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Discussion: John von Neumann—A Case Study of Scientific Creativity

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Edward Teller assesses von Neumann's influence on the development of mathematics, especially of his work on representation theory of noncompact groups. He stresses von Neumann's early realization of the significance of computers. Wigner's recollections of John von Neumann's early years emphasize the influence of von Neumann's early education on the development of his scientific creativity.

Categories and Subject Descriptor: K.2 **[Computing Milieux]:** *History of Computing—people.*

Nagy: The name of John von Neumann has been associated with the development of the computer, and, in some respects, symbolizes the modern electronic computer age. As this symposium will indicate, von Neumann contributed to the development of what has been described as the "von Neumann" and the "non von Neumann" machines.

Numerous legends have survived concerning von Neumann's formidable scientific creativity and talents, some of which are described in Paul Halmos' essay in *American Mathematical Monthly,* 1973. These legends neither tell us about von Neumann's scientific talent, nor do they offer much insight into his creativity. Therefore, I would like to ask this distinguished panel of von Neumann's colleagues to comment on the components of scientific creativity based on their knowledge of von Neumann and on their own experience and research.

Let us begin in chronological order with Eugene P. Wigner, the boyhood friend of von Neumann, who attended the same high school in Budapest.

Wigner: It is really a pleasure to remember my associations with John von Neumann. It is unpleasant to realize that he is no longer with us. There have been many articles written about him, and I will try not to repeat them. In fact, that will not be difficult because there are a great many things that might be said about von Neumann.

I first met von Neumann when he was 13 and I was 14 at the Lutheran Gymnasium in Budapest, which was an excellent school. In particular, we had a great mathematics teacher, whose name was Ratz, who was very interested in teaching. I should mention that he was made Director of the Gymnasium once, but after 2 years he resigned, because he said he preferred to teach. Well, anyway, Ratz gave private classes to von Neumann because he realized even at that time, how deeply von Neumann was interested in mathematics and how much he would be able to contribute to it. Ratz also gave me books and articles to read. He was really a wonderful teacher. I think von Neumann was very much indebted to him.

Von Neumann had many friends when he was a young man and that continued, of course, but already as a high school student he had many friends. He loved to take walks with them and talk to them. We discussed mathematics quite a good deal. He was very advanced, partly as a result of the teaching, but partly also because he read a great deal, and he was interested in many fields of study.

Well, after high school, he went to various schools; he studied for 1 year in Budapest and then in Berlin, Hamburg, and Switzerland. He was known every place. As we heard, he received a degree from Budapest, but that was only a formal matter; he could have received many degrees.

He first became an assistant professor in Berlin, then in Hamburg. In Berlin, I was together with him for 2 years when I was assistant to the new professor of theoretical physics. My degree was in chemical engineering, not in mathematics or physics. But I was very much interested in mathematics and physics, and we talked about the new quantum mechanics, which was for a long time, a mystery.

Von Neumann contributed to the basic theory of quantum mechanics that emerged. He was, I think, the first who gave a proof that even, well not a perfectly correct proof, that even unbounded self-adjoint operators can be decomposed into characteristic values and characteristic functions. That was a very important observation and it was soon made complete. But then he also had a queer theory of what the measurement process was; whenever a measurement is made, the self-adjoint operators are brought over into one of those characteristic values. This was very important and stays with us even though it is not easy, really, to prove. In fact, I think I have presented a proof that only a very limited number of self-adjoint operators can be measured, really, even according to quantum mechanics.

However, when I wrote the paper and submitted it to a journal, the journal refused it. They did not want to publish it. I mentioned to von Neumann that it wasn't accepted. He said, "Oh, give it to us, we'll publish it in the *Annals of Mathematics* and we'll be very pleased." He was really very objective, very helpful.

Well, I will mention one more thing before I stop and that is that I became very interested in several particle problems. There were some very interesting papers written on the subject but they did not seem entirely clear. So I decided to work on particle problems. First, I determined the representations of the symmetric group for two particles—it was not difficult. Then I determined the symmetric group for three particles and four particles—that was not difficult. These were elementary calculations. But when I tried it with five particles—well, I could not solve that problem. So I thought why don't I ask von Neumann if the problems have been solved by the mathematicians. I went to him and told him about three and four particles, of course he knew about two particles. He told me he knew of an article that might be of some help to me, and that he would get it to me the next day. The next day he pointed out the article of Frobenius and Chu that influenced me tremendously for the next 3 years. I was entirely under the influence of that article. Of course, I worked with it later.

