1932—Moving into the new physics

The exciting events of the early 1930's raised high hopes for progress in nuclear physics and, before the end of the decade, had changed its pace, scale, cost and social applications.

Charles Weiner

In 1972 we celebrate the fortieth anniversary of the "annus mirabilis" of nuclear and particle physics. Seen from the perspective of the present, the cluster of major conceptual and technical developments of 1932 mark that "marvelous" year as a very special one. It began with Harold Urey's announcement in January that he had discovered a heavy isotope of hydrogen, which he called "deuterium." In February James Chadwick demonstrated the existence of a new nuclear constituent, the neutron. In April John Cockcroft and E. T. S. Walton achieved the first disintegration of nuclei by bombarding light elements with artificially accelerated protons. In August Carl Anderson's photographs of cosmic-ray tracks revealed the existence of another new particle, the positively charged electron, soon to be called the "positron." And later that summer Ernest Lawrence, Stanley Livingston and Milton White disintegrated nuclei with the cyclotron, an instrument that would generate almost 5-million electron volts by the end of that eventful year.

New particles, new constituents of the nucleus and powerful new techniques for probing its structure—they all provided a wealth of fresh challenges and opportunities for theory and experiment. Physicists who remember the excitement of those days sometimes sound as if they were relishing an excellent wine when they smile and comment: "It was a great year."

What were the circumstances and the immediate impact of these events? Was their significance recognized at the time? And what effect did they have in the decade that followed? Historians ask such questions in the hope that the answers may reveal more about the nature of scientific activity and the processes and consequences of scientific change than is evident in a mere listing of key discoveries. Particularly interesting are the social structures and processes that helped create the environment for doing nuclear physics and influenced its relationships to the scientific community and to the larger society in which it functions. The events of 1932 helped open new fields of research and led to important changes in the pace, scale, cost, organization and style of experimental physics research. In addition, the rapid growth of nuclear physics gave rise in the 1930's to public expectations of applications, expectations that were to be fulfilled in unanticipated ways before the end of the decade.

These developments are being illuminated through historical documentation and research studies underway at the American Institute of Physics Center for History and Philosophy of Physics. Here I shall draw on some of the results to provide glimpses of the circumstances of the 1932 discoveries and the immediate effect on some of the discoverers and their colleagues. Wherever possible, these individuals will speak for themselves, in excerpts from the letters they exchanged or from interviews I have more recently conducted with several of them. I shall also sketch the effect of the 1932 events on the growth of nuclear-physics research in the US in the 1930's and indicate briefly the special role of Lawrence's Berkeley laboratory: We shall see that one of the most striking effects of the "annus mirabilis" was its impact on the social organization and support of academic research.

These glimpses cannot provide a full or balanced picture, nor even a chronological listing of the many interconnected conceptual, technical and social factors involved. But they do offer some insight into the spirit of the times.

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News from the US

The stage was set at the very beginning of 1932, and the action that was soon to unfold into the dramatic developments of that year was already underway. Some of the highlights of the developing situation in the US are seen in a letter written on 8 January by Joseph Boyce of Princeton to John Cockcroft, his friend and former colleague at the Cavendish Laboratory in Cambridge:

> "I have just been on a very brief visit in California and thought you might be interested in a brief report on high voltage work there and in the eastern US as well. At Pasadena [Charles] Lauritsen continues work with his 700 000 volt x-ray tube.... He is now waiting for the GE to furnish him other transformers to go to still higher voltages. [Robert] Millikan and Anderson are working a Wilson chamber between the poles of a very large magnet and are obtaining cosmic ray recoil and disintegration tracks whose curvatures can be measured.... Everyone seems most enthusiastic about [the results], even people outside Pasadena. Some of the photographs show simultaneous ejection of (+) and (−) particles of high speed, as if

