Supplement to Behavioral Ecology

International Society for Behavioral Ecology

ISBE Newsletter

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Editorial

This issue contains another suite of book and workshop reviews. I would like to thank all the contributors who volunteered their time to submit these, as the quality of contributions continues to be excellent. In addition, there are a few other highlights to the current Newsletter to which I would like to direct members.

First, congratulations goes out to one of the society's members, Innes Cuthill, for receiving one of two Nature/NESTA awards for creative mentoring in science (see announcement in Society News on page 4). The results, recently appearing in Nature (2005, vol 434, page 421), announced that Innes was awarded the prize for a mid-career researcher who has shown exemplary conduct in mentoring. Next time you see Innes at a conference, make sure to give him a pat on the back.

In Fall 2004, Wendy King (ISBE Archivist) arranged to have all of the past issues of the ISBE Newsletter converted into pdf format for archiving on the Newsletter's webpage. A visit to the website (web.unbc.ca/isbe/newsletter) now provides anyone with a complete run of the newsletter from 1987 to the most recently published issue. As I posted these issues onto the website, my curiosity got the better of me and I spent several pleasurable hours perusing the content of these issues. I was amazed at the diversity of styles and iniatives undertaken by past editors, some of which have been retained by subsequent editors, and some of which appeared to have fallen by the wayside. I asked Wendy whether she would consider doing a retrospective of the newsletter's history to comment on these changes (for better or worse), and that historical look appears on pages 4-5. Now that this archive is available, I know it will be

Contents of this Issue

an invaluable resource for myself and future editors, as well as be of interest to the society's members. Hopefully, some of these forgotten initiatives can be resurrected.

Bob Montgomerie and Tim Birkhead have contributed a thought-provoking article on Scientific Misconduct to this issue (pages 16-24). Their treatment of this oft-taboo subject is both professional and direct, aimed at bringing the topic out of the realm of coffee-room gossip and into a forum of open discussion. Montgomerie & Birkhead have developed a survey designed to help define what constitutes misconduct (also available online at Queen's University); even if members don't formally respond to the survey, it should serve as a starting point for both reflection and debate of this issue.

During the 2004 ISBE meeting, I had members whowere willing to serve as reviewers for the Newsletter fill in cards indicating their address details and interests. This has worked nicely, and several of the reviews in this issue and in the coming fall issue were drawn from those responses. In the coming months, I will be putting a similar information form on the newsletter's webpage that interested parties can complete and email back to me. This will allow me to create a database of potential book reviewers that I can use as books arrive at my office; hopefully this will increase the representation of more members being called upon to contribute to the newsletter.

As a final note, the submission deadline for future issues will be Sept 1st and March 1st each year, as opposed to the 15th of these months. These new deadlines will help Oxford University Press in their goal of getting journal and newsletter out earlier to members.

Ken Otter
Newsletter Editor

CONTRIBUTIONS TO THE ISBE NEWSLETTER

The ISBE Newsletter publishes Book Reviews, Conference and Workshop Reviews and Commentary Articles of interest to the *International Society for Behavioral Ecology*. *The ISBE Newsletter will only consider work that is not already published or intended to be submitted for publication elsewhere.*

Book Reviews: Reviews are generally solicited by the Editor as new books arrive at the office, and are deemed to be of interest to the society. 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 this office. Authors may submit a list of possible reviewers. Alternately, members who wish to review a particular text should contact the Editor. The Editor will provide reviewers with instructions and a style sheet. Reviews are typically 1500 Words.

Workshop/Conference Reviews: Workshop and/or Conference reviews should be prepared in one of the following two formats. ***Brief synopses*** (max 1500 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 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 on topics relevant to the society will be considered for publication. 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 and other artists 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 fall/winter newsletter will be 1 Sept 2005.

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Society News

Most Society News – workshops, conferences and job postings – is now publicized on our website (web.unbc.ca/isbe/newsletter). This allows ads and announcements to be posted shortly after receipt so that deadlines falling between newsletter distributions can be advertised. If you would like to advertise workshops, conferences or job postings of interest to the society, contact Ken Otter (otterk@unbc.ca) for posting.

CHANGES TO SPOUSAL MEMBERSHIP PROGRAM

Spousal memberships, whereby individuals could pay a nominal fee to join the society without subscribing to the journal if their spouse was also a society member, has been replaced. Everyone now has the option to join the society without taking a subscription to the journal. Such memberships will receive the Newsletter and announcements for the biennial conference. Information on this process is available on the society's (web.unbc.ca/isbe/ISBEmembership.htm) and Oxford University Press' *Behavioral Ecology* webpages (beheco.oupjournals.org).

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).

ISBE 2006 CONFERENCE

The 11th Congress of the ISBE will be held in Tours, France, 23-28 July 2004. Details can be found at **www.isbe2006.com.**

NEWS OF MEMBERS

Innes Cuthill received one of two Awards for Creative Mentoring in Science given jointly by the journal *Nature* and the UK's National Endowment for Science Technology and the Arts (NESTA). *Nature*/NESTA solicited nominations from students from all scientific disciplines, and several of those for Innes' nomination are cited. Only two awards were given among all scientific disciplines in the UK. Innes is an active member in the society, and served as editor for *Behavioral Ecology*. Congratulations, Innes!

WORKSHOPS AND OTHER MEETINGS

(more detailed information is available on the website)

IEEE Swarm Intelligence Symposium

The second in the series of IEEE International Symposia on Swarm Intelligence will be held 8-10 June, 2005 in Pasadena California

http://www.ieeeswarm.org

International Ethological Conference

Budapest, Hungary. 20-27 August 2005.

http://www.behav.org

Measuring Behavior 2005

The 5th International Conference on Methods and Techniques in Behavioral research, will be held in Wageningen, The Netherlands, 30 August - 2 September 2005.

http://www.noldus.com/mb2005/invitation.html

The 24th International Ornithological Congress will be held in Hamburg, Germany, 13-19 August 2006. The scientific program committee has been formed and a web page is in place:

http://www.i-o-c.org/

The Evolution of Sexual Size Dimorphism

Workshop organized by Wolf Blanckenhorn, Tamas Szekely & Daphne Fairbairn.

21-26 August 2005, Switzerland

http://www.bath.ac.uk/bio-sci/szekely/workshop/SSD%20Workshop2%20webmod.htm

A 19-year Retrospective Look at the ISBE Newsletter

The ISBE Newsletter has been edited by 6 different teams who have mostly held the reins for 2 or 3 years. In recent years, Bart Kempanaers and Ken Otter have each held the post of newsletter editor for 4.5 years. Three editors (Donald Kramer, Paul Schmid-Hempel and Ron Ydenberg) have gone on to serve as editors for *Behavioral Ecology*. Paul Schmid-Hempel also served as society treasurer and helped organize the Zurich meeting while Ron Ydenberg and Donald Kramer co-hosted the Vancouver and Montreal meetings, respectively. Martin Daly was the first society treasurer. It seems these newsletter editors are a hard-working bunch! Although each editor gives the newsletter his/her personal stamp, it is worth pointing out that 4 of the 7 editors were Canadian and only one (Nancy Thornhill) was a woman.

Certain characteristics of the newsletter have remained constant. The most noticeable are that plans for future meetings and reports on past meetings have been well documented. Our biennial conference is vital for communication of recent research findings among members and is the event that encompasses executive and general business meetings of the society. The newsletter functions to keep the membership (especially those who can not attend conferences) informed of society news. Calls for nominations to the executive council have usually been distributed prior to each meeting, helping ensure that we have a democratic society. Since the journal editors, conference organizers and newsletter editors are all appointed by the executive council, it is important that nominations come from the membership at large. Regular reports from the journal editors have kept members up–to-date on submission rates, acceptance rates, publication lags, citation rankings, contract negotiations with Oxford University Press and the financial situation, etc. Announcements of job opportunities and meetings of interest to behavioral ecologists have also been present in the newsletter from the start.

In the formative years of the society from 1987-1988, Donald Kramer requested suggestions for a society, for a possible new journal and for future meetings. He sent out a call for members and the tentative constitution. From 1988-1990, Martin Daly continued to report on progress with journal plans and included both general business and executive meeting minutes. The journal *Behavioral Ecology* was launched via the newsletter in April 1990 which listed instructions for authors. In addition, a treasurer's report and the first journal editors' report were published.

For the years 1991-1992, the team of Nancy Thornhill and Paul Schmid-Hempel transformed the newsletter from a strictly informative vehicle to a wider publication by instituting an editorial section and soliciting articles of correspondence, contributions and opinions. For entertaining reading, see Shykoff (1991) "Female behavioural ecologists respond to novel male traits" in Vol. 3, No. 1 that was a report on the Uppsala meeting. It was also during these years that the process of electing executive members via a newsletter ballot was initiated.

