

Raghavendra Gadagkar

Two Cultures

at the Wissenschaftskolleg



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My life at the Wissenschaftskolleg was so rich and varied and so full of new and wonderful experiences that it is impossible to reminisce about all of them in this short essay. Like most other Fellows I am sure, I greatly appreciated 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. I must say that by hiring talented and committed individuals and giving them the opportunity to interact with so many different Fellows, year after year, the Kolleg has produced a human resource that is probably unparalleled. The many and varied discussions we had dur-

ing 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 numbers of Indian scholars at the Kolleg this year added its own unique flavour. For part of the time, I had the inspiring company of my group members, Amitabh Joshi, Leticia Avilés, and Somdatta Sinha. Berlin, I soon discovered, harbors 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 three planned projects and an unexpected hobby. Here I will only briefly mention the projects that are being, or will be, published. I will describe the hobby in some detail as I suspect it might interest readers more than the projects.

My Projects

1. *The Social Biology of Ropalidia marginata*: This is 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 primitively eusocial tropical wasp *Ropalidia marginata* as a model system. The book was written before I came to Berlin, but I used my time here to revise and finalize it. As I write this, it has just been published (*The Social Biology of Ropalidia marginata – Toward Understanding the Evolution of Eusociality*. Cambridge, Mass.: Harvard University Press, 2001).

2. *Policing in Insect Societies*: In many insect societies, a conflict between queens and workers is expected over male production. Workers are not only more closely related to their own sons, they are also more closely related to other workers' (their sisters') sons than to the queen's sons. However, if queens mate with three or more males, the situation, changes dramatically – workers are now more closely related to the queen's sons than they would be to other workers' sons. In the latter situation little queen-worker conflict is predicted because, even though each worker would still benefit from producing her own sons, workers cannot agree on which one of them should do so. Thus they should prevent each other from reproducing and rear the queen's sons instead. This prediction has come to be known as worker policing and is supported by significant empirical evidence. I read all the theoretical and empirical work that has been done in this field during the past 15 years since the phenomenon was postulated and wrote a long, historical review. This project will be continued with my collaborator, Christian Peeters of the University of Paris.

3. *Reproductive Strategies*: My book, *Survival Strategies* (Cambridge, Mass.: Harvard University Press, 1997) attempted to explain to the non-specialist how cooperation and conflict are closely inter-linked in animal societies and how biologists attempt to explain why animals behave the way they do. The examples in this book were drawn from diverse animal species and were usually concerned with the struggle for survival. During my stay at the Kolleg, I began work on a sequel to this book, tentatively titled *Reproductive Strategies*. In this book I hope to show, once again to the non-specialist, how cooperation and conflict are finely balanced even among a mated pair of animals. Using examples from a variety of species, I hope to show how complex reproductive strategies are in animal societies and yet that there is an underlying logical theoretical framework that makes sense of this diversity.

My Hobby

Since I was a Ph.D. student and, indeed, throughout my professional career, I have had the great privilege of working at India's arguably most prestigious research institute, the Indian Institute of Science in Bangalore. As India's oldest and largest institute of science and as a post-graduate 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 and humanities. 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 a dream come true for me. This is what I think made my life at the Kolleg so interesting. The formal discussions after the Tuesday colloquia and the informal discussions at lunches and dinners were fascinating and inspiring, and sometimes they gave me a new perspective on my own work. In the first Tuesday colloquium, for example, Debora Shuger spoke on censorship in early modern England. At that time I was writing a paper on policing in social insects. The parallels between what I was studying and what she was speaking about were uncanny. I remember adding a section on “Equilibrium between policing and misbehaving in social insect colonies” after listening to Debora.

As time went by, observing, contrasting, and trying to understand the behavior 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. 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 do with wasps in Bangalore, I continued to do with my Co-Fellows at the Wissenschaftskolleg 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 worthy of reporting that can be organized systematically and explained. Hence I restrict myself to the behavior of my Co-Fellows during the Tuesday colloquia. I made observations during every colloquium I attended and used the method 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, since 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, but only one natural scientist used only two quotations.

Fascinating as such contrasts are, they hold little interest for 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 behavioral 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 behavior, behavioral ecologists have discovered three kinds of explanations. These are: (1) *random genetic drift*, (2) *natural selection*, and (3) *phylogenetic constraints*. Some behavior 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 behaviors are maintained (do not disappear) because they are significantly beneficial to the actors and are preferentially preserved relative to alternative behavior patterns. This is called natural selection, or simply *selection*. Yet other behavior patterns exist for 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 behavior pattern. Can we attempt to attribute the contrasts between the behavior 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 behavior patterns are maintained by drift and history, but not by selection. Neither behavior 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 as well as 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, if there was no chair available for example, 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 thousand kilometers, could not have reimbursed my airfare 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? Behavioral ecologists often face a similar situation and are very familiar with behavioral polymorphism. The reason why the polymorphism is maintained is that while one behavior 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 one's 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 behavior patterns. Thus natural selection cannot discriminate between the two behavior 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 favorite 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 behavioral 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 behavioral 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 "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 to use quotations and the relative lack of it among natural scientists.

As I have said before, these ideas are mere hypotheses in need of verification and, even as hypotheses, they are very incompletely developed. I need many more observations and a great deal more analysis. That is perhaps the reason why I have been invited again in 2001/2002!