Prof. Brahm Prakash Memorial Lecture

Fun and Joy of Science: Learning from Anomalies and Discontinuities

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1. I deem it a great honour and a special privilege to deliver the 2002 Professor Brahm Prakash Memorial Lecture. When I look at the list of previous speakers, I find that they were men of great eminence. Several of them were closely associated with Professor Brahm Prakash, and knew him closely – both personally and professionally. I cannot lay claim to either of these qualifications. Yet I thought here was an opportunity for me to pay my homage to the memory of this great son of India. Professor Brahm Prakash was an outstanding scientist and a leader, who made lasting impact on Indian science and technology, especially in the field of materials development and high technology systems integration. His contributions were critical and decisive as they came during the formative and crucial phases of the development of our atomic energy and space programmes. Apart from this, Professor Brahm Prakash was a man of sterling personal qualities; indeed he would be a great role model for all of us today. I am, therefore, grateful to you for giving me this opportunity for paying my own humble tributes to Prof. Brahm Prakash.

Research in a 'Zone of Comfort

2. I have titled this lecture as "Fun & Joy of Science: Learning from Anomalies and Discontinuities". Why have I chosen this topic? I find that many of our scientists and engineers are generally comfortable in status quo. Most of us are happy with organized research. A problem is posed and a solution is found. We use all the known tools of science, theoretical and experimental; be they instrumental, which span an amazing range of length and time scales or computational, whose power and reach are becoming mind-boggling. Invariably we develop

models and theories and try to fit the experimental data or we do vice versa. When the models fit the experiments, we are all happy. The student is happy, since he can finish his Ph.D. thesis in time and, perhaps seek his postdoctoral in that land of opportunity, namely United States of America. The guide is happy, since he feels here is one more of his contribution to the pool of scientific knowledge, and also because one more research publication is under his belt. The referee of the paper is happy, since the theory or model fits the experiment; surely if there is a fit, both the theory and the experiment must be right! He does not have to stretch himself. The editor is happy too, since he is not publishing a paper, which is likely to raise controversies. So this is the happy zone, a comfort zone!

3. But what about those problems, which are not in the comfort zone? Those unresolved problems, which have been crying for answers for years, but are too risky to try. What about those observations, which look anomalous, since they go beyond what would be expected by common sense? What about those sudden discontinuities that appear on the horizon? When a theory or a model is developed, most points fit the line of prediction, but some data fall outside. Are these just experimental aberrations, or is there a deep message in them, which can open up a new frontier? It is my feeling that many of us leave such things alone, like a fast rising ball outside the off stump. We do know that trying to hit it can bring rich rewards but there is a danger of getting caught behind too! I am going to persuade you to believe that there are a lot of rewards in taking those risks and moving out of our comfort zones to solve problems that are challenging and risky at once.

4. I will cover diverse aspects of this issue. First, I will share some of our own joy and fun in working in this zone of discomfort. Can one organize funding of risky problems in science? I believe we can. Again I will share our own experience in doing it, first at the laboratory level, then at the CSIR level and then at the National level. I am also going to extend this to technological issues, just to show as to how an eye for anomalous observations has led to new discoveries and new technologies. From science and technology, I will move on to organizational issues, namely the issue of creating 'innovative organizations', where such risk taking can be inbuilt in the ethos and culture of the organization. I am also going to share my thoughts on how India can become a leader in technology, if it learns to take risks.

5. Let me begin by sharing some of the joy and excitement I have had in my research career by chasing anomalies, looking for points outside the prediction line, from failed experiments and from lucky accidents. In a general lecture like this, I cannot go through the scientific details. But I will cite the references for those, who may be interested in understanding the underlying detail.

