

THE
UNIVERSITY
OF CHICAGO
MAGAZINE



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Don Morris, AB'36
Editor
Jane Lightner
Editorial Assistant

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The University of Chicago
Alumni Association
5733 University Avenue
Chicago, Illinois 60637
(312) 753-2175
John S. Coulson, '36, President
Arthur Nayer, Director, Alumni Affairs
Ruth Halloran, Assistant Director
Lisa Wally, AM'68
Program Director
Regional Offices
1542 Riverside Drive, Suite F
Glendale, California 91201
(213) 242-8288
320 Central Park West, Suite 14A
New York, New York 10025
(212) 787-7800
1000 Chestnut Street, Apt. 7D
San Francisco, California 94109
(415) 928-0337
5850 Cameron Run Terrace
Alexandria, Va. 22303
(703) 768-7220

COVER: *Although her article in this issue deals with her experiences as a botanist in South America, Patricia Armstrong (SM'68) appears here as photographed on a previous expedition, to the Juneau ice fields of Alaska.*

PICTURE CREDITS: *Page 1, Mary Ann Tiffany; Pages 5, 7, Town and Country; Page 8, 9, Charles Armstrong; Pages 11, 12, 16, Patricia Armstrong; Page 31, Lynda Caspe.*



Three questions

About the sustained nuclear chain reaction

- 1. How would the development of atomic energy have gone, if it hadn't been for the war?***
- 2. Who invented the chain reaction, anyway? (Since this is just a question of fact that some of you may know, I also ask Question 2B: When was the invention made?)***
- 3. This question is really a pointed one which I like to raise whenever I stand in front of a captive audience with representatives from government and business: How important do you consider that the role of knowledge-oriented science was in the development of nuclear energy?***

Herbert Anderson

While you are thinking about how you might answer those questions, I'll take you back in history.

It's always difficult to know where history begins. But in my own mind, the story of the development of the chain reaction begins in Sweden. It begins with Otto Frisch, and I thought it would be appropriate to let you have the description of that beginning in his own words:

This is where I came in, because Lisa 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 [Otto] 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, and gradually the idea took shape that this was no chipping or cracking of a nucleus but rather a process to be

explained by [Niels] Bohr's idea that the nucleus was like a liquid drop. Such a drop might elongate and divide itself;

Dr. Anderson, professor in the Department of Physics and the Enrico Fermi Institute of Nuclear Studies, was an original member of the team which achieved the first sustained release of nuclear energy. The accompanying article, recalling some little-known aspects of that development, is excerpted from a talk Dr. Anderson gave in December as part of ceremonies marking the thirtieth anniversary of the achievement. Present at that occasion, which included a symposium on energy needs and policies, were representatives of Congress, the Atomic Energy Commission, Argonne National Laboratory, the business community, the diplomatic corps and the University.

and when I worked out the way the electric charge of the nucleus would diminish the surface tension, I found that it would be down to 0, just around $Z=100$, and probably quite small for uranium. Lisa 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, and 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 that 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, and then I went back to Copenhagen and just managed to tell Bohr about the idea as he was catching his boat to the United States.

And 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, and nobody had.

This was exciting news for Bohr. The idea of the liquid drop was his idea and he had been looking for experimental evidence that his idea was the right one. Fission was just the thing. When he arrived in New York, on that January 16, 1939, he was so excited about his new discovery, that he just had to tell it to someone, although he had been cautioned not to let the cat out of the bag before Frisch had done the experiment.

So a few days after settling in Princeton, he came to Columbia looking for Enrico Fermi; he wanted to see Fermi's reaction to his great news. He looked for him in one of the laboratories. Fermi wasn't there, but I was. Undeterred, he came right over, grabbed me by the shoulder and said: "Young man, let me explain to you about this new phenomenon in physics called fission." And he rushed to the blackboard and began to explain how the fission occurred according to his idea of the liquid drop.

It was a fairly exciting experience for me to see such important news from such a great man, and as soon as he left, I felt that I had to find Fermi and tell him what happened. When I got into his office, which was on the seventh floor, and before I had a chance to say anything, he started out, "I know what you want to tell me about. Let me explain to you about fission." He went to the board, and he showed how the two particles would come apart, and the energy yield, and all that kind of thing. I have to say that Fermi's explanation was a lot clearer to me than Bohr's was.

At that time I was a graduate student. I had helped build the cyclotron, and I had just about completed some equipment that I had made for some research that I was going to do in neutron physics. Among other things, I had constructed an ionization chamber and an amplifier, which were the kind of instruments popular in that day,

and it just seemed to me that this apparatus might very well adapt itself to seeing the fission process occur.

