Introduction to Research

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Lecture - 04
Group Discussion on Research Part 4 of 4

Prof. Prathap Haridoss: So, I think one of the things which comes out of the discussion is that you know, this trying to reproduce a work as the starting point, as Arun said and as Andrew also pointed out.

First of all, it makes you very familiar with the area and it also helps you built your sort of the kinds of experiments that you are trying to try out. So, and in that process one of the ideas that you have to become comfortable with is that, eventually when you are I mean further down the road your results may add up to something significant. But, on a day to day basis, most of the time your experiments or your results are going to be small steps, each individual step will add to something.

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So, just making a small experimental setup may take you like 2-3 weeks. To get an

experimental setup, that is working correctly, where you have been able you know when we said you know reproduce some original, some already published data. That experimental setup is able to actually consistently reproduce some data. So, that takes some time. So, if you have managed to you know, taken, if you used about 2-3 weeks to setup that experimental process that is not a waste of time and you should feel happy, that you should feel happy that you have actually progressed from having nothing you had a clean state, you had a clean board ahead of you and then, now you have a setup that consistently gives you data that is believable, defendable and believable.

Now, you can try some new sample. So, this idea that there are small steps that you have to work on is something that you have to get used to and for those small steps to occur you really need to be a regular researcher. So, you have to put in those hours in the lab. It is not something that you can you know, suddenly one fine day everything will be in the lab and your first experimental work, it does not work that way. You have to keep working on it slowly and steadily and therefore, those regular hours in the lab really make a difference. I would also like to add that you know, sometimes there is this misconception that only if you are working late night hours that you are actually doing great work. It is not like that, if you even if you work very regularly from 8 in the morning to 5 in the evening and then take your evening off and be free and comfortable, you could still we doing great work. You could still be doing research work, you may be thinking about your research later in the evening without necessarily running the experiment at that point and time, but still that would be a lot of good work. So, just keeping up those regular hours, helps you add those small, small steps which then add up to something that is significant.

Prof. Abhijit P. Deshpande: All of this, actually the notebook that Andrew mentioned earlier is a central aspect. So, noting down let say, you suddenly while you are looking at some equations and you notice some dependence. So, it depends on the cube root, note it down, observation. So, it is a simple small fact, but it is an observation that should come into your note book or let say after 1 week of things, you plot it something, something, plot it graphic, put it in your notebook, that is a simple result that you got.

Prof. Arun K. Tangirala: It is lot easier to do this electronically also.

Prof. Abhijit P. Deshpande: Yes, electronically. So, all that should go into your notebook.

So, these are all these simple observations, results which then eventually will be helpful to build later on.

Prof. Andrew Thangaraj: Yes. So, one for the point you want make about all these when you get a small result, you have to be first of all excited by it, you have to believe in it and you have to be confident about it and you have to proudly talk about it to others; your students, your advisor, everybody and you have to show that you know, simply it might look like, why should I be so proud about something so small? I am doing something so small, other people are doing great. Yes, they might be doing something great in your opinion, but you have to be also proud of the small thing you are doing.

The reason is, there is lots of reasons for it because when you are proud of that you keep thinking about it a lot, you keep going back and back about thinking about every single way in which it can be extended and all that builds up on this small result of yours and you get a bigger result. So, I mean Prathap was mentioning, it is experiments a lot and it does not necessarily apply only for experimental work, even for theoretical work all these things are true. The final big result that you derive, a theorem that you derive, does not come just like that. You know, you might show it for one special case one day and then one more special case but, if you don't believe in it you are not excited by it, you do not have confidence in it, you are not going to keep exhausting all the cases and build the general idea that gives you the big result. So, it is very important.

Prof. Arun K. Tangirala: I just wanted to add, the continuation of what Andrew just said. Somehow there is a misconception that experiments have to be performed carefully and simulations don't have to be. The same rules, when we teach courses on design of experiments we make it very clear, that all the rules that you learn or the theory that you learn in design of experiments equally applies to simulations. The only difference in simulation is you can play around a lot more. But, research is not just about playing around it, its playing around with a purpose in mind. So, even experiments are done with a purpose, simulations are done with a purpose. So, you have to choose your simulation settings. You have to know, how your simulator works? What kinds of solvers are being used? And what is the engine that's running underneath and so on?

