How to Design Experiments in Animal Behaviour*

16. Cutting-Edge Research at Trifling Cost

Raghavendra Gadagkar

I have had multiple aims in writing this series of articles. My primary aim has been to show how simple and innovative experiments can be performed at almost no cost, by nearly anyone, to create significant new knowledge. The history of science shows that this is true in most areas of scientific research, albeit to varying degrees. I have focussed on the field of animal behaviour both because I am more familiar with this field than others, but also because, the field of animal behaviour is especially well-suited for such low-cost research. It has also been my aim, of course, to discuss the principles of ethology (the scientific study of animal behaviour), through the medium of these experiments. My motivation in writing this series is to bring social prestige to low-cost research, make the practice of science more inclusive and democratic, and empower large numbers of people to become knowledge producers rather than merely remain knowledge consumers. The people I especially have in mind are, less-endowed sections of society, including, but not restricted to, underdeveloped countries, marginalised institutions and individuals, students, the general public, amateurs, and all those with little or no access to large research grants and sophisticated laboratory facilities, for whatever reason.

***Note*: Some passages in this article are reprinted from Suggested Readings [4, 5, 15 and 16].**

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Animal Behaviour: An Especially Ideal Subject for Low-cost Research

Keywords
Animal behaviour, low-cost research, science funding, grant-free research, democratizing science, diversity in science.

In the fifteen articles that preceded this one, and were published in this journal between August 2018 and November 2020, I have illustrated experiments meant to answer such questions as, how do wasps find their nests, do bees have colour vision, how do ants find the shortest path, how do bees estimate distance flown, how do ants estimate distance walked, why are male wasps lazy, how do wasps decide who would be their queen, why do wasps fight, does experience matter in fighting fish, why don't male frogs do their best when they sing to attract females, why is mimicry in snakes imperfect, why do hosts care for cuckoo eggs, and why do parents and offspring quarrel? Experiments attempting to answer each of these questions illustrate how it is possible to make significant new discoveries by conducting simple, low-cost experiments, both in the laboratory and in nature. Although they do not require any expensive or sophisticated facilities, the ability to succeed in conducting such experiments depends crucially on many other attributes and skills of the researchers. These include adequate knowledge of the empirical literature, an understanding of the theoretical foundations of the discipline, lasting passion and undying curiosity, a healthy disrespect for authority, confidence that there is much about the natural world waiting to be discovered, willingness to undertake labour-intensive manual work, identifying the appropriate study animal, asking the right questions, designing innovative experiments with adequate controls, foresight in framing expectations, caution in coming to conclusions, ruling out alternate explanations, recognising the level of precision that is necessary and adequate for the question at hand, conducting sound statistical analyses, respect for a negative result, and more. I have discussed many of these in some detail in the 'Reflections' section of each article.

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In this final article, I will make some general remarks about the importance of low-cost research, for science and scientists, and for society.

The Importance of Low-cost Research for Scientists

It is not impossible to do first-rate research without applying for and obtaining financial grants, and I will return to the topic of grant-free research later in this article. But for the majority of professional scientists employed in research and educational institutions, it has become the norm to apply to various funding agencies, both government and non-government, for so-called 'grants', in order to carry out research. It is almost a universal experience of such grant applicants that they are granted less money than they had requested. Conducting research with less money than one had budgeted for is, therefore, the norm. How do we deal with this situation? Do we simply downsize the quality and quantity of our research correspondingly and tell the funding agencies that they will get what they paid for? This, of course, makes no sense because we are not doing research for the sake of the funding agencies. The nearly universal practice of obtaining grants for doing research has the danger of creating a mindset that our research effort is a contract, with and for, the funding agencies. At least in most areas of basic science, we should be doing research because we are passionate about it.

