held them to be. On the other hand, it is clear that during Preclassic times Tikal was not a hothouse, self-propagating sport. From probably as early as 700 B.C. it depended on trade with distant regions for basic raw materials, and its neighbors may have depended on a variety of Tikal products.

What traveled these trade routes? Comparative stylistic studies may eventually show conclusively that certain features of early Tikal culture are of highland derivation. However, in any such study, quality, quantity, and time must be very carefully considered before donor and recipient, or innovator and emulator, can be specified with assurance and the direction of diffusion can be pinned down.

Much remains to be learned of Preclassic Tikal. Despite the magnitude of the North Acropolis excavation, the work was limited by a shortage of time and funds. However, without the knowledge gained from this work, Tikal could not figure in the profound searchings for the beginnings and fulfillments of Mesoamerican and particularly Maya brilliance. The most disturbing aspect of Tikal is the quantity of architectural masses as yet untouched. Excavation of these might greatly augment our knowledge of the Preclassic Maya of the southern lowlands. Archeology depends on reliably amassed information. Learning the facts of Preclassic times is costly when settlements, such as Tikal, which are critical to such a study have been constantly built and rebuilt. It is clear that solid answers to the questions of cultural beginnings which plague Mesoamericanists cannot come quickly. To think otherwise is to discount the extraordinary limitations of archeology.

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The History of the Theory of Structure of the Atomic Nucleus

J. Hans D. Jensen

I have had occasion during recent weeks to think of my teachers. One man who had a decisive influence on my early attempts to gain some understanding of nuclei stands out among them: he was Niels Bohr. Thus it seems appropriate today to look back and to examine the background from which our concepts of nuclear structure emerged.

The author is director of the Institute of Theoretical Physics at the University of Heidelberg. This article is the lecture he delivered in Stockholm, Sweden, 12 December 1963, when he received the 1963 Nobel Prize in physics. It is published here with the permission of the Nobel Foundation; it will be included in the complete volumes of Nobel lectures in English, published by the Elsevier Publishing Company, Amsterdam and New York.

I shall 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 stable isotopes with nuclear masses which were not very accurate, a few nuclear spins, an estimate of nuclear radius of about $1.4 \times 10^{-13} A^{\frac{1}{3}}$ centimeter, the phenomenon of natural radioactivity, and a few nuclear reactions. Ideas on nuclear structure were still dominated by Prout's hypothesis of 1815; this was that electrons and protons, the only elementary particles known at the time, were bound together in a nucleus in such a way that A protons and $A-Z$ electrons formed a nucleus of charge Z. But from the point of view of quantum mechanics this picture led to a great puzzle. Consider the deuteron as the simplest example today. According to this picture, the deuteron contains two protons and one electron, just like the ion of the hydrogen molecule. Yet in the deuteron these particles occupy 10^{-5} times less space in linear dimensions than in the hydrogen molecule. According to the uncertainty principle, very strong forces must be present in order to confine electrons to such a small space. These new forces should then show up 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. Because of lack of time I cannot go into other difficulties arising from this picture.

In view of these conflicts many physicists including Niels Bohr were inclined to expect far-reaching changes in our basic physical concepts as well as in quantum mechanics (*1*).

At the same time there was an attempt to postulate that alpha particles form the basic building blocks of nuclei. One warning by Schroedinger still persists in my mind from those days. During the late '20's he accused the participants in a Berlin seminar of lack of imagination. In his temperamental manner he said, "Just because you see alpha particles coming out of the nucleus, you should not necessarily conclude that they exist inside it in the same form!" And he used a highly illustrative example to demonstrate how this reasoning can lead to a completely erroneous conclusion.

It is very remarkable how little information could be obtained at that time about the nucleus from the study of alpha decay. Max von Laue described this very clearly in a letter to Gamow in 1926; he congratulated Gamow on his explanation of the Geiger-Nuttal formula (*2*) in terms of the tunneling effect and then went on: "However, if then the phenomenon of alpha decay occurs predominantly in the region outside the nucleus because of quantum mechanics, it is obvious that we cannot learn a great deal about nuclear structure from it." Gamow tells that at first he was quite perplexed after reading these lines, but after he had thought about it he decided that von Laue was right. The situation that very little insight into the structure of the nucleus could be gained from this oldest nuclear phenomenon presisted for a long time. Only about 6 years ago was some progress made when Mang applied the shell model to the problem of alpha decay. It seems to me that Mang's results completely confirm Schroedinger's skeptism; obviously the alpha particles really first appear during emergence from the nucleus.