To make sure that, you have chosen the right settings and tool that you are using is being used for the purpose because, simulator is a very deadly weapon and you can actually

end up producing so much data and only a small fraction of it is going to be useful to you and you may be lost in the deep of data. So, please remember that the rules that apply, the theory that applies to experiments or equally applies to simulation as well and like Andrew said, even in to theoretical work.

Prof. G. Phanikumar: And then, there is a small point about the discipline also. When we see one small result that is promising and we are proud of it. To grow into a big topic, worth making a publication or a presentation and may be becoming a whole thesis by itself, that process requires lot of discipline. So, putting in those hours every day and the being very meticulous about the day to day work and then subjecting every extension to the logical process is very important and, if you go around the labs where research is being done, whether it is universities or national labs or a research divisions of companies, you actually do not see too many people in dirty clothes, long hair, incompetent, working in wee hours, actually you see normal people, well dressed and coming to their work for a significant number of hours every day.

Maybe more than an average employer because you see that people do put in more number of hours of work in research than outside, that is mainly because of the passion. But, you need to put on a regular basis and in a disciplined manner, so that you can grow your small idea to a big one.

Prof. Andrew Thangaraj: But Phani, there are some badly dressed researchers also.

Prof. Arun K. Tangirala: And having said that, I think I just want to re-iterate what Prathap just said that. See, in research your thinking does not end with the time you close your lab or your computer or your notebook and so on. The thinking process is a 24 hour process; it even appears in your dreams. I do not know if you know Kekule discovered the Benzene structure in his dream, right. Which means, he was really thinking into it, may be thinking about it while he was eating, in his shower, where he was walking, running, everywhere so I think research that thinking process, the thought process that goes into it, is a never ending one. You cannot close that lab of the mind that JC Bose mentioned, that lab cannot be closed. All the physical labs you may close, but not the mental lab that you have. So, make sure that you are thinking about the problem and you will hit the treasure very soon.

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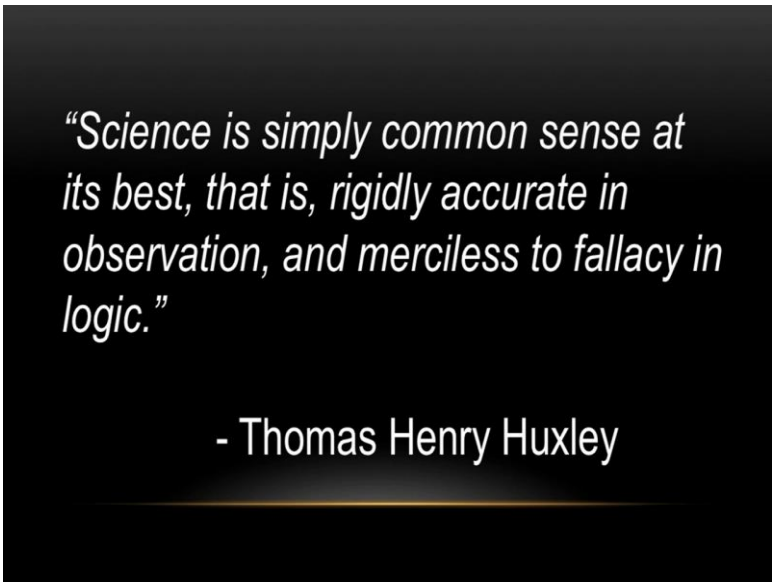

Prof. Prathap Haridoss: So, another quote in this context of whatever we have been discussing so far, is attributed to Thomas Huxley and it simply says "Science is simply common sense at it is best, that is, rigidly accurate in observation, and merciless to fallacy in logic." So, I think that sums up some of the ideas that we have been try to convey so far. We will now look at, how do you as you start collecting all those small results and you know, building up your depository of results.

