Q & A

Bennett G. Galef

Bennett Galef is Emeritus Professor in the Department of Psychology, Neuroscience and Behaviour at McMaster University in Hamilton, Ontario, where he has been a member of faculty since 1968, the year he received his Ph.D. in comparative and physiological psychology from the University of Pennsylvania. His doctoral thesis on the role of stimulus novelty in eliciting aggressive behaviour of wild Norway rats was followed by decades of research on social influences on the development of food preferences of Norway rats and the mate choices of both male and female Japanese quail. He has also participated, in collaboration with his wife Mertice Clark, in many studies of the reproductive behaviours of Mongolian gerbils. He has been active in the development of the field of social learning from its earliest days, co-editing books and organizing conferences on the topic. As co-founder, and for many years co-organizer, of the Winter Animal Behavior Conferences he encouraged integration of psychological and biological approaches to study of animal behaviour. Since retiring in 2004, he has been engaged primarily in experiments using social learning of food preferences in Norway rats as an empirical system to examine predictions from formal models as to when animals should rely on socially acquired information and whose behaviour they should copy.

How did you become interested in the study of animal behaviour? I was a most unpromising graduate student, interested in neither the research that I was conducting on human concept formation nor the intellectual foundations of the field of cognitive psychology. I drifted aimlessly through my early graduate-school years without becoming the least engaged in the scientific enterprise. I was not greatly surprised when I managed not only to fail both my Master's and Ph.D. oral examinations but also to totally alienate my research supervisor. He was quite right to write me off. I was totally useless. Fortunately, while ostensibly studying for my comprehensive examinations in human cognition, reading material I found both dull and unconvincing, I happened to stumble on Niko Tinbergen's *Herring Gull's World* and *Curious Naturalists*. I found both absolutely fascinating. Midway through my third year of graduate school, I switched fields and advisors and have never looked back.

Under such circumstances, how did you get a career? During my final years of graduate school, I was far too busy completing my thesis research to do much reading or writing. So I went onto the job market with no publications and no real knowledge of my field. I particularly remember a job interview at Stony Brook, where the faculty tried, I'm afraid unsuccessfully, to find any area of animal behaviour about which I might converse knowledgeably. There was little to no chance that I would be successful in finding an academic appointment, so I was also looking in both the private sector and government for alternatives. I was, however, very lucky. Although the peak years of academic employment in the United States had passed, Canada was desperate to staff its many new universities, and I managed to land a job in the Psychology Department at McMaster University, then a small school with, it so happened, a world-class group of faculty interested in animal learning.

I spent my first few years as an Assistant Professor reading everything I could get my hands on in the fields of Comparative Psychology and Animal Behaviour, and discussing the subject with everyone I could induce to visit McMaster. I was also particularly fortunate to encounter G. C. Williams's *Adaptation and Natural Selection* early in 1968, putting me ahead of most psychologists and ethologists in my thinking about the evolution of behavior. However, without the generosity of leaders in the community of biologists, particularly Danny Lehrman, Niko Tinbergen and Ken Roeder, I would never have gotten started.

What were the most important experiences in your development as a biologist? Three experiences come immediately to mind, each of which had significant impact on my subsequent thinking about animal behaviour. First, a second-year undergraduate course on the philosophy of science taught by the great logical positivist, Carl Hempel, in which he discussed at length the work of Ignaz Semmelweiss, the discoverer of the cause of puerperal fever. Semmelweis's work has always served me as a model of truly great science. So although I came to the study of animal behavior without any background in the area, I did have a compelling model of how science should be done. Second, a graduate laboratory course in comparative psychology taught by Paul Rozin and others, in which students formed small groups, designed an experiment and then ran it. Although the experiment my group attempted on aggression in Siamese fighting fish was a total flop, the experience left me with the feeling that animal behaviour research could be done by anyone with sufficient interest. And third, a sabbatical year that W. John Smith arranged for me to spend at the Smithsonian Tropical Research Institute on Barro Colorado Island in Panama. Although I was never to do any further field research, the months spent both trying to understand the behavior of free-living agouti and helping a half-dozen energetic young field workers with their work gave me an appreciation of the challenges and rewards of behavioural research outside the laboratory that greatly increased my respect for those working with animals in uncontrolled environments.

How did you end up studying the behavior of rats in the laboratory? My education, such as it was, was as an experimental psychologist, and, at the time I emerged from graduate school, the only animals whose behaviour experimental psychologists took really seriously were rats and pigeons. The choice of a subject species for my own work was rather limited.