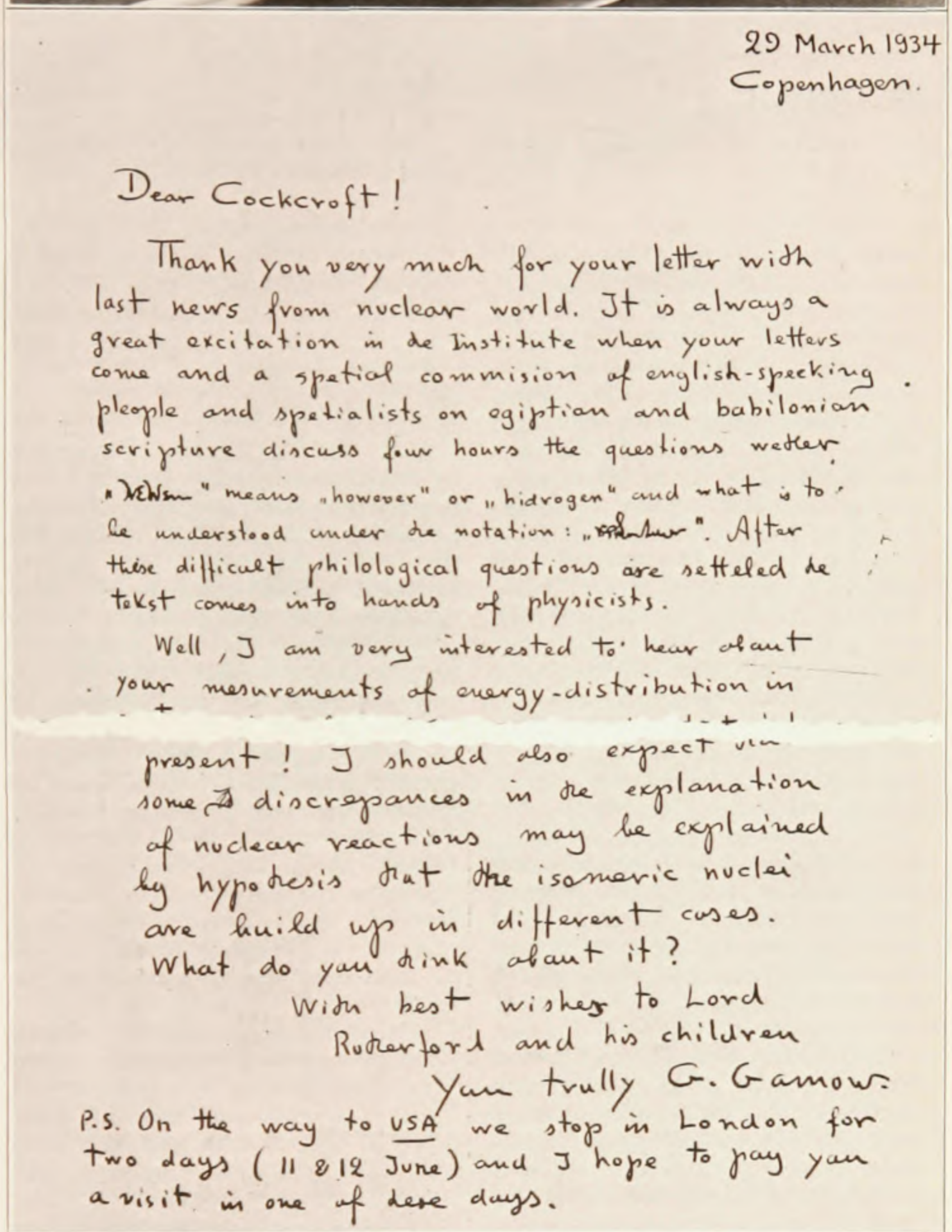
both a proton and an electron were knocked out from a nucleus by the cosmic ray.... With that and the high voltage developments everywhere it looks as if cosmic ray work will become a laboratory problem for a while rather than a mountain-climbing excuse."[1]

Boyce's news about the Caltech cosmic-ray photographs and the possible nuclear reactions involved had in fact already been brought to Cambridge by Millikan, who was visiting at the Cavendish Laboratory in November 1931 when he received copies of the photographs in a letter from Anderson. After describing the puzzling tracks—for example, "a *positive* particle moving *downward* or an *electron* moving *upward*"—Anderson concluded: "A hundred questions concerning the details of these effects immediately come to mind.... It promises to be a fruitful field and no doubt much information of a very fundamental character will come out of it...."[2]

The fundamental information did "come out" in August 1932, when Anderson identified the curious tracks in some new photographs as evidence of a "positively-charged particle comparable in mass and in magnitude of charge with an electron."[3]

Boyce's January 1932 letter went on:

"But the place on the coast where things are really going on is Berkeley. Lawrence is just moving into an old wooden building back of the physics building where he hopes to have six different high-speed particle outfits. One is to move over the present device by which he whirls protons in a magnetic field and in a very high frequency tuned electric field and so is able to give them



29 March 1934
Copenhagen.

Dear Cockcroft!

Thank you very much for your letter with last news from nuclear world. It is always a great excitation in de Institute when your letters come and a spetial commision of english-speeking pleople and spetialists on egiptian and babilonian scripture discuss four hours the questions wetter „[illegible]" means „however" or „hidrogen" and what is to be understood under de notation: „[illegible]". After these difficult philological questions are setteled de tekst comes into hands of physicists.

