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The Princeton Mathematics Community in the 1930s
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EUGENE WIGNER

(with ALBERT TUCKER)

This is an interview with Eugene Wigner at his home in Princeton on 12 April 1984. The interviewer is William Aspray with the assistance of Albert Tucker.

Aspray: Can you tell us how it happened you came to Princeton?

Wigner: That's easy to tell you. It was in 1930. I was sort of assistant professor at the Institute of Technology in Berlin. One day I received a cable offering a visiting professorship at about eight times the salary which I had at the Institute of Technology. I thought this was an error in transmission. John von Neumann received the same cable, so we decided that maybe it was true, and we accepted. But it was clearly a nice occasion and we both enjoyed it.

Aspray: Who instigated it?

Wigner: A Dutch physicist, Paul Ehrenfest. Ehrenfest recommended that two of us be invited, because coming to a new country one feels very lonesome unless one really knows somebody. Von Neumann and I were very close friends. He was at the University; I was at the Institute of Technology. He was, of course, a five times more able mathematician than I was, but I knew perhaps a little more physics.

Aspray: This was 1930?

Wigner: I received the invitation in 1930, but we came here early in February '31.

Aspray: So that was before the new Fine Hall was open?

Tucker: Fine Hall was opened in September '31, and you came here for the spring term of '31.

Wigner: Johnny and I had a joint office at Palmer Physical Laboratory, and we enjoyed it very much. Soon enough we got acquainted with the whole mathematics and physics departments, because both those departments were small in today's measure. They each had, I think, about a dozen people. Today they both have about three dozen.

Aspray: In the fall when the new building was opened, were you and von Neumann moved over to offices there?

Wigner: Yes. For a little while we had, again, a joint office, but then he acquired a separate office, which was later acquired by Einstein. But he wasn't much interested in an office anyway.

Tucker: That was the office that you later had?

Wigner: Yes, it was.

Tucker: The office next to the department office?

Wigner: Yes. It was an elegant and big office, and I was very proud of it.

Aspray: Were you impressed with the facilities of the new building?

Wigner: That is not the kind of thing that impresses a person who is most interested in science, but I was impressed when Mr. Einstein came. What year did Einstein come?

Tucker: In the fall of '33.

Wigner: That was wonderful. We saw each other often. He could speak English, but was not at all at home with the English language, so he liked to speak German to people who could speak German. I was not only able to speak German, but also helped him to speak English. That's perhaps a little exaggerated.

Aspray: Were there a good number of discussions between Johnny and Einstein?

Wigner: I don't think so. Einstein was not fond of discussions with people unless they collaborated with him actively. Johnny was extremely fast, a wonderfully quick thinker. Einstein was an extremely deep thinker, and he didn't like to have arguments. When we took walks, we often talked about politics. It was, as we realized, a serious problem to prevent Hitler from conquering the world. We hoped he would not conquer Holland, not France, nor Poland. We spoke much about it, and very seriously. Einstein was also very concerned about it.

Aspray: Who were the people you most often talked with and worked with?

Wigner: That depends on the time. In the beginning it was Johnny von Neumann; we even wrote joint articles. Later on, I had excellent students who were working on a doctor's degree. The first one I had was Frederick Seitz. He became the president of the National Academy of Sciences, and then of Rockefeller University. My second student was John Bardeen, who won two Nobel Prizes. They were very, very able students, and our collaboration was interesting.

Aspray: Did you have other students?

Wigner: Yes. The third one was Conyers Herring. You know he's not a famous person, not even now. But I really admire him. He knew a great deal. In those days, the four of us, Conyers Herring, John Bardeen, Fred Seitz, and I, were most interested in solid-state physics. Conyers knew more solid-state physics than anybody I ever met. I once asked Fred Seitz a question of a specialized nature and he told me, "Ask Herring, he knows it."

Aspray: What year would this have been?

Wigner: '33, '34, perhaps '35.

Tucker: 1935. John Bardeen took his doctorate in 1936. He was taunted as being a graduate student in mathematics. During his first year he held the JSK Fellowship. Those are the initials of an anonymous donor. Mathematicians could get a Procter Fellowship and so on, but the only one that was only a mathematics fellowship was the JSK Fellowship. The stipend was not especially large, so it was usually given to a first-year graduate student. Bardeen had it his first year. He took his general examination in mathematics, and so it was only his thesis ...

Wigner: It was entirely physics. But he did impress me as a great mathematician. He was a very able person, needless to say, and he worked very hard. He was successful, of course, or he would not later have received the Nobel Prize. The Nobel Prize was for work in solid-state physics, but not for the work of his doctoral thesis.