The discovery of free neutrons completely changed the situation. Now it became possible to divorce 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 propose the hypothesis that protons and neutrons are the fundamental building blocks of the nucleus. (Rutherford had already proposed this in a conversation before Chadwick's discovery, and Harkins had published the suggestion.) The specific nuclear forces which act between them must be responsible for binding the nucleus. Heisenberg was the first to point out the consequences of this hypothesis and to arrive at important formulations and results in his series of noteworthy papers in *Zeitschrift fuer Physik* (1932–33).

These ideas can be separated into two stages. First, the saturation phenomenon is accepted as an empirical fact, that is, the approximate proportionality of nuclear binding energy (showing up as mass defect) to the particle number A, as well as the proportionality of the nuclear volume to A—roughly as $V = A \cdot r^3(4\pi/3)$, where $r = 1.4 \times 10^{-13}$ centimeter. The numerical value of r was simply a gross estimate at that time; now we know from Stanford experiments that it is about 20 percent smaller. These facts as well as the results of scattering experiments led us to the conclusion that nuclear forces must have a short range. In spite of this shortness of range, Heisenberg in one of his papers considered the nucleus as a superposition of two Fermi gases (a neutron gas and a proton gas) which freely permeate each other and which are confined to the volume given above by an averaged potential. The basic fact that the 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 gets the right order of magnitude for the curvature of the parabola obtained by taking an $A =$ const. cross section through the surface of binding energies of stable nuclei. The opening of the parabola is too large by a factor of about two; with the new nuclear radius obtained by Hofstadter the agreement is even better. Similarly, the decrease in the ratio $Z:A$ with increasing mass number results as 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, that is, the idea of free motion of individual nucleons in an average potential. Every further development was an almost necessary extension of these ideas to a system with a finite number of particles (*3*). At the same time the Leipzig school as well as Wigner and his co-workers devoted themselves to the study of light nuclei, mainly on the basis of the shell model. The particular stability of the nuclei, ${}_2He_2{}^4$, ${}_8O_8{}^{16}$, and ${}_{20}Ca_{20}{}^{40}$, was not the only thing explained in this way. For example, Wigner and his co-workers came to a quantitative conclusion that the unknown nuclei, ${}_{16}S_{20}{}^{36}$ and ${}_{20}Ca_{28}{}^{48}$, should be even more stable; later these were in fact observed in mass spectrometers as natural isotopes with very small abundance. While this was somewhat a matter of luck in view of insufficient knowledge of the forces, it was nevertheless one of the first predictions of nuclear theory to be verified experimentally. In addition, around the same time (1937) Hund and Wigner, independently of each other, developed the concept of supermultiplets that played such an important part in classifying nuclides and in the systematics of beta decay. This concept was based on the specific charge and spin independence of nuclear forces. In the notable work of Bethe and Bacher in *Reviews of Modern Physics* (1936), which soon became known as "Bethe's bible," very convincing arguments were presented to show that nuclear forces in fact show a very weak spin dependence; in particular the spin-orbit coupling should be very weak.

In the years immediately following the discovery of neutrons 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 encounters with both Copenhagen and Niels Bohr; in Copenhagen I was privileged to witness attempts at theoretical interpretation of the rapidly accumulating experimental data.

Two new phenomena were particularly important for the development of our concepts of nuclear structure. They were: relatively high effective cross sections for the nucleon-nucleon scattering, and the sharp, closely spaced resonances discovered by Fermi, Amaldi, and co-workers in slow-neutron scattering and capture. The latter phenomenon could be explained not at all in terms of the picture in which the neutron is moving in an average potential. Thus Niels Bohr's concept of the "compound nucleus" came into being. In this picture, the state of the nucleus is characterized by intimate coupling of all nucleons to each other; this description does not allow us to speak of the motion of a single nucleon independently of the simultaneous state of motion of all the other nucleons. However, this intuitive, semi-classical 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; this originated outside Copenhagen, but, after being seen on every blackboard of the Copenhagen institute at all hours, naturally it received appropriate space in the above-mentioned "Bethe bible." Probably every theoretician pondered long and often about its interpretation and even about its proof; and it occupies many minds even today.

