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Photo by Danielle Alio, Princeton University, Office of Communications

Interview with Abel Laureate John F. Nash Jr.

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This interview took place in Oslo on 18 May 2015, the day before the prize ceremony and only five days before the tragic accident that led to the death of John Nash and his wife Alicia.
Nash's untimely death made it impossible to follow the usual procedure for Abel interviews where interviewees are asked to proof-read and to edit first drafts. All possible misunderstandings are thus the sole responsibility of the interviewers.

The prize

Professor Nash, we would like to congratulate you as the Abel laureate in mathematics for 2015, a prize you share with Louis Nirenberg. What was your reaction when you first learned that you had won the Abel Prize?
I did not learn about it like I did with the Nobel Prize. I got a telephone call late on the day before the announcement, which was confusing. However, I wasn't entirely surprised. I had been thinking about the Abel Prize. It is an interesting example of a newer category of prizes that are quite large and yet not entirely predictable. I was given sort of a pre-notification. I was told on the telephone that the Abel Prize would be announced on the morning the next day. Just so I was prepared.

But it came unexpected?
It was unexpected, yes. I didn't even know when the Abel Prize decisions were announced. I had been reading about them in the newspapers but not following closely. I could see that there were quite respectable persons being selected.

Youth and Education

When did you realise that you had an exceptional talent for mathematics? Were there people that encouraged you to pursue mathematics in your formative years?
Well, my mother had been a school teacher, but she taught English and Latin. My father was an electrical engineer. He was also a schoolteacher immediately before World War I.

While at the grade school I was attending, I would typically do arithmetic – addition and multiplication – with multi-digit numbers instead of what was given at the school, namely multiplying two-digit numbers. So I got to work with four- and five-digit numbers. I just got pleasure in trying those out and finding the correct procedure. But the fact that I could figure this out was a sign, of course, of mathematical talent.

John F. Nash jr. and his wife Alicia were received by His Majesty King Harald V. at the Royal Palace. (Photo: Håkon Mosvold Larsen/ NTB Scanpix.)

Then there were other signs also. I had the book by E.T. Bell, "Men of Mathematics", at an early age. I could read that. I guess Abel is mentioned in that book?

Yes, he is. In 1948, when you were 20 years of age, you were admitted as a graduate student in mathematics at Princeton University, an elite institution that handpicked their students. How did you like the atmosphere at Princeton? Was it very competitive?
It was stimulating. Of course it was competitive also – a quiet competition of graduate students. They were not competing directly with each other like tennis players. They were all chasing the possibility of some special appreciation. Nobody said anything about that but it was sort of implicitly understood.

Games and game theory

You were interested in game theory from an early stage. In fact, you invented an ingenious game of a topological nature that was widely played, by both faculty members and students, in the Common Room at Fine Hall, the mathematics building at Princeton. The game was called "Nash" at Princeton but today it is commonly known as "Hex". Actually, a Danish inventor and designer Piet Hein independently discovered this game. Why were you interested in games and game theory?

Well, I studied economics at my previous institution, the Carnegie Institute of Technology in Pittsburgh (today Carnegie Mellon University). I observed people who were studying the linkage between games and mathematical programming at Princeton. I had some ideas: some related to economics, some related to games like you play as speculators at the stock market – which is really a game. I can't pin it down exactly but it turned out that von Neumann[1] and Morgenstern[2] at Princeton had a proof of the solution to a two-person game that was a special case of a general theorem for the equilibrium of n-person games, which is what I found. I associated it with the natural idea of equilibrium and of the topological idea of the Brouwer fixed-point theorem, which is good material.

Exactly when and why I started, or when von Neumann and Morgenstern thought of that, that is something I am uncertain of. Later on, I found out about the Kakutani fixed-point theorem, a generalisation of Brouwer's theorem. I did not realise that von Neumann had inspired it and that he had influenced Kakutani.[3] Kakutani was a student at Princeton, so von Neumann wasn't surprised with the idea that a topological argument could yield equilibrium in general. I developed a theory to study a few other aspects of games at this time.

You are a little ahead of us now. A lot of people outside the mathematical community know that you won the Nobel Memorial Prize in Economic Sciences in 1994.
That was much later.

Yes. Due to the film "A Beautiful Mind", in which you were played by Russell Crowe, it became known to a very wide audience that you received the Nobel Prize in economics. But not everyone is aware that the Nobel Prize idea was contained in your PhD thesis, which was submitted at Princeton in 1950, when you were 21-years-old. The title of the thesis was "Non-cooperative games".