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How then do we ensure that the quality of our research does not simply scale with the quantum of money we can raise for the purpose? Maintaining the same high quality of research with a reduced budget requires a great deal of creativity and innovation. The quantum of money and the quality of facilities that researchers can muster for a given kind of research varies enormously depending on the researcher's standing in the field and his or her geographical location and institutional affiliation. And yet, I see surprisingly little discussion, let alone training, in how to get more and better research done with less money. This topic is almost taboo in the scientific community. Indeed, there is a positive selection for raising and spending more money, but I will come back to this below. There is an urgent need to change our mindset and initiate a discussion on how to do the best possible research with less money. Those who are well-endowed with big grants will not do this for us. Those of us who have less research money

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than we would like, whether this is because we are from the developing or underdeveloped world or because we are from underprivileged institutions or sections of the society, have to take the lead. Of course, our discussions and findings may help those with more money than us to do even better science than they might otherwise have done, and this will be our contribution to science as a whole.

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Nevertheless, there is only so much we can do by trying to make the best of an inadequate grant received for research already planned and a proposal already submitted. A much more effective way of doing great science with less money is to choose an area of research that we can pursue with maximal efficiency with the quantum of funds that we are likely to be able to raise. There is wide variation in the cost of research, whether due to the number of personnel required, the costs of travel to research locations, or the nature of the sophisticated equipment and technology or due to the cost of chemicals or other consumable supplies. Some areas can be pursued with maximal efficiency at relatively small costs while others may need orders of magnitude greater financial investments. It is a great mistake to think that areas of research that require less money are less important, less intellectually challenging or less interesting. There is little correlation between the cost of conducting research and its importance, or interest. If we give more importance to conducting first-rate scientific research rather than to the area of science we might work in, there is great scope to tailor our research to the funding likely to be available while keeping the bar on top-quality research consistently high. For convenience, I will refer to low-cost and high-cost research as if they are binaries, but of course, there is a continuum.

Funding situations are bound to vary, going both up and down, with time, with our (changing) geographical and institutional locations, our inevitably increasing age, our standing in the field, with the change of governments and their priorities, with changing fashions and needs of the society, not to mention wars and natural calamities. If we ignore these changes and inflexibly pursue the same kind of research at all times, then what will inevitably vary is the quality and quantity of our research. It follows then that if we want to keep the quality and quantity of our research approximately constant, through the changing fortunes in terms of grants and other facilities, we must adaptively alter our research areas to suit the times and the circumstances. This is not as impossible as it may first sound. Having very broad interests and being widely read and interested in many different areas of science and beyond, is necessary to do high-quality research even in a single area. Thus, being well equipped to undertake top-class research in any one area will automatically make it relatively easy to change our areas of research. Innate curiosity, pleasure in creative innovation and passion for the truth will let us slide easily across disciplines. These should, therefore, be the primary items in our tool-kit, rather than specialised knowledge of a narrow discipline or rare expertise in the use of some high technology. Interest in a broad array of questions rather than an infatuation with particular methods or techniques or even particular model systems is sure to facilitate mobility across areas of research.

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I find it surprising, therefore, that we place so little emphasis on the problem of how to choose a research question. In few other areas of human enterprise do people embark on long-term, not to mention life-long plans with as little feasibility analysis, as scientists do in choosing the areas of their research. Historical contingency seems to explain nearly all variation in the choice of research areas among scientists. If we change this aspect of our scientific culture and training and begin to choose our areas of research more pragmatically, not only will we be able to do first-rate research with less money, but we will also be able to adapt to changing fortunes in funding while maintaining the quality of our research. The bottom line is that we should be able to work as close to the limit of our intrinsic ability as possible, unconstrained by external limitations such as funding. I have seen both in myself and in others that it can become a habit to say that we did quite well given the constraints. But when the constraints are not inevitable, we should position ourselves in physical or disciplinary space so as to work at the limit of ability. One way to begin is to make it a habit to compare ourselves with others in absolute terms, not after factoring in our real and imagined constraints.

The Importance of Low-cost Research for Science

Somewhat distinct from its importance to individual scientists, low-cost research is also profoundly important for science as a collective human enterprise. If different areas of science require different amounts of money and sophisticated instrumentation, as I have argued above, it follows that we might neglect areas that need very little investment, if some of us do not diligently pursue low-cost research.