The ground state of a nucleus also was mainly described in terms of Bohr's picture. A concept of nuclear matter was formed in which this nuclear matter is packed to saturation density and has binding energy proportional to its volume; for real nuclei it has surface tension with surface energy proportional to its surface. The "Bethe bible" also contains an excellent discussion of the basis of these assumptions. The greatest success of this model was the description of nuclear fission by Bohr and Wheeler (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 emerging as free particles from the nucleus during nuclear transformations must exist in the same form inside the nucleus, was heavily emphasized by Fermi's papers on beta decay (1933–34). In these papers the above-mentioned dilemma, which arises from the concept of "electrons inside the nucleus," was literally dissolved into nothingness. Fermi drew radically important consequences from the idea that the proton and the neutron are two quantum states of a single fundamental particle, the nucleon. Between these two states quantum transitions can take place (Fermi used Heisenberg's version of the isospin formalism in his theory). Such a quantum transition is accompanied by the creation of an electron and a neutrino. Today's young physicist, who already as a student juggles creation and annihilation operators on the blackboard, can hardly get the feeling of what a conceptual breakthrough was contained in Fermi's formulation. As an illustration, let me quote from a historical letter by Pauli in December 1930 in which he proposed for the first time his neutrino hypothesis to his befriended colleagues: "I came to a desperate conclusion . . . namely, it seems possible that *inside the nucleus* there can *exist* electrically neutral particles which I shall call neutrinos (*4*). . . . The continuous beta spectrum becomes understandable if one assumes that, during beta decay, emission of an electron is accompanied by *emission* of a neutrino." I emphasize the words *exist inside the nucleus* and *emission*. Pauli certainly did not choose these words simply to make his ideas more digestible to his experimenting colleagues, but because the words represented the physical ideas of those days. This is even more remarkable in view of the fact that the concepts and techniques of particle creation used by Fermi had been available long before in the so-called second quantization of Jordan, Klein, and Wigner. However, two years later, in his *Handbuch* article Pauli himself regarded it only as a mathematical trick; Fermi's work finally convinced him that there was real physics in it.

Yukawa's work also occurred in that half of the decade. He showed that the forces between nucleons are transmitted by a field which must show retardation effects and quanta associated with these retardation effects, the mesons. The latter are perhaps of secondary importance in the nuclear structure problems since it was practically established in 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 nonrelativistic quantum mechanics (*5*). 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 it is permanently inscribed in my memory from our conversations. This idea was as follows: Since the field is strongly coupled to its source, the hitherto existing picture of the "compound nucleus" may be much too naive. Perhaps, the only sensible concept is to consider the whole nucleus as a field which is highly nonlinear because of strong coupling; when this field is quantized it gives, in addition to other conserved quantities like angular momentum, integral charges Z, and energies (that is, masses) that form a spectrum with values close to the integral numbers A on which the "excitation energy" bands are superimposed. The assumption that in the nucleus there exist Z protons and $(A-Z)$ neutrons such as we encounter as free particles in appropriate experiments would make almost no sense.

Naturally, the skepticism of Schroedinger (mentioned at the beginning) would thus be formulated in its extreme. However, Niels Bohr conceived a picture of the nucleus which closely resembles our current concepts in high-energy physics on elementary particles and "resonances" (for example, such as ρ-, ω-, and η-mesons). Certainly, one should not lose sight of this point of view in nuclear physics either, although it has been shown since (*6*) that it is possible to speak of the existence of individual nucleons in a nucleus as a useful approximation.

The picture of the nucleus just described is in accord with the fact that just by glancing at the table of stable isotopes we can see that the nuclear properties are continuous functions of A and Z. To be sure, there were indications of discontinuities and windings in the valley of the energy surface. I have already pointed out the exceptional cases of the nuclei with Z and $N =$ 2, 8, 20. It also seems strange that the alpha energy does not increase uniformly as one goes further away from alpha-stable nuclei in the mass valley; instead it is largest right at the polonium isotopes. This indicates that special exceptions occur for $Z = 82$. Similarly, in the diagram in which alpha energies are plotted against Z and N, we see curves with steep slopes from $N = 128$ to $N = 126$; Gamow called this figure the "Heisen-Berg." The work of Seaborg and collaborators made the profile of these "hills" even more striking. Elsasser, Guggenheim, Ivanenko, and others 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. But, mainly because of the success of Bohr's compound-nucleus model, there was a tendency to consider these phenomena as curiosities of little significance to the fundamental question of nuclear structure.

The war years and the first few years thereafter put physicists in Germany into oppressive isolation, but at the same time, remarkably enough, these years provided some leisure to pursue many a question, even perhaps some problem seemingly leading nowhere in particular. At that time I held frequent discussions with Haxel in Berlin and Göttingen and with Suess in Hamburg of the empirical facts which single out the above-mentioned numbers. Suess paid more and more attention to them, primarily in his cosmo-chemical studies: he found that in the interval between the known numbers, additionally the numbers Z and $N = 50$ and $N = 82$ were clearly prominent (*7*). Haxel, at first quite independently, encountered the same numbers from other nuclear data.