Well, I am very interested to hear about your mesurements of energy-distribution in

present! I should also expect von some discrepances in de explanation of nuclear reactions may be explained by hypothesis that the isomeric nuclei are build up in different cases. What do you think about it?

With best wishes to Lord Ruderford and his children

Your trully G. Gamow.

P.S. On the way to USA we stop in London for two days (11 & 12 June) and I hope to pay you a visit in one of dese days.

John Cockcroft and George Gamow (right) work on a nuclear-physics problem in December 1933. Gamow's theoretical ideas of 1928 spurred on the building of the Cockcroft–Walton accelerator, which in 1932 achieved the first nuclear disintegration by artificially accelerated particles. In a letter written 29 March 1934, Gamow exchanges the latest news with Cockcroft. Cockcroft's minuscule handwriting and Gamow's unique English spelling were notorious but apparently did not interfere with communication between the two men. (Photo, by K. T. Bainbridge, from Niels Bohr Library; original letter in Churchill College Library, Cambridge.)

velocities a little in excess of a million volts. With this he has already had proton currents of the order of 10^{-9} amps.... Then there is the Hg ion outfit.... This has already given Hg ions in excess of a million volts, by the use of about 50 000 volts high frequency.... Several more units can be added to it, all driven by a master oscillator. Then a similar device with higher applied voltages and longer electrodes to use with protons. The fourth is a whirling device for protons in a magnet with pole pieces 45 inches in diameter, with which he hopes for at least 3 million volts, perhaps more.... Then a small tesla-coil x-ray outfit is already installed, and the remaining room is reserved for a Van de Graaff electrostatic generator. On paper this sounds like a wild damn fool program, but Lawrence is a very able director, has many graduate students, adequate financial backing, and in his work so far with protons and mercury ions has achieved sufficient success to justify great confidence in his future...

Back in the east [Merle] Tuve at Washington [Carnegie Institution] is working on the development of tubes to stand high voltages, and has ordered a six foot sphere to build a one-ball Van de Graaff outfit for about 3 million [volts]. I think I sent you clippings about Van's [Robert Van de Graaff's] own results and plans...

On the way west I stopped at New Orleans for the Physical Society meeting. The most interesting paper was Urey's on the hydrogen isotope. The spectroscopic evidence alone, as reported in the abstract is quite convincing, but [Walker] Bleakney in our [Princeton] laboratory has been able to confirm it with a mass spectrograph..."[1]

Discovery of the neutron

All of these developments described by Boyce were of great interest to the physicists in the Cavendish Laboratory, where work aimed at probing the nature and structure of the nucleus had been pursued under Ernest Rutherford for more than a decade. These efforts began to pay off dramatically early in 1932.[4] James Chadwick had been searching for the neutron ever since Rutherford had suggested in his Bakerian Lecture in 1920 that such a particle might exist. He followed up observations made in 1930 by two German scientists, Walther Bothe and H. Becker, which were subsequently extended at the end of 1931 in Paris by Frédéric and Irène Joliot-Curie. Chadwick's own recollections of the circumstances provide some of the flavor of the event:

"One morning I read the communication of the Curie-Joliots in the *Comptes Rendus,* in which they reported a still more surprising property of the radiation from beryllium, a most startling property. Not many minutes afterwards [Norman] Feather came to my room to tell me about this report, as astonished as I was. A little later that morning I told Rutherford. It was a custom of long standing that I should visit him about 11 a.m. to tell him any news of interest and to discuss the work in progress in the laboratory. As I told him about the Curie-Joliot observation and their views on it, I saw his growing amazement; and finally he burst out 'I don't believe it.' Such an impatient remark was utterly out of character, and in all my long association with him I recall no similar occasion. I mention it to emphasize the electrifying effect of the Curie-Joliot report. Of course, Rutherford agreed that one must believe the observations; the explanation was quite another matter.

