Supplement to Behavioral Ecology

ISBE International Society for Behavioral Ecology

Newsletter

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Message from the President

It is my privilege to thank the organizers of the past summer's highly successful behavioral ecology congress in Montreal, which was so very enjoyable, socially as well as scientifically (*see conference review on page 5*). Luc-Alain Giraldeau, Don Kramer and their crew of about 50 people at Université du Québec à Montréal and McGill University arranged the conference in a lively and colorfully festive part of the city. This contributed to the friendly and informal atmosphere of the well-organized meeting, which had over 700 participants, 285 talks, 257 posters and a pleasantly wild party at Sucrerie del la Montagne, in the maple forest.

During the congress week it became abundantly clear that behavioral ecology is as vigorous a research field as ever. New fascinating questions are being asked, many of them by young researchers and students recently attracted to the field. Novel molecular tools are providing answers to many questions that were entirely inaccessible a decade or two ago. New exciting integrations are taking place between behavioral ecology and fields such as life history theory, endocrinology, immunology, neurobiology and reproductive physiology, leading to exciting discoveries, many of which were reported in Montreal.

One of the high points was a talk by a major figure in our field, Robert Trivers, who very fittingly delivered the society's first Hamilton Lecture. The Hamilton Lecture was established at the previous congress in honor of Bill Hamilton, who died in March 2000 from malaria contracted during fieldwork in Africa. Bob Trivers gave an exciting and warmly acclaimed lecture on "Selfish genetic elements and social behavior".

After a vote of the officers and editors, the winner of the Frank A. Pitelka award this year was Todd Blackledge. He received his award from the Past-President, Steve Emlen, for the best paper published by a young researcher in our journal during 2000-2001: "The evolution of cryptic spider silk: a behavioral test" (Blackledge, T.A. & Wenzel, J.W. (2000) *Behavioral Ecology* 11:142-145).

Many thanks and congratulations to the organizers and contributors to the excellent and memorable Montreal meeting! If you were not there, another similar chance arises in Jyväskylä, July 10-15 2004, where Rauno Alatalo and his colleagues are now preparing for the 10th Jubilee Congress in a very nice part of Finland (*see announcement on page 4*)

The biennial scientific meeting is one of the major reasons for the existence of our society. Another is the journal, *Behavioral Ecology*, which is also doing very well, with increasing impact factor and submissions. A report on the journal is found in the Minutes of the General Business Meeting in this newsletter. Two of the former Editors, Innes Cuthill and Ron Ydenberg, completed their terms this summer, and Gunilla Rosenqvist does so early next year. The Society owes a great debt to them for all their admirable efforts to successfully maintain and even increase the already high quality of the journal. We are fortunate to welcome as replacements three persons with much editorial and publication experience, including the writing of highly acclaimed books: Anne Houde, Ian Owens and Marlene Zuk. To handle the increase in submissions and the change to electronic submission from 2003, the editorial structure has been modified. From July this year there is a Chief Editor, David Westneat, and in 2003 there will be 6 Editors (presently 4). We are also happy to welcome the new representative of Oxford University Press, Cathy Kennedy, who is not only an experienced scientific editor, but also knows our field well, with a Ph D in behavioral ecology.

I would also like to thank our retiring officers, Past-President Stephen Emlen and Secretary Marion Petrie, and Councilors Jane Brockmann and Andre Dhondt. Welcome to the incoming new persons: Jack Bradbury as President-Elect, Paul Ward as Secretary, and Hanna Kokko and Nina Wedell as new Councilors. Many thanks also to the retiring members of the editorial board: Andrew Cockburn, Nick Davies, Larry Dill, Marion Petrie, Francis Ratnieks and Marlene Zuk, and welcome to their replacements: Göran Arnqvist, Alexandra Basolo, Laurent Keller, Ellen Ketterson, Kate Lessells and Mats Olsson.

We are all fortunate to serve the society in such an exciting time for behavioral ecology.

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Society News

DONATED SUBSCRIPTION PROGRAMME

Please help colleagues in need. Every donation will help increase scientific contacts across the world. In a time when nationalism is again raising its ugly head, this is more important than ever. For details, see the advertisement on the inside back cover of *Behavioral Ecology* volume 12(4).

SPOUSAL MEMBERSHIP

For $5 per year spouses of full members can become members of ISBE. Spousal members receive the newsletter and information concerning biannual meetings, but do not receive a subscription to the journal. Contact the Treasurer for more details.

ISBE 2004 CONFERENCE

The 10th Jubilee Congress of the ISBE will be held in Jyväskylä, Finland, 10-15 July 2004. Details can be found at www.isbe2004.com.

WORKSHOPS AND OTHER MEETINGS

XIXth (NEW) International Congress of Zoology will be held in 2004 in Beijing, China.
Basic information, such as correspondence, first announcement, online registration and how to organize a symposium, is available on the web page http://www.icz.ioz.ac.cn/.
The research papers presented in the congress will be published in *Acta Zoologica Sinica*.

Fisheries Society of the British Isles Annual International Symposium: Fish as models of behaviour. Norwich, UK. 30 June - 4 July 2003.
Details can be obtained from:
Tricia Ellis-Evans
Pace Projects
65 High Street, Toft, Cambridge CB3 7RL, England.
Tel. 01223-263477
Fax. 01223-264663
Email: tricia@paceprojects.co.uk
or check the FSBI website:
http://www.leicester.ac.uk/biology/fsbi

GRANTS AND JOBS

Grants and Job postings are listed in detail on the newsletter's webpage: web.unbc.ca/isbe/newsletter/index.htm

MINUTES OF ISBE GENERAL BUSINESS MEETING, 10TH JULY 2002.

Chair: Malte Andersson, Society President

Agenda

1. Local meeting report
2. Oxford University Press report
3. Editors' report
4. Treasurer's report
5. Newsletter report
6. 2004 meeting report

Malte Andersson, opened the meeting and explained its main purposes were to hear the Society officers' reports and to raise issues of general relevance. There were around 90 members present.

Don Kramer gave a report on the Montreal meeting. There were 285 talks and 257 posters, with a total of 701 participants (including the plenary speakers). Forty volunteers had helped with the organization. The budget is sound.

Cathy Kennedy, Senior Editor at Oxford University Press, reported that the journal is

doing well compared to other journals, with subscriptions increasing. She stressed that members should **ensure their libraries register their IP numbers with the press if they are taking the journal.** The Impact Factor of the journal is increasing over the years. The finances are sound and this is the first year in which the Society will receive income from the journal, increasing in later years. Online submission is planned for 2003.

Ron Ydenberg gave the Editors' report. Submissions are increasing, with 639 since the last report, which is 71 per editor per year. 182 were accepted and 371 rejected (the others are still in handling), giving a 33% acceptance rate. 1314 reviews from 853 referees were handled. The lay summaries are working well. The blinded reviewing procedure is also working well, even though there are some complaints, but Ron commented that reviewers are not as good as they think about identifying authors. Ron Ydenberg and Innes Cuthill have come to the ends of their terms and are being replaced by Ian Owens and Marlene Zuk. There will be a change in the organizational structure of the journal editing when electronic submission is introduced.

Walt Koenig gave the Treasurer's report. The Society is in good shape, especially as it has become a tax-exempt organization in the USA. The main income comes from the journal, money in the bank and (sometimes) (small) profits from the meetings. The main expense is the Newsletter. The Society committee decided to help participants from disadvantaged countries attend its next meeting.

