INTERVIEW

Standing Conventional Wisdom on its Head

An interview with RAGHAVENDRA GADAGKAR by HARI SRIDHAR.

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Hari Sridhar (HS): The motivation for this interview is the talk you gave in Young Ecologists Talk and Interact (YETI) conference in 2009, which was in the form of advice to young ecologists on how, you think, science should be done. One of the points you made that stayed with me for long after the talk was about making the transition from being a student to doing research – from taking courses to doing research – which you said involves a process of 'unlearning'. Can you tell us a little more about what form you think this 'unlearning' should take?

Raghavendra Gadagkar (RG): One of the points I emphasised was how to make the transition from taking courses and passing exams to doing research and making discoveries. And I said that if you want to make this transition, you really have to turn around 180 degrees because the optimum strategy for being successful in taking courses and passing exams is quite the opposite – not just different but the opposite – of the optimum strategy for making discoveries. For example, if one wants to take courses and pass exams then it makes sense for the person to be in a place where one is comfortable. If you have to write a test and you know that you will be given four questions and you have to answer any two, it makes sense for you to focus on the two where you are most comfortable. It doesn't make sense for you to say: I know the answers to these two, I don't know the other two, but I am going to try and answer those for which I do not know the answer. But if you are doing research that is exactly what you have to do. It doesn't make any sense to say I know this, therefore, I am going to work in this area. You have to work on what you don't know. You have to move away from the comfort zone of knowledge and familiarity and position yourself in the zone of discomfort, comprising of ignorance and unfamiliarity. In other words, you must enjoy feeling stupid. If you say that it's frustrating because you don't know what's going on then you are probably not cut out for research. Also, for taking courses and passing exams you often have to focus on storing information and recalling at the right time, which is not at all useful for research. Now that is easy to say – most people will agree with that – but the problem is much deeper and psychological. Our entire social structure is built on great prestige for knowing facts and a great shame for not knowing facts. Somehow, thinking is not part of our social culture. I guess that's because for most things you don't have to think - somebody has thought it for you. You just have to know what it is. In research obviously, that doesn't work. You have to do exactly the opposite and you have to learn how to think *de novo*.

There is another problem with doing this, which is that you need a certain kind of mental energy to absorb facts and recall them and a very different kind to be able to think through something. These two are not the same. You can train yourself either to do this or to do that. Seldom can you do both; there is a trade-off between these two. Take the example of people who become millionaires after answering all those questions on quizzes – they are really optimised so that they can store and recall information efficiently. They don't need to think. And that's a good thing when you are taking exams and doing courses. In fact, all said and done, in most exams, thinking is still a minor thing. In fact, even if it is required, you also need to remember lots of facts in order to do the thinking. In research, you don't have any of those problems. If you want to optimise yourself for thinking, then you have to tell yourself that you don't need to carry the facts with you. You can get them whenever you want. If you clutter your table with all the supplies you have in your house, you can't cook. So all the items should be on their shelves and you take them out whenever you want them and then put them back. You should have space to cook. The equivalent shelf for research is your library or the internet. Take it, use it, but keep space in your brain to do the thinking. But in order to do that, you must get over this feeling of pride in knowing facts and feeling ashamed of not knowing them. I tell my students: do not be proud that you know something and do not be embarrassed that you don't know something. In fact, I go to the extent of saying that those of you who already know the facts are the unfortunate people, because once you know a fact it's very difficult for you to actually derive it *de novo*. It's very hard for you to walk that path. Your mind refuses to do that. Also, it's very boring. Imagine somebody gave you a really nice mystery novel and says: I recommend you read it and by the way, that's the guy who actually committed the murder. You will not want to read that book. You will want to read that book because you want to know who the killer is. That's what drives you to read the book.