Somewhat distinct from its importance to individual scientists, low-cost research is also profoundly important for science as a collective human enterprise. If different areas of science require different amounts of money and sophisticated instrumentation, as I have argued above, it follows that we might neglect areas that need very little investment, if some of us do not diligently pursue low-cost research. If everyone is in the race to get large grants and pursue research questions that require such large grants, and worse still, if those who fail to get large enough grants do the best of a bad job in the same area, then surely many important areas will remain neglected. If the importance and intrinsic interest of scientific areas are uncorrelated with how expensive they are, it follows that many important and exciting areas will remain unresearched. Ironically, if everybody succeeds in getting large grants, then many areas will suffer—science as a whole will suffer! We usually further exaggerate the problem by spreading the total amount of money available too thinly so that nobody has enough money. Yet, everybody is trying to pursue expensive research. It would be prudent to allocate sufficiently large grants to some individuals to pursue research questions that are inevitably very expensive and encourage others to pursue research questions that do not need large investments. But this will only be possible if we do not treat those who get small grants as losers and deprive them of dignity and social prestige. I will have more to say about social prestige later. I often come across discussions about low-cost technology and low-cost technology substitutes but seldom about low-cost science.

I am less familiar with other fields, but at least in biology, the variation in the cost of doing research in different areas and the neglect of low-cost areas is glaring. The long-term negative consequences of such neglect are already being felt, and some would say the damage already caused is irreparable. At the most fundamental level biology needs to find, identify, and classify the Earth's vast biodiversity. The fact that over 90% of the species remain uncatalogued and do not even have a name is a failure of monumental proportions. And this is usually one of the least expensive areas of biological research. To see this in perspective, imagine that 90% of the naturally occurring elements on Earth were yet to be discovered. Our woefully inadequate knowledge of the Earth's biodiversity and how this tragedy is twice compounded by the rapid and irreversible loss of species on the one hand, and equally rapid and irreversible loss of taxonomists, on the other hand, have been repeatedly lamented [1–3].

Natural history is slightly (but only slightly) more respectable but an equally low-cost enterprise that remains mostly neglected. However, it is evident that questions and hypotheses for subsequent research almost always stem from an exploration of the natural world that is open-ended, and fuelled by a passionate love of nature and a spirit of adventure. Somewhat higher up on the social prestige scale is what we might broadly call organismal biology, which, as we have seen repeatedly in this series provides abundant opportunities to answer important questions with clever and simple low-cost experiments. And yet, we are very far from utilising these opportunities on a large scale. The main drawback of all these kinds of research seems to be that they are low-cost and, therefore, not well respected.

We have to closely examine the growth of knowledge (or the lack thereof) in specific fields to understand how impoverished our future research will be if we do not pursue taxonomy, natural history and organismal biology, much more vigorously than we have been doing in recent times. To take an example closer to my area of expertise, while reviewing the state of our knowledge of the social wasp genus *Ropalidia*, and lamenting on our ignorance of swarm-founding species, I concluded that: "To understand the social dynamics of such societies with hundreds of queens and thousands of workers cooperating to build and repair the nest, deal with predators and parasites, self-organise division of labour to

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forage for and feed tens of thousands of larvae and stage periodic swarms to make new colonies, along the lines of similar knowledge of independent-founding *Ropalidia*, would be a naturalist's challenge even as it is a theoretician's dream. Be that as it may, there isn't even a single account of their swarming behaviour and colony foundation, which is their unique feature. Unless major corrective steps are taken, the prospects of improving our knowledge in the future will remain bleak, owing to large-scale habitat destruction and accompanying species loss, dwindling of the numbers of field naturalists and the nearly complete obsession of the community of social insect researchers, with understanding the genetic, developmental, and molecular mechanisms of a small number of phenomena in fewer than a handful of model organisms" [4].