From 1993-1996, Ron Ydenberg started a president's message but seemed to prefer to omit the editorial. The archives were established and missing material was collected. A poll was conducted concerning the organization of biennial meetings, the result of which was published in February 1994. A new section for members' news was started but, unfortunately, this has since been mostly reserved for obituaries. Items of a housekeeping nature, such as voting on changes to the constitution and establishment of a mailing list, also appeared. Lists of the executive council were standardized with useful address coordinates. The first Frank Pitelka award was announced, to be given to a young, promising researcher who has published in *Behavioral Ecology* in the last 2 years.

For the years 1996-2000, Bart Kempenaers reinstated the editorial section of the newsletter while retaining the president's message. His first issue announced the inclusion of book reviews and 'Forum' articles on timely issues in behavioral ecology to free up space in the journal for research papers. Vol. 12, No. 2 contained a particularly controversial article on fluctuating asymmetry by Palmer & Hammond (2000). A simultaneous request for drawings and photographs to accompany articles seems to have gone unheeded. General business minutes from biennial conferences were once again included after a 6-year hiatus. The last issue of the newsletter during this time announced the William Hamilton Memorial Lecture to be held at ISBE conferences.

Starting in 2001, Ken Otter aimed to start up a 'Student Forum' but this endeavor seems to have morphed into student reviews of workshops and conferences. The book review section has become very successful with an average of 5 reviews per issue. Bart Kempenaers's call for newsletter illustrations was finally answered by Ken Otter who started including cartoons and after 2 years, artists other than the newsletter editor also appeared. Only one photo has been printed and I would encourage more to be submitted in the future. Ken Otter's most evident innovation has definitely been creation of the website at the University of Northern British Columbia in October 2001. Newsletters have since been published electronically as well as in printed form and notices of grants and job postings no longer appear in the newsletter but only on the website.

In reviewing the past newsletters, I was struck by how frequently and consistently the membership was consulted for direction in all areas of the society. Now that we have 35 newsletters posted on the world wide web, it is possible for those interested in forming a new international society or in setting up a new scientific journal to see how it can be accomplished (or at least who to contact for advice!). When our society was first envisioned almost 20 years ago by Jerram Brown, Thomas Caraco and Christopher Barkan in Albany, NY, I doubt that they dared imagine such success. The newsletter has played, and continues to play, a pivotal role in the ISBE and we owe each of the editors a round of applause. Bravo!

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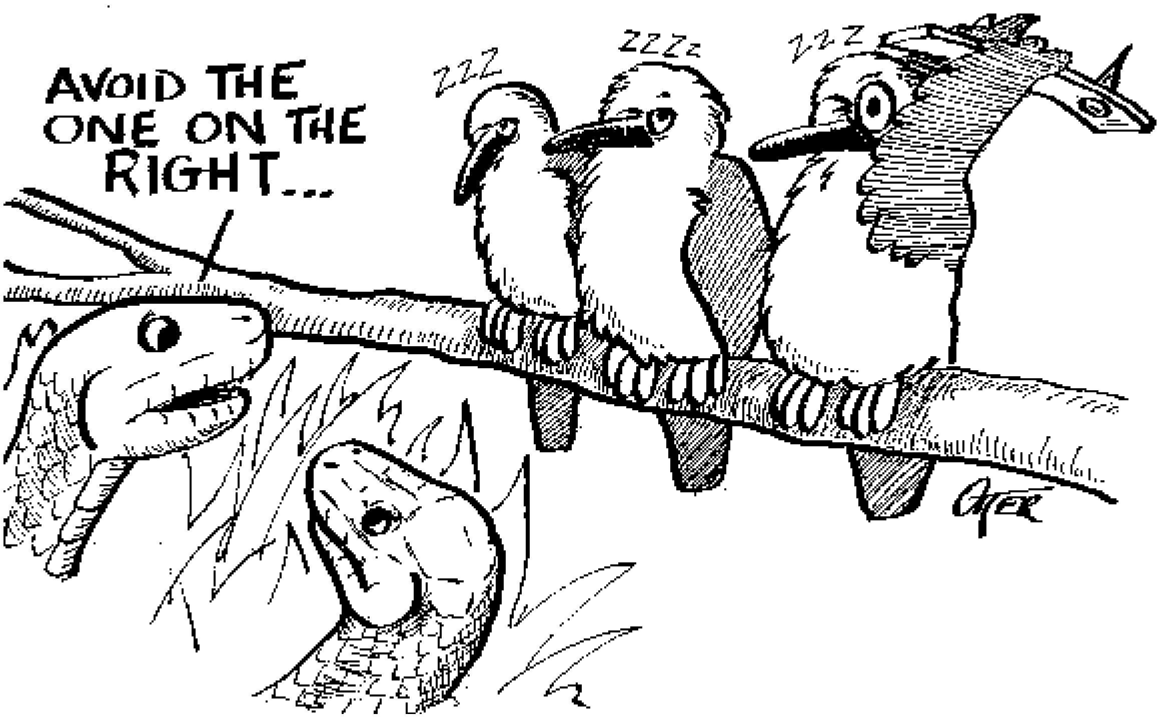

Book Reviews

Communication, Squabbles and Parent-Offspring Conflict

More Than Kin and Less Than Kind: The Evolution of Family Conflict.

Doug W. Mock. Belknap Press, 2004. 288 pages.
ISBN 0-674-01285-2 (hardcover).

The publication of this new book might come as a surprise if you thought that Doug Mock had said it all in his earlier, more technical, volume with Geoff Parker (Mock & Parker 1997). However, here he presents a very readable popular science version of the often complex field of parent-offspring conflict. Although there is clearly a lot of overlap in subject matter between the two books, they are very different in their aims and their target audiences. This recent work is a wonderfully well-written and entertaining book that explains everything from first principles. In the style of the best popular science writers, such as Matt Ridley, this book exposes the elegance of the theories and the neatness and wonder of empirical discoveries without overly taxing the reader with technical jargon and obtuse game theoretical logic. There is also the fun of reading first-hand accounts of Doug's innovative field research projects, as over the years they lurch from inspiration to desperation, and from possible disaster to eventual enlightenment.

For those of us working in the field of parent-offspring conflict, this is definitely a non-specialist book to recommend to our students, colleagues, friends and relatives. At last, they might understand why we spend half our time pouring over complex mathematical modeling papers and the other half of the time out in all weathers getting covered in droppings from various species of baby bird! From the first photograph of the author at a very young age with his three older brothers (who taught him all he needed to know about dominance hierarchies in the nursery), to the very last picture of Doug with his pet dog, this is a pleasantly personal account and the recognizable voice of the author is present throughout. This is a review of much of the past research in this field and is presented in an admirably simple and engaging way. I have to admit that whilst reading I often found myself scribbling additions to my undergraduate lecture notes each time I came upon a better way of explaining a theory or presenting some experimental result. It is also the sign of a good popular science book when, as a scientist, one does not find oneself getting more and more annoyed as it gets closer and closer to one's own area of research. I am glad to report that I had no such problems in this instance.

Obviously, there are always bits of a book like this that one could quibble with, such as the lengthy description of inbreeding depression near the beginning, which I couldn't really find a use for in this context. However, all is forgiven when you read one of the many amusing stories or amazing facts that pepper the pages of this book. For example, did you know that parent wood storks can fish just as successfully when blind-folded, or that young sand tiger sharks actually cannibalize one another *in utero* and that this was discovered by Stewart Springer who got bitten by one during the first oviduct dissections of this species? My favorite popularization of an evolutionary problem concerns the issue of who controls food allocation in the nest: "as Stalin once observed and the 2000 U.S. presidential election demonstrated, it doesn't matter who gets to vote; what matters is who counts the votes".

As the author often likes to do, each chapter begins with an apt and amusing quote. The best of these from this book has to be: "When the only tool you own is a hammer, every problem begins to look like a nail." (Abraham Maslow). Surely this isn't a sneaky dig at those people who have perhaps concentrated too much on Godfray's (1991) handicap model of begging as a costly honest signal? This particular chapter then goes on to detail some of the best bits of work on chick begging that have appeared during the last 10 years or so. The literature concerning exaggerated begging strategies in brood parasites is excellently summarized. I was surprised, however, to see no discussion of studies like that of Briskie et al. (1994) demonstrating a similar negative effect of relatedness (in terms of extra-pair paternity) upon the intensity of begging calls across species, which I've always considered one of the best pieces of evidence linking us back to the signaling models. No doubt the answer lies not in emphasizing signaling at the expense of sibling competition, nor the opposite, but rather in understanding how the two combine to produce the array of systems we see in nature.