Ken Otter gave the Newsletter report. The book reviews are a success. He intends to publish workshop proceedings, especially where no published proceedings are planned. He is also interested in receiving commentary pieces, especially from students. Cartoonist wanted! Send him stuff - it might well get in. There is a website at web.unbc.ca/isbe.

Rauno Alatalo informed about the next meeting in Jyväskylä, Finland, July 10-15 2004. Jyväskylä is three hours from Helsinki. The logo is an attractive displaying black grouse. Details at www.isbe2004.com.

Steve Emlen, Marion Petrie, Jane Brockmann and Andre Dhondt have completed their terms as Past President, Secretary and committee members. They are replaced by Nick Davies, Paul Ward, Hanna Kokko and Nina Wedell respectively. Jack Bradbury joins the committee as President Elect. Malte Andersson thanked all the outgoing officers and welcomed the new ones

Malte Andersson closed the meeting to uproarious applause.

Paul Ward, ISBE Secretary

SWEET CANADA, CANADA, CANADA: REVIEW OF THE 2002 ISBE, MONTREAL.

I must admit, my first impressions of this conference were not good. Our party couldn't follow the directions provided on the meeting website and got lost among the crowded streets of central Montreal. Even when we reached the UQAM campus, we had problems locating the actual conference building because it was so poorly advertised. It never ceases to amaze me how much of the confusion that plagues conferences could be solved by a few simple arrows or cheaply made signs. Still, a quick trip to the nearby dorms to drop off my stuff and freshen up worked wonders, and I returned to the reception in a more positive frame of mind. Fortuitously, the start of the conference overlapped with the end of the International Jazz Festival, held just a few blocks away from campus. Like many other delegates, I hastened downtown after the mandatory glasses of reception wine, and spent an affable evening

listening to the various bands and watching the street performers.

With more time to get my bearings the following morning, it was immediately apparent that the building in which the conference was held was perfect. The Agora was a large, spacious atrium, multi-layered to allow for easy people-finding, and with *ad libitum* supplies of bagels, fresh fruit, and, most important of all, coffee. Moving between talks was quick and easy since the majority of the rooms were right next to each other, save for a few far-flung outposts thrown in to provide a bit of a challenge. Rooms were designated using animal icons, which generally worked well, although I think the extra expense of having the icons in colour would have avoided some of the navigational problems people had in the first few days.

A quick breakdown of talks by taxon revealed that birds were by far the most popular study subjects (43% of talks), with insects, fish and mammals fighting it out for second place (16%, 15%, and 14% respectively). The equivalent figures for poster presentations were remarkably similar (birds 46%, insects 16%, fish 16%, mammals 12%), which presumably meant that the organizers didn't allow study taxon to influence the poster/talk decision. I also assumed that they hadn't been 'intimidated' by fame either, as I noticed several established researchers standing beside posters while graduate students were given plenty of opportunity to display their talents in talks.

In the light of Tim Birkhead's cry for primatologists to attend ISBE meetings in the last newsletter, it was good to see that around a quarter of the mammal presentations concerned primates. One could make an equally plaintive appeal to those studying reptiles and amphibians, two common and widely distributed groups of animals that to me seemed strangely under-represented, particularly as both have been the focus of some high profile studies.

For those students, like myself, who suffer from a nagging unease about their taxonomic inertia, ISBE conferences are great opportunities to appreciate the sheer diversity of the animal kingdom while simultaneously realizing how much of biology stems from a few simple observations. To give a popular, albeit simplified, example, sperm are small and cheap while eggs are large and precious. Hence, males try to maximize the fertilizations they achieve, whereas females try to optimize theirs, and this pattern pretty much holds in all dioecious organisms, regardless of their size, habitat, abundance, neural complexity, or aesthetic appeal. Once you are familiar with the basic principles of a particular research topic, it's a rewarding experience to merely remain in one's seat throughout an entire session and see how people studying wildly different animals approach the same question. It might not have been a coincidence that some of the most illuminating sessions that I attended were also those that were the most diverse; for example multiple mating (a mammal, bird, snake and a newt), fertilization dynamics (two birds, a mammal and a fish) and mate choice (a fish, two birds and a moth).

This was a mammoth conference (260 talks in five parallel sessions, plus 240 posters), and I don't have the space to discuss those talks that I found especially well presented or novel. Still, I can offer a few personal observations on how the featured research areas have waxed and waned. As a testament to the diversity of research on show, I couldn't say that the conference had a dominant theme. Unsurprisingly, sexual selection and mate choice are both still going strong, and the number of presentations about multiple mating was an emphatic demonstration of the impact DNA fingerprinting has had.

Extra-pair paternity in birds was particularly well represented, and seems to have been given a shot in the arm by some novel genetic analyses. Cooperative breeding was also a popular subject, with one session devoted to avian systems being especially impressive. For those ornithologists who don't study cooperative

species, it was interesting to note that the pendulum seems to have swung towards unravelling the subtleties of monogamy, with lekking and strongly polygynous species considerably less prominent. The evolution and function of signals, both visual and aural, received much attention, as did sex allocation, particularly in birds. I did get the impression, however, that people have been happy to accumulate empirical data on sex ratio variation in a wide range of species, but have run out of steam when it comes to taking the field forward. To me, this is reminiscent of the tidal wave of data collection that followed the discovery of extra-pair paternity in birds, which then faded to a trickle as people tried to figure out what the species diversity actually meant.

Immunocompetence has retained its popularity, although there were few explicit tests of its progenitor, the Hamilton & Zuk hypothesis. In fact, apart from Frank Cezilly's excellent plenary on manipulation of host behaviour, parasites no longer seem *de rigeur*. Modeling seemed less common that in previous ISBEs, there was little work on pre-existing sensory biases, and the MHC doesn't seem to have attracted the attention that it deserves. Fluctuating asymmetry was conspicuous by its near-absence, and there was nothing on the aerodynamics of bird flight in relation to tail streamers.

I didn't see anything that I thought was revolutionary, although one could detect topics which are definitely gathering momentum. The emphasis on maternal effects was the most obvious example, particularly the differential allocation of steroids and antibodies into birds' eggs. A full oral session devoted to learning and cognition was a welcome addition to the ISBE compass, since these subjects, while not new, were entirely absent from the last meeting in Zurich.

On a negative note, there were times when the meeting gave me an uncomfortable feeling of shallowness. The field of behavioral ecology has been described by its arch-cynics as nothing more than a collection of just-so stories. Although I don't share such an extreme view, I was concerned by the general lack of objectivity with which people presented hypothesis-supporting data, and the readiness with which they glossed over critical assumptions. The attitude among some behavioral ecologists seems to be that for a study to be believable, one only needs to convince other behavioral ecologists. This view is short sighted, in my opinion. If behavioral ecology is to gain acceptance by other established disciplines, then it must treat them with more respect than the cursory nod they are currently afforded. Assumptions borrowed from other fields should be carefully verified before proceeding, preferably with the help of a specialist collaborator.

To be fair, I got the impression that behavioral ecology is at least trying to integrate research from other disciplines, and some of the more convincing talks probably ended up that way thanks to the input of their endocrinologist, physiologist or geneticist co-authors. The more networks are opened, the better. As any spider will tell you, the more strands that are attached, the stronger the web. Anyway, soapbox off!

