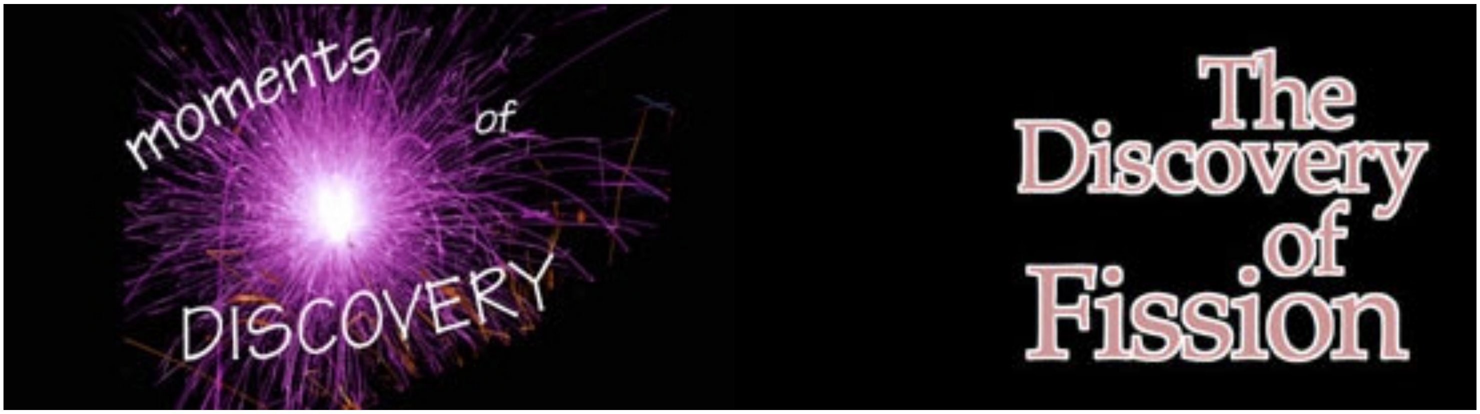

Teachers Guide

Note to teachers

The overall goal of this exhibit is to help students to recognize scientists as people, engaged in understandable human activities. The individuals whose voices are heard in this exhibit all achieved high recognition in the history of science. Nevertheless they are similar to us and to our students, for their lives, like ours, were influenced by politics, careers, and the circumstances of the times. Listening to these scientists will help make the study of science and history a more personal experience.

This exhibit aids the inclusion of science in history courses and of history in science courses. Varied attempts have been made over the past half-century to provide curricula which pay attention to the people involved in science. Many science teachers presently include some historical background in their courses, and many history teachers include some mention of modern scientific development. Many do not. This exhibit has been designed so that it will be appropriate for all these teachers. It can serve as a new resource for those teachers who currently treat history of physics while also serving as a "one shot deal" for teachers willing to give history of science a first try.

This exhibit can be a tutorial for both teachers and students. Outside the classroom this unit can serve as a tutorial for teachers whose background does not include knowledge of the history of nuclear physics.

This exhibit can also be an opportunity for professional development. Physics teachers can strengthen their background in the history of nuclear fission and the physics of nuclear model building through self-study of the module. The history in this exhibit recalls the discovery of the electron and the mass-energy equivalence ($E = mc^2$) and then progresses through the work on atomic models with its main emphasis on attempts to understand what happens when a neutron strikes uranium. Teachers can better understand the struggle of scientists to understand the nature of this interaction as they listen to the scientists themselves describe their involvement.

This exhibit can be a vehicle for interdisciplinary team teaching. Science and social studies teachers can collaborate in a meaningful way to create a unit that leans on the expertise of the respective teachers. As science teachers share their knowledge of the discoveries surrounding nuclear fission, social studies teachers can share their knowledge of events in the world during this time period. Together, they and their students will benefit from a more comprehensive view of the human dimension of science.

There are as many ways to utilize this material as there are teachers, but three sample formats are presented below. The first describes independent work for students (requiring no preparations by the teacher). The second shows how the materials can be used for a single class presentation (again requiring no preparation by the teacher). The third provides for a two-day class presentation.

We need your feedback so we can do more exhibits like this! Both our funding and our enthusiasm could falter if we don't hear from users. Please e-mail us at chp@aip.org or use the online form (at https://webster.aip.org/forms/feedback.htm) to tell us how useful this was to you (a brief word is great, comments and suggestions better still).

Contents of This Exhibit

Audio clips and accompanying text: These are central to every format of presentation. Test trials of these materials showed that in classroom use, it is best to have students read the text simultaneously with listening to the audio, rather than listen to the audio alone. Reading the text helps students to (1) understand the few voices that have foreign accents, (2) refer to helpful schematic illustrations, and (3) appreciate photographs of the physicists set into the text.

Permission is granted to the instructor to make photocopies of the text for the purpose of providing every student or every pair of students with a copy, for classroom use.

We need your feedback so we can do more exhibits like this! Both our funding and our enthusiasm could falter if we don't hear from users. Please e-mail us at chp@aip.org or use the online form (at https://webster.aip.org/forms/feedback.htm) to tell us how useful this was to you (a brief word is great, comments and suggestions better still).

Articles reprinted here: Original research—

- O. Hahn and F. Strassman, "Concerning the Existence of Alkaline Earth Metals Resulting from Neutron Irradiation of Uranium," *Naturwissenschaften* vol.27, p. 11 (Jan. 1939), summary, translated by H. Graetzer in *The Discovery of Nuclear Fission* (N.Y.: Van Nostrand Reinhold, 1971), p. 44-47.

- O. Hahn and F. Strassman, "Verification of the Creation of Radioactive Barium Isotopes from Uranium and Thorium," *Naturwissenschaften* vol.27, p.95 (Feb.1939), summary, translated by H. Graetzer in *The Discovery of Nuclear Fission*, p. 48.

- L. Meitner and O. Frisch, "Disintegration of Uranium by Neutrons: A New Type of Nuclear Reaction," *Nature* vol. 143, p. 239 (16 Jan. 1939).

- N. Bohr, "Disintegration of Heavy Nuclei," *Nature* vol. 143, p. 330 (25 Feb. 1939).

- O. Frisch and J. Wheeler, "The Discovery of Fission," *Physics Today*, p. 43-48 (November 1967).

- J. Wheeler, "Mechanism of Fission," *Physics Today*, p. 49-52 (November 1967).

Personal accounts—
Laura Fermi, "Departure," a chapter from *Atoms in the Family* (Chicago: University of Chicago Press, 1954), pp. 125-135.

A historical account of the discovery of fission—
Esther Sparberg, "A Study of the Discovery of Fission," *American Journal of Physics* vol. 32, p. 2-8 (Jan. 1964).

A commentary on the teaching of history and physics—
Gerald Holton, "The Two Maps," *American Journal of Physics* vol. 48, p. 101-119 (Dec. 1980).

Chronology: A brief chronology is given, which may be used as a summary and reference for the events described in audio clips in this exhibit.

Exercises: Assorted activities, demonstrations, questions, problems, and experiments are suggested. These exercises are organized on an accompanying summary chart under the headings of History, Physics, Science and Society. They are also grouped in terms of applicability for use before or after listening to the audio.

Further, the History and the Science and Society exercises are indicated for Discussion, Investigation, or Research.

—Discussion exercises (**D**) require no preparation or reading by the student. These exercises can be used for class discussions or as homework assignments.

—Investigation questions (**I**) require the reading of an article which is included in this exhibit, or the use of reference works such as an encyclopedia. Instructors can make the articles available for a more comprehensive assignment.

—Research questions (**R**) require library work. Some of these exercises are quite extensive and should be treated as long-term projects.

The physics exercises are identified as simple or complex.
—A simple exercise (**S**) requires no background material and is a suitable class or homework assignment.

—A complex exercise (**C**) requires that the student have access to a physics text or to some laboratory equipment.

Additional Readings and links: An annotated bibliography for instructor and student use.

The National Standards

The National Science Education Content Standards "outline what students should know, understand and be able to do in the natural sciences over the course of K-12 education." Although most science teachers are aware of the subject matter understandings (e.g., Physical Science Standards) in their respective disciplines, too little attention is devoted to the categories of:

- Science in personal and social perspectives
- History and nature of science
- Science and technology
- Science as inquiry

This exhibit provides material that speaks to these dimensions of science content knowledge as well as the required Physical Science Standards. This exhibit is an excellent vehicle by which to bring the full Content Standards to the science classroom.

Physical Science Standards: The Discovery of Fission unit and related teachers' guide provides an introduction to the structure of atoms as outlined in the 9-12 content standards. This includes historical accounts and problem solving involving the mass and charge of atoms, the Coulomb force as well as nuclear structure, nuclear forces, fission and radioactivity.

Science in personal and social perspectives: The discovery of nuclear fission and the subsequent development of nuclear weapons and nuclear power are arguably the most important interplays between science and society in the latter 20th century. The National Standards call for students to understand topics related to Natural and Human-Induced Hazards and topics involving Science and Technology in Local, National, and Global Challenges. This exhibit strongly supports this dimension of content knowledge.

History and nature of science: The Discovery of Fission unit and related teachers' guide provide an example of curriculum materials that support this content standard. The script and exercises emphasize Science as a Human Endeavor, speak to the Nature of Scientific Knowledge and provide Historical Perspectives.

Science and technology: the scientists involved in the discovery of fission note the interplay of science and technology in various discussions. From the Cockcroft-Walton accelerator to the chemistry laboratory of Hahn, Strassman and Meitner to Frisch's ionization chamber and pulse amplifier to Anderson's sharing of equipment with Fermi to the replication of experiments in New York, Paris and California, students will be well aware of how science and technology are inseparable in the quest for knowledge.

Science as inquiry: One component of the inquiry content standard is that students should understand that scientists engage in inquiry and the nature of that engagement. In the Discovery of Fission, students learn about the types of questions that scientists ask, how they rely on technology to gather data, how mathematics is used and how scientific explanations must adhere to specified criteria. They are also introduced to different kinds of investigations and communication of scientists.

Lesson Plans

The instructor can allot no class time:
Students can benefit from the exhibit through independent study. Students visit the exhibit online and perform exercises assigned by the teacher. For example, one exercise can be chosen from the summary chart and given to the class. Or different exercises can be chosen from the summary chart and assigned by the first letter of the student's last name. Or, as a third alternative, a group of exercises can be offered and each student can choose which avenue to explore.

The instructor can allot one class day:
Students can visit the exhibit as a group during class time. In this case, copies of the text should be available to every student or pair of students. Permission is granted to the instructor to make photocopies of the text for the purpose of providing every student or pair of students with a copy for classroom use. Students may be given one homework assignment prior to seeing the exhibit and one after. The summary chart can help the instructor choose an appropriate task.

The uninterrupted audio track and all the rest of the unit can be purchased at nominal cost on a CD-ROM by filling out the order form online at http://www.aip.org/history/mod/order.html which can be played in class or made available to individual students who have access to a computer.

MOST TEACHERS WILL PROBABLY FIND THE ABOVE THE BEST WAY TO USE THE EXHIBIT

OR—Teachers may elect to have students view the exhibit independently (see above) and later use class time for discussion and exercises.

The instructor can allot two class days:
DAY 1: Students view the exhibit for half of the class.

The remainder of the class is used for comments on one or more discussion questions, such as those on the accompanying chart.

A homework assignment should be chosen, for example from the summary chart, reflecting teacher or student interest.

DAY 2: Students view the second half of the exhibit.

The beginning of the class may center on reviewing homework or on fresh questions chosen by the instructor. The remainder of the class time may be spent on discussion questions emphasizing the exhibit as a whole.

Homework should be assigned. This might require the reading of an article included in the exhibit or library or Internet research as well as written exercises.

The 5E Model

The 5E model of good science instruction recommends that teachers structure the lesson so that it includes the following components: engage, explore, explain, elaborate and evaluate. In using the *Discovery of Fission* exhibit, teachers can adopt the 5E model in the following manner:

Engage: Students have all heard of nuclear power, nuclear weapons and nuclear medicine and arrive in the classroom with many opinions regarding these technological applications of our nuclear knowledge. Students should be given the opportunity to articulate their prior conceptions. Teachers should be attentive to the students' understanding so that the subsequent instruction can provide a rationale for students to continue their prior beliefs or to replace them based on their study.

Explore: Students can read and listen to the script and begin to explore the events leading to the discovery of nuclear fission. They can continue their exploration by responding to some of the exercises that are designated as "Before visiting the exhibit." These include investigations of the year 1932, the importance of the fission discovery in physics and history, as well as experiments and calculations on the size of the nucleus and decay series. The exploration can continue during the script with a historical perspective on Lise Meitner, science and society discussions on the communications, the media and the nature of discovery in addition to the calculation of energy from fission and the concept of beauty as it relates to physical phenomena.

Explain: Students should study the articles that are included in the exhibit. The original research articles may be a bit difficult in their entirety but should be attempted. Science students get too few opportunities to read any original literature. The other articles and essays were chosen as part of the exhibit because of the different perspectives that they bring to our understanding of the history of nuclear fission.

Elaborate: Students should have the opportunity to apply the knowledge from the script to new situations. Exercises that are denoted "After visiting the exhibit" can be used to focus student attention on the role of chance in history, the excess neutrons required for a chain reaction and Laura Fermi's story of her family's departure from Italy. After their involvement with this exhibit, students can also pursue the larger questions of the societal impact of nuclear technologies including weapons, power and medicine. Much has been written about the decision to drop nuclear bombs on Japan, the arms race and nuclear proliferation. With a foundation in the physics of nuclear reactions, students may wish to pursue a more intensive study of the safety, benefits, dangers and decision making surrounding nuclear power. Some suggestions are included in the *Additional Readings and Links*.

Evaluate: Many of the exercises can be used as evaluative tools for what students understand and are able to do. The teacher should help students set the criteria for successful achievement. What is the level of expectation in terms of the physics problem solving or the related research items? Evaluations can also include group projects that require students to produce informational pamphlets, to perform or create additional physics simulations, or to compose an essay or play that draws out the human and scientific elements in the history of nuclear fission.

The 4 Question model

The 4 Question model of science instruction requires that students be able to answer the following questions:

- What does it mean?
- How do we know?
- Why do we believe?
- Why should I care?

In using the *Discovery of Fission* exhibit, teachers can adopt the 4 Question model in the following manner:

What does it mean? Students should be able to explain the physics of nuclear fission including the structure of the atom and nucleus, the nuclear and electrostatic forces and radioactivity. They should also be able to explain the content standards of the National Standards in the domains of inquiry, technology, society and history.

How do we know? We know because we did experiments. How was the scientific information concerning nuclear fission accumulated? What experiments were done? For example, why were Hahn and Strassman uncertain of whether the product was radium and barium? What evidence did Thomson have for the existence of the electron?

Why do we believe? Models for the nucleus and for the fission of the nucleus were proposed. Calculations involving the energy release using $E = mc^2$ were shown to be consistent with the experiments. Other, unrelated calculations, involving the electrical potential of the daughter products before fission were also shown to be consistent. The model then makes predictions about stability of nuclei and which nuclei can undergo fission. These predictions are also verified experimentally. We believe because the theories and models make predictions and the predictions are confirmed experimentally.

Why should I care? The nuclear power debate can be more informed if the non-scientific community understands some of the physical principles involved. Questions of safety, radiation damage, and half-lives of waste products must be involved in policy decisions on nuclear power and disposal of nuclear wastes and transportation of nuclear materials. Issues of science policy should include informed debate. Students' lives will be impacted by the costs and availability of adequate power in the future.

A WORD TO THE WISE
Physics teachers may well lack experience in leading discussions. We all know, however, that it is not possible nowadays to think about science without taking account of different viewpoints on social questions. Since the interaction of science and society is often taught only superficially in social studies courses, science teachers need to explore the issues. It is recommended that teachers have a number of discussion questions created or chosen from the chart, so that if one does not develop into a useful class dialogue, a second or third question can be presented.

History teachers frequently lack experience with science demonstrations, problem solving, or explanation of scientific theories. We all know, however, that the citizen can no longer separate understanding of modern history from basic ideas about science itself. Since many students have a very rudimentary science education, a unit in the history of science may be one of their few encounters with scientific reasoning, and it is important to be sure they can follow the logic of the science itself. It is recommended that the instructor test a demonstration before presenting it to the class. Similarly for science problems, a previously worked out solution or explanation will always lead to a better class presentation.

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Suggested Exercises

BIOGRAPHICAL REFERENCES: The following sources can be used as references for most of the scientists appearing in the exhibit. Many are available in public libraries. There are also biographical materials available on the Web.

—*Dictionary of Scientific Biography* and *The Concise Dictionary of Scientific Biography* (Scribner's)
—*Asimov's Biographical Encyclopedia of Science and Technology* (Doubleday)
—There exist biographies or (*)autobiographies of Bohr, *Compton, Einstein, Fermi, *Frisch, *Hahn, Joliot, Rutherford, *Szilard, and others.

1. Ernest Rutherford left New Zealand for England and studied with J. J. Thomson. Niels Bohr and Otto Hahn went to England to study with Ernest Rutherford. Strassmann studied with Hahn, and Frisch with Bohr. Using biographical sources investigate other student-teacher relationships among illustrious scientists. Do all famous scientists have successful students? Must one have a great teacher to be a great scientist? How important is the student-teacher relationship in terms of your own achievement? **D,R**

2. On his 1939 voyage by ship to the U.S., Niels Bohr had a blackboard in his stateroom. What would you expect a physicist of Bohr's stature to request today if he were to cross the Atlantic? Would a physicist take a ship, a jet, the Concorde—or simply use the Internet? What difference would it make? How would the story of the spread of the news of fission have changed if the discovery had taken place last year? **D**

3. George Gamow, a noted physicist, said that "the fission of the uranium nucleus can be considered a very interesting paragraph (but only a paragraph) in the story of physics." Fission has taken on an importance beyond this because of the technological applications which are derived from its discovery. How has the discovery of fission and its byproducts, nuclear power and the atomic bomb, influenced the way our society views science? **D**

4. Write an essay contrasting science as it would be if society took no interest in its technological applications and science as it is today. If people did not apply science to practical uses, how would its role in our society change? What other field of study would it most resemble? Would governments support scientific research? Would the same type of person pursue science as a career? **I,R**

5. Lise Meitner lived in Germany from 1907. She headed a research unit and became respected as a scientist throughout the world. In the 1930s, she was protected from Nazi persecution in spite of being Jewish because she was still an Austrian citizen. After Hitler took control of Austria, Meitner was no longer protected and, in 1938, was forced to flee Germany. She never again found a job in a major research center. **D,I**

Imagine that you are Meitner and write a letter to a cousin, or to a respected scientist, or to a government official of Germany. In this letter or letters, express your feelings about science, politics, religion and the situation you find yourself in. (Incidentally, such letters from Meitner have been preserved in archives.)

6. Enrico Fermi and Emilio Segrè did not discover uranium fission although fission did indeed occur during their 1934 experiments. Segrè is quoted as saying, "The whole story of our failure is a mystery to me. I keep thinking of a passage from Dante: 'O crucified Jove, do you turn your just eyes away from us or is there here prepared a purpose secret and beyond our comprehension?'" What is Segrè implying by this quote? How might world history have been altered if the discovery of fission occured before the emigration of physicists to the U.S. and well before the start of World War II? What does this suggest about the role of chance in history? **D,I**

7. It is not hard to follow the reasoning Frisch and Meitner used to calculate the energy released in fission. One method they used relied on Einstein's discovery that energy released (E) is equal to a loss of mass (m) times the speed of light (c = 3 X 10^8 meters/sec) squared. **S**

Consider a typical fission reaction:
$^{235}U + {}^{1}n \longrightarrow {}^{140}Xe + {}^{94}Sr + 2\,{}^{1}n$

The Xe rapidly decays into ^{140}Ce, and the Sr into ^{94}Zr, with the emission of electrons of negligible mass. We know now that—
mass of ^{235}U = 235.044 atomic mass units (amu)
mass of ^{1}n = 1.009
mass of ^{140}Ce = 139.905
mass of ^{94}Zr = 93.906

Step 1. Find the sum of the masses of the initial nuclei.
Step 2. Find the sum of the masses of the final nuclei, and subtract from the mass you found in Step 1.
Step 3. Calculate the energy released, using $E = mc^2$. 1 amu = 1.657 X 10^{-24} grams.
(Check: directly in terms of energy, with c^2 already multiplied in, 1 amu = 931 Mev = 1.49 X 10^{-3} ergs.)

8. (This problem uses the results of the preceding exercise.) A typical nuclear reactor generates 1000 Megawatts (10^9 Watts) of thermal energy. If it operates for 100 days, what mass of uranium-235 does it consume? **S**

9. The discovery and exploitation of fission did not require knowledge of the famous equation $E = mc^2$. In fact, at the time the masses of the radioactive daughter nuclei were not known well enough to make a good calculation. Frisch and Meitner calculated the energy release by a second method (which was the only method Joliot used). **C**

Step 1. Note the fission reaction in exercise 7. The radius R of a nucleus is related to its atomic number A by the approximate equation

$R = KA^{1/3}$ *where* $K = 1.07 \times 10^{-15}$ m.

Calculate the radii of the Ce and Zr nuclei.

Step 2. Assume that at the moment the uranium breaks into these fragments, the distance between the centers of the two fragments is equal to the sum of their radii. Calculate the electrostatic force of repulsion between them. (The charge of each nucleus is equal to the number of protons in it.)

Step 3. The kinetic energy of the two fragments after they have moved far apart must be equal to the electrostatic potential energy they have before they separate. The electrostatic potential is equal to the work done in moving the two nuclei in from a great distance to a distance equal to the sum of their radii. An equation for this is derived in your textbook:

$$\text{work} = U = \frac{kq_1q_2}{r}$$

What is the sum of the kinetic energies?
Where did this energy come from?
Compare with the energy calculated using $E = mc^2$. Why are the two numbers not exactly equal?

(*Note for the teacher:* we presume the above equation is derived in the textbook you use. But the symbols may not be the same; modify the exercise if necessary to bring into line with your textbook.
(For the last part of the exercise, we assume that you also assigned exercise 7.)

10. One typical fission reaction is: $^{235}U + {}^{1}n \longrightarrow {}^{144}\mathrm{Ba} + {}^{89}\mathrm{Kr} + 3\,{}^{1}n$ **C**

Using a table of stable nuclides, estimate what is the number of excess neutrons in the Ba and Kr fission products? How many neutrons would you expect to be given off as these nuclides decay, according to your arithmetic?

In fact, after the initial three neutrons, seven beta particles are emitted. The Ba undergoes four successive beta emissions to become ^{144}Nd; the Kr after three beta decays becomes ^{89}Y. Write down the seven disintegration equations for these processes. The various elements that turn up will be part of the "fallout" from a nuclear weapon or the "wastes" of a nuclear reactor. **Extra credit:** look up the half-lives of all these fission products.

11. In the fission reaction of the preceding exercise, the two main fragments repel one another strongly because both are positively charged. Newton's Second Law shows that the smaller fragment will have the larger acceleration. Assuming both nuclei are initially at rest and using conservation of momentum, find the ratio of their velocities and the ratio of their kinetic energies. **C**

In a nuclear reactor, the fragments strike nearby atoms and knock them out of place, gradually damaging the metal that holds the fuel. If you were designing a fuel element for maximum lifetime, would you expect Ba or Kr fragments to be a greater problem? Why? (Remember that a fragment's mass, velocity, and electric charge will all play a role when it hits an atom.)

12. In the late 1930s, scientists knew of three possible outcomes when a nucleus was bombarded with a neutron. **C**

(i) a proton might be emitted,
(ii) an alpha particle might be emitted, or
(iii) a beta particle might be emitted.