Some Personal Adventures in a 'Zone of Discomfort'

6. I began my career in non-Newtonian fluid mechanics and rheology. Non-Newtonian fluids are complex fluids, such as polymer solutions and melts, suspensions, some biological fluids and so on. They are structured fluids and give rise to several bizarre flow phenomena. I remember standing right here in this Faculty Hall and delivering a lecture on 'Fascination of non-Newtonian Fluids' about ten years ago, which has appeared in Current Science1. The experiments that I am going to speak about today are simple model experiments, which gave

us results, which were totally counter- intuitive.

7. First an anomalous observation that was reported in late sixties but explained by us in late eighties. Convective heat transfer in rapid external flows has been extensively studied. It is well known that the heat transport coefficient increases with an increase in the velocity. However, in late sixties, it was found that when such heat transfer experiments are conducted in dilute viscoelastic polymer solutions across tiny cylinders, something amazing happens. At a particular critical velocity, the heat transport coefficient becomes independent of the velocity! Rapid external viscoelastic flows was a subject of intensive study in late sixties. Most efforts were focussed on using the conventional boundary layer analysis pioneered by Prandtl in the early part of the century. In 1980, we broke new grounds by challenging the conventional wisdom[2]. We argued that at the leading edge of the cylinder, it is the elastic stresses that balance the inertial ones, and not the viscous ones. This led to the formulation of an elastic boundary layer concept, the thickness of such a boundary layer being independent of the free stream velocity.

8. As a chemical engineer, I have an interest in looking at problems of both convective heat and mass transfer. Let me take a mass transfer problem now. A classical problem is the dissolution of a particle in a solvent. If you take a cube of sugar and try to dissolve it in water, you find that the time of dissolution continuously reduces as the size of the cube reduces. This is universally true. But we saw something strange, when we looked at the dissolution of a solid sphere made of a polymer. We measured the time of dissolution as we kept on reducing the size of the polymer particle. We found that the time of dissolution

kept on reducing. However, something strange happened. After a particular critical size of the particle was reached, the time of dissolution became constant[3]. In other words, no matter how small did one make the particle, the time to dissolve it always remained the same. We saw other anomalies[4], where an externally small amount of residual solvent led to an enhancement in dissolution rate by a factor of 100! The key to this anomaly was provided by us through a series of mathematical models [5-7] and detailed *in situ* experiments on particle dissolution by using high resolution solid-state NMR [4-5], which gave us an insight into the events at the molecular level. We detected the crucial role of reptation dynamics as well as the dynamics of the disengagement of the dangling chains from the polymer-solvent interface that was responsible for such a behaviour. It also gave us a very simple way of measuring the reptation time. What was equally interesting was that we were able to explain some unexplained anomalies in the adsorption – diffusion problem in polymer solutions too [8-9]. Again, it was an anomalous observation, which gave us new insights in polymer chain dynamics.

9. Let me move from a dissolving solid polymeric particle to a non-dissolving particle, which is moving through a polymer solution. If you take a polymer solution in a long tube with a large diameter and drop a sphere, it attains its terminal settling velocity after some time, which remains constant for a given sphere and a solution under ordinary circumstances. We took a polymer solution and dropped a sphere and measured the terminal settling velocity. When we dropped another identical sphere after the first sphere had settled, strangely the terminal velocity was higher. It kept on increasing with each successive drop, until it reached a plateau [10]. The second observation

was even more strange, that was made by Professor Astarita from Italy in sixties. He dropped a sphere at the centre of the tube. Then he dropped another sphere, not at the centre, but slightly away from the centre, after the first sphere had settled at the bottom. Curiously enough, the same sphere refused to follow the line of drop. It tended to move towards the centre line, a path that the first sphere had taken, and then settled along the centre line. For several decades, these phenomena remained unexplained. It was only in 90s that we found an explanation. We recognized the critical role of energetic interactions, which form transient networks, which break rather easily, but take a long time to reform. All previous efforts were based on only looking at the physical networks. Several anomalies, which were associated with the behaviour of such solutions, were explained by us [11-12]. Again, it was our continuous chase of these anomalies, which led to the development of our Energetically Crosslinked Transient Network (ECTN) model. Recently, we also provided a proof of the molecular level events that take place in the breakage and reformation of these networks by doing some unique Rheo-NMR experiments[13], which substantiated our intuitive arguments used in the model.

