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Glimpses at the history of the nuclear structure theory

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During the last weeks, I have often thought of my teachers, especially of the one man who had great influence on my attempts to gain some understanding of nuclei, Niels Bohr. I think it is also appropriate at this occasion to consider first the background from which our concepts of nuclear structure emerged.

I can devote only a few sentences to the time preceding Chadwick's discovery of the neutron (1932). At that time our information regarding the nucleus was very sparse. All we had was a chart of known stable isotopes with nuclear masses which were not very accurate, a few nuclear spins, an estimate of the nuclear radius, about $1.4 \cdot 10^{-3} A^{1/3}$, the phenomena of natural radioactivity, and a few known nuclear reactions. Ideas on nuclear structure were still dominated by Prout's hypothesis of 1815, that electrons and protons, the only known elementary particles, are bound together in a nucleus in such a way that A protons and A-Z electrons form a nucleus of charge Z. But from the point of view of quantum mechanics a great puzzle was inseparably inherent in this picture. Consider the deuteron as the simplest example. According to the model, the deuteron contains two protons and one electron, just like the ion of the hydrogen molecule. Yet in the deuteron the linear dimensions are 10^{-5} times smaller than in the hydrogen molecule. The uncertainty principle requires very strong forces to confine electrons to such a small volume. These non-Coulomb forces should then show up just as well in the hydrogen spectrum and change the Balmer formula; in particular, they should give rise to a much larger splitting than that discovered later by Lamb. I cannot discuss other similarily grave inconsistencies of the model in this limited time.

In view of these conflicts many physicists, including Niels Bohr, were inclined to expect far-reaching changes in our basic physical concepts, even in quantum mechanics*. At that time one was tempted to consider alpha particles

* Some physicists thought that it might even become necessary to give up the conservation laws in their current form, especially in connection with the problem of beta decay.

as basic building blocks of nuclei. However, from those days a warning from Schroedinger still persists in my mind. During the late twenties he chided the participants in a Berlin seminar for their lack of imagination. In his impulsive manner he said: «Just because you see alpha particles coming out of the nucleus, you should not necessarily conclude that inside they exist as such.» And he gave an illustrative example from every-day life to show how such reasoning can lead to fallacious conclusions.

It is remarkable that very little information about nuclear structure could be gained from the study of alpha decay. Max von Laue has pointed this out very clearly in a letter to Gamow in 1926; he congratulated Gamow on his explanation of the Geiger-Nuttal law* in terms of the tunnelling effect and then went on: «however, if the alpha decay is dominated by quantum phenomena in the region outside the nucleus, we obviously cannot learn much about nuclear structure from it.». Gamow says that at first he was quite perplexed while reading these lines, but thinking it over he had to agree with von Laue. The situation that very little insight into nuclear structure could be gained from this oldest nuclear phenomenon persisted for a long time. Only about 6 years ago some progress was made when Mang applied the shell model to the problem of alpha decay. It seems to me that Mang's results justify Schroedinger's scepticism; the alpha particles obviously only form while emerging from the nucleus.

The discovery of free neutrons changed the situation entirely. Now it became possible to separate the grave difficulties of «the localization of electrons in the nucleus», to which I shall return later, from the specific problem of nuclear structure. Thus, in spite of Schroedinger's warning (this time, of course, regarding the neutrons), one could consider the hypothesis that protons and neutrons are the fundamental units within the nucleus. (Rutherford had already suggested this in conversations before Chadwick's discovery, and Harkins had published the same proposal). Specific nucleon-nucleon forces acting between them must be responsible for the nuclear binding. Heisenberg was the first to explore the consequences of this hypothesis, and to arrive at important concepts and results in a series of pioneering papers in the *Zeitschrift für Physik* (1932–1933).

These ideas can be separated into two stages. First, the saturation phenome-

* That is, the fact that the lifetime of an α-emitter changes by 25 powers of ten when the alpha-particle energy increases only by a factor of two.

non can be accepted as an empirical fact, *i. e.* the approximate proportionality of nuclear binding energy to the particle number A, as well as the proportionality of the nuclear volume to A, with a radius already mentioned.The numerical value of r was a crude estimate at that time; now we know from the Stanford experiments that it is about 20% smaller. These facts as well as the results of scattering experiments led to the conclusion that nuclear forces must have a short range. In spite of this shortness of range, in one of his papers Heisenberg considered the nucleus as a superposition of two Fermi gases (a neutron gas and a proton gas) which freely permeate each other and which by an averaged potential are confined to the given volume. The basic fact that stable nuclei have about the same number of neutrons and protons, $Z \approx A/2$, is explained on this basis as a consequence of Pauli's principle. In addition, one obtains the right order of magnitude for the curvature of the parabola defined by taking an A=const. cross-section through the surface of binding energies of stable nuclei; the opening of the parabola was somewhat too large, with the new nuclear radius obtained by Hofstadter the agreement is even better. The decrease of the ratio $Z:A$ with increasing mass number is a natural consequence of the interplay between the accumulating Coulomb interaction and the consequences of Pauli's principle.

