This section features conversations with personalities related to science, highlighting the factors and circumstances that guided them in making the career choice to be a scientist.

Face to Face with Professor P. Balaram*

Prof. N Sathyamurthy talks to Prof. P Balaram

Professor Padmanabhan Balaram is a renowned biochemist known for his work on the structure, conformation, and biological activity of designed and natural peptides. A recipient of the TWAS Prize (1994) and the Padma Bhushan (2014), he has served as the Director of Indian Institute of Science, Bangalore (2005–2014), and was the Editor of the journal *Current Science* from January 1995 to June 2013.

Prof. Balaram applies an amalgamation of techniques to decipher the factors influencing the folding and conformations of designed peptides. His group has investigated peptide sequences playing a key role in the formation of secondary structural motifs such as helices, beta turns, and sheets. Collaborating with Isabella Karle (1921–2017), he has also pioneered the use of alpha-amino isobutyric acid to induce and retain helicity and constrain peptide conformations.

Professor N. Sathyamurthy (NS): Professor Balaram, Good morning! On behalf of the *Resonance* team and on my own, I want to thank you for agreeing for this interaction. It will be put up as Face to Face for the readers of *Resonance*. From what we know from the social media, you did your BSc from Fergusson College, Pune, MSc from IIT Kanpur, PhD from Carnegie-Mellon, and a postdoc from Harvard. You then returned to India. The reason I summarize this before venturing into further questions is because our audience, the readers of *Resonance*, would be keen to know how our leading scientists have evolved over a period of time. Could you please tell our readers why or how you took this path.

Professor P. Balaram (PB): You know, looking back, it's very difficult to say how one took a particular path. Sometimes it just happens. In my own case, I had no intention really of doing research and would have probably happily sat for the civil services examination if I had not gone and studied at IIT Kanpur. It was there that I first experienced the thrill of working

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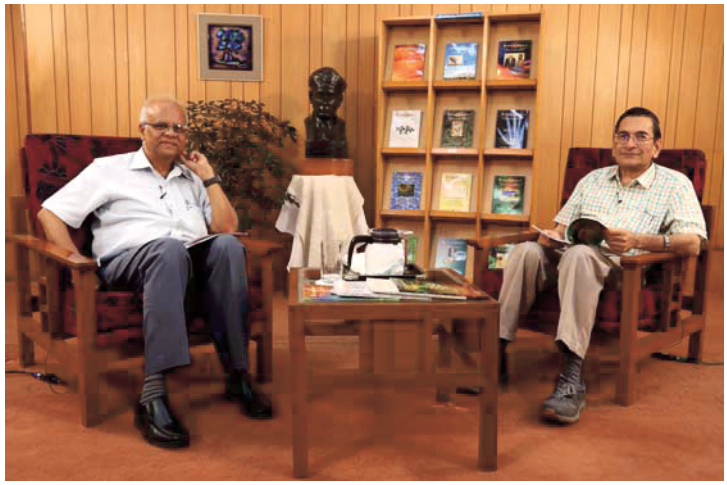
in the lab. And it was sort of inevitable then that one drifted into research. I went abroad, to the United States, and did a PhD. I did a very short spell as a postdoc and came back almost immediately thereafter to India. In those days when you came back to India, you were primarily interested in getting what one would call a 'permanent job', which would then keep you going for the rest of your career. I was fortunate to be selected as a lecturer at the Indian Institute of Science (IISc), Bangalore, where I have stayed ever since. So, I don't think, in making these choices one has actually done something very conscious. Sometimes it just so happens.



Prof. N Sathyamurthy with Prof. P Balaram, Indian Academy of Sciences

NS: It's interesting that you mention that you got excited while doing experiments as a student in IIT Kanpur. In your opinion, do you think that one of the most important things, when you are a student, is to be able to work with your own hands?