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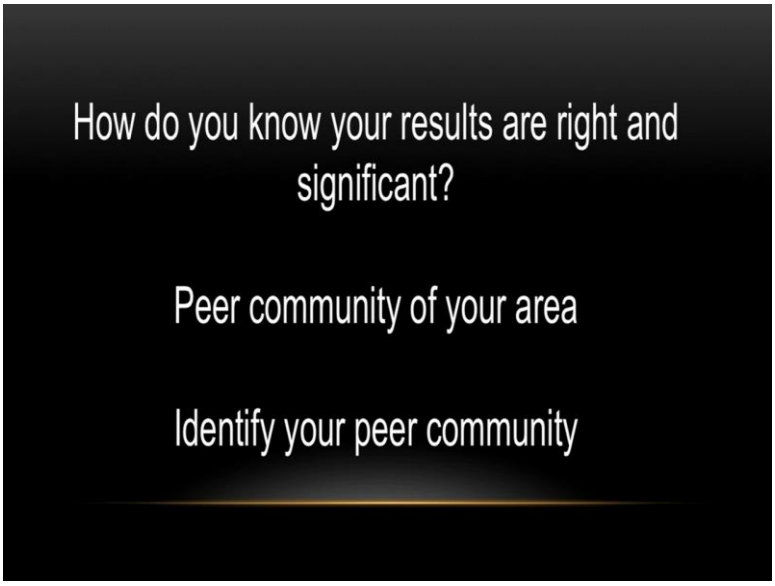

How do you know that your results are right? How do you know they are significant?

And what is the process involved in this? So, maybe Andrew can start with that.

Prof. G. Phanikumar: Yes, I want to say something.

Prof. Andrew Thangaraj: Yes, yes.

Prof. G. Phanikumar: So, basically I want to say that, the research enterprise that we have been having so far several hundreds of years is not a democratic process. That is, if everybody agrees to it, it is it. No, it is not like that. The one expert can prove everybody before him was wrong and then his or her idea can then prevail forever after that. So, it is not a democratic process. It is also not a process where, somebody highly recognized is able to tell and then everybody agrees. So, it is a process where actually the peers are willing to review each other and then hold something as true so long that it does not been disproved. So, that peer part is very important and if you notice the way we go about disseminating the research results, it is basically by peer review and then peers criticizing and validating each of this work and going about. So, identifying what is your peer community is very important and I think professional societies play a big role in that aspect and you have something to say Andrew.

Prof. Andrew Thangaraj: Yes, I think that is important quite a few, we all work like Phani was mentioning it, it is a global area of work today. I mean you cannot say my work applies only for India or some in a local way it does not work, research is global today, your competitors are all over the globe definitely in China, Japan, USA, Europe, everywhere and all of those places are connected together by the peer community and peer community is international and there is also a significant national peer community and you have to identify the peer community. For instance, in Electrical Engineering many of these areas IEEE is a big peer group and the technical societies of the IEEE cover almost all the areas of Engineering and I am sure every area has a similar things that are local chapters, etcetera.

The important thing to do is to identify the journals that are run by these societies of reputed peer communities and then identify conferences that are run by them. There is lot of things there, they will give something called Technical course sponsorship etcetera, etcetera those are different from the conferences that the society itself runs. So, you have to identify those which are critical and then try to see, how high you can go and there are different levels there I mean it is there a different gradations for conference, at different

gradation for the journals, even when it is run by the same society and it is ok, to identify the best you can do and then be wherever you are. But, identify a solid reputed research to peer communities both, nationally and internationally and try to publish your results there and that is the number one way of figuring out how relevant your work is, how good you are.

Prof. Arun K. Tangirala: So, I just wanted to add. Going back to the question of, how do you know your results are right and significant? Now, as I think I have made this remark earlier as well, that you need to have an intuition. Sometimes you do not, sometimes you do. But, you do develop the intuition with experience and somewhere you know, that this result make sense or does not make sense which means, that when you get a result it is important to validate it qualitatively and quantitatively, that is the first important step. When, if you want to say the result is right, the conviction should begin from you, you should not depend on someone to say it is right right and then sharing with your, discussing with your advisor that is a second step because, there is a discussion and may be you are right and you can convince or maybe you are wrong then sharing it with your own research group.