(a) Write the nuclear equations showing what would happen in each of these three possible reactions if aluminum was bombarded:

^{27}A1 + ^{1}n —> ?

(b) Write an equation that will show why Fermi, Hahn, and others believed that bombarding uranium with a neutron produces radium.

13. Why is the number of neutrons emitted per fission important for the creation of a chain reaction? **S**

14. ACTIVITY: Size of the nucleus. A nucleus is much too small to measure with ordinary tools, and indirect means are needed. One method is to shoot particles through a thin foil. By calculating the ratio of particles that bounce off a nucleus and those that miss and go through the foil, the cross-section (roughly speaking, the area) of the nuclei can be calculated. An experiment simulating this is suggested by R. D. Edge in *The Physics Teacher* (March 1978): **S**

Take about 40 United States pennies or marbles and scatter them fairly uniformly over a sheet of paper. Drop a pencil, point first, from four or five feet onto the paper, without aiming. Count the shots that hit a penny, keeping track of the total number of shots but neglecting those that miss the paper entirely. Thirty or so shots should be enough. The probability that the pencil hits a penny is proportional to the ratio of the area of all the pennies to the area of the paper, which for an ordinary sheet of 8.5 X 11" paper is 603 cm^2. Let the area of one penny be *A*. Then the area of 40 pennies is 40*A*. If the total number of shots is *N* and the number hitting pennies is *n*, then

$$\frac{n}{N} = \frac{40A}{603}$$

and the area of one penny is $A = (15)\,(n/N)$.

In fact the area of one penny is 2.83 cm^2. How close is your answer? What are the sources of inaccuracy in the answer you got? How could you get a more accurate answer using more or less the same method?

15. ACTIVITY: Radioactive decay. Objective: to determine the half-life of a sample from experimental data. **C**

Half-life experiments are best done with a radioactive sample and a radioactivity detector. But the experiment can be simulated in various straightforward ways.

Method 1: Dice
The student has one hundred dice (or sugar cubes with a dot placed on one side of each with a felt marker), and one hundred beans. The number six on a die (or a dot on a sugar cube) is chosen as the "decay event." Prepare a chart to record the number of each toss, the number of dice, and the number of beans. The cubes are tossed on a table. Each die with a six is removed and a bean is left in its place on the table. Record the "toss number" (for the first toss this is 1), the number of cubes remaining, and the number of beans placed on the table. Repeat for the second toss, and continue until less than ten dice remain.

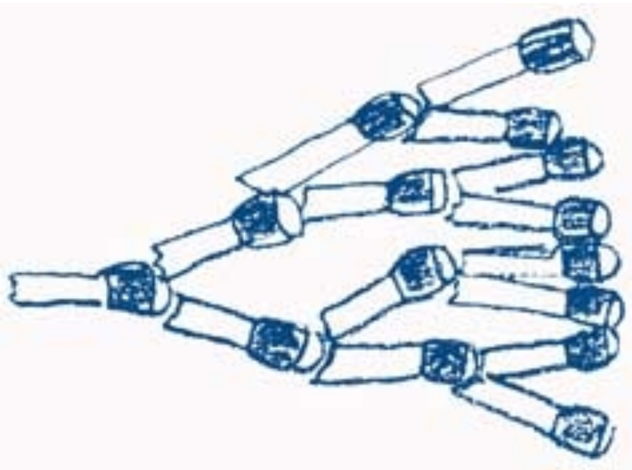
Plot a graph of the number of remaining dice vs. the "toss number." Also plot the number of beans placed on the table. From the graph, determine the half-life of the sample.

Method 2:
If a computer is available, the teacher or student can wite a program to do the tossing. One hundred "X"s are displayed on the screen. The computer assigns a random number to each position, and if that is less than a number chosen to represent decay, the "X" on the screen becomes an "O". A summary chart keeps a tally of the parent nuclei (X) and their daughter nuclei (O) along with a running clock. Students record the number of parent nuclei every 30 seconds. The analysis proceeds as in method 1.

A second set of data is taken where the "X"s are not displayed, and their initial number is 10,000. The graph of this sample is compared with the one with a smaller number of parent nuclei. (Hahn and Strassmann were working with numbers of radioactive nuclei that gave, typically, hundreds of observed decays per hour, and with halflives ranging from minutes to days.)

16. ACTIVITY: Some analogies to the fission chain reaction: **S**

(a) Set up a chain of wooden matches on a fireproof surface in such a way that each match-head lies underneath the wooden end of two other matches. (See diagram; it helps to break the matches so they are very short.)



What happens when the left-most match is lit? How is this like an explosive fission chain reaction?

OR—

(b) Set up a board with thirty mousetraps, each cocked and loaded with masses, for example corks or small rubber balls, which can spring into the air and trigger other mousetraps. What happens when a mass is thrown to trigger a first mousetrap? Try it with a smaller number of mousetraps. Try it with two corks per mousetrap. In what way does this simulate an explosive fission chain reaction?

OR—

(c) Each student is given a ping pong ball (or a piece of tightly crumpled paper). The students are told to throw their ball (or paper) high up in the air if another ball (or paper) falls on their desk. The teacher throws a ball in the general direction of a student. What happens? Try it with two or three balls per student, all to be thrown up together. Try it with fewer students. How does this simulate an explosive fission chain reaction? Try the same exercise with students handing one another the balls rather than throwing them. Now what happens? In this similar to a non-explosive chain reaction, that is, a nuclear pile such as Fermi built?

(*Note for the teacher:* See Richard M. Sutton, "A Mousetrap Atomic Bomb," *American Journal of Physics* **15** (1947), pp. 427-28.)

17. An exercise similar to the following was carried out by Otto Frisch and Rudolf Peierls in 1940. Their answer led them to recommend that the British government attempt to build atomic bombs. The British in turn spurred the U.S. to faster action. **C**

(a) Suppose that you could assemble one kilogram of the fissionable form of uranium (U-235). If it all split at once, how much energy would be released?

(b) Assume that 10% of this energy could be used to move a pile of dirt. How much energy is needed to move one shovelful of dirt to a height of one meter? How many shovelfuls should the uranium have moved? (Make a rough assumption for the mass of a "shovelful".)

(c) If the bomb was used to move dirt and form a hemispherical crater, the dirt at the bottom would have to be moved considerably more than the dirt at the top. Calculate, very approximately, the radius of a crater that could be blasted out by the fission of one kilogram of U-235.

(*Note for the teacher:* We assume you have assigned exercise 7 or done such an exercise in class.

(Fermi made such a calculation already in 1939—estimating the size of a crater a uranium bomb might make in Manhattan. Frisch and Peierls developed more elaborate calculations to find the strength of a shock wave from a bomb. S. Glasstone, *The Effects of Nuclear Weapons*, contains information which can be used to write exercises covering many fields of physics.)

18. At the conclusion of the exhibit, there are two very different reactions to the first controlled release of nuclear energy. Crawford Greenewalt, a young American engineer and executive, was excited by hopes of a better world through atomic energy; Leo Szilard was fearful of the uses of atomic bombs. If you had been there, knowing only what they knew then, what would your feelings have been? How much of such reactions depends upon individual personality, and how much upon the historical experience of successful American engineers, refugee Jews, or contemporary students like yourself? **I**

19. When Niels Bohr was told of fission by Frisch, Bohr exclaimed, "Oh, what idiots we have been that we haven't seen that before. Of course, this is exactly as it must be." This "Aha!" feeling is basic to science. A mental barrier suddenly falls, and what had seemed impossible becomes simple. **D,I**

Consider the following puzzle: "Name a 4 letter word that ends in *-eny*." What is the answer? Why do many people have a hard time finding the answer?

Unscramble the following sets of letters to discover the names of scientists connected with the discovery of fission (for example, **ERFIM** = Fermi). While you do this, keep track of your own mental processes. Write a short essay on the difficulties you run into and the different techniques you use to solve the puzzles. Relate your search for a solution to Bohr's exclamation. Are there similarities between the way scientists work and the way you solve important problems in your life?

ZLIRDAS
NHHA
SARNSTNAMS
SNNIIEET
NOMPCOT
TERMINE

20. A decay by alpha emission is often followed by β decay. Why is it β^- rather than β^+? Specifically, why is there $n \longrightarrow p + e^- + \nu$ rather than $n \longrightarrow p + e^+ + \nu$? **S**

21. Find descriptions of a recent scientific advance in a popular newsstand science magazine and in a more professional journal (*Science, Nature*). One way to do this is to search a popular magazine for an item which the magazine says was "recently reported" in a professional journal. You may also look for items in a newspaper (try the *New York Times Index*) or science news you have seen on television. Discuss the way the news was reported in the different places, with attention to any misrepresentation or sensationalism. Compare different perceptions of the event. **R**

Look for a description of the same event in *Scientific American* and in *Science News*. What role do these magazines play?

If you wished to keep up with science events and you were to devote one hour per week to this, which of the above sources of information would you subscribe to? Can you find similar information on the Web?

22. Watch a television science program, and then search for information on the same subject in some of the magazines listed in the preceding exercise. If you have Internet access, do a Web search too. Which gives you a more complete and accurate understanding of the subject? What different kinds of understanding can you get from different media? Which comes closest to a scientist's form of understanding? **R**

23. The exhibit quoted newspaper articles with sensational claims about the release of vast energy, claims quickly denied by some of the top physicists of the day. Discuss the role of the press and of the scientists in this dialogue. Read carefully Rutherford's statement: was he more or less accurate than the reporters? Were the reporters operating under different concerns than the physicists? Would your response to this question be different if there were no atomic power or atomic weapons today? (In fact fission is a delicately balanced phenomenon, which almost fails to be possible.) **D**

24. Some physicists describe the year 1932 as their "year of wonders." In that year Chadwick discovered the neutron, Urey discovered deuterium, Lawrence and Livingston invented the cyclotron, Cockcroft and Walton experimentally verified that $E = mc^2$ for lithium, and Heisenberg published a paper on the theory of the nucleus. Do library research and write a report on one of the discoveries listed and the people involved with that discovery. **I,R**

25. The year 1932 was also a year of wonders in world politics. What was happening worldwide in 1932? Contrast the general situation with that of physicists. Specifically, what was happening that year to the economy, politics, etc. in the United States, Germany, Italy, Britain, and Japan? How were physicists affected by these events? **I,R**

26. Identify some current scientific-technical developments in their infancy today (genetic engineering, biotechnology, fusion, string theory). Use news magazines, newspapers, the Web, and science magazines. What do you think the future holds for these fields? Specifically, will these fields bring us solutions to some of our problems, or create new problems of their own, or some combination? Discuss this using the remarks of scientists, politicians, and public interest groups. Include a comparison with nuclear physics events of 1932-1942. How is the present the same? How is it different? How successful are predictions of the future likely to be? **I,R**

27. In the exhibit, the chemist Otto Hahn implies that a hierarchy exists within the sciences when he asks his audience, "Are you also afraid of these physicists?" Who is Hahn's audience here? A second example of this implied hierarchy is in a story told by Robert Wilson: "As the director of the Fermi National Laboratory, I was entertaining Professor Bogolyubov, who is director of the Dubna Laboratory. We began to converse, and at a slow moment in the conversation, to keep it going, I traced his career from that of a pure mathematician, to a theoretical physicist, to an administrator, and finally to director of Dubna. I asked him when his fall to a director occurred... and he answered without hesitation: 'Once I had descended from mathematics to theoretical physics, I could do anything!' " **S**

Why is there a hierarchy in science? What other hierarchies exist in our culture? Whom do you respect more than physicists, and whom less? What historical forces or events might lead to a change in the way most people rank physicists?

28. Read the chapter "Departure" by Laura Fermi from her book *Atoms in the Family*. **D,I**

(a) How was the immigration of Fermi to the United States affected because of his status as a scientist? How has immigration policy of the U.S. changed since 1939?

(b) A wife of one of Fermi's associates in Italy said, "Enrico's departure is a betrayal of the young people who have come to study with him and who have trusted in him for guidance." Laura Fermi asks herself the following questions in response. "Of contradicting duties, which should one choose? Should the responsibilities toward one's family or those toward one's students come first?" How would you answer this question?

Suppose instead of "toward one's students," the question read, "toward one's country." Would this change your answer?

29. Consider the sources used in this exhibit. What problems exist in interpreting history from these excerpts (some of which were recorded decades after the event they describe)? How could an oral history be made more valid? What advantages do audio recordings have over the written page? What disadvantages? In your answer, take into account both questions of human memory, and questions of authenticity. **D,I**

(*Note to the teacher:* As mentioned in the acknowledgments on the script, voices have been heavily edited and rearranged. For example, the Anderson excerpts interweave sentences from talks made on two different occasions; the Szilard excerpt has five "cuts" where an interviewer's questions were removed and Szilard's replies were rearranged.)

30. Interview your grandparents or neighbors to find out what recollections they have of this period 1932-1942. Describe what similarities and differences exist between their memories and the audio recording you have just heard. **R**

31. Ernest Rutherford said, "our interest is purely scientific." Interpret what he meant by this and comment on whether it is possible to have scientific interests independent of the rest of the world. Is a scientist responsible for what others do with scientific discoveries? Does a scientist have more responsibility than any other citizen? **D**

32. Leo Szilard says that he feared for the future because he had read H. G. Wells' novel of 1913, *The World Set Free*. Find and read this book (you may need to use inter-library loan), or some other science fiction novel dealing with atomic bombs and written more than thirty years ago, for example: Nevil Shute, *On the Beach*; Pat Frank, *Alas, Babylon*. Compare the author's insights with your knowledge of today. How prophetic are these books? What role do novels like these play in shaping public knowledge and sentiment? **R**

33. Using library sources, find out what happened in 1939-1945 to Frisch, Joliot, Fermi and Bohr. How might their lives, and their work on nuclear physics, have been different if there had never been any likelihood of a Second World War? **R**

34. Read the original article excerpts from (a) Hahn and Strassmann's first "fission" paper; (b) Frisch and Meitner's paper; and (c) Bohr's paper. **I**

Why do Hahn and Strassmann state that they publish their latest experiments hesitantly? Why, at the close of the paper, are they reluctant to state that they have discovered fission? Is the same sort of hesitancy found in the Frisch and Meitner paper?

Why do Hahn and Strassmann show more confidence in their results in their February, 1939 summary?

If you knew nothing about Niels Bohr, could you infer from his paper that he was a highly respected scientist? Give instances of how his paper is different in "personality" from the others.

35. Consider Rutherford's statement that "fundamental things have got to be fairly simple." **D,I**

(a) What does he mean by "simple"? Has science moved toward this goal of simplicity? How?

Do all the problems you have in life have simple solutions? Does "simple" used in this context mean the same as in Rutherford's statement? From what point of view is nuclear fission simple?

(b) Scientists often connect simplicity with beauty. The mathematician and physicist Henri Poincare once wrote:

> *The scientist does not study nature because it is useful to do so. He studies it because it is beautiful. If nature were not beautiful, it would not be worth knowing and life would not be worth living... I mean the intimate beauty which comes from the harmonious order of its parts and which a pure intelligence can grasp...*

The physicist Werner Heisenberg recalled that he once told Einstein:

> *If nature leads us to mathematical forms of great simplicity and beauty... that no one has previously encountered, we cannot help thinking that they are 'true,' that they reveal a genuine feature of nature... You must have felt this too: the almost frightening simplicity and wholeness of the relationships which nature suddenly spreads out before us...*

Is there a viewpoint from which nuclear fission could be called beautiful? Have you ever experienced the type of beauty that the quotes refer to? Describe an event or understanding that made you realize how beautiful something is that people do not easily recognize as beautiful. How could your education be improved so that you can experience some of the beauty that people in special fields of work recognize?

36. Draw a scientist or write a description of a scientist. Compare your image of the scientist with those of other students, and identify stereotypes which many people share. **D,I**

Why do you think people have these stereotypes? Is it worthwhile to have stereotypes? Do stereotypes cause some people to turn toward or away from a career in science? Does an existing stereotype influence what courses you choose to study?

(*Note to the teacher:* After students have made their drawings, they may be helped by considering the following composite portrait reported by Mead and Metraux in *Science* vol.126, p.384-390 (1957). "The scientist is a man, who wears a white coat and works in laboratory. He is elderly or middle aged and wears glasses... he may wear a beard... he is surrounded by equipment: test tubes, bunsen burners, flasks and bottles... he writes neatly in black notebooks... One day he may straighten up and shout: 'I've found it!'... Through his work people will have new and better products... he has to keep dangerous secrets...")

37. Consider how the media present the image of a scientist. Is the presentation different in magazines, novels, movies and cartoons? Do scientists have their own stereotype of what a scientist is like? Is this stereotype different from the one that non-scientists hold? **I,R**

38. There are "visible scientists" in our society—ones that you see on television and read about in magazines, and who always seem to be in the public eye. Identify some of these visible scientists and find out what their field of research has been. Have these visible scientists won the Nobel Prize or other awards given by scientists? How did they become so well known? Do they have a specific cause or are they only identified as scientists? **R**

A larger project for an advanced student or student team can be made from four Web exhibits which all describe "moments of discovery:" nuclear fission, an optical pulsar, the electron, and the transistor. The student(s) should study all four exhibits and discuss similarities and differences -- socially in terms of individuals, scientific institutions, and communication, and scientifically in terms of technologies and thought processes. Students can review the lists of questions in the Teachers' Guides for ideas on directions to follow (perhaps too many!). The students should conclude with general statements about things that seem necessary for all discoveries, at least in modern physical science.

Nuclear Energy Chronology 1896-1945

1896	Becquerel in France discovers unstable (radioactive) atoms.
1897	Thomson in England proves existence of electron.
1903	Rutherford and Soddy in Canada and P. Curie in France discover that radium contains vast stores of energy.
1905	Einstein in Switzerland states equivalence of mass and energy.
1911	Rutherford in England finds that mass of atoms is concentrated in nucleus.
1914-1918	First World War.
1919	Rutherford causes transmutation from one stable chemical element into another, by bombardment with alpha particles.
1929	Start of Great Depression.
1932	Chadwick in England discovers the neutron. Cockcroft and Walton in England produce nuclear transformations by bombardment with artificially accelerated particles.
1933	Hitler seizes power in Germany.
1934	Curie and Joliot in France produce nuclear transformations by alpha-particle bombardment ("artificial radioactivity"). Szilard, in England as a refugee from German racial persecution, envisages a possible nuclear bomb. But most scientists doubt that usable energy could be extracted from the nucleus. Fermi and co-workers in Italy produce nuclear transformations by neutron bombardment. They also produce fission but fail to recognize it.
1935-1937	Hahn, Meitner and Strassmann in Germany investigate products of neutron bombardment of uranium. They assume that these products are transuranian elements (i.e. slightly heavier than uranium).
1938 Aug.	Curie and Savitch in France find a substance seemingly identical to lanthanum among the products of uranium bombarded by neutrons.
Sept.	Munich crisis; Hitler "appeased" with part of Czechoslovakia.

Dec.	Hahn and Strassmann come to the unexpected conclusion that the uranium products include barium and lanthanum, elements half the atomic weight of uranium. They inform Meitner, now a refugee in Sweden.
Christmas	Frisch visits his aunt, Meitner; they interpret the Hahn-Strassmann result as a splitting of the uranium nucleus in two. Hahn, in close touch by letter, comes to the same conclusion.
1939 6 Jan.	Hahn-Strassmann paper published in *Naturwissenschaften*.
6-13 Jan.	Frisch, returning to Bohr's Institute for Theoretical Physics in Copenhagen, discusses fission with Bohr, who leaves with Rosenfeld to visit the U.S. Frisch experimentally verifies the occurrence of fission.
16 Jan.	Bohr arrives in New York; his news of fission is passed to physicists at Columbia University, including Fermi, who has just emigrated from Italy. Issue of *Naturwissenschaften* containing Hahn-Strassmann paper reaches Joliot in Paris.
26 Jan.	Fission verified experimentally by Dunning, Slack and Booth at Columbia and independently by Joliot. Hahn-Strassmann paper reaches U.S. Bohr and Fermi discuss fission in public at physics conference in Washington, D.C. Experiments to verify fission begin at University of California at Berkeley and many other places.
Feb.	Many physicists think fission may possibly be used to release large amounts of energy in a chain reaction. This would be possible only if several neutrons are emitted in each fission (these neutrons could then go on to provoke further fissions).
8 March	Halban, Joliot and Kowarski in Paris complete experiment showing that some neutrons are emitted in fission.
15 March	At Columbia, Fermi, Anderson and Hanstein, and Szilard and Zinn, complete experiments which parallel the French work. German troops seize the free remnant of Czechoslovakia.

April	The Paris team, followed independently by the Columbia group, finds that two or three neutrons are emitted per fission: enough to make a chain reaction possible. Over Szilard's objections both groups publish their findings. American, British, French, German, and Russian scientists all approach their respective governments to seek support for fission research and a watch on uranium supplies.
May-Aug.	Most scientists doubt that a highly explosive chain reaction is possible, but a few are interested in a nuclear reactor as a power source for industry or submarines. Joliot's group conducts reactor experiments; Szilard attempts to raise funds for similar work in U.S.
Sept.	World War II begins in Europe. Bohr and Wheeler publish theory of fission; they show that only the isotope U-235 will fission easily. This is one of the last openly published papers on fission research.
Oct.	Letter on military implications of uranium, drafted by Szilard and signed by Einstein, delivered to President Roosevelt. He sets up an advisory uranium committee.
1940 March-April	Fermi and co-workers, supported by the uranium committee's funds, study the chances of making a reactor. Others at Columbia study ways to separate pure U-235 from natural uranium. Frisch and Peierls, German refugees in England, realize that a devastating nuclear bomb made of pure U-235 is possible. They send a memorandum to the British government, which sets up an advisory committee.
June	Fall of France. Halban and Kowarski join British fission workers and urge construction of a reactor.
1941 Jan.-Feb.	Berkeley scientists discover plutonium, which like U-235 is fissionable. Some scientists recognize that plutonium can be created in a nuclear reactor and then used to build a bomb.
July	British committee recommends that Britain begin a large nuclear bomb project.
Sept.-Oct.	Compton, Lawrence (http://www.aip.org/history/lawrence/) and others, encouraged by the British, urge U.S. government to begin a large bomb project. Roosevelt commits funds.
7 Dec.	Pearl Harbor; U.S. enters war.

1942	German and Japanese empires reach maximum extent at battles of Stalingrad, E1 Alamein, Midway.
2 Dec.	Fermi's group, now in Chicago under Compton's overall leadership, creates a self-sustaining fission chain reaction—the first nuclear reactor.
1943	German fission program restricted to a small scale for lack of resources; small Soviet and Japanese programs underway. British scientists (with French refugees) join forces with U.S. Manhattan Project.
1944	Production of U-235 begins at Oak Ridge, Tennessee. Large reactors built at Hanford, Washington to produce plutonium. Allies invade Europe through Normandy.
1945 March-May	End of war in Europe. Intense fire-bombing destroys nearly all Japanese cities.
16 July	First atomic bomb test, New Mexico.
6 Aug.	U-235 bomb destroys Hiroshima.
9 Aug.	Plutonium bomb destroys Nagasaki.
14 Aug.	Japan surrenders.

Additional Reading and Links

Unless otherwise noted, the level is appropriate for middle-school students and above.

Web Sites

Students and teachers can find a wealth of related materials on the Web. An investigation of what is available may include searches for:

- Names of the scientists involved: Meitner, Hahn, Strassman, Bohr, etc.
- Nobel speeches of Fermi, Compton, Einstein, Bohr, Hahn, Curie, etc.
- Key words: Nuclear fission, transmutation

Alsos History of Fission
http://alsos.wlu.edu/overview.html

An excellent scientific review, mainly 1932-1945

Nuclear Chemistry and the Discovery of Fission
http://www.chemcases.com/nuclear/nc-03.htm
The story of the 1938 work, especially the chemical side. Site includes other nuclear chemistry (neutron, plutonium, etc.) and an instructor's guide.