So, I often tell my students that I won't give them facts but I will teach them how to think. But in order to do that, I have to find a topic where they don't know the answers already. And I must confess that it is becoming increasingly difficult to do so. Due to the internet, now students know everything. They don't know how to get there but they all have the final answers. So I have to find something that they haven't heard of. And then what happens in a typical class is there are a few students who know it all and are eager to answer and I have to keep them down. I always look for that student who doesn't know the answer so that I can demonstrate through that student, to the others, how to find the answer *de novo*. Sometimes it gets very funny. A student once told me: There's something wrong with you. Yesterday I knew the answer and you didn't let me answer. Today I don't know the answer and you are harassing me. But that is exactly the point. It is by 'harassing' the student who doesn't know the answer that you can demonstrate how to find the answer *de novo,* to the unfortunate ones who already know the answers. So I keep telling them that when I am 'harassing' this student the others can't go to sleep. This student is the altruist who is going to help you learn how to walk this path that you are no longer capable of walking because you already know the answer. What does that mean? That means you shouldn't read too much because you have closed the doors for everything that you have read and got the final answer for. Even as an experiment in learning how to think, that territory is no longer available to you. So there is an optimum amount of reading that you should do. And your reading, as far as possible, should be around or outside your field. If you want to do research, you shouldn't read on too much in your topic, but around your topic, so that you can bring in new perspectives to bear on the topic. If you read all there is to read on your topic you will probably do the same thing that other people have done. It is very hard to think of new ideas. Every idea you can think of has already been thought of.

But even more than that, the real problem I think is that, given the way our brains are constructed, it's much easier for us to master the art of remembering - of inputting and outputting facts. I think our brains are not constructed to think very much, and another big problem in thinking is what I call mental laziness. Very quickly you are lazy to think further. Let me take an example. In an exam, a student is copying from his neighbour. Now, Step 1 is that the student knows that he is copying. Step 2 is that the student knows that I have seen him copying. Step 3 is that the student knows that I know that he knows that I have seen him copying. These are three different things if you think about it. If the student has copied and nothing else has happened that has one consequence. If the student has copied and I have seen him that has another consequence – I can punish him but he doesn't know that. If he knows that I know, he will stop copying, otherwise; he will continue to copy – do you see these are three different things? Now with some difficulty, we can manage these three levels. But you can take it to a fourth level – do I know that the student knows that I have seen him copying? If I know that the student knows, then I know that he is going to stop copying and therefore I must do something else. Now you can take this to infinite steps but try doing this and your mind gives up very quickly. Three is difficult, four is about the limit, and after that, it's all muddled up, because of mental laziness. And that is what we need to avoid. But that's not a problem when you are taking courses and passing exams. So doing research is a very different game. Unfortunately, we do not train people to do research. We mainly train people to take courses and pass exams.

HS: You say one shouldn't read too much. Is there the danger of going too much in that direction? One would imagine that to be able to come up with new ideas, to be creative, one needs to be able to make connections between facts, and those connections might not happen unless one has those facts in one's head.

RG: I have the following model. I make the assumption that anybody who wants to do research is a reasonably intelligent person who will read X amount of stuff in a 24 hour period. All I'm saying is that as large a fraction of that X as possible should be outside and around your field; outside your problem. I am not saying you shouldn't read. Then, of course, you are not able to think. But as far as possible the reading should be outside. In research, you don't want to do what others have already done. You want to do something different. You want to come up with a different theory, a different answer. In research, getting a correct answer is not important, getting a logical answer is important. You don't know which answer is correct. Ultimately, if all these hypotheses are put to test then something turns out to be correct. We don't reward people for being correct, we reward people for saying something new. For doing something that's creative. Therefore, the best strategy is to do what others are not doing. And you can take for granted that most people are working very close to the centre of their problem. So the further you go with your reading, the more are the chances that you will have a different perspective from others. And that's really what matters. So it's not the absolute amount of reading, but the distance of your reading from the actual centre of your problem that matters.

HS: You mentioned in your talk that people who are good at doing courses might not be good at doing research, and that there is unlikely to be a strong correlation between the two...

