=Introduction to Research
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Lec-05

Introduction to Research, this is the outline of my talk. I am going to say a few words about the Idea of a University.

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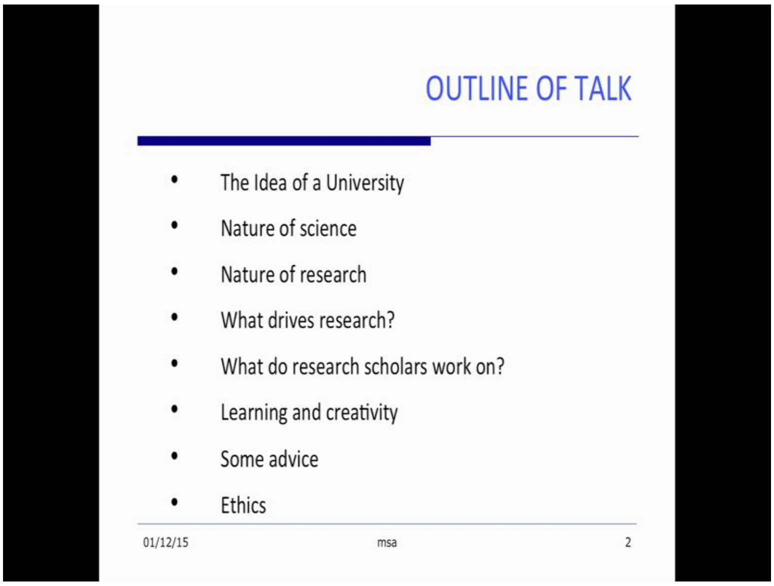

About the nature of science, nature of research, just briefly what drives research, then what do research scholars work on. This is a summary from when I was doing research here, I did a summary of what people work on try to classify the problems, it comes under 6 or 7 classifications. And then I am going to talk about a little bit about learning and creativity, because while course work is mostly about logic; research is a lot about intuition. While you use logic, you make your leads only through intuition. And we have some idea understanding of it from the work of Sperry and coworkers, it is called the Split brain experiments. Sperry got his noble price in 1981 in neuro research. Then I will give some advice that I cannot resist for a research scholars and (Refer Time: 01:06) that's also based on my experience here with students and a last line on ethics.

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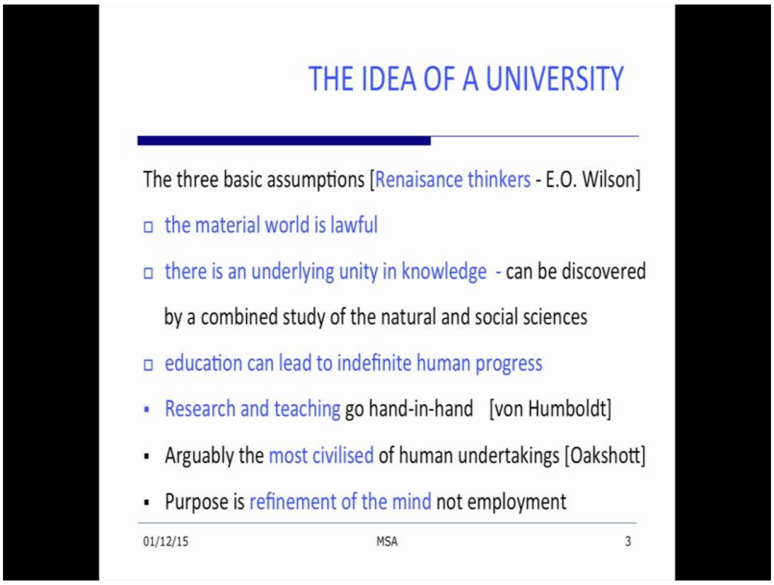

Basically, the university itself is conceded somehow we forget to mention this, and I think we ourselves forget it. University is based on, it is a Renaissance concept; essentially the Current University. In the Renaissance thinkers made three assumptions. The first assumption is that the material world is lawful. That is nonliving world is governed by loss; that's what it means. The second is that there is an underlying unity in knowledge and this can be discovered only by a combined study of the natural and social sciences.

We have divorced the two essentially natural sciences includes engineering, social sciences includes humanities. We think they don't have an effect; I will just give one example for example, when in my own area in molecular thermodynamic, Gibbs made the assumption that all the microscopic states of an isolated system are equally probable. This idea, a priori that everything should be equal unless there is a good reason for it not to be, came from the then social ideas of Kald Marx. If you had been at any other time, the Gibbs may have made some other assumption, but we would have finally come to this assumption because this is what leads to results that compare well with the experiment. But the point is that the ideas in society influence your thinking in terms of science also.

Then the third is that education can lead to indefinite human progress. These are assumptions these are the postulates. Then it was Von Humboldt in the 19th century who said research and teaching have to go hand in hand. He said research brings passion to teaching and teaching rejuvenates the researcher. So both are important. And the ever since then all the research universities have been based on this four premises. There is a nice article by Oakshott, Oakshott was used to be a professor of political science in Oxford and it is called the idea of a university. In fact, you can Google it and find the article.

He says it is arguably the most civilized of human undertakings. The idea is simply of course that education passes on accumulated knowledge to the next generation. And no other animal does that, all other animals have to only learn by copying, so by watching and copying. In fact, that's probably the only the reason we are on top of the food chain. Then but the purpose of education is refinement of mind not employment; I think that is important to realize many people come to education thinking this will lead to employment afterwards; it does but that is a corollary. A refined mind will more opportunities and therefore find, I mean more jobs available for it. But what has happened is this has been twisted out of sing, because employment is certainly important for individuals. So, everybody thinks will I get a better job after this? And therefore, they come for it that is not the purpose of education at all.

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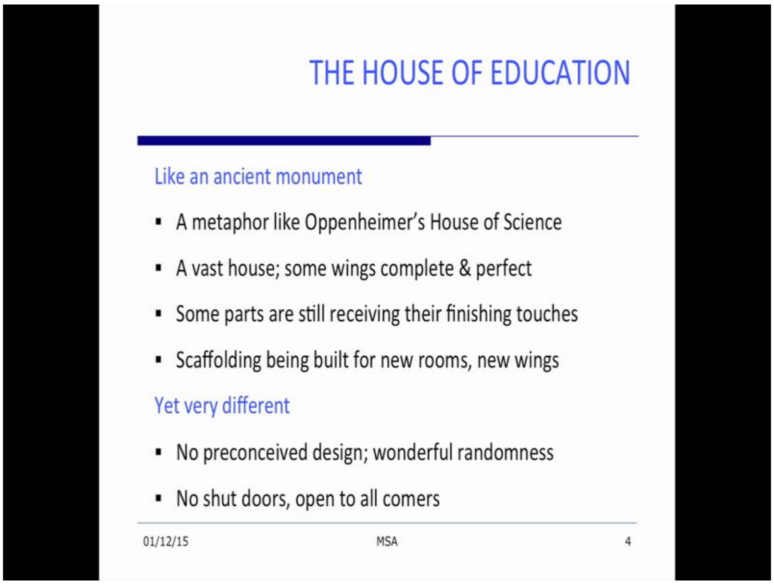

In fact, the universities are now burdened with not only educating students but also placing them. Which should never have been the case. Anyway, I would like to draw on a metaphor, like this metaphor this is Oppenheimer has a book called Science and the Common understanding. It's a beautiful book! should read it. He draws the metaphor he calls it the House of Science; I have extended it to the house of education. It is like an ancient monument, it is a vast house, it is in some wings have complete and perfect and some parts are still receiving finishing touches. And there are also parts for which the scaffolding is being built. And there are new wings being created. But it is also very different from a monument.

First it is not done to your preconceived design and it has a wonderful randomness suggestive of unending growth. And if it has no shut doors it is open to all comers. Of course, this seems a little ironical conceiving the way people struggled to get it into institution, good institutions in India. But that is an artificial result of our not having enough seats for the number of students who seek education. Basically, it is an open house; it is open to all comers. I mean if you want a degree of course, you have to register and so on and there are limitations.

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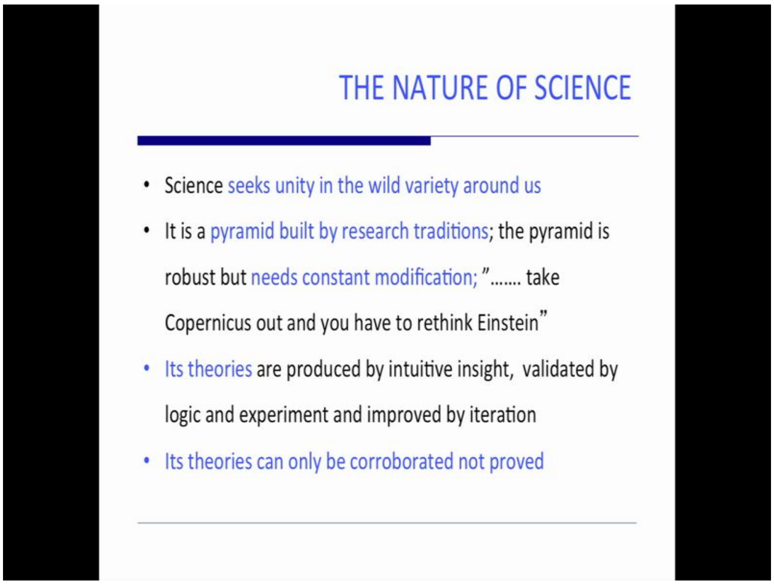

Let me say a few words about science, nature of science. Science seeks unity in the wild variety around us, when I say science it include engineering. And I contrast this in particular with the industry, which thrives on differences. If you go to the industry, you have to first tell everybody what your product is and how it's different from everybody else's, otherwise you cannot sell your product. Whereas, the university seeks unity, it asks what is the minimum number of loss on the basis of which I can describe the entire diversity around us. And you will never seek unity after you leave the university you have to come back to the university to seek unity. It is basically a pyramid built by research traditions; the pyramid is robust, but needs constant modification. Then a saying is that take Copernicus out and you have to re think Einstein. You have to rethink, it does'nt mean you have to completely change everything; normally it's a fairly robust structure, but at the same time, there will be minor changes that occur.