Thus the basic idea of the shell model was expressed for the first time, *i. e.,* the idea of free motion of individual nucleons in an averaged potential. Every further development was an inevitable extension of these ideas to a system with a finite number of particles*. The Leipzig school as well as Wigner and his co-workers devoted great effort to the study of light nuclei, mainly on the basis of the shell model. The particular stability of the nuclei ${}^{4}_{2}He_2$, ${}^{16}_{8}O_8$, and ${}^{40}_{20}Ca_{20}$, was not the only fact explained in this way. For example, Wigner and his co-workers came to a quantitative conclusion that the then unknown nuclides ${}^{36}_{16}S_{20}$ and ${}^{48}_{20}Ca_{28}$, should be stable; later these nuclides were in fact observed in mass spectrometers as natural isotopes with very small abundance.

* However, Heisenberg's interest extended far beyond this stage to the following question: By which properties of the forces can the nuclear saturation be explained? To account for this phenomenon, he introduced the concept of « exchange forces » which he formulated in terms of « isospin » formalism, first invented for this purpose. Thus he created a conceptual apparatus which is still used in discussing the most direct studies of nucleon-nucleon interaction, the scattering experiments. The quantitative results concerning exchange mixtures which would guarantee saturation are by now outdated. It is unfortunate that at that time one did not systematically pursue one other possible explanation of saturation: a property of the forces which is today usually called « hard core » or « most hardcore ». Heisenberg also discussed this possibility in one of his papers.

Although this was somewhat a matter of luck in view of the insufficient knowledge of the forces, it was nevertheless one of the first predictions of nuclear theory to be verified experimentally. In 1937 Hund and Wigner, independently of each other, developed the concept of supermultiplets that played such an important role in classifying nuclides and in the systematics of beta decay. This concept was based on the assumption that nucleon-nucleon forces were essentially charge- and spin-independent. In the article by Bethe and Bacher in *Reviews of Modern Physics* (1936) which was soon called « Bethe's bible », very convincing arguments had been presented to show that, in fact, nuclear forces should not show much spin- and isospin-dependence; in particular the spin-orbit coupling should be very weak.

In the years immediately following the discovery of neutrons, a vigorous development of experimental nuclear physics began. This was partially due to the possibility of performing experiments with neutrons; partially to the completion of the first accelerators and to great improvements in measuring and counting techniques. For me these were the years of my first visits to Copenhagen and meeting Niels Bohr; in Copenhagen I was privileged to witness many attempts at a theoretical interpretation of the rapidly accumulating experimental data.

Two new phenomena were particularly important to the development of our concepts of nuclear structure: relatively high effective cross-sections for nucleon-nucleon scattering, and sharp, closely spaced resonances discovered by Fermi, Amaldi, and co-workers in slow-neutron scattering and capture. The latter phenomenon could not be explained at all in terms of the picture in which the neutron moves in an averaged potential. Thus Niels Bohr's concept of the «compound nucleus» originated. In his model, the state of the nucleus is characterized by an intimate coupling of all nucleons with each other; this description does not permit us to speak of the motion of a single nucleon independently of the simultaneous motion of all the others. However, this intuitive, semiclassical picture of Niels Bohr had to be brought into agreement with the postulates of quantum mechanics. To this day the golden bridge has been the Breit-Wigner formula. It originated independently outside Copenhagen, but it could soon be seen on every blackboard of Niels Bohr's institute. Naturally it received appropriate space in the above-mentioned «Bethe bible». Probably every theoretician has pondered long and often about its interpretation and about its proof; and it still occupies many minds.

One was inclined to describe even the ground state of a nucleus in terms of

Bohr's picture. A concept of nuclear matter, packed to saturation density and having a binding energy proportional to its volume became generally accepted. For finite nuclei a surface tension resulting from a surface energy proportional to the surface was to be added. The « Bethe bible » also contains an excellent discussion of the basis for these assumptions. The greatest success of this model was Bohr and Wheeler's theory of nuclear fission (1939), which contains almost everything that is understood to date (1963) about this phenomenon.

