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Standing conventional wisdom on its head

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Posted on Jul 14, 2017 in OTHER, CAREER DEVELOPMENT and ADVICE

From figuring out the right research question to hiring the right people to dealing with the pressure of publishing papers– young researchers starting up a lab in India face a unique set of problems. Raghavendra Gadagkar, Professor at the Centre for Ecological Sciences and Centre for Contemporary Studies, Indian Institute of Science, Bangalore , shares his views on how to effectively manage these challenges. He urges researchers to step out of their comfort zones and start thinking *de novo.*

Raghavendra Gadagkar (Photo: Souvik Mandal)

You have said previously that the transition from being a student to doing research involves a process of 'unlearning'. Can you please elaborate on what form this 'unlearning' should take?

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 you want to take courses and pass exams then it makes sense for you to place yourself in a place where you are comfortable. For example, you have an exam and you know that you are going to be given six questions and you have to answer any four. It makes sense for you to focus on the four where you are most comfortable. It doesn't make sense for you to say I know the answers to these four, I don't know these two, but I am going to try and answer these two. 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 on 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 discomfort zone of ignorance and unfamiliarity. In other words, you must enjoy feeling stupid. 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 great shame for not knowing facts. Somehow thinking is not part of our social culture. In research obviously, that doesn't work. You have to do exactly the opposite and you have to learn how to think de novo.

Given the large number of people applying, how should one pick candidates for a research programme?

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.

What is your own strategy when it comes to choosing a research question. What makes a particular topic worth pursuing for you?

The answer should not be obvious, it should be hard to find. Only then it's challenging. 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. In trying to fulfil that decision I'm not always in the position of saying shall I answer this question or that question. 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 more challenge in those questions where the answer is not so obvious. Often there are lots of 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.

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?

Not in the beginning but very soon it became obvious. 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.

When it comes to choosing research questions, do considerations such as doability, money required, technology required etc. play a role in your choice?

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 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 facility 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. 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 not to do very well.

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?

That's correct. But I would put 10% of the blame on the people who put the pressure and 90% on the person who succumbs to the 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.

There's another kind of pressure – the pressure to publish in high-impact journals. How do you decide where to send the papers you write?

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, For which, I would send my paper to a journal where it has the highest chance of getting accepted, not *Nature* or *Science*. Can anything be more stupid that judging the quality of work depending on where it's published?

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

I do not judge on numbers 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. 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 think about- What would have happened if this paper had not been published?

When do you feel that way? Does it depend on how novel the work is, does it depend on risk-taking, does it depend on being correct?

It definitely doesn't depend on being correct. It is cleverness. 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 they did it. I am excited by work that anybody could have done, in principle, but only one person did it. The kind of work that makes me think: why didn't I think of that?

How do you encourage and increase creativity in your lab?

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's 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!

A longer version of this interview can be accessed here. (https://cesess.wordpress.com/2017/07/13/standing-conventional-wisdom-on-its-head-a-conversation-with-raghavendra-gadagkar-full-interview/)