For example, when Einstein introduced the limitations of lights movement, and Newton's idea of instantaneous reaction to a force was lost, it did'nt make any difference to the macroscopic world; it only made a difference when you started talking about speeds comparable to light. But on the other hand, you did have to make that modification. The theories of science are produced by intuitive insight and validated by logic and experiment and improved by iteration. Please note that no theory can be proved; it can only be corroborated; you say that this is the theory current theory and as results come in you may have to modify your thinking.

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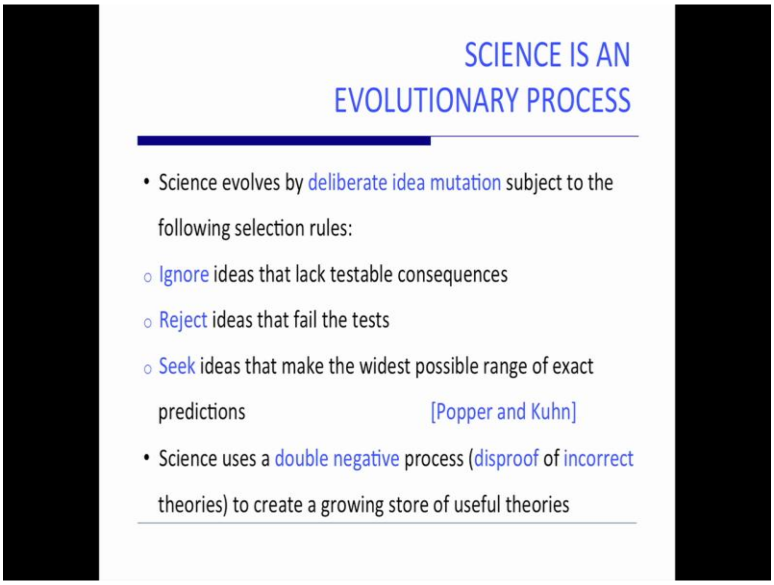

You should read the other book you should read is Popper and Kuhn, they both write, I mean it's not a book they were articles by both of these people. They have also written books, but it is primarily about science as an evolutionary process. What they say is science evolves by deliberate idea mutation, subject to the following selection rules. First, it ignores all ideas that lack testable consequences. You must realize that eliminates about two-thirds of life; because there are lot of things in life do not have testable consequences. In fact, in all of science almost all of science and engineering we deal primarily with materials that don't have a memory and therefore, you are able to deal with them the way we deal with them. If they have a memory then it's very difficult especially if they have a long memory, it's very difficult to have testable consequences for such substances.

Secondly, you reject ideas that fail the tests. If you compare with experiment does'nt agree then the idea has to be rejected. Thirdly you seek ideas that make the widest possible range of predictions. If you can predict two things with one idea and three things with the second idea, you keep the second idea and throw out the first. Because you are seeking unity, you want to know what is the minimum number of basic loss on the basis of which you can explain everything. So, science uses a double negative process. That is, it disproves incorrect theories, it uses double negative process to create a growing store of useful theories.

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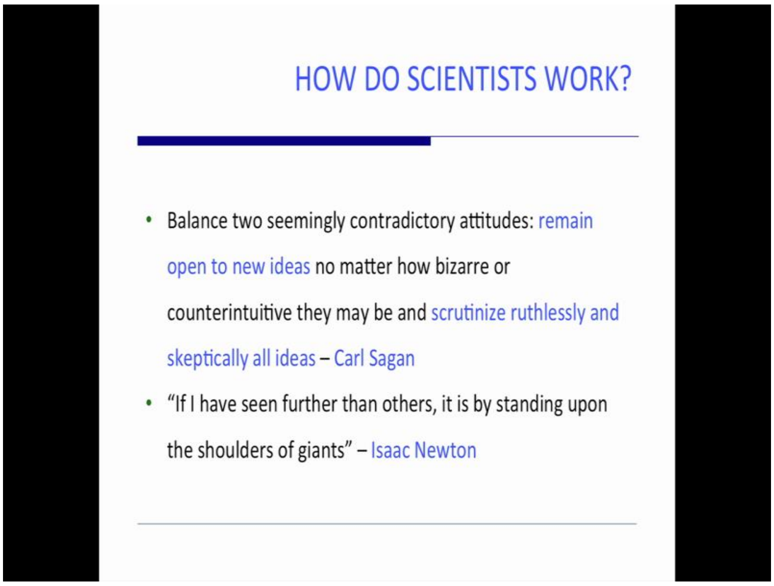

So, how do scientists work? Basically, I have got two quotations that tell you quite graphically. First you balance two seemingly contradictory attitudes; this is from Carl Sagan. Carl Sagan was an astrophysicist who taught in Cornell, he passed away about 10 years ago. And he said the one attitude is to remain open to new ideas, no matter how bizarre or counter intuitive they may be. And the other attitude is, scrutinize ruthlessly and skeptically all ideas that come to you. So, this apparently conflicting ideas are what you have to work with. And Newton said if I have seen further than others it is by standing on the shoulders of giants, it is a very famous quote. What this tells you is please do your literature search very well, because you should know what others have done before you are start doing your work.

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Let me say a few words about the nature of research. Research is basically a creative process; it's complex and it's iterative. It is the search for truth, whether the truth is mundane or profound. It's about going up blind alleys to see if they are really blind. Sometimes you try out a method after four years, you discover it is not a useful method it is still publishable you will still get your PhD, it does'nt mean, you would not get your PhD. You are seeing does not have to of course, normally for a negative result, you should have done experiments too. A pure theory that gives a negative result usually doesn't lead to a PhD. So, you have to be careful, if you are working on pure theory you have to be little more sure of your ground.

Then there are two kinds of research in general the university; one is academic, the other is developmental. Academic is basically curiosity driven, but you must remember even here 99 percent is routine, only one percent is inspired. It is routine because people have said things before you keep verifying those things, you keep testing them out and so on. And one percent inspired is of course the great people if you have seen in science and in engineering, various fields. The developmental research is application driven; it's a team effort it is needs leadership and considerable funding.

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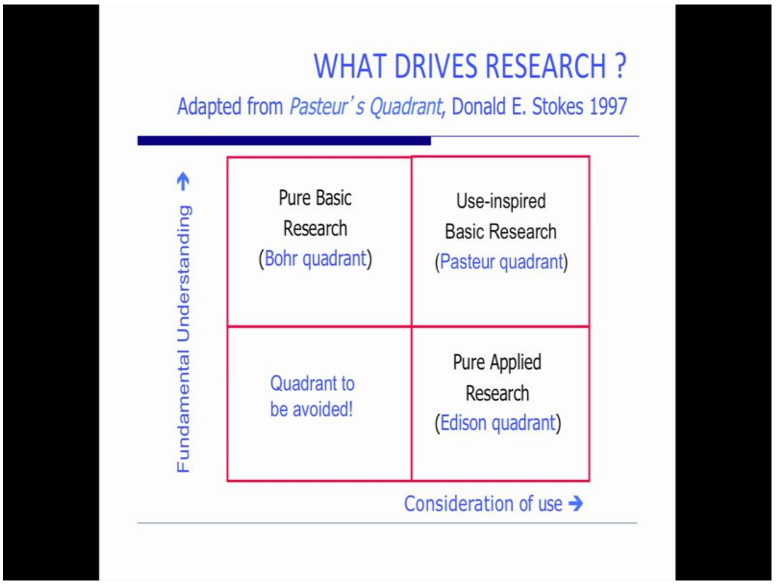

So, what drives research? This is from Donald Stokes book, but it is basically a well-known diagram. On the one hand, what drives research is fundamental understanding, you are trying to understand things in a fundamental way. The other is the consideration of use. So, you divide the areas space into four quadrants and there is a quadrant to be avoided. The Pure basic research quadrant is called the Bohr quadrant, Use-inspired basic research is called the Pasteur quadrant, Pure applied research is called the Edison quadrant. Incidentally, this is a famous quotation of Edison that said, I mean they asked him I believe he succeeded in making the light bulb only out in his 10,000th trial. So, they asked him how, they went and said you failed 9,999 times. He said no, I didn't fail; I found out how not to make a bulb in 9,999 ways, so that's also a contribution to research. So, these are what you have to do. This quadrant is to be avoided, I will say a few more about few words about that later.