The 10 plenaries were wide-ranging, well attended, and well presented. Considering the recent profusion of 'sexy' subjects in behavioral ecology, it was refreshing to find that many people I talked to thought that the highlights of the meeting were the two consecutive plenaries by Louis Lefebvre (innovation rate in birds) and Reuven Dukas (the biological foundation of cognitive constraints), both of which were firmly rooted in classical ethology. The organizers scored a considerable coup in getting Bob Trivers to deliver the Hamilton lecture, on selfish genetic elements and social behaviour, and he certainly didn't disappoint, sliding effortlessly between taxa in a thought provoking exposition of the games genes play.

The three evening poster sessions proved very popular, and people took advantage of the opportunity to view the presentations during the

day. Still, I think the general feeling was that 90 minutes wasn't long enough to get around the two rooms, as evidenced by the difficulty in getting people to leave at the end! Many discussions were carried down to the Bar l'Apres-Cours, where, even after midnight, one could still hear heated and surprisingly highbrow debates raging among gaggles of delegates. This reinforced my personal belief that the most productive poster sessions (and conferences in general) are those in which people are given a few drinks to loosen their tongue, then gently funneled into a large room and provided with even more drinks.

Another great plus for this conference was Montreal itself. Anyone feeling jaded after too many talks merely had to walk out the door and they would be instantly invigorated. Colorful buildings, a cosmopolitan crowd, ethnic shops, lively bars, and, best of all, abundant opportunities for trying some unusual foods. In the space of just five days I sampled the cuisine of Ethiopia, India, Thailand, Greece, China and Japan, often bumping into other behavioral ecologists enjoying the variety. I also saw many delegates slaving over cold beers and fruit juices in the sunny terrace cafes which lined St-Denis Street: it's a hard life.

The conference banquet, held at a traditional 'sugar shack' deep in the Quebec countryside, was certainly a memorable evening. Anyone frustrated by the less than optimal serving arrangements in the suspiciously quaint souvenir shop must surely have been mollified by the bottles of gorgeous, locally produced maple syrup waiting at each table. Still, two sausages, some beans and cold mashed potato was scant reward for what had been a long and hungry wait for the main course, and the evening looked in grave danger of fading away into nothingness. Indeed, many people were probably relieved to get a seat on the first coaches back to Montreal and have an early night. Little did they know what they would miss, as a mysterious stimulus suddenly transformed the huddled cliques of mumbling scientists into a unified mass of swirling legs and flailing arms. I shall never forget the sight of well-respected figures moving benches, forming conga lines, dancing on tables, and basically having a good, old-fashioned sing along.

The organizers, Luc-Alain Giraldeau and Don Kramer, had done a superb job. They deserve particular credit for being aware of, and quickly rectifying, the occasional problems which cropped up during the meeting (non-existent air conditioning, fire alarms, deafening chipmunk alarm calls, inaccessible food tables etc). All in all, this was a memorable conference on many levels, and if its memes are promulgated into the next generation, we can all look forward to Finland in 2004.

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Book Reviews

A Spider's World: senses and behavior

Freidrich G. Barth. (translation: MA Biederman-Thorson). Springer-Verlag. 2002. 394 Pp.

ISBN 3-540-67716-X

While past study of arachnids has largely been dominated by systematics, there has been increasing focus in recent years on arachnid behaviour and sensory biology. This interest is fuelled in behavioral ecologists by the recognition that spiders are tractable systems for gaining insight into a number of theoretical issues. Among sensory biologists and neuroethologists, interest has arisen for a different reason--because spiders have a remarkable set of sensory adaptations that support a diversity of highly specialized habits. Compared with other arthropods, however, we still know relatively little of the sensory processes mediating spiders' interaction with their world. In this book Freidrich Barth presents us with a detailed picture of one of the few exceptions to this general paucity of information – spiders of the genus *Cupiennius*. The book effectively summarizes more than three decades of work on the sensory physiology and neuroethology of this group. While it is, in this sense, a monograph on a single Genus of spiders, the real focus is on fundamental mechanisms by which sensory information is acquired and how this information is related to behavioural context. *Cupiennius* is therefore presented as a kind of model system for spider sensory physiology.

The book is written in a very readable and personal style, which makes it that much easier to digest the large amounts of technical information it contains. For example, interspersed among intellectually challenging presentations of structure and physiology are descriptive accounts of field work (including a first encounter with chiggers), excerpts from correspondence with colleagues, and some historical information. These narrative sections are offset from the main text either in boxes or by use of different typeface, giving the reader the option of avoiding them to follow the main flow of the text if desired. Although expensive, this is a well-produced volume - which also aids ease of reading - and includes a section of color plates as well as numerous figures that are clear, informative, and conveniently placed near to relevant text sections. In addition, the binding allows the book to open flat at any page.

The book is organized in four sections: the general biology of *Cupiennius*; sensory systems; the central nervous system and its peripheral nerves; senses and behavior. The first section, on general biology, is very brief and serves only to identify the genus and outline its habits and habitat. *Cupiennius* are wandering spiders native to Latin America. They are nocturnally active mostly on the broad leaf surfaces of plants such as bromeliads and bananas. This natural history is important because, as we later learn, plant choice by *Cupiennius* has extensive implications for the efficacy and range of their use of vibratory information for prey capture and courtship.

The second section takes us through each of the spider's sensory systems. This is one of the strongest parts of the book, encompassing the physical and anatomical bases of sensory transduction, and the organization and anatomical distribution of sensory organs in each modality. This section is necessarily wide-ranging. Each sensory modality is introduced by a discussion of the basic physics underlying the relevant stimulus energy. So, for example, we are introduced to the mechanics of vibration in plants, the fluid dynamics of air currents and visual optics. Special emphasis is placed on the mechanical senses, as it is largely through

substrate vibrations and near-field air currents that *Cupiennius* detect prey, avoid danger and communicate with conspecifics. Barth's delight at the elegance and "technical perfection" of spider sensory organs comes across clearly, and he paints a richly detailed picture of the sensory world of these spiders.

The section on the nervous system deals mainly with neuroanatomy and the distribution of neuromodulatory substances, rather than the detailed analysis of neuronal circuits that would be expected in a comparable volume on insect sensory biology. This reflects the fact that, compared with other arthropods, electro-physiology of the central nervous system in spiders is problematic and thus is largely unexplored. Barth effectively highlights (in this section and throughout the book) the limits to our current knowledge and points to areas where potential advances are available.

The last section, on senses and behavior, links the detailed mechanistic analyses of sensory function to behavioral measures of the spiders' abilities to discriminate sensory cues. This is, once again, a wide-ranging section as there are many ways to probe behavioral responses, even within a single stimulus modality. In many cases it has required a series of experiments to demonstrate unequivocally that a particular sensory input contributes to the generation of a particular behaviour. Barth takes us, step by step, through multiple examples of this empirical process to demonstrate the relationship between sensory input and the control of natural behaviors. This is a distinctly different level of behavioral analysis from that which most behavioral ecologists are accustomed to. The discussion here is not about how behavioral decision rules are shaped by their adaptive consequences. Rather, the motivating question is how are adaptive behaviors "encoded" in the structure of the sensory and nervous systems of the animal. For example, how is it that *Cupiennius* will respond to a combination of substrate vibration and air currents by attacking when these are generated by prey but by retreating when they are generated by a predator? The differences in these two situations can be quantified in terms of stimulus parameters, and in turn as differences in sensory responses. The tuning of sensory inputs and their relative weighting in response to combined stimuli must reflect the adaptive demands on behavioral responses.

