**Introduction to Research**
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**Lecture - 03**
**Art of inquiry - Postulating**

Prof. Prathap Haridoss: I think, many of the points that you heard so far do convey this idea that first of all, you have to be interested in the area, and that is where we feel, you know, that you should have… when we keep saying passion is something that comes because over period of time you have been very interested in that area and you have recognized that you are interested in that area. So, that itself takes some while for you to figure that out.

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There many things that you might feel that you are interested in, but there is something that really strikes a chord with you, so then that is something that you pickup. I think through this discussion also indicated that there is a general acknowledgment - both in the student community and you know people who have already been researchers and so on - that research is challenging. I mean, there are a lot of aspects of it that are challenging. For I think the most important reason for that is also the fact that we are learning about research as we do research; so especially the first-time researchers. When you come in as a student, you actually still may not fully be, you know, aware of what all is involved here, and as you keep going through the process, you are learning the process. Whereas, when you do course work, it is something that you have done since first grade or first standard onwards you have been doing course work, you do classes, you write exams, you get marks; that is a pattern that you know. This is a completely new pattern that you are handling, which comes, you know, all of a sudden after you do your bachelor's degree or your first masters degree and so on. It becomes challenging for that reason because you are both trying to work to, you know, go through the process, and you know complete the process, and if in the back of your mind, the intention was to get a degree, then that is also there ahead of you. And the whole process of doing it, is also something that you are discovering. So, that is why, that is one of the reasons why it tends to be challenging. And as we also pointed out, there is lot of routine work that is involved here, which you should not over look. It is not like, you know, you come in here and suddenly you come in to a research program somewhere, and then, you know, one fine morning, you suddenly get a bright idea and that's it, your work is done; it never works that way. It is a lot of routine work that goes on, that you have to keep working on, and building on all of that routine work, you know, your experience goes up; your experience goes up, your confidence goes up, your ability to, you know, identify insights into that area starts going up and that is when you actually start making those in-roads, which you can look back and say, you know, I was now beginning to do research; so that is the point that you need to understand. So I would say, you know, even when you look at research, and when you say from the perspective of how you discuss it in front of others, when you do experimental work, and you collect data, in a sense you are doing that drudgery; all those hard work that, you know, is involved in collecting the data. When you discuss it, when you try to put perspective into it, when you try to say that, you know, this is the reason why the experiments gave that particular answer, that is when you are sort of actually doing research, beginning to analyze that data, and convey something out of it, which is more than just simply saying that you know particular parameter increases when you an increase some other parameter. So, that is just data, but why it is increasing is what research is all about.

Prof. Abhijit P. Deshpande: An aspect of why we have said also research is challenging is, is its long term and cyclic nature of it. This I have experienced with second year, third year, undergraduate students when they come to try to just see, you know, I want do a research project, what happens is when you start off, there is of course learning and you are doing new things, and you might get some good results also, but sooner or later three weeks or four weeks down the line, you will… may be either something may not work or may be what you thought actually was opposite of what you are getting, and all those things - so that’s the cyclic process. So, research, somewhere you will find that there are things which actually are not according to your expectations and that is where the what I found is, some of the students who can actually pass that, during those phases to fall back on routine, to fall back on your knowledge, and try to think of something different, and then pass through that negative and then come up again, so that’s the big spirit, that is where it is very, very challenging. You have to persist as Arun said earlier.

Prof. Andrew Thangaraj: It is all in the mind.

Prof. Abhijit P. Deshpande: Yes.

Prof. Andrew Thangaraj: This quote.

Prof. Arun K. Tangirala: I think also, may be to reiterate what Phani mentioned earlier, as you are working through, it is important to keep focus, to stay focused on the short-term part, but not to lose sight of the big picture that you are working towards, and it always helps projecting what you have in the context of the big picture. Where does your result fit in? Is it really fitting into the big picture nicely or is it really taking you away? And the other thing that I want to mention is in research you may start out with the problem definition in mind and as you are working towards a problem, solving the problem, you may find some other problem which requires or probably or which is of equally important proportion or magnitude, that is probably worthwhile carrying out research in itself, which means that your problem definition is subject to change along the pathway. Of course at some point in time, for practical reasons, you have to freeze a problem that you are working on, but however, in the first year of your working, the problem definition can change, and some things that you thought were trivial have really not been addressed by anyone, and you find that there is a lot to work in there, and that can become your own problem of research and so on. So there is a quite a bit of flexibility in the initial years, which has to slowly freeze. It’s like you start off making ice cream, and finally, you have a frozen product as an ice cream, you cannot really have a liquid state also. There is a molten state, then there is liquid state, and then there is a solid state. It goes that way, right.