It so happened that I was just ready to begin experiment, for I had prepared a beautiful source of polonium from the Baltimore material [used radon tubes brought back by Feather]. I started with an open mind, though naturally my thoughts were on the neutron. I was reasonably sure that the Curie-Joliot observations could not be ascribed to a kind of Compton effect, for I had looked for this more than once. I was convinced that there was something quite new as well as strange. A few days of strenuous work were sufficient to show that these strange effects were due to a neutral particle and to enable me to measure its mass: the neutron postulated by Rutherford in 1920 had at last revealed itself."[5]

Chadwick's letter announcing the discovery was to appear in *Nature* on 27 February, 1932[6] and on 24 February he sent proofs of the letter to Niels Bohr in Copenhagen. Bohr then invited Chadwick to come and discuss his work at the small informal conference that had been planned for the second week of April at the Copenhagen institute.[7] These annual week-long conferences had been started in 1929 and

Participants in the April 1932 conference at Niels Bohr's Institute of Theoretical Physics in Copenhagen. Seated in the first row are Léon Brillouin (left), Lise Meitner and Paul Ehrenfest. Seated behind and to the right of Ehrenfest is H. A. Kramers. The first six people, from the left, standing along the wall are Werner Heisenberg, Piet Hein, Niels Bohr, Léon Rosenfeld, Max Delbrück and Felix Bloch. Seated second from the right in the last row is P. A. M. Dirac, with R. H. Fowler on his right. Other visitors to Copenhagen in the group include Walter Heitler, Karl von Weiszäcker, Guido Beck and C. G. Darwin. (Photo: Niels Bohr Institute, Copenhagen.)

they brought together physicists from many different countries to discuss, as Bohr put it, "actual atomic problems."

Chadwick was unable to attend the meeting, but R. H. Fowler of Cambridge was present and provided an up-to-the-minute account of the experimental work underway by Chadwick, Feather and P. I. Dee in their follow-up of Chadwick's discovery. The conference was truly international: The 22 foreign physicists were from 17 institutions in nine countries. Among the participants were C. G. Darwin, Max Delbrück, Paul Ehrenfest, P. A. M. Dirac, R. H. Fowler, Werner Heisenberg, Walter Heitler, H. A. Kramers and Lise Meitner.[8] Bohr's personal style of thinking out loud set the tone for the Copenhagen conferences and stimulated a lively exchange of information, ideas and interpretations. The neutron, like the other topics discussed at the meeting, found a place in the parody of Faust written and performed there by some of the participants:

> "Now a reality,/Once but a vision.
> What classicality,/Grace and precision!
> Hailed with cordiality,/Honored in song,
> *Eternal neutrality*/Pulls us along!"[9]

In June, only two months after the Copenhagen conference, Heisenberg submitted the first in a three-part series of papers that incorporated the neutron in a theory of the nucleus to demonstrate that quantum mechanics could be applied to many existing nuclear problems.[10] That summer he was a lecturer at the University of Michigan's annual summer schools in theoretical physics, which attracted physicists from all over the US and Europe. In November Samuel Goudsmit wrote to Bohr from Michigan, commenting on Heisenberg's lectures:

> "We followed with great interest his new ideas about the nucleus but everyone feels that there still are great difficulties. It is strange and regrettable that the discovery of the neutron did not give some more fertile clues for progress. In many respects the situation has not changed much from what it was at the Rome meeting a year ago, except that the difficulties can now be formulated more sharply. I have been playing around with nuclear magnetic moments, but none of my speculations yielded any results certain enough to communicate."[11]

Bohr replied:

> "Not least in connection with the [difficulties of relativistic quantum mechanics] we have all been very interested [in] the problem of nuclear constitution and the possible clue to this problem offered by the discovery of the neutron. Still I quite agree with you as regards the very preliminary character of any attempt hitherto made to attack the problem on such lines."[12]

An acceptable theory of the nucleus was still beset with difficulties by the end of 1932, but the neutron did attract theorists to nuclear problems because it provided fresh challenges and possibilities for theory. One senior nuclear theorist recently explained: "I went into nuclear physics only after 1932 . . . after the discovery of the neutron in 1932, it was in a general way clear what had to be done . . . I cannot invent something out of nothing. . ." Another recalled: "For me [nuclear physics] started with Heisenberg's paper . . . [he] pointed out that now that the neutron has been discovered, one can think of starting a theory of the nucleus. This impressed me very much."[13]

Accelerators attack the nucleus

Other news from the Cavendish followed on the heels of the discovery of the neutron. On 21 April 1932, about a week after the neutron was discussed at the Copenhagen meeting, Rutherford wrote to Bohr:

> "I was very glad to hear about you all from Fowler when he returned to Cambridge and to know what an excellent meeting of old friends you had. I was interested to hear about your theory of the Neutron. . . .
>
> It never rains but it pours, and I have another interesting development to tell you about of which a short account should appear in *Nature* next week. You know that we have a High Tension Laboratory where steady D.C. voltages can be readily obtained up to 600 000 volts or more. They have recently been examining the effects of a bombardment of light elements by protons. . . ."