PB: Yes, I think, as a student, either at the undergraduate level or at the postgraduate level, it's most important to go and work in a laboratory. For instance, I was interested in chemistry. There's nothing as exciting to a young student as actually messing around in a chemistry lab. You know things are boiling, things are breaking, there is action all the time and in those days people used to work at night in Kanpur and for the first time, you sensed a kind of profession in which timing was not important. The whole day was actually the work day. I think for young students this makes a huge impression on their minds.

NS: Yes, I remember when I was in IIT Kanpur, we used to say that lights never go off in the Southern Labs to indicate that the students work day and night and, of course, the faculty is also there, seven days a week. Now, you were so excited about doing experiments in chemistry that you did a PhD and postdoc in chemistry. But after coming back to India, you worked

predominantly in the area of biology. I am not sure if I should make a distinction between chemistry and biology. I think it's better that you comment on it.

PB: I don't think I really worked in biology. I would still say that much of my work was in chemistry, but it was slanted towards understanding biological processes and biological phenomena. I work primarily in the area of peptides and my choice of working in the area of peptides was determined by the department in which I actually got a position. I was appointed as a lecturer in Prof. G. N. Ramachandran's department, which was the Molecular Biophysics Unit (MBU) at IISc Bangalore. Ramachandran had just moved from Madras. Ramachandran, in my view, was probably one of the most outstanding scientists of post-independence India. His work has found its way into textbooks and his name is forever linked with the kind of work that he did, which led to the Ramachandran map. Ramachandran was a very dominant figure. I was a little less than 25 years when I came to his department. He was, of course, a very senior and established figure. I quickly realized that one had to do something in which he was interested; otherwise you wouldn't have a good time in the department. And then as I listened to the kinds of seminars and discussions which took place, I realized that the field was a very fertile one for a chemist to move into. So I began to work on the field of polypeptide conformations, stereochemistry and so on for which I was well trained.

I didn't know anything about the field until I came to Bangalore. So I think it was really Ramachandran's influence. He sort of nudged me in the direction which I subsequently took, although he wouldn't have realized it himself.

NS: Would you say Prof. G. N. Ramachandran was your role model?

PB: I frankly don't like the term 'role model' because you know when someone asks Virat Kohli whether he's modeled on Sachin Tendulkar he doesn't quite like it. I think everybody would like to be themselves. But you get unconsciously influenced by the people who surround you. And when the people who surround you are exceptional, as Ramachandran undoubtedly was, then I have to say that he was a dominant influence on my thinking. I always wanted to really understand how he actually did all the wonderful work that he did in Madras.

NS: I understand what you are saying. I would like to ask two questions. Would you like to say a couple of sentences about the work that Ramachandran did? People talk about the golden era of physics, including in India, in the early 20th century. Ramachandran was not part of that golden era. So would you like to comment on these two issues?

PB: Yes, I should say a little bit about Ramachandran himself. Ramachandran was a student of physics and he worked with C.V. Raman for his PhD at the Indian Institute of Science (IISc). What Ramachandran did was to work in the area of optics, to begin with, and slowly

after he got his PhD, he began to do the first X-ray diffraction experiments in the physics department at IISc in Bangalore. In 1951, he was appointed as Professor of Physics at Madras University. That was when Madras University started its physics department and they originally wanted Raman to go there. But Raman had by that time reached the age of retirement. So, he suggested that Ramachandran go to Madras. So he went at the age of 29 as Professor of Physics. You can imagine even today a Professor of Physics is a formidable individual. In those days, he would have been an even more formidable individual as the head of the department and all that, at the age of 29. Ramachandran needed a problem to work on. When the British physicist and chemist, I should say polymath, J. Desmond Bernal visited Madras, he suggested to Ramachandran that maybe he should use X-ray diffraction to study the structure of collagen.

Collagen is the most abundant protein in our bodies. It's the protein which is there in connective tissues. It's what gives your skin all its mechanical properties. The structure of collagen was just being investigated in England and the United States by the foremost people in the field at that time, Linus Pauling in America and Francis Crick in England. Ramachandran began to work on the structure of collagen with his postdoc, Gopinath Kartha, who was another outstanding figure. The two of them together took X-ray diffraction photographs of collagen and using the special features of the amino acid composition, eventually worked out the triple helical structure of collagen.