This book gives us as detailed a picture as is currently available of the way a spider filters information from the world around it. It is likely to be most enjoyable for those who have an interest in spider biology or who are fascinated by how biophysics, physiology and biology interact to produce behaviour. While it is a challenging read (in several places, Barth warns that the "impatient reader" may find certain chapters difficult), the payoff for perseverance is a deep understanding of the complexities of a fascinating animal and its sensory world.

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Sperm Competition and Its Evolutionary Consequences in the Insects

Leigh W. Simmons. Princeton University Press, 2001. 434 Pp.

ISBN 0-691-05988-8

The field of sperm competition has seen a steady increase in both interest and publications since Geoff Parker's influential article appeared 32 years ago[1]. Last year, for instance, saw the biggest crop yet (a quick search of the ISI Web Of Science database for "sperm competition" appearing somewhere in the title, abstract or keywords found 133 hits). Parker's original paper and a large proportion of what has followed since have used insects as the study organisms. As Leigh Simmons points out in his book, *Sperm Competition and Its Evolutionary Consequences in the Insects*, although there is more to the field of sperm competition than just insects, the group is unrivalled in the diversity of mechanisms and the evolutionary outcomes of sperm competition that can be found amongst them.

Simmons' self-stated aim of his book is to "provide a conceptual framework for the evolutionary interpretation of observed processes of reproduction". His approach to achieving this aim is to examine a particular process (such as patterns of paternity, copula duration, sperm morphology etc) and using examples from the literature, build a picture of the evolutionary mechanisms that dominate or contribute to the evolution of that process. Simmons is very successful in providing the reader with the wider view whilst not neglecting the exceptions.

The star species of the book is of course the yellow dung fly *Scatophaga stercoraria*, which embellishes the cover. The breadth of the literature written on this one species makes it the sperm competition biologist's equivalent of the geneticist's *Caenorhabditis elegans*. Simmons draws on this literature throughout the book, giving an additional instalment of the *Scatophaga* saga in almost every chapter. But this is not a single species monograph, the book is packed with a diversity of examples and data from the literature. It is well structured into 11 chapters, each one subdivided into easily digestible parts, something which is welcome and familiar to readers of other titles within the "Monographs in Behavior and Ecology" series. Simmons has also provided some very useful summary tables for several of the chapters, together with a chunky reference list. These are features that I am sure I will be diving into again in the future. Furthermore, Simmons' writing style is steady and methodical, taking the reader carefully and thoroughly through all of the major points necessary to have a good understanding of each section.

In the first chapter, Simmons introduces the field of sperm competition from a historical perspective and sets its importance within the stage of sexual selection. He goes on in Chapter 2 to lay down some definitions of terminology used within the field of sperm competition that will no doubt be useful to newcomers and old hands alike. A discussion follows of how patterns of paternity may be measured, analyzed and interpreted together with some cautionary advice on how informative these patterns may or may not be in terms of describing the mechanisms of sperm competition. These first two chapters will be especially useful for people unfamiliar with the discipline.

The next three chapters deal in turn with the morphological, physiological and behavioral adaptations that male insects exhibit as they strive to reduce the likelihood that either their sperm will be in direct competition with the sperm from a previous partner, or that their mate will remate. These chapters contain some really nice examples from the literature, from the more familiar sperm removal abilities of odonates, and the remarkable effects of seminal substances produced by species of *Drosophila*, to the epic copulatory mate guarding of male soapberry bugs (up to a staggering 128 hours).

As a "thirtysomething", the field of sperm competition still displays a mixture of maturity and youthfulness. Maturity is evident in how well current theories describe how long a male should

copulate for (chapter 6), or how many sperm he should ejaculate (chapter 7). Youthfulness can be seen in how poorly we understand the evolution of sperm polymorphisms and sperm morphology (chapter 8). This latter area is closest to my own interests and so I was especially eager to read this chapter, but I was left hungry for more, no doubt because so much remains unknown about this subject.

Simmons deals with the accusation that female influences are often ignored by researchers investigating sperm competition by gathering together the female side of the story into a single chapter. The alternative would have been to address this question throughout the book, but given the small number of studies that have framed their hypotheses from a female perspective that is still probably not an ideal approach. Instead, by saving all of the evidence for the ninth chapter it is much easier for the reader to appreciate the current status that cryptic female choice enjoys. This was one of the best chapters, with again some really interesting examples and clarifications of this generally neglected or misunderstood area.

Sperm competition amongst the social insects is the subject of chapter 10. Due to the unusual mechanism of sex determination in the social insects the occurrence of sperm competition has some fascinating implications that are peculiar to this group. This was the least familiar territory for me and it was certainly one of the most stimulating chapters.

Finally, in chapter 11 Simmons gathers together and discusses briefly the broader significance that sperm competition plays in sexual selection, life history evolution and speciation. I felt that this chapter did seem a bit empty by ignoring examples from other taxa. But that's probably a harsh criticism, since the rest of the book is so good and a line must be drawn somewhere.

The production quality of the book is high, the figures and tables are clearly set out and typographical errors are few (curiously concentrated on page 313). This book is without doubt a significant contribution to the field, despite it essentially being a review it contains numerous new ideas and perspectives. I would certainly recommend this book to anyone actively working in the field of sexual selection (and not just those working with insects) or interested graduate students, since it provides such a clear appraisal of concepts that extend beyond taxonomic boundaries.

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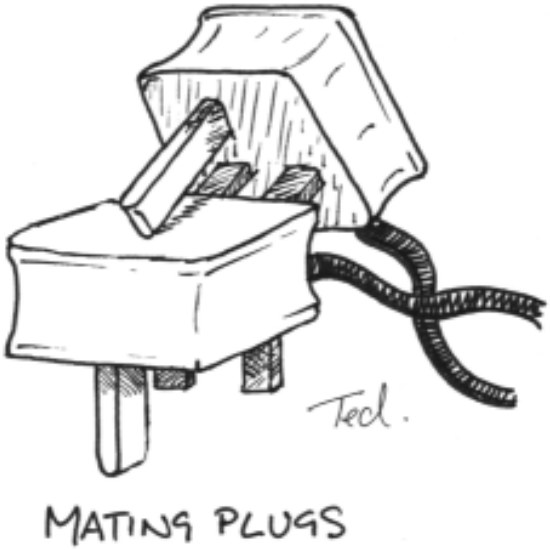

Cartoon by Ted Morrow

Economics in Nature: Social Dilemmas, Mate Choice and Biological Markets

Ronald Noë, Jan A.R.A.M. van Hooff & Peter Hammerstein (Eds). Cambridge University Press, 2001. 276 Pp.

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Natural selection makes animals good economists. This is the underlying rationale for *Economics in Nature*, as indeed it is for much of behavioral ecology. However, don't imagine from the title that this is a comprehensive review of the applications of economics to natural examples. Rather than attempt such an ambitious undertaking, the editors have gathered together a collection of contributions focussing on social and sexual behaviour. These grew out of a 1996 symposium of the Jean-Marie Delwart Foundation which brought together a group of distinguished behavioral ecologists, anthropologists, evolutionary biologists and primatologists. The chapters are arranged in three sections: social networks, biological markets and mating markets. As might be apparent from this list, it is one particular branch of economics, free-market economics, which presents the dominant theme. Nevertheless, any suggestion that the book might be re-titled 'Adam Smith in Nature' (or even 'Natural Thatcherism') would do a disservice to those contributions less focussed on this theme.