Rutherford went on to describe the work of Cockcroft and Walton in which they achieved the first artificial nuclear disintegrations with the high-voltage accelerator that they had been developing at the Cavendish since 1929. He concluded:

> "I am very pleased that the energy and expense in getting high potentials has been rewarded by definite and interesting results. . . . You can easily appreciate that these results may open up a wide line of research in transmutation generally."[14]

Bohr's response reveals that he fully shared Rutherford's evaluation of the significance of this latest development:

> "By your kind letter with the information about the wonderful new results arrived at in your laboratory you made me a very great pleasure indeed. Progress in the field of nuclear constitution is at the moment really so rapid, that one wonders what the next post will bring, and the enthusiasm of which every line in your letter tells will surely be common to all physicists. One sees a broad new avenue opened, and it should soon be possible to predict the behavior of any nucleus under given circumstances."[15]

Thirty-five years later, Cockcroft warmly recounted the atmosphere in the Cavendish when they achieved their results:

> "It was extremely exciting to see the alpha particles in this transmutation. The first thing we did was to call up Rutherford on the laboratory exchange and invite him to come down and have a look at the scintillations, which he did. He, of course, was very excited about it."[16]

The story was soon carried in newspapers throughout the world, reviving alchemical dreams and hopes for new energy sources. For example, *The New York Times* carried articles on the Cockcroft–Walton work, with the following headlines: 1 May, "Atom Torn Apart with Energy Rise;" 3 May, "Hail New Approach to Energy of Atom;" 3 May, "Value Put in Energy Gain;" 4 May, "Atomic Energy;" and 8 May, "Atom Bombarders."

What of the reaction within the physics community? Cockcroft recalled the rapid response that "came from Berkeley and from Tuve's lab in Washington, where they had all been working on development of high-voltage equipment, such as the cyclotron or the Van de Graaff machine, toward just this kind of experiment."[16] On 20 August 1932, Lawrence wrote to

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Cockcroft and Walton:

"I want to thank you very much for the reprints of your epoch making experiments on the disintegration of the elements by high velocity protons, and I hope you will continue to send me accounts of your work in the future. Under separate cover I am sending you reprints of the work of myself and my coworkers on methods for the acceleration of ions, which you may find of some interest.

At the present time we are attempting to corroborate your experiments using protons accelerated to high speeds by our method of multiple acceleration. We have some evidence already of disintegration, though as yet we can not be certain. Unfortunately our beam of protons is not nearly as intense as yours—although of higher voltage. Whenever we obtain some reliable results, of course, we will let you know promptly."[17]

Stanley Livingston recently recalled the Berkeley response:

"With the 11-inch [cyclotron] we had resonant particles of full energy in a collector cup at the edge of the pole. Our first publication was sent in to the *Physical Review* on February 20th, 1932 reporting 1 200 000 volts. Cockcroft and Walton's paper came out later that spring and showed that they had disintegrated nuclei with even lower energies.

Well, we weren't ready for experiments yet. We didn't have the instruments for detection. I had built the machine but had not included any devices for studying disintegrations. So we had to rebuild it. Now, Milton White was a student at that time, following right along behind me. He joined with me that spring in helping to rebuild the machine, and Lawrence also put in an emergency call to his friend Don Cooksey at Yale, who came out. Franz Kurie, a graduate student, also came out with Cooksey for the summer. Meanwhile we re-equipped the chamber with a target mounted inside where it would be hit by the beam, and a thin-foiled window on the side where we could mount counters. I think the first devices used for detecting the product particles were Geiger point counters. We set the threshold low so that they wouldn't trigger with x-rays or ultra-violet and they would count with particles. It wasn't long before we started to observe disintegrations, too. . . ."[18]

The exciting developments of 1932 stirred new interest in nuclear physics and the pace of activity began to quicken as the new techniques and concepts were put into action. The need for personal visits to the laboratories involved was obvious, if one was to keep abreast of the new work. At the beginning of 1933, Cockcroft planned to visit the US to study the work of Tuve, Lauritsen and Lawrence and to discuss with them the future of their various methods of nuclear disintegration. Applying for a travel grant for Cockcroft's trip, Rutherford wrote to the Rockefeller Foundation: "During the last year we have had visits from a number of workers interested in this field, and have given them as much information as we possess on our own methods."[19] He stressed that now it was equally valuable for the Cavendish workers to have similar first hand knowledge of work underway in US Laboratories.

