25 Jahre Wissenschaftskolleg zu Berlin

1981–2006

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Raghavendra Gadagkar

The Evolution of a Biologist in an Interdisciplinary Environment

I had long known and admired Rüdiger Wehner as one who has made some of the most fundamental and fascinating discoveries about the navigational abilities of honeybees and ants. There was always, however, a somewhat mysterious side to Rüdiger – he was somehow associated with a certain Institute for Advanced Study in Berlin, which, he often told me, brought together scholars of all disciplines from biology to music! He often suggested that I spend a year at this institute. What would I do among social scientists, humanists, artists and musicians? More importantly, how could I leave my laboratory, my students and my wasps for a whole year? I usually smiled politely and did not take the invitation seriously. But Rüdiger patiently persisted, as he does so successfully with his experiments on desert ants in Africa. Finally I gathered the courage to spend five months at the Wissenschaftskolleg – after I was promised a break of several months in the middle of the five-month period, when I could go back to Bangalore and reunite with my wasps and my students. Thus began my evolution into a very different kind of biologist, and I have now spent over 20 months during the past five years at the Kolleg. My experience has been remarkable, unforgettable and irreversible. I will here recall my association with this unique institution and attempt to describe its influence on my professional life.

Two Cultures at the Wissenschaftskolleg

During the first year of my stay, my life at the Wissenschaftskolleg was rich and varied and full of new and wonderful experiences. Gradually I began to learn how not to miss my wasps and students too much and indeed to appreciate the rare opportunity to get away from my routine duties and responsibilities back home and to be able to read and write unhurriedly. And like other Fellows, I found the staff very efficient, helpful and warm and the facilities and services exemplary. The many and varied discussions we had during the Tuesday colloquia, the Thursday dinners and the weekday lunches will remain vivid in my memory for a long time to come. The unusually large number of Indian scholars at the Kolleg during that year added its own unique flavour. During part of the time, I had the inspiring company of my group members, Amitabh Joshi, Leticia Avilés and Somdatta Sinha. Berlin, I soon discovered, harbours an unending supply of rich and exotic (at least for me) cultural feasts. My life at the Kolleg was thus both productive and enjoyable as I pursued my planned project and an unexpected hobby.

My Project

When I reached the Kolleg, I had written a draft of a monograph describing some twenty years of research that I and my students have pursued, in studying the evolution of social life in insects, using the Indian primitively eusocial wasp *Ropalidia marginata* as a model system. I put my new-found peace and quiet and the excellent library facilities to good use in revising and finalizing it. The book has since been published (Raghavendra Gadagkar: 'The Social Biology of Ropalidia Marginata – Toward Understanding the Evolution of Eusociality.' Harvard University Press, Cambridge, Massachusetts, USA, 2001).

My Hobby

Since I was a PhD student and, indeed, throughout my professional career, I have had the great privilege of working at what is arguably India's most prestigious research institute, the Indian Institute of Science in Bangalore. As India's oldest and largest post-graduate science university, it has given me nearly complete professional satisfaction. I say nearly complete because, wonderful as it is, the Indian Institute of Science lacks something very important – it has no representation of the social sciences, humanities, arts and literature. I have always felt this to be a very serious drawback and often felt disappointed that many of my colleagues do not seem to share my point of view. The opportunity to interact with colleagues who study economics, political science, psychology, sociology, history, law, philosophy, religion and music at the Wissenschaftskolleg was even more interesting than I had imagined. The reason for this was that I became even more interested in how these 'strange' colleagues pursued their craft than in what they actually did.

