinged on paraffin. These were polonium-beryllium neutrons which are on the average even a bit more energetic than 1 MeV, and he interpreted his results as due to reaction (3). But according to our estimate his observed γ-ray yield was much too large to be explained by this reaction for the fast neutrons he used, by something like a factor of a hundred to a thousand. It was really an impressive discrepancy.

We therefore speculated that in Lea's experiment the fast neutrons were first slowed down and <u>then</u> captured as <u>slow</u> neutrons. But speculation was somewhat frowned upon at the Cavendish, and Chadwick convinced me not to include it in our final version of the "Letter" to <u>Nature</u>; thus the puzzle we had encountered was noted only with the following remark: "A satisfactory explanation is not easy to find and further experiments are desirable." --a typical cautious Cavendish remark. Rutherford allowed himself his famous speculation on the existence of a neutron only in a lecture, his Bakerian Lecture of 1920. In papers he did not speculate readily. By the way, since

this question came up in the discussion this morning, Rutherford had made the mistake of thinking that some long-range α particles were ^{3}He, but when a genius makes a mistake it has its uses, and so he speculated from ^{3}He down to the neutron. He added a remark which was very important, saying in effect that the neutron seems almost necessary to explain the building up of the nuclei of heavy elements!

I do not know whether Chadwick talked to Rutherford about our speculations, but about three months later, when the news of the discovery of slow neutrons by Fermi and his collaborators in Rome reached the Cavendish, solving the puzzle we had found, Rutherford sought me out and turned out to be well informed about our speculations. In fact he practically rushed into my lab, and I can only now psychoanalyze this with hindsight: he seems to have had a bad conscience, but I cannot state this as a fact. I wrote down my recollections of these events for a volume honoring Feather's Jubilee at Edinburgh ("Remarks on the Prehistory of the Discovery of Slow Neutrons," Proc. Roy. Soc. Edinburgh (a), 70, 17, 1971/72). Peierls, who was aware of our speculations, had pointed out in a letter to me that the cross section for reaction (3) would go to zero at zero neutron energy because electric dipole radiations would then give a zero effect; so to say, a p-wave would be needed. The finite cross section which Otto Frisch and others measured accurately later was explained finally in 1936 by Fermi by using the spin dependence of the n-p forces postulated by Wigner and calculating the spin flip magnetic dipole transition.

Until a few years ago, there was still a small discrepancy between experiment and theory in the spin flip capture of slow neutrons by protons,

namely, the experiment gave a cross section about 10% larger than the theory. This was explained by Riska and Brown as being due to meson exchange and nucleon resonance effects. Gerry Brown tells me that ~7% can be ascribed to meson exchange and ~3% to a Δ resonance, although the latter is somewhat model dependent.

The magnetic dipole transition was taken into account in the cross section for the photodisintegration of the deuteron shown in Fig. 5. It is a small effect but important near the threshold. Incidentally, on that curve there are no experimental points near threshold; it would be important to check the theoretical cross section curve there, which is based solely on the value of the thermal neutron capture cross section.

In 1934 we knew of only two nuclei, D and ^{9}Be, for which the γ rays then available had sufficient energy to permit photodisintegration. Szilard and Chalmers were soon able to observe the photoneutrons from Be, using the ingenious Szilard-Chalmers method for separation of radioactive ^{128}I. In this method, an organic compound of normal ^{127}I is bombarded with neutrons. In the capture of the neutrons, most of the time the ^{128}I gets knocked out of the molecule because of the recoil given to it by the neutron that is absorbed and the capture γ rays that are emitted. The knocked out ^{128}I is then separated, and thus the activity is concentrated. In this way one can discover low intensity neutron activities. Szilard told me a story a little different from what Otto Frisch remembered concerning how he found this separation. He said he was trying to think of a way of incorporating ^{128}I, the radioactive iodine, into a molecule that would be of some medical use, for treatment or diagnosis. He was therefore thinking of bombarding a compound with neutrons, but realized that due to the neutron

bombardment--remember he was still using fast neutrons--the compound would be destroyed. He turned this apparent defeat into a positive method of concentrating an activity, which was a true Szilardian touch (See M.G., Introduction to Papers in Nuclear Physics by Leo Szilard: The Collected Works of Leo Szilard: Scientific Papers, B. T. Feld and G. W. Szilard, Eds., The MIT Press, Vol. 1, p. 139, 1972). Chadwick and I studied the angular distribution of the photoneutrons from Be in an arrangement similar to that in Fig. 3, using Rn γ rays, and found the distribution to be symmetrical. We interpreted this as due to a neutron being emitted from an $\ell = 1$ state in ^{9}Be. I believe that is still considered correct. I read it just last night in our old paper; I had forgotten it myself. It was a little bit of "single particle thinking."

In 1937 Bothe and Gentner made an important step forward by using a new γ-ray source of much higher energy, the 17-MeV γ rays emitted in the $^{7}Li(p,\gamma)$ reaction, which I believe were found at Cal Tech. This γ energy is sufficient to photodisintegrate most nuclei. There are very few thresholds above 17 MeV. Bothe and Gentner observed radioactivity in very many nuclei, e.g., $^{63}Cu(\gamma,n)^{62}Cu$ (10 min). It would have been hard to look for the photoneutrons in this case because around an accelerator there is usually some neutron background, and it was much easier to look for the delayed activity that gives an easily detected β ray for each photoneutron ejected.