Before Cockcroft's June trip regular letters kept physicists at the various institutions informed of one another's techniques and results. Lawrence's enthusiasm was evident when he wrote to Cockcroft at the beginning of June 1933: "We have been having a most exciting month in the laboratory. We have obtained so many disintegration effects that it is impossible for me to keep them all in mind. I am almost bewildered by the results. I will mention only a few as I will be seeing you soon. . . ."[20]

Cockcroft later recalled his impressions of that visit to the Berkeley laboratory:

"It was really interesting to see it actually in operation after having read so much about it in the journals. I was very much impressed by the way of working; to see the sealing-wax and string way of working on the cyclotron, which functioned for very short periods of time. They had a two-shift system, one shift doing the experiments, the other shift keeping the cyclotron going. And as soon as a leak developed, the maintenance shift would dash in and the experiment shift would retire backwards. A highly organized system."[16]

Just before Cockcroft left for the US, and a little more than a year after Rutherford had written Bohr that the Cockcroft-Walton results "may open up a wide line of research," Rutherford wrote to Gilbert N. Lewis, the renowned physical chemist at Berkeley, who had supplied him with heavy hydrogen for use as a projectile in the Cavendish accelerators. The aging dean of nuclear physics was enthusiastic about the new prospects for research in the field in which he had pioneered for many decades:

"I was delighted to receive your concentrated sample of the new hydrogen isotope in good shape, and we shall certainly take an early opportunity of examining its effects in our low voltage apparatus which Dr. Oliphant and I have been using the past year.

I have been enormously interested in your work of concentration of the new isotope with almost unbelievable success. I congratulate you and your staff on this splendid performance. I can appreciate the extraordinary value of this new element in opening up a new type of chemistry. If I were a younger man I think I would leave everything else to examine the effects produced by the substitution of H^2 for H^1 in all reactions.

Next, I should like to congratulate Lawrence and his colleagues for the prompt use they have made of this new club to attack the nuclear enemy. Cockcroft showed me the letter of Lawrence giving his preliminary results which are very exciting. These developments make me feel quite young again as in the early days of radioactivity when new discoveries came along almost every week, for it is a double scoop not only to prepare this new material but also to have the powerful method of Lawrence to examine its effects on nuclei. I wish them every success in their work and as soon as we can arrange it, I will try out the effects we can observe at our low voltages."[21]

In October 1933 the Solvay Congress in Brussels brought together most of the major participants in the burgeoning field, and a year later, in London, another international conference on nuclear physics was held. By that time there were many more important new developments to discuss, including Enrico Fermi's theory of beta decay, the discovery of artificially induced radioactivity by the Joliot-Curies in Paris, and the technique of neutron bombardment to produce artificial radioactivity, which was systematically applied and developed by Fermi's group in Rome.

An American who attended the London conference was Frank Spedding, a former student of Lewis. His comments, in a letter to Lewis in December 1934, characterize the rapid pace of nuclear physics in the aftermath of the 1932 events, and show the reaction of a nonspecialist:

"There was also a symposium on nuclear physics. This field is moving so rapidly that one becomes dizzy contemplating it. With talk of the experimental properties of H^3, He^3, He^5, the new artificial radioactive elements, the neutron and positron, and the predicted properties of the neutrino and proton of minus charge, one who has been brought up on the old naïve picture of protons and electrons in the nucleus feels bewildered. I managed to attend a few of these sessions and found them

E. T. S. Walton and John Cockcroft (right) flank Ernest Rutherford in a 1932 photograph taken outside the Cavendish, after their accelerator had disintegrated nuclei by bombardment with protons. (Photo: UK Atomic Energy Authority.)

James Chadwick, working at the Cavendish Laboratory, had been searching for evidence of the neutron ever since Ernest Rutherford's suggestion, in 1920, that such a particle might exist. In 1932, about the time of this photograph, his efforts became successful. (Photo: Meggers Collection, Niels Bohr Library.)

extraordinarily interesting. There was one rather amusing incident that occurred here. Prof. Born had prepared a rather involved paper on the quantum theory of the nucleus. (An extension of Dirac's theory of the electron.) He wrote the paper longhand labelling it "For the Conference on Nuclear Physics." He made his "n"'s and "u"'s much alike so that his stenographer in copying it wrote "For the Conference on Unclear Physics."[22]