So I went to Fermi and I said, "Look, you've just arrived and you don't have any equipment, but I have just the kind that would be good for working on fission. Why don't we work together? I need a good professor who understands the physics and you might need a graduate student who's well equipped with apparatus." He appreciated that there would be some problems, because I was already working for John Dunning, but he talked to Dunning; the switch was accomplished, and I began a collaboration with Fermi that lasted twenty-five years.

We didn't lose any time; we mounted a layer of uranium in this ionization chamber, and took it down to the cyclotron in order to bombard that chamber with neutrons, to see if we could see the fission which ought to take place. But the cyclotron wasn't working well that night. Then I remembered that John Dunning had some of these artificial neutron sources that you can make by mixing radium with beryllium. I found Dunning and together we tried it. Lo and behold, we were able to see the fission of uranium on our cathode ray oscilloscope that very evening.

It was a very propitious moment. Fermi had already left earlier that day to attend a meeting in Washington. Dunning, appreciating the significance of our result, telegraphed Fermi that we had seen the fission process. Of course Bohr was there, too, and the whole meeting just blew up with the news. The physicists called up their labs, and very shortly confirmations came from practically every major nuclear physics lab in the country.

Fermi came back to Columbia and straightway called me and wrote on the blackboard a long list of experiments he wanted to do right away.

One of the experiments that we did very early was to try to find out whether new neutrons were emitted when uranium was bombarded by neutrons. We carried out such an experiment with positive results.

An uninvited suggestion

But then in a very curious way, [Leo] Szilard interjected himself into our work.

Just after we finished that experiment he went to Fermi, and said, "You know, Enrico, you are using a radium-beryllium source and you must know that such a source has rather energetic neutrons. How do you know that there isn't an $(n, 2n)$ reaction which would disturb the results?"

"Well," Fermi said, "you may be right." Szilard said, "It just happens that I have a radium-beryllium photo-neutron source which gives neutrons of much lower

energy, and if you use that you won't have the problem of the $(n, 2n)$ reaction."

Fermi somewhat reluctantly had to admit that the results would be less open to question if the experiment were done with a photo-neutron source, so we carried out the experiment with that source as well. In the paper describing the results, credit was given to a curious organization known as the Association for Scientific Collaboration, and it always seems strange to me as I look back over history, that not a word was said—that it was really Szilard who had the idea, and it was from him that we obtained the source. Anyway, Szilard then became a part of the Columbia group.

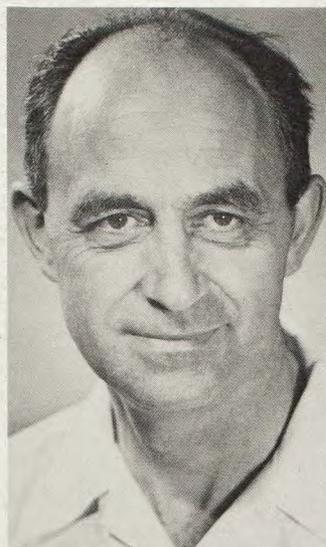
It was curious how he came to Columbia. He was not a member of the faculty. He just sort of appeared one day, because he knew that that's where the action would be. He went to the dean, who appointed him a guest scientist. He then participated in some of the experiments.

Contrasting styles

I remember very clearly how it was, working with Szilard and Fermi. Fermi's idea of doing an experiment was that everybody worked. It was his style to work harder than anybody else, and everyone worked pretty hard. But Szilard was a thinker; he wasn't the one to do manual work. He thought he ought to spend his time thinking; and he didn't want to stay up half the night measuring activities and putting together the various parts of the experiment.

He said, "I realize this work has to be done, but it would be much better for me to spend my time thinking. I will hire a young man who will do whatever is required, and he will do it much better than I could." And so the experiment that we did together was really done by Fermi, Anderson, and a man named S. F. Krewer, although the paper was signed Fermi, Anderson, and Szilard.

It was the first and also the last experiment in which Fermi and Szilard collaborated. The contrast between the Szilard approach and the Fermi approach was really extreme. And it shows how such different kinds of people can succeed in science. Szilard understood this very well.



Enrico Fermi

In fact at one point he pointed out the difference between Fermi's approach and his. When the question came up about whether the chain reaction could go or not, he said, "Well, Fermi and I, we're both conservative in our thinking; according to Fermi, if he sees some chance that the reaction will not work, he doesn't want to say it will; he'd rather be sure of his facts. So he'll continue to work until he can be more certain."