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So, run your research through you know, make a small group presentation. Even if your advisor does not insist on it, you say that I would like to discuss this results in the open with my group mates because and hope that you will get a critical feedback and very

often you will get a critical feedback. Simple discussion like they say, when you are upset, when you are sad, simply pouring out your grief, listening here will really help you. Likewise, when you are excited about your results also, sharing it with your group members really helps you because a lot of perceptives come in.

I think, the key word in research is perceptive and sometimes insignificant results can become very significant. Just with a change of perceptive and that is a very important part and then comes sharing it, communicating it to the scientific community, through conferences, through papers and so on and that is why the peer community plays a very important role. But, the starting process is with you, you are the owner of the problem, you are the owner of the result, you take complete ownership of the credibility of the result, right and that is, for that to happen you should have done enough validation either theoretically, experimentally, through simulations, whatever. You should have a second way of showing that your result makes sense.

Prof. G. Phanikumar: There is one point that we want to make about learning. Do we go into every specific detail of every tool and pick up all the things before we embark on research.

Prof. Arun K. Tangirala: For research.

Prof. G. Phanikumar: So, in research I think there is a combination of multiple techniques of learning.

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So, we normally talk about, what is called Just in time learning, which is applicable to for example, when you are making a dish out of a recipe you know you need to only know about the detail which is necessary to make the dish. You are not going to the theory of what can mix with what, for example;

Prof. Arun K. Tangirala: And you do not want the science of material.

Prof. G. Phanikumar: Do not want to go to the science of material. Then, there is also another learning paradigm called Strategic learning. Where, you basically have a goal and up to achieving that goal only you are interested, you are not interested in anything else and then, there is another paradigm called In-depth learning, where you are really interested to know go to the bottom of the knowledge and then get that knowledge to your long-term memory. I think research encompasses all these three in a way. For example, let us say you want quickly make a plot in a way that conveys the idea, you want to pick up a mat lab, the script for that. So, you just learn to that extent, you do not need to go into see, how mat lab actually is doing all these things?

And then, Strategic learning you need to basically solve a problem. You understand only to the extent that is necessary to solve but then, when it comes to your own core area of research, I think it is very important to achieve in depth learning. So, it is very important to be little agile in the way you learn about in research, the way you have been doing in this high school or when you are preparing for this entrance exams, doesn't apply to

research. So, your attitude of learning has to change the moment you want to start on research.

Prof. Arun K. Tangirala: And all three have different time scales, obviously.

Prof. G. Phanikumar: All of these are at different time scales.

Prof. Arun K. Tangirala: Different time scales and PhD typically involves all these three and while converges to in-depth. Starts with Just in time and ends up with In-depth.

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Prof. Prathap Haridoss: Right. So, in research one of the aspects, we have not touched upon so far is Teamwork. You actually consciously or unconsciously participate in Teamwork. Rarely have we, I mean it is not that it is impossible but, in general we don't have people who are completely isolated and then doing research on their own. There are some, but most in most of cases we are working with the group. You are typically student in a group or a Post Doc in a group and so, there are people with a wide range of different experience who are working along with you some you are learning from, some you are teaching and sometimes the roles are reversed and so that is a process that you have to get comfortable with and you have to understand.

So, it is always going to be Teamwork and you have to be you know willing and happy participant in it. You will invariably find groups and you will invariably find the occasional researcher who does not like to participate in Teamwork. I think in the long

run that's not a great idea, maybe in the short run you do see 1 or 2 of them being successful, they try to hide the resources, they do not share the resources and so on. But, in the long run that is not a good idea. World does get along, we spoke about peer community and so on. It is a same peer community, that they will see that you will see and just a matter of time before people accept that you know, this is not a person that is easy to work with. So, Teamwork is very important. You should know how to work with the people around you and you should know how to share credit. So, when you write publications you should know whose work was this? Was the core of that particular publication and therefore, that person should get priority and so that something that you should become comfortable with and just to share a quote here it is from Issac Newton.