The war interrupted this story, and there was not much progress in the photonuclear field until after the war. Early in 1948 Baldwin and Klaiber in a classic paper (No. 5 in the Fuller and Hayward volume) reported the use of x rays up to 100 MeV from the G.E. Betatron to induce (γ,n) reactions in

a number of nuclei. They found a cross section which increased rapidly from the threshold, reached a maximum, and then decreased rapidly as the energy of the x rays rose beyond about 20 MeV. They interpreted the decrease as due to competition from (γ,2n) reactions. Since they measured, e.g., in Cu, only the 10-min ^{62}Cu, the longer lived activity of ^{61}Cu would have remained unnoticed. I found their explanation unsatisfactory because the drop in intensity seemed much too precipitous, and it seemed worth while to try to interpret their data as a resonance process instead. Soon Perlman and Friedlander showed that ^{61}Cu was indeed only weakly produced.

In the spring of 1948 I was spending a day at Argonne National Laboratory hunting neutron-induced isomers with Ed der Mateosian, Mike McKeown and Carl Meuh hause. I ran into Edward Teller and asked him whether it would not make sense to consider the apparent resonances found by Baldwin and Klaiber as nuclear analogues of the so-called "Restrahlen" (residual rays) which Rubens first studied long ago in polar crystals. These resonances, which are seen in the infrared, had been explained as due to the motion of a positive ion lattice against the negative, and Teller was well acquainted with this theory. In the nucleus, of course, this would be a vibration of the protons against the neutrons set in motion by the γ rays, an early example of collective behavior in an electromagnetic transition. With various assumptions about restoring forces and the role of the nuclear surface we were able to calculate the resonance energies E_0, for which we obtained for one model, which turned out to be a happy approximation, a dependence on mass proportional to $A^{-1/3}$, i.e., inversely proportional to the nuclear radius. (Paper No. 6 in the Fuller and Hayward book.)

Figure 6, a printout (from the photonuclear data bank at the Bureau of Standards) which Fuller kindly sent me, shows the production of photoneutrons from ^{197}Au. Note that the (γ,2n) reaction indeed sets in only well after the maximum cross section has been reached.

Figure 7 shows the product of the resonance energy E_0 and $A^{1/3}$ as a function of mass number A. Above A ~130 this product approaches a constant value.

Figure 8 shows an interesting example, ^{181}Ta, where the γ-induced nuclear reaction has a double hump appearance, predicted independently by Danos and by Okamoto for nuclei that are strongly deformed and hence have a large static quadrupole moment. The reason for the double hump is simply that there are different effective radii for the long and the short axes.

Figure 9 shows how this can be explicitly proved by aligning nuclei, in this case ^{165}Ho, which is easier to align than Ta and is also highly deformed. The Ho nucleus is aligned either at right angles or parallel to the photon beam.

For the lightest nuclei, where the photoeffect shows a lot of structure, the single-particle model is much more effective. The reason why our approach works best for heavier nuclei is that a light nucleus is "all surface," while we had postulated a volume effect.

There have been many variations and improvements of our 1948 paper on the giant dipole resonance, using both the collective and the single-particle approach, starting on the collective side with Steinwedel and Jensen (Paper No. 7 in Fuller and Hayward's book), and on the single-particle side with Burkhardt (Paper No. 10), followed by Wilkinson (Paper No. 11), Brown and Bolsterli (Paper No. 14), and others. In a paper to appear shortly (Phys. Rev. C) Myers, Swiatecki, et al. discuss a macroscopic model in which the motion is treated as a combination of the displacement mode and the acoustic mode.

In 1950 Bethe and Levinger (Paper No. 8) considered the importance of sum rules where no specific models are assumed for the nuclear vibrations. Sum rules permit counting of the number of charges involved in the photoelectric transition, and any excess found is in a way a measure of meson effects that are otherwise hidden. These sum rules are therefore very important. They have now been pursued to high energies. Experiments on total cross sections up to about 100 MeV show how some sum rules saturate by then and others do not. Unknown to all of us, A. B. Migdal (Paper No. 4) had considered nuclear dipole vibrations in a 1944 paper, which reached this country after World War II and to which Levinger drew attention later in his book ("Nuclear Photodisintegration," Oxford University Press, New York, 1960). Migdal was guided by the observation, already contained in Bethe's 1937 review, that dipole transitions in the low energy regions are comparable in rate only to quadrupole transitions; but remember in these days no absolute rates were known. In a sense, the deuteron photodisintegration rate was the first absolute electromagnetic rate, and because it agreed with theory it gave us courage to go into other ramifications of electromagnetic transition rates.

The nuclear photoelectric effect plays a role in astrophysics, in nuclear technology, in the identification of elements and isotopes, and in other fields. I just heard an interesting talk by J. Peebles where he reminded us that in the first minute after the "Big Bang" the most important reactions going on were $n + p \longleftrightarrow d + \gamma$. The rate of these reactions, which can be considered the first and simplest nuclear reactions, determines the ultimate $H:D:{}^4He$ ratio, which is of such importance for the world we live in. Had we ended up with a fifty-fifty mixture of deuterium and hydrogen it might have perhaps confused some biologically important molecules.

During the late thirties while still at the Cavendish, and more so later at Illinois and Brookhaven, my research interests turned to isomeric transitions in nuclei, very naturally in a way since these were γ-ray transitions but of different multipole order. We were able to classify all the known isomeric transitions into a number of electric and magnetic multipole transitions, which we designated E0– E5, and M1—M4 (M.G. and A. W. Sunyar, Phys. Rev. 83, 906, 1951; R. D. Hill and M.G., Rev. Mod. Phys. 24, 179, 1952; and M.G. and J. Weneser, Ann. Rev. Nucl. Sci. 5, 1, 1955). For most excited nuclear states of relatively low energy, say <1 MeV, the lifetime is now measurable and usually known. Techniques have been developed over the years to measure both very short lifetimes, of the order of 10^{-14} sec or less, and very long lifetimes. Some of the longest lived isomers have lifetimes of several centuries. Thus there is a huge ratio, $\sim 10^{23}$, over which transition rates can be studied. Most people look down their noses at work with something as unfundamental as heavy nuclei. However, if one is interested in high multipole transitions, one is forced to go to heavy nuclei. As we have already seen, for the deuteron there is a great paucity of nuclear levels because of the short range of the forces where most levels get squeezed out and the deuteron has no bound excited state. In fact, going up in mass, the first bound excited state occurs for mass seven, in ^{7}Li and ^{7}Be; therefore, to study transition rates of different multipoles, one must use heavier nuclei.