Trinity Nuclear Weapons History
http://nuketesting.enviroweb.org/
Online archive of documents, mainly from 1945 forward

Los Alamos Lab History
http://www.lanl.gov/external/welcome/history.html

Fission Movie *(QuickTime animation)*
http://www.pbs.org/wgbh/pages/frontline/shows/reaction/interact/fiss1.html

Federation of American Scientists
http://www.fas.org/
Founded by atomic scientists in 1946, the FAS provides a gateway to current nuclear arms issues.

Alsos Digital Library for Nuclear Issues
http://alsos.wlu.edu/
Large annotated list of books and other materials, mainly on the Manhattan Project.

History of Physics Syllabi
http://www.aip.org/history/syllabi/
Includes reading lists for courses on 20th-century physics and nuclear affairs.

More History of Physics Exhibits
http://www.aip.org/history/exhibit.htm
Award-winning exhibits on Einstein, M. Curie, W. Heisenberg, E.O. Lawrence, and other nuclear pioneers, and a comprehensive set of links.

Readings

Many of these are out of print, but your local library may be able to get them through inter-library loan.

- **Anderson, David L.** *Discoveries in Physics: Supplemental Unit B of Project Physics Course.* New York: Holt, Rinehart and Winston, 1973.

Chapter 3 reviews the discovery of fission at the high school level (a good companion to the voices in the exhibit). The book's prologue and epilogue discuss different models for scientific discovery.

- **Badash, Lawrence.** *Scientists and the Development of Nuclear Weapons : from Fission to the Limited Test Ban Treaty, 1939-1963.* Atlantic Highlands, N.J. : Humanities Press, 1995.

A fine compact account at the advanced high school - college level.

- **Fermi, Laura.** *Atoms in the Family: My Life with Enrico Fermi.* Chicago: University of Chicago Press, 1954 (reissued by American Institute of Physics, 1987).

A biography of Fermi by his wife, with excellent accounts of the personal accounts of his work and insights on the lives of other physicists.

- **Graetzer, H.G., and D. L. Anderson.** *The Discovery of Nuclear Fission.* New York: Van Nostrand-Reinhold, 1971.

A complete historical account using excerpts from original scientific papers. For advanced high school students and above.

- **Hahn, Otto.** *Otto Hahn: A Scientific Autobiography.* New York: Scribner's, 1966.

Includes an informative account of Hahn's part in the discovery of fission.

- **Kragh, Helge.** *Quantum Generations: A History of Physics in the Twentieth Century.* Princeton, NJ: Princeton University Press, 1999.

A solid and readable survey at the high school - college level by a historian of science.

- **Rhodes, Richard.** *The Making of the Atomic Bomb.* New York : Simon & Schuster, 1986.

The best popular history from the 1930s through the Manhattan Project, well-written but long.

- **Segrè, Emilio.** *From X-rays to Quarks : Modern Physicists and Their Discoveries.* San Francisco: W. H. Freeman, 1980.

Popular history of 20th-century physics, by a Nobelist from Fermi's group.

- **Shea, William R., ed.** *Otto Hahn and the Rise of Nuclear Physics.* Dordrecht: D. Reidel, 1983

Detailed scholarly articles on the history of Hahn's work and fission.

- **Smyth, H. D.** *A General Account of the Development of Methods of Using Atomic Energy for Military Purposes.* Washington, DC: Superintendent of Documents, 1945

The official account of the work on the atomic bomb, including popular-level descriptions of physical problems as well as project administration.

Reprinted Articles

O. Hahn and F. Strassman, "Concerning the Existence of Alkaline Earth Metals Resulting from Neutron Irradiation of Uranium," *Naturwissenschaften* vol.27, p. 11 (Jan. 1939), summary, translated by H. Graetzer in *The Discovery of Nuclear Fission* (N.Y.: Van Nostrand Reinhold, 1971), p. 44-47.

"Concerning the Existence of Alkaline Earth Metals Resulting from Neutron Irradiation of Uranium"

O. HAHN AND F. STRASSMANN

Naturwissenschaften, Volume **27,** p. 11 (January 1939)

In a recent preliminary article in this journal[1] it was reported that when uranium is irradiated by neutrons, there are several new radioosotopes produced, other than the transuranic elements—from 93 to 96—previously described by Meitner, Hahn, and Strassmann. These new radioactive products are apparently due to the decay of ^{239}U by the successive emission of two alpha particles. By this process the element with a nuclear charge of 92 must decay to a nuclear charge of 88; that is, to radium. In the previously mentioned article a tentative decay scheme was proposed. The three radium isotopes, with their approximate half-lives given, decayed to actinium, which in turn decayed to thorium isotopes.

A rather unexpected observation was pointed out, namely that these radium isotopes, which are produced by alpha emission and which in turn decay to thorium, are obtained not only with fast but also with slow neutrons.

The evidence that these three new parent isomers are actually radium was that they can be separated together with barium salts, and that they have all the chemical reactions which are characteristic of the element barium. All the other known elements, from the transuranic ones down through uranium, protactinium, thorium, and actinium have different chemical properties than barium and are easily separated from it. The same thing holds true for the elements below radium, that is, bismuth, lead, polonium, and ekacesium (now called francium). Therefore, radium is the only possibility, if one eliminates barium itself. . . .

When uranium is bombarded with slow neutrons, it is not easy to understand from energy considerations how radium isotopes can be produced. Therefore, a very careful determination of the chemical properties of the new artificially made radioelements was necessary. Various analytic groups of elements were separated from a solution containing the irradiated uranium. Besides the large group of transuranic elements, some radioactivity was always found in the alkaline-earth group (barium carrier), the rare-earth group (lanthanum carrier), and also with elements in group IV of the periodic table (zirconium carrier). The barium precipitate was the first to be investigated more thoroughly, since it apparently contains the parent isotopes of the observed isomeric series. The goal was to show that the transuranic elements, and also U, Pa, Th, and Ac, could always be separated easily and completely from the activity which precipitates with barium. . . .

To summarize our results, we have identified three alkaline earth metals which are designated as Ra II, Ra III, and Ra IV. Their half-lives are 14 ± 2 min, 86 ± 6 min, and 250–300 h. It should be noted that the 14-min activity was not designated as Ra I nor the other isomers as Ra II and Ra III. The reason is that we believe there is an even more unstable "Ra" isotope, although it has not been possible to observe it so far. . . .

The decay scheme which was given in our previous article must now be corrected. The following scheme takes into account the needed changes, and also gives the more accurately determined half-lives for the parent of each series:

$$\text{“Ra I”?} \xrightarrow[<1\ \text{min}]{\beta} \text{Ac I} \xrightarrow[<30\ \text{min}]{\beta} \text{Th?}$$

$$\text{“Ra II”} \xrightarrow[14 \pm 2\ \text{min}]{\beta} \text{Ac II} \xrightarrow[2.5\ \text{h}]{\beta} \text{Th?}$$

$$\text{“Ra III”} \xrightarrow[86 \pm 6\ \text{min}]{\beta} \text{Ac III} \xrightarrow[\text{several days?}]{\beta} \text{Th?}$$

$$\text{“Ra IV”} \xrightarrow[250\text{–}300\ \text{h}]{\beta} \text{Ac IV} \xrightarrow[<40\ \text{h}]{\beta} \text{Th?}$$

The large group of transuranic elements so far bears no known relation to these isomeric series.

The four decay series listed above can be regarded as doubtlessly correct in their genetic relationship. We have already been able to verify some of the "thorium" end products of the isomeric series. However, since the half-lives have not been determined with any accuracy yet, we have decided to refrain altogether from reporting them at the present time.

Now we still have to discuss some newer experiments, which we publish rather hesitantly due to their peculiar results. We wanted to identify beyond any doubt the chemical properties of the parent members of the radioactive series which were separated with the barium and which have been designated as "radium isotopes." We have carried out fractional crystallizations and fractional precipitations, a method which is well-known for concentrating (or diluting) radium in barium salt solutions.

Barium bromide increases the radium concentration greatly in a fractional crystallization process and barium chromate even more so when the crystals are allowed to form slowly. Barium chloride increases the concentration less than the bromide, and barium carbonate decreases it slightly. When we made appropriate tests with radioactive barium samples which were free of any later decay products, the results were always negative. The activity was distributed evenly among all the barium fractions, at least to the extent that we could determine it within an appreciable experimental error. . . .

Next the "indicator (i.e., tracer) method" was applied to a mixture of purified long-lived "Ra IV" and pure $MsTh_1$; this mixture with barium bromide as a carrier was subjected to fractional crystallization. The concentration of $MsTh_1$ was increased, and the concentration of "Ra IV" was not, but rather its activity remained the same for fractions having an equivalent barium content. We come to the conclusion that our "radium isotopes" have the properties of barium. As chemists we should actually state that the new products are not radium, but rather barium itself. Other elements besides radium or barium are out of the question.

Finally we have made a tracer experiment with our pure separated "Ac II" (half-life about 2.5 h) and the pure actinium isotope $MsTh_2$. If our "Ra isotopes" are not radium, then the "Ac isotopes" are not actinium either, but rather should be lanthanum. Using the technique of Curie,[2] we carried out a fractionation of lanthanum oxalate, which contained both of the active substances, in a nitric acid solution. Just as Mme. Curie reported, the $MsTh_2$ became greatly concentrated in the end fractions. With our "Ac II" there was no observable increase in concentration at the end. We agree with the findings of Curie and Savitch[3] for their 3.5-h activity (which was however not just a single species) that the product resulting from the beta decay of our radioactive alkaline earth metal is not actinium. . . .

The "transuranic group" of elements are chemically related but not identical to their lower homologs, rhenium, osmium, iridium, and platinum. Experiments have not been made yet to see if they might be chemically identical with the even lower homologs, technetium, ruthenium, rhodium, and palladium. After all one could not even consider this as a possibility earlier. The sum of the mass numbers of barium + technetium, 138 +101, gives 239!

As chemists we really ought to revise the decay scheme given above and insert the symbols Ba, La, Ce, in place of Ra, Ac, Th. However, as "nuclear chemists," working very close to the field of physics, we cannot bring ourselves yet to take such a drastic step which goes against all previous experience in nuclear physics. There could perhaps be a series of unusual coincidences which has given us false indications.

It is intended to carry out further tracer experiments with the new radioactive decay products.

REFERENCES

[1] O. Hahn and F. Strassmann, *Naturwiss.* **26,** 756 (1938).
[2] Mme. Pierre Curie, *J. Chim. Phys.* **27,** 1 (1930).
[3] I. Curie and P. Savitch, *Compt. Rend.* **206,** 1643 (1938).

Although the preceding article by Hahn and Strassmann does represent the "moment of discovery" of nuclear fission, it is interesting to add a brief postscript from a publication which followed it four weeks later. Whereas the tone of the previous article was tentative, uncertain, and hesitant, the next one is completely conclusive and confident. It is likely that they had some personal communication with Miss Meitner during the interval to settle their qualms about taking "such a drastic step."

One other interesting point arises in the following article. It should be recalled that neutron irradiation of uranium produced some 16 radioactive species, 10 of them supposedly transuranic, and 6 of them being radium or actinium. Even though the radium-actinium family had been conclusively shown now to be fission products, Hahn and Strassmann still believed that the transuranium group was correctly placed (see item 5 in the following summary). They were not ready yet to accept the idea of fission in its full sweep. Like good, conservative scientists, they were unwilling to cross off the transuranic series until new and positive identifications were made, which took four more months.

["Verification of the Creation of Radioactive Barium Isotopes from Uranium and Thorium by Neutron Irradiation; Identification of Additional Radioactive Fragments from Uranium Fission"](#) **. O. Hahn and F. Strassman. Naturwissenschaften, February 10, 1939, volume 27, p. 89-95. Translated by H. Graetzer.**

"Verification of the Creation of Radioactive Barium Isotopes from Uranium and Thorium by Neutron Irradiation; Identification of Additional Radioactive Fragments from Uranium Fission"

O. HAHN AND F. STRASSMANN

Naturwissenschaften, Volume **27**, pp. 89–95
(10 February 1939)
translated by H. Graetzer

. . . Summary:

1. The creation of barium isotopes from uranium was conclusively demonstrated.
2. For thorium, the formation of barium isotopes was also established.
3. Some suggestions are made regarding the atomic weights of the barium isotopes.
4. Evidently, some of the barium isotopes produced from thorium and uranium are identical.
5. It is our belief that the "transuranic elements" still retain their placement without change, as previously described.
6. A second group of fission fragments, Strontium [element 38] and Yttrium [element 39], was determined.
7. By an appropriate experimental arrangement, the formation of a noble gas was established, which in turn decays into an alkali metal. It has not been possible yet to show if the substances in question are xenon-cesium or krypton-rubidium.

In a rather short time it has been possible to identify numerous new reaction products described above—with considerable certainty, we believe—only because of the previous experience we had gathered, in association with L. Meitner, from the systematic study of uranium and thorium reaction products.

"Concerning the Existence of Alkaline Earth Metals Resulting from Neutron Irradiation of Uranium"

O. HAHN AND F. STRASSMANN

Naturwissenschaften, Volume **27**, p. 11 (January 1939)
translated by H. Graetzer

In a recent preliminary article in this journal[1] it was reported that when uranium is irradiated by neutrons, there are several new radioisotopes produced, other than the transuranic elements—from 93 to 96—previously described by Meitner, Hahn, and Strassmann. These new radioactive products are apparently due to the decay of ^{239}U by the successive emission of two alpha particles. By this process the element with a nuclear charge of 92 must decay to a nuclear charge of 88; that is, to radium. In the previously mentioned article a tentative decay scheme was proposed. The three radium isotopes, with their approximate half-lives given, decayed to actinium, which in turn decayed to thorium isotopes.

A rather unexpected observation was pointed out, namely that these radium isotopes, which are produced by alpha emission and which in turn decay to thorium, are obtained not only with fast but also with slow neutrons.

The evidence that these three new parent isomers are actually radium was that they can be separated together with barium salts, and that they have all the chemical reactions which are characteristic of the element barium. All the other known elements, from the transuranic ones down through uranium, protactinium, thorium, and actinium have different chemical properties than barium and are easily separated from it. The same thing holds true for the elements below radium, that is, bismuth, lead, polonium, and ekacesium (now called francium). Therefore, radium is the only possibility, if one eliminates barium itself. . . .

When uranium is bombarded with slow neutrons, it is not easy to understand from energy considerations how radium isotopes can be produced. Therefore, a very careful determination of the chemical properties of the new artificially made radioelements was necessary. Various analytic groups of elements were separated from a solution containing the irradiated uranium. Besides the large group of transuranic elements, some radioactivity was always found in the alkaline-earth group (barium carrier), the rare-earth group (lanthanum carrier), and also with elements in group IV of the periodic table (zirconium carrier). The barium precipitate was the first to be investigated more thoroughly, since it apparently contains the parent isotopes of the observed isomeric series. The goal was to show that the transuranic elements, and also U, Pa, Th, and Ac, could always be separated easily and completely from the activity which precipitates with barium. . . .

To summarize our results, we have identified three alkaline earth metals which are designated as Ra II, Ra III, and Ra IV. Their half-lives are 14 ± 2 min, 86 ± 6 min, and 250–300 h. It should be noted that the 14-min activity was not designated as Ra I nor the other isomers as Ra II and Ra III. The reason is that we believe there is an even more unstable "Ra" isotope, although it has not been possible to observe it so far. . . .

The decay scheme which was given in our previous article must now be corrected. The following scheme takes into account the needed changes, and also gives the more accurately determined half-lives for the parent of each series:

$$\text{“Ra I”?} \xrightarrow[<1\text{ min}]{\beta} \text{Ac I} \xrightarrow[<30\text{ min}]{\beta} \text{Th?}$$

$$\text{“Ra II”} \xrightarrow[14 \pm 2\text{ min}]{\beta} \text{Ac II} \xrightarrow[2.5\text{h}]{\beta} \text{Th?}$$

$$\text{“Ra III”} \xrightarrow[86 \pm 6\text{ min}]{\beta} \text{Ac III} \xrightarrow[\text{several days?}]{\beta} \text{Th?}$$

$$\text{“Ra IV”} \xrightarrow[250\text{-}300\text{ h}]{\beta} \text{Ac IV} \xrightarrow[<40\text{ h}]{\beta} \text{Th?}$$

The large group of transuranic elements so far bears no known relation to these isomeric series.

The four decay series listed above can be regarded as doubtlessly correct in their genetic relationship. We have already been able to verify some of the "thorium" end products of the isomeric series. However, since the half-lives have not been determined with any accuracy yet, we have decided to refrain altogether from reporting them at the present time.

Now we still have to discuss some newer experiments, which we publish rather hesitantly due to their peculiar results. We wanted to identify beyond any doubt the chemical properties of the parent members of the radioactive series which were separated with the barium and which have been designated as "radium isotopes." We have carried out fractional crystallizations and fractional precipitations, a method which is well-known for concentrating (or diluting) radium in barium salt solutions.

Barium bromide increases the radium concentration greatly in a fractional crystallization process and barium chromate even more so when the crystals are allowed to form slowly. Barium chloride increases the concentration less than the bromide, and barium carbonate decreases it slightly. When we made appropriate tests with radioactive barium samples which were free of any later decay products, the results were always negative. The activity was distributed evenly among all the barium fractions, at least to the extent that we could determine it within an appreciable experimental error. . . .

Next the "indicator (i.e., tracer) method" was applied to a mixture of purified long-lived "Ra IV" and pure $MsTh_1$; this mixture with barium bromide as a carrier was subjected to fractional crystallization. The concentration of $MsTh_1$ was increased, and the concentration of "Ra IV" was not, but rather its activity remained the same for fractions having an equivalent barium content. We come to the conclusion that our "radium isotopes" have the properties of barium. As chemists we should actually state that the new products are not radium, but rather barium itself. Other elements besides radium or barium are out of the question.

Finally we have made a tracer experiment with our pure separated "Ac II" (half-life about 2.5 h) and the pure actinium isotope $MsTh_2$. If our "Ra isotopes" are not radium, then the "Ac isotopes" are not actinium either, but rather should be lanthanum. Using the technique of Curie,[2] we carried out a fractionation of lanthanum oxalate, which contained both of the active substances, in a nitric acid solution. Just as Mme. Curie reported, the $MsTh_2$ became greatly concentrated in the end fractions. With our "Ac II" there was no observable increase in concentration at the end. We agree with the findings of Curie and Savitch[3] for their 3.5-h activity (which was however not just a single species) that the product resulting from the beta decay of our radioactive alkaline earth metal is not actinium. . . .

The "transuranic group" of elements are chemically related but not identical to their lower homologs, rhenium, osmium, iridium, and platinum. Experiments have not been made yet to see if they might be chemically identical with the even lower homologs, technetium, ruthenium, rhodium, and palladium. After all one could not even consider this as a possibility earlier. The sum of the mass numbers of barium + technetium, 138 + 101, gives 239!

As chemists we really ought to revise the decay scheme given above and insert the symbols Ba, La, Ce, in place of Ra, Ac, Th. However, as "nuclear chemists," working very close to the field of physics, we cannot bring ourselves yet to take such a drastic step which goes against all previous experience in nuclear physics. There could perhaps be a series of unusual coincidences which has given us false indications.

It is intended to carry out further tracer experiments with the new radioactive decay products.

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"Disintegration of Uranium by Neutrons: a New Type of Nuclear Reaction"**. Lise Meitner, and Otto Frisch. Nature, Feb. 11, 1939, volume 143, p. 239.**

Disintegration of Uranium by Neutrons: a New Type of Nuclear Reaction

Lise Meitner and O.R. Frisch
Nature, **143,** 239-240, (Feb. 11, 1939)

(Taken from http://dbhs.wvusd.k12.ca.us/Chem-History/Meitner-Fission-1939.html)

On bombarding uranium with neutrons, Fermi and collaborators[1] found that at least four radioactive substances were produced, to two of which atomic numbers larger than 92 were ascribed. Further investigations[2] demonstrated the existence of at least nine radioactive periods, six of which were assigned to elements beyond uranium, and nuclear isomerism had to be assumed in order to account for their chemical behavior together with their genetic relations.

In making chemical assignments, it was always assumed that these radioactive bodies had atomic numbers near that of the element bombarded, since only particles with one or two charges were known to be emitted from nuclei. A body, for example, with similar properties to those of osmium was assumed to be eka-osmium (Z = 94) rather than osmium (z = 76) or ruthenium (z = 44).

Following up an observation of Curie and Savitch[3], Hahn and Strassmann[4] found that a group of at least three radioactive bodies, formed from uranium under neutron bombardment, were chemically similar to barium and, therefore, presumably isotopic with radium. Further investigation[5], however showed that it was impossible to separate those bodies from barium (although mesothorium, an isotope of radium, was readily separated in the same experiment), so that Hahn and Strassmann were forced to conclude that *isotopes of barium* (Z = 56) *are formed as a consequence of the bombardment of uranium* (Z = 92) *with neutrons.*

At first sight, this result seems very hard to understand. The formation of elements much below uranium has been considered before, but was always rejected for physical reasons, so long as the chemical evidence was not entirely clear cut. The emission, within a short time, of a large number of charged particles may be regarded as excluded by the small penetrability of the 'Coulomb barrier', indicated by Gamov's theory of alpha decay.

On the basis, however, of present ideas about the behaviour of heavy nuclei[6], an entirely different and essentially classical picture of these new disintegration processes suggests itself. On account of their close packing and strong energy exchange, the particles in a heavy nucleus would be expected to move in a collective way which has some resemblance to the movement of a liquid drop. If the movement is made sufficiently violent by adding energy, such a drop may divide itself into two smaller drops.

In the discussion of the energies involved in the deformation of nuclei, the concept of surface tension has been used[7] and its value has been estimated from simple considerations regarding nuclear forces. It must be remembered, however, that the surface tension of a charged droplet is diminished by its charge, and a rough estimate shows that the surface tension of nuclei, decreasing with increasing nuclear charge, may become zero for atomic numbers of the order of 100.

It seems therefore possible that the uranium nucleus has only small stability of form, and may, after neutron capture, divide itself into two nuclei of roughly equal size (the precise ratio of sizes depending on finer structural features and perhaps partly on chance). These two nuclei will repel each other and should gain a total kinetic energy of c. 200 Mev., as calculated from nuclear radius and charge. This amount of energy may actually be expected to be available from the difference in packing fraction between uranium and the elements in the middle of the periodic system. The whole 'fission' process can thus be described in an essentially classical way, without having to consider quantum-mechanical 'tunnel effects', which would actually be extremely small, on account of the large masses involved.

After division, the high neutron/proton ratio of uranium will tend to readjust itself by beta decay to the lower value suitable for lighter elements. Probably each part will thus give rise to a chain of disintegrations. If one of the parts is an isotope of barium[8], the other will be krypton (Z = 92 - 56), which might decay through rubidium, strontium and yttrium to zirconium. Perhaps one or two of the supposed barium-lanthanum-cerium chains are then actually strontium-yttrium-zirconium chains.

It is possible[8], and seems to us rather probable, that the periods which have been ascribed to elements beyond uranium are also due to light elements. From the chemical evidence, the two short periods (10 sec. and 40 sec.) so far ascribed to ^{239}U might be masurium isotopes (Z = 43) decaying through ruthenium, rhodium, palladium and silver into cadmium.

In all these cases it might not be necessary to assume nuclear isomersim; but the different radioactive periods belonging to the same chemical element may then be attributed to different isotopes of this element, since varying proportions of neutrons may be given to the two parts of the uranium nucleus.