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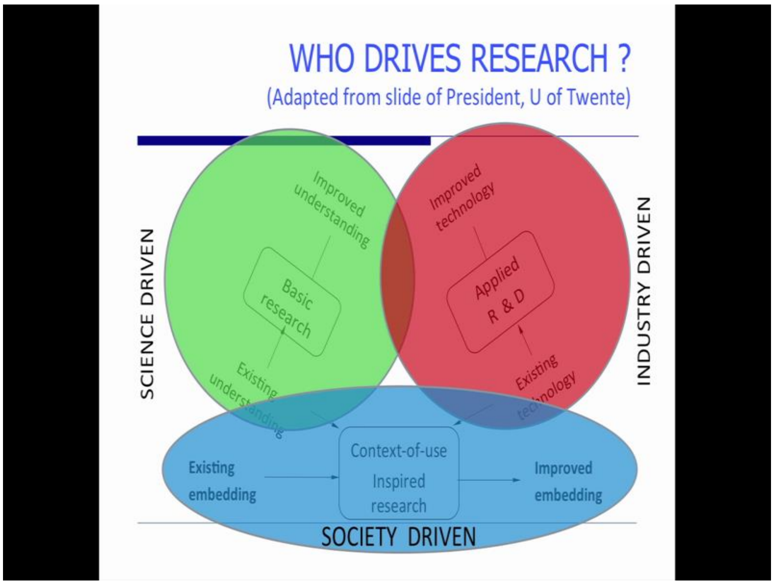

This is a slide that I borrowed and adapted from President of the University of Twente; we met in a meeting in Korea, it was about research in what how university should pursue it. And the idea is the following, primarily you have basic research which has takes existing understanding to an improved understanding, then you have existing technology which goes to improved technology. So, we have industry driven science and research.

The reason I put this in is, because I think we should realize increasingly what's happening is we are not able the control the applications of science and technology. So, you have some results that come out, turns out that you have made an improvement to understanding, you have made an improvement to technology. But there is a side effect in typically for example, it may be an environmental side effect of a technology. And then there are lots of objections to it and many scientists and technologies have complained that these people who object to it are eco-terrorists. But actually they are not, the point is they didn't know about this technology till you told them about it. So, the idea is increasingly in universities that you should have a society driven research, where you should inform people about what you are working on, where it will go, so that social scientists are also involved right at the beginning and they warn you that this might lead to consequences.

So, the idea is if you take them in the team right at the beginning then you are likely to produce technologies that will be actually useful. Now a lot of your technologies may not be useful. So, there is always a risk of your running into problems, because you did not take these considerations into account. So, it's called context of use inspired research; it is you must know, where it is going to be used, and you must discuss it with people in humanities and social sciences. Sociologist will tell you the society will not take kindly to this or they may say with the existing structure this technology can lead to problems and so on.

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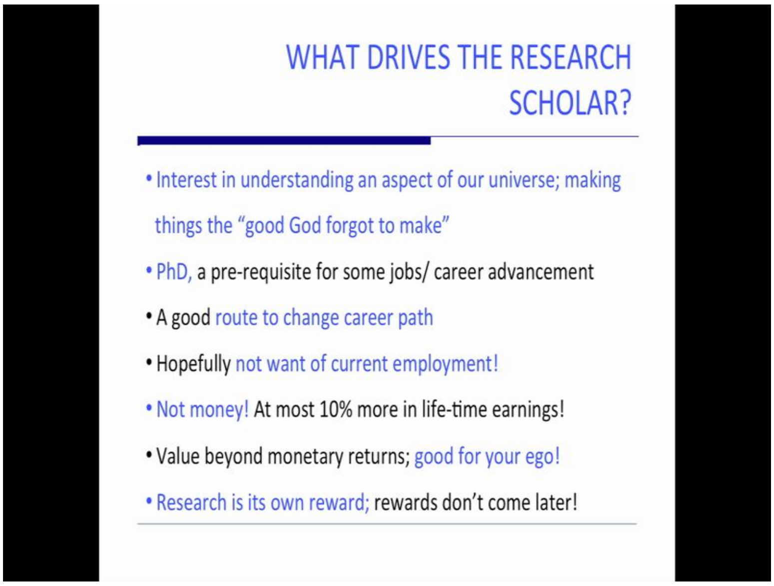

What drives the research scholar? I am hoping it is interest in an understanding of some aspect of the universe or making things the "good God forgot to make" that is a quote from it's an old quote now, I think some 2001 or 2002, the Arthur Miller was the president of the US Institution of Engineers, American Institutional Engineers. And they have a one week celebration in February of not an Engineer's day, they have Engineer's week. They will have lots of posters, and Miller was apparently sitting in a bus in New York. When a little girl was sitting next to him with her mother, and this girl kept asking her mother, what do engineers do? Then she kept looking around these posters, and finally, she said oh now I know what engineers do, they make all the things that the good God forgot to make. He says that is a very good description of what engineers do. So that could be one reason why you are a research scholar in this institute, which is a good thing.

But PhD is also a prerequisite for some jobs in career advancement, if you want be a faculty member, if you want to be a research scientist, you are going to get a PhD. It's also a good route to change career path. If you are in one area and you want to change to another area, its good to come back to graduate school do a PhD in that area. Then hopefully it is not for want of current employment; unfortunately, a sizable fraction of our research scholars do come because of this reason. And it is also not money, this result is actually for chemical engineers but I suspect it's true of all engineering. They say in chemical engineering there is a huge survey that the American institute of chemical engineers did. They found a maximum of ten percent more in lifetime earnings, I mean you have to leave out the outliers some people make it to the top no matter what their discipline is those people you leave out. By and large you take people who have ordinary career paths and 10 percent more in terms of lifetime earnings. So, it is not because you are going to make more money. Hopefully for the love of it because you are curious and so on.

So, the value is actually beyond monetary returns of course, its good for your ego. If somebody has to call you doctor so and so. It is a little harder to curse you. And then research is it's own reward; this is something that I want to emphasize. Very often research scholars seem to think the reward comes later, that PhD is only a piece of paper, it is something that recognizes what you have done, but more than that if the research doing the research itself is the reward that's when you are suddenly understand something then I think you have.

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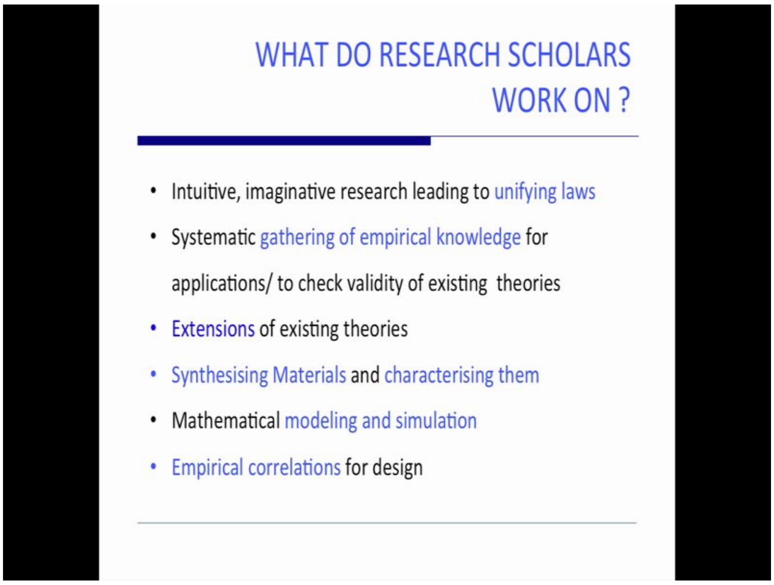

Then what do research scholars work on? This is a sort of summary of the kinds of problems people work on. First intuitive imaginative research leading to unifying laws; this is rare. If you are in that category then you do not need this lecture, you do not need any of it. The second is systematic gathering of empirical knowledge for applications this is measuring properties. For example, you may measure the viscosity of many substances. Of course, lots have been measured, but there are a new substance coming up, you will have to measure their viscosity for the sake of applications. Or you may be doing it in order to check the validity of existing theories. I think these constitute about 60 percent or it may be little less, may be about 40 percent of the entire work. If you look at journals and look at all the papers that come you will find this is a very large fraction. Then extensions of existing theories, this is also quite common. A theory may be applicable to Newtonian fluids, you may want to extend it to non-Newtonian fluids.

Then synthesizing materials and characterizing them. This is always been a big area and now it is much more so because of so called nano science and technology. 1959, I think when Feynman gave a lecture to the American physical society. The lecture was titled There is a Plenty of Room at The Bottom; it was the first lecture on nano science. And essentially Feynman said that we have reached the point when we can manipulate individual molecules or sets of molecules, and therefore, you can change the structure of material, so that you can get the properties you want. See, when I was a student this was unthinkable and I mean that tells you partly how old I am, but basically it was unthinkable. They said this is the property, you substitute it that into the different transport equations, you predicted what would happen, and you say if you have this material, this is what will happen.