This book repeats the familiar plea for a proper investigation of offspring "need", which is crucial to understanding everything from nestling gape color to facultative siblicide. It is certainly curious that studies exploring the physiology and psychology of offspring nutritional need seem to be a long time coming. Importantly, we still lack a developmental context for the concept of offspring need, and a usable theoretical framework regarding time-scales for any fitness returns (i.e. short-term hunger versus long-term growth?). A related, but less often heard, request could be for greater understanding of parental "responsiveness" to offspring signals. This is the equivalent of direct parental intervention in sib-sib aggression, includes patterns of parental allocation that are not simply determined by scramble competition between offspring, and could therefore encompass any parental favoritism both within and between broods. As with offspring need, parental responsiveness depends upon dynamic changes in physiological state and the psychology of the individual in assessing their own condition versus the need of various offspring. Parent-offspring conflict thus concerns not only the evolution of offspring strategies aimed at fulfilling their needs, but also parental strategies concerning how best to respond to such need. Rather as the study of sexual selection has historically been more about showy males than the female preferences that drive such systems, research in parent-offspring conflict has perhaps concentrated too much upon competitiveness and signaling in offspring instead of exploring more deeply the nature of parental responsiveness.

It would clearly be unreasonable to expect a volume of this nature to offer any new synthesis within such a rapidly developing field. This is very much a book about our discoveries so far, although in doing so it does expose some of the major gaps in our knowledge and understanding that we still need to fill. And if and when we do get there, then I hope that someone will write as engaging a book as this about it all.

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References

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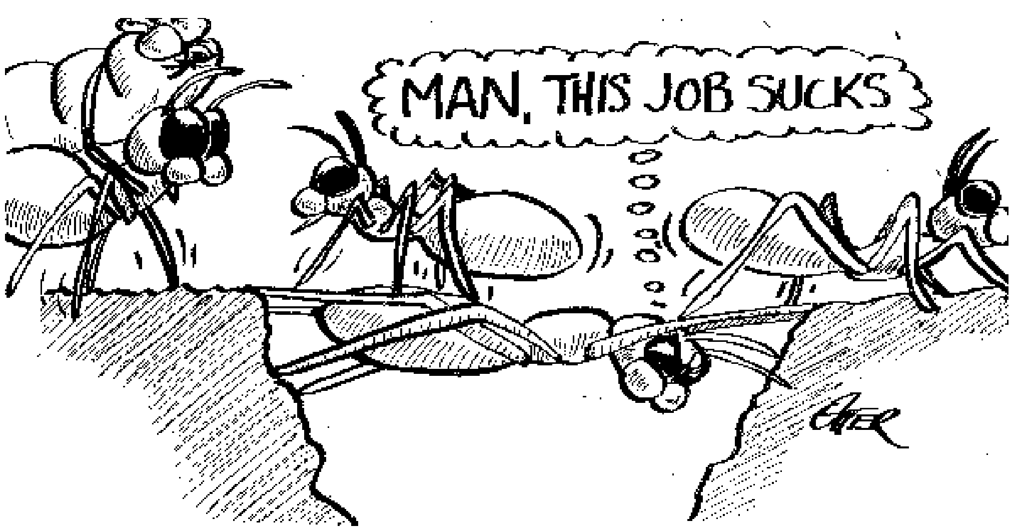

EUSOCIALITY BREAK-DOWN

Ecology and Evolution of Cooperative Breeding in Birds

Walter Koenig & Janis Dickinson (eds). Cambridge University Press, 2004. 293 Pp.
ISBN 0 521 82271 8 (hardcover), ISBN 0 521 53099 7 (paperback)

Fourteen years after the publication of Stacey & Koenig's (1990) work on cooperative breeding (CB) in birds, Koenig & Dickinson present us with a very updated review of some of the major developments in this fascinating phenomenon in birds and, with less emphasis in this book, mammals. This edited book includes 14 chapters authored by some of the most active researchers in the field, and a list of more than one thousand references, 2/3 of which being post-1990. For those who, like me, 'feared' a traditional update of single species accounts, the editors provide a pleasant surprise with "a thematic volume based on major concepts and issues" rather than "a follow-up compilation focused on individual species".

JD Ligon and DB Burt open the series with a chapter on *Evolutionary origins* of CB. By using modern phylogenetic-based approaches they pinpoint two important issues in the comparative analysis of CB: distinguish between homologies and analogies, and separate origins from maintenance of specific traits. An important contribution of their analysis is their emphasis on altriciality as an ancestral condition favouring the evolution of CB. They also make an interesting methodological contribution suggesting that if CB is ancestral among altricial birds (as suggested by their and other works) then current ecological correlates of CB should be studied by comparing CB species and closely related species that have recently lost CB. The chapter concludes with three appendices that provide the most updated list of CB species available. Ch. 2 by J Ekman et al. deals with *Delayed dispersal*. Delayed dispersal remains an important, although not essential, condition for CB. They focus their analysis on the benefits and costs, for both parents and offspring, of offspring delayed dispersal stressing benefits of extended parental care and territory quality-dependent costs of resource sharing. The *Fitness consequences of helping* are discussed in Ch. 3 by JL Dickinson and BJ Hatchwell. After reviewing the current functions of helping (summary in Table 3.1), they proceed with an analysis of the empirical difficulties of uncoupling group size and helping effects and also uncoupling staying and helping. Some interesting contributions are made with the concepts of *kin neighbourhoods* that allow offspring to join relatives during the non-breeding season and kin-biased helping acting "as a positive feedback loop, potentially resulting in habitat supersaturation…defined here as an excess of individuals beyond the number that would be supported if young were unable to remain in or return to their natal group". I was especially pleased with their emphasis on individual behavioural plasticity, a common ontogenetic feature of most social vertebrates. RG Heinsohn addresses the issues of *Parental care, load-lightening, and costs* in Ch. 4. Costs and benefits of parental and alloparental care are analysed using a simple life-history model that considers both the breeder and the helper's perspectives. Although the model is limited to load-lightening effects, the inclusion of a more complete matrix of cost/benefits for the partners in the interaction would have improved the predictive power of the model: e.g. costs of helping for the breeder, such as attraction of predators. Such costs can explain cases where breeders actively repel subordinates from the nest area with consequences for load-lightening.

In Ch. 5, A Cockburn analyses *Mating systems and sexual conflict* in CB species. This is a challenging and thought provoking chapter. Cockburn's major focus is to analyse patterns of sexual conflict within complex CB societies, in doing so, he introduces a new classification of mating systems found among CB species (Table 5.1). Although he rightly stresses aspects of female choice relevant to sexual-conflict theory that should be considered in any model of CB, avoidance of costs of parasite transmission are surprisingly ignored. His rather pessimistic view about the unlikelihood of compressing "the diversity of cooperatively breeding birds into a single model" is nicely rebutted by his own Table 5.2 which, to me, suggests that classification of mating systems among CBs could be based on scales of interspecific means and standard deviations for percentage of polyandrous broods.

Sex-ratio manipulation is discussed in Ch. 6 by J Komdeur. After introducing the classic sex-allocation theory he summarises and discusses sex allocation biases among CB species (Table 6.1), with special emphasis on cases of facultative control of offspring sex ratios (Table 6.2). Special attention is given to the mechanisms of sex ratio (especially primary and secondary) bias, concluding that there does not seem to be a single mechanism common to all CB species. I personally missed a discussion of the potential role of maternal hormones on sex ratio bias at hatching. MA Du Plessis discusses *Physiological ecology* of CB in Ch. 7. This is an important area for future research that remains grossly underdeveloped: role of proximate physiological mechanisms in evolution and maintenance of group living and helping. He correctly organizes the chapter into physiological correlates of CB during the non-breeding and during the breeding season, stressing the importance of physiological benefits of group living in the non-breeding season. *Endocrinology* is an area of rapid growth in studies of the proximate mechanisms of CB as indicated by SJ Schoech et al. in Ch. 8. This is a field that allows relatively easy experimental manipulations through "phenotypic engineering". The chapter emphasizes the role of sex steroid hormones, glucocorticoids and prolactin as endocrine factors affecting reproductive, parental and alloparental behaviour, and how behaviour in turn can affect reproductive physiology through endocrine mediation.