It's quite remarkable, when you look back at this achievement. This was done at a time when all these structures were being worked out either at Caltech by Pauling or in Cambridge by the British group. This was absolutely contemporary work. Of the three structures that marked the dawn of structural biology, that is, the alpha helix of proteins, the DNA double helix of Watson and Crick and the collagen triple helix of Ramachandran, technically, the collagen triple helix was the most difficult structure to postulate.

I will use one technical term here. Proteins are polymers which are composed of amino acids and every third residue in collagen is the amino acid glycine, which is the smallest amino acid. Therefore, if you want a structure in which you have multiple chains which wind around a central axis, you would put the smallest amino acid residue in the interior of the structure. Because that's the only residue which can be accommodated. As I heard Ramachandran tell the story, he got the idea of the triple helix one day suddenly, when he was trying to solve this problem. While watching his wife braid her hair in the way of a traditional Tamil lady, he realized that you could have chains which coiled around themselves to give a rope-like structure. In retrospect, this looks remarkably simple, but it was, I think, an insight which came to him. I think, for students it's most important to understand that insight comes only to some people at some times. Suddenly, the problem appeared to have been solved. Subsequently, the story gets very interesting. Francis Crick of DNA fame was also working on the structure of collagen.

Ramachandran published his paper in the journal, *Nature*. Crick, of course, was quick to point out that the Ramachandran structure was wrong because in that structure atoms came too close to one another and, therefore, the structure would be, sterically impossible. This affected Ramachandran a great deal and he began to ask the question of what was 'sterically impossible'. Another way of putting it was how close could atoms come to one another. Therefore, one needed to find the van der Waals contact distance. If you can consider atoms as hard spheres like cricket balls you can't push them together beyond a point. They can only touch one another. So that would be the distance of contact. So Ramachandran and his colleague V. Sasisekharan and a PhD student, C. Ramakrishnan then looked at the available crystal structures. Remember, this was in the early 1960s. Very few structures were available and they looked them up and made a table of what was called van der Waals contact distances. Eventually, they showed that the collagen structure was in fact a stereochemically possible structure and, therefore, likely to be the correct structure. They also came up with a very general principle applicable for all proteins which permitted mapping of the stereochemically allowed conformations or three-dimensional structures of protein chains. This then led to the famous Ramachandran map, which you will now find in biochemistry textbooks. In fact, in the West sometimes young graduate students (I've had this experience myself) ask you whether 'Ramachandran' is a noun or an adjective because they don't know what it is; but it's the name of a person associated with two-dimensional maps that are used to understand the three-dimensional structures of proteins. So, Ramachandran's name is now immortalized in the scientific literature. At the time when I came to the department in Bangalore, I did not have the kind of appreciation for Ramachandran's work that I have now. At that time I was young. I remember his asking me whether I knew anything about the structure of collagen. I was truthful. I said, "No, I do not know anything about the structure of collagen", and he was not very happy. But over the course of many years, particularly by interacting with my colleagues Ramachandran, Sasisekharan, and Ramakrishnan, I slowly learned much of what was known about polypeptide conformations. That then became the area in which I worked.

NS: In a lighter vein, thank God Mrs Ramachandran was braiding her hair in the traditional way and not like the modern girls letting their hair loose.

PB: My suspicion is, Ramachandran would have still got the structure I suppose.

NS: One technical question: The van der Waals radius was already defined in Pauling's book, *The Nature of the Chemical Bond*. So Ramachandran was able to correlate the stereochemical angle and the van der Waals radii. Is this correct?

PB: Not entirely. For much of the protein computational work that is done today it is a Table which appears in his second paper which is used. Over a period of time, to account for experi-

mental data, atoms are no longer considered like cricket balls but they have now become tennis balls, and you can press them a little bit further against one another. So, there is a bit of give and take as far as van der Walls distances are concerned.