As time went by, observing, contrasting and trying to understand the behaviour of social scientists (for convenience, I am including everybody other than natural scientists under the label 'social scientists') and natural scientists became an obsession with me. The formal Tuesday colloquia gave me a great opportunity to study the behaviour of my colleagues. I often found myself paying more attention to the manners and methods of the speakers than to the contents of their message. I am an ethologist and observing animals is my profession. What I did with wasps in Bangalore, I continued to do with my co-Fellows at the Kolleg in Berlin. The wasps I study in Bangalore are most fascinating, but my co-Fellows in Berlin did not disappoint me either. Fortunately, there were also a sufficient number of natural scientists, thus making it possible for me to attempt a comparative study. In the short time available to me (relative to the time I have spent observing wasps), I made many interesting observations. As in my observations with the wasps, only such observations are worth reporting that can be organized systematically and explained. Hence I restrict myself to the behaviour of my co-Fellows during the Tuesday colloquia. I made observations during every colloquium I attended and used the method that we call focal animal sampling. As you can imagine, my focal animal was always the speaker.

Among the many interesting contrasts I discovered between the social scientists and natural scientists, I report the three contrasts given below, as they were the clearest, i.e., there was no need for statistical analysis.

1. The Sit-Stand Dichotomy: All social scientists in my sample sat while they presented their colloquia, while all natural scientists did so standing.
2. The Read-Speak Dichotomy: All social scientists in my sample read out their presentations from a prepared text, while all natural scientists spoke extempore.
3. Quote-Unquote Syndrome: All social scientists used numerous quotations from other scholars to make their points, and only one natural scientist used only two quotations.

Fascinating as such contrasts are, they hold little interest to the modern ethologist unless we can at least begin to 'explain' and 'understand' the reasons for their existence. And that is what I attempted to do, with limited success, during my daily walks on Koenigsallee or Kurfürstendamm. Success was limited because I did not have the opportunity to conduct manipulative, experimental investigations, as I am so used to doing with my wasps! I am therefore obliged to propose the following explanations merely as hypotheses awaiting verification. In my branch of evolutionary biology, sometimes called behavioural ecology, we are often faced with a similar task of explaining why animals behave the way they do. In their attempts to explain a variety of patterns of animal behaviour, behavioural ecologists have discovered three kinds of explanations. These are (1) random genetic drift, (2) natural selection and (3) phylogenetic constraints. Some behaviour patterns are neither particularly beneficial nor particularly detrimental and therefore they are neither lost nor do they eliminate the alternative and go to fixation. The laws of statistics govern the dynamics of their spread and persistence. This phenomenon is called random genetic drift, or simply drift. Other behaviours are maintained (do not disappear) because they are significantly beneficial to the actors and are preferentially preserved relative to alternative behaviour patterns. This is called natural selection, or simply selection. Yet other behaviour patterns exist because of historical reasons; changing them is not easy, perhaps too expensive. This explanation is called a phylogenetic constraint, or simply phylogeny or history. The important point is that these three explanations are not necessarily mutually exclusive; one or more of them may be involved in maintaining a certain observed behaviour pattern. Can we attempt to attribute the contrasts between the behaviour of social scientists and natural scientists to any of these processes?

The Sit-Stand Dichotomy

Why do social scientists sit and natural scientists stand while making their presentations? My hypothesis is that these different behaviour patterns are maintained by drift and history, but not by selection. Neither behaviour pattern is significantly more or less effective in producing a successful presentation. Clear evidence of this was obtained because there were several memorable presentations, both by sitting social scientists and by standing natural scientists. That a historical constraint is also involved became obvious when I asked one of the social scientists why she and her colleagues prefer to sit while making their presentations. She said her audience would surely consider her horribly pompous if she stood up to make her presentation; she found the idea unthinkable, but if she had no choice but to stand while reading her paper, for example if there was no chair available, then she would certainly begin with an apology and an explanation. Natural scientists surely have a contrasting opinion. Only once in my career have I been forced to give my talk sitting. I was running a high fever and my hosts, who had flown me several hundred kilometres, could not have reimbursed my air fare if I did not give my talk! Therefore I had no choice but to give my talk. However, there was no way I could have stood up for an hour. I asked for a chair and gave my talk sitting, feeling most uncomfortable and, yes, pompous! Of course, I began with an apology and an explanation!