I learned many things from von Neumann, and it was very nice and useful to be with him, to take walks with him, and to listen and argue with him. I think I will stop here because I have reached a point when our lives changed significantly.

Nagy: I have only one question. Von Neumann enrolled as a chemical engineer in Zurich, but he was especially interested in mathematics. Is it true that when Herman Weyl, the world's most distinguished mathematician, briefly left his academic post to attend a symposium, he asked von Neumann, a student of chemical engineering, to take over his classes?

Wigner: (Corrects Nagy's comment, to indicate that Weyl was one of the most distinguished mathematicians of his time.) That is very interesting, but I am afraid I do not know about that. But he got his degree in mathematics, really.

Nagy: In Budapest?

Wigner: In Budapest. I am also a chemical engineer, you know, and I never had any degree in any other subject. But von Neumann was not really interested in chemical engineering. I worked for 2 years as a chemical engineer.

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Nagy: Moving ahead, the Los Alamos period was a very important period in John von Neumann's life. At Los Alamos, he was close friends with Edward Teller. Professor Teller, would you comment on your relationship with von Neumann.

Teller: We began working at Los Alamos late in March, 1943. I was among the first to arrive. It was an isolated community. Few of us were allowed to travel at all.

Von Neumann was among the exceptional ones who did not work at Los Alamos, but came to visit from time to time. I had known him in my last year of high school, met with him maybe two or three times; then I had seen quite a bit of him in the United States, at Princeton. We knew each other well.

When he arrived, maybe a couple of months after Los Alamos started, we had a problem. How to assemble nuclear material so that it would not react in a premature way. Von Neumann heard about that problem when he arrived; immediately he gave us a suggestion, which I will not repeat, because it happened to be wrong. I went to work on it to see whether one could squeeze something out of it, but it didn't work.

The evening of the first day he arrived, after dinner, I told him that one of the crazy young people, Neddermyer, was trying to assemble fissionable nuclear material in the most simple way; namely, they were assembling it with a high explosive from all sides, making a spherical shell that moved toward the center.

Of course, I have been asked again and again, what is the source of von Neumann's creativity? Von Neumann was interested in everything. He was interested in every intellectual problem. So when I told him about this project, he started to make some simple calculations, so simple that I could repeat them to you. He assumed, as everybody did, that the solid material was incompressible. Based on this assumption, he determined that tremendous accelerations and tremendous pressures would be developed. Even von Neumann did not arrive at that conclusion right away. It took him all of 25 minutes. By that time, I happened to remember that at the pressures he calculated, and even at lower pressures, materials are not incompressible. So the assumption on which all of us had been working, that materials *cannot* be compressed was incorrect. It became clear, that our job at Los Alamos was quite a bit easier than we had believed as materials could be compressed. Now you know, this is a very simple point, but it so happens that it has not been generally mentioned.

One of the remarkable things that I have to tell you as a supplement was that the next morning we went in to see J. Robert Oppenheimer. Oppenheimer was a very special person, a very intelligent person. He grasped at once the importance of this, and the whole direction of the work in the laboratory was changed.

Von Neumann, then, as was his wont, started to get interested in details. How to use high explosives to make a spherical shock. How to calculate in detail the implosion of compressible materials. The latter turned out to be too difficult for von Neumann, but not too difficult for the suggestion he made, and that was to use computers. Not the kind of thing we are talking about now, not the kind of mechanics with which my grandchildren are playing, but incredibly clumsy machines which took a few seconds to make one step. In the end these machines did the job and made the design which we didn't use.

We didn't use that design because we did not believe that the machines could be trusted. In the end, what we did was based on nothing more than the suggestions of the first evening. Now I would like to relay to you two more direct experiences because they connect with problems mentioned before.

I, like Eugene Wigner and like von Neumann, like to walk. In Los Alamos, very shortly after the end of the war, I remember that von Neumann and I went for a walk and discussed the question: "What is the meaning of a classical measurement?" I thought that I knew. Unfortunately, the problem is of the kind in which accuracy and completeness are complementary. If you are accurate, you are not complete. If you are complete, you are not accurate. I must do the best between Scylla and Charybdis. Von Neumann said that Niels Bohr had never defined what a classical measurement was. I tried to tell him, but he disagreed; in the end I told him what Bohr did, emphasizing a point that is not often mentioned, although it is not new. I explained to von Neumann that a classical measurement is one in which the entropy increases—where enough disorder is made so that you cannot retrace it. You cannot get back from the measurement to the original state—the original state in the atoms that were the subject of the experiment. I am telling you this because I got quite interested in von Neumann's questions. Later—I believe it was in 1962, on the 60th birthday of Heisenberg, from whom I learned about the uncertainty principle—I wrote a little statement which said that in a classical measurement, it is quite important that the entropy should increase, because if it were not so, then one could retrace, reproduce the original atomic predictions which have no part in classical physics. This is one of many examples of how von Neumann has influenced our work.