By bombarding thorium with neutrons, activities are which have been ascribed to radium and actinium isotopes[8]. Some of these periods are approximately equal to periods of barium and lanthanum isotopes resulting from the bombardment of uranium. We should therefore like to suggest that these periods are due to a 'fission' of thorium which is like that of uranium and results partly in the same products. Of course, it would be especially interesting if one could obtain one of those products from a light element, for example, by means of neutron capture.

It might be mentioned that the body with the half-life 24 min[2] which was chemically identified with uranium is probably really ^{239}U and goes over into eka-rhenium which appears inactive but may decay slowly, probably with emission of alpha particles. (From inspection of the natural radioactive elements, ^{239}U cannot be expected to give more than one or two beta decays; the long chain of observed decays has always puzzled us.) The formation of this body is a typical resonance process[9]; the compound state must have a life-time of a million times longer than the time it would take the nucleus to divide itself. Perhaps this state corresponds to some highly symmetrical type of motion of nuclear matter which does not favor 'fission' of the nucleus.

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"Disintegration of Heavy Nuclei". Niels Bohr. Nature, Feb. 25, 1939, volume 143, p. 330.

Letters to the Editor

The Editor does not hold himself responsible for opinions expressed by his correspondents. He cannot undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.*

NOTES ON POINTS IN SOME OF THIS WEEK'S LETTERS APPEAR ON P. 337.

CORRESPONDENTS ARE INVITED TO ATTACH SIMILAR SUMMARIES TO THEIR COMMUNICATIONS.

Disintegration of Heavy Nuclei

THROUGH the kindness of the authors I have been informed of the content of the letters[1] recently sent to the Editor of NATURE by Prof. Meitner and Dr. Frisch. In the first letter, these authors propose an interpretation of the remarkable findings of Hahn and Strassmann as indication for a new type of disintegration of heavy nuclei, consisting in a fission of the nucleus into two parts of approximately equal masses and charges with release of enormous energy. In the second letter, Dr. Frisch describes experiments in which these parts are directly detected by the very large ionization they produce. Due to the extreme importance of this discovery, I should be glad to add a few comments on the mechanism of the fission process from the point of view of the general ideas, developed in recent years, to account for the main features of the nuclear reactions hitherto observed.

According to these ideas, any nuclear reaction initiated by collisions or radiation involves as an intermediate stage the formation of a compound nucleus in which the excitation energy is distributed among the various degrees of freedom in a way resembling the thermal agitation of a solid or liquid body. The relative probabilities of the different possible courses of the reaction will therefore depend on the facility with which this energy is either released as radiation or converted into a form suited to produce the disintegration of the compound nucleus. In the case of ordinary reactions, in which the disintegration consists in the escape of a single particle, this conversion means the concentration of a large part of the energy on some particle at the surface of the nucleus, and resembles therefore the evaporation of a molecule from a liquid drop. In the case of disintegrations comparable to the division of such a drop into two droplets, it is evidently necessary, however, that the quasi-thermal distribution of energy be largely converted into some special mode of vibration of the compound nucleus involving a considerable deformation of the nuclear surface.

In both cases, the course of the disintegration may thus be said to result from a fluctuation in the statistical distribution of the energy between the various degrees of freedom of the system, the probability of occurrence of which is essentially determined by the amount of energy to be concentrated on the particular type of motion considered and by the 'temperature' corresponding to the nuclear excitation. Since the effective cross-sections for the fission phenomena for neutrons of different velocities seem to be of about the same order of magnitude as the cross-sections for ordinary nuclear reactions, we may therefore conclude that for the heaviest nuclei the deformation energy sufficient for the fission is of the same order of magnitude as the energy necessary for the escape of a single nuclear particle. For somewhat lighter nuclei, however, where only evaporation-like disintegrations have so far been observed, the former energy should be considerably larger than the binding energy of a particle.

These circumstances find their straightforward explanation in the fact, stressed by Meitner and Frisch, that the mutual repulsion between the electric charges in a nucleus will for highly charged nuclei counteract to a large extent the effect of the short-range forces between the nuclear particles in opposing a deformation of the nucleus. The nuclear problem concerned reminds us indeed in several ways of the question of the stability of a charged liquid drop, and in particular, any deformation of a nucleus, sufficiently large for its fission, may be treated approximately as a classical mechanical problem, since the corresponding amplitude must evidently be large compared with the quantum mechanical zero-point oscillations. Just this condition would in fact seem to provide an understanding of the remarkable stability of heavy nuclei in their normal state or in the states of low excitation, in spite of the large amount of energy which would be liberated by an imaginable division of such nuclei.

The continuation of the experiments on the new type of nuclear disintegrations, and above all the closer examination of the conditions for their occurrence, should certainly yield most valuable information as regards the mechanism of nuclear excitation.

N. BOHR.

At the Institute for Advanced Study,
Princeton, N.J. Jan. 20.

[1] [NATURE, 143, 239 and 275 (1939)].

"Departure," a chapter from *Atoms in the Family: My Life with Enrico Fermi*, by Laura Fermi. Chicago: University of Chicago Press, 1954.

(14)

DEPARTURE

On December 6, 1938, we left Rome with our two children and their nursemaid. The journey to Stockholm was as comfortable as a train journey can be with two weary children who were soon to be eight and three years old, respectively, and who could be pleased by neither their toys nor their books.

Except for a slight incident at the German border, the only other memorable diversion to the rhythmic monotony of the train was the crossing of the rough Baltic Sea on a ferry and the shattering clangor of a pile of dishes that tumbled down from a dining-room table as the ferry hit an unusually violent wave.

The incident at the German border was so slight that it was only a brief moment of anxiety, not worth relating but for the fact that it reflected the strain and tension of our last month in Rome. We had been under the constant fear, common to all who plan to leave a country under difficult political circumstances, that we might not succeed in carrying out our projects. Something, we thought, might come up between our plans and their completion, a specific act of the government against us, a new law, the sudden closing of international frontiers, or the outbreak of war.

Enrico had never admitted he was worried. In our family his was the role of reassuring, of never having a worry or a doubt. When the order came for all Jews to surrender their passports and have their race recorded on them, I had been frightened. I foresaw a lengthy delay in the best of cases, Enrico leaving without me in the worst. But Enrico had preserved his calm and professed his confidence that everything would be well in the end, that with the help of an influential friend we would overcome this difficulty, as we had overcome others. He was right as always: within two days I had my passport back, and no record of race was on it.

Despite his reassurance and the fact that he restated it several times for no apparent reason during the first part of our journey, when the Italian guards at the Brenner Pass had examined our passports and returned them to us with no comments, Enrico had seemed relieved.

Then it was a German guard's turn to inspect our passports. He was standing in the corridor outside the door of our bedroom, stiff and official, a personification of our past and present anxieties. He turned our passports in his hands searchingly and appeared unsatisfied. Enrico rose from his seat and stood in the corridor, waiting, his thin lips so tightly pressed together that they had disappeared inside his mouth. The moment unfolded with unbearable slowness. Nella, always sensitive to our moods, became restless. Why, she wanted to know, was that gentleman taking so long with our passports? Why did he turn all the pages over and over again? Did I think something was wrong? Would the man send us back to Rome and to Mussolini?

"Be quiet, Nella. Everything is all right!"

Everything *had* to be all right. Once I had accepted the decision to leave Italy, I felt as if I had always wanted to leave; as if all my yearnings and expectations had built up over the years in one direction: America; as if failure to continue our journey now would offset a lifelong dream.

Enrico spoke to the guard in German. Was anything the matter? Had we obtained a visa from the German Konsulat? the guard asked. He did not seem to find it in our passports. When Enrico turned the pages and pointed out the visa, the stiffness vanished from his muscles, his thin lips were visible once more. The German guard saluted and smiled. Germans and Italians were good friends, were they not?

"Now, Nella, you can go back to sleep. Nothing was wrong. Now climb into the berth with Giulio and take care not to wake him up. Soon the train will start moving and will rock you."

Soon the train was in motion, forward, away from Italy.

The parting from friends and relatives had not been so hard as I had expected. I had told so often the "official version" of our trip to America—that Enrico was to teach six months at Columbia University in New York and that at the end of his engagement we would return to Rome—that I had come to believe in it. Besides, I repeated to myself, even if we were to settle in the United States, I could always come back for a visit. What could prevent me? The racial laws? There was nothing in them against my traveling. A war? Why doubt Hitler's sincerity when at Munich he had declared he had no further territorial ambitions? Why heed the pessimists rather than the optimists?

I had closed my eyes to the evident. The war was to come, and all hopes of Italy's last-minute shift in alliance were to vanish. Fascism had united with naziism, and Italy was little more than a province of Germany. Still fascism resisted total nazification. It preserved its identity in part. What was left of its individuality made it vulnerable, a designated victim of naziism. The German occupation was to result in tragedy for most Italians and in a more urgent, immediate tragedy for the Italian Jews. Some fled to hide in the Italian mountains, and some crossed the Alps on foot, into the relative security of Swiss concentration camps. They were guided by smugglers who knew the unguarded passes, who helped them carry bundles and babies, while little children who could stand on their feet had to walk endless hours. Some changed their names and lived in disguise and constant fear; and a large number, mostly the old who had felt protected by their age, were rounded up by the Germans and deported to labor camps and gas chambers.

This was to happen five years later, and, luckily for me, I had no premonition of these events at the time of our departure. My worries were mixed with a certain spirit of adventure, and a good part of my mind was absorbed in the cares of the moment.

A few people knew that we were to settle in the United States, among them the Amaldis and Rasetti. They came to say goodbye at the station in Rome, and with them we paced the platform along the train, while Nella and Giulio took possession of our two bedrooms first, scattering their toys around under their nurse's vigilant eye, and then they flattened their noses against the window to look at us.

This parting had a significance that none of us wished to put into words. It was the formal ending of a co-operation that had started almost twelve years previously. The group had dwindled down. After Segré had gone to Palermo in 1936 and while Rasetti was in the United States for a long visit, co-operation had been limited to Enrico and Edoardo Amaldi. The bulk of the experimental and theoretical work to interpret artificial radioactivity and behavior of slow neutrons was carried on by the two of them alone. But so long as an active nucleus of the group remained, there was always a possibility of restoring it to its former strength. Not so now. Emilio Segré, who had gone to the United States for the summer session at the University of California in Berkeley, had watched the trend of events in Italy. He had decided not to come back. His wife and year-old son had joined him.

Franco Rasetti was quietly looking for a position outside Europe. He was to leave Italy in July, 1939, and become professor of physics at Laval University in Quebec. Of the old group, only Edoardo Amaldi planned to stay on in Rome. The responsibility of keeping the Roman school alive rested on him, on his will power and on his abilities.

As we swiftly paced the platform up and down in the cold December morning, the comment to these unspoken thoughts came from Ginestra Amaldi. She said then what she had said before, when I had told her of our decision to leave:

"Enrico's departure is a betrayal of the young people who have come to study with him and who have trusted in him for guidance and help."

"No, you are not being fair," Edoardo remonstrated. "Enrico honestly meant to fulfil his duties toward his students. He would not have left them without timely warning, had circumstances been normal. His reasons for leaving the country are impelling and independent of his will. Fascism is to be blamed, not Fermi."

Ginestra shook her head, and her expression became stubborn, as only the expression of usually yielding and gentle people may become. Her words were an incomplete formulation of her thoughts. Questions were in her mind that have troubled humanity since man started his quest for rules of conduct valid under all circumstances. Of contradicting duties, which should one choose?

Should the responsibilities toward one's family or those toward one's students come first? Should love of one's country come before love of one's children? Should one forgo the opportunity to take one's family to security and a more suitable environment in which to raise children, or should one remain under a despised government waiting for a possible chance of helping fellow-citizens from within? And, perhaps the most perturbing of all moral contradictions, should a woman forget her duties as a daughter to follow the call of those as wife and mother?

I knew Ginestra well enough, her deep devotion to her parents, her unshakable religious faith, to be able to read all this in her stubborn eyes. Edoardo had also read her thoughts, and his words had been an attempt at balancing her emotional convictions with his own rationalism. But questions that have not found an answer over the centuries cannot be settled in the few minutes before a train departs.

Ginestra was still stubborn, and I was full of doubts, when the railroad man shouted: "All aboard."

"I hope I'll see you soon," Rasetti said, and his voice was more subdued than I had ever heard it.

We climbed inside the train, lowered a window, and leaned out to wave goodbye to our friends one last time, as the train whistled and shook itself into sudden motion. When they had faded away, we closed the window against the sharp air of the winter morning. I took off and carefully laid down my new beaver coat—part of the refugee's trousseau, in Enrico's words, that we were taking along instead of money—and slumped in my seat.

The aqueducts and the umbrella pines of the Roman countryside sped by.

"Nothing can stop us now," Enrico said.

Now we were part of a moving train, of an inflexible schedule that would refuse to hesitate at frontiers and in forty-eight hours would bring us to Stockholm.

The fallacy in this reasoning is that trains have doors, and through those doors people can be made to get off. Neither Enrico nor I dared to mention this fallacy, although we were both aware of it. It conferred infinite power upon guards and customs officials; it distorted the significance of their actions; and it transformed the minutes they spent over our passports into bits of eternity.

Luckily, that sort of eternity moves along with time. It moved along with our train. We came out of Italy and Germany, and we could relax. We could enjoy the luxury of our first-class accommodations, of traveling with a maid along, who could find little more to do than roll Giulio's big curl on her finger to shape it in a long tube on top of his head.

That she could come with us was due to the Nobel Prize. When in October I had expressed the wish to have her with me during the first months in New York, Enrico had gone to sound out the American consul, who had been very helpful so far. The consul had not been encouraging: the Italian quota was filled up, and there was no hope of an immigrant's visa for the maid. As for a visitor's visa, what assurance could she offer that she would actually be a visitor, that she would return to Italy and not outstay her permit? We almost gave up. Then the Nobel Prize came and produced a blossom of smiles inside the consulate. The fiancé whom our maid produced was considered a sufficient guaranty, almost a hostage, of her coming back. In a few days she had her visa.

The Nobel Prize achieved other feats at the American consulate. When the American physician who examined us found that Nella used her right eye only and could see little from the left, he was inclined to raise serious difficulties: American health standards were to be kept high; Nella's vision defect ought to be corrected before we should be granted permission to enter the States, he contended. But the words "Nobel Prize" whispered into his ear silenced his objections.

The Nobel Prize, powerful as it was, did not exempt Enrico from taking the required arithmetic examination, which was considered as some sort of rough intelligence test. A woman came into the doctor's office, where candidates for immigrants' visas were waiting, and questioned them all.

"How much is 15 plus 27?" she asked Enrico.

Deliberately and with pride he answered "42."

"How much is 29 divided by 2?" "14.5" Enrico said. Satisfied that his mind was sound, the woman went on to quiz the next candidate. Giulio was too young for arithmetic. Nella and I passed the test. But the family of a retarded ten-year-old girl who could not keep her figures straight was denied a visa they had dreamed of for years.

At last our train arrived in Stockholm. We made our children wear the first leggings of their lives against the rigors of the northern climate, and we got off the train. Then we were dragged into the vortex of the Nobel celebrations.

There was the prize award on December 10, the anniversary of Nobel's death. Only the prizes for literature and for physics were awarded in 1938. Pearl Buck, the American writer of novels with Chinese background, and Enrico sat in the center of the stage in the Concert Hall. The hall was filled to capacity with bejeweled women in low-necked gowns and solemn men wearing white ties, tails, and heavy decorations on colored ribbons. Behind Pearl Buck and Enrico were past years' recipients of Nobel Prizes and members of the Swedish Academy.

Attentive and stiff in their tall armchairs with embossed leather backs and carved lion heads, Pearl Buck and Enrico faced the audience while listening to music and speeches. Plump and attractive in a soft evening dress, the train of which she had gracefully gathered around her, a pensive smile on her pleasant face, her hands demurely resting on her lap, Pearl Buck sat still; her stiffness was the outward projection of her bewilderment at the undemocratic manifestations of an Old World order and of her astonishment at being the object of such a manifestation.

Enrico sat stiff because he could not do otherwise. Stiff with the expectation of a dreaded but likely mishap: that the heavily starched front of his evening shirt might suddenly snap and thrust out in a protruding arc between the silk lapels of his full dress suit, with an explosive sound, at his first incautious move, as it had done many times before. Although dedicated to measurement, Enrico had not yet recognized that fronts of ready-made shirts were too long for him.

The Nobel medals and diplomas were given to Pearl Buck and Enrico by King Gustavus V of Sweden, who got up from his seat in the center of the first row on the floor but did not climb onto the stage. He waited for the two recipients to come, one at a time, across the stage and down the four steps to the floor. Tall and thin, he bent down over them his ascetic face, in which the skin had the pallor and the transparency that makes one wonder whether the blood of old aristocrats is not truly blue.

His Majesty shook hands with Enrico, when his turn had come, and handed him a case with a medal, a diploma, and an envelope. ("I think," Nella said later, in her calm, speculative tone, "that the envelope is the most important of the three because it must contain the money.")

With the three objects in his hands, Enrico retraced his paces backward, up the four steps and across the stage, because to royalties you must never turn your back. So, without even looking over his shoulder, outwardly sure of himself, he found his way to his leather-backed chair and happily dropped into it. Of this feat he was to brag for years to come!

And there was the night when I danced with the prince.

"The night when I danced with the prince" was the name of a popular perfume in Italy. It was intended to appeal to young romantic girls in whose dreams a charming prince often comes and asks them to dance.

If ever as a romantic girl I had dreamed forgotten dreams of this sort, not even in the dim imagery floating out of the mind in the night did I raise my hopes to a Crown Prince who was to ascend a throne within my lifetime. Crown Prince Gustavus Adolphus, now King Gustavus VI, was fifty-six years old in 1938, as dark and as robust a man as the king his father was white and frail. With him I danced the Lambeth Walk in the marbled splendor of the Town Hall. I had never danced it before, but the Crown Prince was a good leader. He was a man who gave support in a dance and inspired confidence, who wore honest, round, dark-rimmed eyeglasses à la Harold Lloyd; a man with the solid attributes of the good human being, not the unreal creature in the dreams of an incorrigibly romantic girl.

And there was the king's dinner at his palace, with a galaxy of princes and princesses, of court dignitaries and ladies-in-waiting; ladies, who, like all other women, examined the material of my evening dress—also part of my "refugee's trousseau"—and asked where I had bought it and who had made it into a gown.

King Gustavus V was my second king, and his dinner party my second dinner at a king's. I had eaten my first royal dinner five years before, with my first king, Albert of Belgium, the mountain climber.

It was in October, 1933, and we were in Brussels for the Solvay meeting of physicists. At all previous Solvay meetings the senior or the most prominent physicist from each country and his wife were asked to the Royal Palace. In 1933 Enrico and I were included in the invitation, for Enrico was the only physicist from Italy.

A short before-dinner conversation with the queen, during which my foremost concern had been to abide by the rule of never saying No to Her Majesty, had proved inconclusive and unsatisfactory because I felt I was behaving wrongly. Then we had gone in for dinner. At the place of honor by King Albert sat Marie Curie, twice a Nobel Prize winner, for chemistry and for physics. She was past middle age and had the aloof and absorbed expression of one used to taxing her intellectual powers at all times. Seated beside her, the mountaineer king seemed relaxed and affable. He slumped down and sought comfort in his big chair, he laid his strong arms on the table, next to the golden plate in front of him. The center of the table was set with golden plates, but they stopped short of me, and I missed my unique chance to eat off gold.

King Albert was a hearty eater. When fruit was passed to him in a beautiful basket, he took a pear in his large hand, and with a knife he peeled and quartered it, still holding it in his hand. My mother had taught me to lay my fruit down on my dish, to hold it firmly there with the fork, and to carve the peel off with skilled use of the knife. My mother claimed this was the only acceptable way of peeling fruit in good society.

Although twenty-six years old and a married woman five years, I exulted while watching the king and his pear and envisioned my future triumph over my mother.

Unlike King Albert's, King Gustavus' appetite was poor. Of this we should have been warned; as soon as he laid down his fork and knife after just a bite or two, the waiters, who had stood a few steps behind the sitting royalty and guests, sprang forward to the assault, grabbed and took away all plates, and served the next course, only again to remove the plates that there was no time to empty, as soon as the ascetic king had had enough.

Of all the princes and princesses who sat at the king's table, the most charming, the most softly human, was Sybille, the wife of the king's grandson. She talked to me at length in the Town Hall. Her words flowed out of her pretty mouth with easy friendliness as she inquired after my children, as she spoke of her three little daughters with motherly pride. (Later she had one more daughter and a son.) Despite the glistening diadem on her head, despite the big pearls on her white décolletage, I forgot that she might be a queen one day and addressed her as I would a newly acquired friend. Perhaps there was something prophetic in my attitude: Sybille will never become a queen, because her husband, Prince Gustavus Adolphus, who might have become Gustavus VII, was killed in an airplane crash in 1947. His and Sybille's son, now heir apparent Carl Gustavus, was then one year old, and Sybille was left with a task more difficult than preparing to become a queen—that of raising a future king.

A motion picture taken in the Concert Hall in Stockholm during the ceremony of the Nobel awards was soon thereafter shown in various countries. It produced a wave of criticism in Italy.

In 1938 the award of the Nobel prize to an Italian was viewed with manifest doubt, almost with fear, in official quarters, as something which might displease the intransigent northern ally. Hitler had prohibited Germans from henceforth accepting the Swedish award when, in 1935, the Nobel Peace Prize was conferred on Carl von Ossietzky, author and pacifist, while he was detained in a Nazi prison on charges of being an enemy of the state.

Proof of the press uncertainty was the fact that Pearl Buck and Fermi had equally shared an exceedingly brief, three-line announcement in the Italian papers. Subsequently, Fermi had gone to Stockholm and had committed a double crime: first he had failed to give the Fascist salute to the king of Sweden; then he had shaken hands with the king, a gesture that had been condemned in Italy as un-Roman and unmanly.

This wilful restraint on the most harmless of human actions seems a joke now that fascism is long dead and done for. The tragedy of Italy lay not in the fact that of such jokes there were many, but in the fact that a large number of people took them seriously. Enrico's handshake with King Gustavus was taken seriously.

Karl Sandels

At the Nobel Ceremony with Pearl Buck: Will the Shirt Front Snap Out?

"A Study of the Discovery of Fission" . **Esther B. Sparberg. American Journal of Physics, January 1964, volume 32, p. 2.**

A Study of the Discovery of Fission

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Nuclear fission was discovered twenty five years ago by Hahn and Strassmann. Was the road to the discovery as torturous as some have contended? The roots are examined; the surprising climax and its impact are described. Some scientists have commented on the discovery in retrospect. The discovery of fission is scrutinized both in the light of these comments, and of the general characteristics of scientific discovery.

THE discovery of fission was announced by Otto Hahn and Fritz Strassmann twenty five years ago in *Die Naturwissenschaften.*[1] So unexpected was the news, and so sensational, that it immediately provoked a tremendous outpouring of scientific papers. Just three and a half months later, Feather asserted in a review article, ". . . so much has been published that rigorous selection is necessary in any report of the subject."[2] Turner[3] estimated that nearly one hundred papers had appeared on the subject within the year. Reflecting the excitement of the period were the many practically simultaneous discoveries made in 1939.[4]

On this twenty fifth anniversary year,[5] it seems appropriate to review the discovery of fission. Lise Meitner[6] has asserted that "viewed in the light of our present knowledge, the road to that discovery was astonishingly long and to a certain extent the wrong one . . .." Was the path really so devious? Has Meitner, who played a leading role in the discovery of fission, been engaging in "Monday morning quarterbacking?" What were the roots of the discovery? What factors contributed to the successful solution of a difficult problem? Did the experimenters devise new techniques? Were any radical ideas formulated during the course of the investigations? What characteristics of scientific discovery described by students of the subject can be noted?