Tucker: I was particularly interested in the graduate students and tried to keep track of what had happened to them. This morning we were talking to Valentine Bargmann. He happened to mention a student he remembered, one from the '40s by the name of Olum. He said he didn't know what had happened to Olum. I was able to tell him: he is president of the University of Oregon. I have always tried to keep track of what has happened to the graduate students.

Wigner: Well, you knew that Fred Seitz had a most successful life.

Tucker: Oh yes. I met up with Fred on two occassions after he was at the top of things. I was a member of the first committee to award

the Sloan Fellowships, and Fred Seitz was also. In fact, he was the chairman of that committee. Then I was on the first committee to award the National Medal of Science, and Fred Seitz was on that committee also. I seem to run into him every time I go to New York. He seems to go to the same restaurant that I do.

Wigner: I thought he eats at home as a rule.

Tucker: Well, I have encountered him twice at Pearl's Restaurant, which is a nice Chinese restaurant near Rockefeller Plaza.

Aspray: Can you tell me about some of your other students in the 1930s?

Wigner: Many of them were very, very able. In fact, most of them were very, very able. I could give you a list of them. Later on I became intersted in other parts of physics, and, in particular, in nuclear physics much before fission was discovered. Leonard Eisenbud was one of my students. He took a doctorate degree with my help, and we both wrote a book together. My students were very able and very friendly, and we had very good relations. Maybe there was an exception, but not more than one.

Aspray: How well did the mathematical physicists and the other physicists working in Fine Hall interact with the mathematicians?

Wigner: Very well. We had a tea every afternoon from 4:00 to 4:30, and we'd talk to each other. I'd ask questions of them if I didn't know some mathematics, and they helped me. There was a close collaboration. I am sorry that this does not exist to half that degree any more. I don't know the mathematicians, the mathematicians don't know me, and there is not enough collaboration. About three years ago I asked several of them a question, but no one knew the answer.

Aspray: Did most of the students that you got have a mathematical background?

Wigner: No, most of them were physicists. Fred Seitz, for example, was a physicist, but he knew how to make calculations. We calculated, according to quantum mechanics, the heat of evaporation of a solid. That involved a great deal of mathematics and of finding good approximations to equations, which is a physicist's job. Bardeen was almost fully a mathematician. Huntington was more nearly a physicist, as was Eisenbud.

Tucker: What about the man who went to Cal Tech, and who won a Nobel Prize?

Wigner: Who was that?

Tucker: Tukey mentioned him. He was here about 1939.

Wigner: From '36 to '38 I was in Wisconsin. The mathematics department decided I was no good, and told me to find another job. So I went to the University of Wisconsin. It was a great blessing for me, because in Princeton I did not feel at home. Princeton was like an ivory tower. At present it is not; it is a friendly, pleasant place. I returned to Princeton in the fall of '38.

Aspray: When you came back it seemed like a more congenial place to you?

Wigner: It became more and more congenial, and I tried to make it that way. I was originally a visiting professor at Princeton University, and I had no influence. But I came back to a wonderful job, the Jones Professorship.

Tucker: The young man I was trying to think of, who's not so young anymore, is Richard Feynman.

Wigner: He was not my student. He was John Wheeler's. Of couse he probably took classes from me, but his doctoral thesis was written with John Wheeler. Of course he is a famous and extremely able person, both in research and in teaching.

Tucker: Do you remember an English physicist, Maurice Pryce?

Wigner: Yes, I do remember him. He established many important rules in theoretical physics.

Tucker: What has happened to him?

Wigner: I don't know. He was a very able and very friendly colleague.

Tucker: There was a good group of young Englishmen here at that time. One, who perhaps you didn't know, but I knew very well, was Henry [J.H.C.] Whitehead.

Wigner: I met him.

Tucker: There was the man who became famous in computing, Alan Turing.

Wigner: Again, I had little contact with him.

Tucker: He was a student of Church's, and he did his Ph.D. with Church.

Wigner: You have a marvelous memory.

Tucker: Of course I have been thinking about this oral history for some time now, so I have had opportunities to refresh my mind on these things.

Aspray: Could you tell me something about some of the other people who were part of the mathematical-physics community?

Wigner: Who was the chairman of the physics department? He had a very British name. Edwin P. Adams. Well, I must admit I had little understanding of the physics teaching here. I went to Adams' classes, but he often got himself into a somewhat confused state, and he was not really a great physicist. We had here also A.H. Compton, who was an experimental physicist.

Tucker: He was the chairman of the department when I arrived in Princeton, and a year later he was called to be president of MIT.

Wigner: He was very able, very good, very friendly. When did you come?

Tucker: I came in 1929. I was here about a year and a half before you came.