Prof. Prathap Haridoss: So, some of the things that we have been discussing so far are nicely captured in this quote which is from JC Bose.

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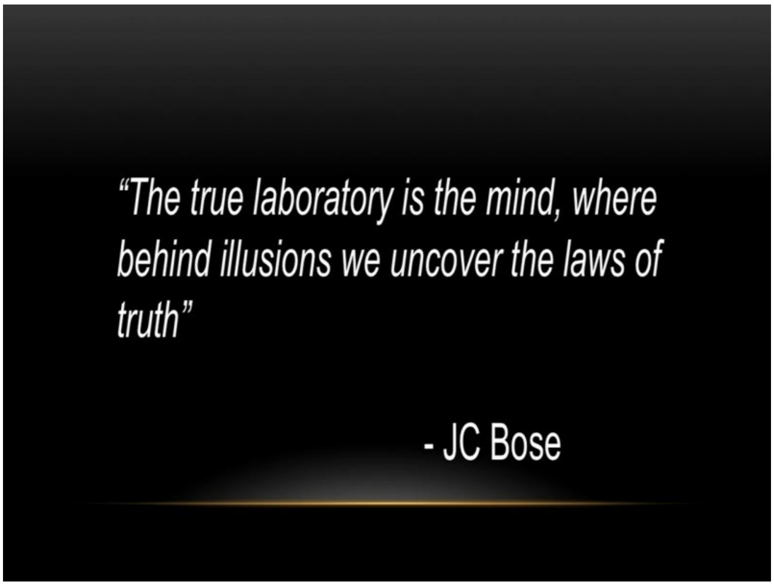


And it simply says, “The true laboratory is the mind, where behind illusions we uncover the laws of truth”. So, that is the quote attributed to JC Bose and it sort of brings out this idea that, you know, we are applying our mind to look at new things, in new ways, and trying to come up with learning, which we can then convey to others.

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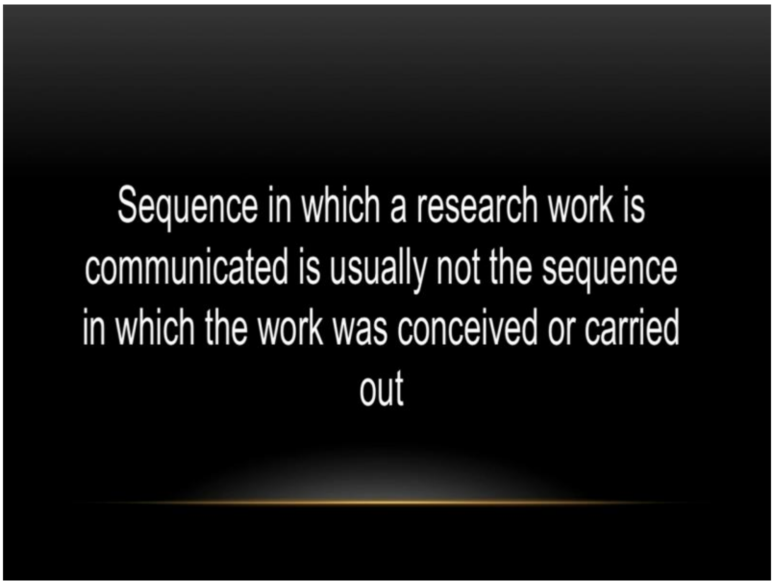


As we also discussed through this process about the mundane aspects of research; we also had this comment from Abijith, which was that it is a cyclic process. So, often you start off, and you feel very happy you know that your initial experiment started working, and then I know two, three weeks down the road something does not work; and I would say that is the biggest difference between what I say an undergraduate student trying experiments for the first time goes through and say a more seasoned researcher goes through. Typical undergraduate student going through research or attending the research for first time gets totally disheartened, when the first set of experiments fail, and it really looks like the whole process was pointless, the whole exercise was pointless. Whereas a true researcher, who has had experience in the process appreciates that, you know, this is likely to happen, is quite comfortable with the idea that, you know, his first set of experiment did not work out, and sits back, and then tries to analyze, ok we got this and it did not go the way we wanted, so where have we gone wrong or where is it that we need to reassess our approach. That is something that we would like to now discuss, which is dealing with failure both when you do the work, when you try to present the work, and may be, how other people have had experience with it.