NS: You mentioned that Ramachandran was a physicist working on a biology problem, so to say. But Ramachandran himself was originally trained as an electrical engineer. Isn't that right?

PB: No. Ramachandran was not trained as an electrical engineer. He had done a degree in physics and then came to join the electrical engineering department at the Indian Institute of Science. The story (possibly apocryphal) goes that he wasn't interested in electrical engineering but in physics. So, he would come and stand in front of Raman's laboratory waiting for Raman to come in every day. Raman saw this and spoke to him, and Ramachandran expressed his interest in physics. Raman quickly realized that Ramachandran was an exceptional student. Raman called the professor of electrical engineering telling him that Ramachandran was too smart to be an electrical engineer and that he was transferring him to physics. So, you can see that Raman would not have endeared himself to his colleagues. But he took Ramachandran.

NS: If Ramachandran could do it in the 50's and 60's in India, people ask this question: Why is it that Indians working in post-Independence India have not done work that gets the Nobel Prize?

PB: I think it's a sort of unfair criticism to make of Indian scientists today. I feel you have to compare scientists at the time that they were working. In the early 1950s, science was still a relatively limited activity in the United States. The United States' postwar thrust to put a lot of money into universities for scientific research was in its early stages. The scientific community worldwide was a relatively small one. People are sometimes fortunate, given the time at which they work in a field. It was probably much easier to get into the Indian Test cricket team 50 years ago than it is today because everybody wants to get into it. Every parent, who can afford it, now put their children into coaching camps, now that the potential of the area has been recognized.

Today's science is a vast enterprise and the kind of money and effort that is being spent everywhere must be recognized. There is China, there is America, there's Japan and Europe, and so on. Therefore, the Indian contribution will necessarily be smaller and smaller with respect to the worldwide expansion.

NS: Then what do you think that the young scientists should do?

PB: I don't think scientists should worry too much about Nobel Prizes. They shouldn't worry too much about doing science for the sake of recognition. But they must look at the

task of doing science as something that they would like to do.**I mean every artist does not paint expecting that his paintings are going to fetch a million dollars**. **Every poet does not write a poem expecting to become Rabindranath Tagore.** We do need to do things that we are interested in. Science is an extremely exciting activity. Today my only regret is that I am not younger. I still like messing around. I still like talking to students, and I still like doing something exciting. And if you don't do something all the time, your chances of finding anything are negligibly small.

NS: So would you say that curiosity-driven science is still relevant?

PB: I would say, as far as science is concerned, it is driven by curiosity. And I think today's emphasis on applications, even before you understand something, is not likely to yield fruits. I think if you do very good science it will automatically have an application. And the only way to do good science is to be curious, and do what you like to do and do it despite what anybody else around you thinks about it. This is very important in science.

NS: You have spent nearly four decades of your life studying the structure of proteins. If I remember right, you have also worked on introducing some unnatural amino acids in order to understand protein structure. Looking back, do you have a feeling that you have uncovered some facets of protein structure?

PB: Yes, I'll say a word about my own work. When it started, I was actually looking for a problem to do and that problem would have to be in the area of proteins or polypeptides because Ramachandran's department was focused on this. This was a biophysics department in which every faculty member had been trained in physics or physical chemistry and I was the first person who was trained in organic chemistry. Therefore, I was expected to do chemical and biological work, whereas the other people would work on something closer to their training in physics. So I began to do synthetic work on making small peptides, small fragments of proteins by chemical synthesis. When I was looking for things to do, I came across a paper, which corrected a structure determination of a naturally occurring molecule. When I looked at it there was an unusual amino acid in the structure. When I looked up this amino acid, it was not available in the catalog, and it appeared to be synthesizable from simple available chemicals, acetone and ammonia. Therefore, I thought if I made that amino acid, which nobody else had, I would be able to make molecules with it and slowly begin my career. It was not one of the 20 genetically coded amino acids, and it turned out it was relatively easy to make. Afterwards, we began to make peptides with it and then to my utter surprise, all the molecules that we made gave very beautiful crystals. Our department was full of physicists wanting to do X-ray diffraction, and so we began to do X-ray diffraction on these and determined their structures. At that time, the general feeling was that peptides were hard to crystallize, but these