Now you know what you want and you ask, can you make a material that would respond to shear-stress in this fashion. So, you can ask such questions, you can ask for materials for example, we have materials that are conducting one way and non-conducting the other way. And they are used often in applications where heat is conducted one way, but you can touch the material from the other side very practically touch it. So, I think there are now what you have is a great potential to synthesize materials, you can change molecular additives here and there, and create substances that have properties that you want, but you have to characterize them of course. That is a very large fraction of the research.

Now at the moment, I think nano science and technology probably covers sixty percent of all current. Mathematical modeling and simulation, this is an important part although I must warn you that you should never get into mathematical modeling, if you don't have a physical understanding of the system. I mean doing mathematical modeling for its own sake is meaningless, although there are some useful occasions where you do that. For example, you do neural networks and this is all this is done primarily for control. It is not so much for understanding the science behind it, but to understand the relationship between output and input of a system; and use that the control the system. In chemical engineering, it is used, presumably in metallurgy also because in large number of systems you have a lag in measurement.

Ideally what you would like is find a product, set a set point, find the difference in properties between the product, and what you wanted, use that difference to drives the process to correct it that is feedback control. But in most systems in chemical engineering, particularly the feedback comes too late. For example, if you are saponifying oil to produce soap, you are adding sodium hydroxide to oil to produce soap. If you have added too much sodium hydroxide, you will know only after the results come back from the lab; by that time that batch is gone. So, what people do is now do a mathematical model of the process, they give the disturbances that come in the feed to the model and ask what is the consequence. Use that consequence to control the process. So, basically mathematical modeling is done in order to do anticipated control.

Then second is simulation. Modeling gives you conceptual understanding and simulation supposed to give you actual understanding. So, mathematical modeling and simulation is now a large fraction of the work. Finally, empirical correlations for design should realize that the lot of our… the science is still inadequate. A lot of systems are not sufficiently well understood. And you must also realize very often industry is ahead of us; that is if there is a profit to be made people will find a technology to make that material or to produce that service even if you do not understand the science behind the technology. So, very often science comes after the technology is successful. You will always need empirical correlations for design. Partly also because we do not understand turbulence. So because you don’t understand turbulence, you cannot do the correlations for heat transfer for all kinds of processes. So, what you have to do is do it empirically.

And if you look at analysis basically in all of research, we divide a large problem that is to be solved into parts. we divide the small parts sometimes as a conceptual difficulty, you leave the half that contains the conceptual difficulty and solve all the problems related to the other half. And once all of those are solved people take the other half again you divide it into half. So, analysis always proceeds that way. We do not know how to proceed otherwise; occasionally a person has an intuitive flash and he produces an understanding that completes the design process. But 99 percent of the cases we still have to cross the final design using empiricism. So, empirical correlations for design are very much a part of engineering design.

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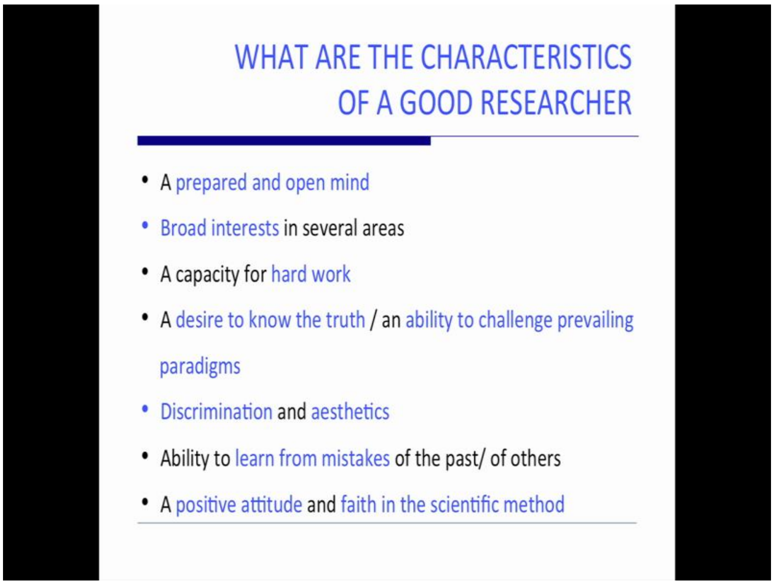

So, now what are the characteristics of a good researcher. The first thing you need is a prepared and an open mind. when I say open mind, I don't' mean a empty mind. I mean a mind that is full, but is still receptive to new ideas. Then you must have a broad interest in several areas, I must say a lot of you although you have chosen your field still don't know where your interest is. So, I think it is important for you to discover your interest and to keep your interest broad in several areas partly because an idea in one field may help progress in another field.

So, I would recommend for example, that you attend a large number of seminars. In fact, in my own case my PhD problem, I solved the problem, actually I had to deal with molecules with angle dependent forces, and the theory was already known for molecules mono atomic substances. And in order to deal with this I have to deal with rotation, and I had to deal with four dimensional harmonics in the mathematics. And I was quite ignorant what is already in the field at that that time; in chemical engineering there was nothing. So, I ended up deriving a lot of properties of four dimensional harmonics. And I very uncomfortable with them because the algebra was very complex. I had results that looked somewhat clumsy. Then finally, I went to a physics department seminars simply because they have gave very good eats. But the seminar was by a guy called Rose and still remember, I didn't understand 90 percent of what he said, but he was talking about angular momenta and quantum mechanics. And he suddenly showed me the four-dimensional harmonics results and that were already been done and I just had to go back and verify, I was very happy that my results were right, but they had very elegant notation and a beautiful way of treating them. The idea is that it gave me such confidence then I could proceed further; I think sometimes the idea comes from various fields, you never know where it will come from.

Then third property you need is capacity for hard work. I think there is no substitute for hard work. Anything you get without hard work will always leave you a little insecure. So, I mean it's always… our students have a favorite way of saying their fundaaes are weak da, so I don't know. The main reason they are weak is because you haven't put on the hard work to understand them. And then a desire to another characteristic is desire to know the truth; you must have a desire to know the truth. In this context, nowadays it is particularly important that you don't pretend to the truth. I mean there are people who are good research workers who are fallen for this, who have done plagiarism or have manipulated their experiments, it never pays in the long run. So, you must have a desire to know the truth and an ability to challenge the prevailing paradigms.

Then Discrimination and aesthetics. I think this is very important. Ultimately all the research proceeds based on your sense of aesthetics. You think this is a beautiful way of doing it and not that. And I think that's very, very important you have to develop a sensitive aesthetics. When you read a paper just reading the abstract you should be able to say whether the treatment is beautiful or not. I think you should have an opinion on that. You may change it, but you should cultivate tastes.

Discrimination is very important, because if I teach you thermodynamics and you have a problem in thermodynamics, you also read transport phenomena which is irreversible processes. If you arrive at an explanation of a thermodynamic problem at equilibrium problem through a non-equilibrium process, it is a very bad way of dealing with it. I mean ultimately an equilibrium process should not require that you know how the system changes when it's not at equilibrium.

I mean it's basically you must have the discrimination to say I will use only tools that belong to the equilibrium case and not use tools that belong there. So, and the other thing I keep telling undergraduates is and it applies to you, when you do course work in graduate school; discrimination also means know the amusing story is that I show an apple and ask you what is it, and you shout orange. Then you have told me that you don't know an apple, you don't know an orange; you don't know both. And a person like me, I grade by negative marks. I give you a 100 and then subtract whenever you make a mistake. So, if you kept quite you would have lost only 5 marks, if you shout the wrong answer you get minus 5 twice.

So, I think it's important that you be able to discriminate between in this case apples and oranges. Then ability to learn from mistakes of the past and mistakes of others; this is a peculiar ability that human beings have and I think it's important that you learn that. And finally, a positive attitude and faith in the scientific method. Basically, you can challenge the method. But basically you have to have faith that this is a process by which I can do things in a reliable manner.

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Let me say a few words about learning and creativity. The split-brain experiments are experiments that were conducted by Roger Sperry. He got the 1981 Nobel Prize in

Physiology and Medicine. He and his coworkers have elucidated the learning process. There's a beautiful book by his student, Gazzaniga, on the brain but I think a very nice summary is given by a person called Blakesley. It is called the right brain, and this is the summary. It says, essentially two sides of the brain are fundamentally different. The left brain is logical, it thinks in words and is good at step-by-step reasoning. And the right brain is intuitive and musical and uses visual images to draw conclusions.