Similarly, when asked to comment on the opportunities for future research in insect social behaviour, I argued that "Studying the molecular mechanisms that make social behaviour possible requires access to well-equipped laboratories and significant infrastructure and funding. It is best done by a minority of the research community that can command such resources. The vast majority of researchers who cannot command the required resources should not be forced to do molecular biology at a suboptimal level but must be encouraged and empowered to do first-rate natural history and organismal biology. Researchers from economically backward but biodiversity-rich countries in Asia, Africa and Latin America are ideally placed to undertake first-rate natural history and discover new species and new phenomena and feed the molecular biologists with new research questions. It is sadly ironical that these researchers are often under pressure to use the meagre resources of their countries to enter into a losing competition with laboratories in advanced countries to study the molecular biology of social behaviour, instead of proudly studying the rich biodiversity in their backyard, at a fraction of the cost. The onus is on research policymakers in the developing countries to create an environment where their scientists can undertake with pride, the kind of research that they can do best" [5].

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The Importance of Low-cost Research for Society

Quite apart from its importance for individual scientists and the healthy growth of science discussed in the two previous sections, low-cost research is crucially important for the society as a whole. The most obvious importance is the saving of money, but I will discuss this in a later section. Here I will focus on an even more important but less tangible gain to be had from promoting and pursuing low-cost research. Low-cost research is the single most important way to make the practice of science—the production of scientific knowledge—democratic and inclusive. Presently the opportunities to pursue scientific research are extremely unevenly distributed along numerous axes. This is so obvious and so well known that I will not belabour the point. But let us remind ourselves of some of the most common axes of inequality. The first and perhaps the most severe inequality is between rich and poor countries, between developed, developing and underdeveloped countries. Expenditure on science as a fraction of the GDP varies between countries by orders of magnitude. Tragically, the fraction of GDP is an inappropriate measure for comparison because expensive research needs money irrespective of the GDP of the country conducting such research.

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Given that the GDP itself varies enormously and that countries with high GDP generally spend a higher fraction of their GDP on science, the resulting inequality in money available for science is truly mind-boggling. If scientists in all countries follow the same model of doing science and attempt to work in the same areas using the same methodologies, the variation in the quality and quantity of scientific knowledge production will rival the inequality in money available, nay, it will be worse because developing countries have other disadvantages due to shortage of trained scientists and poor education. The importance of low-cost research, especially for developing countries is enormous, and only if we learn to adapt to this situation by learning to get more science for less money, can we hope that science can be democratised and become more inclusive.

In every country, there is enormous inequality in the money available for research between different institutions. Some of this is due to their varying ability to use internal resources to support research, so-called intra-mural funding. There is also substantial inequality in the capacity of scientists in different types of institutions to raise external resources, so-called extramural funding, often unrelated to genuine variation in their ability to do research.

Inequality is not just between countries. In every country, there is enormous inequality in the money available for research between different institutions. Some of this is due to their varying ability to use internal resources to support research, so-called intramural funding. There is also substantial inequality in the capacity of scientists in different types of institutions to raise external resources, so-called extramural funding, often unrelated to genuine variation in their ability to do research. This is true in all countries, including the richest ones that spend the most money on research. The starkest variation in India is between research institutes and traditional Universities, with undergraduate colleges being relegated to a distant third position, and high schools being pretty much barred from seeking such funding. Despite a growing effort worldwide, to reduce the disadvantages that early-career scientists are bound to face, there is great inequality between early-career scientists and established scientists, even after correcting for any possible differences in talent and competence. It follows that early-career scientists should be more interested (and should be allowed to be more interested) in exploring low-cost research to mitigate the consequences of their relatively low funding to compete with established scientists.

Much of modern science, especially when pursued in mainstream academia, is characterised by slow maturation of scientists, with long periods of apprenticeship and late transition to the status of independent researchers. Maturation here is not measured so much by age as by accomplishment and track record in producing scientific knowledge. Relatively low-cost research may therefore be an important option for fast track movement to the rank of an independent scientist. It can be especially attractive when mid-career movement between research questions is possible and even appreciated, as it often is. What can be smarter than letting your research questions and strategies evolve to suit your changing funding fortunes while maintaining a consistently high quality of your research? If early-career scientists don't have a level playing field in acquiring funding and other facilities for doing science, students are in a worse situation. However bright their ideas and whatever be their level of competence, students need to work under a so-called Principal Investigator or PI (more on PI later) and seldom have the opportunity to be independent scientists.