Among the results of our classification of isomeric transitions, two are worth mentioning. The M4 transitions were particularly related to the closed shells discussed by Maria Mayer and by Jensen, Suess and Haxel, because the M4 transitions appeared systematically, e.g., below the 50 shell as p1/2 ⟷ g9/2 transitions. We found M4 isomers just below 50 but not

immediately above until one gets close to the next shell with 82 protons or neutrons. Let us see how well the transition rates agree with just a simple single particle radiating. The formulae for the expected rates were calculated by Weisskopf and Moszkowski. Figure 10 shows first of all that the predicted E^{-9} dependence in the γ-ray lifetime for M4 transitions is correct. The line shown has the correct slope. We drew this particular line through the points; the line suggested by Weisskopf's formula would be very close. Weisskopf himself didn't take his absolute values too seriously and was very surprised when he saw this. In fact we didn't get it into such good shape until one day, on the way by train to M.I.T. to give a colloquium, while going over my slides I noticed that the points looked like two lines. By making a statistical weight correction, whether the transition goes, e.g., from p1/2 to g9/2 or the other way, equivalent to a factor of 5 in this case, the points were made to coalesce into a single line in time for the colloquium.

Another result worth mentioning (see the paper with Sunyar) is that in the region of the highly deformed nuclei, which have large static quadrupole moments, we found so-called fast E2 transitions (faster than the single-particle formula predicts by factors up to ~100), and we explained these as a "cooperative" phenomenon whereby the nucleus as a whole was undergoing quadrupole motions. This is, one might say, the dynamic analogue of Rainwater's explanation of the static quadrupole moments, and it helped in the formulation of the unified theory of nuclei by Bohr and Mottelson, who considered these particular fast E2 transitions as related to shape deformation. They had assumed rotational states where the rotor keeps a constant moment of inertia in the various states of spin, 0, 2, 4, 6, etc. But as more data appeared, it could be shown that ground state bands of all even-even

nuclei, except the closed shell nuclei, can be systematically described by one formalism if it is assumed that the moment of inertia increases in a regular way (Scharff-Goldhaber, Dover and Goodman, Ann. Rev. Nucl. Sci. 26, 239, 1976).

In this connection one more investigation may be of interest. It was mentioned this morning that the question was perhaps still unresolved then in the minds of some people (though resolved in most people's minds) whether γ rays are emitted first and then converted or whether the conversion of γ rays is really a direct process with the electron removing energy from the nucleus in competition with γ-ray emission. The second was generally believed. Experiments by Segrè and Wiegand, and by Daudel et al. on the lifetime changes in the electron capture decay of ^{7}Be, which manifestly depends on the electron density at the nucleus and which could be changed by changing its chemical binding, indicated a method. We could look for lifetime changes in internal conversion by making different chemical compounds. If the γ rays were emitted first and later absorbed, it shouldn't matter much what the compound is. However, if the electron can directly induce transitions, then of course the lifetime can be changed by pushing the electrons either closer to the nucleus or farther away, which can be done by making different chemical compounds.

In 1951 Bainbridge had a year's leave from Harvard and came to Brookhaven. Together we undertook a search for the effect of chemical structure on lifetime, using the isomer with the lowest transition energy known at the time, ^{99}Tc. A 2-keV, highly converted E3 transition of 6-hr half-life is followed by an easily detected M1 γ ray of 140 keV. Since we were interested in seeing M4 transitions for our collections of points for the M4 line (Fig. 10), we had to scrounge M4 transitions wherever we could. Therefore, since we could estimate how long ^{99m}Tc would live if the intermediate

level at 140 keV were not there, we just calculated what the intensity of the crossover transition should be, and sure enough we found it to have exactly the predicted intensity (see Fig. 11). Thus, we could put one more point on the M4 line where we were already expecting it. In the course of these experiments, Elizabeth Wilson (now Mrs. Charles Baker) of our Chemistry Department, who was working with us, learned to make very pure ^{99}Tc sources. Bainbridge found beautiful lifetime differences between different compounds (metal, oxide, sulfide), and a theory was developed by John Slater, also visiting Brookhaven then, which roughly showed that for those of our compounds in which the electron density was expected to be higher at the nucleus, the lifetime was shorter (K. T. Bainbridge, M.G., and E. Wilson, Phys. Rev. 84, 1260, 1951). Thus we proved explicitly that internal conversion competes with γ-ray emission, a point which, as I said, was by that time already implicitly believed by most people concerned with such questions.

In the last two decades the study of electromagnetic interactions with nuclei and its natural continuation to single nucleons has moved into new fields, particularly since one has learned to use inelastic electron scattering on nuclei and also, of course, on nucleons. By treating the electron as a source of virtual photons, one can measure many more giant resonances: M1, etc., E2, etc. Excitations that cannot be reached by real photons can be measured with the help of virtual photons, and of course the giant electric dipole resonance also shows up here very nicely. Much work is being done in that way.

The field of electromagnetic interactions of nuclei is now a huge one. To summarize what we have learned: If you want to "look" at a nucleus in a relatively gentle way, why not use photons; and, if you cannot look with real ones, why not use virtual ones.

I should like to thank Everett Fuller and Evans Hayward of the National Bureau of Standards for their invaluable help in preparing this talk.