Hammerstein's opening chapter sets the scene. Many of the questions behavioral biologists are interested in have parallels in economics. What is more, Darwinian evolution should have provided animals with the solutions achieved by rational decision makers in economic theory. So, drawing on economic approaches, rather than re-inventing wheels, should be a profitable enterprise for behavioral biologists. However, as Hammerstein makes clear, it's not always as straightforward as that and "the facts often do not meet theoretical expectations". The current status of the field of cooperation is a case in point. We have a popular paradigm based on playing Tit-for-Tat in iterated Prisoner's Dilemma games, yet here it comes under fire on both theoretical and empirical grounds. To a game theorist, Axelrod & Hamilton (1981) were just re-stating "folk theorems" that "all the folks in game theory" knew, while from an empiricist's perspective, the models have proved difficult to apply and test. Hammerstein's chapter gives a valuable insider's guide to the profit and loss account resulting from applying economic games to animal behaviour.

Part I on Social Networks begins with Ostrom's review of human behaviour in social dilemmas. "Not all behaviour is inconsistent with theory" she concludes encouragingly. Theorists often seem to subscribe to the old saying that three's a crowd, contenting themselves with models of dyadic interactions. Nunn and Lewis set out to remedy this apparent agoraphobia in the cooperation literature by investigating how models of collective action derived in economics might be applied to animal groups. Attention then turns to primates with van Hoof's discussion of how reconciliation behaviors can be seen in an economic context.

In Part II, the advocates of the 'Biological Market' approach (Noë, Barrett & Henzi, and Bshary) set out their stalls. Their basic premise is that the text-book Prisoner's Dilemma-based paradigm is unrealistic because it is based on unreasonable assumptions, such as having dyadic interactions with fixed partners. The impression is one of fieldworkers grappling with applying the models handed down to them by theoreticians, finding a divide between theory and practice, and searching for an alternative. This reflects a growing rebellion against the 'Tit-for-Tat' paradigm, and a gathering of some of the disaffected around the Biological Market banner. The argument goes that we are used to individuals choosing mating partners, so why not choose cooperative partners? The advantage of the biological market approach, say its advocates, is that it is based on a more realistic n-player game with partner choice. But is all this Prisoner's

Dilemma-bashing fair? Is the biological market approach any more than a Prisoner's Dilemma with partner choice (which has, after all, been incorporated into more recent Prisoner's Dilemma simulations anyway)? Are Barrett & Henzi right that the traditional approach will "inevitably fail"? And is market selection really so distinctive and important that it deserves to be recognized alongside sexual selection as a special kind of selective pressure?

Where we think the market approach does have something more to offer is in its consideration of market value: if individuals are seen as buyers and sellers exchanging commodities then supply and demand can determine the terms of their trading relations. It was Noë's insight that a baboon holding a resource that two others wanted could play them off against each other that led to the application of market ideas in social behaviour. Barrett & Henzi build on this by arguing that chacma baboon behaviour can be better understood by considering grooming as a commodity that is traded, either for itself or for some other service such as access to infants; then Bshary argues for the importance of partner choice in interpreting the results of a series of experiments on the fascinating "cleaner fish market".

The section is concluded with Hoeksma & Schwartz's demonstration of how an economic principle, Ricardo's Law of Comparative Advantage, can be applied to understanding trading relationships among plants and animals.

Having applied partner choice ideas from sexual selection to cooperation, Part III goes full circle and applies biological market theory back to 'mating markets'. However, although the chapters by Pawłowski & Dunbar (human mate choice), Pomiankowski & Iwasa (sexual selection), Parker & Ball (sperm competition), and Dunbar (male mating strategies) make clear that mating strategies evolve against a market background, it is less obvious that the ideas they present, though significant in themselves, are necessarily champions of the new paradigm. The chapters by Pomiankowski & Iwasa and Parker & Ball contain most of the book's mathematics, the remainder of which is scattered somewhat sparingly throughout the text. This will no doubt be a welcome relief to many, but will perhaps disappoint those who like their economics to come with equations. Indeed, Parker & Ball dispense with their mathematical proofs after setting the scene in their chapter on sperm competition, preferring instead to concentrate on explaining their findings in less formal terms.

The book as a whole is not as heavily theoretical as one might anticipate from the subject matter: rather, it reflects a healthy mix of theory and empirical data. The effective use of text-boxes guides the reader through specific topics by explaining key aspects of the author's argument. The high standard of presentation of the volume is only slightly marred by several typographical errors.

At the recent European Conference on Behavioral Biology, a distinguished ethologist asked one of us: "What's all this about biological markets? Never heard of them!" This book deserves to be widely read, especially by those interested in cooperative behaviour, and so should increase awareness and stimulate more applications of the market approach, which has already progressed since this book was written. One can only hope for more books like this so that market forces will bring down the price to a more affordable level.

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Of Moths and Men.

Judith Hooper., Fourth Estate, London, 2002.
ISBN 1841153923

This book will give you nightmares! It is the scariest popular science book I have ever read. It is scary because it plays right into the hands of the creationists, leaving evolutionary biologists like ourselves prostrate, naked and vulnerable in a forest full of army ants. The standard text-book example of natural selection in action is Bernard Kettlewell's work on the melanistic and non-melanistic peppered moth *Biston betularia*. It appears in virtually every biology book, and ever since it was published it has been **the** example of natural selection. What Judith Hooper has done through some remarkable investigative journalism and with great literary flair is to claim that Kettlewell's experiments are suspect. She doesn't actually quite say the results were faked, but she comes about as close as it is possible to do so, without saying it directly. When I checked her references I was surprised to discover that the peppered moth community had apparently known that Kettlewell's results weren't water-tight for some time and was therefore more surprised than ever that I hadn't heard about this. I asked my colleagues, including high school teachers, and none of them knew about it either. This in itself is interesting, because it suggests that no-one in evolutionary biology has been especially keen to broadcast the devastating news that their prime example is not entirely reliable. The most damning piece of evidence against Kettlewell that Hooper presents is this: when Kettlewell started his first moth release experiments he struggled to obtain enough re-captures. He told his mentor, E. B. Ford, who wrote back telling Kettlewell he **must** get the numbers up. And sure enough, according to Hooper, that's exactly what he did. She looks at various possible explanations for why the numbers suddenly increased, such as a change in the weather, but dismisses those. But she ignores the possibility that the energetic Kettlewell simply worked harder after a prod from Ford.

Regardless of whether or not Hooper has deliberately put a creationist spin on the peppered moth story or whether she has distorted the truth in order to sell her book, there is an important object lesson for behavioral ecologists here. Evolution is under threat from all sorts of quarters, and we have a responsibility to test evolutionary hypotheses as rigorously, as carefully and as honestly as possible. Compared with chemistry and physics, and some other branches of biology, such as molecular biology, field studies of animal behaviour and ecology are extraordinarily difficult, not least because behaviour is complicated and environmental conditions often vary in a million subtle ways from area to area and over time. Replicating results in behavioral ecology can therefore be difficult. But this shouldn't stop us, and what is needed is replicate studies of the same species under similar and different field or laboratory conditions. However, within behavioral ecology there is simply no culture of automatically replicating studies. This is partly our own fault - there's little incentive to 'waste time' double-checking someone else's results, when we could be getting on and doing something novel. It is also the fault of grant awarding bodies, who, with ever increasing demands on funds, are reluctant to 'waste' funding on studies whose objective is to check up on something already done. Journal editors are at fault too: they are reluctant to 'waste' space, publishing studies that verify (or contradict) previous results. The reluctance for replication is especially true when someone has an appealing story - like that of the peppered moth - which confirms (in its original form at least) what we want to believe. There are other reasons why behavioral ecology is especially vulnerable to poor and unreplicated science. Behavioral ecology arose from a paradigm shift in evolutionary thinking and as is well known among historians of science (but not, it appears, among many of its practitioners),

paradigm shifts typically create opportunities for poor science[1,2]. How a particular field develops depends on a delicate balance between innovative hypotheses, which is what will drive us forward, and being utterly honest in our findings. On the face of it at least, behavioral ecology has been very successful[3], but just imagine whether we would feel the same if, in fifty years time, any of our major players were subject to the same scrutiny as Kettlewell.