[1] O. Hahn and F. Strassmann, Naturwiss. **27**, 11 (1939).
[2] N. Feather, Nature **143**, 878 (1939).
[3] L. A. Turner, Rev. Mod. Phys. **12**, 1 (1940).
[4] H. H. Goldsmith, Sci. Monthly **68**, 292 (1949).
[5] It is also just twenty years since Otto Hahn received the Nobel Prize for the discovery of fission.
[6] L. Meitner, Advan. Sci. **19**, 363 (1963).

ROOTS OF THE DISCOVERY

The discovery of fission[7] was not an accident.[8] Nor did it result from serendipity.[9] It represented the climax of a crescendo of activity in nuclear science during the thirties. Although Hahn and Strassmann were indeed surprised by their unexpected results, the hypothesis of fission was inevitable in the light of the intensive investigations in the field. If Hahn and Strassmann had not made the discovery, it would have been made by others. In fact, I. Joliot-Curie and Savitch almost made the discovery in 1938. Several weeks after the publication of the Hahn and Strassmann paper, Philip Abelson in a letter to the *Physical Review* announced that he had found what seemed to be "unambiguous and independent proof of Hahn's hypothesis of the cleavage of the uranium nucleus."[10] A series of anomalous experiments gives rise to restlessness that is soothed only by an adequate solution. To compare the discoverers of fission to the Three Princes of Serendip, who according to Horace Walpole, "were always making discoveries, by accidents and sagacity, of things they were not in quest

[7] The word "fission" was first used in the paper by L. Meitner and O. Frisch, Nature **143**, 239 (1939).
[8] An example of a popular viewpoint is in Time **33**, 21 (1939).
[9] William L. Laurence, *Men and Atoms* (Simon and Schuster, Inc., New York, 1959), p. 12. After this manuscript was sent to the editor, a viewpoint similar to the author's was expressed in an editorial by Philip Abelson. Science **140**, 1177 (1963).
[10] Philip Abelson, Phys. Rev. **4**, 418 (1939). R. Hewlett and O. Anderson, Jr., confirm that at the time of the Hahn–Strassmann discovery, Philip H. Abelson in his doctoral research at Berkeley was also so close to the discovery of fission that he almost certainly would have made it within a few weeks. Richard G. Hewlett and Oscar E. Anderson, Jr., *The New World, A History of the United States Atomic Energy Commission* (The Pennsylvania State University Press, University Park, 1962), Vol. 1, p. 12.

of"[11] seems superficial and naive, for the scientists were in quest of a solution to a very definite problem.

Their immediate problem was an attempt to clarify some previous baffling results of Joliot–Curie and Savitch who were investigating the neutron bombardment products of uranium. This area of research had been opened up by Enrico Fermi after the discovery of the neutron (1932) by Chadwick and the discovery of artificial radioactivity by the Joliot–Curies (1934). After reading about the work of the Joliot–Curies, Enrico Fermi decided to try to produce artificial radioactivity with neutrons[12] since they had the advantage of a projectile with no charge and hence were not repelled from the positive nucleus. Alpha particles from radium or radon dislodge neutrons when they strike beryllium. Before striking the target, the neutrons may be slowed down by paraffin to produce increased radioactivity in the target element. This effect had been unexpectedly discovered by Fermi and his associates.

Fermi's plan was to systematically bombard all the known elements starting with hydrogen. When uranium was bombarded with neutrons, Fermi and his associates discovered that they had produced more than one radioactive product from the reaction. The products seemed to be four in number, as revealed by four different half-lives. Since there were only three known isotopes of uranium, one of the products was mistakenly thought to be element 93. This seemed a valid assumption, because the capture of a neutron by a nucleus usually produced an unstable isotope emitting a beta particle and thereby having an atomic number greater by one. Since their investigations also showed that these newly formed radioactive species could not be the isotopes of any element from radon (86) to uranium (92), they announced in 1934[13] that they had probably produced element 93. "We did not have enough imagination to think that a different process of disintegration might occur in uranium . . . and we tried to identify the radioactive products with elements close to uranium on the periodic table . . . we did not know enough chemistry to separate the products . . . and we believed that we had four . . . while actually their number was close to fifty."[14] Yet a German chemist, Ida Noddack, pointed out that their methods did not disprove the existence of lighter elements as bombardment products, "but this thought was considered to be wholly incompatible with the laws of atomic physics."[15] It was also suggested that the unknown product might be protactinium (91).

THE BACKGROUND OF HAHN AND MEITNER'S INTEREST IN THE URANIUM PROBLEM

The published reports of Fermi's work in *Nuovo Cimento* and *Nature* were so fascinating to Meitner that immediately she "persuaded Otto Hahn to renew our direct collaboration . . . with a view to investigating these problems. So it was that in 1934, after an interval of more than twelve years, we started working together again, with the especially valuable collaboration, after a short time, of Fritz Strassmann."[6] They immediately set themselves the task of repeating Fermi's experiments to ascertain whether or not the thirteen minute element was a protactinium isotope.[16] Since they were the codiscoverers of protactinium (element 91) in 1917, they were very familiar with its chemical properties. In fact, Hahn's interest in radiochemistry had been inspired by Ramsay thirteen years earlier; Hahn relates: "The story must go back to the year 1904, for that was the year in which I was 'transmuted' from an organic chemist into a radiochemist and thus began to learn about radioactive elements."[17] He recalls that when Sir William Ramsay asked whether he would like to work on radium, he replied that he knew nothing about the element. Ramsay retorted that "it was an advantage to be able to approach the subject with a mind free from preoccupations."[18] During his studies with Ramsay in England and later Rutherford in Montreal, he discovered two

[11] Horace Walpole in a letter to Mann, 28 January 1754, as quoted in *The Reader's Encyclopedia*, edited by William Rose Benet, (Thomas Y. Crowell Company, New York, 1948), Vol. 4, p. 1012; also described in *The Oxford Companion to English Literature*, p. 710.

[12] Laura Fermi, *Atoms in the Family* (The University of Chicago Press, Chicago, 1954), p. 83.

[13] E. Fermi, Nature **133**, 898 (1934).

[14] E. Fermi, as quoted by Laura Fermi. See Ref. 12, p. 157.

[15] O. Hahn, Sci. Am. **198**, 78 (1958).

[16] Otto Hahn, *New Atoms* (Elsevier Publishing Company, Inc., New York, 1950), p. 17.

[17] See Ref. 15, p. 76.

[18] See Ref. 16, p. 142.

new radioactive species. ". . . in the course of these studies I was acquiring what was to be of the greatest help to Strassmann and myself later in discovering the fission process: namely, a thorough familiarity with methods of separating radioactive substances."[17] Rutherford[19] himself, writing of this period, related: ". . . the results so far obtained by Hahn are of the greatest interest and importance."

In 1907, Lise Meitner began the association with Hahn that lasted for thirty years until it was severed by the actions of the Nazi regime. Their investigations involved a study of the beta rays of their almost complete set of radioactive elements. In 1917, as previously mentioned, they discovered protactinium. "It was our work on the chemical properties of this substance that gave rise later to our keen interest in investigating the irradiation of uranium with neutrons."[20] Their attempts to resolve the problem of Fermi's unknown isotope seemed to confirm Fermi's views.

In these experiments, they used an indicator technique, adding a beta-emitting protactinium isotope to the unknown neutron bombardment product. Separation of the mixture was then attempted by chemical precipitation. Results indicated that neither protactinium, thorium, nor actinium was present since the natural protactinium isotope separated out from the unknown product. It was, therefore, probably element 93, since they did not consider the possibility of splitting heavy nuclei into lighter ones. Yet Meitner noted that it was disturbing to find such a long chain of successive beta disintegrations.

They continued their researches on transmutation products and found what they thought to be transuranium elements 93–98.

CURIE AND SAVITCH DISCOVER A 3.5-H PRODUCT

Meanwhile, Joliot–Curie and Savitch also became interested in this area of research and reported that they had found a new thorium isotope. Upon checking their results, the German team wrote back to Irene Joliot-Curie that they were unable to find this isotope. Meitner[21] has ruefully noted that they searched for the thorium isotope in the filtrate resulting from the precipitation of the "transuranium" elements. Since, according to their scheme, uranium, protactinium, and thorium were definitely in the filtrate, the precipitate was assumed to be composed of the "transuranium" elements. As they did not expect the filtrate to contain either the lower atomic weight, or the "transuranium" elements, they therefore examined only the precipitate; "Here was our mistake." Even when they examined the filtrate for thorium, they did not think of looking for anything else.

In the meantime, in 1937, Joliot–Curie and Savitch made a perplexing discovery. They found a new 3.5-h product[22] which did not separate with the transurnaium elements according to the Hahn–Meitner method of platinium precipitation. This isotope, instead, precipitated with lanthanum (atomic weight 139, and atomic number 57), a medium element rather than a heavy one; moreover this new product resembled lanthanum chemically.

They were puzzled at finding an element with properties resembling lanthanum among the elements supposedly beyond uranium in the periodic table. Yet they did not realize the identity of lanthanum with this element of half-life 3.5-h because of some anomalies in the chemical evidence. Hahn confirms[15] that if they had been convinced that their 3.5-h element was indeed lanthanum, they would have been the discoverers of fission.

HAHN AND STRASSMANN REPEAT THE EXPERIMENT

Upon reading the Curie–Savitch paper, Hahn and Strassmann (Meitner had left Germany in July 1938) decided to repeat the experiments, since the results were incomprehensible to them. How could an element with chemical properties similar to lanthanum be considered a transuranium element assigned to the region beyond uranium in the periodic table? In the course of these investigations, they found what they assumed to be four radium isotopes, since they

[19] Ernest Rutherford, *Radioactive Transformations* (Charles Scribner's and Sons, New York, 1906), p. 69.
[20] See Ref. 15, p. 77.
[21] See Ref. 6, p. 364.
[22] I. Curie and P. Savitch, Compt. Rend. **206**, 1643 (1938), from the bibliography of the Hahn and Strassmann paper.

precipitated out with barium. Radium and barium are chemically similar since they are in group 2a of the periodic table, and so the radium will be carried with the barium precipitate. Accidental impurities, they were sure, could not have caused the results, for barium had been precipitated according to Strassmann's suggestion as an extremely pure chloride. The idea that these radioactive isotopes might be isotopes of barium itself did not occur to them at the time, for Hahn writes[23] that "since it seemed from the state of knowledge of nuclear physics that barium could not be produced from uranium by neutron bombardment . . . we could only conclude that the products must be isotopes of radium . . . still it was a strange affair to be producing radium from uranium under the conditions of our experiment." The transmutation of radium from uranium required the successive emission of two alpha particles and slow neutrons had never before been observed to stimulate the emission of alpha particles from uranium. The results of these experiments were published in 1938.[24]

THE PAPER ANNOUNCING THE SPLITTING OF THE URANIUM ATOM

In order to clarify their results, Hahn and Strassmann continued their investigations and they separated and positively identified what they assumed to be radium 88 and its daughter product, actinium 89. The unknown radioactive products from uranium bombardment were precipitated with a barium carrier. Since they had remained with barium, they were thought to be isotopes of radium. There appeared to be four isotopes of radium among the products, as shown by the four different half-lives. From these isotopes, "radium IV" with a half-life of 250–300 h, was separated with barium. Attempts to separate the artificial "radium IV" from barium by the fractional crystallization method of M. Curie were unsuccessful. A method they developed for precipitating the unknown with the carrier as chromates rather than as chlorides or bromides failed also.

Since it seemed possible that their inability to achieve a separation might be due to the fact that only extremely small quantities of "radium IV" were present, Hahn and Strassmann decided to check their results. They used small quantities of natural radium isotopes of an intensity just detectable with a Geiger–Müller counter; these weak isotopes were found to be separable from barium. They then used indicator experiments, mixing natural radium isotopes with their artificial "radium IV." When this mixture was added to barium, again the natural radium isotopes were separated by fractional crystallization, while the artificial one remained behind with the barium. "Radium IV" must be barium.

> "We come to the conclusion: our "radium isotopes" have the properties of barium; as chemists we should really say that considering the new substances, they may not be radium but barium, because there is no question of other elements but radium or barium."[25]

As a further confirmation, they tested the decay products from "radium IV." If the isotope was barium, then lanthanum would be its daughter product. When a natural actinium isotope was mixed with one of the primary decay products of the artificial "radium IV," with lanthanum added as a carrier, the decay product remained with lanthanum, and must therefore be a lanthanum isotope rather than actinium. This was the 3.5-h product that had been found by Curie and Savitch.

THE CONCLUSIONS OF HAHN AND STRASSMANN, AND THEIR IMPACT

Since Hahn and Strassmann, using every available technique, were unable to separate their supposed radium isotope from barium, they were forced to conclude that the "radium IV" was not radium but barium.

> "As chemists we really have to change the symbols of Ra, Ac, and Th to Ba, La and Ce instead, which is based on the scheme used in the above briefly described experiment. As nuclear chemists we are in a way close to physics, and according to all experience up to now in nuclear physics, we cannot decide to make this contradictory jump. It could be that a series of strange accidents has distorted our findings."[26]

Their conclusions were couched in these cautious words because the idea that a nucleus could be broken up had never occurred to anyone;

[23] See Ref. 15, p. 80.
[24] O. Hahn and F. Strassmann, Naturwiss. **26**, 755 (1938).
[25] See Ref. 1, p. 14.
[26] See Ref. 1, p. 15.

neutron bombardment products were always considered to be very close in atomic number to the parent atom. Hahn explains,[27] "Our overcautiousness stemmed primarily from the fact that as chemists we hesitated to announce a revolutionary discovery in physics." It was not until they again checked their results, published in their next paper, that they showed beyond a doubt that their "radium" isotopes were actually barium, and that an atom could be split.

Despite the hesitant manner in which Hahn and Strassmann first expressed their findings, the immediate impact of this paper on the scientific world was enormous. The results were widely known even before publication, for Hahn at Christmas 1938 had written with great excitement to Meitner in Sweden of the results of the experiments. What did she, as a physicist, think of the results?

> "On reading the letter, I myself was thoroughly excited and amazed and also, to tell the truth, uneasy. I knew the extraordinary chemical knowledge and ability of Hahn and Strassmann too well to doubt for one second the correctness of their unexpected results. These results, I realized, had opened up an entirely new scientific path—and I also realized how far one had gone astray in our earlier work!"[21]

Meitner immediately informed her nephew Otto Frisch of the astonishing news. Frisch confessed:

> "When Lise Meitner told me that, I would not listen at first; the barium nucleus is only about half as heavy as uranium, and I could not see how half of the uranium nucleus could have been chipped away by the impact of a single neutron. But as we talked, a new picture gradually took shape . . . ."[28]

The nucleus was compared to a liquid drop with reduced surface tension due to the repulsion of the component protons. If the movement of the nucleus was violently increased by a captured neutron, the nucleus would form a "waist" and divide into two lighter nuclei. "In view of the similarity of this process to cell division, we called it (at Frisch's suggestion) 'fission' . . . ."[29] Because the products of the nuclear reaction have less total mass than the reactants they realized that energy was released, which they estimated at 200 MeV. About three weeks later, on 16 January 1939, Meitner and Frisch sent a letter to *Nature* setting forth their theoretical explanation of fission.[7] Frisch quickly followed with another letter confirming that he had been able to detect the energy released during fission.[30]

Niels Bohr learned of the new developments from Frisch just prior to Bohr's departure for the United States, and he spread the news in America of the splitting of the uranium atom. The discovery and immediate theoretical explanation of Meitner and Frisch were announced by Bohr on 26 January 1939 at a conference on theoretical physics at the Carnegie Institution of Washington. So electrifying was the effect of the report that many immediately returned to their laboratories to measure the ionization energy of the fission products. The general public was informed of the scientific ferment by *Time Magazine* in an article which read, "Last week, the Hahn report reached the United States, and physicists sprang to their laboratories to see whether they could confirm it. Early this week, the laboratories of Columbia, Johns Hopkins and the Carnegie Institution announced confirmation."[31]

THE FIRST DETECTION OF THE DISINTEGRATION OF URANIUM AND SCIENTIFIC DISCOVERY

This account of the investigations that produced the discovery of fission illustrates the effect of the "relentless pressure of accumulating knowledge"[32] on the emergence of the novel concept that atoms may be split in transmutation. As long ago as 1793, Goethe[33] declared: "The most beautiful discoveries are made not so much by men, as by the period . . . they mature in the course of time . . . ." The discovery of artificial radioactivity, the identification of the neutron and its immediate use as a tool of research, and the continued refinement of radiochemical techniques were the principal agents. During the four years of explorations in this particular area of research, the near-birth of the idea of fission was prevented by uncertainties

[27] See Ref. 15, p. 82.
[28] O. Frisch, *Atomic Physics Today* (Basic Books, Inc., New York, 1961), p. 20.
[29] See Ref. 28, p. 365.
[30] O. R. Frisch, Nature **143**, 276 (1939).
[31] Time Mag. **33**, 21 (1939).
[32] A. J. Ihde, Sci. Monthly **67**, 429 (1948).
[33] J. W. Goethe, cited by L. L. Whyte, Harper's Mag. **20**, 25 (1950).

in experimental techniques, and by the then-accepted concept that an atom could not be divided; but the labor pains caused by anomalous experimental results finally became severe enough to force the new concept to emerge into the scientific world. When the chemical evidence was indisputable, the birth was hesitantly announced by Hahn and Strassmann.

The influence of the generally accepted theories of the time upon scientific progress may be examined in the light of the Hahn and Strassmann findings. The suggestion that an atom could be divided had simply not occurred to anyone except Ida Noddack, whose proposal was ignored, and so no investigator looked for medium atomic weight species among neutron bombardment products. As noted previously, Lise Meitner regretfully commented that they did not examine the filtrate remaining from the precipitation of the "transuranium" elements because they did not believe it contained any elements below 90. Yet, many of their tests did point to the impossibility of separating neutron bombardment products from their lower atomic weight carriers—and each time this problem was attributed to experimental difficulties. Finally, the accumulated evidence could not be denied. Hahn has reiterated, "These tests were in opposition to all the phenomena observed up to the present in nuclear physics."[34] Even Einstein asserted, ". . . this was not something I could have predicted."[35]

Preconceived ideas may indeed act as a brake to progress, but they are also part of the supporting framework upon which new knowledge is built. The four years that elapsed before the puzzling and intriguing problems were solved were in part caused by the barricade of preconceived ideas. Contributing equally to the delay was the difficulty of the experimental techniques.

The final identification of barium as a transmutation product resulted from the painstaking application of nuclear science methods by researchers with long experience in the field. The most difficult problems that had been surmounted were radiochemical ones—the separation and identification of tiny quantities of radioactive materials. Although no innovations were evident in the Hahn and Strassmann experiments, some refinements had been made by Hahn, Meitner, and Strassmann. Hahn's consistent interest in radiochemistry is shown by his researches on the formation of mixed crystals between radium and barium just prior to these investigations.[36]

Fermi's mistaken identification of element 93 was caused by these experimental difficulties. The state of the art in 1934 was not sufficiently mature to provide the right answers. The four years between the investigations of Fermi and those of Hahn and Strassmann may be considered the period of the discovery of fission, and illustrates well the suggestion of Kuhn[37] that "the process of discovery is necessarily and inevitably one that shows structure and that therefore extends in time." The discovery had its roots in the puzzling products of neutron bombardment, and its logical culmination in the identification of barium.

Was the road to the discovery so "astonishingly long" and "to a certain extent wrong" in the light of present knowledge, as Dr. Meitner has asserted? The path was certainly not direct—but is the path to a discovery ever straightforward? A glance at the history of science would reveal that any scientific discovery was preceded by a period of groping; that blind alleys often delayed the final solution. In the light of the state of knowledge after the discovery has been made, the investigators appeared at times like sleepwalkers. Wouldn't the platitude about the superiority of hindsight to foresight be relevant here?

A question that may next be considered is the effect of nonscientific factors on the emergence of productive scientific ideas. That fission was discovered in a most rigid and authoritarian political environment raises the question of the relationship between political freedom and scientific progress. This is a difficult question to answer in terms of the discovery of fission, for the German scientists at the Kaiser Wilhelm Institute seemed somewhat insulated from the political Zeitgeist. Yet, they were not completely isolated, for Lise Meitner had been forced to leave

[34] See Ref. 16, p. 23.
[35] Albert Einstein, *Out of My Later Years* (Philosophical Library, Inc., New York, 1950), p. 188.
[36] See Ref. 16, p. 22.
[37] T. S. Kuhn, Science **136**, 764 (1962).

Germany. Despite the fact that the Nazi leaders had ordered the subjugation of all science to the needs of the state, Hahn and Strassmann were engaged in fundamental research that had no immediate applications. Fermi and his associates were also able to pursue fundamental investigations at the great physics school of the University of Rome in Fascist Italy. These activities indicate that islands of intellectual honesty may remain uninundated by their surroundings, which is a disappointing conclusion to those who would prefer to equate progress in basic science with political freedom. These "island" institutions were headed by brilliant men, who were very much interested in both their faculty and their students, and they were operated in an atmosphere of warm communication and relative leisure, where investigations were carried out without outside pressure. The general climate in the nonacademic world was one of great respect for learning; in spite of the anti-intellectual forces in the Fascist countries, the traditional regard of the Europeans for the academic profession and for scholarship could not be so easily eradicated.

The discovery of fission was propelled by the times; it was also propelled by men, and illustrates well the statement of Boring[38] ". . . great men make great discoveries. Yet it may be that the Zeitgeist determines the great discovery, and that he who makes the discovery, the Zeitgeist's agent, is great merely because the times employed him." Otto Hahn was a recognized leader in the field of radiochemistry, and had worked in this area for over thirty years. His interest in identifying uranium bombardment products was natural, and his attempts to unravel puzzling experimental results were persevering and meticulous. The triumphant discovery of barium resulted from his finesse, and genius, in that particular area of research. His contributions were officially recognized when he received the Nobel Prize in 1944 for the discovery of fission. To neglect to note here that the identification of barium was merely the apex of a pyramid of investigations would be inexcusable. Many others played essential roles in the development of the story. Among them, as mentioned previously, were Meitner, Fermi, Joliot-Curie, and Savitch.

The great impact of the announcement of fission illustrates yet another characteristic of scientific discovery—the surge of experiments and related ideas produced in the scientific world by the unexpected birth of the new concept. The concept of the splitting of the uranium atom by slow neutrons led immediately to the realization and experimental demonstration that (1) a large amount of energy was released during the fission process, (2) neutrons were liberated thus suggesting the possibility of a chain reaction, (3) uranium 235 was the isotope responsible for fission, (4) delayed neutrons are emitted, (5) elements 93 and 94 would be produced if uranium 238 absorbed neutrons, (6) element 94 (plutonium) was fissionable. The successful operation of the first chain-reacting pile in December 1942, accelerated by the needs of World War II, was the victorious culmination of four years of concentrated research.

Thus, the unexpected and surprising identification of barium initiated a torrential outpouring of related investigations; the fission of uranium had been produced by the inevitable convergence of new ideas and techniques, utilized by skilled investigators operating within a general and scientific Zeitgeist that influenced both the manner and date of the successful conclusion.

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[38] E. G. Boring, Proc. Am. Phil. Soc. **94**, No. 4, 341 (1950).