In Ch. 9, WD Koenig and J Haydock review *Incest and incest avoidance* mechanisms among CBs. After reviewing evidence for incest and inbreeding depression in CBs and concluding that both are rare, they list a series of incest avoidance mechanisms suggested for CB species. Several specific cases are analysed in some detail. RD Magrath et al. contribute with an exceptionally well written introduction to *Reproductive skew* theory in Ch. 10. This chapter is a must reading for all those who are interested in a general theory of CB. The major models are clearly explained, including synthetic models and, above all, models that incorporate female choice and sexual conflict are also considered. The difficulties of empirically testing the models are also analysed and useful suggestions are made. *Joint laying systems* are reviewed by SL Vehrencamp and JS Quinn in Ch. 11. After providing some taxonomical and life-history correlates of joint nesting and devoting ¾ of the chapter to detailed accounts of joint-nesting taxa, the reader is rewarded with the final 5 pages that include stimulating evolutionary insights and applications of reproductive skew theory to the understanding of joint nesting. JR Walters et al. discuss the *Conservation biology* of CB species in Ch. 12. Their preliminary analysis does not indicate that CBs are over or under-represented among endangered or threatened species and suggest that some aspects of habitat fragmentation may affect viability of CB species that have short-distance dispersal. Surprisingly, potential impact of parasites and diseases on population viability of CBs are not mentioned.

In Ch. 13 AF Russell reviews *Mammals: comparisons and contrasts*. The chapter is structured around the three basic questions of CB and provides a good basis for comparisons between avian and mammalian systems. S Pruett-Jones concludes the series with a *Summar*y Ch. 14 where he drives some general conclusions through 13 "summary statements…that all researchers working on cooperative breeding in birds, or at least the majority, could agree on". I only regretted the exclusion of statements regarding proximate physiological and endocrinological mechanisms (Chs. 7 and 8).

This book is an essential reference for advanced undergraduate and postgraduate students and, above all, researchers interested in CB. It provides state of the art accounts of some major evolutionary issues concerning CB and strong guidance for future developments in this field. Special consideration should be given to the development of more sophisticated synthetic models of CB based on reproductive skew and mate choice. Some areas that, in my opinion, should be given high priority in future studies of CB, but, sadly, are ignored in this book are: (a) cognitive capabilities and neurobiology, (b) immunoendocrinology, and (c) parasitology (brood, ecto, endo). The future looks bright for CB research!

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Biology and Conservation of Wild Canids

David W. Macdonald & Claudio Sillero-Zubiri (eds.) Oxford University Press, 2004. 450 Pp.
ISBN 019-851555-3 (hardcover), ISBN 019-851556-1 (paperback)

Biology and Conservation of Wild Canids stems from the Canid Biology and Conservation Conference, held by the IUCN/SSC Canid Specialist Group in September 2001. This book and a second written output of the conference, the Second Canid Action Plan (Sillero-Zubiri et al. 2004), are 'sister' (and complementary) outcomes; the book is an insight into the scientific research on wild canids over the past 25 years, whilst the action plan is a practical tool aiming to shape and guide future studies and conservation effort. It seems that one publication could not go without the other on your bookshelf.

The book is divided into three parts. The first reviews a variety of topics that summarize the current research trends on wild canids, the second focuses on selected case studies, and the third aims to give 'a conservation perspective' to the whole book. The reviews span a range of topics from basic aspects of canid biology (geographical distribution, sociality), through to evolutionary history, phylogeny and systematics, population genetics, management conflicts with human populations, canids diseases and techniques available to limit/increase/monitor canids. Each chapter stands as an independent review with no apparent link between chapters. In this first part, Chapter 3 on population genetics was very interesting to read, including up to date genetic studies on wild canids that use the complete array of molecular genetics techniques and a very good selection of case studies where genetics has been deployed for conservation actions. As for instance, in the case of swift foxes (*Vulpes velox*) and kit foxes (*V. macrotis*) which genetic work finally aided to recognise as separate species and prompted researchers to initiate conservation measures. Similarly in the case of Darwin's fox (*Pseudalopex fulvipes*), genetic studies showed that the population found on Chiloé Island (off the west cost of Chile) was not introduced there by humans, it is genetically distinct and essentially the progenitor of the mainland South American fox species. As a result, the Chiloé population became of immediate conservation priority. Recently the evolution of sociality in canids and in mammals as a whole has been pivotal to many behavioural ecology studies and it is also the focus of Chapter 4. However this chapter profoundly reflects the authors' ideas and not always exemplifies a balanced synopsis of other independently-performed research. For instance when illustrating cooperative breeding, reproductive suppression or dispersal examples provided by the authors fail to consider other independent studies on these topics. One of the hypotheses put forward to explain the evolution of sociality in canids is the Resource Dispersion Hypothesis (RDH). The case studies of the second part of the book are biased towards testing this hypothesis, without equal weighting to testing alternate hypotheses. This is a definite weakness of the book. Moreover, scepticism has recently arisen as to whether the RDH should be regarded as a hypothesis, rather than a theory, and therefore tested against other hypotheses (Revilla 2003); the unbalanced editing means only one chapter criticises the RDH.

Wild canids are amongst the taxa that most often come into conflict with human populations. Foxes, wolves, dogs and jackals predate domestic livestock, pets or wild game and all too often for the sake of their own survival, they are disliked (if not loathed) in turn by rural and city dwellers or hunters. They also can carry zoonoses; hence the perceived need to manage or control wild canids. All these issues are discussed in three chapters (Chapters 5, 6 and 7), which partially overlap in their contents and perhaps might have been condensed in one or two chapters without losing content. Thus, the first part of the book gives an overview of canid biology and management that is somewhat redundant and at least in part biased towards particular views.

The second part of the book is dedicated to case studies selected as an example of long term studies that have enhanced a deeper understanding of canid biology, covering topics such as population processes, life history strategies, behavioural ecology, and prey selection. The Arctic fox (*Alopex lagopus*) study (Chapter 8) is one such example. The authors examined how arctic fox life history strategies may adjust to different predictability of resources. In a Swedish population of Arctic foxes, food is a cyclic resource, whereas in an Icelandic population, food is temporally and spatially predictable. The two populations have divergent strategies with different reproductive investments - highly variable in the unpredictable environment and constant in the predictable one - and much longer dispersal distances in the unpredictable environment as opposed to the predictable one. Clearly, different studies are characterised by different duration, spatial constraints, and research interests and hence, achieved knowledge of the species of interest. For instance, long term projects focused on island populations such as the Island fox (*Urocyon littoralis*) endemic to the California Channel Islands (Chapter 9) or the grey wolf (*Canis lupus*) on the Isle Royal in Lake Superior in North America (Chapter 18), have an in-depth knowledge of population density and structure due both to their insular distribution and to the duration of studies. A peculiar case is the Ethiopian wolf (*C. simensis*) (Chapter 20), which is endemic to a rare alpine habitat within the Ethiopian highlands, and is *de facto* a species with a confined 'insular' distribution. As a result, all these insular studies achieved good knowledge of their study subjects in terms of behavioral ecology, population structure, and genetic inbreeding and endeavor to convert these scientific acquaintances into conservation actions. On the other hand, for other more elusive species such as Blanford's fox (*V. zerda*) (Chapter 11), the dhole (*Cuon alpinus*) (Chapter 21) and the side-striped jackal (*C. adustus*) (Chapter 16), only limited knowledge about their biology is available and further research extended elsewhere in the species geographic distribution is needed. Different studies differ also for their time constraints. At the two extremes of the spectrum there are grey wolves in North America studied for half a century by David Mech's group (Chapter 19) and red foxes (*V. vulpes*) (Chapter 12) in UK for three decades as the focus of Steve Harris and colleagues' Bristol fox project. At the other extreme, the chapter on bat-eared foxes (*Otocyon megalotis*) was studied just for the length of a doctorate study in 1992. With such a short study and with nothing previously published, the value of this case study seemed more questionable and stood out among the more detailed competing chapters. In contrast Chapter 13 on raccoon dogs (*Nyctereutes procyonoides*), was an example of well written case study, which examined the morphological and behavioural differences between two introduced raccoon dog populations: a Finnish population recently introduced, and the Japanese population that has been isolated from the mainland populations for 12,000 years. The authors, thus, bring evidence to support the species status of the Japanese population with obvious implications for conservation.

The third and last part of the book contains one chapter (Chapter 23) which aims to summarise the answers (and questions) derived by the field studies, and to assess whether these provide a satisfying framework to underpin conservation policies for wild canids. This chapter was the most confusing to read. I expected it to bring unity, but instead the editors choose to centre their discussion on proposals for conservation projects presented to the Canid Specialist Group (the editors are Chair and Vice Chair). Undoubtedly the editors' last words in a book are the hardest challenge in an edited book, but this chapter did not adequately conclude it.