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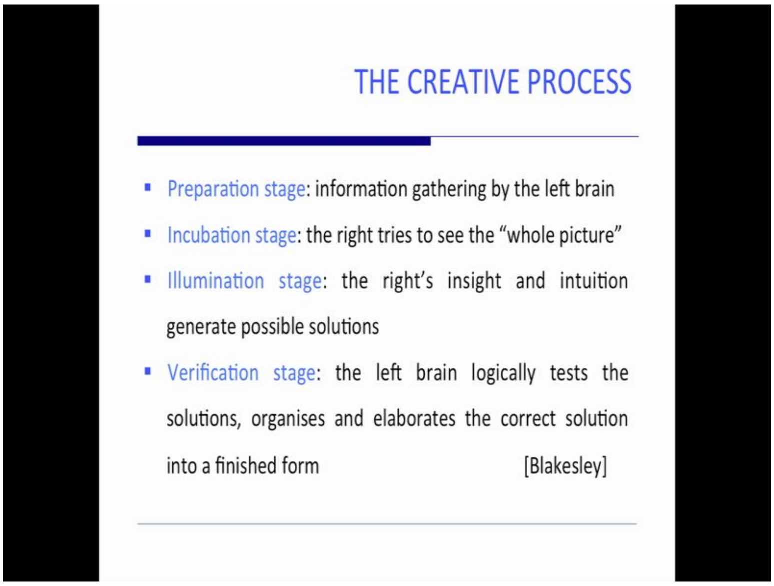

The creative process itself this is a over simplified summary but gives you a pretty good idea. There's a preparation stage where information is gathered by the left-brain. In fact, I keep telling my colleagues, if students don't listen in class, don't worry, their left-brain is anyway collecting the data, it's actually true you will know when you read for an exam. And even when you are not attentive in class when you are reading for the exam you suddenly tell yourself oh! this is why the guy was going on and on in class about this. You suddenly understand something and you know why the teacher went on and on and on about it even though you are not listening, because your left-brain was listening. But that's the preparation stage where you gather data, it's information gathering stage.

The second and the third stage belong to the right brain. It is called the incubation stage when the right brain tries to see the whole picture. And the illumination stage when the right brains insight and intuition generate possible solutions and you think you have hit the right solution. But this is often this happens to you at night when you are reading just before the exam. Suddenly things fall into place and everything seems to be right, but when you enter the hall, it breaks down and you become nervous again. And this is typical I mean the right brain is intuitive, it generates solutions but very often these solutions are wrong. So, you need a verification stage where the left brain logically tests the solution. In fact, we take this for granted now. But it is only Galileo, who said experimentation is very important, verification before that there was a peculiar situation.

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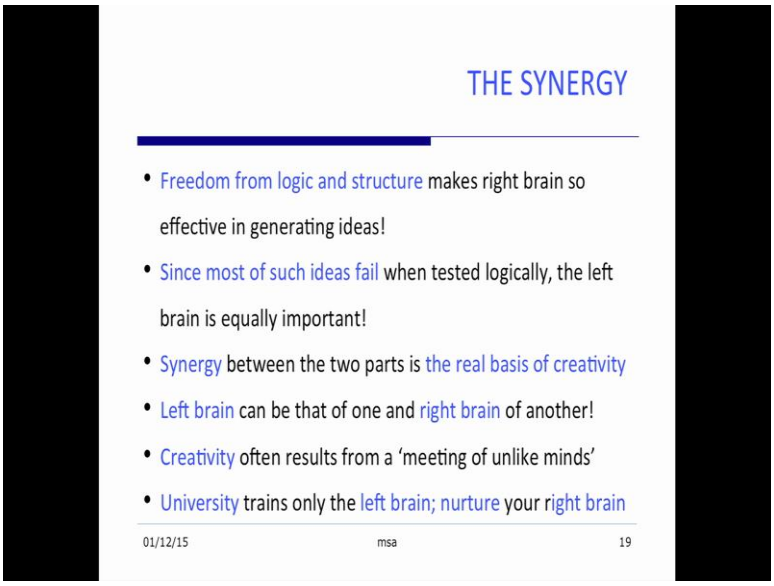

First let me say this. Freedom from logic and structure is what makes the right brain so effective in generating ideas. Since most such ideas fail when tested logically, the left brain is equally important. It is a synergy between the two parts that is the real basis of creativity, you need both. And basically, the course work is usually based on logic. All of university education is mostly based on logic, because I cannot really teach you intuition, I can only tell you what I understand logically I can explain to you. So, you have to nurture your right brain, you have it and you should nurture it. And you should realize that in our tradition and in the Greek tradition, the left brain was usually that of one person in the right brain was that of an another. Typically, we collected data and went to a sage who used his right brain his or her right brain and explained to you how all the confusing things that you had in your mind fit together, but because the sage said it we couldn't verify it.

I think my favor as anecdote is that of Aristotle you know, he said women have fewer teeth than men. So, some 260 AD and it was 1450 AD before somebody finally, said no, no Aristotle was wrong, I have actually counted the teeth of men and women they have the same number of teeth. And mind you, Aristotle had two wives and he never counted that teeth apparently. So, but I think this kind of thing happened in both and it's Galileo who said you have to question everything. And now if you have a good new theory of science, you have to propose an experiment that may lead to its downfall. For example, Einstein proposed that you measure the bending of light during a solar eclipse from a place in Africa, where it was a cloud you know cloudless weather and there was new moon. So, you could actually see the light and bending. And Edington went and measured it, it was exactly as Einstein predicted. If it wasn't Einstein's theory, would have been thrown out, but Einstein had to propose that experiment.

So, basically the creativity results from a meeting of unlike minds that's somebody's strong right brain, somebody has a strong left brain. So, you need not be in the same person and you should have a meeting of unlike minds which is why in the university I recommend very strongly you tend to one tends to group into like minds. You know you make friends of people who are like minded. But I think in the university you should make friends with people who have an unlike mind. Even if they make for a little unpleasantness, the chances of joint creativity are higher. The university basically trains the left brain you have to nurture your right brain.

I will tell you incidentally in my thermodynamics course, I have told students often that even if it's a complex problem you have an intuitive understanding of it. So, write the answer on the top. So and then I tell them, then do the problem logically and verify if logic confirms your intuition. Typical of IIT students, they write that intuitive answer in pencil, work out the problem; if it does not agree, they erase it and write this answer there. I tell them there are no marks for it but they still play it safe, it's not for me, it's for your own sake. If your logic confirms your intuition, you will get self-confidence and very often, you have a feel for what's right, what's likely to be this?

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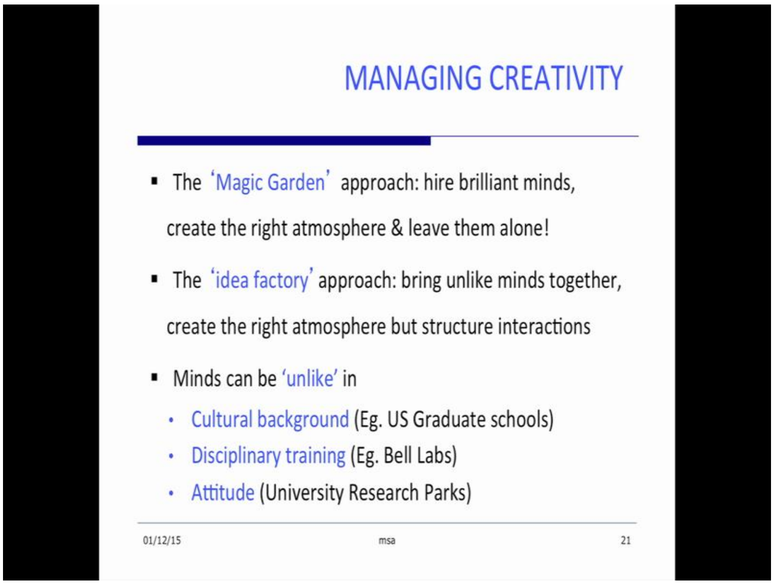

So, then I want to talk a little bit about managing. This is special to IIT madras because we have a research park. The university is the source of almost all creativity in history. And question is how can university manage its creativity. There are two methods that are described usually. A guy called Shrager describes these. And I think he has a book on creativity I forget the name of the book. Basically, there is a magic garden approach where you hire brilliant minds create the right atmosphere and leave them alone. If you are director of the institute you pray because they may sometimes retire without doing anything. This is the risk you have to take, because the brain is very good you must have it in the university. But the second approach which is called the idea factory approach was actually evolved in research labs in the US in the thirties and forties, Bell labs is the most famous for it. The idea was to bring unlike minds together from different disciplines together, and allow them to create the right atmosphere, give them a lot of freedom, but structure interactions.

And anyway, minds can be unlike in different ways. First unlike success of meeting of unlike minds is the US graduate school. Typically, where they take people from different cultural backgrounds. You must realize that science is universal, but the scientist is not; the scientists has a cultural background therefore, a set of prejudices. So, if you want to overcome at least some prejudices, you must mix people from different cultures. And the

US did it by accident, because the country immigrants, they have been very successful. Second is disciplinary training, that is one I have told you about Bell labs. They brought together mathematicians, physicists, chemical engineers, electrical engineers all kinds of people in one group and that group produced most of the results in solid state physics, even Bardeen was in that group. But afterwards they called it the solid state physics group, then they lost a lot of their sink, because now you needed a visa. You should have done solid state physics. Either you are undergraduate or postgraduate to get in. I think you need to have a mix.

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And the third is attitude. University research parks, where you have the… I think I have; Yes, the university research park is basically a property based venture near a campus like we have done here. It creates a local concentration of skill and technology. It promotes innovation competitiveness and entrepreneurship. It helps convert research ideas into innovative technologies. It houses R and D of companies. Creates and nurtures start-ups and drives technology-led regional development.