Prof. Andrew Thangaraj: Yes. Actually, if you look at it, most research methodologies course, layout like a path by which you do your research. You do literature survey, then you identify a problem, and work on the problem, you solve it, then you publish it, you get out. At least in my experience, I have seen very few problems work that way. In case you kind of identify any area, start working, you do something, and then you invariably may not make progress, then you stumble on something else, then you try something else, then you see some other area, you listen to some talks, somebody is talking about that or maybe I try this; so it kind of goes randomly. There is colleague of mine, who put it very nicely, he said, research is a random walk. So, when you are walking in that fashion there will be failure, and of course, you should expect that; I mean, this is not something unknown to people but dealing with that becomes hard. Maybe in the initial phases you can deal with it little bit because you have not really published it, but when you try to publish the work, even there you will see things will get rejected. First time you submit your work, very high probability, it will get rejected because you are trying to convey something to somebody else, very first time you are writing something, it will happen that way, and you have to be expecting it. The idea is there are two ways to react to it, when something gets rejected or something does not work, you can get either totally dejected or you can throw it away, or you will get totally combative,say, no this guy is talking nonsense, etcetera. So, you have to hit a nice middle path; you take what is possible. And one great advantage of research, which you can use to get over your failure, in many cases, you should change your initial assumptions. You are allowed to do that, this is not a text book problem, where conditions are given and you have to get the answer right; you can keep changing the questions. So, quite often people miss that point; you change some assumptions, you change the application, you can change so many things to make even your failure become a success.

Prof. Arun K. Tangirala: That was a keyword; assumption was a keyword ringing in my mind just a few minutes ago. Every research work is based on certain assumption; there is no research work which is devoid of any assumptions, and what I tell my students is, first be aware of the assumptions that you have made. That is the frame work in which you are solving the problem.

Prof. Andrew Thangaraj: Yes.

Prof. Arun K. Tangirala: And also ask yourself whether those assumptions are too restrictive; I mean it only applies to your mind and it only that you have conceived of those assumptions or do you really find many problems falling into this frame work. So, your assumption should not be too restrictive; as long as that's not the case, as long as that's the case that your assumptions are not too restrictive and as long as your results are consistent with the assumptions, then you are absolutely fine. And I think failures definitely are bound to happen, and they can happen at different stages, like Andrew said, it could be at the publication stage or it could be just in your own discovery as Abijith said, something completely contrary to your expectation, to your intuition can happen. But remember there is nothing like success or failure in research, what is important is, what you expect and what you have discovered, and taking that into your account and coming up with the solid discovery, with the solid answer to a question. Even saying that there exists no proof for some theorem or for some result is also a great result. That should not be taken in a negative stride. Again, failures are very important, but most importantly what I would say is when you get a result, to avoid may be rejections by reviewers and so on which are bound to happen, I think you should be convinced of your result first, that's a most important thing. Many-a-times, we have come across different students, some students are not sure of the results and the advisor has to really put in some effort to convince that yes, this is right. And there are situations where the student, the PhD student, the researcher is convinced that this has to be the result, even if the advisor says no. So, that conviction of what your result has is very important and that requires a combination of your intuition, and of course, a procedure that you have followed, and of course, with validation. There are three things: intuition, and then discovery, and validation. If you have done all of these in a proper way, then you can be convinced about your result and it is all a matter of convincing the other person. But, of course, dealing with failures is important, and failures occurring towards… what you call as a failure, you should have to first define a failure, but what people call as failure and if it occurs towards the end of your degree or so-called expected duration, then yeah, it is more difficult to say and that is where I think advisors play a very critical role in cushioning, and in giving comfort, and so on, and giving advice in the right direction. In fact, sometimes with just change of idea, you can convert your failure to a success.

Prof. Abhijit P. Deshpande: Yeah, and failure can be in various ways, I mean, one example where I had with my PhD advisor was I was writing some two page summary report, and it went through iterations and the first, second, third, fourth iteration, and then we both again met and again he pointed out something, and so that is where I had a sense of failing because I was getting irritated, and that is where, he then told me that look, this is not something to get irritated about. I mean, So, there, then if you again persist with it and say ok let me learn from whatever is being the process I am going through, then seventh and eighth iteration, finally I was able to write that report. Based on what is expected of me for being a researcher. So there again a sense of failing was there, but somehow I was able to come out of it by persisting.

