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REMINISCENCES OF A PARTICLE PHYSICIST*

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AFTER-DINNER TALK at the GLUEBALL WORKSHOP
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Some people think that old scientists should only look back, not forward. I hope that the chairman of the organizing committee of this workshop, Suh Urk Chung, did not think so when he asked me to talk about my reminiscences as a particle physicist. The title was his choice. I might have called it "The good old days."

Let me recount some of my experiences as a student in the early thirties. During that time there were many great developments in nuclear physics and cosmic-ray physics, both experimental and theoretical. Some of these are now considered to be part of particle physics, which inherited many of the techniques, results, insights, and styles of research from these older fields.

One man's reminiscences may give you a little of the flavor of how research was done in those days. Remembering the "good old days" occasionally reminds us that then things were done in a simpler way, and maybe we can still recover some of these ways. This is, perhaps, the most useful aspect of reminiscences. Also, few of you will have read the original literature of that time, and if you are historically minded you might be interested in interconnections which were sometimes not reported or even recorded. It is difficult to reminisce without bringing out some of the later ramifications which followed from one's work while trying to resist the temptation to think that one deserves credit for later developments. You might call this temptation "inverted funnel vision": how one sees the influence of one's own work widening with time, as if one were looking through the narrow end of a funnel. In Cambridge people didn't worry too much about credit. There was a saying there, "You cannot both do a thing and get the credit for it."

In after-dinner talks people often unashamedly steal ideas from others, but I shall try to steal only from my earlier talks and refer to other people when I borrow from them. In an earlier after-dinner talk at BNL, when I was about 10% younger than now, I remarked, quoting Churchill: "I am a man of simple taste, the best is good enough for me."

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In 1930 I started as a student at the University in Berlin, at that time a foremost center for physics. In the front row at the physics colloquium you could find Planck, Einstein, Laue, Nernst, Schroedinger, Hahn, Meitner; sometimes other well-known physicists, like Bothe, Hertz and Meissner would attend. You see, the best was good enough for me.

Soon after the neutron was discovered Meitner gave a special colloquium about it. She was so excited that she talked of a neutron hitting a "brass nucleus", eliciting thunderous laughter. But, after all, we should not be too hard on her; the "brass nucleus" has seven "isotopes", less than e.g. tin, which has ten.

After the Nazis came to power, at the end of January 1933, the students who sympathized with them often rioted, stopping the lectures at the University. One such day, in March of 1933, I went to the library to read some of the new journals. There was a popular science journal which reported that a well-known American chemist, G.N. Lewis of Berkeley, had separated one cc of heavy water. I was struck by what seemed then an extraordinary feat of isotope separation and jotted down a few ideas of what one might do with heavy hydrogen. Among them was the thought that one might disintegrate its nucleus with γ-rays. You might say that the deuteron (as it was later called) plays a role in nuclear physics equivalent to that of the hydrogen atom in atomic physics. But, unlike atoms, nuclei do not have an infinite number of bound excited states. The lightest nucleus which has a bound excited state – and only one – is ^{7}Li. Thus, excitation of the lightest nuclei by γ-rays forces us to study transitions to the continuum, i.e., photo-disintegration.

At that time I wanted to become a theoretician. One day I visited Schroedinger at his apartment to discuss a possible thesis with him; however, either during the course of that conversation, or very soon thereafter, we each decided to leave Germany. My parents had already left by then and were urging me to leave too.

Being a man of simple taste I wrote to Bohr, Pauli and Rutherford, asking whether they would accept me as a student. Rutherford was the first to answer, maybe the only one; I didn't wait to hear from the others, but got a visa to England and left Berlin in May. When I arrived at the Cavendish Laboratory, I discovered that one also had to belong to a college or some equivalent to be accepted as a student at Cambridge University. I found out that the least costly way to study was to join something called "Fitzwilliam House", a substitute for a college. When I met Chadwick and told him of my intention, he made, in his taciturn way, a rather cryptic remark: "If I were you, I would join a College; they do things for you."

I asked a research student I had met, David Schoenberg, who since has become a well-known solid-state physicist, to point out some good colleges. He told me to walk to some of those nearby: Trinity, St. John's and Magdalene, in that order. These colleges

have had some illustrious physics students or fellows, like Newton, Maxwell, Rutherford, Dirac, Blackett and many others whose names you know well.