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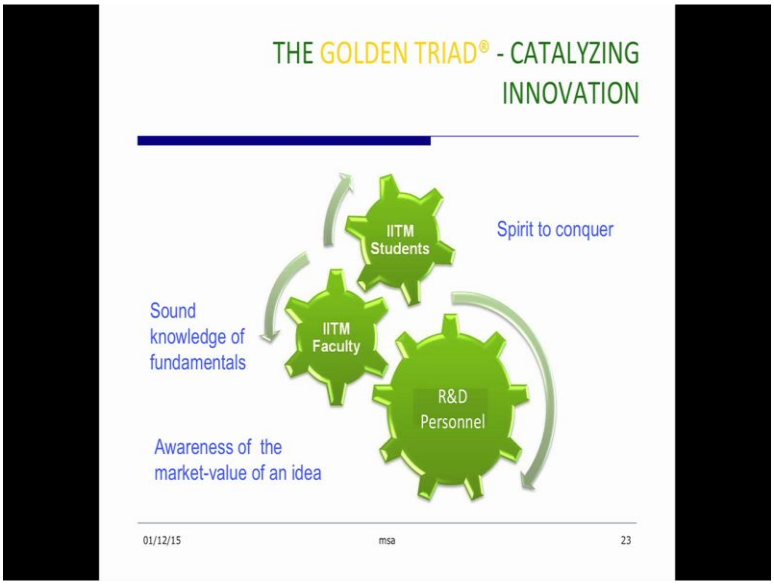

The idea in this is this it's called the GOLDEN TRIAD. You have three types of people: the students who have a spirit to conquer and IIT faculty who have a sound knowledge of fundamentals, and R and D personal from the industry who have an awareness of the market value of an idea. So, if you have these three minds together then… see students are likely to come up with a large number of ideas, but they have the advantage that they can come up with all kinds of wrong ideas, nothing happens to them. A proff for example, is considered an expert once you finished your PhD, you are considered an expert. Then if you say some rotten ideas, they will say don't you even know this. Whereas when you are a student you have the freedom, you can make 99 mistakes in the 100th idea may be great hit and that is all that is required. So, you need this combination.

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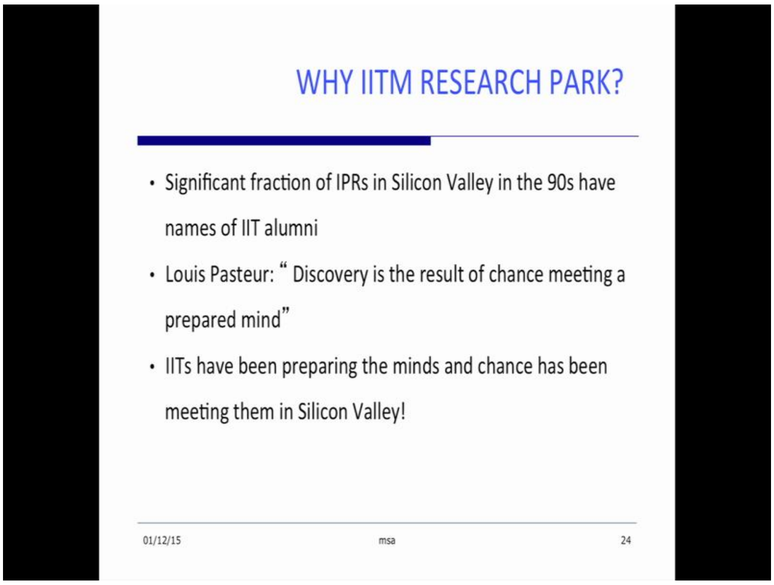

And why IITM research park? We did an analysis of IPRs in Silicon Valley which is probably created the largest number of IPRs per unit time. In the 90s, and we have found a very large number of them have the names of alumni, IIT alumni almost 50 percent. So, I mean they have many names one of them is an IIT alumnus. And Louis Pasteur said Discovery is the result of chance meeting a prepared mind. In particular, I think he was talking about the discovery of penicillin. I mean for example, had an idea of what was when he saw this fungus. When he saw this thing grow, he realized that there was a cure for diseases.

Anyway, so he said this, he said discovery is the result of chance meeting a prepared mind in experimental research. I omitted that, and I quoted this to MHRD and I said IITs have been preparing minds and chance has been meeting them in Silicon Valley. So, I said they need to meet chance in our backyard and MHRD agreed.

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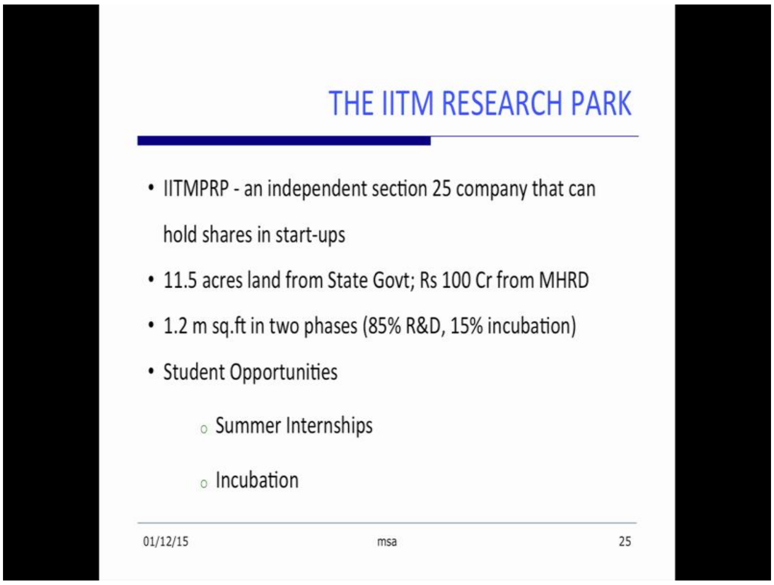

So, we started the IITM research park; it is an independent section 25 company, I think now the section has been changed, called section 8. You can hold shares in start-ups, IIT cannot. We got 11.5 acres from the State Government. I wanted it just outside the campus and luckily for us the MGR film city was closed down. So, they had 40 acres in out of which we got about nearly 12 acres, 11.5 acres. And the state government was very generous, they gave us that land; it is adjoining us. The idea is that the values in an academic institution are different from values in a market place. So, you shouldn't mix them if possible. So, I preserve in fact, I told the chief secretary. He said, you have 630 acres why are you asking me for 12. I said 630 acres of pure academic land where only Saraswati will be worshiped, and I want a place where I will worship Lakshmi along with you and with the industry. And just on that basis, he said yes, he was very enlightened chief secretary that time. And he gave me seven acres then the government changed they gave me another five and half acres. So, we got about eleven and half acres total.

And MHRD gave us 100 Crores, it took about 7 years, but finally, they gave us a 100 crore loan. They going to make it a grant. And we decided to put 1.2 million square feet in two phases in this; 85 percent is for R and D, 15 percent was incubation. Research Park, University Research Parks exist everywhere else in the World. In India, this is the first research park. In fact, when I started talking about it in 2001, when I became director 2005 is when Dr. Chidambaram was then our chairman he wanted me to go to the US to attend meeting of the association of University Research Parks. He said you go represent India. I said what India? what research park? there is none in India. He said you are the only one talking about it, so you go. And I went there and the first speaker was a Chinese, this girl lady, she is a quite young lady; she got up and said china has a very modest program only 100 research parks, only 100 acres per research park, only 10,000 companies in each research park and only 1.2 billion dollars of support from the government.

So, I put up my hand and said can I go last. I was next journalist and she said nothing doing you have to speak in this order. Then I went up and said, one research park, 10 acres site, you know even our budget was 300 cores at that time. But any way, actually china has over done it, a lot of our research parks are empty. But the point is, I think research parks are an important source of innovative technologies. Now research parks, as far as Research College is concerned also you give a chance for summer internships, because you get ideas from research there. And incubation, if you want start companies, a lot of IIT people have started companies already.

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So, this is the research park you must have seen it. I don’t know if you have visited it there, ok good.

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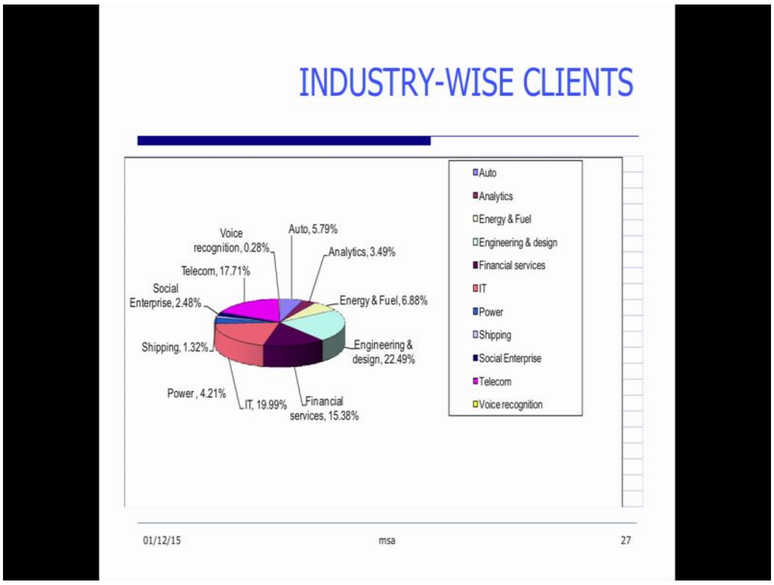

Then this is our clients; we have got clients in all sectors. And there is also a lot of interaction between the clients that is also generated a lot of research. I think we have total of some 75 patents per year now. We used to be five and this is a very amusing because in 2005, IIT Madras got the award for the University that produce the largest number of patents. So, the commerce secretary called me and said I want to congratulate you. I said are you sure we got only 5, he said others have less. So, its not that we are not creative, but this patenting the idea of intellectual property right was not there, and we created a cell here, but after the research park came this is increased tremendously.