peptides began to crystallize everywhere. It wasn't that we were better than anyone else. It was just that the molecules were crystallizable. This allowed me then to think of problems where one could mimic the structure of proteins, using what one calls now as conformationally or stereochemically constrained amino acids. The Ramachandran allowed conformations could be selected out using these amino acids. I then worked on this for many years. Because of this work, I became interested in proteins and then began to do protein biochemistry and worked with enzymes. My work has been primarily in the area of what I would call polypeptide chemistry, protein biochemistry and I've used a variety of techniques. I like to use any technique which comes along. And so I've never been particularly specialized, be it nuclear magnetic resonance or crystallography or computational methods or infrared spectroscopy. At some point or the other, I've used all of these. So, effectively I've been viewed by most of my colleagues as someone who is not very specialized and a little bit of a generalist.

NS: So you were looking at a problem and then using an appropriate tool rather than having a tool and looking for a problem to apply to?

PB: Yes, I think it's very important to have problems. The tools come along. This, I must say, is one thing that I learned at Harvard, in R.B. Woodward's laboratory. Because, if you look at every one of Woodward's major syntheses, the tools to study the molecules or the intermediates did not exist at the time that he conceived the synthesis. But every tool came along. Most dramatically, in the case of vitamin B12, which I witnessed, two techniques appeared on the scene; nuclear magnetic resonance (NMR), specifically, Fourier transform NMR and high-performance liquid chromatography (HPLC), which is commonplace today. But in 1972, the only place where HPLC was being done in an academic laboratory, was at Harvard on the B12 synthesis.

NS: So, what is the most important thing that you learned by working in Woodward's lab at Harvard?

PB: One thing I learned in Woodward's lab was the different ways in which people worked. The group was international. There were Germans, there were Swedes, there were Japanese, there were Englishmen and the odd Indian. Not many Chinese in those days. Maybe an American-Chinese. But everybody had their own working style in the laboratory. At times, I thought that the least disciplined were the English and the Indians. Woodward was an inspirational figure. One of the things one learned from him was how hard one had to work. Woodward was different in many ways. He was a very easy man to admire, but a very difficult man to follow. You know you can't work 16–18 hours a day. You can't work without sleep. These were things he could do. He would prepare his lectures endlessly – for eight hours, 10 hours and rehearse; and then go and give the lecture. His lectures were very famous. I've heard a lecture which

lasted for over three hours, which is now available on the Internet. But there had been lectures, which lasted for five.

NS: This was on the synthesis of vitamin B12?

PB: Yes, it was on the synthesis of vitamin B12, at Harvard where he spoke for over three hours.

NS: People today emphasize on collaborations. During your studies of protein structure, you were collaborating with Dr Isabelle Karle. Once I heard from you that you had not met her and yet you were collaborating for a long time. Was that easy? Was it before the Internet era?