At Trinity the Senior Tutor told me that they were full up: it was already August and the new term started in October. At St. John's I was told they would let me know "in due course"; for me this was a new phrase which it took me a while to digest. At Magdalene the Senior Tutor said: "Ah, you are a refugee. I suppose we ought to have one." Then he added: "I suppose you have no money; we had better give you a hundred pounds." This was one half of what was needed per year to study in those days. My Cambridge friends now find it hard to believe that a Senior Tutor could make such a decision without first going to a committee. How much more red tape there is now! Here is a lesson for us today: put individual judgment ahead of rules.

In October I started at the Cavendish. There, at the colloquium, were Rutherford, Chadwick, Ellis, Fowler, Dirac, Mott, Cockcroft, Walton, Aston, Oliphant, Kapitza, Chandrasekhar, Bhabha and occasionally J.J. Thomson and Eddington. The spotlight was now clearly on Cambridge. At present you could not find as large a number of great scientists in one place.

Since I wanted to be a theoretician I was assigned by Rutherford to Ralph Fowler, the resident theoretician at the Cavendish, and incidentally Rutherford's son-in-law and the father of Peter Fowler of Bristol, who is probably known to most of you. Ralph Fowler was an ideal supervisor, because he let you work on your own. I worked on the role of spin in nuclear disintegrations, especially the reactions between the lithium isotopes and the hydrogen isotopes, which Cockcroft and Walton had just investigated. One conclusion I reached was that the spin of ^{6}Li, which from spectroscopic measurements of limited resolving power had seemed to have a value of 0 was really 1, as later confirmed by Fox and Rabi using atomic beams.

To write up my work I had to know the masses of the lithium nuclei, and when I happened to ask Mott he advised me to see Chadwick, who would know such things. So I went to see him, and after he enlightened me about the nuclear masses, I had the courage to bring up the possibility of the photo-disintegration of the "diplon" which I had thought about more than a year earlier. At that time the deuteron was called the diplon at the Cavendish, while others first called it deutron, without the second e. But Rutherford objected that this sounded too much like neutron when you had a cold, as you often had in Cambridge.

Chadwick listened, but at first he didn't seem too interested; then I emphasized that since the diplon and proton masses were well known, measuring the photo-disintegration with a γ-ray of known energy would yield the diplon binding energy from the photo-proton

energy, and thus the neutron mass. At this point he became very interested. There had been three competing determinations of the neutron mass: Curie and Joliot had reported a very heavy neutron mass, and Lawrence a very light mass, while Chadwick's mass was in between, less than the hydrogen mass. This seemed to support an idea Rutherford had been propagating for a dozen years, ever since he had predicted the neutron, that it should be considered as a very closely bound compound of a proton and an electron and thus weigh less than a proton plus an electron. At that time there were very many "Letters to the Editor" about the neutron mass because its value was so much in the air; this reminds me a bit of the many "Letters" nowadays about the top-quark mass.

About six weeks later Chadwick met me in the corridor and said, "Were you the one who suggested to me the photo-disintegration of the diplon?" I said, "yes". He said, "Well it works. It worked yesterday. Would you like to work with me?" This was very gentlemanly of him. So I went to ask Fowler. He said, "Fine, you go ahead and work with Chadwick." I surmise that Chadwick and Fowler had talked this switch over with Rutherford who was officially in charge of all research students at the Cavendish.

After a little work we found a neutron mass of 1.008, definitely heavier than the hydrogen atom. Our result has stood up: there are now six more decimals known and though the mass scale used now is a little different, the neutron mass still starts out with 1.008.

With the neutron now known to be definitely heavier than the hydrogen atom, the idea of the neutron being a proton with a closely bound electron had to be discarded, and the neutron was from then on had to be considered as an elementary particle of its own. One day I realized with a tremendous shock that this elementary particle, being heavier than the hydrogen atom, ought to decay. That an elementary particle could decay seemed to me extraordinary at the time, and since I now knew the expected beta end-point from the mass difference n - H I worked out the expected lifetime of the neutron using the so-called Sargent relation (named after a Canadian physicist who had plotted the lifetimes of beta emitters against their energy). Since Sargent's relation was deduced for complex nuclei, I realized that whatever lifetime I got would probably be longer than the real one. Well, I deduced thirty minutes for the half-life which has turned out to be about three times as long as the actual half-life.