For Szilard, if there's even a small chance that the reaction *will* work, then he feels that he should start taking precautions. He should alert people, tell them what might happen, what the dangers might be, and be ready for the contingencies that might come about.

Although we are primarily concerned with Fermi here, I do want to inject a bit more about Szilard.

Let me tell you in Fermi's own words what he thought about Szilard. He said, "I don't know how many of you know Szilard. No doubt many of you do. He is certainly a very peculiar man, extremely intelligent."

And then, since the audience was amused by this, he said, "I see that's an understatement. He's extremely brilliant. And he seems—at least that is the impression that he gives to me—to enjoy startling people."

Szilard proceeded to startle physicists by proposing to them, that given the circumstances (it was early 1939, and war was very much in the air)—given the danger that atomic energy and possibly atomic weapons could become the chief tool for the Nazis to enslave the world—it was the duty of the physicists to depart from what had been the tradition of publishing significant results as soon as the *Physical Review* or other scientific journals might turn them out.

Instead, he said, one had to go easy, to keep back some of the results until it was clear whether these results were potentially dangerous or potentially helpful to our side. Szilard had that kind of foresight that led to the origin of secrecy in the atomic energy project. It was a hard burden to carry; not so hard during the war, but particularly hard later.

The problem of money

One of the interesting questions is how did we get the money to do the chain reaction? The answer illustrates the difference between the Fermi and the Szilard approaches.

Szilard realized from the beginning that this enterprise wouldn't go unless we could get the money for it, and he also realized that in some way we had to alert the government. Most of you know the story about how he found the way to alert the government through President Roosevelt. Szilard wrote a letter for Einstein to sign; he gave it to Alexander Sachs, who had an inside track, and got it to Roosevelt.

After some weeks, the White House called and said that there was going to be a meeting with Lyman Briggs, the director of the Bureau of Standards, a Colonel Adamson of the Army, and a Commander Hoover from the Navy and they would be willing to meet with the three Hungarians, [Eugene] Wigner, [Edward] Teller, and Szilard, to discuss what was needed in the way of atomic energy. Merle Tuve of the Carnegie Institution of Washington sat in. Here is how Szilard tells it:

“It was our general intention not to ask the government for money, but only ask for the blessing of the government, so that we could go to foundations and raise the funds and get some coordinated effort going. However, these things never go the way you’ve planned them. In the course of the meeting, Tuve ventured the opinion that the work that Fermi had going didn’t require very much, and the most that he could imagine that it would cost would be about \$15,000.

“When the representative from the Army heard this, he said, ‘Well how much money do *you* need?’ And I [Szilard] said, all we need money for at this time is to buy some graphite, and the amount of graphite which we would have to buy would cost about \$2,000. Maybe a few experiments which would follow would raise the sum to \$6,000—something of that order of magnitude.

“At this point, the representative of the Army started a rather long tirade. He told us that it was naive to believe that we could make a significant contribution to defense by creating a new explosive. He said that if a new weapon was created, it usually took two wars before one knew whether the weapon was any good or not. And then he explained rather laboriously that, in the end, it is not weapons that win the wars, but the morale of the troops.

“He went on in this vein for a long time, until suddenly Wigner, the most polite of us, interrupted him. He said, in his high pitched voice, that it was very interesting to hear this. He had always thought that weapons were very important, and that weaponry is what costs money, that this is why the Army needed such a large appropriation. He said he was very interested to hear that he was wrong—that it is not weapons but morale that wins the wars; and if this is correct, perhaps one should take a second look at the budget of the Army—maybe the budget could be cut.

“Colonel Adamson wheeled around to look at Mr. Wigner and said, ‘Well as far as that \$2,000 is concerned, you can have it.’”

That’s not as funny as it sounds, because that was the first money that the government ever gave in the support of scientific research. And, to compare the budget today to the budget of those times, you can see that Colonel Adamson took a very big step.

And there’s also a little dig which I can’t resist making. The industrial people didn’t show up too well either. Szilard says, “In recalling this period I should mention, that until the government showed interest, I was un-

decided whether this development ought to be carried out by industry or by the government. And so just a week or two before the meeting in Washington, I met with the director of research of the Union Carbide and Carbon Co., W. F. Barrett. There was some mixup in the appointment, because they expected Fermi, but it was I [Szilard] who turned up.

“There were five people sitting around a table, and I told them that the possibility of a chain reaction between uranium and graphite must be taken seriously. At this point, I said, we could not say very much about this possibility, and that we would talk about it with much greater assurance if we had first measured the absorption of neutrons in graphite. It was for this purpose that we would need about \$2,000 worth of graphite. I wondered whether they might give us this amount of graphite on loan. The experiment would not endanger the graphite, and we would return it to them.

