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Ei-ichi Negishi, professor at Purdue University, is one among the three organic chemists who received the Nobel Prize in Chemistry in 2010. He is well known for discovering Palladium-catalysed cross coupling reactions, better known as the 'Negishi coupling'. He delivered the Morris Travers lecture at the Indian Institute of Science on 10 January 2013, on 'Transition metal catalysis for a sustainable and prosperous world'. *Current Science* interviewed Negishi after his talk. Following are excerpts from the interview.

Can you briefly describe your early research?

I was with Professor H. C. Brown at Purdue University as a postdoctoral student. There the research was all about organoboron chemistry. I was fascinated by Brown's hydroboration reaction involving boron. But the product boranes were in most cases oxidized to get corresponding alcohol. That was a fantastic reaction, but it is just one reaction. I wanted to use organoboron compounds for the synthesis of many other organic compounds that eventually led me to this cross coupling. What I was dreaming about first, eventually went to Professor Suzuki because he used boron and the reaction is now called 'Suzuki coupling'.

What are the key elements for the discovery of the Negishi coupling?

Now you use the right word 'discovery', which I emphasize very much in research and I usually show a scheme including ten elements of discovery. I call these 'ten conditions for discovery'. I think for discovery we have to have dream, which is my kind of discovery. So, discovery starts with a dream. But little lower kind of discovery starts with a need. In my case, I had a dream of synthesizing all kinds of organic compounds; give me an organic compound and I will make it. It took 14 years until my first discovery, i.e. from 1962 to 1976. To make a scientific discovery you have to be like a puppeteer, controlling ten strings. You need to recognize your needs and have high levels of desire or dreams. These two things should form good plans and in my case it was the Lego game like plan, which is the third component of discovery. Good judgement, extensive knowledge and more importantly many good ideas. These are all intellectual components. On the other side, we need to have mental strength. Many researches do not go as we hope but we should have strong will power. When many experiments seem to fail you need to be optimistic, I call it 'eternal optimism'. One should never get pessimistic. Brown was a typical example. The last component is 'luck' – serendipity. I think we should be able to discover many things without luck but if you are lucky, it is so much better. So, serendipity to me is the last component. All these things must be fed to systematic explorations, which represent the central part of discovery. Brown, my mentor, was a superior individual in systematic explorations. I think I learned from him how to systematically explore. Luckily, I have discovered a fair number of important things, Negishi coupling being just one of them.

How has the focus on organic chemistry changed over the years?

Organic chemistry historically has been through many phases. When I was a student the British chemists Robert Robinson, Alexander Todd, both Nobel Prize winners, were regarded as our leaders. Then, the centre of organic synthesis shifted to America. The first American giant was Robert Woodward; a Harvard professor who was able to synthesize relatively simple compounds by today's standards, but the great majority of organic chemists did not know how to synthesize them. Woodward was able to synthesize compounds like quinine and strychnine. So, he sort of opened the door to modern organic synthesis. He started doing this in the 1940s and 1950s, and in 1965 he won the Nobel Prize for his synthetic endeavours. Since then, putting together the big organic molecule became the central issue of organic chemistry. In 1990, E. J. Corey, another Harvard university professor received the Nobel Prize in the same general area. Since then, however, no one has won a Nobel Prize in the area of organic synthesis for the sake of synthesis. Our synthesis is not just for the sake of synthesis. Ours is synthesis for the sake of production of useful organic compounds for the mankind. Our kind of synthesis produces compounds that we need in medicine, biologically active compounds, electronic material, and so on. When you see a television, you see a thin screen for which they use an electronic material which is synthesized by our reactions. This is a more appropriate area of organic chemistry now. In the future, more and more biologically active molecules will become synthetic targets, which are very complex and different from previous targets since they are mostly chiral and optically active. It is very difficult to synthesize optically active compounds pure. We are pleased and proud to be able to say we have just come up with a synthetic method to synthesize a wide range of optically active compounds in pure forms. Essentially, all biological compounds are optically active compounds; these are the left and right-handed versions. Thalidomide is a very good example. On one-hand it is a very good tranquilizer, and on the other it is toxic. When a pregnant lady takes the latter, during a certain pregnancy period, crippled babies are born. Hence, we must synthesize biologically active molecules optically and enantiomerically pure, which can be done today as needed. Over the years changes are seen from artistic/pure scientific emphasis to more practical/real life need, from pure organic to bio-organic.

Did your life change after the Nobel?

Very much! Before the Nobel I may be travelling two months a year, after the Nobel I have been travelling nine to ten months a year! In 2012, I visited fifteen countries over five continents. Only continents I did not visit are Antarctica and Africa.

As a prolific author what do you think of scientific publication and how important is it for a scientist?

It is very important, because without scientific publication other people will never know what we have done. Scientific publication is one of the media through which we come across what others have done. But today in some countries, people think publication in a good journal is of utmost importance. In some countries, if you publish in *Journal of American Chemical Society* and so on, you get money from the government. But, I strongly disagree with this. Publication is important only because your results are very important. We should never reverse the order. Results are the most important factors. When you get good work you should publish as other people should know it. But without good results you should not publish. In some countries, I think people thrive to publish. They think that this is the most important thing in research. Publication is extremely important, and it should include your research and good ideas which are far more important than publication itself. A strange phenomenon is going around in the world. Sometimes, postdoctoral aspirants come to me and ask 'I want to work on *JACS*/*PNAS* projects', which does not exist as such. We work hard and as a consequence of our good work we may consider publishing the work in *JACS/PNAS*. You cannot put a cart in front of the horse. Their mindset is distorted. I say we do not have *JACS/PNAS* project. You and I should make it and then publish. It is a true story.

How should one choose a mentor in chemistry?

Top notch scientists are usually good mentors like the way they need to be. But some are probably not, as they care less about you working with them. Even if they are not the world's best scientists and they are terrible; still you can find some good things from them. Even if the person is very nice, if he/she does not know about how to do research, you cannot learn. The single most important factor in choosing good mentor in science is how good scientist he/she is. If he/she is a world top notch scientist, regardless of what kind of experience you go through, they should move to be good mentors.

What is your message to young Indian researchers?

Good discovery is the most important thing in research. If you do not make a discovery, you are not a successful researcher. You just interpret in a sophisticated way. Place 'discovery' at top of the list, above knowledge acquisition and other things. Knowledge is a collection of many things that others have discovered. Discovery makes new knowledge. Hence, place discovery way ahead of knowledge. In some countries, perhaps including India, knowledge acquisition has been a little bit overemphasized in the past. For future growth, increasingly greater emphasis should be placed on discovery, which is the single most important thing for scientific activities.

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