Did you have any idea how revolutionary this would turn out to be? That it was going to have impact, not only in economics but also in fields as diverse as political science and evolutionary biology?
It is hard to say. It is true that it can be used wherever there is some sort of equilibrium and there are competing or interacting parties. The idea of evolutionists is naturally parallel to some of this. I am getting off on a scientific track here.

But you realised that your thesis was good?
Yes. I had a longer version of it but it was reduced by my thesis advisor. I also had material for cooperative games but that was published separately.

Did you find the topic yourself when you wrote your thesis or did your thesis advisor help to find it?
Well, I had more or less found the topic myself and then the thesis advisor was selected by the nature of my topic.

Albert Tucker[4] was your thesis advisor, right?
Yes. He had been collaborating with von Neumann and Morgenstern.

Princeton

We would like to ask you about your study and work habits. You rarely attended lectures at Princeton. Why?
It is true. Princeton was quite liberal. They had introduced, not long before I arrived, the concept of an N-grade. So, for example, a professor giving a course would give a standard grade of N, which means "no grade". But this changed the style of working. I think that Harvard was not operating on that basis at that time. I don't know if they have operated like that since. Princeton has continued to work with the N-grade, so that the number of people actually taking the courses (formally taking courses where grades are given) is less in Princeton than might be the case at other schools.

Is it true that you took the attitude that learning too much second-hand would stifle creativity and originality?
Well, it seems to make sense. But what is second-hand?

Yes, what does second-hand mean?
Second-hand means, for example, that you do not learn from Abel but from someone who is a student of abelian integrals.

In fact, Abel wrote in his mathematical diary that one should study the masters and not their pupils.
Yes, that's somewhat the idea. Yes, that's very parallel.

While at Princeton you contacted Albert Einstein and von Neumann, on separate occasions. They were at the Institute for Advanced Study in Princeton, which is located close to the campus of Princeton University. It was very audacious for a young student to contact such famous people, was it not?
Well, it could be done. It fits into the idea of intellectual functions. Concerning von Neumann, I had achieved my proof of the equilibrium theorem for game theory using the Brouwer fixed-point theorem, while von Neumann and Morgenstern used other things in their book. But when I got to von Neumann, and I was at the blackboard, he asked: "Did you use the fixed-point theorem?"

"Yes," I said. "I used Brouwer's fixed-point theorem."

I had already, for some time, realised that there was a proof version using Kakutani's fixed-point theorem, which is convenient in applications in economics since the mapping is not required to be quite continuous. It has certain continuity properties, so-called generalised

[1] 1903–1957.
[2] 1902–1977.
[3] 1911–2004.
[4] 1905–1995.

continuity properties, and there is a fixed-point theorem in that case as well. I did not realise that Kakutani proved that after being inspired by von Neumann, who was using a fixed-point theorem approach to an economic problem with interacting parties in an economy (however, he was not using it in game theory).

What was von Neumann's reaction when you talked with him?
Well, as I told you, I was in his office and he just mentioned some general things. I can imagine now what he may have thought, since he knew the Kakutani fixed-point theorem and I did not mention that (which I could have done). He said some general things, like: "Of course, this works." He did not say too much about how wonderful it was.

When you met Einstein and talked with him, explaining some of your ideas in physics, how did Einstein react?
He had one of his student assistants there with him. I was not quite expecting that. I talked about my idea, which related to photons losing energy on long travels through the Universe and as a result getting a red-shift. Other people have had this idea. I saw much later that someone in Germany wrote a paper about it but I can't give you a direct reference. If this phenomenon existed then the popular opinion at the time of the expanding Universe would be undermined because what would appear to be an effect of the expansion of the Universe (sort of a Doppler red-shift) could not be validly interpreted in that way because there could be a red-shift of another origin. I developed a mathematical theory about this later on. I will present this here as a possible interpretation, in my Abel lecture tomorrow.

There is an interesting equation that could describe different types of space-times. There are some singularities that could be related to ideas about dark matter and dark energy. People who really promote it are promoting the idea that most of the mass in the Universe derives from dark energy. But maybe there is none. There could be alternative theories.

John Milnor, who was awarded the Abel Prize in 2011, entered Princeton as a freshman the same year as you became a graduate student. He made the observation that you were very much aware of unsolved problems, often cross-examining people about these.
Were you on the lookout for famous open problems while at Princeton?
Well, I was. I have been in general. Milnor may have noticed at that time that I was looking at some particular problems to study.