“Well, Barrett said, ‘You know, I’m a gambling man myself, but you’re asking me to gamble with stockholders’ money, and I’m not sure that I can do that.’”

Compton’s dilemma

How did the project come to Chicago? There were several reasons. One was that this atomic energy project was being run mainly by enemy aliens. There was a war on, and Fermi and those Hungarians were officially enemy aliens—hardly the right people to be in charge of a war project. Furthermore there were two other lines already occupying the Columbia faculty. One was Harold Urey’s project, separating isotopes by diffusion through barriers. And John Dunning had a big enterprise, separating isotopes by gaseous diffusion. So Columbia felt that it had about as much as it could handle in this type of wartime activity.

A committee was formed, the so-called S-1 Committee; [Arthur] Compton was a principal member of the committee; there was also E. D. Murphy of the Standard Oil Company and Ernest Lawrence, among others. It was this committee that decided who would run the project. Compton decided that he liked the whole idea and would bring it to Chicago, and that’s how it got here. He then called Fermi and asked him if he would like to come to Chicago. Fermi agreed, because Chicago looked like a very attractive place.

It’s also interesting to recall how it happened that the chain reaction took place here, on campus. As a matter of fact, the original intention was to construct the pile at the Argonne Forest, a site outside the city. Construction had been going on. But around October 20 there were some labor difficulties, and it was clear that we would be ready to assemble the pile *before* the building was completed. This threatened a serious delay.

Fermi went to Compton to tell him that he believed he could make the chain reaction work safely right here in



Leo Szilard before the erstwhile West Stand at Stagg Field.

Chicago. Compton said, "Let's hear your analysis." When Compton was satisfied that Fermi knew what he was talking about, Compton decided to follow his suggestion.

He did have this consideration: "The only reason for doubt"—to quote Compton—"was that some new, unforeseen development might appear under conditions of release of nuclear energy of such vastly greater power than anyone had previously handled. We did not see how a true nuclear explosion could possibly occur, but the amount of potentially radioactive material present in the pile would be enormous, and anything that would cause excessive ionizing radiation in such a location would be intolerable.

"The outcome of the experiment might thus greatly affect the city, and as a responsible officer of the University of Chicago according to every rule of organizational protocol, I should have taken the matter to my superior.

"But this would have been unfair. President Hutchins was in no position to take an independent judgment of the hazards involved. Based on considerations of the University's welfare, the only answer he could have given would have been no. And this answer would have been wrong. So I assumed the responsibility myself. In the building under the west stands of Stagg Field was a

squash court, and I told Fermi to use this room and go ahead with the critical experiment."

An added note from Compton: on November 14, at a meeting of the S-1 Committee in Washington, "I reported what we were doing. When I mentioned that we were preparing to perform the critical experiment on the Chicago campus, faces went white. General Groves rushed to the nearest phone to find out from the Army in Chicago whether in fact, it was impossible to use the new building at Argonne Forest, and it was evident that Groves did not like what we were doing in the least. But I was not told to stop the experiment. Everyone knew the need for speed. The element of risk involved was accepted as a hazard of war."

A prior discovery

The answer to Question 2 comes out of Szilard's memoirs. He says, "In the fall of 1933, I found myself in London. I kept myself busy trying to find positions for German colleagues who had lost their university positions with the advent of the Nazi regime.

"One morning I read in the newspaper about the annual meeting of the British Association, where Lord Rutherford was reported to have said that whoever talks about the liberation of atomic energy on an industrial scale is talking moonshine.

"Pronouncements of experts to the effect that something cannot be done have always irritated me. That day as I was walking down Southampton Row and was stopped for a traffic light, I was pondering whether Lord Rutherford might not prove to be wrong. As the light changed to green and I crossed the street, it suddenly occurred to me that if we could find an element which is split by neutrons and which would emit two neutrons when it absorbed one, such an element, if assembled in sufficiently large mass, could sustain a nuclear chain reaction, liberate energy on an industrial scale and make possible the construction of atomic bombs.

"The thought that this might be possible became an obsession with me, and it led me into nuclear physics, a field in which I had not worked before, and the thought stayed with me even though my first hunches in this regard turned out to be wrong.

"In the spring of 1934 I applied for a patent which described the laws governing such a chain reaction. It was the first time, I think, that the concept of critical mass was developed, and that a chain reaction was seriously discussed.

"Knowing what this would mean—and I knew it because I had read H. G. Wells—I did not want this patent to become public. The only way to keep it from the public was to assign it to the government, so I assigned this patent to the British admiralty."