"*The two maps*" Oersted Medal Response at the joint American Physical Society-American Association of Physics Teachers meeting, Chicago, 22 January 1980. **Gerald Holton. American Journal of Physics, December 1980, volume 48, no. 12, p. 1014.**

The two maps
Oersted Medal Response at the joint American Physical Society–American Association of Physics Teachers meeting, Chicago, 22 January 1980

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Mr. President, ladies and gentlemen. I thank you warmly for the award of the Oersted Medal. The names of the previous medalists, from Edwin Hall, Robert A. Millikan, and Arnold Sommerfeld on to our day, form a kind of roll call of our particular Mount Olympus. Each additional entry must necessarily feel more overwhelmed. But there is at least one thing in common: I looked at some of the Responses presented in the past and found that precedent encourages personal reflection. Allow me to adopt this mode today, first to share a serious concern, and then to acknowledge some pleasant debts.

I. THE OERSTED INVISIBILITY

The various efforts mentioned in your gracious citation stemmed from the idea that science should treasure its own history, that historical scholarship should treasure science, and that the full understanding of each is deficient without the other. If I were asked to indicate the chief motivation behind this once unusual, not to say perverse, idea, I would have to do so in the form of a stark statement that can be supported, on this occasion, only sketchily. It is this: At a time when passionate unreason around the globe challenges the fate of Western culture itself, the sciences and the history of their development remains perhaps the best testimony to the potential of mankind's effective reasoning. Therefore, if we do not trouble ourselves to understand and proudly claim our own history, we shall not have done full justice to our responsibility as scientists and as teachers.

A good way to illustrate this point is to glance at the work of the man whose name graces the award. Even among physicists, Hans Christian Oersted (1777–1851) is little known, although an argument can be made that he was a modern kind of scientist. He announced his great discovery, that a magnet needle can be deflected by what was then called a galvanic current, on July 21, 1820 in a broadside which he had privately printed and distributed to the foremost scientists of Europe.[1] The publication led to a veritable explosion of scientific work, for example, the great discoveries in electromagnetism by Ampère and Faraday. Technical applications followed quickly also, starting with an electric telegraph.

Oersted, then 43 years of age, was professor of physics at Copenhagen. An autodidact, he had first studied pharmacy and languages, and also contributed to chemistry and other sciences. He was an early evolutionist in biology, a man with wide interests in science and outside. Among his works are some on the relation of science to poetry and to religion, in which he shows himself to be a gentle but persuasive rationalist.

Oersted saw his chief task to be the discovery of the unities in nature. Far from having stumbled accidentally on the fact that a magnetic field surrounds currents, as the popular myth still has it, he had sought for years for the effect which practically everyone else, during the two decades of widespread experimentations with voltaic cells from 1800 on, had missed. At least eight years before his discovery of 1820, he had declared his faith that light and heat and chemical affinity, as well as electricity and magnetism, are all "different forms of one primordial power," and he had announced that attempts must be launched "to see if electricity has any action on the magnet." Oersted had been deeply impressed by the philosophical works of Immanuel Kant where he found the argument that all physical experiences are due to one force (*Grundkraft*). He also accepted many views of his friend Friedrich W. J. Schelling, a leading exponent of *Naturphilosophie*, who provided an enthusiastic program to find the unity of all natural phenomena, and specifically the unity of physical forces, in such proposals as: "For a long time it has been said that magnetic, electrical, chemical, and finally even organic phenomena would be interwoven in one great association .... This great association, which a scientific physics must set forth, extends over the whole of nature."

R. C. Stauffer, in whose paper the quotation from Schelling is given, also cites the response of Mme. de Staël, on concluding her discussion with him on *Naturphilosophie*, that "systems which aspire to the explanation of the universe cannot be analyzed at all clearly by any discourse: words are not appropriate to ideas of this kind, and the result is that, in order to make them serve, one spreads over all things the darkness which preceded creation, but not the light which followed." Be that as it may, the lack of Cartesian clarity did not impede the influence of *Naturphilosophie* on Oersted (or for that matter, on Ampère, Faraday, Julius Robert Mayer, and others).

The thematic presupposition of the unity of forces led Oersted to look for the connection between electricity and magnetism through a convincing experiment. His success identifies him as the modern initiator of the grand unification program—that great source of motivating energies of modern physics, with a direct genetic influence on Faraday, Maxwell, Einstein, and on to this year's Nobel Prize winners. In the physics meetings we are having here, Oersted, at least in spirit, would surely have felt very much at home.

Moreover, Oersted's apparatus for demonstrating the unity of nature's phenomena was eminently sensible, at least in terms of his presuppositions. He sent progressively larger electric current through high-resistance platinum wires. First the wire got hot, then the wire began to give off light, and then he saw the effect on the magnet needle. Some day I hope to look at Oersted's laboratory books in Denmark to see if he also looked for a gravitational effect as Faraday did later; it would have been a likely extension in the thinking of a unifier.

In his later years, Oersted dedicated himself to many social causes, such as the freedom of the press, and to

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science education. One of his biographers notes that when Oersted died in 1851, the students arranged for a torch-light cortege in a huge funeral procession in which 20000 people are said to have participated. (I count on the American Association of Physics Teachers, in due course, to make corresponding arrangements for its Oersted Medalists.)

Now there is no doubt that Oersted's advance in physics changed history in two ways. It opened up physics itself to a succession of unifying theories and discoveries without which the modern state of our science would be unthinkable. And his key discovery, embodied in every electric motor, also triggered the engineering advances that have produced the modern technological landscape. One would expect such a person to be visible in works of history. Yet, despite his role in the initiation of vast changes in science, engineering, and through it our very society, Oersted is virtually absent from both science books and history texts, not least those which young persons typically encounter in school. If one looks into historical encyclopaedias for what happened in 1820, one finds a plethora of events of a quite different sort. For example, Karl Ploetz's compendium of chief dates of history notes for 1820 that "Austrian troops reestablish order in Italy"; and the big *Encyclopaedia of World History*, edited by William Langer, records for 1820 that King George III, having earlier been declared insane, dies.

Let me dwell a little on this phenomenon, which we may call the *Oersted Invisibility*, for we are getting to a significant truth about the influences that shape the intellectual formation of young people throughout the world. William Langer's widely used *Encyclopaedia* was not meant to be a synthetic work and does not represent the more sophisticated work of scholars in history today. But the two main concerns that animate the book are still alive in the teaching of history as most young people encounter it in their formative years: the chronological presentation of historically important "facts" and the "periodization" of the sequence of facts into labeled categories. As to the first, Langer says in his introduction it is his function to provide "a handbook of historical fact"—as it turns out, chiefly political, military and diplomatic history. He explains that for him to have gone also into other achievements such as science would have taken him too "far afield." In my copy (2nd edition, 1948) there *is* a section entitled "Scientific Thought and Progress," precisely 2-1/4 pages out of the total 1270—or less than 0.2% (which happens to be better than the proportion of fundamental science support in our GNP). As an attempt to put some order into the chaos of facts, periodization of history into segments entitled the "Age of ," the "House of ," the "Revolutionary Period," etc., provides a seemingly well-bounded shape to the various fragments of time. But it encourages only occasionally a comment that parts the dark curtain behind which historians are debating. (Was Thucydides right in considering the war of 431–421 B.C. and the war of 414–404 B.C. to be in reality one period, that of the Peloponnesian War? How far back do the causes of the revolutionary outbreaks of A.D. 1848 go?)

The net effect, at least on most young minds, must be that the purpose of history appears to be the provision of a well-labeled place for every miscreant if only his factual mischief was on a big enough scale, and to give some space to every ruler, whether effective and beneficient or not. In such books, genealogies of the mighty abound, from the Ch'in Dynasty and the succession of the Merovingian kings to the sequence of Czars and Presidents. One is reminded of Voltaire's complaint that "for the last 1400 years the only persons in Gaul apparently have been kings, ministers, and generals."

Scientists and their works are virtually taboo. If you look for Newton in my copy of Langer, you will find him on p. 431, under the heading "Third Parliament of William III": "Isaac Newton, master of the mint." The four-word description is not what you or I might have chosen; but at least he is mentioned, which is more than can be said for Galileo or Kepler or even Copernicus. (Others have done worse: Arnold Toynbee's list of "creative individuals," from Xenophon to Lenin and Hindenburg, included not a single scientist.)

On the other hand, Langer's book does give a detailed survey of, say, the vicissitudes and successions of the Ottoman emperors, from Bayazid II, remembered by Langer for being "the least significant of the first ten sultans," through the exploits of Selim I ("The Grim") and Selim II ("The Sot," described as an "indolent ruler, much given to drink"), and so on through the last of them, Mohammed IV, who is described as a boy of ten, followed by a period of anarchy. It might almost sound amusing, until one asks what the costs were for the unmentioned mass of humanity doomed to be born, to live, and to die in the dark back alleys of history.

Every holy war gets its place in Langer's and so many books like it. There is a tiny admixture of humane figures, a Marcus Aurelius, a Jefferson, or a Ghandi; but they are lost in the succession of genocidal maniacs, from Emperor Tiglath III of Babylon, who innovated the idea of consolidating his conquests by "deporting entire populations," to Josef Stalin. As John Locke asked: "What were those conquerors but the great butchers of mankind?" Moreover, from one development to the next, any rational conclusion or extrapolation is seemingly hopeless. No wonder that even many of today's historians, in the words of J. H. Plumb, "have taken refuge in the meaninglessness of history."[2] Thus the Ploetz compendium—which was the model for Langer, and, as it happened, also the book I used in high school in Vienna—started on a promising note, from a rational point of view, with the entry for 19 July 4241 B.C. as the precise date the good Dr. Ploetz proposed for the introduction of the calendar in Egypt. But it, too, rapidly went downhill through the whole sorry list of massacres and delusions, and ended with an entirely unprophetic but appropriate entry, for 27 September 1934, which ran as follows: "Declaration by England, France, and Italy, guaranteeing the integrity and independence of Austria." I can well believe the famous story about another boy, some 15 years earlier but in a school not far from mine. "You are a clever chap," the history teacher said to Viki Weisskopf, "but you don't know any dates!" "Oh, I do," he replied. "I know all the dates; I just don't remember what happened on them."

II. THE BIFURCATED MAPS

I have of course been a little hard on William Langer, a distinguished diplomatic historian, who later became a valued colleague of mine, and who, perhaps partly because of my teasing, did put a good deal more science into the last edition he edited. But the main point I wish to make still stands: wherever their schooling may take place, young people encounter, through the historically oriented set of courses as usually taught, a view of the accomplishments and destiny of mankind that almost celebrates the role of passionate unreason. And if the student is one of the relatively few who also takes substantial science courses, he or she will encounter from that direction a very different picture of mankind's interests and attainments. Indeed, the opposition between these stories is so great that it must seem to many students that there are really two different species involved.

Let me put it in terms of an image from my own schoolroom in Vienna, an image surely not qualitatively different from the one you carry in your mind, or which, *mutatis mutandis*, is being found even today in your town's school. The curriculum at our Gymnasium emphasized history, literature, and ancient languages, a triad that merged into one message. Latin and Caesar's wars; Greek and the Iliad; medieval German and the Nibelungenlied; the chief tragedies of German theater, the Bible, the Edda sagas, and the painstaking probing of the ever-unfinished sequence of historic battles: I must confess it was a powerful and blood-stirring brew—but with only occasional traces of the rational processes.

On the other hand, in our science classes we encountered an entirely different universe. Here was the finished and apparently unchanging product of distant and largely anonymous personages, unchallengeable monuments to their inexorable rationality—but with only occasional traces of historic development. Just as the historian neglects science—Richard Hofstadter said of the historian "he may not disparage science, but he despairs of it"—the scientist is silent about history—the record, a scientist put it to me once, of the errors of the forgotten dead.

It was a cultural schizophrenia which I could not formulate, but which I also could not dismiss. I can capture it best by recalling two very different maps that were hanging in front of my class, to stare at and wonder about, year after year. You probably saw them, too. On the left side was always a geopolitical map of Europe and Asia. (America and most of Africa were evidently on some other planet.) In some storage closet there must have been a whole set of such maps because the left map was regularly changed, a new one for each period, and with each change we students could see the violent, spasmodic, unpredictable pulsation of shapes and colors in the wake of the thrilling story of conquests. On the right side was a very different map—the Periodic Table of the elements: the very embodiment of empirical, testable, reliable, and ordered sets of truths. That map was never changed, although there was a rumor in the benches that some of the blank spaces were being filled in, and even that a previous student of that very Gymnasium, a man named Wolfgang Pauli, had shown that the different features of the table were the consequence of some underlying great idea, rather than some accident on which nature had settled.

To the mind of the child exposed to these two maps, the utter differences between them and thus between the subjects they represented were so profound as to seem unbridgable. On the left side, the forces shaping history were the four horsemen of the Apocalypse. On the right side the forces shaping science were, to use modern terms, the four forces of physics. To the young mind, it seemed like a division that demanded some sort of decision. We know from the autobiographical notes of scientists that this dichotomy can help to focus a career choice. In Einstein's famous remarks, it is the choice, in early youth, between a world "dominated by wishes, hopes, and primitive feelings," and on the other hand, "this huge world which exists independently of us human beings," the contemplation of which "beckoned like a liberation." It is only much later in life that it dawns on such a student at what great costs this separation was being nurtured, that these two kinds of destiny are in fact intertwined, that these two developments stem from two potentials within the same person.

In the meantime, however, students in our classes were left to wonder what the moral point of this bifurcated education would be. Concerning our personal destinies there was in fact a sharp division of opinion between our parents, on one side, and our history books as well as those few teachers who had any interest in discussing such matters, on the other. The parents' theory was that the purpose of the curriculum was chiefly to prepare for the school's final examination without which one could not go on to university. However, our physical education teacher spoke explicitly for the view that presented a different scenario for us: We would be there to fight one day, to revenge the loss of our territories in World War I, and of our honor in the treaties of Versailles, Trianon, and St. Germain. We were being readied to change that map on the left once more.[3]

The great historians we studied seemed, from that second point of view, useful preparations. For example, the father of historical writing, Herodotus of Athens in the Age of Pericles, declares the aim of his great book on the history of Greece to preserve, as he puts it, "the great and wonderful actions of the Greeks and the barbarians [from] losing their due meed of glory." What he means is set forth right at the start. A certain Gyges, upon being made King of Sardis, "made inroads on Miletus and Smyrna . . . . Afterwards, however, although he reigned for 38 years, he did not perform a single [further] noble exploit. I shall therefore make no further mention of him, but pass on to his son and successor . . . Ardys. This Ardys took Priene, and he made war upon Miletus." Our class quickly got the idea that here was an altogether more glorious type. Even better was his son Alyattes who, Herodotus says, had inherited from his father that war with Miletus, and who "performed other actions very worthy of note." Herodotus tells us one of these: In warfare, "he cut down and utterly destroyed all the trees and all the corn throughout the land and then returned to his own dominion . . .. The reason that he did not demolish also the buildings was that the inhabitants might be tempted to use them as homesteads from which to go forth to sew and till their land; and so each time that Alyattes invaded the country he might find something to plunder." Indeed, worthy of note.

Turning to a more recent work, we found that Georg Wilhelm Friedrich Hegel, in his great *Philosophy of History*, struggled with the meaning of history and concluded that "the final cause of the world at large, we allege to be the consciousness of its own freedom on the part of spirit, and *ipso facto* the reality of that freedom." I must confess that this formula, when presented in history class, was not easy to unpuzzle; but it had a nice ring to it. History as the evolution of freedom seemed an appealing idea. But just at that point in our studies, this train of thought was deprived of a good deal of its credibility when one Friday evening in

March, the portion of the map of Europe showing Austria turned suddenly brown, and our history teacher, like many teachers in the other subjects, turned up in Nazi regalia on the following Monday.

Hegel also had said, quite correctly, that the most profoundly shaping experience on the mind of the West was Greece of the Homeric Period. The reading of the *Iliad* was meant to be our most permanent memory; I must confess I like it best of all our classes. Just for this reason let me use it further to crystallize my point. You recall the early scene on the beach before Troy. Agamemnon, leader of the Greeks who are laying siege, has lost his own mistress, robs Achilles of his, and thereby launches 1000 pages of dactylic hexameter. Agamemnon explains himself in a way which Homeric Greeks accepted: "Not *I* was the cause of this act, but Zeus." It is the habit and prerogative of the Gods to put *atē* (temptation, infatuation, a clouding of consciousness) into a person's understanding. "So what could I do? The Gods will always have their way." Achilles, the outraged victim of Agamemnon's action, takes the same view. Without Zeus, he agrees, Agamemnon "would never have persisted in rousing the *thumos* (passionate, violent response) in my own chest."

In fact, throughout the *Iliad*, temptations and infatuations, *atē*, and passionate and violent response, *thumos*, are the chief forces motivating human action. As E. R. Dodds explains in the fine book *The Greeks and the Irrational*, *atē* is, in fact, a partial, temporary insanity, "and, like all insanity, it is ascribed not to physiological or psychological causes, but to the external, 'daemonic' agency." Only very rarely does one encounter in the *Iliad* a glimpse of rationality—dispassionate reason sensitive to long-range consequences—as a motivating force. This role falls to Athena, the goddess of good counsel, who occasionally intervenes in the brawl and carnage (even at the risk of conflict of interest, since on the side she is also a deity of war). There is the famous scene in Book I when Athena catches Achilles by the hair and warns him not to strike Agamemnon. She is visible to Achilles alone and persuades him to put back his sword. For a moment, there is sanity. But on the whole, the glorious poem takes place against the background of a dark, archaic world dominated by will and force, dreams and oracles, blood lust and atonement, portents and magical healing, orgiastic cults, superstitious terror, and the obbligato of reckless massacres—the world governed by the irrational self, the *thumos*, the strong force that surfaces today as tribalism, racism, and the longing for combat.

After the Homeric Age—rather precipitously, as history goes—the landscape does change. It is the period identified as the Greek Elightenment, during which there was, in a phrase Dodds quotes approvingly, a "progressive replacement of mythological by rational thinking among the Greeks." The rise of this first enlightenment, in the 6th century B.C., coincides with the first stirrings of Western science as we understand it.

There were bound to be enthusiastic mistakes; for example, Protagoras, an early Sophist, has gone down in history as perhaps the first optimist who thought that virtue could be taught, that history could be cured, that intellectual critique alone could rid us of "barbarian silliness" and lift us to a new level of human life. But at least his intention was right. Science, and rational thought which produced science, are beachheads in the soul that otherwise would be largely given over to *atē* and *thumos*. The existence of such beachheads allows one to hope for a change in the balance of potentials in the individual and, therefore, in the balance of forces that have raged over the geopolitical map since prehistoric times.

I submit that this fact defines, today more than ever, an essential part of the task of all who claim to be teachers. To neglect it is to invite peril. The subsequent history of Greece itself reinforces this point. Around 430 B.C., at the end of the reign of Pericles and the beginning of the Peloponnesian War which finished with Athens' surrender and Sparta's triumph, that first Age of Enlightenment gave way. Teaching astronomy, or expessing disbelief in the supernatural, now could have grave consequences. There followed some thirty years of trials for heresy, with victims such as Anaxagoras, Socrates, Protagoras, perhaps also Euripides, and of course an unknown number of less prominent ones. In the long series of conflicts between reason and passion, Athena, the Weak Force, had lost.

One of the causes for this turn of events appears to be that, from the late period of Plato onward, intellectuals situated themselves not *in* but *beside* society. Dodds writes: "As the intellectuals withdrew further into a world of their own, the popular mind was left increasingly defenseless . . . ; and, left without guidance, a growing number relapsed with a sigh of relief into the pleasures and comforts of the primitive." Greece saw again a great rise in cults, in magical healing practices, in astrology, and other familiar symptoms. It ushered in the long decline or, to give its proper name, the "Return of the Irrational." It was as if the bicameral mind had become aware of its rational strength—and been frightened by the possibility of freedom from the death dance of history, and freedom from the external gods to whom one could spin off the responsibility for the excesses of *atē* and *thumos*. (I am quite aware that a group of philosophers, from Nietzsche to Spengler, from Husserl to our day, puts the blame on the contrary on science itself, on what they call "excesses" of the rational. Their arguments are saved from dismissal as utter absurdity by the unhappy fact that science, too, has frequently lent itself to be a weapon in the service of our Dionysian and antihuman drives.)

The decline of Greece shows parallels with our present predicament. We are at the tail end of the second experiment with rationalism, the fruit of the scientific revolution of the 17th century and the era of enlightenment that followed. That schoolroom I described, and all the others across the globe, may really have functioned as a trap designed to keep us from escaping a destiny that is archaic except for its modern, much larger scale. The most persuasive evidence that the human mind has the power to progress, individually and cumulatively, from ignorance and confusion to sensible, testable, shareable world conceptions—the kind of triumph of man's rational potential of which his science is eloquent testimony—was all but sabotaged by the method of presentation, itself an institutionalization of our reluctance to honor the imperatives of sound reason. Indeed, the very facts of science which we had to memorize seemed the work of some deity that plants them in its passive victim, even as Zeus planted the *thumos* in Agamemnon. Never once was the liberating idea presented to us that the findings and very methods of science are the results of an historical process by which mere humans seek sense and expose nonsense, and that the potential for this process is in each of us.

III. HELPING ATHENA

All that was long ago. Many have worked hard on improving the effectiveness of educators. But the admonition of Max Weber, in his magisterial "Science as a Vocation," has become no less urgent. Rational thought, he reminded his audience, has the moral function of leading to self-clarification, of helping the individual "to give himself an account of the ultimate meaning of his own conduct," and so to be able better to make decisive choices. Without it, life "knows only of an unceasing struggle of these various gods with one another ..., the ultimately possible attitudes toward life [remain] irreconcilable, and hence their struggle can never be brought to a final conclusion." Today, as we watch the reign of the irrational in world affairs, I find it hard to believe that we have succeeded in making a qualitative change for the better in the products coming from our classrooms. Moreover, in our century the would-be conquerors who are writing themselves into the pages of the new history books have learned how to hire and use for their bloody work students coming from our science classes. It is an ominous conjunction of science and history, making it that much more difficult—and essential—to hold on to the vision that the trajectory of history *can* bring us to a time when, at long last, the goddess Athena in our very soul wins out consistently over the dark passions.

This ancient aim (which Werner Jaeger, in his great work *Paideia: The Ideal of Greek Culture*, identified as the chief hope of education in antiquity) should now be a special concern of those who, through their own life's work, have learned how one distinguishes between fact and delusion, between the demands of eternal law and of internal longing; of those who care most to find out how things work and cohere. The immense authority of, say, an Oersted came of course from the painstaking and repeatable demonstration of a beautiful and useful discovery. The history of science can show that might comes from being right, rather than, as in the rest of history, more often than not the converse. Bringing science and history together in that kind of conjunction—in scholarly research and in the classroom, for scientists and for nonscientists—is one effective way to enlarge the beachhead of reason.

There are others, there have to be others; but it has been my motivating (and perhaps now no longer quite so perverse) view that this way helps focus a young mind exactly on the point where the confrontation should take place, between the habits responsible for the kaleidoscopic sequence of follies on one side, and, on the other, the kind of passionate yet sane thinking that shaped the development and testing of, say, the Table of the elements or of elementary particles.

I have no illusion that more chemistry, more physics, more mathematics will, by themselves and soon, produce wise leaders and wise followers. Protagoras *was* too simple. There is no quick cure for the barbarian silliness within us which shows up so grotesquely in the acts of the present-day Agamemnons, generalissimos, premiers, shahs, and ayatollahs. We must also not be misunderstood to be defending some inevitable benignancy, purity, and progressiveness of science. Paradise will not come upon invoking the name and deeds of Mendeleyev or Wolfgang Pauli. And yet, enough of us must act nevertheless as if something of this sort can happen eventually; for otherwise it will not change. As we, and the future teachers who pass through our hands, face those young students, there is the opportunity to assert and demonstrate the rational powers of Athena as a complementary and balancing element in the productive life of the human spirit.