PB: This was long before the Internet era. You see, we had reached a stage in making peptides where we had gone on to fairly large peptides. And to our surprise, these were crystalline and, therefore, you could apply X-ray diffraction. But in those days we did not have diffractometers in Bangalore. Collection of data was not easily possible, but even more importantly, the molecules had reached a size where direct methods of phase determination were hard to apply, and the methods of protein crystallography could also not be applied. They fell in an intermediate range. Isabella Karle was the world's leading expert in pushing direct methods as far as they would go. In fact, her husband Jerome Karle received the Nobel Prize for the mathematical methods. Isabella Karle, who was left out of the Nobel citation did all the structure determinations which established the method. She was an eminent scientist and was interested in peptides. I knew her work. I was young back then and never thought twice about writing her a letter in 1985. She wrote back a very cautious letter, telling me nicely that she could not commit herself to do this, but if I sent her the crystals, she would take a look at them. In those days, after the student got the crystals, I would sit at my desk, take them out and put them on a glass slide, transfer them to the test tube using small implements, rather like the kind of things that dentists put into your teeth. Then we would seal up this glass tube and go to the post office and send it. After I sent her the first crystal, she found that it diffracted wonderfully. She'd never had a molecule of that size. She was most excited, and she wrote back. At that time she would send everything by airmail. Fax had not appeared. This was probably 1985, and we had a minor disruption because I think shortly after we started the collaboration, Jerome Karle got the Nobel Prize. Isabella wrote saying that because of the festivities, the work was a bit delayed. She then began to determine these structures. So we would collect the crystals, pack them and send them away and wait for the postman. I must say that the postman has played one of the most important roles in my career, both at the time I got a Fellowship to go abroad and later to get structural data.

She was much older than me. She passed away some time ago, in her 90s. When I started collaborating with her, she was already probably 65 or thereabouts. And then I had a very

fruitful collaboration. Our first paper was published, probably in 1985 and our last paper was in 2014. So, it was a long collaboration.

NS: It's interesting to note the age of Isabella Karle when your collaboration started. In this country, people think that even scientists should retire at 60, 62 or 65. Don't you think that a lot of talent is being wasted in the process in this country?

PB: Prof. Sathyamurthy, your question is a loaded one, and anybody who hears my answer will say that I am biased. Strictly speaking, like in the United States, there should be no age discrimination in many things, science amongst them. I don't think age should matter. It should really be only one's ability – physical and mental. Being physically fit doesn't mean that you are mentally able. Also, there are many people who are physically not very strong but are mentally perfectly fine. So everybody should be allowed to do what they're good at. Where funds are limited, one should give funds and space to younger people, but I don't think one should put everybody out to graze in the fields. You are just wasting people. You're wasting teachers. You're wasting researchers. Many of them could be used to advise in projects, using their expertise. You see, there is no substitute for experience. Today, for example, when you go to the World Cup, Mahendra Singh Dhoni might still be the most important man in the Indian team. Remember 37, 38 or 39 is an advanced stage in all forms of cricket. Roger Federer is doing pretty well in tennis. Science doesn't require that kind of physical stamina. It requires mental stamina and the question is, "how do you judge mental stamina?" Now, here we have some young people listening to us. It's quite possible that my mental stamina might exceed theirs. We don't know. I wouldn't say age is important. Isabella Karle worked right to the end. I will tell you one thing about her. Every letter of hers was handwritten, right to the very end; even when she reached 90, and they were beautifully handwritten letters. The only concession to technology was that they used to arrive by Fax instead of Airmail. She never entered the Email era, and so there was absolutely no question of the Internet.

NS: That's great! Today people talk about systems biology. During this conversation, you emphasized on physicists looking at biology problems. You yourself a chemist started looking at a biology problem. What do you think is the progress on systems biology, and what happens to the traditional botany and zoology subjects, in this country and elsewhere?

PB: I'll take a while to answer this question because this requires some explanation. Traditionally, if you were going to understand a biological organism, be it a human being or a single-celled bacterium, the traditional way is to break it down into its component parts and try to understand all the component parts. This is what is called reductionism, and molecular reductionism has dominated biology all through the 20th century. This approach comes from physics, where if you want to understand matter, you must understand atoms; if you want

to understand atoms, you must understand what they are made up of. It's the same story in biochemistry.