RG: Let me refine that a little bit. I think there is a trade-off between doing well in courses and doing well in research. But I wouldn't say that people who do well in courses will not do well in research. I don't think people are fixed. If you have trained yourself to do well in courses, but are unwilling to unlearn, then you won't do well in research. But if you are willing to unlearn and now have a new strategy then you can do quite well. There is a trade-off between the strategy to do well in courses and the strategy for doing good research, but it is entirely possible that the same person will be able to change the strategy. So this has relevance to the question you were asking about how one should pick candidates for a research programme. If there was a simple one-to-one correlation - a person who is good in taking courses and passing exams will not be good in research - then, of course, it is very easy. You give an exam based on courses and whoever fails you take for research! But it doesn't work like that because some people are able to change and some people are not able to change. Also, some people are so intelligent that they will be able to optimise the strategy required for passing courses but then shift to the strategy to do good research. That is what makes it much harder to select people. You can't simply say I will take the people with the lowest marks. That's not going to work because the people with the lowest marks might not be capable of doing either. People with the highest marks maybe capable of doing both, but also may not be. And then there are people who are very poor in taking courses and passing exams but actually very good in research. So you have to work very hard at selecting people. And the one way to do this is to actually put them to a test in thinking during your selection process

HS: Isn't that difficult given the number of people who apply?

RG: Not so difficult. Ideally, take them outside their zone of comfort and see whether they can think. It is not so difficult. In 30 minutes you can judge whether a person is capable of thinking. It depends on how you spend those 30 minutes. What we typically do is for 25 out of 30 minutes we ask them for the facts they know, and for barely 5 minutes we ask them to think – that is the problem.

HS: But what you are suggesting is possible only with a smaller subset of shortlisted candidates. How do we do the first step when a large number of people apply?

RG: I think you need a minimum of 30 minutes, ideally an hour. So if you are willing to spend 50 hours you can only have 50 to 100 candidates. But you will have to have some other method to bring the first set of applicants down to 50 or 100. I don't think there is any way of getting around that. But of course, you can design written exams that test thinking abilities rather than recalling of facts.

HS: You said that while in an exam it pays to choose the easiest question, in research it might be best to choose the one we know the least about. I would like to ask you about your own strategy when it comes to choosing a research question. What makes a particular topic worth pursuing to you?

RG: The answer should not be obvious, nor should it be so obvious that you will find the answer rather quickly! Only then it's challenging, otherwise, it's not. Having said that, I must also say that in real life, for my own research - this takes us off into a completely different territory, so we won't go there just yet, but I'll just say - I have chosen to understand everything humanly possible about one species of social wasp. That is my decision. That's what I want to do. So in trying to fulfil that decision, I'm not always in the position of saying whether I shall answer this question or that. I need to answer all or most questions in order to go to the next level. So in real life, I don't always discard questions because they are easy or obvious. I do them quickly. But I find it much more challenging to address questions where the answer is not so obvious. Often there are lots of little, not so interesting, questions that you have to answer to be in a position to get to the big, more interesting and more challenging questions.

HS: You have been working with this one species for over 30 years. Right from the beginning did you know that this was going to be a long-term research interest?

RG: Not in the beginning, but very soon it became obvious. See, I had been playing with this species for a long time, but I will say that I seriously started working on this species roughly in the year 1980. And in 5 years it was clear to me that there is enough gold here that one can spend one's whole life in it. It took about 5 years, not longer than that.

HS: Going even further back, when did you first realise that you were interested in doing research?