Schroedinger's remark, that one should not necessarily assume that the particles, observed as free particles emerging from the nucleus during nuclear transformations, must exist in the same form inside the nucleus, was emphasized in Fermi's paper on beta decay (1933-1934). In these papers the abovementioned dilemma, which arises from the concept of « electrons inside the nucleus or inside the neutron », was literally dissolved into nothing. Fermi drew radical consequences from the idea that the proton and the neutron are two quantum states of one single fundamental particle, the nucleon. Between these two states quantum transitions can take place. Such a transition is accompanied by the *creation* of an electron and a neutrino; (Fermi used Heisenberg's concept of isospin and its formalism in the theory of beta transformation). Today's young physicist, who already as a student juggles creation and annihilation operators on the blackboard, can hardly fathom the importance of the conceptual breakthrough contained in Fermi's theory. As an illustration, let me quote from a historical letter sent by Pauli to several friends and colleagues (December 1930) in which he proposed his neutrino hypothesis for the first time.

« … bin ich auf einen verzweifelten Ausweg verfallen, … nämlich die Möglichkeit, es könnten elektrisch neutrale Teilchen, die ich Neutrinos* nennen will, in den Kernen existieren … Das kontinuierliche β-Spektrum wäre dann verständlich unter der Annahme, dass beim β-Zerfall mit dem Elektron jeweils noch ein Neutrino emittiert wird … »

« I came to a desperate conclusion … that inside the nucleus there may exist electrically neutral particles which I shall call neutrinos*. The continuous beta spectrum becomes understandable if one assumes that, during beta decay, the emission of an electron is accompanied by the emission of a neutrino… »

* In his letter, written long before Chadwick's discovery, the word « neutron » appears instead of « neutrino »; the latter was adopted by Pauli later, following a suggestion by Fermi.

I emphasize the words *exist inside the nucleus* and *emission.* Pauli certainly did not choose these words simply to make his ideas more palatable to his experimental colleagues, but because the words characterize the physical concept of those days. It is all the more remarkable in view of the fact that the concept and mathematical technique of particle creation used by Fermi had been available long before in the so-called second quantization of Jordan, Klein, and Wigner. However, even in 1932, in his Handbuch article Pauli himself regarded that rather as a mathematical trick, it was Fermi's work which finally convinced him that there was tangible physics in it.

Yukawa's theory also became known in the middle of that decade. He emphasized that the forces between nucleons are transmitted by a field, which must show retardation effects, and quanta associated with the retardation effects, the mesons. The latter are perhaps of secondary importance in nuclear structure problems, since it was established by Heisenberg's investigations that in the nucleus the nucleons move so slowly that one may hope to understand the essential features of nuclear structure by using non-relativistic quantum mechanics. However, the strong coupling of the Yukawa field to its source is extremely important; its strength, $g^2/\hbar c$ is of the order of magnitude of ten (in contrast with the Sommerfeld constant $e^2/\hbar c = 1/137$ in electrodynamics). This led Niels Bohr to an idea on nuclear matter, which, to my knowledge, he never wrote down; but from conversations it has remained ineradicably engraved in my memory: since the field is strongly coupled to its sources, the hitherto existing picture of the « compound nucleus » may still be much too naive. Perhaps, the only sensible concept is to consider the whole nucleus as an « Urfeld » which is highly nonlinear because of such strong couplings. When this field is quantized, it must give (in addition to other conserved quantities, like angular momentum) integral charges Z, and energies (i.e. masses) that form a spectrum with values close to the integral numbers A, on which the « exaction energy » bands are superimposed. The assumption that inside the nucleus there exist Z protons and (A- Z) neutrons such as we encounter them as free particles in appropriate experiments would then hardly make any sense.

Schroedinger's scepticism (mentioned at the beginning) would thus be formulated in its extreme. Nevertheless, Niels Bohr had thus hinted at a picture of the nucleus which closely resembles current concepts in high- energy physics on elementary particles and « resonances » (e.g., such as the hyperons or the ϱ-, η-, etc. mesons). Certainly, one should not entirely forget such a point of view in nuclear physics either, although it has meanwhile been shown*, that it is legitimate to speak of the existence of individual nucleons inside the nucleus in a useful approximation.