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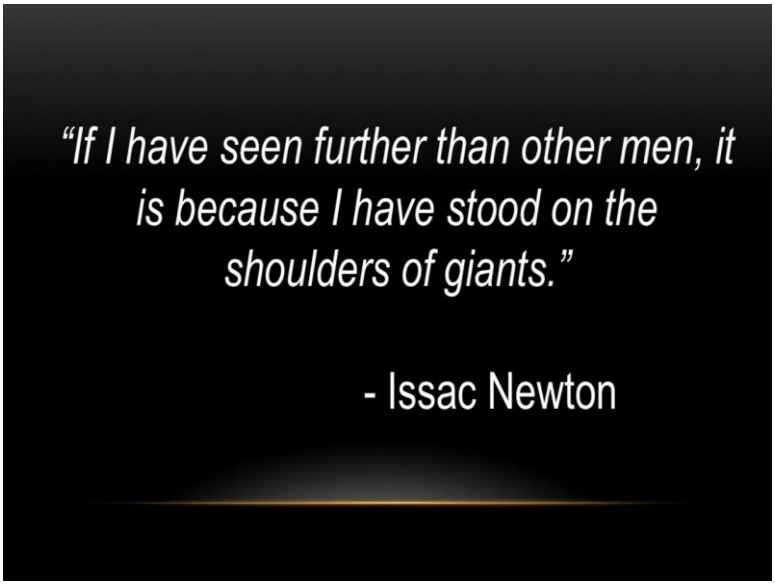

And is something I am sure many of you heard of it says "If I have seen further than other men, it is because I have stood on the shoulder of giants." And, I think that very beautifully captures this idea of teamwork. In fact, he is talking of teamwork in a grander scale, where he is talking of you know not just his group but, you know other people who worked before him, in various parts of the world. Who have contributed to that area and he has understood those contributions and built on those contributions. So, that's teamwork in a grander scale, but there is team work even it at a local scale.

Prof. Abhijit P. Deshpande: Even much local scale for example, you may be using the same computer, you may be using the same balance to weigh things and so there is

teamwork involved in everything because it is coordination, inter working with each other. So, all of these are very important aspects of.

Prof. Arun K. Tangirala: You know, interacting with team really gives you good breadth of the research. Which is also important in a research not just your own depth getting lost and so on. Just not going, single one sighted.

Prof. Andrew Thangaraj: So, having a active group for instance, going into an active group which is publishing in conferences regularly, publishing in journals regularly.

Prof. Arun K. Tangirala: It is positively contiguous.

Prof. Andrew Thangaraj: Yes. It is huge. Just gives you the setting in the atmosphere and completely changes your attitude to everything and you will be surprised how that can make a big difference to starting out in research, going in to a active groupers, very crucial and that it is you might say it is, some of them you might work with and they might be part of a team, some of them you may not even work with. That they are talking about things and they are exciting you and you are motivated by them. It works in a great fashion.

Prof. Prathap Haridoss: So, yes. We are sort of a getting closer to the conclusion of this discussion, just a couple more points, before we look at some general aspects about you know, Human characteristics, Research characteristics and then we will close this discussion.

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So, one of the points that you should get accustom to is that, you have to be the most critical examiner of your work. So, while you know, you send your work out and you get you know, peer review and you are likely to most likely to get some kind of critical comment. You should be the one who is most ready to critically look at your work and to examine whether you really have done something to the level of detail that your discussion is trying to convey and so on and that requires lot of honesty with yourself with the your immediate group.

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Because, when you convey these things in you know, very blunt and clear manner to your immediate co-workers, you also get feedback which is very, very useful. So, that you have actually covered much of the ground that you need to cover when you send a publication out, so that when a peer who is not working with you, looks at your paper. He or she also understands that you know, you have really covered a lot of a detail, you have looked at all possibilities and therefore, your result is much more believable. So, I think honesty is very fundamental in research because ultimately you are trying to discover something. If you are not being honest about it, there is no meaning in saying you discovered something in it. So, that's it.