Though with some flaws, I thoroughly enjoy reading this book. It brings to the reader the fascinating world of a very special family of carnivores: foxes, wolves, dogs and jackals, which mankind has feared and admired since the dawn of humanity. However, many of the authors come from the editors' research group, and not all other independent voices are represented. For instance, one such example of a more comprehensive review on single species is the recent *Wolf* by Mech & Boitani which brings to the reader a balanced perspective of research undertaken across the world. Despite this, this book constitutes an account of research on the wild canids and it may prove useful as a supplementary tool in an undergraduate course of behavioral ecology, conservation or population ecology for the plentiful examples of cooperative breeding, the different levels of complexity of sociality, and the degree of flexibility that wild canids exhibit in both society structure and mating system. This most definitely does not represent the last word on wild canids: carnivore intraguild competition, dispersal behaviour, communication are still poorly understood and feature amongst the topics which are likely to attract future research. This book represents the second comprehensive publication on wild canids, coming 25 years after Fox's milestone on wild canids. Hopefully it will prompt further research especially for the remaining mainly unknown canid species, as conservation can only be achieved through knowledge.

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Avoiding Attack. The Evolutionary Ecology of Crypsis, Warning Signals and Mimicry

Graeme Ruxton, Thomas Sherratt and Michael Speed, Oxford University Press, 2004, 249Pp
ISBN 0-19-852860-4 (paperback) 0-19-852859-0 (hardback)

Perhaps the sign of a good book is that your complementary copy's gone missing by the time you come to review it? Or rather it's just that you're not sure who might have walked off with it? Whichever it is, I'm happy to say that I finally tracked down my copy of *Avoiding Attack*, as I really enjoyed reading about aspects of animal coloration that I haven't properly thought about for years.

The book is introduced with a half-apology: all scientists have to focus on their own narrow specialty, and therefore the book only covers aspects of animal coloration relating to predator-prey interactions. Only? This is in itself quite an achievement. When we think about antipredator coloration, we immediately think about crypsis, warning signals, and mimicry. These are covered in depth in the book, but in addition, there are plenty of other intriguing adaptations to read about.

Avoiding Attack is split into three intuitive sections. The first covers how animals hide from predators, and what adaptations they have to help them match their backgrounds and avoid detection. Whilst there are classic examples, such as the controversial case of industrial melanism in the peppered moth, there is also a large number of other less familiar adaptations that are discussed. For example, the authors ask: why are animals transparent, is countershading an aid to crypsis, and how do animals cope with trying to match multiple backgrounds? Some of these examples are discussed in depth in relation to the visual systems of predators, and I found myself particularly fascinated by these. However, I was unsure what to make of the style of this section, which ranged from authoritative to playful. I found this, combined with many reiterations that more empirical work was required, quite jarring to read. It just really didn't work for me, and I was glad that this seemed to peter out later in the book.

The second section is about how to avoid attack after detection. Once detected, secondary defenses such as spines and toxins can increase survival, especially when accompanied by warning coloration. This section tackled some important theoretical issues, but the chapters seemed to have taken quite different approaches in explaining them. For example, I found Chapter 6 surprisingly hard going as I was talked through the theoretical ideas behind anti-predator signaling in algebra, whilst Chapter 8 elegantly took me through the initial evolution of warning signals, keeping the maths behind these ideas in the odd figure. I am not entirely sure about the targeted readership, but for people who think in terms of 'defenses' rather than '*d*', and 'risk from attack' rather than '*t*', then several sections are going to be pretty heavy going. The boxes containing models and simulations work very well, and I think I would have found it easier to have all the models compartmentalized in a similar way.

The final third of the book focuses on antipredator adaptations aimed at deceiving predators. The obvious case is of course signaling unpalatability when an individual is in fact palatable (Batesian mimicry and automimicry), and these are dealt with in detail. In addition, there are chapters on other aspects of adaptive resemblance (such as floral mimicry), and also on deflection and startle. As Ruxton et al. point out in their Conclusion, the functions of eye-spots in fish, stripes on weasel tails, and false head markings on butterflies are very poorly understood. In addition, there seem to be a number of adaptations - such as the flashing of brightly colored hindwings by otherwise cryptic moths aimed at startling naïve predators - for which, again, there is little evidence. I agree with the authors that there are a lot of unknowns in the functions of antipredator coloration, and I do hope that the ideas in their book kindles a renewed interest in these fascinating adaptations.

The book follows several other classic texts in adaptive coloration, notably Poulton (1890), Cott (1940), and Edmunds (1974). So, the obvious question is how *Avoiding Attack* stands on the shoulder of these giants? Well, I think it measures up very well. Since these books were written, empirical studies in the evolution of adaptive coloration have changed the field, perhaps most notably in my own area of warning coloration and mimicry. The book really takes a mechanistic approach to understanding adaptive coloration, and, despite my reservations about explaining models in the main text, I think on the whole Ruxton et al. do a good job of explaining new theories and ideas. For example, what if crypsis is costly because it prevents individuals from exploiting multiple habitats because of the need to match a particular background? This kind of question potentially changes the way that we think about adaptive coloration, needing to consider the costs and benefits to all types of coloration, not just those that seem obviously costly, like warning signals. The breadth of the book is impressive, and I'm not surprised that it took Ruxton et al. two years to research and write. It certainly deserves to be recognized as one of the important texts in adaptive coloration.

Okay, now for the few niggling problems I had with the presentation. First, I was surprised by the number of errors in the text. I spotted a large number of typing and formatting errors, including extra line spacing in places, missing commas, and incorrect use of apostrophes. Also, I found the author index frustrating, since it was not an exhaustive list. And the referencing also needs improving – I am still looking for a listed Rowe & Guilford paper in the actual text, and when I tried to look up a citation from one of the chapters, although cited as '1989a' in the text, there was no '1989a' or '1989b' in the reference list. This might sound really petty, and maybe I pay more attention to these things than everyone else, but for a £37.50 ($74.50 US) academic book from OUP, it's something I'd hope to see, and if I'd paid £80.00 ($144.50) for the hardback copy, I would have felt genuinely miffed. One final disappointment was the color plate. Since the book is about anti-predator adaptations, I immediately turned to it, looking for weird and wonderful creatures that I hadn't seen before. But I was met by the caterpillar from the front cover and two Müllerian mimics on one side, whilst on the other there was a full page spread of the outcome of a simulation. Given the scope of the book, I'm surprised that the plate didn't have more to amaze me. But these are small things given the overall achievements of the book, and perhaps could be improved for the next edition?

And I'm quite sure this book will be reprinted and reprinted. It's a useful book for both researchers working in antipredator coloration, and evolutionary biologists in general, providing an excellent overview of how the field stands today. It is potentially useful for undergraduate degree courses (although it is quite technical in places), and it's certainly a great introduction to the field for postgraduate students. It will no doubt become the book we all reach for when we struggle for a particular example or paper, and it also has new and old ideas on which to dwell on in the bath. And on top of that, if it keeps disappearing from my office at its current rate, I could singly keep the book in print for years to come…

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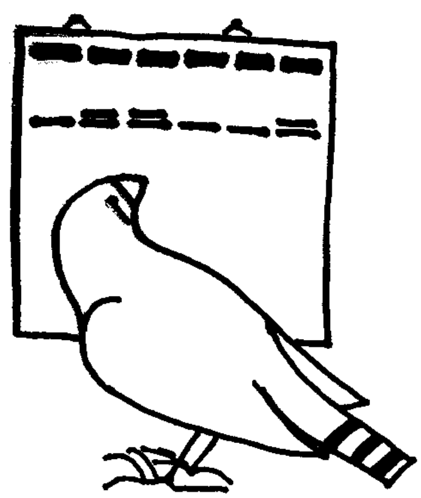

FAMILY PLANNING IN ZEBRA FINCHES

Cartoon by Joanna Rutkowska (see Workshop review next page)

Workshop & Conference Reviews

Maternal Effects in Zebra Finches – Status Quo and Where We Go

The workshop on "**Maternal effects in zebra finches – status quo and where we go**" took place in Bielefeld (Germany) on 14 - 16 March, and was well organized by Klaudia Witte. There were 17 participants from the following institutions: University of Glasgow, University of St. Andrews, University of Cambridge, University of Groningen, MPI for Ornithology in Seewiesen, University of Bielefeld, Jagiellonian University and Lund University.