Wigner: But Arthur Compton was still here when I came.

Tucker: Oh.

Wigner: We have to look it up.

Tucker: Later on, Rudolph Ladenburg came.

Wigner: Yes, and he was a good friend. We knew each other in Berlin. The fact that we already knew each other contributed to the friendship we had here. As you know, if somebody is very reserved, it is difficult for him to make new friendships. I had a high regard for him. When I was asked to leave Princeton it was a committee which voted three to one that I be asked to leave, and he was the one who voted that I be asked to stay. But it was a blessing that I was asked to leave, because in Wisconsin I became a real American. I liked the people at the University of Wisconsin, and there was no ivory tower there. They admitted that I knew how to plant potatoes.

Tucker: There was a young physical chemist who was very frequently in Fine Hall and then he went to the University of Wisconsin. Do you remember him? I think he told me that he helped teach you how to drive a car.

Wigner: I know whom you mean. [See page 8.] You have a marvelous memory.

Aspray: When did Valentine Bargmann come?

Wigner: I would estimate in '37.

Aspray: Did you have close dealings with Bargmann?

Wigner: Yes. You know we were both interested in the application of group theory to physics. I started to work in that area very soon after I became also formally a physicist, and that was in 1926. I was, of course, always most interested in physics.

Tucker: Your Ph.D. was in chemical engineering.

Wigner: Right. In '26 I officially became a physicist. Even when I was a chemical engineer, I subscribed to the German journal of physics, *Zeitschrift fuer Physik*. I read in it an article by M. Born and P. Jordan who contributed very significantly to the foundation of quantum mechanics. Reading their article overwhelmed me—I realized that physics would change tremendously. Until then, physics was largely overwhelmed by the belief that man is not bright enough to understand microscopic phenomena. Perhaps he isn't. We don't know yet, because it's not fully understood. Anyway this article gave me the impression that it consituted tremendous progress. Two weeks later I received an invitation (of Richard Becker) to become an assistant of the newly appointed professor of theoretical physics at the Institute of Technology in Berlin. I decided that physics is more bountiful than I realized before, and I accepted the invitation. My salary was about $37 a month, which even in those days was very little. But after a year, I was invited to Goettingen, and many other things followed. I became interested in the application of group theory to theoretical physics when I was 26 and a half years old.

Aspray: Were there mathematicians at Princeton with whom you discussed group theory?

Wigner: May I say that the article was about a 3-particle problem. I realized that this is connected with group theory, because I knew group theory from much earlier. When I finished that article I realized that the result could be extended to more than three particles. I talked to Johnny von Neumann about it. This was at Berlin. He knew which article to give me to read: one by Frobenius and Schur. It had an enormous effect on me. I owe much to Johnny von Neumann, who gave me that article to read.

Aspray: Who else at Princeton did you talk to about these matters?

Wigner: Howard Percy Robertson, I had some contact with, but it wasn't very close. Also Condon, but again, it wasn't very close. I liked them, but I am afraid they didn't like me.

Aspray: We haven't mentioned Hermann Weyl yet. Can you tell me something about your relations with him? When did you first get to know him?

Wigner: When he came to Princeton I knew about his work, and I quoted it also. You know he was interested in group theory. But in Princeton we were really strangers to each other. He never mentioned my work in his book on the application of group theory to quantum mechanics, even though practically all that is in the book was contained

in publications by me and in joint publications by Johnny von Neumann and me. I resented that because I needed a job then.

Aspray: Are there other people you remember especially well from the 1930s?

Wigner: Oswald Veblen, of course. He was a wonderful person, and I appreciated the contact with him very much. He was thoughtful and understanding.

I liked Condon also. He gave very good classes. I went to many classes partly to get acquainted with people and partly to learn English. This is not easy to learn for a stranger because of the less-than-perfect connection between spelling and pronunciation. I became well acquainted also with Gregory Breit, who came to Princeton for two years. We worked together, and we understood each other well, and we published a paper together, perhaps only one, but one that led to our being widely recognized. The result is called the Breit-Wigner formula, which you may have heard about. But it is really misnamed, because the same formula exists for the absorption of light and was recognized much earlier in Germany. But it also works for neutrons. Actually our formula does contain a factor not contained in the formula for light; it's the cross-section at very low energy, which is proportional to the reciprocal of the square root of the energy.

Tucker: How early did you meet Dirac?

Wigner: I met Dirac when he came to visit Princeton. We were close friends. Both of us were unmarried, and we went together to restaurants quite regularly. It happened that my younger sister had to separate from her husband, and I decided I'd bring her to Princeton so she would be in a new environment. We went together to a restaurant and Dirac came into the restaurant, looked at me and looked at my sister, and was sort of flabbergasted. My sister noticed that and asked me, "Who is that person?" I explained and she said, "Do invite him to eat with us. He would feel lonesome alone." He did come to eat with us. That is how they got acquainted.