10. Let me stay with this problem of dissolution and motion for a little longer. If you take a polymer solution and cool it, or put it in a non-solvent you find that the polymer precipitates out. You can easily re-dissolve the precipitated polymer, when you warm it up or put it in a good solvent. But something interesting was found by Professor Metzner and his team in late seventies. When they forced a polymer solution to undergo a shear flow in a conette device, the polymer precipitated out even at room temperature. This was explained by accounting for the contribution of the free energy of deformation of

molecular molecules in altering the phase diagrams[14]. What was strange, however, was that the polymers, which were precipitated by such a process, would not re-dissolve, no matter what you did. When the polymer solution was forced to go through a porous disk, they were subject to strong extensional flow. A fibre was formed. It would not re-dissolve too. It was in 2001, that we showed the role of deformation induced hydrophobicity to explain this anomalous phenomenon[15].

11.We used an aqueous solution of flexible polyacrylamide molecules as a model system. We showed that deformation can induce strong cooperative inter-polymer hydrogen bonds between the stretched polymer chains. Further, the zipping of hydrogen bonds also increased the effective hydrophobicity of the chains and prevented the redissolution of the fibres in water. A clear fall out of the theory was the suggestion that a polymer having a semi-flexible configuration, strong proton donor and acceptor groups and a hydrophobic backbone can, in principle, would show a tendency to form strong fibres. The semi-flexibility could be induced physically by either strong shear / elongation flows, or chemically by copolymerizing flexible and rigid comonomers. This strategy could be adopted for making silk like strong fibres by synthetic means at mild conditions. It is a strange coincidence that we sent our paper for publication in July 2000, where we proposed the possible role of hydrodynamically induced hydrophobicity in the spinning of spider silk. In 2001, a paper appeared[16] on the spinning of spider silk in *Nature*, and I quote from it *"The high stress forces generated during this stage of processing probably bring the dope molecules into alignment and into a more extended conformation, so that they are able to join together with* *hydrogen conformation of the final thread. As the silk protein molecules aggregate and crystallize, they will become more hydrophobic, which should induce phase separation and hence the loss of water from the surface of the solidifying thread".* We were, of course, delighted, because that was exactly what our model was predicting, although through a rather simplistic model.

Serendipity and Indian Science

12. Let me now come to the issue of serendipity or lucky accidents and Indian science. As we know, sometimes we reach unknown destinations accidentally. This has happened for centuries. In 1786, Luigi Galvani noticed the accidental twitching of a frog's leg and discovered the principle of electric battery. In 1858, William Henry Perkins was trying to synthesize Synthetic quinine from coal tar and he came across a coloured liquid, a synthetic dye. This was the beginning of the modern chemical industry. Leo Bakeland was looking for synthetic shellac and he accidentally found Bakelite. That was the beginning of the modern plastics industry. In 1929, a gust of wind blowing over Alexander Fleming's molds, as we know, created the new antibiotic age. As a proud Indian, it worries me as to why such a wind did not blow over the laboratories of Indian innovators! Why did we not get one breakthrough, which had the potential to lead India to such a new industry or even an entirely new product through such accidents? Does this mean that those lucky accidents did not at all take place in India? Or if they did take place, were we equipped enough to spot them? What should not be forgotten is that a trained mind is required to spot these accidents. Eyes do not see what the mind does not know. Perhaps there are other reasons. Let me explain this through our own experience.

13. We have been working on gels, which are three-dimensional networks. We have been especially interested in gels that imbibe large quantities of solvent. Our earlier emphasis was on super-absorbing gels, which imbibe 100-500 of water per gm of gel. Our idea was not only to synthesize such gels in the laboratory, discover new applications, but also to investigate as to why they work in the way they do. We had a few Ph. D. students working on these fundamentals.