Prof. Arun K. Tangirala: In that context, I just want to say that there is nothing like a final result and that's it. Research in that field is over, right. People typically use mature and so on and infancy the researcher infancy and so on. So, what is more important is when you critically evaluate your results, to ask first of all if the assumptions that you have made are not too restricted as I remarked earlier. And secondly, whether the result qualitatively makes sense and most and thirdly whether people are able to reproduce what you have done. In fact, there is a theory of reproducible research in which the researchers are encouraged, if they are doing computational work. These days, they are encouraged to a publish quotes. So, that they can be reproduced or you know if there is an experimental data out of which you have drawn some inferences, post that data, if it is not confidential and so on.

So, you should ask yourself if this result is a onetime affair or you can actually repeatedly do it and in many times, on many occasions when you are performing experiments there is a repeatability issue associated with it, you should have done enough repeatability analysis. So, the critical part is that where you get your result, look at the credibility of the result and ask if it is, if it make sense, if it is reproducible and then of course, talk to your advisor to find out if it is significant.

Prof. Prathap Haridoss: So, I think in this discussion we started off with certain philosophical aspect about, what is Research? Then, we have through the past half hour or so looked at lot of you know mundane day to day aspects of research, which is what you will actually experience on the ground, when you are researcher in a group and right now, we will again step back and look at a few more philosophical aspects which will help us put a larger perspective on the whole discussion and the process and with that we

will close. So, may be Abhijith can.

Prof. Abhijit P. Deshpande: Yeah, I mean we started out by posing the question, right. That, what PhD and M Phil? And, it is we want to get these degrees, we want to make good livings. So, that is really the underlying feature in some sense. But, all throughout our discussion you also heard that you being the centre of all of this. So, researcher sometimes wants to step back and try to look at you know, what does a researcher do? And how can how is it? Sort of all this hard work and all these thought and all these labs, lab mind laboratory being mind. So, how do we sort of place ourselves? How does a researcher place ourselves and so in all normal things we are just humans? So, just like humans are curious, all the humans want to actually improve things around us and we of course, all have very appreciation for beautiful things. So, similarly for motivation for research you know, we many times want to justify our work that we are doing in terms of we are trying to do something which because nobody knows about it. So, therefore, I want to know about it. Therefore, I am a researcher.

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Some other times we may step back and say that no, some of the thing that I am trying to do is actually to improve, may be make less effort for some manual work, may be to take something in 2 seconds, what used to take longer time? Of course, these days with the world around us also we are equally, not just humans that we are concerned about it is the earth as a whole. So, therefore, betterment of society and world around us, is also an

important aspect of why we do research and of course, one very important aspect in all of this is also we find, I mean we have talked about joy, we have talked about all these emotions, we have talked about being passionate. So, therefore, there is a duty and essay text in science. There is something when we look at a nice equation, we feel very nice about it. When we see a very elegant experiment we say, oh wow! How could it be shown so elegantly? So, therefore, there is for being a researcher all of these could it be, could be also involved.

Prof. Arun K. Tangirala: So, that is an art in researches

Prof. Abhijit P. Deshpande: Yes.

Prof. Arun K. Tangirala: We have talked about the science part of it.

Prof. Abhijit P. Deshpande: Yes, yes.

Prof. Arun K. Tangirala: But, I think research is both science as well as an art and of course, you know the degree of art varies from a research problem to the other. But, I think it is a very important to ask ourselves and be honestly with the purpose of carrying out research. I think that's it.

Prof. Andrew Thangaraj: Various, there are various ways of looking at it. I think one other way of asking this question is we spoke a quite a bit about, how to do a research? And there is also why aspect and Why, we said in terms of passion extra but, where does the passion come from. So, ultimately the why is answered individually by different people and I am sure you will have your own answer in you when it comes to it.

