Life of a modeler

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Science is referred to as the *art of the solvable*. What it really means is that science is the art of *defining* a problem that is solvable. I am for the moment ignoring the 'solving' part since it is generally believed that defining the problem constitutes 90% of the work! Off and on I think about how researchers select problems (for researching, otherwise there are plenty of them!), how they actually do research and so on. What I have here is a Brownian account of those. The generic question here is: 'Is there a scientific method?' In its strictest sense, a method is an algorithm, a recipe. If the method is implemented, results must follow: research problems should be generated, if you like, in some automatic way. Imagine how nice it would be if such a method was available. A PhD student should come into IISc, practise the method, pose a well defined problem, and graduate in three years or less!

There is a hitch. Research is about creating *new* knowledge. Recently I read some thing of a parody on this bit about 'new'. What is the answer to 1137 times 7089? Most likely, this will be a 'new problem' since no one might have asked this question. If some one did, one surely can find a combination of two, if not two, three or more numbers whose product no one has considered! As the combination is new, it follows that the result is new. But is this research? In some sense, therefore a problem worth researching has to have some significance. Generating such a problem, which at the same time is solvable, involves some creativity. Can there be a method to be creative, an algorithm for discovery? It is generally believed that the answer is no and, it looks like there can be no such thing as a scientific method. The question we posed leads to a dead end: not a good research topic. To make progress (or to make the question answerable), we may allow some error bar to this definition. Let me *justify*. We all know about music. Music is a creative art (if you disagree, don't go any where near Narendra), but one can discern styles or *gharanas*. In this sense, there can be methods that can be imparted or people can be trained, to practice the art of research. I have been a modeler, at least since I came to IISc, and I want to describe life of a modeler. May be you can tell me if there is a method in this madness.

A model is a *story* of cause and effect in a complex phenomena, which at the same is quantitative. First thing I will tell you is that modelers need to be very imaginative. In the golden days of USA, CVs in the wild west used to read: 'Have gun, will travel'. I am not sure if they were body guards or 'supari' sellers. I can best describe CV of a modeler as: 'Have imagination. Will model'. Imagination is illustrated by the beautiful lines of Lennon: 'Imagine no need for greed or hunger, nothing to kill or die for'. But the imagination of a modeler is a lot more. It is also about cause and effect: 'Is this why there is hunger? Is this why one kills?' A modeler must have imagination to see common features in apparently different phenomena. Then only knowledge from diverse fields can be synthesized and applied to generate an interesting *hypothesis* or a problem to investigate. Modeler is curious and imaginative: wonders why some thing happens, and asks what if some thing that occurs in a phenomena also occurs in a totally different context, and so on. Mind wanders and dwells. It is these aspects that *create* a research problem. Prof Kumar is a great modeler. He is able to use his imagination to see 'fundamentals' that enable him to deal with Ayurveda, IPR *etc.* Let me warn you of dangers of being imaginative. Prof Kumar and I had a girl student, call her Ms X, and she was not at all unhealthy, if you know what I mean. One day, we were walking together and Prof Kumar saw a 'munni', sort of a very healthy girl. He greeted her with great affection and I was wondering who she was. When I asked him, he looked at me as if I was struck with Alzheimer's and said she is Ms X. Of course, she was not, and later when Ms X found out about this error, she was furious! I attribute the error entirely to imagination, and I will let you decide who is being too imaginative! It turns out that our learning or knowledge makes it difficult for us to be imaginative. I read in an article written about how Ramanujan, the mathematical genius, was an interpreter of dreams. Every one knows about Namakkal goddess and Ramanujan. Interestingly, the article quoted another example, that of of Don Newman, a maths professor in *the* MIT in the fifties. He apparently was struggling with a complex problem, and solved it in a dream. He dreamt that John Nash (A beautiful mind) appeared in his

dream and explained the solution. I have a model of this. One theory in psychology is that mind consists of three parts: Parent, Adult and Child. The child is wild, imaginative and can run amok. Naturally, the Parent tries to discipline, and places restrictions. Adult finds the mediated resolution of conflicts. When Newman's mind's child is trying to be imaginative, the parent was disciplining and solution would not appear in the conscious state. Adult found a via media: where Nash, who the parent is not concerned with, appeared and brought the solution to consciousness. Don't worry about the model, but as illustrated by the incident, a modeler must let the mind wander, here and there, seeing similarities, synthesizing, questioning, let ideas to find answers occur and not suppress them too quickly, and let ideas ripen. Is there scope for imagination every where? Obviously, scope is greater in phenomena with great complexity. I think, perhaps, scope is less in mature fields. Fields mature when a lot of knowledge has been acquired and 'parent' has the upper hand! Greater creativity is required to find significant and solvable questions in them. But I will be unfair if I did not emphasize control. For example, one might dream of creating trees that grow roots up and branches down, so that we can pluck fruits easily! Imagination must also be controlled, and knowledge is needed for this. One has to mount creativity on top of knowledge. Knowledge can be acquired in many ways. One can read. One can listen to lectures. All of this is necessary but not sufficient to be a modeler. A modeler has to discuss with *sympathetic and tough* colleagues. It is discussion that sharpens ideas, makes them realistic, and impart many dimensions.

I am not sure if I did say some thing about the problem generation part. Prof Lokras used to sing a song which went like: It was clear as mud, but it covered the ground. I hope I did better. However, life of a modeler is that of a nomad. Ideally, problems will have to be such that the answer is not obvious *a priori*, the solution must have some novelty or surprise (Surprise is the essence of Science) when it is found, and the listeners must accept it at once (and kick themselves) when the solution is explained. One is constantly searching for such problems, and a modeler is indeed a nomad. Searching makes one insecure, all the time on the lookout, changing areas of involvement, to find interesting problems. But, like a nomad, a modeler can see new scenes and, if lucky, some of them will be glorious panoramas from the top of a mountain. Nomads are wanderers and all know that rolling stone gathers no mass, what ever that might mean.

Are there other ways of doing research? And where a method can be found? I am on slippery ground here but let me venture. A very popular example is that of a program of research. A program starts with a *mega* idea, it can even be obvious or some one has generated it *etc.* Once the idea is generated, a program of implementing it can be followed. For example, use of solar energy could be the mega idea. One then initiates a program of finding materials to harvest light energy, ways of converting it into electrical or thermal energy, designing apparatus to achieve this *etc.* I have a feeling that here also, it is difficult to find innovative ideas and there is a danger that one can fall into the category of finding the product of 1137 and 7089. An advantage is that, at least, one does accumulate information and can relate to problems of the world: health, food *etc.* Another popular method is to work in an area. In a well defined area, the challenges or problems are known to one and all. 'What is to be done?' is not a question. But well known problems have well known obstacles! An example of such an area is turbulence. Here the difficulty is to find innovative ways of getting around the obstacles. The well known problems might have baby problems but then .... An advantage here is that researcher acquires expertise in a well defined domain. Another style I can see is the acquisition proficiency in a set of tools and or techniques (to be distinguished from skills) and look for problems to solve. However, this does not seem to be a method of generating problems, but only about solving problems. May be, there are other *gharanas.*

It appears to me that for modelers, generation of research problems is a problem. I think that there is no way of getting around being innovative and creative, at some level, to find solvable, meaningful and impact making research topics. I have a question. Is there *really* no method to be creative?

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