This was the second workshop on this topic, and certainly not the last one. The first workshop on maternal effects in zebra finches was organized by Ton Groothuis, Nikolaus von Engelhardt and others (University of Groningen) in Groningen two years ago.

In Bielefeld, ten speakers presented their recent results on maternal effects and related topics in zebra finches, and due to a relaxed time-table we had plenty of opportunity for intense discussions. The studies focused on manipulations of a bird's social environment, the influence of male color rings, the availability of food or carotenoids and the influence of maternal androgens on maternal reproductive success. The studies also showed that female investment into eggs can influence offspring sex ratio, embryonic and post-hatching development and survival of the young. Attention was paid to sex-specific effects, such as sex differences in growth, begging rate, survival and immune response. Several studies looked at long-term consequences of maternal effects on the offspring's future performance during adulthood, as measured by sexual attractiveness, quality of male song and reproductive performance of these young. These studies followed the consequences of maternal effects at the intergeneration scale, undoubtedly an important element in studies of evolutionary trade-offs. Several of these results were consistent between different populations; however, some of these results still require replication using similar experimental designs. It was also suggested that future research should pay greater attention to the significance of paternal effects.

Two participants would like to initiate projects in which we eagerly agreed to join:

1) Wolfgang Forstmeier from the Max Planck Institute for Ornithology, is planning to use microsatellite markers for zebra finches for scanning up to 30 different populations on which research in behavioral ecology is been conducted. He would, ideally, like to get samples from 50 randomly-chosen individuals from each population. This will allow him to investigate the genetic structure of each population, as well as the degree of relatedness between populations. Knowing how similar / different our populations are may help to understand whether differences in findings between labs are due to environmental or genetic factors. Laboratories that provide DNA samples for this survey will in turn learn which markers exhibit the highest polymorphism, and hence are most suitable for studying relatedness in their own zebra finch population. For more details on this project please contact Wolfgang Forstmeier at: forstmeier@orn.mpg.de.

2) Lucy Gilbert from the University of St. Andrews pointed out that several studies reported significant effects of male color rings on maternal investment without actually providing evidence that female zebra finches show consistent preference for this artificial ornament. She suggested collecting and evaluating all available, unpublished data on results of preference tests with red- and green-ringed zebra finch males. Anyone who has studied this topic is welcome to contribute to this multi-lab collaborative review. Please contact Lucy Gilbert at: Lg18@st-and.ac.uk.

Being zebra finch addicts, the participants of the workshop missed terribly their study species left back home. Luckily, the program of the workshop included a visit of the zebra finch colony at the facilities of the University of Bielefeld. It gave us a unique opportunity to see the only population of wild zebra finches in Europe. The nightlife of Bielefeld was enjoyed nearly as much as the visit to our beloved birds.

The workshop was sponsored by the German Zoological Society.

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Commentaries

A Beginner's Guide to Scientific Misconduct.

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Scientific misconduct, like the weather, is a subject that everyone talks about. But is scientific misconduct a problem that we can actually do something about? Our own discipline—behavioral and evolutionary ecology—has certainly been abuzz with talk about scientific misconduct for the past several years, but when it comes to doing something about it, the usual reaction is that "the situation is deplorable and someone should do something about it, but not me". Certainly at last year's ABS meeting in Oaxaca, we were rarely involved in a conversation with our colleagues that did not eventually get around to the subject of scientific misconduct.

But why is there so much talk about scientific misconduct, and so little action when it comes to doing something about it? Besides the seemingly obvious instances when a scientist is caught fabricating data, embezzling funds, or plagiarizing the work of others, there is always gossip about scientific misconduct when some published work seems just too good to be true, or does not hold up to detailed scrutiny or replication. However, even when misconduct is detected or suspected, few of us are willing to do anything about it, probably for several reasons rooted in the sociology of science. First, an accusation of scientific misconduct might well be mistaken—although some published results might look suspicious, they may be genuine. Such false accusation can be damaging both to the accused and the accuser. Second, there is often the fear that exposing misconduct within one's own discipline will somehow tarnish the whole field. There seems to be no concrete evidence, however, that this is actually true. Third, many scientists fear that their own reputations will be sullied if they accuse others of misconduct, even if those accusations are correct. This fear appears to be well-founded as demonstrated in various recent cases of scientific whistle-blowing (Broad & Wade 1982, Judson 2004). Third, there is often a fear of retribution and even lawsuits by the accused and their friends and colleagues. Again, recent cases show that this is a reasonable fear (Judson 2004). Finally, most of us claim to be too busy to get involved in what could be a lengthy and emotionally-charged process. Moreover, there is a widespread notion that science is largely self-correcting and that problems like scientific misconduct will generally sort themselves out and go away (Anonymous 2004).

After much discourse about this over the past 15 years, we have come to the conclusion that a decidedly different approach to scientific misconduct is desirable—one based both on an open dialogue about the issues, rather than rumor and innuendo about specific cases, and on some specific public and private methods of dealing with expected instances. In this article we provide some historical background to the problem and an introduction to some of the relevant literature. We also discuss potential reasons for scientific misconduct, and we provide a guide to recognizing and dealing with suspected instances of misconduct in your own field of research. We hope that this guide will provide the basis for further discussion and, maybe, a better appreciation of the scope of this issue and the inherent problems in trying to do something about it.

At the outset, we need to make it clear that we believe that scientists should be held to a higher standard than is often accepted in other human endeavors. Science is fundamentally a search for the truth about nature and any practice that deviates from that goal is unacceptable. Thus, scientific misconduct is by definition always damaging to the scientific enterprise, and while it can, for a while at least, sometimes benefit the perpetrator, the scientific community always suffers. In our opinion, science is a purist enterprise that functions best when we pursue the truth and can trust in the work of our fellow scientists. In this article we show, however, that scientific misconduct is not always easy to define, and there is no agreed-upon way to deal with it. Nonetheless, we are certain that continuing to bury our heads in the sand is the least desirable solution.

Historical Background

Gregor Mendel might well be called the father of scientific misconduct, not because he was necessarily a wrongdoer, but because his published work sparked more than a century of controversy about the validity of his data (Fairbanks & Rytting 2001). Only two years after Mendel's 1866 paper was 'rediscovered', Weldon (1902) suggested that his reported ratios might be too good to be true. In a more famous analysis, Sir Ronald Fisher (1936) showed that the fit of Mendel's data to expectation was so unlikely that some sort of bias must have crept into his work. Several explanations for the apparent anomalies in Mendel's data have been proposed, involving various shades of what we might now consider to be scientific misconduct. For example, Mendel might have done many experiments and simply reported those that provided the closest fit to expectation. Second, he might have stopped counting seeds when the ratios were as close as possible to the expected ratios, although Mendel himself claimed not to have done this. Third, he might well have unconsciously biased his counting so that the data were actually closer to expectation than they should have been. Finally, it is possible that some assistant might have fudged the data. Fisher (1936), for example, said that "To suppose that Mendel recognized this theoretical complication, and adjusted the frequencies supposedly observed to allow for it, would be to contravene the weight of evidence supplied in detail by his paper as a whole. Although no explanation can be expected to be satisfactory, it remains a possibility, among others, that Mendel was deceived by some assistant who knew all too well what was expected. This possibility is supported by independent evidence that the data of most, if not all, of the experiments have been falsified so as to agree closely with Mendel's expectations." More recent analyses appear to have exonerated Mendel in any wrongdoing, though the details are complex (Fairbanks & Rytting 2001) and not entirely convincing. Nonetheless, the case is interesting at least for its persistence, as well as for the questions it raises about the nature of scientific misconduct and the often daunting task of proving that misconduct has actually transpired. Some have even suggested that the question of misconduct is irrelevant as the ends (Mendelian genetics) more than justify the means by which they were established, and thus that Mendel's published data are really of little scientific interest (Fairbanks & Rytting 2001). We disagree wholeheartedly with that view as we see published data and analyses as a fundamental building block in the development of science, and the publication of false data as a clear violation of the public trust.

Scientific misconduct has received a lot of ink in the past century in books (e.g., Broad & Wade 1982, Judson 2004), in both the popular and scientific news media (e.g., Koshland 1987, Dalton 2004), and in the scientific literature (e.g., Friedman 1992, Swazey et al. 1993). Here we highlight four unrelated cases that illustrate the breadth of what might be considered to be scientific misconduct, the difficulties sometimes involved in being certain that scientific misconduct has occurred, and the consequences for the authors when their apparent misconduct has been identified.