To some extent, this may be because students may genuinely need more training, experience and maturity, but why should we let this be exacerbated by the dependence on a high level of funding? Students may sometimes face a trade-off between doing expensive research, as it already is being done under the banner of their mentors, or do more independent research with lower costs and alternate strategies. In such situations, enhancing the quality of their independent research by cleverly choosing their research area to be relatively unaffected by less money may come in very handy.

Even more stark inequalities in funding opportunities and even more uneven playing fields are indeed faced by amateurs, not to speak of the general public who may wish to and be quite capable of conducting scientific research. Should their research be of correspondingly lower quality? By paying attention to low-cost research, they can certainly get more for less money both by cleverly aligning their interests and inventing innovative alternatives to traditional methods and technologies employed by the privileged professional, who has less need to innovate to save costs. Indeed, they have the opportunity to show the way and put the professionals to shame. It is widely known, although not always admitted, that there is not a level playing field across other axes such as gender, race and ethnicity. Smartly employed low-cost research, maintaining high quality without lessening interest and importance has a useful role to play in many such situations.

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By promoting low-cost research as a means to mitigate the ills of inequality, I am by no means justifying or condoning inequality. Access to funds, facilities, opportunities, recognition and appreciation are unequal across multiple axes and for numerous reasons, and their levels are unacceptable. We must continuously endeavour to create a level playing field. With the best of intentions and the brightest of ideas, this will take time. The question is, what do we do in the meanwhile? It is also almost always true that the power to change the situation lies more with the haves rather than with the have-nots. Low-cost research, therefore, has a special place for those who fail to get high levels of funding, for whatever reason. My optimistic three-step dream is that (1) the privileged will do all they can to level the playing field, (2) the underprivileged will strategically use low-cost research and other methods to supplement the efforts of the privileged to level the playing field, and finally (3) the underprivileged will become the privileged and remember to continue efforts to see that all privileges reach all deserving people.

I will close this section with a brief discussion of the advantages of democratising science and making it all-inclusive. The advantages may seem obvious but let us state them explicitly. First, there is a moral imperative to provide equal opportunities for all people irrespective of nationality, wealth, age group, professional affiliations, race, caste and gender to pursue science and become knowledge producers. We frequently hail the importance of making access to knowledge universal, but I think equal opportunities to participate in knowledge production is an even more important prerequisite for people to have dignity and self-esteem.

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But the imperative to democratise science goes well beyond the moral. It is a prerequisite for the healthy growth of science itself. Scientists are all too human, complete with social, political religious and idiosyncratic prejudices. It is unreasonable to expect that all individual scientists are coldly objective truth seekers. This was memorably expressed by Richard C. Lewontin in his *The Genetic Basis of Evolutionary Change* (1974): "It is a common myth of science that scientists collect evidence about some issue and then by logic and 'intuition' form what seems to them the most reasonable interpretation of the facts. As more facts accumulate, the logic and 'intuitive' value of different interpretations change, and finally, a consensus is reached about the truth of the matter. But this textbook myth has no congruence with reality. Long before there is any direct evidence, scientific workers have brought to the issue deep-seated prejudices; the more important the issue and the more ambiguous the evidence, the more important are the prejudices, and the greater the likelihood that two diametrically opposed and irreconcilable schools will appear" [6].

I have, therefore, argued elsewhere that we should find ways of letting some scientists who satisfy certain high standards of competence and who are wedded to some pet hypothesis to pursue their passions so that the scientific community can wait and watch and see when and where their ideas fail. Only such scientists will be willing to risk their careers and reputations to take their hypotheses to their logical conclusions. The rest of us may rather timidly stop after early signs of failure, owing chiefly to the low esteem with which we hold negative results. While it is neither possible nor necessary for every individual scientist to be totally dispassionate, it is important and possible for the scientific community as a whole to be objective [7]. The historian of science Naomi Oreskes has argued that society trusts (or should trust) science because scientific knowledge is based on agreement and verification by large groups of scientists. She persuasively argues that consensus is rather hollow unless scientific communities are not only inclusive and encompass geographical, national racial and gender diversity but also embrace traditional or civilisational knowledge including traditional knowledge of tribal people, farmers, fishermen, patients and midwives. To repeat her mesmeric metaphors, "diversity serves epistemic goals", and "the non-expert world is not epistemically vacuous" [8, 9]. I would argue that low-cost research, which finds alternate ways of achieving high-quality research, would be a powerful ally in fostering such diversity in the scientific community.