14. In mid-eighties, I was in Delhi, when I saw the front cover of an issue of *Nature* carrying this beautiful photograph of spatio-temporal patterns on gels, which were discovered by Tanaka from MIT. I was fascinated, since we had never noticed these patterns. I took a xerox of this cover page, brought it to Pune, and showed it to my Ph.D. student. I told him, "Look at what Tanaka has discovered for the first time and he has made it to the cover page of *Nature*. I wish we had discovered these strange patterns, we would have also made it to *Nature*' He looked at me and said, "But sir, I had observed these two years ago'. I was shocked. I said, 'why did you not tell me about them?" He said, "Sir, I thought it was not something normal. So I did not tell you." I trust in his answer lies the malady of Indian science. We are so much in search of the 'normal' that the abnormal frightens us. The lucky accident did happen in an Indian laboratory, but the one who saw it was too scared to see the significance of it. Anyway, we got all our students together and told them the importance of such observations. We told them how major breakthroughs have taken place because of people looking for and sometimes when they get lucky, actually noticing such accidents. There was a cultural shift and it

did pay a rich dividend, but almost ten years later.

15. I am located in Delhi now but I am in my laboratory during weekends with my students. Otherwise, I can only keep in touch with them by E-mail or by phone. One night at around 11 PM, I got a call from Shiny Abraham, who was doing her Ph.D. with me in NCL in Pune. An excited Shiny told me that she had found something fantastic. Normally, in gels, when volume transitions occur, a gel cylinder becomes a larger cylinder and vice versa. A gel sphere becomes a larger sphere and vice versa. She told me that she had found that a gel cylinder she was playing around with spontaneously turned into a hollow sphere with a coconut like structure. And what is more, she could reverse this process too! We had a breakthrough for the first time ever, since such a magical transition and self-organization at a macroscopic level was never observed before. During the lecture I will explain the phenomenon, but those of you, who are interested in the phenomenon can find it in our just published paper[17]. As an aside, I am sure, that if we had not encouraged students to spot such lucky accidents, we would have discovered it on the front page of *Nature* again, after someone from MIT or Cambridge would have found it!

16. Sometimes serendipity knocks on your door, but you do not hear it. The discovery of cynoacrylate adhesives, popularly known as Superglue, is a classical case. Harry Coover of Eastman Chemical Company was assigned the problem of finding an optically clear plastic from which precision gunsights could be cast. He was working with some cyanoacrylate monomers, which showed promise, but he was plagued by a recurring problem: everything these monomers touched

stuck to everything else, which he recorded. However, he didn't see this as serendipity, just as a severe pain! He was thinking about gunsights, and nothing but gunsights. The adhesive qualities of these monomers were a serious obstacle in his path. The research was successful, but the end of the War brought this project to an end. He forgot the stubbornly-sticking cyanoacrylates. Serendipity had knocked, but he did not hear it.

17. Moving ahead a few years to 1951, there was a need to discover stronger, tougher and more heat-resistant acrylate polymers for jet plans canopies. Coover was now supervising a new crop of eager young chemists who were investigating the properties of the same cyanoacrylate polymers that I had been working with earlier. The monomers were difficult to make, even more difficult to purify and still more difficult to analyze for purity. Someone in the group prepared what he thought was a pure sample of ethyl cyanoacrylate and decided to measure its refractive index in order to characterize its purity. The measurement was made and recorded. When the scientists attempted to separate the prisms, they could not! They were worried that the refractometer was ruined. Coover, however, suddenly realized that what they had was not a useless instrument, but a unique adhesive. Serendipity had given him a second chance, but this time his alert mental process led to inspiration. Immediately, Coover asked the scientists for a sample of his monomer and began gluing everything he could lay his hands on – glass plates, rubber stoppers, metal spatulas, wood, paper, plastic – in all combinations. Everything stuck to everything, almost instantly, and with bonds that could not break apart. In that one afternoon, cyanoacrylate adhesives were conceived, purely as the result of serendipity. These adhesives not only had a

significant impact on consumer and industrial applications, but also became a promising answer to a surgeon's dream of a tissue adhesive.