Prof. Prathap Haridoss: I also want to add that, you know, when we talk of you send in a paper for publication, and then it comes back rejected, and I think there are already few comments on these aspects. So one of things you should be open to, I mean, I am not suggesting that you should be over confident, but one of things you should be open to is the possibility that the expert who looked at the paper may also not be right. So, they send, they look at your paper from some perspective, their own knowledge of the field and so on, their idea of what is important and what is not important in that area, their idea of what is possible, not possible, etcetera. Lot of things they are bringing into the process when they look and evaluated your paper. So, when you get a review from a journal, which comes from, you know, few different reviewers whom the journal has approached, it is not necessary that they are always right. So, feel free to openly look at the review. When you get the review even if it says, no, we do not accept this paper, it is not up to the standards or something is not correct about it, you read it, don't just say that ok two out of three people said it is not correct, so that is means it is not correct. So you make an independent assessment of the report that you have got ok. So, you look at it, you try to assess it, and then see if that makes sense to you, whether you agree with what they have said or you feel that they have actually missed the point. In which case, you need to, you know, revisit, how you present your work; you may need to give the right kind of background so that when you send it to another journal, the reviewer does not go into the same tangent. You are preparing them appropriately so that they are directed into your line of thought. So, that is the way you need to look at it.

Prof. Abhijit P. Deshpande: Important aspect of dealing with failure also is to be able to talk about it and I think there are multiples sort of places, where such help can be there.

So, your own friends first you can talk, your own lab mates or your advisor; and so, there is a one important point, which we will discuss the later on, is also to recognize a peer community, so we will come to that little later in our talk.

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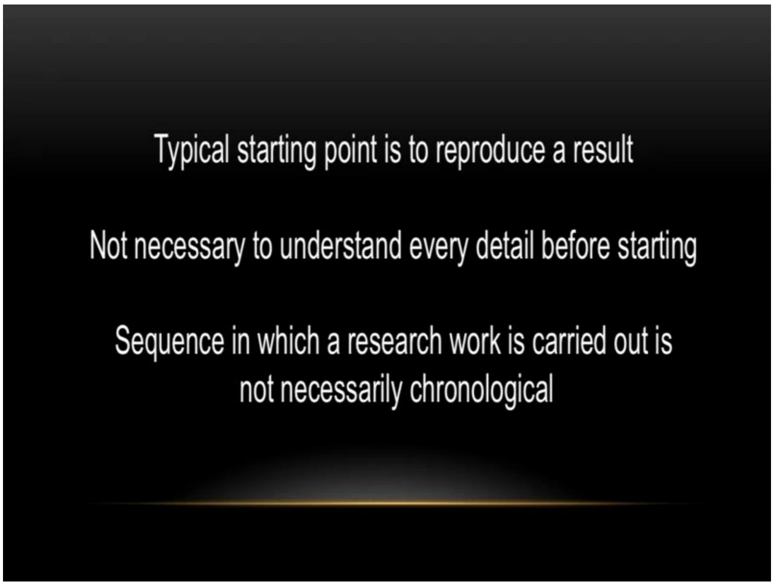


Prof. Prathap Haridoss: So, one point that I want to bring out here is again when you read research papers, and you also you know as part of this thing that you were told to, you should do some literature survey and so on. Often those papers are, in fact, not often, always those papers are presented in a very systematic and sequential manner, ok so and as a first-time researcher when you read those papers, and you think of the work that you are doing in the lab, you really feel bad because you are not doing anything in that level of sequence at which they are doing it. Now, very systematically they have done some three, four things; they have systematically identified a specific set of materials which they need to work on, and they have done only you know 25 experiments, from that they were able to get a very nice pair of graphs, from that they could you know clearly tell you that one particularly region is a maximum, they give you the answer. So, that's very nicely presented in a paper. The truth is, often in most cases, that is not how the work actually happened. So, they would have also gone through the same kind of frustration that you are currently going through, they are trying out various different samples, and many of them are, you know, completely giving them tangential results or not at all showing any kind of relevance to that particular parameter that they are trying to explore. They learn something from it, maybe they have chosen a system that doesn't work and so on, they change the system and go on. So, in the paper they publish, they do not often tell you that we looked at 45 systems out of which 39 failed, only 6 of them were interesting and those 6… they do not show you all those graphs of failed research, all of that they ignore; they just show you the 6 that work and in the order in which it logically leads to the conclusion that they have eventually reached. But behind each paper there is a lot of failure in terms of samples that didn't work, in terms of analysis that was wrong, may be experiments that were wrong, experimental setups that were wrong, all of which is not getting reported. So what you experience in the lab is not new, everybody experiences similar stuff in the lab; I mean equipment fail, experiments go wrong and so on. We just have to keep working with it; so, that is something that I want to.