For a long time it has seemed puzzling that the neutron should be heavier than the proton, because the proton has the extra Coulomb self interaction which should make it heavier than the neutron. The mass of the neutron and the magnetic moments of the nucleons were early hints of some underlying structure but this was not understood until much later. The puzzle of the neutron mass is still with us, though it has been swept

under the rug by moving it to a deeper level. One now says: the neutron is heavier than the proton because the d quark is heavier than the u quark, don't you see! So now I am waiting for an explanation of the d-u mass difference, something present-day theories are not yet able to tackle.

A few years later the lifetime of an elementary particle was established for the first time: that of the muon. Neutron decay was not observed until after World War II when the intense neutron beams produced by reactors became available. Today it seems quite trivial that elementary particles should decay. I'm trying to give you the spirit that at one time it didn't seem trivial, at least not to me.

We measured the photo-disintegration cross-section of the deuteron, and during evenings I attempted to calculate it from "first principles" but did not get far in a week or so. Chadwick must have concluded that such a calculation is very difficult, because I later learned that when Bethe and Peierls, who had already made their reputation in atomic physics, visited him, he dared them by saying "I bet you cannot calculate the photo-disintegration cross-section of the diplon." However, they did it promptly – a good start for their careers in nuclear physics.

We had an interesting early encounter with an apparent absence of time-reversal invariance. When we measured the photo-disintegration cross-section of deuterium, $d + \gamma \rightarrow p + n$, we noticed a big discrepancy: the inverse cross section, $p + n \rightarrow d + \gamma$, had been previously measured by a student of Chadwick's, D.E. Lea, who observed γ-rays when fast neutrons interacted with paraffin. The cross section he reported was about a thousand times larger than should be expected by a time-reversal argument from the photo-disintegration cross section we had just measured – nothing puny like the 2 parts per 1000 for the K_L! Of course, we had a strong belief that time-reversal should hold, and I therefore speculated that the neutron capture which Lea had observed, was not that of fast neutrons, but that the neutrons were first slowed down in his paraffin target and then captured as slow neutrons. Unfortunately, speculation was very much frowned upon at the Cavendish because Rutherford disliked it, and Chadwick, who had first accepted my speculation in an early draft, later changed his mind and convinced me to leave out this speculation in the final form of our paper. Therefore, when we wrote our note to "Nature" in July 1934, we only made the typical Cavendish type of understatement: "A satisfactory explanation is not easy to find and further experiments seem desirable." By late October Fermi and his collaborators had discovered slow neutrons. Rutherford, who presumably had heard of my idea from Chadwick during their daily meetings, and perhaps had talked him out of this speculation, rushed to my lab when he learned the news from Rome and said: "He'll give you credit, won't he?" I said, "Why should he? He didn't know about

it." I learned much later from a remark published by Chandrasekhar that Fermi had told him that the only discovery where his subconscious had played a role was the discovery of slow neutrons. Perhaps his subconscious remembered the difficulty we had encountered with time-reversal. As you know, the discovery of slow neutrons ultimately led to reactors and a new international "ball(istic) game." The lesson I learned was that history is not time-reversible!

After slow neutrons came along, it was easy to conjecture that they should be able to disintegrate ^{6}Li into lighter particles, in contrast to the (n, γ) reactions which Fermi and his collaborators had studied in heavier nuclei. If you think of the reaction $^6\mathrm{Li} + \mathrm{n} \rightarrow {}^4\mathrm{He} + {}^3\mathrm{H}$, and compare it with the reaction Cockcroft and Walton had observed, $^6\mathrm{Li} + \mathrm{p} \rightarrow {}^4\mathrm{He} + {}^3\mathrm{He}$, you see immediately that there ought to be more energy released in the slow neutron reaction than in the proton reaction, since the neutron is heavier than the proton and the ^{3}H and ^{3}He masses are very close. Unlike proton reactions, the neutron reaction should work at zero neutron energy, because of the absence of Coulomb repulsion. So we tried it and it worked right away, and with a good cross section which has turned out to be nearly a thousand barns. This reaction is used to make ^{3}H and is now much in the news. As you know, ^{3}H is credited by some for its help in keeping world peace by its role in the nuclear deterrent. We joked when we found the slow neutron reaction that we should patent it, but patents were taboo in Rutherford's Cavendish. ^{3}H decays with a 12 year half-life and the decay product, ^{3}He, is, as you know, very valuable for low temperature research; I like to call it a "cold war surplus" (you can fill in the hyphens where you like).