18. One cannot help wondering as to how many potentially important inventions lie dormant in the recorded observations of scientists, which at the time were judged to be irrelevant to their research objective. This should serve as a reminder to all of us to be open-minded and curious enough to pursue unexplained events and unexpected results that may unlock new secrets and lead to new and exciting discoveries in the future. I shall explain in this lecture as to how our attempts to take SEM pictures of some metal complexed polymeric gels had utterly failed. We too were frustrated. It is these sets of failed experiments that gave us a breakthrough in discovering 'self-healing' gels for the first time in the world!

On Hard Problems and on Working Hard at Them

19. If you analyze the winners in science, often times you find that they are ones, who chose interesting problems. A key is in the ability to pose, rather than merely solve, high-level problems. Solving an easy problem has a low payoff, because it was well within reach and does not represent a real advance. Solving a very difficult problem has a high payoff, but frequently it may not pay at all. Many problems are difficult because the associated tools and technology are not advanced enough. For example, one may do a brilliant experiment but current theory may not be able to explain it. Or, conversely, a theory may remain un-testable for many years. Thus, the region of optimal benefit lies at an intermediate level of complexity. These intermediate problems have the highest benefit per unit of effort because they are neither too simple to be useful nor too difficult to be solvable. Today's

competitive science is based on this domain. But there is no substitute to focusing energy on these difficult problems, which have a handsome pay off in the long run. Difficult problems require confidence, patience and years of hard work too.

20. James Watson felt sure that it was going to be possible to discover the molecular nature of the gene and worked hard at it – even to such an extent that he was fired from the Rockefeller Fellowship that he had. Einstein has been quoted as saying that, when he was 15 years old, he asked himself what would the world look like if [he] were moving with the velocity of light. To attack that problem he inquired into the nature of equations that had been set up for electro-magnetic fields—Maxwell's equations. It was the study of Maxwell's equations that led Einstein to his special theory of relativity. Einstein started thinking about the problem when he was 15; he was 25 when he formulated the special relativity equations.

21. Linus Pauling worked on a problem for ten years too before finding the solution. It is interesting to hear a story from Linus Pauling himself.

"Often my original ideas have come as the result of training my unconscious mind to think about a problem. I gave as an example the one on the theory of general anesthesia. I was in Boston as a member of the scientific advisory board of Massachusetts General Hospital in 1952, and this board was lectured to by the professor of anesthesiology at Harvard-Henry K. Beecher. Beecher said something that I hadn't known, that the noble gas xenon can act as a general anesthetic agent. So I said to my son (who was studying medicine).

"How do you think xenon can serve as a general anesthetic agent, since xenon doesn't form any compounds in the human body? It must be some sort of a physical action. I don't understand it" I thought about it day after day for several days; in the evening when I would go to bed, I would like there and think about the problem…. After a while I stopped that. Then, seven years later, I was reading a scientific paper on crystal structure, and I said to myself, I understand anesthesia. I worked for about a year gathering data, and then I published my paper on "A molecular theory of general anesthesia." So I had trained my unconscious mind to keep this problem in view, and whenever any new thought entered my head, any new piece of information, I would connect it up with that problem to see if there was any connection…"

Funding Risky Research

22. I have spoken as a scientist. Let me now speak as a science administrator. Can we fund risky research, kite flying or crazy ideas or out-of-the-box thinking? I think we can and we should. Let me share our experience of doing this at the laboratory level, at the CSIR level, and now even at the national level.