Consensus is rather hollow unless scientific communities are not only inclusive and encompass geographical, national racial and gender diversity but also embrace traditional or civilisational knowledge including traditional knowledge.

Familiar Objections to Low-cost Research

I am amazed that when I espouse low-cost research, some people raise objections. The most common concern I hear is that such arguments in favour of low-cost research will reduce funding for science; politicians and funders will use the same arguments to cut back on funding. Well, even if there was any truth in this fear, we cannot, of course, be dishonest and inflate the cost of our research, nor can we morally justify spending more money than is required—after all it is somebody else's money. But I think the fear itself is entirely unjustified. I am not arguing for less money but more efficient use of funds so that more people can do cutting-edge research for the same total amount of money. Needless to say I am of course not claiming that we cannot do great science by spending a lot of money, nor am I saying that some areas of science do not need a great deal of money. All I am saying is that a great deal of good science can be done with little or no money.

In *We Are All Stardust* (2015) [10] the German physicist, author and essayist, Stefan Klein says of VS Ramachandran, the Indian-American neuroscientist and author of *Phantoms in the Brain* (1998) [11], "While other neuroscientists spend millions on their experiments and perform expensive computed tomography scans on dozens of test subjects, he uses quite simple materials. Sometimes all he needs is a mirror, a wooden box, or a cotton swab in order to achieve spectacular results." When asked whether he had anything against technology, VS Ramachandran replied, "I have nothing at all against fancy equipment. We need it and use it at times. But personally, I do research because I find it fun. And high-tech science seems less gratifying to me. The greater the distance between the raw data and the conclusion drawn from an experiment, the more boring it is....Luckily, I studied medicine in India. There you had to fall back on your intuition and very simple tests in order to make a diagnosis. And if that didn't work, we just had to come up with something."

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The amount of money that we should spend on science, how we distribute that money and how we should spend money are all different and independent arguments. Consider the case of a country like India. Given our population and our proven ability to educate and train large numbers of scientists and the impressive track record of hundreds of our scientists, I think it is not unreasonable for us to aim for a 10-fold increase in our scientific output, say in the next decade. Needless to say, the chances of getting a 10-fold increase in funding is, of course, zero. However, I believe that a doubling of the budget and a five-fold increase in the overall efficiency of science per Rupee spent are well within the realms of possibility.

Barriers to Low-cost Research and How to Overcome Them

Apart from the relatively harmless casual and verbal objections to low-cost research that are usually made as counterpoints during a discussion which I have referred to above, I am afraid there are real and severe barriers to pursuing low-cost research. I will briefly mention three and add three partial remedies.

Apart from the relatively harmless casual and verbal objections to low-cost research that are usually made as counterpoints during a discussion, there are real and severe barriers to pursuing low-cost research.

The first barrier is the universally witnessed sentiment, "money is power". Money is indeed power for scientists too. It is not merely the psychological feeling or illusion of power, but grant money brings real power because of the way academia is organised. In many institutions around the world, scientists with large grants are allowed to buy themselves out of many duties, especially teaching and administration, both of which take a great deal of time, but are also part of being responsible members of the academic community. In some extreme cases, this means that a Professor is on the rolls of an institution in name only. Not surprisingly, the temptation to have large grants, even if they are not really necessary for the best research outcome, is great.

Secondly, money brings prestige. And not just to the social standing of the winners of large grants but more detrimentally, also to their research. According prestige to research proposals in direct proportion to the quantum of money requested begins with the receipt of the application at the grants offices. Small grants are sometimes much more casually dismissed, and very large requests result in a team of experts making a site visit to the applicant's institution and laboratory. Another kind of problem is the perception (justified in some cases) that you get a certain fixed fraction of the money you ask, so that the more you ask, the more you get. I find it amazing that most applicants accept even very substantial cuts in the money asked for and neither complain nor report having to drastically alter their research plans, let alone decline the grant. Something is clearly wrong!