LEGENDS TO FIGURES

1. Picture taken by Prof. P. Harteck and kindly given to me.
2. Picture of Sir James Chadwick (from H. Massey and N. Feather, Biographical Memoirs of Fellows of the Royal Society 22, Nov. 1976).
3. Arrangement for measuring angular distribution of photoneutrons from $D + \gamma \rightarrow p + n$ (J. Chadwick and M.G., Proc. Roy. Soc. London Ser. A 151, 479, 1935).
4. Reproduction of a penciled note from Rutherford to Oliphant, undated, early 1935: "Hand over 25 cc tube of heavy water to Goldhaber for the time being."
5. A summary of theoretical and experimental cross section data on the photodisintegration of the deuteron from threshold to 1 GeV. The solid curve is based on a Bethe-Peierls effective range theory. The magnetic dipole cross section of L. Hulthén and B.C.H. Nagle (Phys. Rev. 90, 62, 1953) has been smoothly joined to it. The calculation of F. Partovi (Ann. Phys. NY 27, 79, 1964), represented by the symbol N, extends to meson threshold and includes multipoles up to L = 3. The experimental data at low energies come from the review article by L. Hulthén and M. Sugawara (Encyclopedia of Physics 39, p. 1, Springer, Berlin, 1957). Near 20 MeV the (γ,p) cross section of D.M. Skopik, Y.M. Shin, M.C. Phenneger, and J.J. Murphy, II, (Phys. Rev. C9, 531, 1974) coincides nicely with the attenuation measurement of J. Ahrens, H.B. Eppler, H. Gimm, M. Kröning, P. Riehn, H. Wäffler, A. Zieger, and B. Ziegler, (Phys. Lett. 52B, 49, 1974). The (γ,p) measurements extending to 65 MeV are from L. Allen, Jr. (Phys. Rev. 98, 705, 1955). The (γ,p) cross section at the meson threshold region is from the work of J. Buon, V. Gracco, J. Lefrancois, P. Lehmann, B. Merkel, and Ph. Roy (Phys. Lett. 26B, 595, 1968).

6. The photoneutron cross section for gold. The open circles represent the total photoneutron cross section, $\sigma(\gamma,n) + \sigma(\gamma,2n) + \sigma(\gamma,3n)$. The triangles are twice $\sigma(\gamma,2n)$ and the crosses ten times $\sigma(\gamma,3n)$. The solid lines are merely to guide the eye. The total cross section is fitted by a Lorentz line having the parameters: $E = 13.70$ MeV, $\Gamma = 4.75$ MeV, and $\sigma = 540$ mb. These results were reported by A. Veyssiere, H. Beil, R. Bergère, P. Carlos, and A. Lepretre (Nucl. Phys. A159, 561, 1970).
7. The giant resonance energy times $A^{1/3}$ (which is proportional to the nuclear radius) as a function of A (from E. Hayward, Photonuclear Reactions, NBS Monograph 118). The horizontal line corresponds to the $E_o = 80A^{-1/3}$ prediction of the hydrodynamic model with up-to-date parameters. The data for the light nuclei are from J.M. Wyckoff, B. Ziegler, H.W. Koch, and R. Uhlig, (Phys. Rev. 137B, 576, 1965). The data for the heavy nuclei were obtained at Livermore (B.L. Berman, Atomic and Nuclear Data Tables 15, 319, 1975).
8. The photoneutron cross section for tantalum, $\sigma(\gamma,n) + \sigma(\gamma,2n) + \sigma(\gamma,pn)$. Circles, triangles, and alternate dots present four independent determinations of the cross section from the activation curve. The errors indicated are standard deviations based on the counting statistics. The open and closed squares represent various choices for the correction for the emission of two neutrons in the $(\gamma,2n)$ process. The solid curve is the sum of two Lorentz lines having the parameters: $E_a = 12.45$ MeV, $\sigma_a = 308$ mb, $\Gamma_a = 2.3$ MeV, $E_b = 15.45$ MeV, $\sigma_b = 348$ mb, and $\Gamma_b = 4.4$ MeV. The two resonances correspond to charge oscillations along the one long and two short axes of the nuclear ellipsoid. (From E.G. Fuller and M.S. Weiss, Phys. Rev. 112, 560, 1958.)

9. The total photoneutron cross section, $\sigma(\gamma,n) + \sigma(\gamma,2n) + \sigma(\gamma,pn)$, measured for a holmium target polarized parallel and perpendicular to the incident photon beam. The solid lines are for the situation in which the unpolarized cross section was represented by the sum of two Lorentz lines having the parameters: $E_1 = 12.28$ MeV, $\Gamma_1 = 2.5$ MeV, $\sigma_1 = 215$ mb, $E_2 = 15.78$ MeV, $\Gamma_2 = 5.0$ MeV, and $\sigma_2 = 249$ mb. (From M.A. Kelly, B.L. Berman, R.L. Bramblett, and S.C. Fultz, Phys. Rev. 179, 1194, 1969.)

10. Normalized lifetime-energy relations for M4 transitions with spin correction. The line shown is fitted to the experimental points and is given by the equation

$$\tau_\gamma \text{ (sec)} = \frac{1.0 \times 10^4 (2J_i + 1)}{A^2 E^9} \text{ (E in MeV)} \quad ,$$

where the simplified statistical weight correction $(2J_i + 1)$ permitted us to lump together the isomer families from different shells. (From M.G. and A.W. Sunyar, Phys. Rev. 83, 906, 1951.)

11. ^{99m}Tc decay scheme. (From J.W. Mihelich, M.G., and E. Wilson, Phys. Rev. 82, 972, 1951.)