Milnor made various spectacular discoveries himself. For example, the non-standard differentiable structures on the seven-sphere. He also proved that any knot has a certain amount of curvature although this was not really a new theorem, since someone else[5] had – unknown to Milnor – proved that.

[5] István Fáry.

John F. Nash jr. at the Common Room, Institute of Advanced Study, Princeton. (Courtesy of the Institute for Advanced Study. Photo by Serge J.-F. Levy.)

A series of famous results

While you wrote your thesis on game theory at Princeton University, you were already working on problems of a very different nature, of a rather geometric flavour. And you continued this work while you were on the staff at MIT in Boston, where you worked from 1951 to 1959. You came up with a range of really stunning results. In fact, the results that you obtained in this period are the main motivation for awarding you the Abel Prize this year.

Before we get closer to your results from this period, we would like to give some perspective by quoting Mikhail Gromov, who received the Abel Prize in 2009. He told us, in the interview we had with him six years ago, that your methods showed "incredible originality". And moreover: "What Nash has done in geometry is from my point of view incomparably greater than what he has done in economics, by many orders of magnitude."

Do you agree with Gromov's assessment?
It's simply a question of taste, I say. It was quite a struggle. There was something I did in algebraic geometry, which is related to differential geometry with some subtleties in it. I made a breakthrough there. One could actually gain control of the geometric shape of an algebraic variety.

That will be the subject of our next question. You submitted a paper on real algebraic manifolds when you started at MIT, in October 1951. We would like to quote Michael Artin at MIT, who later made use of your result. He commented: "Just to conceive such a theorem was remarkable."

Could you tell us a little of what you dealt with and what you proved in that paper, and how you got started?
I was really influenced by space-time and Einstein, and the idea of distributions of stars, and I thought: 'Suppose some pattern of distributions of stars could be selected; could it be that there would be a manifold, something curving around and coming in on itself that would be in some equilibrium position with those distributions of stars?' This is the idea I was considering. Ultimately, I de-

veloped some mathematical ideas so that the distribution of points (interesting points) could be chosen, and then there would be some manifold that would go around in a desired geometrical and topological way. So I did that and developed some additional general theory for doing that at the same time, and that was published.

Later on, people began working on making the representation more precise because I think what I proved may have allowed some geometrically less beautiful things in the manifold that is represented, and it might come close to other things. It might not be strictly finite. There might be some part of it lying out at infinity.

Ultimately, someone else, A. H. Wallace,[6] appeared to have fixed it, but he hadn't – he had a flaw. But later it was fixed by a mathematician in Italy, in Trento, named Alberto Tognoli.[7]

We would like to ask you about another result, concerning the realisation of Riemannian manifolds. Riemannian manifolds are, loosely speaking, abstract smooth structures on which distances and angles are only locally defined in a quite abstract manner. You showed that these abstract entities can be realised very concretely as sub-manifolds in sufficiently high-dimensional Euclidean spaces.

Yes, if the metric was given, as you say, in an abstract manner but was considered as sufficient to define a metric structure then that could also be achieved by an embedding, the metric being induced by the embedding. There I got on a side-track. I first proved it for manifolds with a lower level of smoothness, the C^1-case. Some other people have followed up on that. I published a paper on that. Then there was a Dutch mathematician, Nicolaas Kuiper,[8] who managed to reduce the dimension of the embedding space by one.

Apart from the results you obtained, many people have told us that the methods you applied were ingenious. Let us, for example, quote Gromov and John Conway. Gromov said, when he first read about your result: "I thought it was nonsense, it couldn't be true. But it was true, it was incredible." And later on: "He completely changed the perspective on partial differential equations." And Conway said: "What he did was one of the most important pieces of mathematical analysis in the 20th century." Well, that is quite something!

Yes.

Is it true, as rumours have it, that you started to work on the embedding problem as a result of a bet?

There was something like a bet. There was a discussion in the Common Room, which is the meeting place for faculty at MIT. I discussed the idea of an embedding with one of the senior faculty members in geometry, Professor Warren Ambrose.[9] I got from him the idea of the realisation of the metric by an embedding. At the time, this was a completely open problem; there was nothing there beforehand.

I began to work on it. Then I got shifted onto the C^1-case. It turned out that one could do it in this case with very few excess dimensions of the embedding space compared with the manifold. I did it with two but then Kuiper did it with only one. But he did not do it smoothly, which seemed to be the right thing – since you are given something smooth, it should have a smooth answer.