The Read-Speak Dichotomy

Why do social scientists read from a prepared text and natural scientists speak extempore? Here I think selection is the important explanation, neither drift nor history. Reading from a prepared text and speaking extempore are far from being equally effective. But if one is more effective than the other, why does the ineffective one not disappear? Behavioural ecologists often face a similar situation and are very familiar with behavioural polymorphism. The reason why the polymorphism is maintained is that while one behaviour pattern is effective for some individuals, a different pattern is effective for others. This may have to do with differences between the two kinds of animals – differences in body size, state of health, location in own or foreign territory, access to information, etc. The two kinds of individuals, with their inherent differences, are equally fit when they adopt their respective behaviour patterns. Thus natural selection cannot discriminate between the two behaviour patterns and eliminate any one; hence the polymorphism is stable.

I think there is a similar situation among reading social scientists and speaking natural scientists. Speaking extempore is surely a more effective way of communicating with the audience, of holding their attention and of responding to their body language. Reading from a prepared text is hardly as good for these purposes, but it has the great virtue of making it possible, even easy, to be precise and predictable in what one says, to exercise great care in one's language, choice of words, grammar and style. I think speaking and reading have contrasting properties and are each useful in different contexts. I would argue that what a natural scientist says is often more important than how he says it. In contrast, how a social scientist says what she says is often at least as important as what she says. This difference is of course only relative. Even within the natural sciences, one often encounters such differences. My favourite example is the contrast between a synthetic chemist, for whom content is far more important than style of presentation, and an evolutionary biologist, for whom style of presentation is at least as important as content. Although my own subject lies closer to the social sciences in this regard, I think there is a general dichotomy between the social and natural sciences. The results of a (natural) scientific experiment can be communicated in pretty much the same way by many different individuals. Historical or sociological analyses, on the other hand, often have a unique imprint of the author and would hardly be the same if presented (orally or in writing) by someone else. I therefore suspect that natural scientists often sacrifice choice of precise words, style and other nuances of language for the opportunity to communicate more directly with their audience. On the other hand, social scientists prefer to forgo that opportunity in order to pay greater attention to the precise language and style of their presentation. I hypothesize that it is these differing needs of the two cultures and the unique suitability of speaking and reading for their respective needs that maintains this behavioural polymorphism.

Quote-Unquote Syndrome

Social scientists' love of quotations and the natural scientists' rare use of them is perhaps the most interesting of the three differences. Stated somewhat strongly, I think that, for a social scientist, the ultimate happiness is to find a quotation in the existing literature that says exactly what she wants to say – and the older the source of the quote, the better. A natural scientist would be devastated if he found that somebody had already said what he wants to say – and, the older the quote, the worse it would be. Here also I would propose selection as the mechanism that maintains this behavioural polymorphism, but the selective pressures that maintain this polymorphism are bound to be quite different. Natural scientists place a great premium on novelty. They 'discover' and 'invent,' and you don't discover and invent the same thing repeatedly. The validity of the discovery or invention depends of course on its repeatability, but validity is not a sufficient criterion for publication, for instance. A paper is often rejected on the grounds that the same phenomenon has already been described in another organism. I would also argue that, relatively speaking, natural scientists often have (or at least they think they have) more 'objective' criteria for validating their claims – 'many others also think so' or that 'such and such a famous person thinks so' is not usually necessary and often not good enough. Relatively speaking, such apparently 'objective' criteria are not always available for many arguments in the social sciences and humanities, and their practitioners seem to recognize this. Here validity depends, at least to some extent, on how many people and which people also think so. If my understanding of these differences on the value of novelty and the criteria for validity are even partly correct, they could explain the propensity of social scientists for using quotations and the relative lack of it among natural scientists. Of course these ideas are mere hypotheses in need of verification and even as hypotheses they are very incompletely developed.