Biochemistry is chemistry; the only qualification is that you are dealing with complex molecules. Reductionism has been wonderfully successful if you want to ask a question at the molecular level. In order to understand complex biological systems better, you must understand the interactions between all the components and the manner in which exquisite control is maintained in a cell, of all the chemistry that is going on. If you look at a metabolic chart, it is full of chemical reactions, illustrated by chemical structures with arrows connecting them. Enzymes, invariably proteins, catalyze every reaction. The chart will look like a traffic map of roads going all over, everything connected. It's a network. The people who understand networks best are electrical engineers because they've been dealing with networks all the time. Today, when the grid fails we know that some very important node has failed in the network. So the idea of networks, nodes and connectivity comes in. But if you look at every chemical reaction, the rate is determined by a rate constant. If it's an equilibrium, it's determined by an equilibrium constant also. But then all these reactions are coupled to one another. So you have this enormous complexity in intermediary metabolism. I think this is one of the first areas in which the systems approach is proving to be very fruitful. In a way, an assembly line industrial process has a starting point and an ending point. But in biochemistry, there's no starting point and no ending point, but many things are circular. And they branch again into other circular reaction sets. So one should look at the traffic map inside the cell very carefully. This is the systems approach, but the systems approach can be expanded. You can ask the question, what makes a liver a liver. It's not just a cell, it's a large collection of cells. Now the question is, "Can I now understand enough about this to simulate a liver on a computer?"

NS: The next logical question would be then, creation of life. Is this a chemical problem or is it beyond chemistry?

PB: No, I think the fundamental problems of life are beyond chemistry. They are even beyond biology.

NS: Let me ask it differently. Would we be able to create life in the laboratory? In a true sense?

My view, at least at present, is, "It isn't going to be possible in the foreseeable future". I suspect that the foreseeable future is a very very long future. There's a very famous statement made by a very prominent biologist, Lynn Margulis, who passed away some time ago. She's the one who recognized that mitochondria inside cells are like bacteria and, therefore, eukaryotic cells must have arisen during the course of evolution, by one kind of bacterial cell engulfing another bacterial cell. The new cell acquired both mitochondria and a nucleus. So she suggested that archaea and bacteria fused together to give a eukaryotic cell. The 'tree of life' has branches:

bacteria, archaea and eukarya. But it's a tree which doesn't have roots and it doesn't have a trunk either. This is because we don't know how long is the connection between chemical evolution and biological evolution. Darwin was very careful. He called his book, *The Origin of Species*. He never called it the origin of life. So speciation and biological evolution were determined by natural selection. But in one small paragraph towards the end of his book, he says, may be life evolved in some small warm pond. Available chemicals presumably got together and made a cell, and then cells learned how to do other things. That's a complete black box at the moment. **So, the completely unsolved problems are: How did chemical diversity originate on Earth? What were the earliest kind of self-replicating organisms on earth? And how did organismal complexity then reach the level of bacteria, archaea and eukarya?**

NS: Does Miller's experiment throw any light on this?

PB: No. Miller's experiment is a primitive experiment. It shows that molecules can be formed under drastic conditions from simpler molecules and those simpler molecules are the ones which you might expect in the atmosphere and in interstellar space. There's a paper that has just appeared in *Nature*, which talks about what was the first molecule to be formed in the universe. The first molecule to be formed in the Universe, the authors suggest, is a species which has a helium atom joined to a proton. So in stars, it is helium and hydrogen which were the first sort of elements to appear, and the subsequent synthesis under diverse extreme conditions would lead to simpler molecules and from there to the kind of molecules, which are responsible for biochemistry. It is a long long way: hydrogen cyanide, oxygen, carbon monoxide, carbon dioxide and, of course, minerals. These are a few molecules people talk about.

NS: It is interesting you mentioned about this paper on the formation of HeH^+. Formation of H_2, the hydrogen molecule itself still remains an open question.

PB: Yes, in fact, I was struck by this paper. I would have thought hydrogen was simpler than helium but they are definitely silent on hydrogen.

NS: We will talk separately on this. I want to come back to your career. You were a scientist and then you became an administrator. You played a very important role in policy making in this country. You also managed to be the Editor of *Current Science* for many years, and every two weeks you were also writing wonderful editorials. Many of us read your editorial even if we didn't read the rest of *Current Science* issues. Now I want to ask this silly question: How did you manage all that?