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And these are some incubates.

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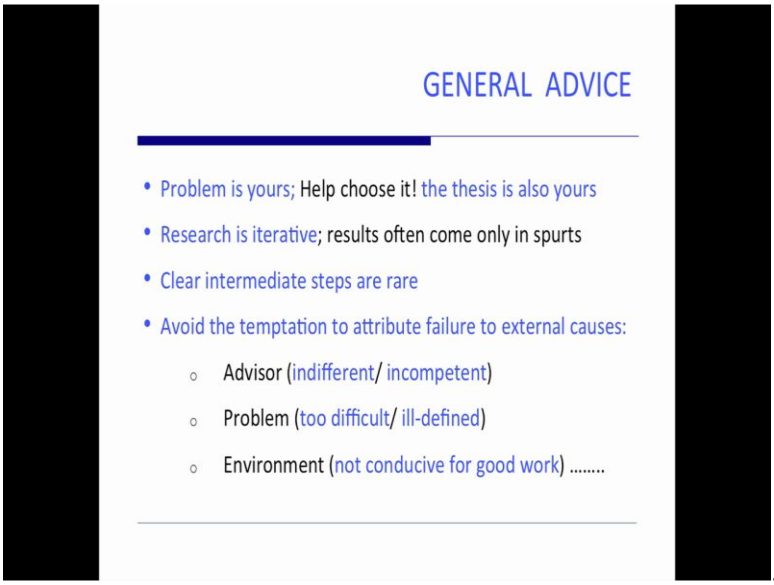

So, let me now get to some advice. General advice, the PhD problem is yours. So, you have to help choose it. By and large, we find, I had 18 PhD students only two of them helped me choose, I may help choose their own problems. We give a general area because most of us faculty have an area of expertise. So, we see within this area, if you want to you pick up a problem. Usually I used to give students about a year, year and half and they never came back with the problem; and one or two cases in two cases they came up with and. So, the thesis is also yours, when you write your thesis it should be in your style. Of course, if you make mistakes your guide will correct it, but by and large, you should have a style of writing. It's a story after all.

And research is iterative, you must remember that results very often come in spurts. There are whole times year, year and half when no result comes and you can feel very frustrated. And there are no clear intermediate steps, they very rare and you have to avoid the temptation to attribute the failure to external causes. Normally the adviser is what takes that. Every student thinks that advisor is indifferent, incompetent, they will come and tell you when you deal research, sir I have a problem and very seriously they will look at you they will say you should not be angry with me. But you know my advisor is indifferent and things like that. Actually, it is not indifferent, usually you take a PhD student, because you have run out of ideas you have got some basic idea but you don't know, how to tackle a particular problem. You are hoping this new mind will come up with an idea. And then you put it together. That's how… I mean if I could solve the problem myself why would I take a student at all. So, it is not occasionally this is true; 1 in a 100 may be true or 1 in a 1000. But by and large adviser is not indifferent because he also does not have an idea, he is waiting for the nucleus have an idea to come. His incompetence is not something that a student can usually judge; it's very rare.

Then the other thing is he say problem is too difficult it's ill defined. These are things that… first of all problem too difficult is common assumption, but very often after they finish they are very happy that they solved a difficult problem. It's ill defined very often you have to define the problem carefully. You have to know what's possible, what's not possible. The environment is not conducive for good work. Normally I find if a student is really good, they get into these conflicts right at the beginning, I mean not, they do not say the adviser indifferent, they go argue with the adviser has to why I should work on this problem. But that's out of interest. If you have goofed for three years and then you go and complain the adviser is indifferent or the problem is difficult; that means, you are just trying to escape from a disciplinary action that might follow. So, I think the important thing as that the problem is yours, you have to define it in such a manner that’s meaningful. You must know after you finish the problem, what’s the direction of your research will be if you continue as a faculty.

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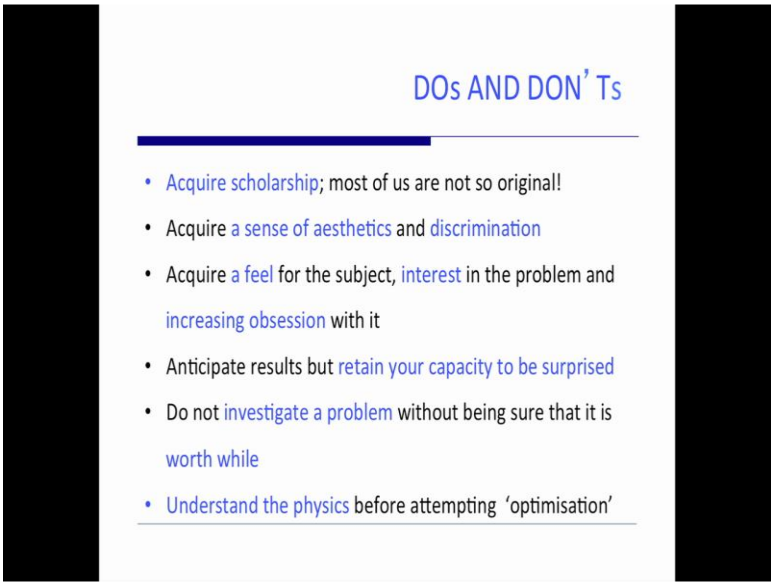

There is some dos and don'ts. Acquire scholarship. When I say most of us are not so original, I did not being you know pessimistic. It is a fact that we have… there are very few people who are really original. I am taking about Newton, Einstein people of the highest caliber. But most of us are not that original. And what we are talking about is solving a problem for the first time but solving it has a follow up of some theories that has already been formulated. But you should acquire scholarship. I think that’s very, very important. So, people very often don’t take courses every student and the guide also comes to me and say: Sir we want to finish these four courses in the first year, as if it something we got rid off. So, you take whatever course is available actually you should take courses that are meaningful to your research, and if you take more courses it doesn’t matter. In fact, I probably have a record for the largest number of graduate courses in US in Florida, because the Chairman told me why are you taking, so many courses I told him this: I told him I am not original, so I want scholarship.

And he was a very nice gentleman. He called me in the evening, he said this was in a coffee room. He said come and see me young man at 4.30 in the evening. And the secretary Karen Walker told me, Ananth you are in trouble! When he says come and see me young man, he is going to give you a lecture that you will be sorry to hear. So, I went in at 4.30 and I told him before you say anything let me say this: I think I am very clever in my class of thirty students, I think only Charlie the bros may be cleverer than me. I am certainly cleverer than all others, but on the other hand I do not think I am original enough to impress myself. Then he surprisingly he smiled he called for coffee and he said you are quite mature, you have to make this decision, but don't underestimate yourself, you should work very hard. The point is of course, you shouldn't underestimate yourself, but on the other hand, it is important to realize that many people are not so original. It is good to acquire scholarship, because when you learn many areas then ideas come in the area that you are really interested in.

I have already said this aesthetics and discrimination. Acquire a feel for this subject and interest in the problem and increasing obsession with it. I am afraid that is not something that seems to happen. I wish people would do that, I mean I have before I became director at least not many students many students did not know me. So, I would walk to the canteen, and try to over hear what students were discussing. Of course, they discuss a lot of politics, lot of cinema. But I was hoping at least 15 percent of the conversation will be about their research; I am afraid it wasn't! I think it was about ten percent I am hoping it will increase with time.

Then anticipate results, but retain your capacity to be surprised. When a synopsis is presented in dean researches office, people present their results as if it's routine. Some of it couldn't have been routine they must have been (Refer Time: 47:55) then I asked them did you really expect this? Then he says yes. Then I asked them what is your intuition about this; then they say the opposite. Then I tell them then why aren't you surprised, somehow you have lost that ability to be surprised. I think that shouldn't happen. Then do not investigate a problem without being sure it's worthwhile but this must begin right at the beginning. You must discuss with your guide, because the guide is also can also be mistaken about the value of our problem. There I mean at the PhD level you are more like equals. Then understand physics before attempting optimization people try it to do modeling and optimization without understanding the physics.