RG: Oh! That was very long ago. I probably didn't know the meaning of research at the time that I realized that this is what I wanted to do. I mean I was curious and interested in science. But again there was this problem: I never thought that science has to be done in exclusion of other things. It never occurred to me that if I did science I couldn't do literature. I found it very funny. In those days, you had to choose in 8^{th} standard - Science stream, Arts stream or Commerce stream. I was completely at a loss because the two subjects that I was extremely interested in, and which I thought I will pursue for the rest of my life, were Science and Hindi literature. My Hindi teacher thought I was born to do a Masters in Hindi and my Science teacher thought I was born to do a PhD in Science. I thought I was born to do both! But I had to very reluctantly drop my dream of studying Hindi at that stage. And then when I came to study Biology, they said choose between Maths or Biology! So I had to drop Maths. And then later they asked me to choose between Molecular Biology or Animal behaviour. I said I want to do both. They said you can't do both, you can only do one. The higher I went, the more doors closed – very strange indeed.

HS: Going back to your own philosophy when it comes to choosing research questions – the pieces of the puzzle, not the big questions – do considerations such as doability, money required, technology required, etc., play a role in your choice?

RG: Absolutely. And I think the biggest mistake that people make is they do not do what I call a feasibility analysis. You are a postdoc in National Institutes of Health (NIH), you are working on a problem, and you bring a little piece of that problem and come and join some place in India and want to do the same thing. You are not worried about whether you can do the same thing with the same level of competitiveness; you do not worry enough about what you will need, how much money you will need, what kind of facilities you will need. Nobody worries about these matters, or at least not enough. They start-off and then they complain. I think that's the biggest problem. What you need to do is choose a research problem where the rate-limiting step is only your intelligence. That's what should actually finally stop you, not money, facilities or anything else. If you choose a problem where the rate-limiting step is your intelligence, then you will not be frustrated. You can say this is all I could do because that's all the brain I have. Whereas if you say: oh!, I could have done so much better if I had more money or if I had that equipment, that's a ready-made excuse to not do very well.

But again, one has to qualify this. In the real world, the time when you should adopt this philosophy is not when you are a PhD student; instead, it's when you become an independent scientist. In most cases, I think a PhD programme is best treated as an apprenticeship. I will give you an example. A friend of mine once sent me a message saying that he went to one of the branches of the National Museum of Natural History and met a young person who was very interested in spiders. The person said that nobody was helping her and he asked her to write to me. So she wrote to me and said that she had some very interesting ideas for spider research but was very frustrated because nobody was able to help her. I told her I didn't know too much about spiders, but she could come and visit us for a week. So she came for a week and outlined her research plan and then said that it's so frustrating that nobody is working on this! I said you should be frustrated if somebody is already working on your idea. If nobody is working on your idea, that's nice! I told her she might be planning to be someone's assistant to work on the problem of her choice, but why should she be? I told her that she should safeguard her idea and acquire all the skills that she needs to help her solve the problem later, when she is an independent scientist. Your PhD or postdoc is often only meant for you to acquire the toolkits that you need for your research career. You don't necessarily have to do your best work when you are a student or a postdoc. And in today's modern science where you need lots of techniques and skills to be able to tackle a cutting-edge problem, you should use your PhD and postdoc to equip yourself with those skills. But what most people do is that they either hope to do Nobel prize-winning work during their PhD, or they get latched on to a problem forever that ideally should only be used for training. For instance, if you want to come to my laboratory and learn a technique, then you will have to work on my problem. But then, you should not get stuck with my problem and spend the rest of your life on my problem and forget your own problem. So one must learn how to use the time meant for PhD and postdoc, effectively. I once had to tell my student- don't try to get a Nobel Prize for your PhD work, because most likely you will have to share it with me! If you wait, you might get it by yourself!

HS: Did these considerations – the money required, the technology required, etc., – go into your decision to work on Ropalidia?

RG: Absolutely! It was clear to me that the rate-limiting step should be my brain. And until today that is the rate-limiting step. In the beginning, I took a conscious decision that I am going to create a situation where the rate-limiting step, not for quantity but for the quality of my work, would be my ability to use my brain. Not anything else. So there's no excuse if you fail, otherwise, you always have a readymade excuse even before you start. People say: oh! but he is in Harvard; you can't expect me to do as well as him. You start with the assumption that you can't do as well as him. For me, there is no excuse in the world I can give why anybody else in any part of the world should be able to do this better than me. I can't think of an excuse, except that I didn't think of it or am not capable of doing it.

