**A Short interview with three eminent scientists. Lecture 14**

**1. Interview with Professor V. Devanathan**

*What are the requirements for a successful research career?*

*Prof. V. Devanathan* : Motivation and innate interest in the topic of his research pursuit are the requirements for a successful research career. If a person takes the research not by compulsion but by his own choice, then he will not feel it as a burden but pursue it as a hobby. *Science is at its best when it is a part of a way of life* - this is the inscription that is found on the foundation stone of Institute of Mathematical Sciences, Chennai and truly describes the correct aptitude for a successful research career.

*Is it possible for an average student to come up with novel results in a research problem? If so, what kind of approach he should follow?*

*Prof. V. Devanathan* : Usually, the assessment of a student as good, average or bad is based on his performance in the examinations. There are some who are good in examinations with a good memory for reproduction but lack in deeper understanding of the subject and originality in approach. There are some who are not so good in examinations but show originality in thinking and follow unconventional or novel approach to the subject. There are a few who are good both in examinations and research. So, an average student with an ability of average performance in the examinations, need not feel different if he has *originality in thinking* and *self-confidence*.

*During a research career, a young researcher may come across disappointing moments like not getting expected results, rejection of a research article from a journal, etc. What kind of mode of approach a researcher should have to face such situations?*

*Prof. V. Devanathan* : *Success begets success and failure begets failure*. Success and failure are like two sides of a coin and one is bound to face them alternatively in the course of one’s research career. Elation at the time of success and depression at the time of failure are usually mitigated if one works in collaboration with others. At the time of depression, the co-workers come to the rescue and prop up the sagging spirit.

*In our manuscript we have mentioned the following:*

*Each and every bit of work has to be done by the researcher. A young researcher should not do the entire work in collaboration with others. The researcher is advised to perform all the work starting from identification of the problem to report preparation by himself under the guidance of supervisor.*

*Please give your views on this point.*

*Prof. V. Devanathan* : At the initial stages, the researcher gets the support of the research group in which he is working and he acquires the knowledge of the group effortlessly. The weekly informal seminars, if conducted within the group, will increase the pace of learning and help to clarify and crystallize the problems. This process of learning is made easier if the young researcher works in collaboration with others. This is true both for theoretical and experimental work. At present, the experimental work is almost a team work and successful research group is one in which the group leader allots the specified work to individuals taking into account his ability and expertise.

**2. Interview with Prof K.P.N. Murthy**

*The common belief is that research is laborious and painful. Many times you have mentioned:* Doing research is an entertainment*. Please, elaborate on this statement of yours.*

*Prof K.P.N. Murthy* : Research not only constitute a discovery or creating a new paradise but also consist of obtaining a personalized understanding of a phenomenon. The struggle that you go through for obtaining an insight into a phenomenon or getting a hold of a nuance and the extessy that you get when you get an understanding of a phenomenon or obtaining a new way of explaining of that phenomenon may be unmatched. This ecstasy is nothing to do with what yours creative have impact on science and society. However, it is the ecstasy of what Einstein got when he created special theory of relativity or Feynman when he created quantum electrodynamics or Raman when he found the so-called Raman lines. It is this makes the research an enterprise of joy. It is that makes a research an entertainment.

*Is it necessary for a beginner of research to learn all the aspects of theoretical, experimental and numerical techniques involved in a topic before he take-up an actual research problem?*

*Prof K.P.N. Murthy* : A certain basic knowledge about physics and mathematics is must for starting research. That is it. Several things you learn doing research. Ignorance of even some of the basic elements is no hindrance for creativity. What is required for doing good research is an enthusiasm, a commitment and willingness to go back to basics and learn them right.