23. When I was the Director of National Chemical Laboratory in early nineties, we set up a 'Kite Flying Fund'. What was the philosophy behind this fund? In science, only those are remembered, who say either the first word in science or the last word in science? India has not done it often enough. Why? Because, among other things, we have not dared, risked, gambled or deliberately funded risky research. We said we will support ideas, which aim to attain some unattainable goals, meet some stretched targets, or follow novel strategies in

problem solving, that have never been used before. Here the chance of success may be one in one thousand. This fund generated a lot of excitement. I remember a fierce competition among scientists, where many innovative ideas sprang up - some of them even leading to a paper or two in *Nature* and *Science*!

24. When I moved to CSIR, we used the 'Kite Flying Fund' concept at NCL to create a 'New Idea Fund'. We invited the entire chain of laboratories to submit ideas, which had explosive creativity, and where the chance of success may again be even one in thousand. During the last 5 years we have received over 350 new ideas but we have funded only 15 of them; we are so tough on our criteria on what constitutes explosive creativity. This initiative has spurred our scientists to aim for increasingly higher level of innovation in CSIR and even individual laboratories are setting up such funds now. However, when we first introduced this fund, I remember a well-meaning friend alerting to me that this is going to be an excellent fodder for audit, because by definition, we were supporting failure rather than success.

25. At a national level, we have launched a New Millennium Indian Technology Leadership Initiative (NMITLI). The words 'technology leadership' are deliberate. They will continually remind us that we want to create an India, that will 'lead and not 'follow'. NMITLI is a vehicle, therefore for India to attain a global leadership position, in niche areas, based on technology advantage by forging true 'Team India' partnership with publicly funded institutions & academia. NMITLI looks beyond today's technology and seeks to build, capture and retain for India a leadership position in the global arena based on technology by synergising the best competencies of publicly funded R&D

institutions, academia and private industry. It is based on the premise of consciously and deliberately identifying, selecting and supporting risky ideas, concepts, technologies, etc. which could be potential winners.

26. In the short period of two years since its launch, 14 massively networked projects involving over 110 R&D institutions/academia and around 45 industry partners have been catalyzed. This is the biggest Indian Knowledge network so far, where private sector has participated. The Government has invested around 100 crores in NMITLI, that is coordinated by CSIR. The projects evolved cover a wide spectrum of technologies ranging from defunctionalisation of carbohydrates as building blocks for chemical industry of the future for replacing petroleum based hydrocarbons; to stimuli sensitive nano-particle based drug delivery systems for specific therapeutics, to flat panel liquid crystal display systems, with switching speeds that are hundred times faster than the state-of-the art systems! Most of these projects seek to usher in a complete new paradigm in technology perspective with support for risky ideas, daring and creativity.

27. Let me explain that risk taking is the key and therefore, in scientific research, there should be no place for those who preserve the systems in a pre-fabricated and unaltered way. A friend of mine, who is a CEO of a company from abroad, once said 'we do not shoot people, who make mistakes. We shoot people who do not take risks. What do you do?' I said, 'In India, we shoot people, who take risks!' I believe this is true. The most risk-averse are government laboratories. In fact, it is more often than not that such institutions are run by rules and regulations than by objectives. The system of S&T audit in our

laboratories needs an urgent relook.

28. One must understand that manufacturing and S&T are two different endeavors, culturally and operationally. In manufacturing, we look for zero defects and no failures, whereas in science, there is a fundamental right to fail. An interesting analysis has been done by Stephen and Burley in 1997 for Industrial Research Institute, which lists out the significant odds facing would be innovators by analyzing consistent data from new product development, potential activity and venture capital experience. It has been shown that there is a universal curve, which illustrates the number of substantial new product ideas surviving between each stage of the new product development process. Indeed, out of 3000 raw ideas (hand written), 300 are submitted, which lead to around 125 small projects, further leading to 9 significant developments, 4 major developments, 1.7 launches and 1 success. In India, it is the other way around, since if they are abandoned at an intermediate stage, there is a risk of audit objections.