LORD RUTHERFORD
April 1934

FIG. 1

J. Chadwick.

FIG. 2

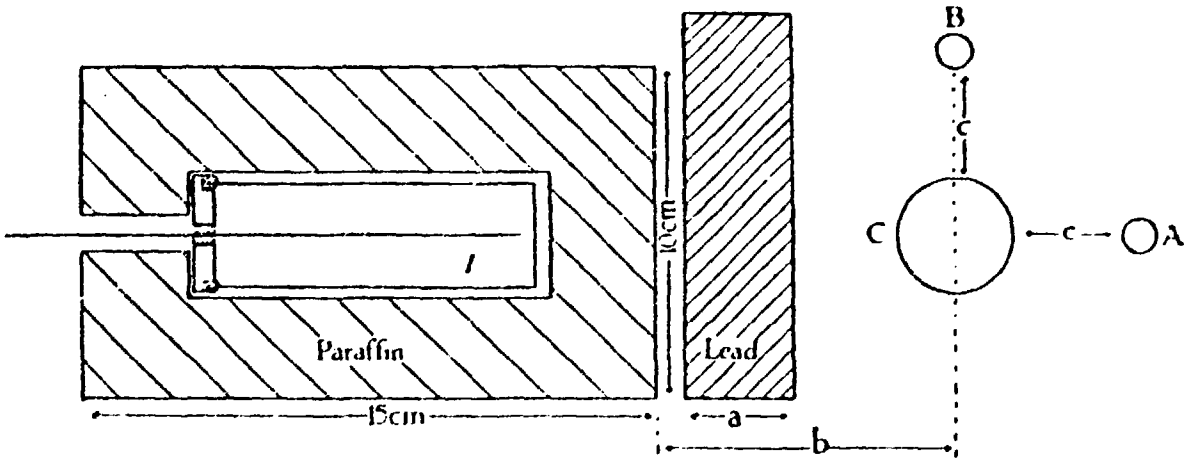

FIG. 3

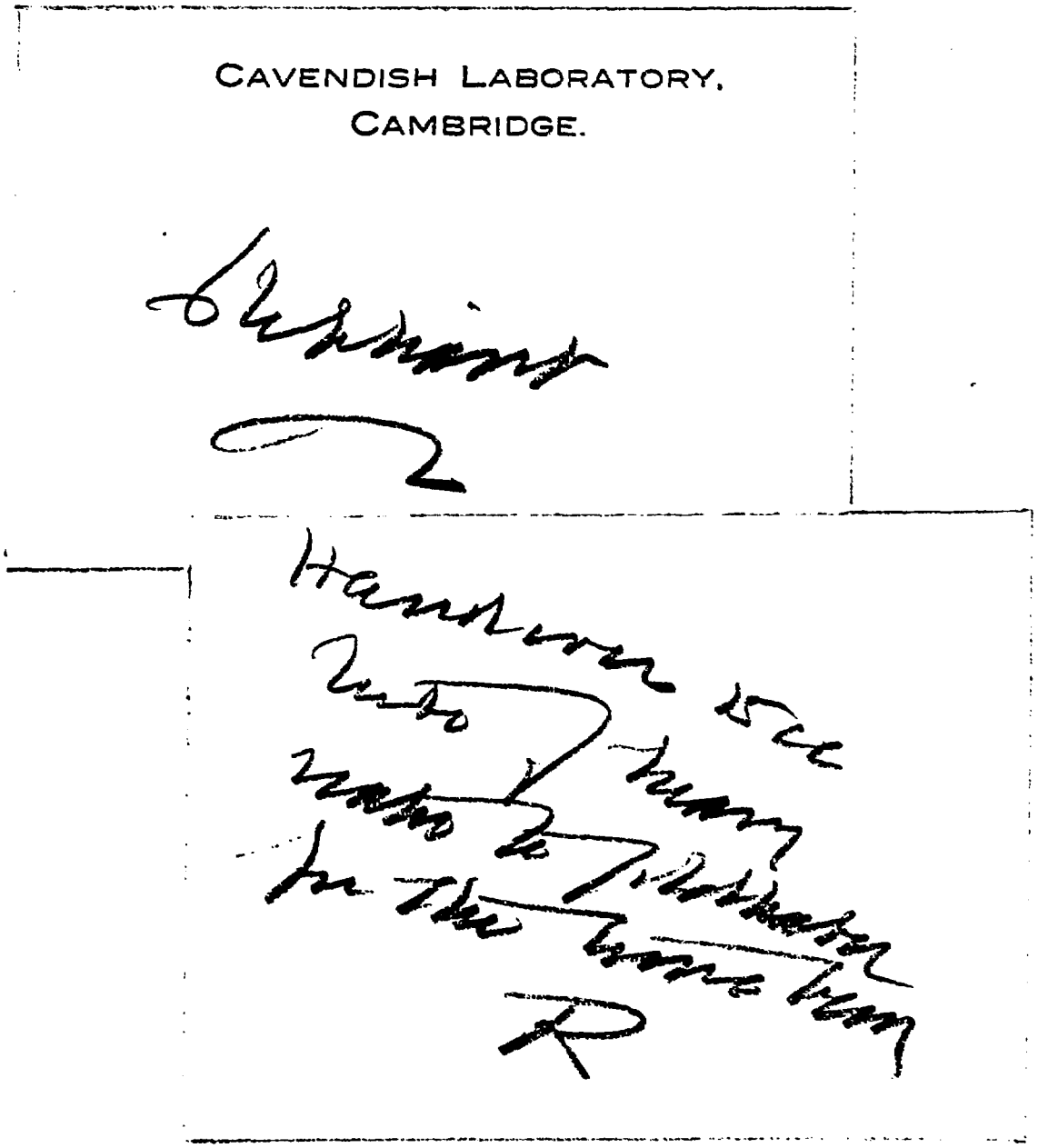

FIG. 4

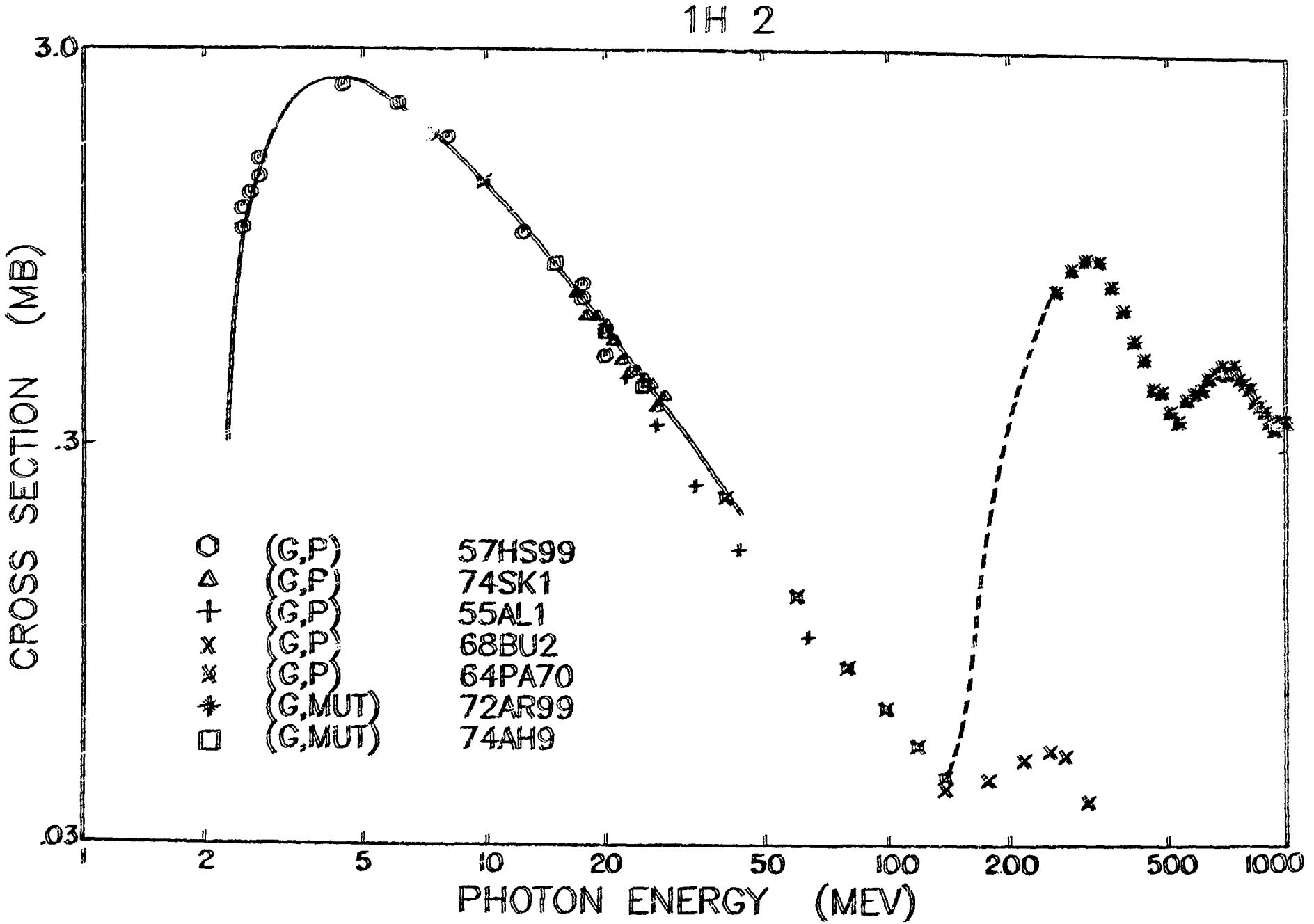
1H 2
3.0
.3
.03
CROSS SECTION (MB)
2
5
10
20
50
100
200
500
1000
PHOTON ENERGY (MEV)
(G,P) 57HS99
(G,P) 74SK1
(G,P) 55AL1
(G,P) 68BU2
(G,P) 64PA70
(G,MUT) 72AR99
(G,MUT) 74AH9