It also depends on where you are. Of course, if you end up in NIH you can choose different problems, but even there you must make sure that the rate-limiting step is your brain and not how much money you can get. You may get 10,000 dollars or 10 million dollars. The trouble with scientists often is, instead of saying: you tell me how much money I can get and I will think of a creative scientific problem for that money, they say: you tell me how much money you can give me and I will find a problem for which that money is not enough! I believe that you can do creative work at any level of facilities or money or whatever. Problems will change, but not the creativity.

HS: But there are external forces that make people use the approaches/technologies that are in fashion, e.g., the pressure to use molecular approaches in ecology today?

RG: That's correct. But I would put 10% of the blame on the people who create the pressure and 90% on the person who succumbs to the pressure. I would not absolve people who don't blame the individual but rather the pressure. What efforts do people make not to succumb to pressure? I think we do very little to avoid doing what we don't like to do. We immediately succumb to pressure. Often we succumb to imagined pressure. And even if it is real pressure we do precious little to fight the system. So I'm not convinced by this argument.

What have people done to fight the system? Very little indeed. Whether it is the pressure to use the latest technology for the sake of using it, to publish in *Nature* whether or not your work is good enough; or to have a collaboration with some famous person whether or not it is required. All this pressure is real. But I have not seen people fight it enough. I have seen people succumb to the pressure too easily, and once they succumb, they want the rest of the world to believe that it is not possible to resist the pressure. So they create this myth and the pressure builds up. It's a self-fulfilling prophecy.

In fact, it is probably nice that there is this pressure on people to do wrong things which leads me to fight the wrong and be different from others. After all, what is the route to success? Doing what other people have not done, not succumbing to pressure is, in fact, the route to success. You should not succumb to pressure and become like everybody else. Your goal is not just to become an assistant professor, but to be different from others. Ninety percent of the people who are assistant professors become associate professors. If that's all your goal is, then there is no problem. But that should not be your goal. People complain that it is the system, it is the society, it is the peer pressure, which makes us do all of this. I don't buy any of it. I'm extremely sceptical of it. I rarely come across people who resist pressure. People always succumb to pressure and they complain. I want to see more examples of people who resist pressure.

HS: I want to talk about another kind of pressure – the pressure to publish in high-impact journals. How do you decide where to send the papers you write?

RG: There are only two things that matter to me. One is, as far as possible, it should go to the audience I would like to reach. Today, that is becoming less important because you put it on your website and people will see it. So what is the most important consideration? It should get published, it should not get rejected, which is the opposite of a typical strategy adopted by the scientists today. The typical strategy is, no matter what one writes, one first sends it to *Nature*. When it gets rejected, one sends it to *Science*. It gets rejected then one sends it to *PNAS*. And then it will trickle down after 3–6 rejections till it finally finds its level. This is what people do. This is hugely wasteful for everybody. The first problem is that the top journals like *Nature* get all the papers in the world! So they have to reject some 99%. It's a huge waste of everybody's time. In fact I actually know people who say: I sent a paper to this journal and it got accepted. What a shame, I should have sent it to a higher journal! This is the world we live in - completely crazy! I would like to send my paper to a journal where it has the highest chance of getting accepted. Now if you want to increase that probability what do you do? You send it one step below what you think the paper deserves! I will tell you a story about one journal. In this journal they reject whatever they don't like, but of the things they like they publish a few and for the others they say: this is not good enough for our journal but we have a sister (step-sister?) journal which we can send it to. If you say yes, it most likely will get published there. I was surprised by this because I felt how can that journal tolerate this? That you are the trash basket for the bigger journal. So I met the editor of that journal and asked him. But he said no, it's so easy for us because we get papers that are reasonably good and that have already been refereed and we just publish them. Then I told him who I was and he said: Ah, you are one of the few guys who send their papers directly to us. In the long run does it really matter? The idea in your paper is what matters. Think of what might happen a hundred years from now. People will not look at your paper because it is published in *Nature* or in *Science*, but for the idea it contains. Can anything be more stupid than judging the quality of work depending on where it's published? Can you think of anything more absurd? Recently I heard this very interesting statement. Somebody said "just because our paper is published in *Nature* doesn't mean it's wrong"! But I don't even blame *Nature*. They are doing the right thing from their own point of view. We have sold our souls to journal editors. How did we let this happen?