But a few years later, I made the generalisation to smooth. I published it in a paper with four parts. There is an error, I can confess now. Some 40 years after the paper was published, the logician Robert M. Solovay from the University of California sent me a communication pointing out the error. I thought: "How could it be?" I started to look at it and finally I realised the error in that if you want to do a smooth embedding and you have an infinite manifold, you divide it up into portions and you have embeddings for a certain amount of metric on each portion. So you are dividing it up into a number of things: smaller, finite manifolds. But what I had done was a failure in logic. I had proved that – how can I express it? – that points local enough to any point where it was spread out and differentiated perfectly if you take points close enough to one point; but for two different points it could happen that they were mapped onto the same point. So the mapping, strictly speaking, wasn't properly embedded; there was a chance it had self-intersections.

But the proof was fixed? The mistake was fixed?

Well, it was many years from the publication that I learned about it. It may have been known without being officially noticed, or it may have been noticed but people may have kept the knowledge of it secret.[10]

May we interject the following to highlight how surprising your result was? One of your colleagues at MIT, Gian-Carlo Rota,[11] professor of mathematics and also philosophy at MIT, said: "One of the great experts on the subject told me that if one of his graduate students had proposed such an outlandish idea, he would throw him out of his office."

That's not a proper liberal, progressive attitude.

Partial differential equations

But nevertheless it seems that the result you proved was perceived as something that was out of the scope of the techniques that one had at the time.

Yes, the techniques led to new methods to study PDEs in general.

[6] 1926–2008.
[7] 1937–2008.
[8] 1920–1994.
[9] 1914–1995.

[10] The result in Nash's paper is correct; it has been reproved by several researchers (notably Mikhail Gromov) using the general strategy devised by Nash. Nash gave his own account on this error in the case of embeddings of non-compact manifolds in the book *The essential John Nash* (eds. Harold W. Kuhn and Sylvia Nasar), Princeton University Press, 2002.
[11] 1932–1999.

Let us continue with work of yours purely within the theory of PDEs. If we are not mistaken, this came about as a result of a conversation you had with Louis Nirenberg, with whom you are sharing this year's Abel Prize, at the Courant Institute in New York in 1956. He told you about a major unsolved problem within non-linear partial differential equations.

He told me about this problem, yes. There was some work that had been done previously by a professor in California, C.B. Morrey,[12] in two dimensions. The continuity property of the solution of a partial differential equation was found to be intrinsic in two dimensions by Morrey. The question was what happened beyond two dimensions. That was what I got to work on, and de Giorgi[13], an Italian mathematician, got to work on it also.

But you didn't know of each other's work at that time?

No, I didn't know of de Giorgi's work on this, but he did solve it first.

Only in the elliptic case though.

Yes, well, it was really the elliptic case originally but I sort of generalised it to include parabolic equations, which turned out to be very favourable. With parabolic equations, the method of getting an argument relating to an entropy concept came up.

I don't know; I am not trying to argue about precedents but a similar entropy method was used by Professor Hamilton in New York and then by Perelman. They use an entropy which they can control in order to control various improvements that they need.

And that was what finally led to the proof of the Poincaré Conjecture?

Their use of entropy is quite essential. Hamilton used it first and then Perelman took it up from there. Of course, it's hard to foresee success.

It's a funny thing that Perelman hasn't accepted any prizes. He rejected the Fields Prize and also the Clay Millennium Prize, which comes with a cash award of one million dollars.

Coming back to the time when you and de Giorgi worked more or less on the same problem. When you first found out that de Giorgi had solved the problem before you, were you very disappointed?

Of course I was disappointed but one tends to find some other way to think about it. Like water building up and the lake flowing over, and then the outflow stream backing up, so it comes out another way.

Some people have been speculating that you might have received the Fields Medal if there had not been the coincidence with the work of de Giorgi.

Yes, that seems likely; that seems a natural thing. De Giorgi did not get the Fields Medal either, though he did get some other recognition. But this is not mathematics, thinking about how some sort of selecting body may function. It is better to be thought about by people who are sure they are not in the category of possible targets of selection.

When you made your major and really stunning discoveries in the 1950s, did you have anybody that you could discuss with, who would act as some sort of sounding board for you?

For the proofs? Well, for the proof in game theory there is not so much to discuss. Von Neumann knew that there could be such a proof as soon as the issue was raised.

What about the geometric results and also your other results? Did you have anyone you could discuss the proofs with?

Well, there were people who were interested in geometry in general, like Professor Ambrose. But they were not so much help with the details of the proof.