The third barrier to low-cost research comes from the practice in many institutions (thankfully not so common in India) to depend rather heavily on the overheads that extramural grants bring, to run the institutions, including paying salaries and constructing buildings. This practice incentivises getting large grants as its most benign effect and makes it impossible to do low-cost research as its most deleterious effect. Selection for faculty who will bring in large grants begins at the hiring stage and continues through cycles of assessment and promotion, not to mention awards and accolades. As we saw earlier, this results in the uneven growth of different areas of science and the neglect of important areas, their only crime being that they don't need large grant money. Surely, institutions should not be built on a business model that depends heavily on overhead grants from extramural funding for research.

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Unfortunately, even institutions that are not built on this pernicious business model have made it a social norm to accord unnecessary prestige to faculty who bring in large grants. It is quite the norm to prominently display the list of grants earned on our CVs, often ahead of the list of our publications. All this is surely a great disservice to science and should be done away forthwith. At the very least, scientists and their work should be evaluated irrespective of the grants brought in. Ideally, the money spent on research should be in the denominator of the performance index. I have never heard a good argument for why evaluation should not be in terms of research output per Rupee or dollar spent. Perhaps it will be a bit unfair to those who pursue expensive research, but that is better than penalising those who do inexpensive research. Moreover, I think it is not unreasonable to put some pressure on those who spend a great deal of someone else's money, public or private, to perform well. Perhaps we should rename 'grants' as 'loans', to be repaid with commensurate scientific knowledge. Sometimes when I speak about doing good science with less money, I am amused to see my interlocutors deflect the argument and say let's just talk about how to do good science, why bring money into the picture? This is quite absurd. Doing good science with less money requires many more skills, and often more imagination and creativity, than just doing good science.

We can do even more to promote low-cost research. Some years ago, I was invited by the students of a prestigious research institute to speak about my life in science. The students with expected creativity had christened the series of talks The Life of PI, a take on the popular novel by that name by Yann Martel [12], except that PI was meant to be 'Principal Investigator'. I began by praising their creative title for the lecture series and spent the next 15 minutes telling them why I hated their title, or more precisely why I hated the title, Principal Investigator. PI was unheard of when I was a graduate student or even when I was an early career scientist. It seems to have been invented in the last two or three decades and has gone to fixation driving to extinction all rivals including professor, scientist, mentor, faculty member, etc. As far as I know, PI was invented by granting agencies to know who is to be held responsible for the grant to be well spent. There is nothing more principal about the PI. People now ask me how many PIs are there in your Department and I say all 100.

Why should it be a foregone conclusion that the one who gets the grant is the principal contributor to the science that is being done? Is it not possible that a junior colleague, a student or even a technician plays the principal role in the research being done? Perhaps different actors may play principal roles in different parts of the research. I think it is a mistake to decree a fixed and predetermined hierarchy in a research environment. Science is meant to be non-hierarchical, and we are unnecessarily creating a mindset and further empowering the already powerful. A lowly student at the bottom of the power hierarchy is further frightened into submission and told in no uncertain terms as to who is the boss. How can we expect students to question and challenge the PI, or do we not want them to do so? Besides, any good set of ethical guidelines will discourage grant of authorship to those who

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simply provide the money for the study. Thankfully my PhD and post-doc mentors were not called PI in those days, or I would have been deeply offended. In neither case did my mentors, wonderful as they were, play a principal part in my research. I find it pompous enough to be called group leader, but PI? Never. I feel honoured to be a mentor to my students and friend to my colleagues, and willing to be the PI responsible only for my funders and their financial auditors.

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My quarrel is not just with 'PI' but with 'grant' itself. I am not saying that there should be no grants; all I am saying is we should provide space for 'grant-free' research; indeed, we should encourage grant-free research. Instead, it seems to go without saying that research begins with applying for a grant. How to choose your area of work, how to ask the right questions, how to design an experiment, how to collect data—all such questions seem to have been relegated to a lower priority. I have attended far too many workshops designed to mentor early-career scientists where 'how to get big grants' was the question of paramount importance for both the mentors and the mentees. It seems to be the assumption that getting a big grant guarantees excellent research. In such workshops, I would instead like to see a discussion of all the cool research we can do in the grant-free mode.