I will tell you another story: some years ago I was invited to give a talk in a University in the USA. Before my talk, they wrote to me asking if I had some free time and whether I could meet some of the faculties in the life sciences department. They gave me a list. Is there anyone in the list who you wish to schedule a one-on-one meeting with? I looked at the list and found that most of them were my friends. So I wrote back to them saying that I know all of them and I can't choose any one or two for having one-on-one meetings with. Instead, give me a one-on-many meeting with your students. They said this is great and all the PhD students and postdocs of the department were scheduled to meet me for 2 hours over a pizza lunch. So I met the students and discussed about the issues around research and publications. Some of them said: it's very easy for you to say all these, but we are students. We have to succumb to the pressure of publishing in high impact journals because otherwise, we won't get jobs. So I said I agree with you, you are absolutely right. I said: for advancing your career you do whatever you want, do all the 'wrong' things that the system wants you to do, no problem. But very soon you will be sitting on the other side of the table to judge others. I am only asking you to make this resolve that you will not judge anyone by the journal in which he or she has published, by the impact factor, or by the H-index. If you agree to do this the world will change in 10 years. But the world has not changed because you will begin to believe that what you have done by succumbing to pressure, is the correct thing to do. So when you say you know this is wrong but you are doing it for survival, you slowly begin to believe that this is the correct thing and you make sure everybody else does it. Otherwise, the world should have changed by now. So this idea of saying I do it because I can't help it, is actually not true.

HS: But, today, do you think it is even possible to come up with ways of judging huge numbers of applicants for jobs/fellowships/PhD, etc., without resorting to convenient metrics like H-index or number of publications?

RG: Absolutely. Why are you imagining that there are always hundreds of applicants? There are hundreds of applications sometimes, when we may be forced to use unsatisfactory metrics. But the tendency always is to think of one unlikely scenario where you have no choice but to do something, and, with that, hide all the hundreds of scenarios where we can do much better. We say how else can the president of a large university with 3000 faculties decide who is good? Why should he decide who is good? He should not. He should depend on many others. If I have to judge people I will read the papers of those whose work I understand. For those whose work I don't understand, I will get the help of trusted colleagues who understand. There will be at least one person in this world who will be able to read and judge another person. And if there is nobody, then you take that candidate anyway - he must be good if there is nobody in the world who can judge him. So we don't have to imagine extreme situations all the time.

There was a time, 20 years ago, when I used to spend some days in selection committees for choosing people for one thing or another - a prize, a fellowship, a job. In every such committee somebody would come and give us a very nice description of the work of the applicants/nominees before we made our decision – these were really like a series of mini-seminars on a wide range of topics. It was a most interesting and most satisfying experience. But now, in the same selection committees, everybody comes and talks only about the number of publications, citations and H-index. I hear this for hours or days and I'm bored to death. We just don't seem to read anymore. This is a fact of life. Because these metrics are available, we have stopped reading. It's absurd. The world in which we live is completely absurd. And I think we are not realizing it.

HS: So when you find yourself in a position where you have to judge the work of your peers or students, what do you look for?