Prof. Arun K. Tangirala: Having the analogy that I wanted to give is, when you go to some one's place, and you know, that person is presenting you with a new dish, I am found of analogies, and you really like that dish and it is been presented very nicely, ornated, and when you ask for the recipe, there is a list of instructions given in a very detailed and a sequential manner, and you think really that the person followed that. That has come out of experience. That cook will not tell you that there were ten dishes of this types which were burnt earlier; this is the eleventh one that is being presented; and this eleventh one is success because of the experience with those ten ones. And obviously, nobody wants to present that because sometimes it may not be relevant and sometimes probably it will put you in bad light. But most importantly for the benefit of readership and the audience always the paper is presented in a coherent manner, because you don't want the reader to go through the same torture that you have gone through in discovering this. So, I think it is a very important point that Prathap makes, and you should only read the paper to know what it is saying, but rather not to mimic the sequence in which they actually arrived at, that they used to arrive at the result.

Prof. Andrew Thangaraj: Yes.

Prof. G. Phanikumar: Which also means that when we read a research paper, we also do not read from the first line to the last line in a sequential manner. We can actually look at what is new there, and what is that I need to do to validate what that person has done, and then what is it that I can extend. So, even the reading of paper is not in the same sequence as it is written, and very often, actually, we now need to also think whether we should go through every single detail that is there in that paper to be able to conform that they are up to something good that I can build up on. So, I believe that we take some kind of a philolological approach, that is given these conditions what is it that I need to learn, pick up, so that I can go and validate my hypothesis, and then, go for the idea that I want to prove. So in other words, there is something like a black box approach, we do not need to know every single detail, but at the same time we do not need to also brush away the importance of details. So we should come to the details where is necessary, but we should be able to move forward with the assumptions.

Prof. Andrew Thangaraj: That is true. So, I think again we have been focusing on a new researcher who is beginning to do research, and one very good way to start your research, of course, is to go out and once you fix the area and area is decided, go out and see what is latest happening in that area.

Prof Arun Tangirala: Not reinvent the wheel.

Prof. Andrew Thangaraj: Go, look at recent conferences, the most recent conference what is been published, what is being talked about, and then maybe you pick a paper which you like for some reason, and the first thing to focus on, I believe is to be result oriented when you read the paper. How can I reproduce the same result that this person has, is it even possible? If your conditions are not allowing you to do it, maybe you should not focus on that, you should move to other.

Prof. Arun K. Tangirala: There is always one advantage to that; you are trying to reproduce a result.

Prof. Andrew Thangaraj: Yes.

Prof. Arun K. Tangirala: One, of course is, it helps you get familiar with all the nit bits, and the other thing is may be that researcher has made a mistake.

Prof. Andrew Thangaraj: Yes.

Prof. Arun K. Tangirala: Not deliberately of course; hopefully not, but has over looked and you probably have the chance to do that. Always remember, the creator has enough made sure that there are enough complexities in this world. That every researcher forever can actually keep discovering something new; that is what I believe in.

Prof. Andrew Thangaraj:Yes. Yes. That is true. And actually, when you are result oriented, you will also feel much better about doing your work. You are doing something; I mean not just reading and reading and reading. So, many initial researchers fall into this trap that they need to know all the basics of every single tool or technique that is being used in that paper before they can reproduce that result; that is not really needed.

Prof. Arun K. Tangirala: That is why I tell my students that, look at the baby, when all of us were babies, we really did not say that I learnt how to crawl and walk first, and then only learnt how to eat or vice versa. I was trying to learn how to pick an object, how to eat, how to crawl, how to pinch, how to scream everything at the same time and this not necessarily happening in a sequence. I think beyond a certain point, reading should happen as a parallel action rather than as necessary.

Prof. Andrew Thangaraj: It is true and when your result oriented, once you figure out how to get that result you will also know what tweaks you can make.

Prof. Arun K. Tangirala: True and also what material to read up further.

Prof. Andrew Thangaraj: Yes. Exactly. So, that does not mean that you can ignore large chunks of the paper and then hope to get anywhere, you know.

Prof. Arun K. Tangirala: Right, Right.

Prof. Andrew Thangaraj: To be able to reproduce, you should know enough about that paper, the important aspects of the paper, which help in getting that result, you should know, and you should know them very deeply; only then you can innovate and make those little tweaks.

Prof. Arun K. Tangirala: True. Actually, that clears the lot of haziness that exists in initial stages and kind of brings clarity.

Prof. Andrew Thangaraj: Yes.

Prof. Arun K. Tangirala: You start seeing, where what your direction is, what material to read up, and your learning becomes more contextual.

Prof. Andrew Thangaraj: Yes.

Prof. Arun K. Tangirala: So, you really know which text book to pick up or which research paper to read, and in fact, how to read future papers.

Prof. Andrew Thangaraj: Yes, correct.