We need a change of culture. If someone makes a break-through, then we - the scientific community - should channel resources into checking the results. Obviously what you or I consider a break-through might differ, but if someone publishes something that fundamentally changes our thinking, then funding bodies should be more prepared to fund careful, responsible replication studies. Journal editors and referees should also be more sympathetic towards those who re-test fundamental ideas in behavioral ecology and to those whose results aren't perfectly tidy. But authors too should be prepared to be more honest; if something doesn't work out the way you expected, don't dress it up as something it isn't. I don't pretend that what I am proposing would be easy, but at least we might move forward with rather more certainty than we do now. It is only by stepping back - possibly with the benefit of increasing years, but also by considering the implications of Hooper's book - that we have any hope of behavioral ecology having a lasting effect. The good news is that a start has been made and Mike Majerus in Cambridge is repeating Kettlewell's study at this very moment.

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Commentaries

How are animal care guidelines applied to invertebrates?

The "Guidelines for the treatment of animals in behavioral research and teaching"[1] used by editors of various journals in assessing submitted manuscripts appears to apply across the board to all taxa of animals. But should the same standards really apply to a sponge as to a chimpanzee? or to an urchin larva vs. a mouse? Our recent experiences suggest that these would be fruitful topics for discussion.

We recently investigated autotomy in porcelain crabs - small abundant intertidal creatures well known for their propensity to shed limbs. We were interested in examining the anti-predatory benefits to autotomy, and how they may differ with condition and context. Autotomy is not a predator avoidance strategy; it is a phenomenon that occurs only after capture. Therefore, it would have been impossible to study in a natural field setting. Instead, we used staged encounters in laboratory enclosures. This enabled us to closely track the events that occurred following capture of porcelain crabs. We initially attempted to watch predation encounters by standing over the enclosure, but found this to be ineffective because both predator and prey seemed more intimidated by us than interested in each other. Furthermore, predation often occurred after more than an hour of nothing happening, and when it did, interactions were too quick to be objectively assessed by real-time human observation. We therefore videotaped trials, which solved both problems. We carried out about 200 such trials to replicate various combinations of predator and prey species, sizes and genders; in about half of these trials, the porcelain crab was eaten by the predatory crab.

We submitted a manuscript based on this work – demonstrating an anti-predatory benefit to autotomy in porcelain crabs – to the journal Animal Behavior. It was rejected after 8 months, without having undergone peer review, based on animal care concerns. This came as a surprise to us, since neither of our universities regulate the use of invertebrates for research. The reason given for rejection without review by Animal Behavior was that we should have more conservatively adhered to the recommended "Guidelines"[1]. In particular, they were concerned about the large sample size. This was perhaps larger than needed to demonstrate an anti-predatory benefit, but as we explained to the editor, it was necessary for examining condition- and context-dependence of the phenomenon, the subject of a second manuscript. The committee was also concerned that we allowed the porcelain crabs to get eaten following autotomy. However, because we remotely videotaped trials, we had been unable to rescue the prey immediately following autotomy. While unfortunate for the crabs, this also yielded useful information: we discovered that prey that escape by autotomy invariably are eaten in repeat attacks, while prey that escape by fighting often can indefinitely avoid being eaten. After a fruitless attempt at rebutting the animal care concerns, we submitted the manuscript to the journal Behavioral Ecology. The editorial staff at Behavioral Ecology explicitly considered animal care concerns raised by the manuscript, and opted to continue with the review process and eventually accepted the paper for publication[2].

This story illustrates that the criteria for animal care are subject to considerable interpretation. Perhaps the time has come for a forum to discuss animal care standards for invertebrates, so that both researchers and editors can adhere to more objective, explicit criteria. The "Guidelines"[1] indicate that "*an investigator must always weigh the potential gain in knowledge against any adverse consequences for the animals and populations under study*". For the first part – consequences to the animals – it might make

sense to take into account the sophistication of the animal's nervous system. Complex, well-developed brains are likely associated with greater suffering (experience of pain, fear, sadness, etc.) than are tiny, simple brains. We made this case to Animal Behavior, suggesting that the death of 100 tiny crustaceans (average size < 1 cm) at the hands (claws, actually) of predators was justified by what we had learned about the benefits of autotomy. The mention of size drew scorn from the corresponding editor, who claimed that it was irrelevant to animal care concerns. Clearly this is a difficult ethical issue that merits further discussion! Our sense, in any case, is that the amount of gained knowledge needed to justify cutting up a sponge or overheating zooplankton on microscope slides, say, is much lower than what is required to justify hurting a monkey or a dolphin. Certainly this has been an unstated rule of thumb: embryologists routinely kill thousands of urchin embryos, for instance, while no primate researcher would consider killing even one baby gorilla. But without some sort of metric of levels of acceptable suffering, such views are completely subjective.

For the second part of the recommendation in the "Guidelines"[1] regarding adverse consequences to populations, a similar sliding scale might be useful. In this case, the focus is on population traits, not pain, and the discussion should probably be expanded to include organisms other than animals. For instance, from a conservation standpoint, killing a large redwood might be much worse than killing a laboratory-bred mouse (even though the reverse would be true from the point of view of sensation of pain). Likewise from a conservation standpoint, harming a rare salamander would be worse than a guinea pig, even though the latter has a more sophisticated nervous system. Mortality rates in nature might also be considered – a higher bar of justification might be required for killing something that is likely to have lived a long time without intervention than something that has an expected lifespan of hours, days, or weeks. Community consequences of removing the individual could also be discussed. Again, most researchers apply common sense rules of thumb that encompass these points, causing what they consider inconsequential harm to populations even for small studies, class projects, etc., and only carrying out more substantial harm to populations when they feel it is strongly justified by the importance of the scientific question.

Perhaps it is time to hold an open forum on these issues, and to develop more explicit criteria that will provide clarity to researchers and editors alike. Our suggestion would be that the level of knowledge required to justify harm to organisms with simple nervous systems or those with abundant, short-lived individuals should be lower than that required to justify harm to organisms with complex nervous systems or those with rare or long-lived individuals. However, there will inevitably be a difficult gray area between these extremes. In any case, discussion of such issues should make the process more transparent and fair to all involved. Once developed, the criteria should be applied evenly to similar species across studies, whether they are target organisms or food sources. We suspect, for instance, that if we had carried out a study of Dungeness crabs and had mentioned in the methods section that we provided them five porcelain crabs a day as food, the welfare of the porcelain crabs would not have been subject to strong scrutiny, even though the suffering by the crabs would have been the same, with similar predation, autotomy, and death rates. If animal care committees of journals decide to regulate invertebrates, they should do so fairly across the board, including food species used for larger predators.

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Comments and debate on the issues surrounding the treatment of different animal taxa raised in the article above can be directed to the Editor of the Newsletter.

Workshop Reviews

The ISBE conference in Montreal this past summer was a venue for discussion on a variety of issues. Three of these topics were formalized into workshops, and the following reports are synopses of those discussions.