RG: If the judgement is to be based on reading, then I read. And when I don't understand I ask. I ask other people to read and explain it to me. I do not judge based on number of papers or citations or impact factors. I judge on the content and I try to understand the content and I try to compare the content. That's baseline for me. I am really impressed by a piece of work if I feel: Why didn't I think of it? That's my ultimate test. A lot of things are boring. Anybody can do it. If I had to pick one out of 10 people then I would apply that criteria. In addition, I would say: What would have happened if this paper was not published? Would it have made a difference to the field? You can always say all data is necessary, and, in the future who knows somebody may need it. Fine, but suppose I want to give a prize to one out of 10 people, I would certainly apply this kind of criteria. What would have happened if this paper had not been published? Would the field have changed? Would the field have slowed down? And do I feel that I wish I had done this piece of work? Now if you say that my method is subjective, of course it is. That is why we should keep changing committees frequently so that all manner of subjective assessments by diverse sets of people will even out.

HS: What about a piece of work makes you feel: why didn't I think of that? Does it depend on how novel the work is, does it depend on risk-taking, does it depend on being correct?

RG: It definitely doesn't depend just on being correct. It is cleverness. There is a clever way to do things and there are dull ways to do things. For example, let's say you take a well-known technique in one field and apply it to another field in an extremely exciting way. If you apply it in the same field there is nothing so great about it. Sometimes when I see work which is highly-valued or published in *Nature*, I ask myself: why did *this* person and not *that* person do this? Often the answer is: Because only this person had access to this data, or this population, or that instrument. That is not so great. It's not surprising that this person did it. I am excited by a piece of work that anybody could have done, in principle, but only one person did it. That's the kind of work that makes me think: why didn't I think of that?

HS: Could you name a few pieces of work or scientists whose work provokes that reaction in you?

RG: I could probably do it, but I prefer to give you a slightly different answer to the same question. This is something I have thought through and written about. When I was an undergrad, I was absolutely fascinated both with animal behaviour and molecular biology and I used to read everything I could get my hands on, on both of these subjects. But I had a very different reaction to what I read. When I read in molecular biology, it was absolutely fascinating, and I've described it as a play being played on a stage in heaven. It was all wonderful, but I never felt jealous because, as an undergrad student in Bangalore, the thought never crossed my mind that I could have done those things. But whenever I read a paper in animal behaviour, in addition to feeling awe, I felt jealous. I felt why did I not do this? it's something I could have done. That is the difference in my reaction to these two kinds of things. I couldn't have discovered DNA polymerase as an undergraduate in Bangalore, but I could have discovered 'imprinting'. So it certainly depends on what I can do and what I cannot do. If it's something that anybody can do, and you do it, that's great. If only you could do it, and you did it, then well it's okay, but not that great.

HS: Can you give us some specific examples of work, like imprinting, that impressed you?

RG: There I have this whole set – I have written about it - of examples from both fields. Let me give you a recent example to emphasize that correctness is not important. When WDS Hamilton [1] came up with his idea of inclusive fitness, he realized, and only he realized, that because of haplo-diploidy, it's easier for Hamilton's rule to be satisfied in Hymenoptera than any other group of animals, because the relatedness between true sisters is 0.75. This was a major breakthrough because 11 out of 12 independent origins of eusociality had happened in the order Hymenoptera, which represents only 2% of the animal kingdom. In the remaining 98%, it happened once (in termites). In those days that's all that was known: 9 or 10 times it happened in 2% of the animals and once in the remaining 98% and it is the former group that was predisposed to eusociality because sisters were related by 0.75. It turned out to be wrong in the end but that is completely irrelevant. To me, this was a creative leap. And I wished I had thought of it. Even today, even though I have been partly responsible for proving that it is wrong, I still feel I wish I had thought of it. The original formula of what we now call Hamilton's rule was creative on its own but it was even more creative to realize that in haplo-diploidy it works much more easily. Or take Trivers's [2] work. Just the whole idea of parent-offspring conflict is such an elegant, such a beautiful, idea. Anybody should have thought of it, especially after Hamilton's paper in 1964. But nobody thought of it between 1964 and 1972. If you just look at the idea, it is so creative. That there would be a zone where both parent and offspring would agree that more investment should go to the offspring, and then there would be a period when they disagree, and finally there will be a period when both will agree that no more investment is appropriate. I wish I had thought of it. If I think hard I can also come up with experimental strategies or designs that I consider creative – Von Frisch's [3] experiment, for example, where he came up with the so-called 'fan experiment'. He wanted to prove that honeybees actually got information about the direction from the dance of the scout bees. So in his experiment, he trained bees to take sugar from a particular feeder at a particular angle say, 250 m from the hive. To test their knowledge, he put not one test feeder but an array of test feeders, and he didn't put them at 250 metres but at 200 metres. Now, these two are strokes of the genius. He didn't put them at 250 metres but nearer because he wanted to rule out the possibility that, at 250, the original bee had left some scent. And then by having the array rather than a single test feeder, he realised that, the bees would make an error indeed; but the error should be symmetrical on both sides, and they should fall off, and the maximum number should be in the middle, and then there should be a symmetrical fall off on both sides. That's exactly what he found. Absolutely brilliant!