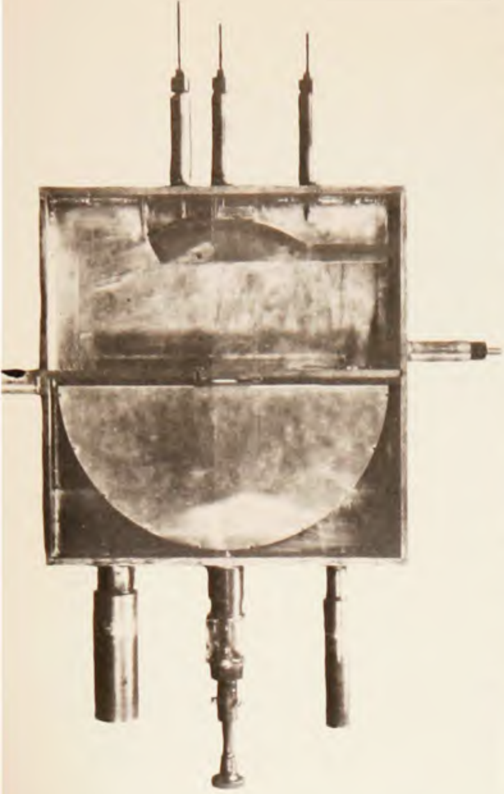
Indications of growth

One thing *was* clear about nuclear physics in the early 1930's: It was growing more rapidly than any other field of physics. This was especially true in the US, which provided a particularly fertile environment for the new and growing field to take root. The effect of the *annus mirabilis* can be clearly seen in the jump in nuclear-physics publications in *The Physical Review* between 1932 and 1933. The results of a study by Henry Small at the AIP Center for History and Philosophy of Physics show that in *The Physical Review,* nuclear-physics papers, letters and abstracts increased from 8% of the publications in 1932 to 18% in 1933 and reached 32% by 1937. A further examination of the dramatic increase in the number of nuclear-physics publications in *The Physical Review* between 1932 and 1933 shows that 42% of the increase was due to publications involving the neutron and 18% to those involving disintegration by protons. Publications from Berkeley alone accounted for 38% of the total increase in nuclear-physics papers between 1932 and 1933.[23]

While the total number and proportion of nuclear-physics papers was rising, the number of nuclear-physics papers that acknowledged funding was increasing even faster. In 1930, before the *annus mirabilis,* nuclear-physics papers constituted a very minor percentage of the papers in *The Physical Review* and a similarly small percentage of the funded papers. By 1935, however, when nuclear physics accounted for 22% of all papers in *The Physical Review,* fully 46% of the total funded papers were nuclear. And by 1940, when 34% of *The Physical Review* papers were in nuclear physics, they accounted for 55% of the funded papers. Clearly, nuclear physics was not only growing but also becoming a relatively heavily funded research subject. In fact, by 1939 fully one third of the nuclear-physics papers were being funded.

Another indication of the growth of a field is the number of new physics PhD's whose dissertation research is on a topic within the field. Here again nuclear physics showed an increase in the US from 2 new PhD's in 1930 to 41 in 1939. It was the only field of physics to increase steadily through the decade, and from 1937 on more new PhD's specialized in nuclear physics than in any other single field.

Of course, the growth of nuclear physics in the 1930's was not due solely to the discoveries of 1932. But these discoveries did help to focus the attention of a significant part of the physics community on nuclear phenomena and on the new possibilities for fruitful research in that field, possibilities which were expanded yet further with the development, availability and increasing-



Berkeley cyclotrons. Lefthand photo shows chamber of the 11-inch cyclotron. In early 1932, Ernest Lawrence and M. Stanley Livingston achieved a 10^{-9}-ampere, 1.22-MeV proton beam; later experiments with this chamber confirmed the artificial disintegration of lithium that Cockcroft and Walton had observed at lower energies. The 60-inch Berkeley cyclotron (right) was built for medical applications. This 1938 photograph shows Luis Alvarez astride the magnet-coil tank, Edwin McMillan on the "D" stem casing and, standing (left to right), Don Cooksey, Dale Corson, Lawrence, Robert Thornton, John Backus and Winfield Salisbury. (Photos: Lawrence Radiation Laboratory.)

ly productive use of particle accelerators. These instruments became central to experimental work at a number of new research centers that began to flourish during the period.

Special role of Berkeley

Because Berkeley, and particularly Lawrence's radiation laboratory there, played such a major role in these developments, let us take a brief glimpse into the Berkeley scene in the 1930's. Clearly the Berkeley work was very important in 1932, and it accounted for a large part of the field's subsequent productivity in the US. Throughout the 1930's Berkeley not only produced more nuclear-physics papers and PhD's than other US institutions but also had the lion's share of funded nuclear research. These statistics, however, are only a part of the story, for Berkeley also played a key social role in developing the entire field of nuclear physics internationally.[24]