A Personal Note

In my experience, a discussion of high-cost versus low-cost research often boils down to a discussion of molecular biology versus animal behaviour, ecology and evolution. But of course, this need not be the case. Such debate can involve almost any area of science, be it physics, chemistry or biology and also in virtually any areas of biology, including expensive versus inexpensive ecology and animal behaviour or even high-cost versus low-cost molecular biology. But there is a good reason why the discussion often boils down to molecular biology versus behaviour and ecology, in my case. This is because molecular biology and animal behaviour are the fields I am trained in, and the two areas closest to my heart. I will, therefore, end this article and indeed, this series on a personal note, reproduced here from [15].

"As an undergraduate, I read voraciously and indiscriminately, partly because there was little else to do. Of all that I read, two books completely blew my mind. One was *The Double Helix* [13] by the Nobel Laureate James D. Watson. This book was inspiring at many levels and instantly made me a life-long addict of molecular biology. I subsequently read every book and research paper in the field of molecular biology that I could lay my hands on. The discovery of DNA, its demonstration as the hereditary material, the elucidation of the double-helical structure of DNA, the proposal and subsequent proof of semi-conservative replication, the unravelling of the steps in the synthesis of proteins and the study of bacteria, bacteriophages and plasmids were all like an epic play being played out in the theatre of heaven where Gods like Watson and Crick, Luria and Delbruck, Messelson and Stahl, Ochoa and Kornberg, Nirenberg and Khorana lived and continuously scripted, directed and enacted various acts and scenes. And these ever novel and mesmerising scenes in the play came to me almost daily, in the form of research papers in various journals. The feeling that I was a lowly earthly being watching an epic play in heaven with awe and respect was enhanced by the fact that these topics were not part of our [study] curriculum.

But I also read well beyond molecular biology. The other book that I can easily single out for having made a life-long impact on me was *King Solomon's Ring* (1952) by Konrad Lorenz [14], not yet a Nobel laureate but soon to become one, at the time I read him. The study of animal behaviour [on the other hand], was a complete contrast to the epic molecular play in heaven. It was an earthly matter. Charles Darwin, Konrad Lorenz, Niko Tinbergen, Karl von Frisch, Oskar Heinroth, Douglas Spalding, Jocob Von Uexküll, Ivan Pavlov, Desmond Morris were all earthly beings close to me and I admired them in a wholly different kind of way—not in awe but as a fellow compatriot. The reason for this was that they all did what I felt I could also do quite easily, at least in principle.

As an undergraduate student trapped in an environment without access to any well-equipped research laboratories, I perceived a massive, insurmountable technological chasm between molecular biology and me, and hence molecular biology was a play being enacted in heaven. Ethology, the study of animal behaviour on the other hand, was well within my capacity to pursue.

There was no reason for me to feel jealous of Watson and Crick for having discovered the structure of DNA—it was not something I could have done anyway. But I did feel a tinge of jealousy that it was Konrad Lorenz and not I who had discovered imprinting in birds, that it was Karl von Frisch rather than I who deciphered the honey bee dance language, that it was Douglas Spalding and not I that had put little hoods on new-born chicks and showed that their pecking behaviour was instinctive, that it was Niko Tinbergen and not I that had placed a ring of pine cones around the nest of wasps and discovered that the wasps use landmarks to locate their nests".

"At the end of my PhD, I was in a serious dilemma, being equally in love with both Molecular Biology and Animal Behaviour. The difficulty, or should I say impossibility, of doing cutting-edge research in molecular biology under Indian conditions, was brought home painfully to me every day of my PhD. If I were to continue with molecular biology, it would have to be in the USA or some such developed country. But if I could swap animal behaviour into my profession and molecular biology into a hobby, then, of course, I could stay in India and spend the rest of my life doing low-cost research on the Indian paper wasp *R. marginata*. I chose the latter option...and I have never regretted my decision" [16].

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