I had hoped that because I used wild, rather than domesticated, rats in experiments, biologists would see my work as relevant to their interests. However, I failed to appreciate the extent to which Norway rats' lack of a well-defined ecological niche posed problems for integrating my work into the biology of the day. So, I worked

as a comparative psychologist until the field was largely taken over by those who seemed intent on denying a role for evolutionary process in the study of animal behavior. Then I started calling myself an animal behaviourist.

Why social learning about food in rats? In the 1960s and 70s, the study of ingestive behavior was a central topic in both physiological psychology and developmental psychobiology, and rats were the main subject species in both areas of investigation. Studies of social influences on the development of rats' food preferences fit well with this theme.

Reading the work of Tinbergen and other ethologists had, I think, led me to a greater appreciation than many of my fellow experimental psychologists of the potential of field studies as sources of hypotheses about the behavior of animals. I had discovered a rather obscure paper by an applied ecologist, vonFritz Steiniger, in which he provided evidence consistent with the view that free-living wild rats were able to dissuade their fellows from ingesting human-introduced poison baits. Poison-avoidance learning was then the hottest topic in animal learning and, quite by chance, my laboratory was configured so as to make looking at the possibility of social learning of poison avoidance in rats, both wild and domesticated, easy.

How did your work become of interest to biologists? The critical event was an invitation from Jay Rosenblatt to contribute a chapter to *Advances in the Study of Behavior.* I spent nine months writing a manuscript for Jay in which I attempted to place the work I was doing on social influence on the development of rats' food choices in the context of other studies of social influences on behavioural development. Although there were numerous, interesting published studies of social influences on the acquisition of adaptive patterns of behaviour in animals, and some, such as the studies of sweet-potato washing in Japanese macaques or song learning in white crowned sparrows, were well known, no one had tried to define a cohesive field out of these widely scattered reports.

Emergence of a general interest in animal social learning was also impeded by the plethora of terms used by different authors to describe the social learning that they observed. This vocabulary was sufficiently chaotic and contradictory that it interfered with, rather than facilitated, communication among workers in different disciplines who might be interested in social influences on behavior acquisition. Indeed, arguments about terminology seemed to have turned biologists off the whole subject of 'imitation' in animals. In the late 1980s I undertook a systematic review and synthesis of the terminology of social learning suggesting that the question, so central to psychological studies of animal social learning, as to whether animals could truly imitate, was not so important from a biological point of view.

How did working in Canada influence your career? I owe a great debt to the Natural Sciences and Engineering Research Council of Canada (NSERC) for 40 years of continuous funding. Although the size of grants in Canada has never been large by US standards, grants were both reliable and predictable, and this has been a tremendous benefit for the kind of work I like doing. Further, NSERC grant-selection committees pay far more attention to track record than to the five-page combined progress report and research proposal that they require every three to five years. Consequently, I have been able to continue work on problems that interested me for a dozen grant cycles without having to keep proposing 'new' lines of research, just to remain funded.

Has the nature of academic life changed? I doubt that I would fare particularly well in today's academic environment. At least at my University, there is currently an emphasis on participation in collaborative mega-grants, having a large laboratory, publication in a handful of journals with the highest citation indexes and pursuit of coverage by the local popular press. None of these things have been of particular interest to me. Quite the contrary.

As I have told my students (probably far too often), I view a life in science as a marathon, not as a sprint. My goal is to ask simple questions arising from clearly stated hypotheses, to use both simple experimental designs and transparent statistical analyses, to proceed one step at a time, experiment after experiment, frequently replicating main effects, until I can be quite sure that when others attempt to repeat my procedures, they will get the same results that I did. Not exactly the sort of approach likely to reap accolades today.

So, was it all just a series of lucky breaks? Clearly, luck has played a huge part in my career. Many would say I got a job when I shouldn't have; I was granted funding when I shouldn't have been; I got excellent students when I shouldn't have; and so on. But hard work has been a non-trivial part of the picture. During the 40 years that I have spent at McMaster, I have always worked seven-day weeks. As a result, I was usually several months ahead of any of the myriad deadlines that are an unavoidable part of academic life, and was nearly always in a position to take advantage of any unexpected opportunity that came my way and to give every project that I engaged in my best effort.

Because one thing almost invariably leads to another, being able (and willing) to accept and respond promptly to every invitation, no matter how unpromising, has provided rich rewards, if you interpret 'rewards' sufficiently broadly. Readers of this article are likely to be engaged in a most peculiar profession: one in which the only rewards for working hard and well that most of us will ever garner are invitations to do even more work for no extra pay (the present article is a case in point). The sole excuse for my having been involved for 40 years in such a ridiculous enterprise is love of the work itself.

What advice would you give someone considering a career in behavioural research? That's easy. If you love it, do it. If not, do something else.

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