Paul Kammerer was responsible for probably the most celebrated example of scientific misconduct in biology, engagingly described in Arthur Koestler's (1971) famous book. Kammerer, you will recall, claimed to have clear evidence for Lamarckian inheritance in the midwife toad. Darwinian/Mendelian biologists were skeptical but Kammerer, who was widely regarded as brilliant, was vain and secretive and would rarely allow outsiders into his lab. His suspicious contemporaries were often accused of professional jealousy, in part because of the public fame that accrued to Kammerer. G. K. Noble (from the American Museum of Natural History) was allowed to visit Kammerer's lab in Vienna where he discovered that the apparent inheritance of black coloration was actually due to an injection of black ink. Noble (1926) published his findings in Nature but Kammerer claimed he had been the victim of a disgruntled assistant. Nonetheless, Kammerer's reputation was ruined and he fell into a deep depression, committing suicide shortly thereafter, en route to a new position in Russia.

Second, and much more recently, Bell Labs has exposed the work of one of their nanoscientists, Jan Hendrik Schön, as largely fabricated (e.g., see Kennedy 2002). Schön was widely regarded as brilliant, publishing on average one paper every 8 days for more than two years, 15 of those in Science and Nature. Clearly, many reviewers liked his work. While he had some supporters during that period, and was widely touted as a shoe-in for the Nobel Prize, there was also a lot of gossip about the validity of his findings. In the fall of 2001, his coworkers finally investigated and found that 16 of 25 papers that they looked at closely contained fraudulent data and another six were suspicious. For example, the same figures were duplicated in different papers with labels changed on the axes, and most of his findings could not be replicated. Bell Labs fired Schön immediately, the USA revoked his work permit, and the University of Konstanz revoked the PhD that they awarded him in 1997 (Anonymous 2004). While everyone acknowledged that Schön was brilliant, it seems that ambition and impatience got the better of him.

Third, Frank Sulloway's (1996) interesting and influential book 'Born to Rebel', on birth order effects, has come under some heavy criticism including accusations that he chose to report only those data that supported his ideas, as well as the failure of others to replicate some of his analyses from the available, published data (Dalton 2004). Several critiques of this book and Sulloway's responses are published in the journal 'Politics and the Life Sciences' [vol 19(2), 2000]. We mention this here, not to judge or vilify Sulloway but to point to the kinds of problems that can arise, even in comparative studies that analyze already-published data, and the potentially long and difficult process of getting to the truth (Dalton 2004). Certainly it seems likely that some critical debate, like that already published about Sulloway's book, is important to the process.

Finally, in our own field, many questions have been raised about some influential publications by Neal G. Smith, a former staff scientist at the Smithsonian Tropical Research Institute (STRI) in Panama. Smith's PhD thesis on the evolution of arctic gulls was published as a well-cited monograph (Smith 1966) and an article in Scientific American (Smith 1967). At the time, Smith's work was widely regarded as a landmark study, eventually making its way into several textbooks as an outstanding example of experimental work on mate choice and isolating mechanisms (e.g., Futuyma 1979). Nonetheless, Smith's (1966) monograph was given a skeptical review by Sutton (1966), a very experienced and well-known arctic ornithologist, and was often rumored to be 'suspect' for the next two decades. Eventually, Richard Snell (1988, 1991) published the results of his attempts to replicate Smith's work, concluding that "much of Smith's (1963; 1966a, b; 1967a, b) 1961 data on gulls at Home Bay could not have been based on actual observations or experimentation. Other data on the composition of pairs of courting plovers (Smith 1969: table 2) in Home Bay were evidently not based on actual observations, as Smith had not yet arrived in Home Bay at the time those data were reportedly collected. Perhaps many of Smith's reported observations were projections of various biological scenarios that he sincerely felt to be correct." In a related, but unpublished manuscript on Smith's (1969) study of ringed plovers, V. C. Wynne-Edwards (1991) concluded that "the desire to produce credible statistics in so complicated a situation may explain why he found it necessary to incorporate a far larger sample than could be found at the head of any one fiord." In fairness, Smith (1991) did reply to Snell's (1988, 1991) criticisms, admitting that some mistakes had been made (e.g., errors in transcribing data) but claiming that those mistakes did not affect his most important conclusions.

The Smith case is particularly interesting in the context of this article for three reasons. First, Smith's gull and plover studies were conducted in very harsh environments, under difficult working conditions, involving specialized techniques and analyses. These features have made this work almost impossible to replicate despite repeated attempts by Snell and others. Second, while the work of Snell (1988, 1991) and the analysis by Wynne-Edwards (1991) seem to point to some serious misconduct, the reply by Smith (1991), while admitting some culpability, might leave some readers uncertain about the validity of the published allegations. Finally, despite the published and private reservations about these studies, we know of no formal attempts to investigate these issues further. Rather, citations of Smith's arctic research have largely disappeared from the textbooks and scientific literature.

What is scientific misconduct?

While some practices clearly constitute scientific misconduct, other aspects of the scientific enterprise are considered to be wrong, fraudulent and morally reprehensible by some scientists but not others. Moreover, there is a wide range of opinions about the severity of various forms of misconduct and what should be done about them. For example, we expect that most of us would agree that data fabrication, plagiarism, and embezzlement of grant funds are serious forms of misconduct that should be dealt with harshly. But as anyone who has encountered plagiarism in undergraduate essays will know, it is sometimes difficult to be certain that deliberate plagiarism has really occurred. For example, students caught apparently plagiarizing will sometimes claim photographic memories, typographical errors, and honest (but sloppy) mistakes in transcribing their notes, making it difficult for the instructor to be certain that the student is really culpable. Even when the evidence for misconduct is clear, scientists often try to wriggle away by blaming assistants, students, and collaborators.

To help illustrate the difficulty in defining what forms of scientific misconduct are most serious, and indeed whether there is even consensus about what constitutes misconduct, we provide a questionnaire at the end of this article. While this questionnaire was designed primarily to gather information, we have found it to be tremendously useful in fostering discussion among our students and colleagues.

We encourage you to fill out the questionnaire and return it to us, but also to use it as a focus for discussion in your own research groups. Like a questionnaire in a popular magazine, you might find this one personally useful, but we hope that you will also help us to gather some potentially very important data. We will submit a summary of the results for publication in a future issue of this newsletter. Thus, by participating in this survey you will help us to determine patterns of scientific behavior in our community of researchers, but you will be able to assess for yourself how your own performance compares.

The questionnaire is also available and can be completed on-line, which you might find easier and more anonymous to fill out, at:

http://biology.queensu.ca/~montgome/sm.

Feel free to photocopy and distribute the questionnaire to your students/supervisors/colleagues, or encourage them to fill out the on-line version.

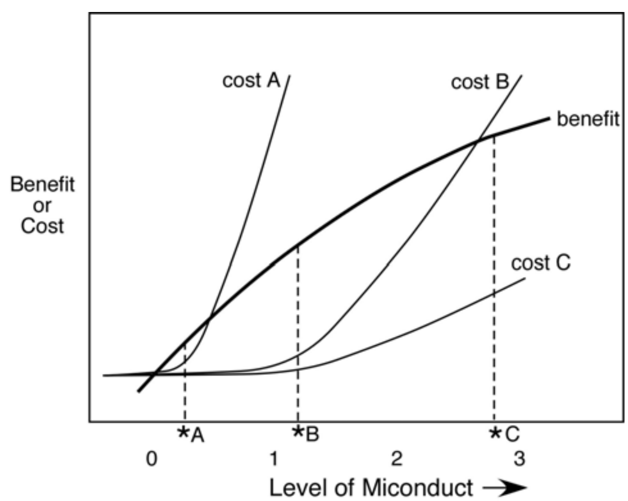
Of course, the categorization of scientific misconduct into levels of severity, as requested on the questionnaire, will vary from person to person. Even for a given scientist, there is likely to be a grey zone between levels that will change with age, experience and circumstance. In our experience it is difficult to fill out this questionnaire without gaining a fresh appreciation of the problems inherent in defining the limits of scientific misconduct.

Who is guilty of misconduct and why?

If all of the items on our questionnaire are considered to be forms of scientific misconduct, then it is highly likely that we are all guilty to some degree. One study (Swazey et al. 1993), involving 4000 researchers from 99 large graduate programs, found that about one third of the faculty had observed student plagiarism, about one fifth of the grad students had seen their peers fabricate data, and about a fifth of all respondents claimed to have sometimes avoided reporting data that did not fit their favored explanation. Clearly, an editorial claim in Science (Koshland 1987) that “99.9999 percent of reports are accurate and truthful" is likely to be well off the mark, and this claim itself, pretending to be based on a highly precise estimate, might be considered to be a form of scientific misconduct.