The Kolleg – A Veritable Incubator for Competent Radicals

Having been able to spend only five months during the previous year, I was kindly invited as Guest of the Rektor for another five months during 2001–02. This year was as exciting as the previous one – my excitement was not numbed by familiarity with the Wissenschaftskolleg. Being in a unique position of having spent two consecutive years at the Kolleg, it was tempting to make a comparison between the two classes. Indeed I could not help attempting comparisons throughout my stay. The class of 2001–2002 was an equally interesting but quite a different assemblage of species. During this year's stay, I was surprised but delighted to be told that I was being considered for an appointment as a non-resident Permanent Fellow at the Wissenschaftskolleg. I spent much time this year thinking about the true function of an institution like the Wissenschaftskolleg. Here is the answer I came up with:

Creative intellectual activity is a complicated business. It is necessary to be both correct and creative. The relevance and importance of being correct, i.e., of conforming to some accepted standard, diminishes as we move from the natural sciences to the social sciences, humanities, literature and finally the arts. Inevitably, one's ability to be original and creative falls rapidly as we move in the opposite direction from the arts to literature, humanities, social sciences and finally the natural sciences. In the natural sciences in particular, there are strong forces that prevent you from being original or creative, and rightly so, because what is original and creative can often be wrong. The publication and acceptance of almost anything in the natural sciences is based on peer review and acceptance. This has the function of ensuring that not too many falsehoods are perpetuated in the name of science. But at the same time, this often curbs radical departures from widely accepted positions. There is no simple way to censor the vast majority of original and creative ideas that are wrong and accept only those that happen to also be correct. It is typical for a reviewer to reject anything out of the ordinary and typical for most of us to accept peer judgment and fall in line with the accepted position. But of course there are occasional exceptions. And it is these exceptional individuals who make the transition between what Thomas Kuhn has called "normal science" and "scientific revolution."

My favourite example is that of Amotz Zahavi of the Tel Aviv University in Israel and his handicap principle. Biologists since Darwin have wondered why the peacock has such an elaborate tail that must surely be a handicap to him while running away from predators. The commonly accepted explanation (attributed to Ronald Fisher, one of the architects of the genetic theory of evolution), is that in the past there must have been a positive correlation between tail length and male quality, and females must therefore have been shaped by natural selection to favour males with long tails. With simultaneous selection on males for having long tails and on females for preferring males with long tails, it has been suggested that, through a process of runaway selection, male tails can get longer than is good for their own survival. This is because even when the positive correlation between tail length and male fitness disappears, females who mate with long-tailed males will have sons with long tails who will in turn be preferred by females of the next generation. Indeed there are several mathematical models that show that such a runaway selection can produce tails that are longer than are good for the males' survival. Zahavi refused to accept this explanation because, to paraphrase his words in a lecture he gave at the Indian Institute of Science, "first we have to assume that females are so clever that they 'know' that long tailed males are fitter and then we have to assume that later females become so stupid that they do not realize that long-tailed males are no longer fit because their tails have grown too long!" In the 1970s, Zahavi wrote a series of now famous papers in which he made the radical suggestion that the peacock's long tail is selected precisely because it is a handicap, not in spite of being a handicap. By carrying around such a handicap of a tail and by not yet having succumbed to a predator, the peacock reliably demonstrates to females that he is indeed fit enough to survive despite the handicap. Zahavi derived from this idea a far-reaching general principle that animal signals in general must impose a cost, a handicap, on the signaller in order to be reliable and thus resistant to faking. The scientific community rejected Zahavi's ideas outright. Several distinguished theoretical evolutionary biologists wrote mathematically sophisticated papers arguing that the handicap principle cannot work. One paper was actually entitled 'The handicap mechanism of sexual selection does not work' (American Naturalist, 127, 222–240, 1986).

Then everything changed in 1990 when Oxford evolutionary biologist Alan Grafen published two papers showing, with the aid of more economically inspired mathematical models, that Zahavi's handicap principle can indeed work, both in the evolution of honest signals in general and in the context of sexual selection. Today Zahavi's handicap idea and the more general, costly, honest signal idea are widely accepted and have considerably altered the way we model and study animal communication and behavioural evolution. The well-known evolutionary biologist John Maynard Smith has graciously admitted publicly that he was wrong in hastily concluding that Zahavi's idea was an error. But of course Maynard Smith says it in his inimitable style: "I was cynical about the idea when I first heard it, essentially because it was expressed in words rather than in a mathematical model. This may seem an odd reason, but I remain convinced that formal models are better than verbal ones, because they force the theorist to say precisely what he means. However, in this case my cynicism was unjustified. It has proved possible to formulate mathematical models showing that what Zahavi called the 'handicap principle' can lead to the evolution of honest signals." (The Times Literary Supplement, 3rd August 2001). I must confess to a certain degree of unhappiness that many people today accept and use Zahavi's handicap principle but call it (disguise it?) as the "good genes model."