Or even the very simple primitive experiment that Tinbergen did to show that landmarks are being used by digger wasps to recognize their nests in the ground. Again, when something is quick and dirty it is even more charming. Tinbergen worked in this place where the wasps kept making holes in the ground and raised their brood there, and he saw that the holes all look the same but each wasp went only to its hole. How do they manage that? He said maybe the wasps have a detailed knowledge of the landmarks around their nest. Now how would you test this idea? Today we might take very detailed photographs and use pattern recognition software and say the grass here is a little shorter and there it is a little later. We can get the computer to map the exact landscape, etc. But Tinbergen didn't do any of that. He reasoned that if they are using landmarks and the landmark differences are very subtle, he can exaggerate the differences. There were lots of dried pine cones lying around and so he put an array of pine cones around the nest and let the wasps fly in and out and learn it a little bit. Then he removed those pine cones and put them a little bit away and reasoned that if pine cones are what it has learnt it should go to, then it should go to the new place without the nest and not to the original nest now devoid of pine cones. And sure enough, that's what the wasp did. Then he asked whether they see the pine cones or do they smell them? He now dipped the pine cones in alcohol to remove whatever smell they may have and put them back. Nevertheless, the wasps still went to where the pine cones were, suggesting that they are not using smell.

In another experiment von Frisch wanted to see if bees had colour vision, so he showed that they could distinguish between blue and green. But then one could distinguish blue and green because they may appear as two shades of grey. How do you remove that possibility? What did von Frish do – he goes to an art shop and asks for every shade of grey paper in stock. He gives all alternatives of grey and says that the bee must be confused by at least one of the shades of grey if it is learning to distinguish between two colours as shades of grey. The bees did not confuse the colours with any shade of grey. I would give that piece of work the prize and not for somebody who went into the brain and recorded everything and showed that they had the right neurons to have colour vision. All that is okay if you can afford it but what von Frisch did was the work of a genius.

HS: How do you encourage and increase creativity in your lab?

RG: One of the easy ways to do this, which we do all the time, is to discuss other people's work. I promote this idea of appreciating something not because it is sophisticated, not because it's published in *Nature*, not because it's correct, but because it is very original and creative. So you can promote this philosophy by constantly judging other peoples' work and then injecting this philosophy while making those judgements. The harder job is to actually get students to become creative themselves. If you are the supervisor, then on the rare occasion when two students come up with two different ideas you can say why you like one or the other based on these criteria. Although that doesn't happen every day. In short, there is no better way than to lead by example – be creative yourself, but that, of course, is harder still!

References

1. https://en.wikipedia.org/wiki/W._D._Hamilton
2. https://en.wikipedia.org/wiki/Robert_Trivers
3. https://en.wikipedia.org/wiki/Karl_von_Frisch