Prof. Arun K. Tangirala: Yes, yes.

Prof. Andrew Thangaraj: But, you have to answer that question. Otherwise, you do not survive for a long time in research, if you don't know or don't appreciate the why inside a few very clearly and that is actually a life philosophy. You should know why you are doing something.

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Prof. G. Phanikumar: So, here I want to relate to what Daniel Pink had summarized from research on motivation. Why do people work? And why people are driven to work? This is something that he has done very nicely in his book, Title Drive and there he brings three aspects.

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One is Mastery, Purpose and Autonomy. So, very interestingly all these three very nicely relate to research you know, Mastery for example, is where you see a sense of getting better and better as you go along. Researchers are like that you know, as you keep on

doing research over a period of time you are very good at the equipment, the techniques, the methodologies, etcetera. So, you can see that you are gaining Mastery. Purpose, we have already discussed, Abhijith has said that. So, we see a larger purpose for the work that we are doing and then Autonomy, researchers want to solve their problems and the problems for the betterment of people around them in their own way, by choosing the techniques that they wish to use, by choosing the specific methodologies that they wish to adopt, etcetera. So, there is lot of autonomy by which people work. So, naturally if you put all these things together, researchers are intrinsically motivated.

Prof. Prathap Haridoss: So, I think we have now reached the conclusion of this discussion. We sort of started with a quote from Albert Einstein and I think we will it is kind of appropriate to close with the quote from him since, many of us recognized him to be a very significant contributor to the scientific community over the years and it simply says this, "Imagination is more important than knowledge" and that's very philosophical statement.

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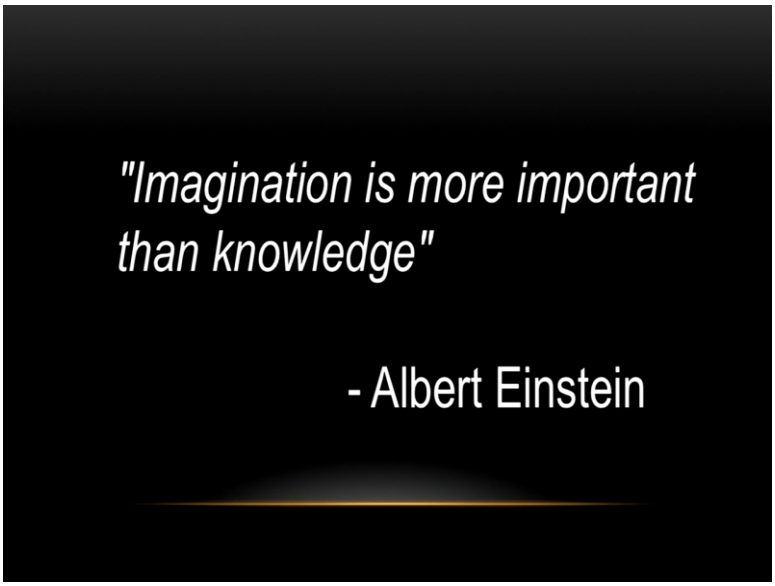

So, you need to think about it and interpret it in a manner that make sense to you and then possibly utilize it in the manner that you feel is appropriate. So, I think with that we will close. I hope you had an overview of what research is and I don't know if you add some closing comments.

Prof. Arun K. Tangirala: I think, what is important is this, if you look at if someone is

looking for a work flow for a research, the disappointing answer is there is no sequential work flow. However, there are at each stage there is a systematic way of doing it. Whatever you are doing, you have to do it in a systematic way and learn to and also, make it a habit to analyze what you get out of and then it is a lot of a trial and error. There is a lot of iterative process, but at every stage there is a feedback you take it and then you find out where the problem is and fix it there and come back and so on. So, there is lot of back and forth. However, at each stage you have to do things systematically and analyze the results that come out of it. But, if you look at the grand scale of things yeah, I know people go back and forth and research and every researcher goes through that, not necessarily that the researchers shares those things.

Prof. G. Phanikumar: So, I would summarize saying that, if you want to do research be at it.