More recently, Amotz Zahavi, along with his wife Avishag, has written a book-length essay on the widespread ramifications of the handicap principle. In a most remarkably bold style, they explain more or less the whole world with their handicap principle – why does a gazelle jump up and down at the approach of a predator, wasting time and energy and making itself visible, why do skylarks sing while fleeing from predatory merlins, why do pelicans in the breeding season grow a bump between their eyes that interferes with their ability to fish, what is the function of the small horn of the rhino, why do animals groom each other, why do host birds not reject the eggs of brood parasites, why has homosexuality evolved, why do animal and human infants beg food so noisily that they attract predators, why was the use of lace by humans so popular among the wealthy in the past and why is it not so today, why do we shout while issuing a threat to someone standing nearby, why do men grow beards and wear bow ties, why do people attempt suicide … their list is endless!

This enterprise of attempting to explain everything with the handicap principle will surely fail at some point, but we will never know exactly where it will fail unless someone pushes it past the precipice and, very likely, falls along with. I think we should be grateful to the Zahavis for altruistically doing this for us. But not everybody thinks so; the peer review system is harsh. The Zahavis' book has been roundly criticized – one reviewer has called it "a work of advocacy" rather than of science and another has almost dismissed it with the statement: "The lack of data does not seem to dampen the Zahavis' enthusiasm – Who needs data when metaphors abound?" (Q. Rev. Biol. 73, 477–479, 1998). I will come back to this, but first permit me to cite one more example, also very dear to my heart.

In the 1940s and 1950s, Karl von Frisch discovered that successful honeybee foragers return to their nest and perform dances, by means of which they are able to communicate to their sisters the distance and direction to the source of food they have discovered. What makes this unique among many examples of communication in animals is that bees appear to use a system of arbitrary conventions, hence a form of language, to communicate with each other. Von Frisch's dance language hypothesis has since been verified by hundreds of independent researchers and has now become an extraordinarily powerful experimental paradigm for studies of animal communication and sensory physiology. Karl von Frisch shared the 1973 Nobel Prize for his discovery with two other ethologists, a rare occasion on which the Swedish Academy had the courage to correct Nobel's anomalous use of the phrase physiology or medicine rather than biology for one of the prizes in his name. But Adrian Wenner of the University of California at Santa Barbara refused to believe the dance language hypothesis. Since the mid-1960s, Wenner has been conducting experiments that in his view disprove von Frisch's dance language hypothesis and support the alternative olfaction hypothesis, which states that honeybees use only odours to locate food. Many researchers starting with von Frisch have periodically attempted to answer Wenner's criticisms but the latter remains unconvinced. What I find most fascinating in the history of this controversy is that successive supporters of the dance language hypothesis praise Wenner and Wells for generating a controversy and forcing them to do better experiments, but in the end they conclude that the dance-language hypothesis holds. But Wenner and Wells continue to remain unconvinced.

Wenner sticks to his position with conviction, and in 1990, along with Patrick Wells, he wrote a book-length argument entitled 'The Anatomy of a Controversy' (Columbia University Press) saying: "After presenting the reasons for our disillusionment with the dance language hypothesis, we cover in the next three chapters various personal encounters as they relate to the sociology, psychology, and philosophy of science." More recently, Michael Polakoff reported his experiments in an article entitled 'Dancing Bees and the Language Controversy' (Integrative Biology 1,187–194, 1998), in which he claims to have "avoid[ed] many of the pitfalls of previous dance language experiments." Praising Wenner's odour-search hypothesis as "a valid and more parsimonious alternative to the flashier and more seductive dance language hypothesis" that "did not receive a warm welcome despite the compelling data," he goes on to conclude, however, that his new results "suggest that odor alone is unable to account for the behavior of the bees recruited by waggle dances" and therefore that "recruits are indeed learning the direction of a food source when they follow dances, as von Frisch asserted 50 years ago."