*Before preparing the final write-up of your research work, you have the practice of discussing the salient features of your findings with a few other researchers. How are you benefited from this?*

*Prof K.P.N. Murthy* : After you have completed a piece of work I find it is a good practice to discuss with your colleagues the important findings that you have made. I have always realized that I got a better understanding of what I have done when I tried to explain to my colleagues about my work in a convincing way. The very act of speaking of what you have done removes the cob-webs in your understandings. I always make it to give a seminar on my work to a larger audience before submitting it to a journal for publication. I feel this is a very good and helpful practice.

*Enjoy doing research and approach it as an entertainment and a mode of getting happiness. This is your suggestion to young researchers. Please, brief it for the benefit of youngsters. In what way will this be helpful to a researcher?*

*Prof K.P.N. Murthy* : In any human enterprise it is important that one likes what one does. The hard work that you have put in a problem does not tired you and rest be assured if you approach a research problem with joy and you will get a good result. Publication of that result and the acceptance that you get from your colleagues become secondary. The satisfaction that you obtained by doing a job well is a reward by itself. I would say that youngsters should have this attitude towards whatever they do.

**3. Interview with Prof Sudeshna Sinha**

*Despite unavoidable tasks a woman of our country has, you have become one of the leading scientists in theoretical physics. What are your advice and suggestions to young researchers particularly to young women researchers?*

*Prof Sudeshna Sinha* : It is indeed somewhat harder for women to concentrate on career planning - especially when their children are young. One will have to accept that household tasks will always be there. The hardest thing is not really the number of hours of work one can put in - but the *quality of concentration* one can achieve. Here discipline comes in. Since women will probably manage to get fewer hours of academic work done every day they need to really plan the academic work they hope to achieve every single day. So it is most beneficial to discipline oneself into shutting off all daily chores *from one’s mind* for some hours every day. The point is to learn efficiency – and to appreciate that one does not have the benefit of unlimited time (as others will make justifiable demands on your time – like children).

Also women may find it hard to pursue academic work at certain points in their life - but they must preserve the self-confidence and will to return to academic after such times are over. They must realize that in 3–4 decades of working life – a few years is not a big deal. They should not think that a break in career is *irreversible*.

*Publishing in reputed journals (like Physical Review Letters) is a dream or prestige for many physicists. What are the secret of yours for regular publications in reputed journals? What type of problems one has to take up for getting published in top-level journals?*

*Prof Sudeshna Sinha* : With journals like Physical Review Letters one must remember two things: First, always try and make a case of the general interest of your results. The commonest grounds for rejection is *lack of broad interest*. This is very subjective of course, and being Indian does not help. But still, at the outset, one should make an attractive statement of the general scope of one’s work (that is, try to answer this hypothetical question: Why should someone not doing research in this exact narrow sub-field be interested in reading my paper). Second point is persistence. Take all criticisms of the paper seriously

(and don’t reply needlessly aggressively to the referees) and try to answer all the criticisms.

Then resubmit, and *don’t give up till the last round!*

*How could a beginner of research come up with novel results?*

*Prof Sudeshna Sinha* : Well, I think coming up with *novel* results is not entirely in one’s hand. There is an element of good fortune here! If the guide of the young researcher can identify a problem that is technically easy to tackle – but whose results can be of considerable potential interest – then there is a good chance for the young researcher to get a novel result. But this is not in the hands of the young researcher, and most often not in the hands of the guide either (as it depends on the subject, timing etc.).

**In this matter I always tell my students: whether you get a novel result tomorrow is a matter of luck, but in a career spanning several decades, if you work steadily and think deeply about the subject, it is almost assured that at some point or the other, you will get a good idea which will lead to a novel result!**

*To get a deep insight into the topic or problem of research, what are the ways a young researcher can follow?*

*Prof Sudeshna Sinha* : One should not just passively *read* papers or books! One should try to work it all out in some detail. While reading passively one feels one has *understood* – but only when one is trying to solve something does one gain any real understanding. In fact it is a great idea to look at the title and abstract of a paper, and then ask oneself how one would have attempted to work on such a problem and only then look at what the authors have done.