The picture of the nucleus just described is in accord with the fact that, just glancing at the table of stable isotopes, we can see that the nuclear properties are continuous functions of A and Z. However, there were indications of discontinuities and windings in the valley of the energy surface. I have already pointed out the exceptional cases of nuclei with Z and N=2, 8, 20. It also seemed strange that the alpha energy does not increase uniformly as one goes further away from the alpha-stable nuclei in the mass valley; instead it is largest right at the polonium isotopes. This indicates that a special discontinuity occurs for Z=82. In the diagram in which alpha energies are plotted against Z and N, we also see curves with steep slopes from N=128 to N=126; Gamow called this feature the « Heisen-Berg ». The work of Seaborg and collaborators made the profile of these peaks even more striking. Elsasser, Guggenheim, Ivanenko, and others had attempted to explain these and other phenomena in terms of the shell model; however, it seemed impossible to accommodate the groups of numbers Z and N= 2, 8, 20, on the one hand and Z= 82, N= 126 on the other, under the same roof. Yet, mainly because of the success of Bohr's compound-nucleus model, there was a general tendency to consider these phenomena as curiosities of little significance to the fundamental questions of nuclear structure.

The war years and also the first few years thereafter brought the physicists in Germany into a stifling isolation, but at the same time they gave us some leisure to pursue questions off the beaten trails. At that time I had many discussions with Haxel in Berlin, later Göttingen, and with Suess in Hamburg on the empirical facts which single out the above-mentioned numbers. To Suess they became more and more significant, primarily in his cosmo-chemical studies: he found that in the interval between the numbers already mentioned, the numbers Z and N= 50 and N= 82 were also clearly prominent**. Haxel, at first quite independently, encountered the same numbers in the study of other nuclear data.

Although my two colleagues tried hard to convince me that these numbers might be a key to the understanding of nuclear structure, at first I did not know what to make of it. I thought the name « magic number », whose origin was

* In particular, through the work of Brueckner and recent literature inspired by it.

** V. M. Goldschmidt also came to the same conclusion; Suess and I had the privilege of discussing it with him in Oslo in 1942 and 1943.

unknown to me*, to be very appropriate. Then, a few years after the war, I had the privilege of returning to Copenhagen for the first time. There in a recent issue of the *Physical Review,* I found a paper by Maria Goeppert-Mayer, « On closed shells in nuclei », where she too had collected the empirical evidence pointing out the significance of the magic numbers. That gave me courage to talk about her work, along with our results, in a theoretical seminar. I shall never forget that afternoon. Niels Bohr listened very attentively and threw in questions which became more and more lively. Once he remarked: « But that is not in Mrs. Mayer's paper! »; evidently Bohr had already carefully read and pondered about her work. The seminar turned into a long and lively discussion. I was very much impressed by the intensity with which Niels Bohr received, weighed, and compared these empirical facts, facts that did not at all fit into his own picture of nuclear structure. From that hour on I began to consider seriously the possibility of a « demagification » of the « magic numbers ».

At first I tried to remain as much as possible within the old framework. To begin with, I considered only the spin of the whole nucleus, since there appeared to exist a simple correlation between the magic nucleon numbers and the sequence of nuclear spins and their multiplicities. I first thought of the single-particle model with strong spin-orbit coupling** during an exciting discussion with Haxel and Suess, in which we tried to include all available empirical facts in this scheme. As we did this it turned out that, because of the spin-orbit coupling, the proton- and the neutron-number 28 should also be something like a magic number. I remember our being elated when we found some hints in the still meagre data that was available at that time. Nevertheless, I did not feel very happy about the whole picture, and I was not really surprised when a serious journal refused to publish our first letter, stating « it is not really physics but rather playing with numbers ». Only when I thought of the lively interest in the magic numbers which Niels Bohr had shown did I dare send the same letter to Weisskopf who forwarded it to the *Physical Review.* Yet it was not until later, after I had presented our ideas in a Copenhagen seminar and been able to discuss them with Niels Bohr, that I finally gained some confidence. One of Bohrs first comments seemed remarkable to me: « Now I understand why nuclei do not show rotational bands in their spectra ». With the accuracy of measurement available at the time, one had looked for such

* I learned only yesterday that the name was coined by Wigner.

* * Fortunately, I was not too well versed in « Bethe's bible » and I did not remember the old arguments against a strong spin-orbit coupling too well.

spectra in lighter nuclei, which according to the liquid drop or a similar model should have relatively small moments of inertia and therefore widely separated rotational levels. As we know today, these light nuclei, like many others, in fact show no rotational bands; Bohr's argument was, of course, that in a picture in which single particles move independently in an average spherically symmetric potential, there was no place for a superimposed rotation of the nucleus as a whole, just as in the system of electrons in the atomic shells.