Berkeley was the home of the cyclotron, the instrument that became central to nuclear physics research as it took root in more and more institutions throughout the world in the 1930's. The early 11-inch model, which first accelerated protons to energies of 1.2 million electron volts by the beginning of 1932 and achieved nuclear disintegrations later that year, had been made possible by a grant of $500 from the National Research Council of the National Academy of Sciences in the spring of 1931. By the spring of 1940 Lawrence had obtained a grant from the Rockefeller Foundation for more than one million dollars toward the cost of creating a 100-million volt cyclotron. In the intervening years—aided by grants from the University of California, the Research Corporation, the Chemical and Macy Foundations, the US Works Progress Administration (WPA), as well as individual donors—several generations of cyclotrons of steadily increasing energy and wide applications had been developed at Berkeley by Lawrence and the team he had assembled there.

The cyclotron had proved to be an excellent instrument for particle-scattering experiments and an unsurpassed producer of powerful neutron sources that could make a large variety of new isotopes, thus providing previously unavailable data essential for a fuller understanding of nuclear structure. These unstable isotopes were also used for therapeutic medical applications and as tracers in pioneering studies of chemical and biological processes. The unique role of the cyclotron as a producer of isotopes began in 1934, after the Joliot-Curies discovered artificially induced radioactivity. Later that year Fermi's group in Rome demonstrated induced radioactivity by neutron bombardment. The Berkeley cyclotron was soon at work systematically producing artificially radioactive isotopes of a number of elements. Lawrence's production of a radioisotope of sodium in 1934 was especially significant because of its potential application to medical therapy.

The potential biological and medical applications helped to create interest in and financial support for the subsequent development of cyclotrons at Berkeley and at other places. During 1935 a number of institutions started to build cyclotrons because they recognized that it was a major tool for nuclear studies. At several of these places—for example, Bohr's institute in

Copenhagen where George de Hevesy was pursuing his tracer studies, the University of Rochester where the physics department was headed by Lee DuBridge and the University of Michigan where Harrison Randall was department chairman—the cyclotron projects were proposed and financed as part of planned collaborative research efforts involving the physics, medicine and biology departments. At Berkeley, such joint efforts were wholeheartedly pursued and were immensely strengthened when the physician John Lawrence arrived from Yale in the mid-1930's to collaborate with his brother and others in a full medical program involving not only isotopes but also experiments in the use of neutron beams for cancer therapy. Radiochemistry also blossomed at Berkeley where strong ties existed between the physics and chemistry departments.

Recognition of the role of the cyclotron in physics, chemistry, biology and medicine resulted in a proliferation of the instruments at institutions throughout the world in the late 1930's, and almost all of these projects depended on assistance from the Berkeley experts. Detailed technical information and advice was communicated through a lively network of personal letters, circulation of unpublished technical memoranda and progress reports, personal visits, and exchange of personnel. Don Cooksey, who played a key role as the Berkeley hub of this international informal communication network, jokingly referred to it in June 1938 as the "Cyclotron Union of the World."[25] At that time Berkeley-trained physicists were building cyclotrons in Copenhagen, Stockholm, Paris, Cambridge, Liverpool, Tokyo, and at more than a dozen US institutions. The Berkeley radiation laboratory played a key role as an international information center, a training school, a supplier of cyclotron-produced radioactive materials for use in other laboratories, and a source of skilled physicists who were available to help other institutions enter the cyclotron field. Thus the impact of Lawrence's laboratory transcended the important results being obtained in Berkeley and had a tremendous multiplier effect on the entire field in the 1930's.

I have described some of the events of 1932 and the immediate responses of some of the participants. It was clear to them that the new developments would open up an exciting period for fruitful research in nuclear physics. The field did flourish in the following years and by the mid-1930's was firmly established in a number of new centers of nuclear research.

In March 1972 champagne toasts were drunk in Batavia, Illinois to celebrate the achievement of accelerating protons to record energies of 200 GeV through the four-mile circumference of the giant new accelerator there. It was a fitting observance of the 40th anniversary of the "annus mirabilis" of 1932, and makes one wonder how soon we might see another "marvelous" year and what its impact may be on physics and society in the decade that follows.

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24. The brief sketch here is based on archival materials from the Lawrence Radiation Laboratory, the Cavendish Laboratory and the Bohr Institute in Copenhagen; physics-department files at several US universities; Herbert Childs's biography of Lawrence, *An American Genius* (Dutton, New York, 1968); and on historical accounts of the cyclotron such as those by M. Stanley Livingston and Edwin M. McMillan in PHYSICS TODAY, October 1959, 18–34, and Livingston's *Particle Accelerators: A Brief History* (Harvard, Cambridge, 1969).
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