The function of avian duets: defining hypotheses, discussing advances and clarifying future directions

Many tropical birds coordinate their vocalizations in the form of strikingly precise duets. Despite numerous hypotheses, there is no current consensus about the function of these remarkable acoustic displays. Development of new experimental techniques and a resurgence of interest in avian duets, as exemplified by a burgeoning number of studies of individually-marked duetting populations, set the stage for a meeting of researchers interested in avian duetting. A half-day workshop involving 23 participants was held on July 7, 2002, in conjunction with the 9th ISBE congress, with the goals of formally defining duets and hypotheses for their function, discussing theoretical and experimental advances, and clarifying future directions in this field of research.

The identity of duet contributors is an important feature of classical definitions of duets. Previous reviews suggested that duets are formed by males and females of a mated pair or extended family group (Thorpe 1972, Farabaugh 1982), but participants agreed that the definition of duetting should be revised to include species that produce coordinated vocalizations outside of an established male-female pair. Emily DuVal (UC Berkeley, Berkeley, USA) highlighted the coordinated acoustic displays of unrelated male-male pairs of lance-tailed manakins (*Chiroxiphia lanceolata*) and Amy Rogers' observed that female eastern whipbirds (*Psophodes olivaceus*) readily duet with neighbors and strangers as well as their social mate. Participants agreed, however, that counter-singing contests between territorial neighbors should not be confused with duets. Descriptions of duets studied by participants highlighted tremendous structural diversity; duets vary in the degree of overlap and synchrony, in complexity and length, and there is variation among species in the sex of the duet initiator, in the similarity of the male and female contributions, and in the frequency with which duets occur relative to solo song. Participants agreed that a working definition of duetting must be broad enough to encompass this diversity and emphasize the temporal properties of the duet rather than the identity of the participants.

Michelle Hall argued that progress in the study of duet function has been impeded by a large number of poorly defined hypotheses for the adaptive significance of duetting. When evaluating these hypotheses, Michelle emphasized the necessity of assessing the costs and benefits of participating in a duet to each

individual, rather than the pair. She stressed the importance of distinguishing between *why both birds sing* and *why they coordinate their songs into precise duets*. Michelle tabulated twelve hypotheses for the function of duets, defining them in terms of whether they suggest a cooperative or conflicting function, whether they feature intra- or inter-pair signaling, and what type of information is conveyed by the duet responder.

Laura Molles (University of Waikato, Hamilton, New Zealand) highlighted the importance of quantifying characteristics of duets (for example, Susan Farabaugh's 1982 methods) for elucidating duet function. In addition, she suggested that thorough investigations of the social and ecological context of duets can provide insight into why some animals produce coordinated vocalizations, in particular by comparing differences in patterns of dispersal, territoriality, and mate attraction between duetting and non-duetting species. Participants agreed that the formation of exclusive, testable predictions based on measurable differences observed in duet structure and duet context would provide a useful expansion of Michelle's theoretical outline.

Nigel Mann, (St Andrews University, St Andrews, Scotland) presenting a written report *in absentia*, highlighted the importance of considering evolutionary history and argued that comparative studies remain a virtually unexplored source of information on duetting. Nigel described his current study which involves mapping vocal characteristics of *Thryothorus* wrens onto a molecular phylogeny, identifying points of transition towards more complex duets, and comparing which ecological and social factors are associated with these transition points. Participants agreed that rigorous comparative studies are an important tool in the study of duets and their function.

Amy Rogers discussed the use and interpretation of removal experiments performed both in isolation and in combination with playback experiments. Amy demonstrated how removals can be used to test predictions of hypotheses relating to joint territory defense and to elucidate the role of duets in mate attraction and pair formation. The length of time birds were removed from the territory was identified as an important factor; participants discussed the necessity of weighing potential information gained against the ethical and practical issues presented by longer term removals.

Daniel Mennill discussed innovative playback and recording techniques that could be employed in the study of duetting animals. Daniel presented a playback design using two-channel playback stimuli to mimic duet contributions through the left and right channels of stereo speakers, which would allow tests of predictions not possible with conventional single-speaker playback. David Logue (Colorado State University, Fort Collins, USA) is piloting the use of a two-speaker design in the study of duetting wrens. In other methodological advances, participants discussed the utility of interactive playback as a tool for evaluating duetting, and microphone array recording as a tool for investigating the contact maintenance and territory defense functions of duetting.

In summary, the workshop revised the definition of duetting, outlined a new theoretical framework, and described innovative experimental techniques that will facilitate the study of duets and their function. Participants discussed the need to identify formalized, exclusive predictions for hypotheses, to obtain more information on the social context of duets, and to conduct comparative studies. The importance of a broader perspective on duetting was also highlighted. For example, the need for research on proximate questions such as ontogeny and mechanisms of duetting, and the relationship of definitions, hypotheses and functions of avian duets with duetting in taxa such as mammals, frogs and insects.

Acknowledgements

We would like to extend our thanks to all those who attended and contributed to the workshop in Montreal, in particular to Laura

Molles and Nigel Mann for presenting and providing additional topics for discussion. We also thank the organizers of the 9th Congress of the ISBE at the Universite du Quebec for providing a meeting room and technical support.

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Discussion on Conservation, Movement and Habitat Selection

Preamble. We organized this workshop for two reasons. First, we wanted to identify common interests pertinent to conservation, movement, and habitat selection among diverse taxonomic and conceptual foci. Second we hoped to provide a springboard for potential subsequent meetings or symposia concerning these topics. The workshop was advertised prior to and at the conference and 55 people attended the lunch time discussion. Prior to the meeting, we circulated a list of questions for discussion to those who planned to attend. These questions were fairly narrowly focused on movement and habitat selection issues and we realized at the meeting that participants were interested in a wider range of questions, and that more people were participating than we'd anticipated. Accordingly, we attempted to broaden the topics for discussion. The original guiding questions are available from Colleen (cstclair@ualberta.ca) if you are interested.

Opening. Judy began with a 10-minute overview intended to introduce some of the reasons and ways that behavioral ecologists can make a contribution to conservation issues. She addressed three themes. The first considered the contexts in which behaviour is most relevant to conservation biology and focused on vital rates, distribution patterns, and assessments of management efficacy. The second suggested that gaps in our knowledge about conservation solutions sometimes stem from missing behavioral information or understanding. The third theme addressed the issue of scale by describing both spatial extent and configuration of habitat as important contributors to conservation issues related to animal movement and habitat selection.

Introductions. We followed these opening comments with a round of self-introductions which, owing to the size of the group, took much of our time. This portion of the discussion revealed a wide base of disciplinary and taxonomic interest in the specific topics of habitat selection, movement, dispersal, and the more general interface between conservation and behaviour. It also revealed interest among graduate students, faculty, and government biologists.

Discussion. To initiate the discussion, Judy posed the broad question: What are some of the untapped situations where behavioral ecologists can make some useful conservation contributions? The remaining minutes of the lunch are summarized here; detailed minutes of the comments are available from Colleen. Many participants expressed the need to work closely with managers to determine conservation issues relevant to them and their jurisdictions. Some noted the untapped potential of working outside of protected areas, on topics or species which are not of direct concern to

conservation, and others suggested that behavioral ecologists might be able to anticipate both problems and solutions that managers had not. We considered that in the process of suggesting and implementing behavioral research relevant to conservation contexts, it is important to acknowledge the population-level dependent variables that are emphasized in most management plans.