Aspray: Did the environment in Fine Hall play a large role in your social life while you were still a bachelor?

Wigner: No. In fact in Princeton I did not know any lady. I recognized wives of some of my colleagues, but at that time I did not know any woman in Princeton. I later got acquainted with several ladies, of course, and one of them married me.

Tucker: The physical chemist I was trying to think of was Joseph Hirschfelder.

Wigner: Yes. I collaborated with him both in Wisconsin and here in Princeton. I also collaborated with Henry Eyring. He became a famous person.

Tucker: I think he was clearly the outstanding physical chemist in Princeton at that time. After he left here I wasn't able to follow his career, but in talking to his students, people like John Turkevich, I found they regarded him highly. I think Hugh S. Taylor regarded Eyring as a greater physical chemist than he himself was.

Wigner: Maybe so. That's difficult to tell. I liked Taylor very much. He was a very friendly person. He was the chairman of the chemistry department. Henry Eyring was a Mormon. He went to Utah and studied and taught very successfully.

Tucker: I heard that after Henry Eyring had decided to go to Utah Hugh Taylor was asked by a friend at a chemistry meeting how he had ever allowed Henry Eyring to leave Princeton. Hugh Taylor said that he had talked and talked to Eyring to persuade him to stay at Princeton, but that he had not been successful. "Well," the friend said, "perhaps you didn't talk long enough." Taylor said, "Well, if I had talked longer, it would have meant that I would have become a Mormon." He was a Catholic.

Aspray: Since you knew Johnny well, could you describe to me the difference between the way you worked and the way he worked on problems of mathematical physics?

Wigner: He knew much more mathematics, and he was much faster than I was. He was really amazing.

Tucker: He was the fastest brain that I've ever seen.

Wigner: He was as much interested in physics, including the experimental results, as I was. Even though he was very fast and very ready to associate new ideas, since mathematics was very large already in those days it took up most of his interest at that time. We eventually wrote three articles together. I asked him to be a co-author, because I felt that I would learn a good deal from him. But he hardly read those articles. I thought it was good if he did the proofs, because I felt he should get acquainted with the subject. He did the proofs, but he left in a dozen misprints. We later wrote a little statement correcting the misprints.

Aspray: How would you characterize what he contributed to physics?

Wigner: The most frequently mentioned contribution is not the one which I would praise most. That is the introduction of the computing machine. Another thing which he contributed and which I admire very much is a novel type of nuclear bomb, the so-called implosion bomb. That was also very important.

But I consider even more important than any of these inventions his book on the mathematical foundations of quantum mechanics. It does contain many rules of quantum mechanics which I'm sure he realized were not final and not really correct. But he wanted to systematize the subject. That he realized it is not complete is evident from the way he

described the measurement process. He wrote that quantum mechanics is not valid for this process, which means that quantum mechanics has a limited validity, and even its basic ideas are in conflict with each other, just as the basic ideas of Newton were terribly separate from the way that the motions of planets were determined, because it is light through which we see the planets. Light was not really contained in Newton's theory.

Aspray: You said that mathematics occupied most of his time.

Wigner: His very last years, excuse me.

Aspray: Even Johnny was not able to know all of mathematics? Is that correct?

Wigner: Likely not, I don't know. You see there are so many things in mathematics that are difficult to know.

Aspray: Do you have some feeling for what parts of mathematics he knew and didn't know?

Wigner: He wrote no articles on number theory. But once I told him—this is a story which is perhaps of some interest—that I was much impressed by a new theorem about which I had read. He said, "Did you read the proof?" I said, "No, but the theorem itself is really amazing." He said, "Well, would you like to have a proof?" I said, "Yes, if you can give me one." Then he asked me six questions: "Do you know this theorem?", "Do you know this theorem?" ... six theorems. I knew three, and I didn't know the other three. And he gave me a wonderful proof, never mentioning the theorems which I did not know and using the theorems which I did know. He was amazing in this respect.

I have a story against him, if you want to hear it. I once told him that I just read, to my amazement, that somebody could multiply two 5-digit figures in his head. He said, "That's wonderful. I'll try it also." I gave him two 5-digit figures. He went to the corner, as he always did when he wanted to think hard, looked up, and mumbled. He did that for about five minutes, and then he came back with a product. I said, "Wonderful, congratulations." He said, "Is it correct?" I said, "No, but to get any result is wonderful." It is very difficult, almost impossible, to multiply two 5-digit figures in your head. After all, for what purpose was paper discovered?