Even though the shell model finally proved to be more than just a convenient language in which the experimentalists could compare their notes, and although during the following years it led us to some understanding of a few fundamental features of nuclear structure, I still had to agree with Robert Oppenheimer when he told me : « Maria and you are trying to explain magic by miracles. » In a lecture at Oak Ridge, Wigner recently said something quite similar, of course more cryptic, in his careful way of choosing his words.

From the beginning it was clear to me as well as to Mrs. Goeppert-Mayer that the shell model could at best approximately describe the ground state and the low-lying excited states of nuclei. While the consequences of the Pauli principle for these states could possibly guarantee the self-consistency of our model, the Pauli principles becomes less and less stringent as the excitation energies become higher, and the nucleon-nucleon correlations arising from nuclear forces become increasingly important. In an exact description such correlations are, of course, also present in the ground state.

Therefore, during my next visit to Copenhagen I had a certain satisfaction when, questioned about news on the shell model, I could instead talk about the ideas which then occupied my namesake Peter Jensen and myself as well as Steinwedel and Danos. Following a suggestion by Goldhaber and Teller, we tried to provide a semi-classical explanation for the recently discovered large dipole absorption in the nuclear photoeffect at 15 to 20 MeV; that is, we described it as an excited state of nuclear matter in which all nucleons are in a state of motion with strict phase relations existing between all of them. In this way the frequency of the absorption maximum, as well as its dependence on the nuclear mass number, could be related in a satisfactory way to the symmetry energy and to the nuclear radius. The width of the « giant resonance » provided a measure of the rate at which such phase correlations disappear. Niels Bohr understood immediately why the study of this particular type of « collective motion » (in the present-day jargon of specialists) was of such great interest to me. Even though the importance of phase correlations be kept down in the ground state by Pauli's principle, we wanted to determine at which excitation energies the correlations enforced by the nucleon-nucleon interaction become dominant over the effect of the averaged forces.

In the following years much work was devoted to the study of such correlations. A most remarkable feature of current nuclear physics was brought to light by the work of Kurath, of the former Harwell group (Flowers, Elliot, and others), and of the young Copenhagen School (Aage Ben Bohr, Mottelson, Nilsson and others). I mean the fact that, even though the groups started from points of view which, whilst complementing also limit each other, their quantitative results seem quite soon to meet, and to overlap, in the next steps of approximation.

The first group started from the shell-model point of view with a spherically symmetric potential, and handled the problem of correlations by calculating the configuration mixing which is caused by the forces acting individually in each pair of nucleons. Thus it was shown that, even with only a few nucleons outside a closed shell, one obtains level sequences very similar to rotational spectra. In this way, although it is difficult to perform a quantitative calculation, one can understand how in the case of nuclei with many nucleons outside closed shells (for example, the rare-earth region and the nuclei beyond) many close-lying and very different particle states can contribute to configuration mixing, creating such correlations that the ground state becomes a strongly deformed nucleus. The Copenhagen group started by treating mainly the latter group of nuclei; they included correlations *ab initio* by assuming in their calculation a non-spherically symmetric, collective potential in which single-particle states are calculated. Then the coupling of the single-particle motion to the collective motion of the remaining deformed nucleus determines the spectra. (The ingenuity of the Copenhagen concept lies in the clever and successful treatment of the interplay of « collective » and ((individual)) features of nuclear motion; this provides the model with adequate flexibility to account for all new empirical facts.) It was shown that this easily calculable « unified model », as Aage Bohr likes to call it, could also explain the spectra of nuclei with only a few nucleons outside a closed shell. In this context one should also mention the new work of de-Shalit, in which the first excited states of nuclei with odd A are explained as a combination of « core excitations of the nucleus A-1 » and the single-particle motion of the odd nucleon.

When one considers all these questions as a whole - the problems of nuclear structure and nuclear forces, as well as the problems of elementary particles -

a verse by Rilke still seems to fairly describe the situation. In the early days of quantum mechanics my late teacher, Wilhelm Lenz, brought this verse to my attention. In it Rilke speaks of his feelings at the turn of the century in terms of a large book in which a page is slowly being turned over, he concludes:

« Man fühlt den Glanz von einer neuen Seite,
Auf der noch alles werden kann.
Die stillen Kräfte prüfen ihre Breite
Und sehn einander dunkel an. »

[« The lustre of the new-turned page one senses,
Where everything may yet unfold,
The silent forces measure their expanses;
Each other dimly they behold. »]