The need for effective communication with managers sparked a discussion about the ideal ways of sharing information in the published literature. Participants highlighted the existence and role of two new media. The first is *Conservation in Practice*, a non-peer-reviewed magazine published by the Society for Conservation Biology that is intended to get information into the hands of managers in a timely and readable fashion. This journal summarizes recent scientific studies, synthesizes bodies of research, reviews recent books and publishes both original and reprinted opinion essays. A second journal, *Conservation Ecology*, is a peer-reviewed online journal, free to any subscriber via the internet, that explicitly recognizes the value of behavioral and other innovative approaches to conservation problems. We also learned of the Animal Behaviour Society's initiative to use its website to provide managers with access to behavioral expertise pertinent to their particular problems.

Finally, we discussed the fact that many behavioral studies have untapped potential to be scaled-up to conservation contexts. Realizing this potential will require that behavioral ecologists think broadly about the applications of their work, talk directly and often to managers who do not have the time or training to think comprehensively about the role of behaviour in research or solutions, and communicate their findings in ways that reach both behavioral ecologists and conservation practitioners.

Summary and Follow-up. Judy ended the discussion session by asking participants to continue to think about which topics are important to the interface between behaviour and conservation generally, and to focus on movement and habitat selection specifically. She also asked whether participants thought it would be profitable to organize a meeting on these kinds of topics, which issues such a forum should address, and how it should be organized. Since the meeting, we have discussed these questions at greater length. With the assumption that behavioral ecologists can make the biggest contribution to conservation biology by focusing on behaviour that potentially has the greatest impact on population parameters, we suggest that it might be best to organize further discussion or symposia that include scientists interested in population patterns (e.g. population biologists) as well as scientists interested in behavioral processes by which they come about (e.g. behavioral ecologists). For this reason, we suggest that further discussion or symposia be held at meetings that could attract both behavioral and population ecologists; the annual meetings of the Ecological Society of America and the Society for Conservation Biology are two potential venues. However, such scientific meetings are unlikely to attract as many managers as is desirable, and so we have considered other ways to draw on the expertise and experience of these people in ways that might help direct conservation-related research in behaviour. One possibility is to construct a survey directed at managers, with which they can inform basic scientists about the range of problems they are handling, and their perception about ways that behavioral research might help them address these problems. Results of this survey might be made available in a venue such as *Conservation in Practice* that is readily available to both researchers and managers. These ideas are preliminary, and Colleen and Judy welcome the input of ISBE members, particularly those who attended or would have liked to attend, the discussion in Montréal. Again, comments can be sent to Colleen who will share them with Judy and other interested people.

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ISBE round table report: a new method to estimate repertoire size

A round table discussion entitled "Avian repertoire size estimation: comparing the complexity of bird song among individuals, population and species" was held at the last ISBE conference in Montreal. The objective of this round table was to present and discuss a novel method of repertoire size estimation. This method is based on an analogy with classical capture-recapture methods with song elements, such as syllables being the units of capture-recapture (see Garamszegi et al.[1] for more details, software is available).

The interest of this approach lies in avoiding the following problems: 1) most of the song studies aiming at estimating repertoire size do not detect all the elements of a given song repertoire and can not evaluate the missed fraction of repertoire size; 2) the probability that each element will be sung and detected may vary between individuals, populations and species, but also between song elements and conditions of recording; 3) different probabilities of detection among song elements can generate a number of negative biases with unknown amplitude, 4) unknown missed fraction of repertoire size may give false results in comparisons of repertoire size if the missed fraction differs between the units of comparison (individuals, population, species).

In this round table, we presented a method incorporating variability in probability of detection. Following a typical sampling scheme, we presented how the proposed method can be practically used, notably to calculate detection probabilities of syllables and thus to correct for potential biases in detection. We also discussed the first results obtained with this method at individual and interspecifc levels and proposed other potential applications. First, using data from the pied flycatcher (*Ficedula hypoleuca*) we compared and discussed the differences between observed and capture-recapture estimated syllable repertoire sizes. Then, we showed how it is possible to detect temporal changes in the repertoire and to compare song composition among individuals by expressing the rate of gain or loss of song elements between two series of recordings. Second, we outlined the framework of a project that targets intraspecific song variation in blue tit (*Parus caeruleus*) populations. Third, we reported the preliminary results of a collaboration (twenty-six biologists involved) that focuses on interspecific variation of avian song complexity. The comparison of the capture-recapture estimated repertoire sizes with previously published data illustrated the need of standard sampling and the importance of methods that takes into account detection probabilities.

The limits of this method were also discussed. Capture-recapture models have been developed originally for estimation of the size of closed animal populations, thus some development may need to be made to tackle specific issues related to bird songs. Moreover, birds often organize their songs in a species-dependent way that may affect the selection of an appropriate model explaining the source of heterogeneity in detection probability. For example, species-specific song switching and repetition techniques require particular estimation procedures that may make interspecific comparisons difficult. However, the study we conducted at interspecific level suggests that when combined with adequate sampling capture-recapture methods may be an efficient improvement over the use of counts of song elements. The use of these methods may help when conducting comparative studies among species using data from descriptive studies, but tests of concrete biological questions both in intra- and interspecific levels await future investigations.

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www.mbr-pwrc.usgs.gov/software/capture.html
www.mbr-pwrc.usgs.gov/software/comdyn.html

Contributing to the ISBE Newsletter

The ISBE Newsletter publishes Book, Conference and Workshop Reviews of interest to the *International Society for Behavioral Ecology*.

Book Reviews: Persons involved in the publishing of books who would like these to be considered for review in the Newsletter may contact the Editor and arrange for their publisher to forward a review copy to be forwarded to this office. Authors may submit a list of possible reviewers. Alternately, members who wish to review a particular text should contact the Editor.

Workshop/Conference Reviews: Workshop/ Conference reviews should be prepared in one of the following two formats. Brief synopses (max 1000 words) may be submitted by either participants or conference organizers at the regular newsletter deadlines. These can include synopses of workshops that will be published in more detailed accounts (book or special journals), and should include information as to where the information will published. Longer reports (max 2500 words) will be considered from large workshops/conferences for which other publications are not stemming. The purpose of the latter format is to provide a venue to disseminate information and discussions that would otherwise not be available to non-conference participants. Anyone attending such a workshop and wishing to publish in the Newsletter should contact the Editor at least ***one month*** prior to submission deadlines. Reports should aim at a critical assessment of the conference, as well as a synthesis of the convergent ideas presented. A synopsis of future directions of research that were reached at the end of the conference should also be included. Anyone attending the workshops may submit reports, but preference will be given to submissions not be authored by conference organizers. A single application for a workshop will be considered, so it may be appropriate to agree upon a reporter at the conference. Graduate students and postdocs are strongly encouraged to consider contributing to writing these reports.

Commentaries: Responses to commentary articles published in the newsletter or articles eliciting discussion or topics relevant to the society will be considered for publication in the newsletter. Authors of such articles should contact the Editor at least *one month* prior to regular submission deadlines to outline the content of the article. The Editor may request submission of the article earlier than regular deadline should outside reviewing be deemed necessary.

Cartoons: Cartoonists are encouraged to submit artwork , either in hardcopy, or as TIFF or high resolution (300 dpi) GIF files. All cartoons published in the newsletter will be credited to the illustrator, and will appear on the Newsletter's website (*web.unbc.ca/isbe/newsletter*).

Deadlines for submission to the spring newsletter will be March 15, 2003.

Ken Otter
Newsletter Editor