FIG. 5

Gold-197

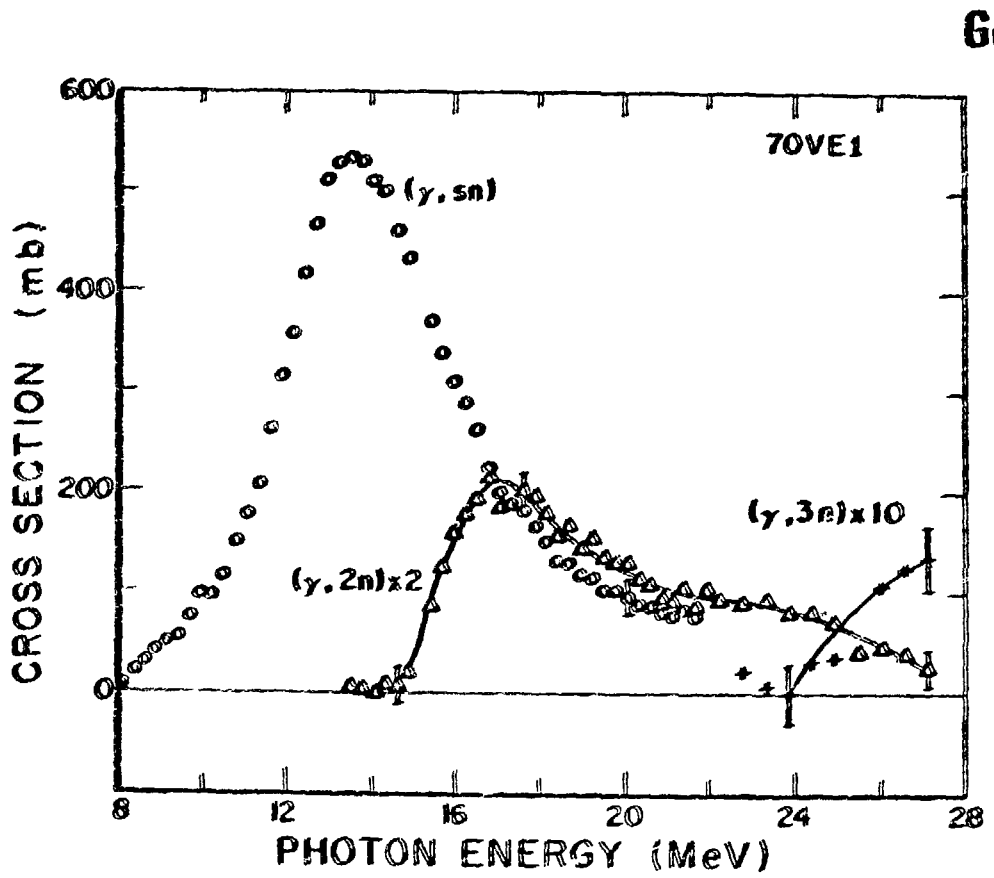

FIG. 6

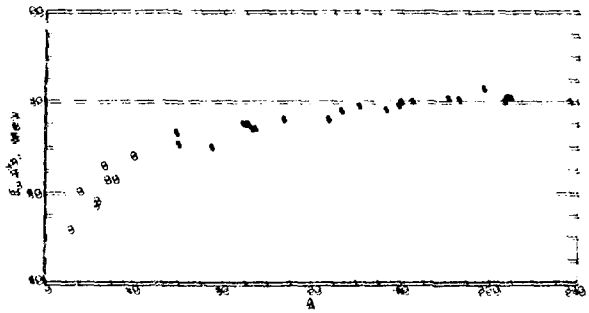

Fig. 7

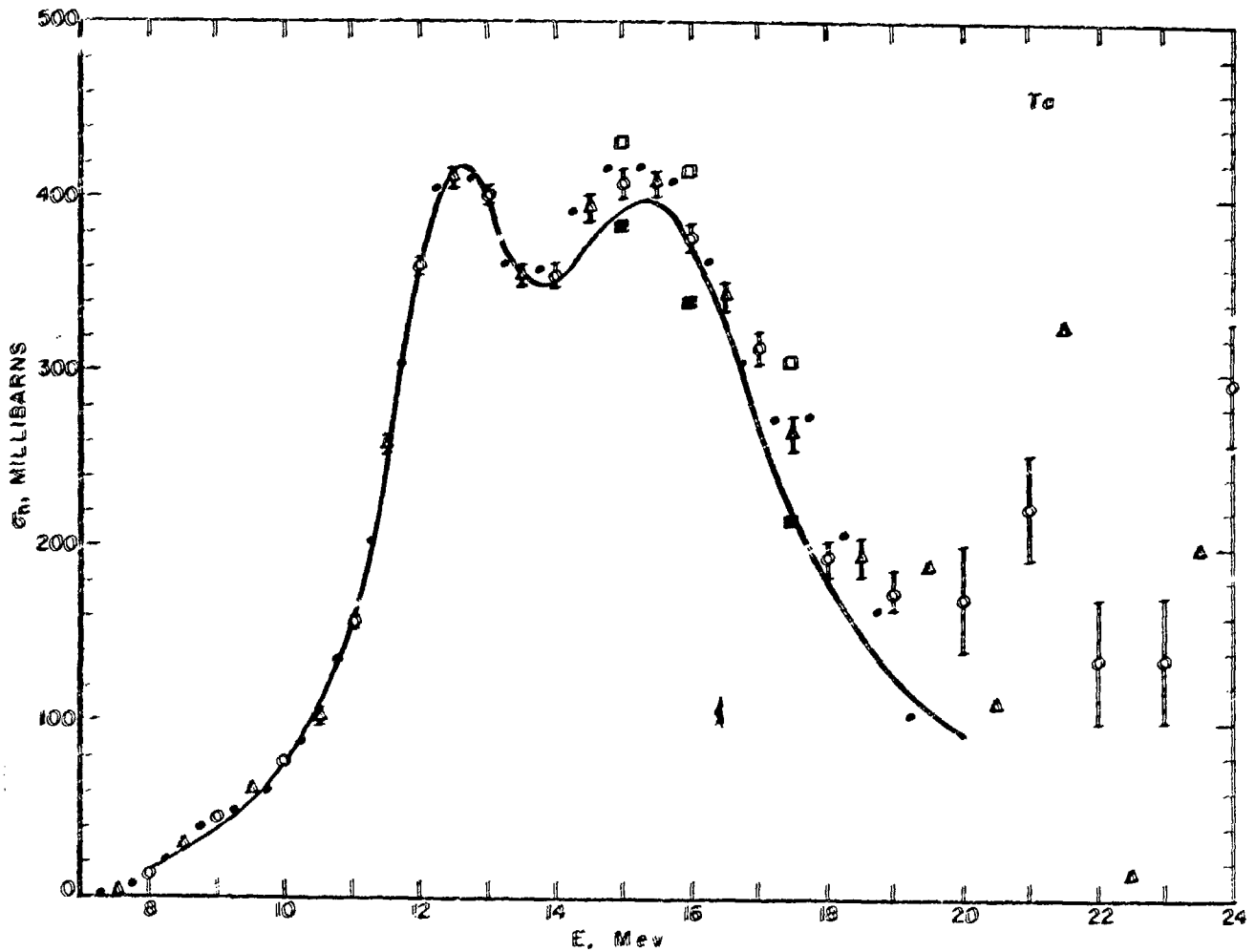
Tc
σ_n, MILLIBARNS
E, Mev
0
100
200
300
400
500
8
10
12
14
16
18
20
22
24