I cannot imagine Zahavi accepting the failure of his handicap principle or admitting that signals need not necessarily be costly to be reliable, any more than I can imagine Wenner accepting the honeybee dance language hypothesis of von Frisch. Is this unfortunate? Actually, I think not. In my view, scientists like Zahavi and Wenner, by sticking to their extreme positions, by refusing to compromise, are doing the scientific community a favour. There is no great harm if individual scientists have their pet biases and prejudices and therefore pursue their pet hypotheses to the extreme. It is only important for the community as a whole to be objective. One way for the scientific community to be objective and get at the truth is to train all practitioners of the scientific profession to be objective, a task that I think is impossible. Indeed I think that it is neither necessary nor possible to train all scientists to be totally objective and to pursue truth totally objectively. Not necessary because, if there are enough radical scientists embracing diverse radical opinions and pursuing their pet hypothesis in different directions, the community can average over these extremes and remain objective. I tend to think of people like Zahavi and Wenner as altruists who uncompromisingly embrace radical positions and are not even persuaded by data contradicting their positions, who put their own reputations at stake and thereby let the community discover how far each hypothesis can be stretched. Without people like Zahavi, we will never know how much of the world we can explain with the handicap principle and without people like Wenner, we would not have seen the kind of clever and sophisticated experiments about the bee dance language that his criticisms have engendered. Of course, Zahavi's handicap principle will fail at some point and Wenner may be proved wrong in the end. But we benefit from them and their uncompromising courage to pursue their points of view.

But aren't scientists supposed to be objective and have an open mind in testing hypotheses and accepting conclusions? Well, I don't think so and therefore I think that is not possible to train all scientists to be totally objective. The reason for this has never been expressed more clearly than by Richard Lewontin in his masterly 'The Genetic Basis of Evolutionary Change' (Columbia University Press, 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 changes 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."

So why not let different scientists pursue their prejudices and see how far they can go? I would like to see the scientific community be more tolerant of such radical scientists. But of course if everybody is allowed to be a radical, there will surely be chaos. What we need are impeccably competent radicals. We should set our thresholds very high and demand the highest possible level of competence before we become tolerant of radical scientists pursuing their radical positions. For the rest of us there is always the harsh peer review system! Such differential treatment of the more and less competent is not easy to institutionalise. It has to be done in a subtle and inoffensive manner. The influence of peers that serves to cull out unfashionable points of view operates not merely during publication. It operates inexorably and invisibly at all times, in formal seminars, in informal discussions, at the coffee table … This is where an institution like the Wissenschaftskolleg plays such an important role. The Kolleg identifies 40 of the most accomplished and creative scholars from around the world and puts them together in very agreeable living and working conditions. For completely different reasons, the Kolleg attempts to give fair representation in each year's class of 40 Fellows to as many different disciplines of scholarly activity as possible. The useful but unintended consequence of this is that it also ensures that each scholar has few or no peers to trim away shoots of thought sprouting outside the narrow radius of acceptability. In fact the opportunity to present one's work and ideas to scholars from completely different backgrounds and training often forces each scholar to go beyond the turf that she would normally restrict herself to during conversation with insiders. I know of no better method of fostering unhindered creativity.

Some Reflections on the Pursuit and Evaluation of Science

Perhaps the most important influence that life at the Wissenschaftskolleg has had on me is that I have now begun to reflect on the science I do and on the way in which I do it. I am grateful to the Kolleg for this habit, because practicing scientists are usually very busy practicing their craft and most of us also devote a significant amount of time to evaluating what our peers do. However, we seldom find the time or have the inclination to reflect on how we pursue our craft and here by craft I mean both the craft of doing science and the craft of evaluating science. If there is any reflection at all about the methods of pursuing and evaluating science, it is done almost entirely by a separate group of 'outsiders' who belong to the disciplines we label history, philosophy or sociology of science and, more recently, science studies. It is not uncommon for practicing scientists to disregard what these 'outsiders' have to say about the pursuit and evaluation of science. Steven Weinberg said famously that the history and philosophy of science are about as useful to scientists as ornithology is to birds! This is funny, but probably somewhat true and if so, rather sad. The only way to make ornithology useful to birds is for birds also to practice ornithology. Hence, scientists must also themselves reflect on their methods of pursuing and evaluating science.