Once we found the lithium reaction we thought that there might be others which we could not predict a priori and systematically went through all the elements we could get hold of. We next found a disintegration in boron, which we ascribed to ^{10}B. It has a very large cross section, and the question arose whether it yields two or three particles. From the masses measured by Aston it seemed that the low energy release we observed was best compatible with emission of three particles. Fermi and his collaborators soon also found the boron disintegration by slow neutrons. Without considering the energy balance they guessed the simpler reaction: two particles. How could we decide this question? At that time a student of Chadwick's, H.J. Taylor, had just finished a thesis about nuclear emulsions; he would shoot alphas into an emulsion, not a very exciting game, and the last sentence in the draft of his thesis was something like this: Nuclear emulsions are useless. One day I happened to have lunch with him, and it occurred to me that if we would put boron inside an emulsion and bombard it with neutrons we would see whether two or three particles are emitted. In those days you didn't go with an idea to a committee; so after lunch we soaked an emulsion in a boron solution, dried it, and bombarded it with slow

neutrons for a few weeks. When we developed it we found about 50,000 single tracks/cm^2, the first time that nuclear events were seen to be produced in a nuclear emulsion by outside radiation. Fermi had guessed correctly, two particles going in opposite directions was the answer. Later we observed three particles with fast neutrons. Pictures of these reactions are reproduced in Rutherford's last book, "The Newer Alchemy", published in the year of his death (Cambridge University Press, 1937, p. 41, plate VIII). Recently three particles were also observed with slow neutrons, but with a tiny cross section because of the low energy release.

Our wrong guess had led to two interesting developments: The nearly moribund nuclear emulsion method came to life again, and we now had definite evidence that something was seriously wrong with Aston's masses, quite a heresy at the Cavendish. There had been one previous hint that something was also wrong with the ^{9}Be mass. I conjectured that an error had crept into Aston's measurement of the ^{4}He/^{12}C ratio which entered crucially in many of the masses of the lighter nuclei. Bethe took it from there and worked out the masses of the light nuclei based on nuclear reactions alone.

I soon built a BF_3 counter, still a useful detector for slow neutrons; nowadays it is sometimes replaced by a ^{3}He counter because of the large cross section of the ^{3}He(n, p) ^{3}H reaction.

The next element which we found to be disintegrated by slow neutrons was nitrogen. We had to decide whether a proton or an alpha particle was emitted, and for that Burcham and I used a nuclear emulsion in which one could distinguish protons and alphas. The reaction turned out to be: $^{14}N + n \rightarrow {}^{14}C + p$. From the proton energy we could deduce the mass of ^{14}C and I estimated from this that its half-life should be ~3 months. I tried to measure its radioactivity by chemically separating ^{14}C from an ammonium nitrate solution bombarded with slow neutrons, but I found no measurable activity. ^{14}C has turned out to have a very long half-life, now known to be about 5,730 years; this has been explained as due to an unforeseen near cancellation of two matrix elements. Secondary neutrons from cosmic rays produce ^{14}C from nitrogen in the atmosphere and this has proved useful for dating objects of biological origin.

Though our work with nuclear emulsions gave this field a shot in the arm, the method did not really take off until the end of World War II, when Powell, Occhialini, and their collaborators at Bristol started to look for cosmic-ray interactions with boron-impregnated emulsions, which Ilford was by then producing routinely. As Powell once explained to me, their idea was to have a specific nucleus with which they could study specific interactions. In the course of their work they discovered the pion and found π-μ decay. They then developed better and better emulsions and made many great discoveries, and particle

physics really took off.

I hope I have given you a feeling for the excitement of those days, when scientific life was simpler, when the hot pursuit of one's ideas was taken for granted, when these ideas could be quickly translated into action, and progress was often measured in days or weeks.

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