FIG. 8

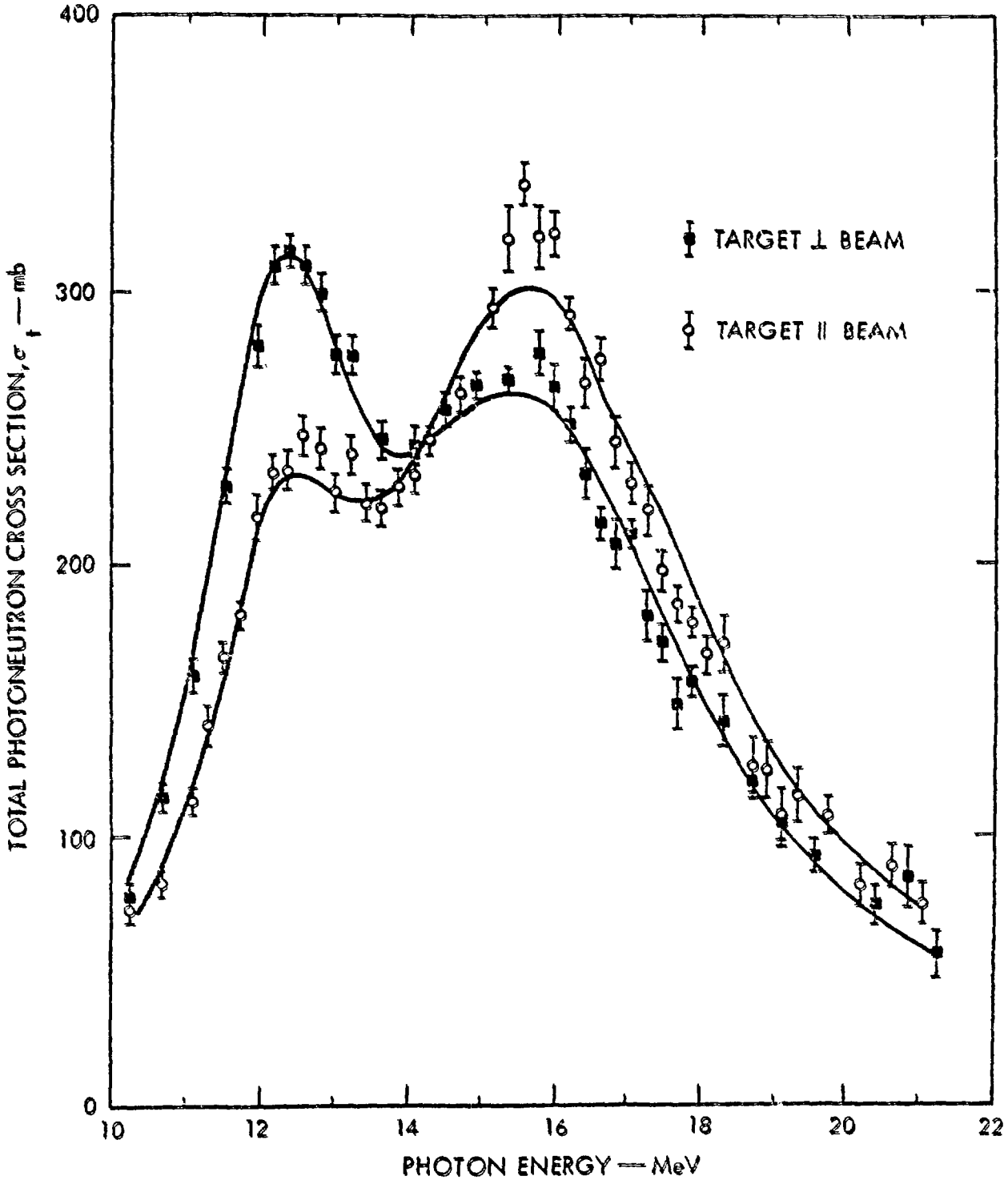
400
300
200
100
0
10
12
14
16
18
20
22
TOTAL PHOTONEUTRON CROSS SECTION, σ_t — mb
PHOTON ENERGY — MeV
TARGET ⊥ BEAM
TARGET ∥ BEAM

FIG. 9

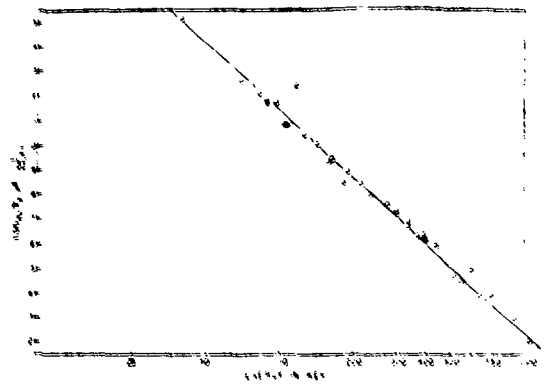

FIG. 10

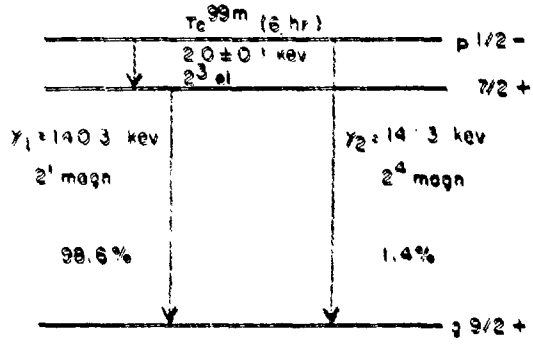
Tc99m (6 hr)
p 1/2 -
2.0 ± 0.1 kev
2.3 el
7/2 +
γ1 = 140.3 kev
2.1 magn
98.6 %
γ2 = 14.3 kev
2.4 magn
1.4 %
g 9/2 +

FIG. 11