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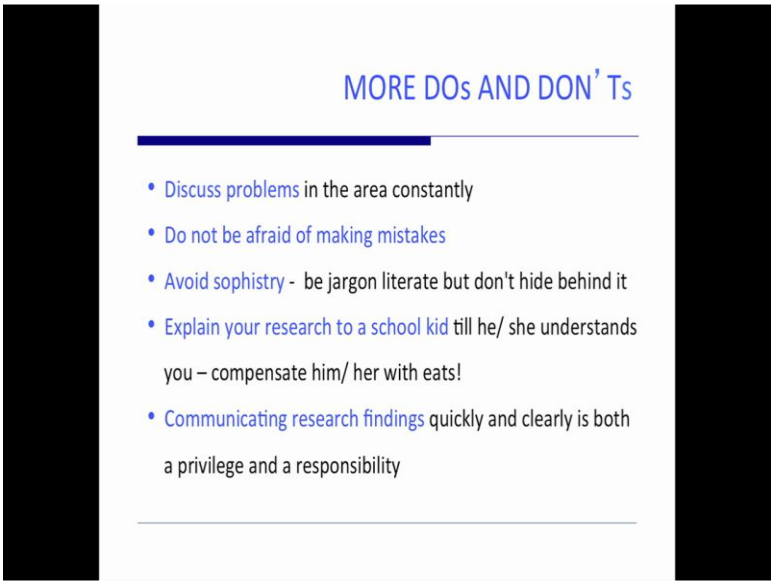

So, more dos and don'ts; discuss problems in the area constantly. I would recommend that very strongly. Because you never know where ideas will come from; people who don’t know the subject can give you very good ideas. Then do not be afraid of making mistakes. Avoid sophistry, you must know the jargon in your field but you shouldn’t hide behind it. Explain your research to your school kid till he or she understands you. And you have to buy them some chocolates or something ice cream. But I have recommended this only two of my students took this suggestion. And they ended up with I mean some of students they explained to where their own cousins or something in school. But the point was that they when they wrote their thesis, I did not have to correct it at all. Because when you explain it you know the story fully. Otherwise, you end up having Ravana kidnapped Sita before Sita marries Rama. And there is no Ramayana at all. You have to have the right order. Anyway, communicating research findings quickly and clearly is both the privilege and a responsibility. It is a privilege, but it is also a responsibility. Because all research is a cumulative effort, everybody pitches inputs in epsilon and all of it together is what solves problems.

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Here I would say make many mental drafts. I think this is important, I prefer mental. Because when you write it takes a long time, but if you can envisage the whole thing and make the draft it would be wonderful. You have to be brief, but complete you have practice preci writing. I do not know if you have done preci writing in school. Our teachers used to make us write you write an essay they will say make it one third the size that is the préci; without losing content. And one student came back came with some 500 pages and then came back with one-third then I said make it one-third further he was about to hit me, but finally he was very happy with his thesis, because it is brief to the point into the excellent, very well written. I think it's not the volume, once you have the ideas right, you have write you have to learn to write it briefly.

Value of the work should be obvious from the abstract; you should write an abstract that tells everybody exactly what you have done in the field. Then second is time spent on the any aspect of the problem is seldom proportional to the number of pages it occupies in print. Sometimes the whole year's work may see only one line in the thesis, this can happen because you have now you tried different ways of doing it, didn't work out and then finally, you arrive at it. So, you will write a paragraph about how some of the approaches you used and how it didn't work, and the actual way you did it will take all the pages. Then give credit where it is to you. I think that's very important and don't plagiarize. I think that's becoming increasingly important. I was just coming from Bombay by plane and one Civil Engineering Professor was traveling with me, he said he had just admitted a student to his group and throw him out on the first week itself. At the end of one week, he was met he met him and he said what are you going to do? He said, I don't know somebody must have done this problem, so I will take this that and put it together in submit a thesis. The man said leave the this thing, you are not going to stay. And also he said the student said if the experiment doesn't work, I know how to adjust things. I think those are things that you have to avoid at all times.

Take reviewers comment seriously. When you have to write your paper up and send it. Reviewers comments can be very damaging sometimes, sometimes very puzzling. I had my most interesting experience was with industrial engineering chemistry, we had done a work on deactivation of catalysts. There is carbon deposition and the catalyst gets deactivated. We had done a very complicated modeling of the system, and the coupled partial differential equations and we solved it had beautiful plots. But by the time we finished all that we have got so tired our results and discussion was one paragraph and we sent it off. And then the reviewer said very good problem, very good formulation, very good solution, so what? That is all, that is all he said. He said, so what? Then a pick ford who was the editor of the journal said I could not agree more with the reviewer, please revise and send it. So, we sat down for one month we took to write the results and discussion.

The idea is, if you have done so much effort, you must be able to explain the results so that others don't have to do the computation and all that effort to understand what the physics is. So, when we finally, wrote the physics the discussion alone, we only changed that section. And then pickford said I don't have to send it to the reviewer I am accepting it, it appeared afterwards. I think in India particularly; we don't write results and discussions with enough care. Very often, you get exhausted by the time you solve the problem therefore, you don't, but I think that's very, very important.

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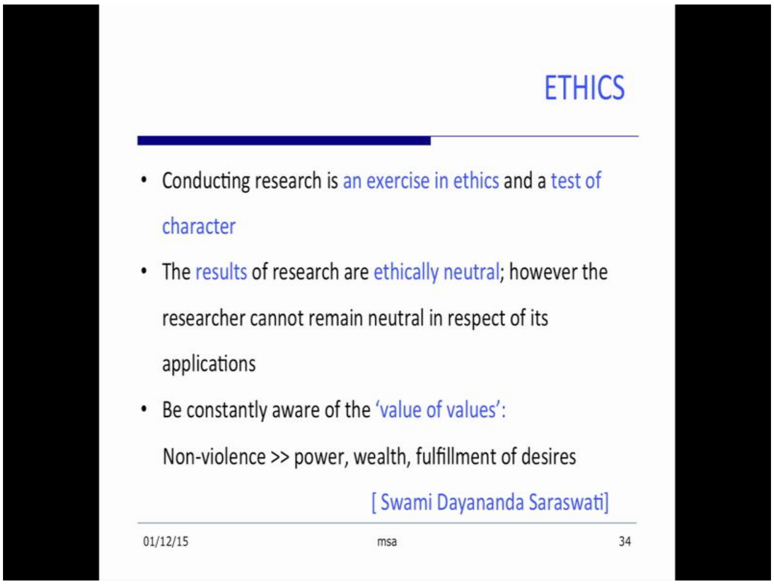

The last slide is about ethics. Conducting research is an exercise in ethics and a test of character. I think that's very important to realize. The results of research are ethically neutral, but the researcher cannot remain neutral in respect of its applications. The most dramatic example, I can think of is the atom bomb. All the research that was done to develop nuclear power was done after quantum mechanics was born twenties, thirties, forties. And when they finally, discovered a understood everything about fission then mean while due to historical accident people were afraid Germany will make the bombs or America rush to make the bomb, and they made it they dropped it. At that time the Japanese scientist were participated in the quantum mechanics and this thing was terribly devastated. He said he never thought his colleagues will do this. But eventually what happened was a lot of scientists went into a depression. Because they felt suddenly they had pursued truth, they had pursued the truth about nuclear fission, and it had resulted in a bomb that killed hundreds of thousands of people and maimed many more for many years.

So, you cannot control this and even after that the hydrogen bomb was worked on and Edward Taylor made a famous statement saying, I cannot help working on the hydrogen bomb, my curiosity is over whelming. How it is used is not my business! I think that's no longer true. The only way to control that it is not really you it is not in your hands but the way to control it is, I think for scientist to write to about the research in common journals in you know popular journals not giving technical details, but telling them what the consequences is the research may be. A well-informed public is the best protection in a democracy against misuse of science. So, I think that’s important.

Finally, I will close with a statement on value of values. And this was Swami Dayananda Saraswati who spoke here in IIT madras about 10 years ago. He passed away recently, he said don’t try to tell students about values, they already know. If you tell for example, if you take a thief and tell him don’t rob its not a good thing, he already knows its not a good thing. So, he said instead talk about the value of values, they are many values. One could be acquisition of power, another value is acquisition of wealth, another could be acquisition to just fulfillment of desires and so on. All of these are values. But there is one universal value which is called non-violence. Because you don’t want to be hurt you shouldn’t hurt others. But Swami Dayananda Saraswati like Gandhiji also he explained that non-violence is not just absence of physical violence, it could be mental violence, it could be any form, but you know when it is violence. So, you know when when somebody has used violence against you.

So, he said teach them that the value of non-violence is greater than the value of power, is greater than the value of wealth and greater than the value of fulfillment of desires. And then tell them that if they acquire wealth, power or fulfill their desires through non-violence means they welcome to it. And Lord Krishna says that’s the path of the dharma; so if you walk the path of the dhrama, I will walk with you; otherwise, you walk alone; there is no punishment, you walk alone. Then he looked, there were 450 students in the hall he looked at them and said, do you think walking alone is easy, and most twenty year olds plus or minus three they all said of course, we will walk alone.

Then he said it was the day after the exam, you know this Taramani temple next to Taramani, I don’t know a few you people still have that story. My students used to say if you come around that temple you are supposed to get five more marks. When you come to the exam you just cycle around it and come to the exam. He says may or may not be true but why take a chance if you get five marks why not. So, then he asked them he somehow knew about it he said and 400 hands went up in the CLT. Then he said that is what Krishna means by walking with you. I mean it’s not something dramatic, it is simply that human beings need something to hold on to and they create various forms of help for themselves. And I think the point he made was very valid that if you know that non-violent ways of getting anything is acceptable, then you welcome to it. If you use violent means, it is just not worth it and I think that’s important remember right through and being ethical is rather important. So, I think that’s the last slide I have.

Thank you for your patience.

Thank you.