PB: I do not know. You know, it just so happened. When Professor Ramaseshan took over

the editorship of *Current Science* in 1988, it had reached a low point as many Indian journals do. He was keen on reviving it. He appeared in my laboratory one day and told me that this was what he was going to do, and he sort of drafted me to the task of helping him. I never figured out how he arrived there and why he thought of me. But I began to work with him on the technical aspects of the journal, in reviving it, what to do and so on. I was always interested in writing. When I was in college, I actually worked on a college newspaper, which I, along with others, started. It was called *The Fergusson Review*. I was always interested in taking up journalism as a career. But in the 1960s that didn't seem to be an option where you would be able to keep body and soul together. So I went to do a Master's degree. But the desire remained with me. So, here was an opportunity to write. I hadn't really thought of writing too much until one editorial board meeting. You know, editorial board meetings are interesting; all the people who attend and make suggestions generally never do anything other than make suggestions. And one senior person said, "You know a journal must have an editorial. Look at *Nature*." So I started.

NS: You mean earlier there was no editorial in *Current Science*?

PB: No, there was no editorial in recent times. I went back to the issues of 1930s and found editorials in the early years. The Journal started in 1932.

NS: Was it started by Raman?

PB: No, it was not started by Raman. Raman gets credit for many things he didn't do. In Bangalore, everything in science in the 1930s is attributed to Raman. It was started in Central College by C.R. Narayan Rao and a small group of three people. Among them was Professor M. Sreenivasayya, who was also there at IISc. This group started *Current Science* in 1932. The driving force for starting a journal came from the then director of the Indian Institute of Science, Martin Foster, who had actually written a note, in 1928, that there should be a journal and so on. Raman arrived in Bangalore only in 1933. Later on, he became the President of the Current Science Association. Once he became the president, he remained the president for life. I think that's where the story started. It did have editorials in the early days, and those editorials were extraordinarily good editorials because they were written at a time when the freedom movement had started. India was thinking about other things, and they were remarkably well-written editorials.

NS: Since this interaction was driven by the *Resonance* team, before I thank you for this wonderful interaction, I want to ask you if you have a message for the young readers or students who are undergraduates. Many of them find their peers going for Engineering and Medicine. What should they do? What should keep them motivated if they want to become scientists?

PB: You know I have never been in the business of giving advice because I think usually people find what they want to do. The only thing that one might say is that you should do what you like to do. Never get into a course or a profession because other people are getting into that profession. If you go into science, it helps to be independent-minded, because you shouldn't be really driven all the time by fashions and what everybody else thinks about what you're doing. Unfortunately, that is not the case today in India. But I would say anyone who's in college should give serious thought to what they'd like to do. If they can't figure out what they'd like to do, they should take some time to figure out what they would like to do. Maybe do a course, and see what it is like and whether they like it or not. I think today's students have an advantage. They all are going to live long. And they all are going to have long careers. So in the beginning, a year or two lost in trying to search out what you want to do isn't really lost, as long as, you use that time effectively.

NS: I guess we lived in better times where we were under no pressure. We did whatever we wanted to. But today's generation seems to feel that they are constantly under pressure – peer pressure, parental pressure, societal pressure, etc.

PB: I would disagree with you on that. I don't think you were in a generation where you did what you wanted to do. You and I were in a generation where we did something, sort of unknowingly, pushed along by circumstances and events. But we belong to a generation which learned to like what we were doing. It's a bit like the arranged marriage versus the love marriage. Arranged marriages worked perfectly well for a lifetime. I would leave it entirely to people to figure out what they want to do. And I think today's generation is much better informed. And there's no excuse for them not to be informed because of all the technologies that are available. They should be able to make up their minds.

NS: Thank you.

Prof. N Sathyamurthy is the Chief Editor of *Resonance*, Indian Academy of Sciences, Bengaluru. Email: nsathyamurthy@gmail.com

Prof. P Balaram is Emeritus Professor, Molecular Biophysics Unit, IISc and DST-YOS Chair Professor, NCBS (TIFR), Bengaluru. Email: pb@iisc.ac.in