I would like to mention one last subject—last historic topic—which is connected with computing machines. When von Neumann visited in Los Alamos, I remember that he withdrew on frequent occasions to give careful consideration to making an electronic computer. He was working on details, but I did not know what a computer was. I did my Ph.D. work on a computer which I had to drive with my hands and which made a lot of noise. But how this machine could do very complicated things, I did not understand. Von Neumann explained it to me. He explained it to me three times because I was so stupid that I did not understand it the first time. Von Neumann could not only do everything, but he could also emphasize the obvious in a way that illuminated complex problems.

He was working after the war on a computing machine—IBM was too slow for him—he worked on one at Princeton. I have a friend at Los Alamos, Nick Metropolis, of whom you have surely heard. Metropolis also learned from von Neumann; his machine was produced at Los Alamos. But the JOHNNIAC [IAS computer of the Institute for Advanced Studies] that von Neumann was working on in Princeton came second. And, I will tell you why: it wasn't because Metropolis had new ideas. The ideas came from Johnny; he also worked them out. But von Neumann made a mistake. He selected 3-inch tubes for the memory. He said these smaller tubes are sufficient—so, why use the bigger, more expensive 5-inch tubes. But Metropolis was more clever; he noticed that the 5-inch tubes were available while the 3-inch tubes were not. That is why Metropolis' MANIAC came before the Princeton JOHNNIAC computer.[1]

Nagy: John von Neumann had remarkable foresight. I would like to conclude by asking Professor Wigner what he thinks would be John von Neumann's main message to us today.

Wigner: I am not sure that science and technology represent the highest interests of intelligent beings. Supposing that somewhere there are intelligent beings who have progressed further than we have. If there were such beings, they ought to have been able to determine that there is life on this planet and have gotten in touch with us. This has not happened.

Therefore, it may be I am afraid, that soon enough, and by that I mean in a few hundred years from now, we will be much less interested in science than we are now. I think Johnny von Neumann would have agreed with that. Perhaps man can be happy and healthy without science, without contributing to science; and, of course, contributing to science already is more difficult because science has spread out enormously and nobody knows all science, not even von Neumann did. But as for mathematics, he contributed to every part of it except number theory and topology. That is, I think, something unique.

Stan Ulam, one of von Neumann's friends and an excellent physicist, said that with John von Neumann's death the world of mathematics lost one of its most original and penetrating minds.

Have I spoken too long? Well, I ought to mention that in mathematics his work on representation theory of noncompact groups, which is similar to his work on the theory of self-adjoint unbounded operators, was very important in the theory of the representations of the symmetric group, which I was very, very fond of. Perhaps I mentioned also that he designed and recognized a rule of the density matrix about the same time as Landau in Russia had recognized it. And perhaps it was mentioned earlier but not with as much emphasis as I would put on it: namely, that he recognized the significance of computer machines before everybody, much before other physicists recognized it. And this was very nice and had very useful effects.

Finally, I could mention that his interest included not only mathematics and theoretical physics, but also other subjects, in particular, economics. He wrote a very nice book together with Morgenstern on economics; its title I think is *The Theory of Games and Economic Behavior.* And I must say that I don't know of anybody at all who is now living, or whom I knew well enough to say I "knew" him, who had as many deep interests in as many different subjects as did von Neumann. And I admired him and continue to admire him for that. Thank you very much.

Nagy: Finally, Nicholas Vonneuman would like to conclude with a brief statement regarding what John von Neumann would have said to the present.

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[1]Nick Metropolis, Review Editor for this issue, corrected the name of von Neumann's computer from "JOHNNIAC" to "IAS." In addition, he offers a slightly different version of Teller's anecdote. Metropolis states that von Neumann used 5-inch tubes and adds that he began using 2-inch tubes, because he realized that they were more reliable. Metropolis says that is why his computer came along before the Princeton computer.

Vonneuman: Some of von Neumann's philosophical ideas are contained in the article, "Can We Survive Technology," and in perhaps two other articles addressed to nonmathematical audiences. He concludes that the final solution to all international problems is not to develop bigger and better countermeasures, but to rely on the plain fact that the human character is adaptable, that human beings have flexibility, patience, and intelligence.