What about Spencer[14] at Princeton? Did you discuss with him?

He was at Princeton and he was on my General Exam committee. He seemed to appreciate me. He worked in complex analysis.

Were there any particular mathematicians that you met either at Princeton or MIT that you really admired, that you held in high esteem?

Well, of course, there is Professor Levinson[15] at MIT. I admired him. I talked with Norman Steenrod[16] at Princeton and I knew Solomon Lefschetz,[17] who was Department Chairman at Princeton. He was a good mathematician. I did not have such a good rapport with the algebra professor at Princeton, Emil Artin.[18]

The Riemann Hypothesis

Let us move forward to a turning point in your life. You decided to attack arguably the most famous of all open problems in mathematics, the Riemann Hypothesis, which is still wide open. It is one of the Clay Millennium Prize problems that we talked about. Could you tell us how you experienced mental exhaustion as a result of your endeavour?

Well, I think it is sort of a rumour or a myth that I actually made a frontal attack on the hypothesis. I was cautious. I am a little cautious about my efforts when I try to attack some problem because the problem can attack back, so to say. Concerning the Riemann Hypothesis, I don't think of myself as an actual student but maybe some casual – whatever – where I could see some beautiful and interesting new aspect.

Professor Selberg,[19] a Norwegian mathematician who was at the Institute for Advanced Study, proved back in

[12] 1907–1984.
[13] 1928–1996.
[14] 1912–2001.
[15] 1912–1975.
[16] 1910–1971.
[17] 1884–1972.
[18] 1898–1962.
[19] 1917–2007.

John F. Nash jr. with last year's Abel laureate Yakov Sinai (right) and Michael Th. Rassias (left). (Photo: Danielle Alio, Princeton University, Office of Communications.)

the time of World War II that there was at least some finite measure of these zeros that were actually on the critical line. They come as different types of zeros; it's like a double zero that appears as a single zero. Selberg proved that a very small fraction of zeros were on the critical line. That was some years before he came to the Institute. He did some good work at that time.

And then, later on, in 1974, Professor Levinson at MIT, where I had been, proved that a good fraction – around 1/3 – of the zeros were actually on the critical line. At that time he was suffering from brain cancer, which he died from. Such things can happen; your brain can be under attack and yet you can do some good reasoning for a while.

A very special mathematician?

Mathematicians who know you describe your attitude toward working on mathematical problems as very different from that of most other people. Can you tell us a little about your approach? What are your sources of inspiration?

Well, I can't argue that at the present time I am working in such and such a way, which is different from a more standard way. In other words, I try to think of what I can do with my mind and my experiences and connections.

From left to right: John F. Nash Jr., Christian Skau, Martin Raussen. (Photo: Eirik F. Baardsen, DNVA.)

What might be favourable for me to try? So I don't think of trying anything of the latest popular nonsense.

You have said in an interview (you may correct us) something like: "I wouldn't have had good scientific ideas if I had thought more normally." You had a different way of looking at things.

Well, it's easy to think that. I think that is true for me just as a mathematician. It wouldn't be worth it to think like a good student doing a thesis. Most mathematical theses are pretty routine. It's a lot of work but sort of set up by the thesis advisor; you work until you have enough and then the thesis is recognised.

Interests and hobbies

Can we finally ask you a question that we have asked all the previous Abel Prize laureates? What are your main interests or hobbies outside of mathematics?

Well, there are various things. Of course, I do watch the financial markets. This is not entirely outside of the proper range of the economics Nobel Prize but there is a lot there you can do if you think about things. Concerning the great depression, the crisis that came soon after Obama was elected, you can make one decision or another decision which will have quite different consequences. The economy started on a recovery in 2009, I think.

It is known that when you were a student at Princeton you were biking around campus whistling Bach's "Little Fugue". Do you like classical music?

Yes, I do like Bach.

Other favourite composers than Bach?

Well, there are lot of classical composers that can be quite pleasing to listen to, for instance when you hear a good piece by Mozart. They are so much better than composers like Ketèlbey and others.

We would like to thank you very much for a very interesting interview. Apart from the two of us, this is on behalf of the Danish, Norwegian and European Mathematical Societies.

After the end of the interview proper, there was an informal chat about John Nash's main current interests. He mentioned again his reflections about cosmology. Concerning publications, Nash told us about a book entitled *Open Problems in Mathematics* that he was editing with the young Greek mathematician Michael Th. Rassias, who was conducting postdoctoral research at Princeton University during that academic year.

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