Although my two colleagues wanted to convince me that these numbers held the key to nuclear structure, I did not know what to make of it at first; I thought the name "magic numbers," whose origin was unknown to me (*8*), to be very appropriate. Then, a few years after the war I had the luck to return to Copenhagen for the first time. There in a recent issue of the *Physical Review* I found the work of Maria Goeppert-Mayer, "On closed shells in nuclei," where she too collected the empirical evidence for the remarkable features associated with these numbers. That gave me courage to talk about this work, along with our results, in a theoretical seminar. I shall never forget this seminar. Niels Bohr listened very carefully and threw in questions which became more and more lively. Once he said: "But that is not in Mrs. Mayer's papers!"; Bohr evidently had carefully read, and thought about, this work. The seminar turned into a long, 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. It was only from that hour on that 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 (*9*) during an exciting discussion with Haxel and Suess, in which we tried to include all the possible empirical facts in this scheme. As we did this it turned out that because of the spin-orbit coupling the number 28 should be something like a magic number. I remember how we looked for some experimental indication of this, and I remember being pleased when we found some indication of it among the still-meager data that were 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 on the ground that "It is not really physics but only playing with numbers." It was only because of the lively interest in the magic numbers displayed by Niels Bohr that I then sent the same letter to Weisskopf who forwarded it to the *Physical Review*. But 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 the first comments of Bohr seems remarkable to me: "Now I understand why nuclei do not show rotational bands in their spectra." With the accuracy of measurement available at that time one could look for such spectra only 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 lighter nuclei as well as most of the others show in fact no rotational bands; Bohr's argument was that, of course in a picture in which single particles move independently in an average spherically symmetric potential, there can no longer be any place for a superimposed rotation of a nucleus as a whole, just as in the system of electrons in an atomic shell.

Even though the shell model finally proved to be more than just a convenient language with which the experimentalists could compare their results and which perhaps brought to light a few fundamental features of nuclear structure, during the following years I still had to agree with Robert Oppenheimer when he told me: "Maria [Goeppert-Mayer] and you are explaining magic by miracles." Only recently in his lecture at Oak Ridge, Wigner said a similar thing, carefully choosing, however, his own words.

From the start it was clear to me as well as to Mrs. Geoppert-Mayer that apparently the shell model could approximately describe only the ground state and the low-excited states of nuclei. While the consequences of the Pauli principle for nucleon-nucleon interactions could possibly guarantee the self-consistency of this picture, the Pauli principle 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, present in the ground state as well.

Therefore, during my next visit to Copenhagen it gave me a certain satisfaction when, questioned about news on the shell model, I could instead talk of the ideas which then occupied my namesake Peter Jensen and me as well as Steinwedel and Danos. Following a suggestion by Goldhaber and Teller, we tried to provide a semiclassical 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 the state of motion such that strict phase relations exist among *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 to the symmetry energy and to the nuclear radius in a satisfactory way. The width of the "giant resonance" provided a measure of the rate at which such phase relations disappear. Niels Bohr understood immediately why the study of this particular type of "collective motion" (as one puts it today in the jargon of specialists) was especially close to my heart. One had to establish at which excitation energies the correlations enforced by the nucleon-nucleon interactions become dominant over the effect of the averaged forces, even if importance of the the correlations is kept down in the ground state by Pauli's principle.

In the following years much work was devoted to the study of such correlations. First of all, a most remarkable feature of current nuclear physics came to light as a consequence of the work of Kurath and of the former Harwell group (Flowers, Elliot, and others) on the one hand, and of the work of the young Copenhagen school (Aage Ben Bohr, Mottelson, Nilsson, and others) on the other. This feature is the fact that, even though the two pictures start from complementary, each-other limiting, points of view, their quantitative results seem immediately to meet and to overlap (*10*).

When one considers all these questions as a whole—the problems of nuclear structure, nuclear forces, as well as the problems of elementary particles —in spite of all the successes perhaps a verse of Rilke may be appropriate. In the early days of quantum mechanics my late teacher, Wilhelm Lenz, brought this verse to my attention. Rilke speaks in it of his feelings at the turn of the century, which he depicts as a large book in which one page is turned over; he concludes:

> "The lustre of the new-turned page one senses,
> Where everything may yet unfold;
> The silent powers measure their expanses;
> Each other darkly they behold."