The risk of failure is high in all educational efforts, and there are always other, more immediately rewarding things one might do instead. But it is a risk worth taking. For those young students in the schoolroom, wondering about the forces that grip the world map and soon caught up themselves in its convulsions—those were you and I; those are now our children; and those should not have to be our children's children, forever.

ACKNOWLEDGMENTS

In retrospect, I am impressed how greatly most of the things I have tried to do depended at crucial points on the willingness of other persons to take a risk on my behalf. This has certainly been true in my work in education, which is the chief subject of this response. Because we are in a time of retrenchment with respect to the support of education, attention should be drawn to the continuing need, especially for those who can afford to do it, to take risks on behalf of people and ideas. On this appropriate occasion, I must mention a few to whom I am especially indebted in this regard.

At Wesleyan University, where I went for my senior year, Professor Walter G. Cady let me be his research assistant in crystal physics, and Professor Vernet Eaton his teaching assistant, thereby giving me my first jobs at a point when my qualifications for these must have been far less obvious than my need and ignorance. Not long afterwards, at Harvard, the same gamble was taken first by Professor Edwin C. Kemble, whose Oersted Medal citation some years ago might well have expanded on his role of providing a splendid example for those of us who taught under and with him while at the same time giving full freedom to develop our own ideas. Professor P. W. Bridgman then agreed to let me do my thesis in his laboratory even though, or perhaps because, the problem I proposed (molecular relaxation at high pressures) was really outside his current work, and even though it threatened to introduce electronic equipment into what had been essentially a dc laboratory.

The next example of risk taking was by a company called Addison-Wesley, then a job printer in Cambridge, which had decided to become a science book publisher. Their new editor, Warren Blaisdell, took a chance when he signed me up for a physics text based on the storyline provided by the history of physics at a time when this was regarded as a very unusual, not to say perverse, idea. And sure enough, when the book was published in 1952, there was for a time only one adoption (mine). Luckily for the company's solvency, the other two authors on their first list of physics texts were Francis Sears and Herbert Goldstein.

In the early 1960s, I had the good luck to meet James Rutherford, then working on his doctorate, and together with Fletcher Watson I encouraged him in his plan to do a physics book that would bring to schools the same point of view about science that had motivated my college text. A little money was needed to free Jim to work on this. His inquiry about funding received a brusk but at least quick

refusal from the National Science Foundation—as it happens, from the very same division that Jim has been heading since 1977. Happily, we quickly found persons in the Carnegie Corporation who were willing to take the risk. Not long afterwards, and now in good part at the urging of the NSF, this pilot effort was greatly expanded in 1964 and became the Project Physics Course development, in which literally hundreds of scientists and teachers helped us over an agonizing and exhausting five-year period. Even then, however, it would not have survived the repeated threats to its funding without the personal intervention of a few friends of the Project, above all I. I. Rabi.

The person who next took a risk upon himself was Vincent Alexander who, as head of the School Science Department at Holt, Rinehart and Winston Publishing Company, recommended commercial publication of the course materials. In consequence, since December 1970, a wide range of printed and other components of the course have been in use in U.S. high schools, as well as colleges, and also abroad in local adaptations from Japan and Australia to Brazil and Italy. It is a real pleasure to watch the flowering of these results, though as every gardener knows, it brings with it continuing labor. The third revised U.S. edition is being printed now, and on returning from this meeting I shall probably find a stack of page proofs waiting on my desk.

Let me close my acknowledgment of the risks others have taken on behalf of ideas for which you have kindly honored me by mentioning here the role of Elmer Hutchisson. When he was the director of the American Institute of Physics, a predecessor of William Koch, he encouraged the bold step of starting, in the AIP building itself, an archive, library, and research effort dedicated to the history of recent and current physics. This became the Niels Bohr Library and Center for the History of Physics, now in the able hands of Spencer Weart and Joan Warnow. It was the model for similar facilities set up since by other scientific societies. But there were long periods when the continued existence of the Center was in doubt from one meeting of the AIP Governing Board to the next, and the decision seemed to depend on whether my Eastern Airline shuttle would make it through the snow, or whether a few early friends of the idea, such as Fred Seitz, would stay to the end of the Board meeting. But all that is now passed. And thanks to an endowment fund drive initiated by Emmanuel Piore and continued by Fred Seitz—to whom I urge you to send your contributions!—we shall be able to assure the continued life of a valuable resource for documenting the rise and achievement of our profession.

[1]Shortly thereafter, he also added that the action between magnet needle and current loop is reciprocal, a discovery usually associated with Ampère, who independently published it later. For good accounts of Oersted's work and motivation, see L. Pearce Williams' account in the *Dictionary of Scientific Biography;* Kristine Meyer, "The Scientific Life and Works of H. C. Ørsted," in *H. C. Ørsted, Naturvidenskabelige Skrifter*, edited by K. Meyer (Host, Copenhagen, 1920), Vol. 1, pp. XIII–CLXVI; Robert C. Stauffer, "Speculation and Experiment in the Background of Oersted's Discovery of Electromagnetism," Isis **48**, 33–50 (1957); Bern Dibner, *Oersted and the Discovery of Electromagnetism* (Burndy, Norwalk, 1961); and Barry Gower, "Speculation in Physics: The History and Practice of Naturphilosophie," *Studies in History and Philosophy of Science* (Pergamon, New York, 1973), Vol. 3, pp. 301–356.

[2]For a brilliant analysis of the changing tasks of historians, see Judith N. Shklar, "Learning without Knowing," Daedalus **109**(2), 53–72 (1980), from which the last two quotations are taken.

[3]For a faithful description of the system of education in a Gymnasium of Vienna by an exact contemporary, see Egon Schwarz, *Keine Zeit für Eichendorff: Chronik unfreiwilliger Wanderjahre* (Athenäum Verlag, Königstein, 1979), pp. 11–18.

"The Discovery of Fission". **Otto Frisch and James Wheeler. Physics Today, November 1967, p. 43.**

The Discovery of Fission

Initial formulations of nuclear fission are colored with the successes, failures and just plain bad luck of several scientists from different nations. The winning combination of good fortune and careful thought made this exciting concept a reality.

by Otto R. Frisch and John A. Wheeler

Otto R. Frisch, professor of natural philosophy (physics) at Cambridge University, England, did research in Berlin (1927–30), Hamburg (1930–33), London (1933–34), Copenhagen (1934–39) and Birmingham (1939–40). During the war he worked on the A-bomb at Los Alamos. He was first to observe energy liberated in the fission of a single uranium nucleus.

John A. Wheeler, one of the first American scientists to concentrate on nuclear fission, worked at the U. of Copenhagen in 1934 as a National Research Fellow with Niels Bohr. Wheeler received his PhD in physics at the Johns Hopkins University prior to his research in Copenhagen. In 1938 he joined Princeton's physics department, where he remains active.

How It All Began

by Otto R. Frisch

THE NEUTRON was discovered in 1932. Why, then, did it take seven years before nuclear fission was found? Fission is obviously a striking phenomenon; it results in a large amount of radioactivity of all kinds and produces fragments that have more than ten times the total ionization of anything previously known. So why did it take so long? The question might be answered best by reviewing the situation in Europe from an experimentalist's point of view.

Research in Europe

In Europe there were few laboratories in which nuclear-physics research was conducted, and I think the word "team" had not yet been introduced into scientific jargon. Science was still pursued by individual scientists who worked with only one or two students and assistants.

Paris harbored some of the most active research laboratories in Europe. It is the city in which radioactivity had been discovered and where Madame Curie was working until her death in 1934. She still dominated the situation: Techniques were quite similar to those used at the turn of the century; that is, ionization chambers and electrometers. This state of affairs is good enough for performing accurate measurements on natural radioactive elements, but it is not really adequate for much of the work on nuclear disintegration. Madame Curie had little respect for theory. Once, when one of her students suggested an experiment, adding that the theoretical physicists next door thought it hopeful, she replied, "Well, we might try it all the same." Their disregard of theory may have cost them the discovery of the neutron.

Cambridge is the second place worthy of discussion. Ernest Rutherford, whose towering personality dominated Cambridge research, had split atomic nuclei in 1919; since 1909 he had, in fact, been keenly concerned with the observation and counting of individual nuclear particles. He first introduced the scintillation method and stuck firmly to it. His great preference was for simple, unsophisticated methods, and he possessed a strong distrust of any complicated instrumentation. Even in 1932, when John Cockcroft and Ernest Walton first disintegrated nuclei by artificially-accelerated protons, they used scintillations to detect the process. By that time Rutherford had realized that electronic methods of particle counting must be developed. The reason was that the scintillation method clearly had its shortcomings. It did not work for very low or high counting rates and was not really reliable. This deficiency was highlighted by the results that came from the third laboratory I want to mention—Vienna.

Vienna is where I began my career and it was in those days a sort of *en-*

fant terrible of nuclear physics. Several physicists were claiming that not only nitrogen and one or two others of the light nuclei could be disintegrated by alpha particles but that practically all of them could and did give many more protons than anybody else could observe. I still do not know how they found these wrong results. Apparently they employed students to do the counting without telling them what to expect. On the face of it, that operation appears to be a very objective method because the student would have no bias; yet the students quickly developed a bias towards high numbers because they felt that they would be given approval if they found lots of particles. Quite likely this situation caused the wrong results along with a generally uncritical attitude and considerable enthusiasm over beating the English at their own game.

I still remember when I left Vienna at just about that time (after having escaped the duty of counting scintillations). My supervisor, Karl Przibram, told me with sadness in his voice, "You will tell the people in Berlin, won't you, that we are not quite as bad as they think?" I failed to persuade them.

Germany had nuclear-physics research in several places. The team of Otto Hahn and Lise Meitner, which had been one of the first groups to study radioactive elements, had at that time separated to carry out independent research. Hahn was working on various applications of radioactivity for the study of chemical reactions, structures of precipitates and similar subjects, whereas Lise Meitner was using radioactive materials chiefly to elucidate the processes of beta and gamma emission and the interaction of gamma rays with matter.

In addition, Hans Geiger was in Germany. He had been with Rutherford from 1909 onwards, in the early days before the nucleus was discovered. Rutherford felt uncertain about the scintillation method and asked Geiger to develop an electric counter to check on it. But as soon as Rutherford saw that the two gave the same results, Rutherford returned to the scintillation method, which appeared to be simpler and more reliable when used with proper precaution. Geiger went back to Germany and perfected his electric counters, and in 1928, together with a student named W. Müller, he developed an improved counter that could count beta rays. Earlier counters were inadequate for this purpose, and scintillation methods were also incapable of detecting beta rays. However the new counters were still very slow because the discharge between the central wire and the cylindrical envelope was quenched by a large resistor of many megohms placed in the circuit; consequently the counting rate was limited to numbers not much greater than with the scintillation method. Even at a few hundred particles a minute there were quite large corrections to be applied.

Walther Bothe was the first to use the coincidence method, both in an attempt to do something about cosmic rays and also for measuring the energy of gamma rays by the range of the secondary electrons they produced. This was really the first reliable method for measuring the energy of weak gamma radiations.

Until 1932, the only source of particles for doing atomic nuclear disintegration was natural alpha particles: either polonium, which was difficult to come by (in fact one practically had to go to Paris) or sources of one of the short-lived decay products of radium, which were very clean but were short-lived and usually had lots of gamma radiation.

The year of discovery

But in 1932, that *annus mirabilis*, not only the neutron was discovered but two other developments took place. In the US Ernest O. Lawrence made the first cyclotron that showed promise of being useful, and in England Cockcroft and Walton built the first accelerator for protons capable of producing nuclear disintegrations. I need not state that this was the beginning of an enormous development; most of nuclear physics as we know it would have never come about without at least one of those two instruments. But the interesting thing is that they played practically no role in that narrow thread that led to the discovery of nuclear fission.

I do not want to dwell on the discovery of the neutron very much because it was discussed in several interesting lectures in 1962 at the History of Science Congress held in Ithaca, New York. The published proceedings contained interesting contributions by Norman Feather and Sir James Chadwick, who showed that the neutron was discovered in Cambridge, not simply by chance with everybody else having done the groundwork, but because a search for the neutron had been going on in Cambridge (admittedly with wrong ideas). The people at Cambridge were keyed up for this discovery. They had made one observation that was important and that tends to be overlooked: H. C. Web-

$$(1)\ {}_{92}\mathrm{U} + n \to ({}_{92}\mathrm{U} + n) \xrightarrow[\text{10 Sek.}]{\beta} {}_{93}\mathrm{EkaRe} \xrightarrow[\text{2,2 Min.}]{\beta} {}_{94}\mathrm{EkaOs} \to$$
$$\xrightarrow[\text{59 Min.}]{\beta} {}_{95}\mathrm{EkaIr} \xrightarrow[\text{66 Std.}]{\beta} {}_{96}\mathrm{EkaPt} \to$$
$$\xrightarrow[\text{2,5 Std.}]{\beta} {}_{97}\mathrm{EkaAu}\ ?$$
$$(2)\ {}_{92}\mathrm{U} + n \to ({}_{92}\mathrm{U} + n) \xrightarrow[\text{40 Sek.}]{\beta} {}_{93}\mathrm{EkaRe} \xrightarrow[\text{16 Min.}]{\beta} {}_{94}\mathrm{EkaOs} \to$$
$$\xrightarrow[\text{5,7 Std.}]{\beta} {}_{95}\mathrm{EkaIr}\ ?$$
$$(3)\ {}_{92}\mathrm{U} + n \to ({}_{92}\mathrm{U} + n) \xrightarrow[\text{23 Min.}]{\beta} {}_{93}\mathrm{EkaRe}\ ?$$

COMPUTATIONS, indicating chains of radioactive elements, were published in a 1938 *Die Naturwissenschaften* article by Hahn, Meitner and Strassmann. —FIG. 1

ster showed that those queer penetrating rays that beryllium emitted when alpha particles fell on it were more intense in the forward direction than in the backward direction. This result was quite incomprehensible if the radiation were gamma rays as everybody believed. Even the French physicists Curie and Joliot shared that belief in the teeth of all theoretical predictions. Then Chadwick's experiment showed clearly that the mysterious radiation consisted of particles having approximately the mass of the proton. There was a bit of confusion at the time because the word "neutron" had been used by Enrico Fermi and Wolfgang Pauli to indicate the particle that later came to be called the "neutrino."

After the neutron was discovered, there was of course a certain rush of activity, but nobody knew quite what to do. Neutrons were rather few in number. They were, after all, secondary products of nuclear disintegration. With only natural alpha sources available at first, neutron production was low.

Moreover the main instrument for detection was essentially the cloud chamber. With cloud chambers only a limited number of tracks due to neutrons could be found. And it was slow work to make any sense out of the few detected tracks of recoil nuclei. Leo Szilard once joked that if a man suddenly does something unexpected there is usually a woman behind it, but if an atomic nucleus suddenly does something unexpected, there is probably a neutron behind it.

Electronic counting methods had only just been developed; largely as a reaction to the wrong results coming out of Vienna that nobody else could confirm, it had been decided that it really was necessary to build electronic amplifiers and counters. Actually the Viennese themselves started that kind of work but were not very successful. The work was also started in Switzerland with some success by Hermann Greinacher. Yet I think the main thread that led to the development of decent counters took place in England, where Charles Wynn-Williams used proper screening and tubes with low noise level etc. to produce electronic counters. Nevertheless those counters, although Chadwick had used them with good effect to pin down the neutron, were still too noisy to be of much use.

GREAT AND GOOD FRIENDS. Lord and Lady Rutherford (left) with Niels and Margrethe Bohr in Rutherford's garden. The photograph was taken about 1930.

Artificial radioactivity

Things really got moving when, in 1934, artificial radioactivity was found by Curie and Joliot. I think they must have been very happy to have made up for their failure to spot the neutron two years previously. Almost to the day two years previously both discoveries came out in the middle of January. They had known for many months before that aluminum bombarded with alpha particles emits positrons, but it had never occurred to them that this might be a delayed process. They had only observed the positron during bombardment. Lawrence and his cyclotron people in California had made the same mistake. In fact they had noticed that the counters misbehaved after the cyclotron was switched off. I am told that they built in special gadgetry so that the counters were automatically switched off together with the cyclotron! Otherwise they would have found artificial radioactivity before the French.

It is astonishing that nobody appears to have thought beforehand that the result of a nuclear disintegration might be an unstable nucleus although the existence of unstable nuclei had, of course, been known for thirty years or more. I have been told that, after the discovery, Rutherford wrote to Joliot and congratulated him on his discovery saying that he himself had thought that some of the resulting nuclei might be unstable, but had always looked for alpha particles only because he was not really interested in beta particles.

As soon as this work became known in January 1934 a lot of people rushed to repeat and extend the experiment. But most of them rushed in a straight line indicated by Curie-Joliot, bombarding other elements with alpha particles. (So did I in Blackett's laboratory in London.)

But in Rome Fermi at that time had already decided that nuclear physics was an important and interesting line, and he had started to set up some instrumentation. So when this discovery came along, he began working quite fast to see whether neutrons would form radioactive nuclei.

I remember that my reaction and probably that of many others was that Fermi's was a silly experiment because neutrons were much fewer than alpha particles. What that simple argument overlooked of course was that they are very much more effective. Neutrons are not slowed down by electrons, and they are not repelled by the Coulomb field of nuclei. Indeed, within about four weeks of the discovery by Curie and Joliot, Fermi published the first results proving that various elements did become radioactive when bombarded with neutrons. Only another month later he announced that bombarding uranium produced some new radioactivity that he felt must be due to transuranic elements. Because both on theoretical grounds (Coulomb barrier and all that) and as far as the experiments confirmed it, all heavier elements were known to absorb neutrons without splitting anything off. And so it was felt that must also be the case with uranium.

This work was of course considerably interesting to radiochemists. Several took it up, but once again, oddly enough, one false result started things really moving—a note by Aristid von Grosse, a German-born chemist working in the US, who thought one of these elements behaved like protactinium. He had done some of the early work with Hahn on protactinium soon after it was discovered in 1917; so his suggestion put Hahn and Meitner on their mettle. They felt protactinium was their own baby and they were going to check it. Lise Meitner persuaded Hahn to join forces again. They soon showed that von Grosse was wrong: It was not protactinium. On the other hand there were so many odd things there that they were captured by this phenomenon and had to go on. The results were most peculiar.

Figure 1 shows one of the tabulations indicating the chains of radioactive elements that Hahn and Meitner had thought identified them. They did not give new names to the transuranic elements that they thought they had identified, but they used the prefix "eka" to indicate that they were higher homologues of rhenium, osmium, etc. up to ekagold. Obviously, Hahn was excited to have a whole new lot of chemical elements to play with and to study their properties. Today, of course, these elements after uranium are known as neptunium, plutonium, americium etc., and are known to be chemically quite different from those that Hahn was studying.

LINKS IN THE CHAIN. Cockcroft (top) and Walton contributed to the new ideas when they disintegrated nuclei by artificially-accelerated protons.

Parallel chains

The results were astonishing for two reasons. In the first place, it appeared that there were three parallel series. And from the yields obtained they must all derive from uranium 238 or possibly one of them from 235 (which is already much rarer). So it looked as if there were at least two parallel chains of isomeric elements. This isomeric property had to be propagated all along the chain of beta disintegrations.

Nuclear isomerism was still fairly new in 1938, and its interpretation was not altogether clear. It had been suggested (as we now accept) that it was due to high angular momentum, but there were also proposals that it might be due to the existence of rigid structures inside nuclei. One could imagine that such a rigid structure might survive a beta decay and might influence the half-life of the subsequent product.

But then there was still the mystery of the great length of those chains. Uranium, after all, was not beta unstable itself. The other elements in that region never had more than two beta decays in succession; yet here four or five had been found. So Hahn the chemist was delighted by so many new elements, but Hahn the radiophysicist or radiochemist was rather worried about the mechanism that could account for them.

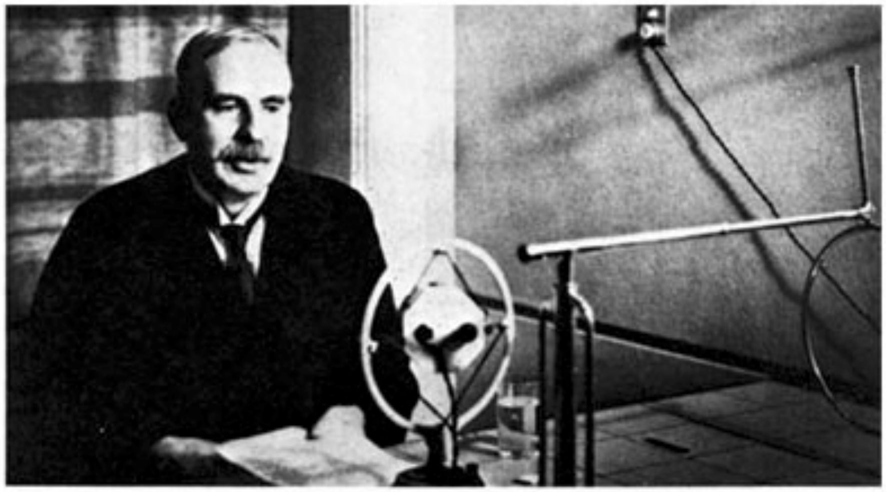
All this work was made difficult by the political situation in Germany. Hitler was in power and the institute had to play a delicate game of politics to prevent racial persecution from removing some of its personnel. In 1938, when Austria was occupied by the Nazis, Lise Meitner felt very insecure; rumors began to float around that she might lose her post and be prevented thereafter from leaving Germany because of her knowhow. A certain amount of panic resulted. Dutch colleagues offered to smuggle her to Holland without a visa. Thus she left Germany in the early summer of 1938, went from Holland for a brief stay in Denmark, and was offered hospitality by Manne Siegbahn at the Nobel Institute in Stockholm.

Near misses

After that, the team that had already brought Strassmann in with Hahn as a second chemist had to carry on without her. In the meantime some work had been started in Paris. It is interesting that they had a different angle. They were at first not so interested in the transuranic elements; but they realized that if thorium is bombarded with neutrons, one ought to find the beginning of the new and missing radioactive chain with the atomic weight 4n + 1. One realizes that the others, 4n, 4n + 2, 4n + 3, are all represented by the natural radioactive series. But the 4n + 1 was missing, and so Irene Curie, the daughter of Madame Curie, together with Hans von Halban, an Austrian, and Peter Preiswerk, a Swiss, set out to search for that series and published some work on it.

Later that team broke up because Halban came to Copenhagen and, for a time, worked with me on the study of slow neutrons. Irene Curie found a new collaborator in Pavel Savitch, a Yugoslav. They tried to disentangle the transuranic elements. Having realized that there was a great variety of different materials, Irene Curie had the good idea of selecting one of them simply by the high penetration of its beta rays. They covered their samples with a fairly thick sheet of brass and only studied the substance whose radiation penetrated. They did not realize that even that method might not select a single substance although the substance appeared to have a reasonably unique lifetime of 3.5 hours. From the chemical behavior they first thought it looked like thorium.

This work was checked by Hahn, who concluded that it was not thorium and wrote so to Paris. Curie and Savitch continued the work and in a later paper in the summer of 1938 acknowledged that the 3.5-hour substance was not thorium but behaved a bit more like actinium and even more like lanthanum. She had come very close indeed to the concept of nuclear fission but unfortunately did not state it clearly. She said that it was definitely not actinium and that it was quite similar to lanthanum, "from which it could be separated only by fractionation." But she did think it could be separated. The reason was probably that she still had a mixture of two substances; in that case of course one does effect a partial separation. Then this work was in turn checked by Hahn and Strassmann who discovered radioactive products that behaved partly like actinium, partly a bit like radium.