29. When we fund ‘futuristic research’, we are funding risks too. But many times, the view of the future is taken by extrapolating the present. This does not always work out. Indeed the ability to speculate on the future is more difficult now than ever before. Even when the pace of change was nowhere near what it is today, the forecasts made by some of the brightest minds went so wrong. Let me recall one such effort. In 1937, the National Academy of Science (USA) organized a study aimed at predicting breakthroughs of the future. Several wise statements about agriculture, synthetic rubber etc. were made. They were essentially based on an imaginative extrapolation of the present. But it missed all the things that happened. It was amusing that in their

predication, there was no mention of nuclear energy, no antibiotic (although it was just 8 years after Fleming), no jet aircraft, no rocketry, nor any use of space! And these are precisely the technologies that have dominated our lives in the last few decades. I, therefore, feel that we must respect judgements of those, who are capable of exceptional flights of fancy, rather than only relying on those, who are experts in narrow areas of specialization.

30. On the issue of funding risky research in industry, my favourite is the company 3M. It has become a leading innovator of products, ranging from the mundane to the breathtakingly complex. This is because the company encourages risk. Take the example of the simple 'Post-it' notepad that is so ubiquitous nowadays. It started off as a failed experiment at making a better adhesive. If you are a company in the business of making adhesives then when you are faced with an adhesive that does not bond very well the immediate instinct would be to shelve the product as a bad 'invention'. But not in 3M. A creative employee thought of a brilliant idea of using the poor adhesive to make easily removable note pads – the 'Post-it' notepad. Today the 'Post-it' notepad is such a wildly successful product. The CEO of 3M, William McKnight, built a company where tinkering by employees is encouraged and an environment is created in which accidents happen. What is more important is that the ideas generated by this tinkering are championed by the management into products that meet real human needs.

31. There is so much to learn from the innovative firms around the world as far as supporting risky research is concerned. Some firms set up goals that stretch your mind. For example, Du Pont has defined a

set of ‘unreachable goals’ like immortal polymers, zero waste processes, elastic coatings as hard as diamonds, elastomers as strong as steel, materials that repair themselves, chemical plants that are run by a single chip and coatings that change colour on demand. These may sound unrealistic but they are publicized widely and enthusiastically supported. Intel motivates its innovations by saying *“Double machine performance at every price point every year”.* Unfortunately, I have to cite only these examples from the western world, since I am not aware of an Indian firm, who supports risks in the way these companies do.

32. We must also understand that the challenge is not only that of funding risky ideas, but also spotting and funding mavericks, who have the potential to create breakthroughs. Such unusual innovators refuse to preserve status quo. Whereas standard science management practices tend to avoid conflicts, such people create conflicts. They bring in unusual spontaneity and exceptionality to the table. Their incentives are personal and emotional. They are not institutional or financial. Such innovators are sometimes extremely intense. Great innovators like Carother, who developed world’s first synthetic fibre nylon, committed suicide. Diesel, who invented diesel engine, also committed suicide. Managing such intense and creative people requires a subtle understanding of the pain of creation that such people undergo day in and day out.

Finally

33. Let me end by saying that science is an exploration of the nature of reality, both inside and outside us. The emphasis here is on things, which are quantifiable and measurable and on theories, which can be

tested and demonstrated and facts, which can be observed and verified by others. Imagination plays a vital part in both science and art, but in science it has certain constraints. As Feynman has said, "*Whatever we are allowed to imagine in science has to be consistent with everything else that we know. The problem of creating something which is new, but which is consistent with everything which has been seen before, is one of extreme difficulty."* At the same time, the difficulty with science is often not with the new ideas, but **in escaping the old ones**. A certain amount of **irreverence** is essential for creative pursuit in science. I believe that if we promote that irreverence in Indian science, by change of personal attitudes, change of funding patterns, creating new organizational values, creating that extra space for risk taking, respecting the occasional mavericks and rewarding the risk takers, then not only will the fun & joy of doing science will increase, but also Indian science will make that difference, that "much awaited" difference.

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