I believe that this is possible in any long-term, stable manner only if we formalise such reflection and make the teaching of such reflection an integral part of science education at the undergraduate, post-graduate and especially at the doctoral levels. I have always found it most remarkable that by merely teaching our students how to operate some instruments or solve some equations, we expect them to master the arts of choosing a scientific problem, solving it, communicating their findings to specialist and general audiences and acting as peer reviewers for other people's attempts to do the same. A little reflection will show that our science education imparts none of these skills.

Choosing a Scientific Problem

The first step in one's scientific career is choosing a scientific problem for investigation. If generation of significant new knowledge is the goal, it seems reasonable to expect that scientists would look for areas of ignorance, areas that have been overlooked or forgotten by others. We all know, however, that this is not how topics are chosen for study. Indeed, exactly the opposite seems to be done. People look for fashionable areas, topics that are of interest to many and themes that are easily accepted for publication in prestigious journals.

Here I wish to emphasize that those of us in the developing world face a special problem that is largely of our own making. If the scientific community was relatively homogenous with a level playing field, this might not be fatal because we could always argue that the smartest scientists will set new fashions and bring about 'scientific revolutions,' while the rest will continue to do 'normal science.' However, we live in the real world, compartmentalized into developed and developing countries with associated scientific communities with very uneven playing fields. Left to market forces, it is inevitable that a disproportionate number of revolutions will originate in the better-endowed scientific communities in the developed countries, while those in developing countries will be almost permanently relegated to doing 'normal science.'

However I am convinced that there are significant opportunities for the simultaneous development of uniquely different perspectives from different parts of the world, especially in biology and of course even more so in the social sciences and humanities. But our own institutionalised scientific structures ensure that any prospect of development of a new and different perspective from our parts of the world is nipped in the bud. We reward scientists who work in fashionable areas, we reward those who publish in prestigious Western journals, we have no time and patience to read their work and judge for ourselves, we reward those of our scientists who are applauded by the

West, we have no self-confidence to make our own independent judgements of the accomplishments of our scientists. In short, we create, nurture and reward followers rather than innovators. I am not surprised that this suits the developed world, but I am surprised that it seems to suit the developing world as well! The net result of all this is that science loses prestige as a career and our bright young people turn to other professions.

The Birth of the Centre for Contemporary Studies

Important as it was, teaching me to reflect has not been the only consequence of my association with the Wissenschaftskolleg. I am pleased to say that now I am in the process of creating a miniature Wissenschaftskolleg at the Indian Institute of Science. As the founding chair of the newly created Centre for Contemporary Studies, Indian Institute of Science, Bangalore, it is now within my capacity to correct the one shortcoming of the Institute that I began this essay with. By organizing a series of seminars, lectures and discussions and by maintaining a steady stream of visiting scholars at the Centre for Contemporary Studies (CCS), I have begun to provide opportunities for the scientific community in the Institute to experience a sample of the best scholarship and creativity outside the traditional boundaries of natural science. During the two years of the existence of CCS I have brought anthropologists, historians, literary critics, philosophers, lawyers, sociologists, broadcasters, journalists, archivists, librarians, filmmakers and film critics to the campus and created opportunities for dialogues with practicing natural scientists and budding students. I cannot entirely predict what the consequence of this will be in the long run. But it is my hope and belief that such cross-disciplinary interaction will make natural scientists more tolerant of radical ideas and more capable of reflecting on the methods they use in the pursuit and evaluation of science. And the students who are exposed to such an influence will, I suspect, mature into scientists less hesitant than I was in the beginning in accepting invitations for a place like the Wissenschaftskolleg zu Berlin.