Notes

1. Some thought that it might even become necessary to give up the conservation laws in their current form in connection with the problem of beta decay.
2. That is, the fact that the lifetime of an α-emitter changes by 25 powers of ten when the alpha-particle energy increases by a factor of two.
3. However, Heisenberg's interest extended far beyond this to the following question: What properties must the forces possess in order to give rise to the nuclear saturation phenomenon? In order to explain this phenomenon he introduced the concept of "exchange forces" which he formulated in terms of the "isospin" formalism first invented for this purpose. This created the conceptual apparatus which is still used in discussing the most direct studies of nucleon-nucleon interaction, namely, 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 the hard core or "almost the hard core." Heisenberg also discussed this possibility in one of his papers.
4. In this 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.
5. However, the retardation effects could be significant: for instance, in precise calculations of "forbidden" beta and gamma transitions.
6. In particular, through the work of Brueckner and recent literature inspired by it.
7. V. M. Goldschmidt also came to the same conclusion; Suess and I had the privilege of discussing it with him in Oslo in 1942–43.
8. I learned only yesterday that the name was coined by Wigner.
9. It was just as well that I was not too well versed in "Bethe's bible"; and the old arguments against a strong spin-orbit coupling were not quite present in my memory.
10. 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 for each pair of nucleons. Thus it was shown that, even with only a few nucleons outside a closed shell, one obtains spectra very similar to the rotational spectra. In this way, although it is difficult to perform a quantitative calculation, one can understand how in nuclei with many nucleons outside closed shells (for example, the rare-earth region and the nuclei beyond radium) there are many close-lying and very different particle states contributing to configuration mixing, creating correlations of the type that can give rise to 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 particle states are calculated. Then the coupling of the particle motion to the 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 nucleon motion; this provides the model with adequate flexibility to account for all the new empirical facts.) It was shown that this easily calculable "unified model" could as well 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 particle motion of the odd nucleon.

News and Comment

High-Energy Politics: Forces Now Jockeying for Position as Plans Proceed for Giant New Accelerator

In about 2 or 3 years, it now seems likely, construction will begin in this country on the most expensive basic research facility ever built—a nuclear accelerator of approximately 200 billion electron volts (bev) that is expected to cost somewhere around $300 million.

The machine, now under design at the Lawrence Radiation Laboratory, in Berkeley, California, would be wholly paid for by the federal government. It has not yet been formally approved by the executive or authorized by the Congress, but the preliminary planning is well advanced, and the ingredients for an affirmative decision are falling into place. When the accelerator, according to a widely accepted schedule, goes into full operation, around the mid-1970's, its annual running costs will be at least $50 million. Particles from the machine will possess at least six times more energy than those from any accelerator now in operation; and though there are serious discussions of eventually building even larger accelerators—the Brookhaven National Laboratory, on Long Island, is studying a 600- to 800-bev machine that might cost $1 billion —it is likely that, at least until the 1980's, the 200-bev accelerator will be the costliest, the biggest, and, as such things are often measured, the most prestigious piece of scientific equipment in the world. Need any more be added to explain why scientists and politicians, sometimes in curious combination, are now maneuvering over the unresolved issues of where the machine will be built and how it will be managed?

The maneuvers have generally occurred out of public view, but in the course of hearings, 2–5 March, before the Joint Committee on Atomic Energy (JCAE) a good deal of light was cast on the current deployment of forces; and subsequent inquiry turned up a bit more. On the basis of what is now visible, it appears that, although peacemaking forces are at work, a scientific-political storm of prodigious proportions may be in the making.

A review of the organizational cast of characters in the brewing storm must start with the Lawrence Radiation Laboratory, which the University of California operates under a contract with the Atomic Energy Commission (AEC), source of virtually all the government money in high-energy physics. Lawrence, which has been designing the 200-bev machine for the past 2 or 3 years, would understandably like to see its creation built in its own backyard, though with some reluctance it now seems willing to concede that the size, cost, and scientific potential of the new machine justify a broad-based management, rather than the management of a single university. However, among many non-Lawrence physicists, there is, justifiably or not, something of a store of ill will toward Lawrence's management of its present facilities, based on the contention that Lawrence has been laggard in admitting outside researchers to the use of what is supposed to be a national laboratory. (In the course of the JCAE hearings, Glenn Seaborg, chairman of the AEC, said that the Lawrence laboratory "is almost completely integrated in the Berkeley campus . . . and is therefore less of a national laboratory than the other laboratories . . . I don't mean by this that . . . visiting scientists aren't welcome. I just think that in any description of that particular laboratory, it is clear that it is a laboratory integrated in a single university.") Lawrence administrators contend that the laboratory is as wide open to outsiders as are the major high-energy