RUTHERFORD was the first to use scintillation methods to detect particles.

There was another near miss at about the same time: Gottfried von Droste, a physicist working with Lise Meitner, looked for long-range alpha rays from uranium during neutron bombardment. If he had supressed the ordinary alpha rays by applying a bias to the amplifier, he would not have failed to find fission. Unfortunately instead of using a bias he used a foil, and that foil was thick enough to stop not only uranium alpha rays but also the fission fragments; nor did he find any long-range alpha rays, which had to be there if radium or actinium isotopes were formed.

Then Hahn and Strassmann checked the chemical properties of this "radium" with care and found that they were identical with those of barium.

A propitious visit

This is where I came in because Lise Meitner was lonely in Sweden and, as her faithful nephew, I went to visit her at Christmas. There, in a small hotel in Kungälv near Göteborg I found her at breakfast brooding over a letter from Hahn. I was skeptical about the contents—that barium was formed from uranium by neutrons—but she kept on with it. We walked up and down in the snow, I on skis and she on foot (she said and proved that she could get along just as fast that way), and gradually the idea took shape that this was no chipping or cracking of the nucleus but rather a process to be explained by Bohr's idea that the nucleus was like a liquid drop; such a drop might elongate and divide itself. Then I worked out the way the electric charge of the nucleus would diminish the surface tension and found that it would be down to zero around $Z = 100$ and probably quite small for uranium. Lise Meitner worked out the energies that would be available from the mass defect in such a breakup. She had the mass defect curve pretty well in her head. It turned out that the electric repulsion of the fragments would give them about 200 MeV of energy and that the mass defect would indeed deliver that energy so the process could take place on a purely classical basis without having to invoke the crossing of a potential barrier, which of course could never have worked.

We only spent two or three days together that Christmas. Then I went back to Copenhagen and just managed to tell Bohr about the idea as he was catching his boat to the US. I remember how he struck his head after I had barely started to speak and said: "Oh, what fools we have been! We ought to have seen that before." But he had not—nobody had.

Lise Meitner and I composed a paper over the long-distance telephone between Copenhagen and Stockholm. I told the whole story to George Placzek, who was in Copenhagen, before it even occurred to me to do an experiment. At first Placzek did not believe the story that these heavy nuclei, already known to suffer from alpha instability, should also be suffering from this extra affliction. "It sounds a bit," he said, "like the man who is run over by a motor car and whose autopsy shows that he had a fatal tumor and would have died within a few days anyway." Then he

THE JOLIOT-CURIES discovered artificial radioactivity.

WIDE WORLD PHOTOS

CENTRAL FIGURES in the discovery were Otto Hahn and Lise Meitner, here shown in front of the institute that bears their names

said, "Why don't you use a cloud chamber to test it?" I did not have a cloud chamber handy and thought it would be difficult anyway. But I used an ionization chamber and it was a very easy experiment to observe the large pulses caused by ion fragments.

I do not think chronology means very much and certainly cannot claim any particular intelligence or originality. I was just lucky to be with Lise Meitner when she received advance notice of Hahn's and Strassmann's discovery. Then I had to be nudged before I did the crucial experiment on 13 January. By that time our joint paper was nearly written. I held it back for another three days to write up the other paper, and then they were both sent to *Nature* on 16 January but published a week apart. In the first paper I used the word "fission" suggested to me by the American biologist, William A. Arnold, whom I asked what one calls the phenomenon of cell division.

The second paper also contained a suggestion from Lise Meitner that fission fragments emerging from a bombarded uranium layer could be collected on a surface and their activity measured. The same thought independently occurred to Joliot, and he successfully did this experiment on 26 January. About that same time the news reached the US; what happened then is discussed by Wheeler.

Serendipitous searches

To come back to my initial question: Why did it take so long before fission was recognized? Indeed, why wasn't the neutron found earlier? Rutherford thought about it and foretold some of its properties as early as his Bakerian lecture in 1920; but Joliot did not read it, expecting a public lecture to contain nothing new! When Curie and Joliot found that the "beryllium radiation" ejected protons from paraffin, they put it down to a kind of Compton effect of a very hard gamma radiation (some 50 MeV), ignoring the objections of theoretical physicists. The neutron was finally observed in Cambridge, where such a particle was expected and had been sought.

At the time the neutron was found in 1932 pulse amplifiers and ionization chambers were available for a facile detection of fission pulses. But that would have been too big a jump to expect. The liquid-drop model of the nucleus was born late; the compound-nucleus idea was conceived by Bohr only late in 1936. It would have been a stroke of genius to think of fission then, and nobody did.

The discovery of artificial radioactivity in 1934 was again a chance discovery; no one had looked for it except Rutherford, who looked in vain for alpha decay. And indeed the Berkeley team turned a blind eye when their counters "misbehaved." After the discovery there was a sheep-like rush to repeat the experiment with only the most obvious variation (I was one of the sheep). Only Fermi had the intelligence to strike out in a different and tremendously fruitful direction.

But then Fermi got on the wrong track: He felt sure that uranium, like other heavy nuclei, would obediently swallow any slow neutron that fell on it. He did make sure that the radioactive substances that were formed from it were different from any of the known elements near uranium. Ida Noddack, a German chemist, quite rightly pointed out that they might be lighter elements; but her comments (published in a journal not much read by chemists and hardly at all by physicists) were regarded as mere pedantry. She did not indicate how such light elements could be formed; her paper had probably no effect whatever on later work.

In the end it was good solid chemistry that got things on the right track. Irene Curie and Pavel Savitch came very close to it; only the presence of two substances with maliciously similar properties prevented them from establishing uranium fission before Hahn and Strassman finally accomplished it. □

Mechanism of Fission

by John A. Wheeler

In early january 1939 the Swedish-American liner, MS Drottningholm carried a short message across the stormy sea from Copenhagen to New York. This message symbolized the steady transfer of nuclear discoveries from Europe to the US that had been going on during the Hitler years.

Although these transfers were fateful for the US and the rest of the world, the act of relaying this particular message was simple: a few words spoken by Otto Frisch to Niels Bohr on the pier in Copenhagen and a few words spoken to Enrico Fermi and me by Bohr on the pier in New York. As a junior participator in the events that occurred then and in subsequent months, I shall relate the activities that led to the publication of a *Physical Review* paper by Bohr and me. In this paper we summarized the thoughts expressed in the message: the liquid-drop model that Frisch had applied to the mechanism of fission and the determinations of packing fraction that Lise Meitner considered when arriving at the first estimate of energy release in fission.

No one looking at such a novel process at that time could fail to call on everything he knew about nuclear physics to seek an interpretation. Fortunately the key ideas for unraveling the puzzle had already been developed. It may be appropriate to recall what had been learned about nuclear physics in the preceding half a dozen years.

Clues to the answer

1933 was a fruitful year for someone like me, who was just earning his doctor's degree. It was the year of the discovery of the neutron and Werner Heisenberg's great paper on the structure of nuclei built out of neutrons and protons. These discoveries made one feel that he might soon know as much about the nucleus as he already knew about the atom.

Encouraged by the vision that inspired so many young men, me included, at that time, I spent 1933–34 working with Gregory Breit, to whose insights I owe so much. He and the group of which I soon found myself a member accepted almost unconsciously the model of the nucleus of that day: neutrons and protons moving in a common self-consistent potential, closely analogous to the electric potential of the atom. "Unconscious" our acceptance of the model was, yes; but also shadowy. None of us took it too literally, especially not Breit, with his caution and insight. Thus he was always willing to consider alpha particles in the nucleus as well as neutrons and protons when that point of view made sense in considering a particular reaction. Breit also directed especial attention to areas of investigation as nearly free as possible of model-dependent issues. Thus much work was done on the penetration of charged particles into nuceli and how the cross section for a nuclear reaction depends on energy. The analysis of scattering processes in terms of phase shifts also received much attention.

With Breit's warm endorsement I spent the following year at Niels Bohr's institute in Copenhagen. Here I was initiated into the study of many new ideas, but nothing was more impressive in nuclear physics than the message that Møller brought back during the spring of 1935 from a short Easter visit to Rome: It told of Fermi's slow-neutron experiments and the astonishing resonances that he had discovered. Every estimate ever made before then indicated that a particle passing through a nucleus would have an extremely small probability of losing its energy by radiation and undergoing capture if the current nuclear model was credible. Yet, directly in opposition to the predictions of this model, Fermi's experiments displayed huge cross sections and resonances that were quite beyond explanation.

Of course a number of weeks went by before the most significant results of this discovery could be sorted out. Everyone was actively concerned, but no one more so than Bohr, who paced up and down in the colloquium and took a central part in discussions.

Liquid drops

The story of the development of the liquid-drop model and the compound-nucleus picture is a familiar one. What is not so clear and was certainly not evident at the time is the distinction between these ideas: (1) The compound-nucleus model shows, in essence, that the fate of a nucleus is independent of the mechanism by which it has been formed, and (2) the liquid-drop model is, so to speak, a special case of the compound-nucleus model, a particular way of making such a model of nuclear structure reasonable. Bohr proposed that the mean free path of nucleon is short in relation to nuclear dimensions instead of being long, as assumed in all previous estimates. This new idea made something like a liquid-drop model exceedingly attractive.

No one looking back on the situation from today's vantage point can fail to be amazed at "the great accident of nuclear physics"—the circumstance that the mean free path of particles in the nucleus is neither extremely short compared with nuclear dimensions (as assumed in the liquid-drop picture) nor extremely long (as assumed in the earlier model) but of an intermediate value. Moreover, all the marvelous detail of nuclear physics turns out to depend in such a critical way on the value of this parameter. As Aage Bohr and Ben Mottelson have taught us in recent years, no one could have predicted the precise one among many alternative regimes in which the phenomenology would actually lie from any advance estimate of the mean free path. Only observation could suffice! Knowing as little as one did in 1935 about the value of this decise parameter, still less about its cirticality, one had no option but to explore with all vigor the idea that the mean free path is very short.

The development of the liquid-drop model, which was applied to a variety of processes, took place in the hands of Fritz Kalckar and Niels Bohr in 1935–37. They applied it to a variety of processes. At the center of every such application stood the idealization of the compound nucleus, that is, the concept that a nuclear reaction occurs

STROLLING THINKERS, Fermi (left) and Bohr, are well known for their important applications and expansions of early ideas of nuclear fission.

in two well separated stages: First, the particle arrives in the nucleus and imparts an excitation; then in some way the nucleus uses that energy for radiation, neutron or alpha-particle emission or any other competing process.

Bohr brings the news

The message that Frisch gave Bohr as Bohr left Copenhagen opened up a new domain of application for this concept of the compound nucleus. By the time Bohr had arrived in New York he had already recognized that fission is one more process in competition with neutron reëmission and gamma-ray emission. Four days after his arrival he and Rosenfeld finished a paper summarizing this general picture of fission in terms of formation and breakup of the compound nucleus.

Rosenfeld had originally accompanied Bohr to Princeton for several months of work on the problem of measurement in quantum electrodynamics. During Rosenfeld's Princeton sojourn Bohr gave less than half a dozen lectures on that issue. Nevertheless, that and many other questions conspired to take much of his time. No one could go into his office without seeing the long list of duties and people he had to give time to. That list made it easy to appreciate the pleasure with which he came into my office to discuss the work that we had under way. We were trying to understand in detail the mechanism of fission and, not least, analyze the barrier against fission and the considerations that determine its height.

First of all, of course, we had to formulate the very idea of a threshold or barrier. How can there even be any barrier according to the liquid-drop picture? Is not an ideal fluid infinitely subdivisible? And therefore cannot the activation energy required to go from the original configuration to a pair of fragments be made as small as one pleases? We obtained guidance on this question out of the theory of the calculus of variations in the large, maxima and minima, and critical points. This subject we absorbed by osmosis from our environment, so thoroughly charged over the years by the ideas and results of Marston Morse. It became clear that we could find a configuration space to describe the deformation of the nucleus. In this deformation space we could find a variety of paths leading from the normal, nearly spherical configuration over a barrier to a separated configuration. On each path the energy of deformation reaches a highest value. This peak value differs from one path to another. Among all these maxima the minimum measures the height of the saddle point or fission threshold or activation energy for fission.

While we were estimating barrier heights and the energy release in various modes of fission, the time came for the fifth annual theoretical physics conference held in Washington on 26 Jan. Bohr felt a responsibility toward Frisch and Meitner and thought that word of their work-in-progress and their concepts should not be released until they had the proper opportunity to publish, as is the custom throughout science. Even though this was the situation, at the outset Rosenfeld did not appreciate all the complications and demands of Bohr's position. On the day of Bohr's arrival in the US Rosenfeld went down to Princeton on the train. (Bohr had an appointment later that day in New York.) Rosenfeld reported the new discovery at the journal club—the regular Monday night journal club—and of course everybody was very excited. Isidor I. Rabi, who was at the journal club, carried the news back to Columbia, where John Dunning started to plan an experiment.

Nevertheless, even on 26 Jan., Bohr was reluctant to speak about Frisch's and Meitner's findings until he received word that they had actually been published. Fortunately that afternoon an issue of *Die Naturwissenschaften*, which contained work by Hahn and Fritz Strassmann, was handed to him; thus he could tell about it. Of course everybody started his experiments. The first direct physical proof that fission takes place appeared in the newspapers of the twenty-ninth.

Shaping the theory

The analysis of fission led to the theory of a liquid drop and this in turn led back to a favorite love of Bohr, who, for his first student research work, experimented on the instability of a jet of water against breakup into smaller drops. He was quite familiar with the work of John W. Strutt, the third Lord Rayleigh. This work furnished a starting point for our analysis. However, we had to go to terms of higher order than Rayleigh's favorite second-order calculations to pass beyond the purely parabolic part of the nuclear potential, that is, the part of the potential that increases quadratically with deformation. We determined the third-order terms to see the turning down of the potential. They enabled us to evaluate the height of the barrier, or at least the height of the barrier for a nucleus whose charge was sufficiently close to the critical limit for immediate breakup.

Here we found that we could reduce the whole problem to finding a function f of a single dimensionless variable x. This "fissility parameter" measures the ratio of the square of the charge to the nuclear mass. This parameter has the value 1 for a nucleus that is already unstable against fission in its spherical form. For values of x close to 1, by the power-series development mentioned above one could estimate the height of the barrier and actually give quite a detailed calculation of the first two terms in the power series for barrier height, or f, in powers of $(1 - x)$. The opposite limiting case also lent itself to analysis. In this limit the nucleus has such a small charge that the barrier is governed almost entirely by surface tension. The Coulomb forces give almost negligible assistance in pushing the material apart.

ROSENFELD, with Bohr, summarized the idea of fission.

Between this case (the power series about $x = 0$) and the other case (the power series about $x = 1$) there was an enormous gap. We saw that it would take a great amount of work to calculate the properties of the fission barrier at points in between. Consequently we limited ourselves to interpolation between these points. In the 28 years since that time many workers have done an enormous amount of computation on the topography of the deformation energy as depicted over configuration space as a "base" for the topographic plot. We are still far from completing the analysis. Beautiful work by Wladyslaw J. Swiatecki and his collaborators at Berkeley has taught us much more than we ever knew before about the structure of this fission barrier and has revealed many unsuspected features for values of x that are remote from the two simple, original limits.

From fission barrier we turned to fission rate. All of us have always recognized that nuclear physics consists of two parts: (a) the energy of a process and (b) the rate at which the process will go on. The compound-nucleus model told us that the rate should be measured by the partial width of the nuclear state in question for breakup by the specified process.

Toward a simpler theory

How could we estimate this width? Happily, in earlier days, several persons in the Princeton community—among them Henry Eyring and Eugene Wigner—had been occupied by the theory of the rates of chemical reactions. Also we derived some useful information from cosmic-ray physics. Who does not recall the many detailed calculations Størmer and his associates made on the orbits of cosmic-ray particles in the earth's magnetic field? Fortunately Manuel Sandoval Vallarta and later workers were able to spare themselves almost all of these details. They had only to employ Liouville's theorem. It said that the density of systems in phase space remains constant in time.

The same considerations of phase space were equally useful for evaluating the rate of fission. It turned out that we could express the probability of going over the barrier as the ratio of two numbers. One of these numbers is related to the amount of phase space available in the transition-state configuration as the nucleus goes over the top of the barrier. We were forced to think of all the degrees of freedom of the nucleus other than the particular one leading to fission. All these other degrees of freedom are summarized in effect in the internal excitations of the nucleus as it passes over the fission barrier. In classical terms this concept is well defined. It is a volume in phase space completely determined by the amount of energy.

The other quantity, appearing in the denominator of the rate-of-fission expression, is linked with the volume of phase space accessible to the compound system. In all the complex motion short of actual passage over the barrier the ensemble of systems under consideration remains confined to the narrow band of energies, ΔE, defined by the energy of the incident neutron. What counts is this energy interval multiplied with the rate of change of volume in phase space with energy for the undissociated nucleus. The beauty of this derivation is the fact that these classical ideas lend themselves to direct transcription into quantum-mechanical terms. Thus the Wentzel-Kramers-Brillouin approximation taught us that volume in phase space determines the number of energy levels. So we concluded that the width—the desired width measuring the probability for fission—is given by a ratio in which the numerator is the number of states accessible to the transition-state nucleus as it is going over the barrier, that is, the number of

PLACZEK was helpful in formulating theories of fission.

states of excitation other than motion in the direction of fission. In the denominator appears the spacing between nuclear energy levels, divided by 2π. Thus we had attained the most direct tie with experimentally interesting quantities. The formula that was obtained in this way for the reaction rate, or the level width, applied to a wide class of reactions as well as to fission, and was more general than any that had previously been available in reaction-rate theory. The new formula gave considerable insight into the rate of passage over the fission barrier.

At this particular point it is interesting to note the caution with which Bohr adopted the formula. He would come in every other day or so, and we would go at it for perhaps a half a day, trying out first this approach and then that approach. But his supreme caution was most evident when we wanted to interpret the number of levels accessible in the transition state. Today that number is called "the number of channels," and we use it as a formula to describe the channel-analysis theory of fission rate. Also we apply similar channel-analysis considerations to other nuclear reactions. But at that time the idea that each one of these individual channels has in principle a definite experimentally observable significance was, for us, of dubious certainty. Still less did we appreciate, until the later work of Aage Bohr, the possibility that each individual channel would have its individual angular distribution from which one could determine the K values of that channel. The cautious phrase that was used in reference to that channel number appears in the following quotation: "It should be remarked that the specific quantum-mechanical effects which set in at and below the critical fission energy may even show their influence to a certain extent above this energy and produce slight oscillations in the beginning of the yield curve, allowing, possibly, a direct determination of the number of channels." Of course we know how later on in the 1950's these variations were observed by Lamphere and Green and others and how they led to direct measurement of the channel number.

Bohr's epiphany

The most important part of this Princeton period happened when I was not in direct touch with Bohr. One snowy morning he was walking from the Nassau Club to his office in Fine Hall. As a consequence of a breakfast discussion with George Placzek, who was deeply skeptical of these fission ideas, Bohr began struggling with the problem of explaining the remarkable dependence of fission cross section on neutron energy. In the course of the walk he concluded that slow-neutron fission is caused by U^{235} and fast-neutron fission by U^{238}. By the time he had arrived at Fine Hall and he and I had gathered together with Placzek and Rosenfeld, he was ready to sketch out the whole idea on the blackboard. There he displayed the concept that U^{238} is not susceptible to division by neutrons of thermal energy, nor is it susceptible to neutrons of intermediate energy but only to neutrons with energies of a million electron volts or more. Further, the fission observed at lower energies occurs because U^{235} is present and has a $1/v$ cross section for capture. We already knew experimentally that neutrons of intermediate energy undergo resonance capture. And, with the help of simple considerations, we could show that the resonance reaction of neutrons with uranium could not be due to U^{235}. We concluded this because we knew that the resonance cross section would exceed the theoretical limit given by the square of the wavelength if U^{235} were responsible for the resonance effect. So the resonance had to be due to U^{238}, and the very fact that the resonance neutrons did not bring about fission proved that U^{238} was not susceptible to fission by neutrons of such low energy. Thus if it was not susceptible at that energy, it would certainly not be susceptible at lower energies; consequently low-energy fission must be due to U^{235}.

A few days later, on 16 April, Placzek, Wigner, Rosenfeld, Bohr, myself and others discussed whether one could ever hope to make a nuclear explosive. It was so preposterous then to think of separating U^{235} that I cannot forget the words that Bohr used in speaking about it: "It would take the entire efforts of a country to make a bomb." He did not foresee that, in truth, the efforts of thousands of workers drawn from three countries would be needed to achieve that goal.

The theory of fission made it possible to predict in general terms how the cross section for fission would depend upon energy. In Palmer Physical Laboratory Rudolf Ladenberg, James Kanner, Heinz H. Barschall and Van Voorhies, just at the time we were working on the theory, actually measured the cross section of uranium in the region from two million to three million volts—and also the cross section for thorium, all of which fitted in with predictions. The same considerations of course made it possible to predict that plutonium 239 would be fissile. For this application of the theory we are especially indebted to Louis A. Turner. One started on the way that ultimately led to the giant plutonium project having only this theoretical estimate to light and encourage the first steps.

Spontaneous fission offered a most attractive application of these ideas in conjunction with the concept of barrier penetration. Another application dealt with the difference between prompt neutrons and delayed neutrons. In conclusion, nuclear fission brought us a process distinguished from all the other processes with which we ever dealt before in nuclear physics, in that we have for the first time in fission a nuclear transformation inescapably *collective* in character. In this sense fission opened the door to the development of the collective model of the nucleus in the postwar years. □

About This Exhibit

This exhibit is based on the educational package, *Moments of Discovery, Unit 1: The Discovery of Fission*, by Arthur Eisenkraft with Lilllian Hoddeson, Joan N. Warnow, Spencer Weart, and Charles Weiner and published by the American Institute of Physics in 1984.

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For further information

Further information on the history of physics can be obtained from the AIP's Center for History of Physics in College Park, MD.

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Niels Bohr, about 1927. Niels Bohr Institute, courtesy AIP Emilio Segrè Visual Archives.

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Ernest Walton, Rutherford, and John Cockcroft outside the Cavendish Laboratory,1932. United Kingdom Atomic Energy Authority, courtesy AIP Emilio Segrè Visual Archives.

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Lise Meitner about 1925 in Tubingen. AIP Emilio Segrè Visual Archives.

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Periodic table fragment.

Hahn's laboratory table used for fission experiments. Keystone.

Otto Robert Frisch, 1967. AIP Emilio Segrè Visual Archives.

Fig. 1—Frisch's drawing.

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Fig. 2—drawings of fission.

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Enrico Fermi in the 1930s. King Features Syndicate, Inc.

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John Dunning at the Columbia University cyclotron in the late 1930s. AIP Emilio Segrè Visual Archives.

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Frédéric and Irène Joliot-Curie at the Radium Institute in Paris, 1934. Radium Institute, courtesy AIP Emilio Segrè Visual Archives.

Lew Kowarski, 1969. AIP Emilio Segrè Visual Archives.

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Dunning at the Columbia University cyclotron, 1938. Smithsonian Institution Science Service Collection, courtesy AIP Emilio Segrè Visual Archives.

Fig. 3—chain reaction.

Szilard. Wide World Photos.

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Kowarski and F. Joliot-Curie, about 1947. AIP Emilio Segrè Visual Archives.

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The first self-sustaining chain reaction at the University of Chicago in 1942. Painting by Gary Sheahan. Copyright © Gary Sheahan.

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Herbert L. Anderson. "Early Studies of the Fission Process." Talk given 27 April 1967 at the Spring Meeting of the American Physical Society. (AIP Niels Bohr Library, Misc. Tapes: 5-67-3).

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