As we point out above, even determining who is the perpetrator of clear cases of misconduct can be a tricky business. In general, many scientists are probably poorly equipped to detect clever cases of misconduct, being more often awed by productivity, creativity, and apparent discovery than skeptical about interesting results, as demonstrated in the Schön case at least. The legal, forensic, and psychological analyses needed to detect and be certain of serious cases of misconduct is often such a daunting task that few scientists are willing to get involved. Moreover, wrongdoers are often brilliant, charismatic individuals who have many supporters even in the face of what looks like clear evidence of malfeasance. The popular film “Shattered Glass” (2003, Lions Gate Films) provides an excellent example of this latter phenomenon in a celebrated case of journalistic fraud.

Like many simple and risky behaviors, the level of scientific misconduct that any individual engages in can possibly be understood by a simple cost-benefit model like this:



where the costs and benefits are perceived by the perpetrator as influencing their own lives, and the misconduct levels are as listed on our questionnaire. Future costs might occur in the form of personal anguish, official censure, difficulty in publishing research or obtaining grants, and loss of grants, research students, prestige or employment. As in all such models the shape of the cost function is debatable but it seems likely that costs will be very low for minor misconduct (say levels 0 or 1 on the questionnaire) gradually accelerating with the severity of the misconduct and becoming very high for the most serious cases. Current benefits accrue in the form of employment, prestige, salary, grants, and awards. Such benefits probably accrue directly as a consequence of both the quantity and the perceived quality of published work.

In this model, the apparently 'optimal' level of misconduct (marked * on the graph) depends upon the researcher's perception of the costs, where most researchers would probably not regard some behaviors as constituting real misconduct (e.g., level 0 on the questionnaire). Remember however that this model is based on the perpetrator's <u>perception</u> of the costs and benefits and thus what might seem to be 'optimal'. It is not based on the costs and benefits or what may actually be 'optimal' to the scientific enterprise, or the <u>actual</u> costs or benefits to the perpetrator. So, for example, a highly ethical scientist who believes that good science requires high integrity and truthful reporting will perceive the cost curve to be much like the 'cost A' curve and thus the 'optimal' level of misconduct to be quite low. Scientists with sociopathic tendencies, on the other hand, will perceive the costs of even the most egregious behavior to be relatively low ('cost C' curve), resulting in some serious misconduct. Moreover, the costs to the scientific community of any real misconduct are likely to be much higher than the costs to the perpetrator. For that reason, we all benefit from being vigilant about misconduct in our own disciplines.

What can be done?

Various measures have been proposed for dealing with scientific misconduct, from doing nothing, on the one hand, to setting up some rigid rules for publication and guidelines for scientific oversight, on the other. Neither of these solutions seems entirely satisfactory. The notion that "Fabricated results tend to be discovered, thanks to the self-regulating mechanism of research regulation" (Anonymous 2004) is probably far from the truth and leads many scientists to believe that no action on their part is required. Particularly in behavioral and evolutionary ecology, where replication is often difficult and exact replication usually impossible, such self regulation is more problematic, and thus the likelihood that misconduct will be detected may be relatively low at present. In our opinion, more rigid standards for publication, such as increased peer review, requiring that original data be put in repository archives, and the establishment of oversight agencies is more likely to impede than enhance progress in our discipline.

There are, however, both private and public responses to the suspicion of scientific misconduct that are relatively easy to implement and have the potential to detect or reduce the severity of misconduct in any discipline. Of course, each scientist needs to make his/her own decision about what constitutes unacceptable conduct in science, and what to do about it if they believe that a scientist has exceeded the limits of acceptable behavior.

In our experience, there are a few private responses to scientific misconduct, as follows:

1) do nothing, business as usual, let someone else worry about it;
2) refuse to review the presumed wrongdoer's papers and grant applications;
3) do not cite the presumed wrongdoer's work;
4) write a letter of complaint to the relevant scientific societies, granting agencies or regulatory bodies (U. S. Office of Research Integrity, etc).

There is nothing new here, though it is our perception that option 1 is by far the choice of most behavioral ecologists, and option 4 is very rarely exercised.

A more public response would be to write papers or commentaries criticizing the miscreant's work, but this response is also rare for reasons discussed earlier. We suggest that a potentially more effective public response would be to develop a system of on-line peer review for every published paper. Reviews on the epinions website (**www.epinions.com**) provide an excellent example of this in a different context, and we have made up an example (fictitious) of how this might work for a journal like Behavioral Ecology (see **http://biology.queensu.ca/~montgome/sm**). On the epinions site, products are rated on a few key variables, the reviewer is identified by a moniker (not necessarily their real name but known to the moderator), and even the commentator's opinions are rated by other readers. Given that most journals, including Behavioral Ecology, are readily accessible on the Internet, such a proposal would be relatively easy to implement. Such on-line commentaries should certainly be edited and moderated, and rebuttals from the original authors allowed. Many people won't like this because it has the potential to foster vigilantism and would expose one's work to more scrutiny than may be desired. Journal editors might also fear that on-line peer reviews would drive authors away from a journal, but maybe that would be true only of those who have something to fear. On the contrary, we believe that it would make published work more reliable and more, rather than less, likely to be cited. Thus a dialog accompanying each published work has the potential to reveal serious misconduct, but also to improve communication and the development of ideas and techniques. At the very least, we feel there is much to be gained by discussing this proposal.

Finally, we think it is rather naïve to believe that better scientific mentoring is a solution to the problem, as some have suggested (Judson 2004). It would hardly be fair, for example, to blame the mentors of those scientists who have been accused of scientific misconduct so far, many of whom were undoubtedly excellent mentors. Nonetheless, good mentorship about scientific misconduct may be useful in helping our students (i) to come to grips with what constitutes misconduct, (ii) to recognize when it has occurred, and (iii) to take the appropriate action when they believe that scientific misconduct has occurred.

None of these solutions is a panacea, maybe especially in behavioral ecology where replication is often difficult and even those suspected of misconduct are rarely exposed. Indeed it should be possible for a clever scientist in our field to fabricate all of their data without raising even the slightest suspicion, especially in light of the rarity of independent replication of any sort. As a scientific society we need to decide whether this situation is acceptable and, if not, we should certainly begin to discuss some potential solutions.

Acknowledgments

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Editor's note – the survey printed on pages 22-24 is being conducted by the authors. While I personally encourage people to participate, it is not an official survey of the ISBE

SCIENTIFIC MISCONDUCT QUESTIONNAIRE

[copyright 2004 R Montgomerie]

Fill in your estimation of the seriousness of each offence listed in the table below, using the following scale:

Levels of misconduct

LEVEL 0: not really scientific misconduct, in my opinion

LEVEL 1: mild misconduct [probably requires no public censure or disciplinary action]

LEVEL 2: moderate misconduct [requires some retraction or correction in literature, and possibly disciplinary action]

LEVEL 3: severe misconduct [requires both censure and punishment commensurate with the cost to the discipline and society at large—should probably lose job/position, be fined, and possibly charged in court]

Please fill out this questionnaire as honestly as you can. We would also appreciate your candid assessment as to whether you think you might be guilty of any of the items listed by putting a ☑ in the 'Guilty' column.

By filling out and submitting this questionnaire, you are giving us permission to use these data in our ongoing research on this subject. Your answers are, and will always be anonymous, unless you choose to sign the questionnaire

LEVEL	Behavior	GUILTY?
	requiring your name to be put on papers for which you have provided only money and/or facilities	
	attempting to publish already published (or accepted) papers in a different journal, with or without some changes to mask the deception	
	not understanding the statistics you are using	
	allowing your name to be put on papers to which you have made no reasonable contribution	
	copying large portions of other peoples' published work without attribution	
	copying large portions of other peoples' <u>un</u>published work without attribution	
	putting fictitious papers on your CV	
	presenting seminars/talks/posters on rough analyses and incomplete data	
	declining to review your share of submitted manuscripts (roughly 3X the number you submit)	
	dividing up your research into the least publishable units	
	using grant funds to attend a conference and then not, or barely, showing up	
	sitting on the review of a competitor's work while you prepare or finish your own work on the same subject	
	deliberately making up some, or all of the data in a manuscript submitted for publication	
	knowingly using statistics that will result in either Type I or II errors in favor of your preferred idea	
	unwittingly using statistics that will result in either Type I or II errors in favor of your preferred idea	
	delaying a manuscript or grant application review to slow the progress of competitors	
	altering your manuscript by using fabricated data or false claims to address a reviewer's comments	
	not checking and verifying that the work of technicians and or coauthors has not been made up or fudged in any way	
	not replicating experiments and observations	

	knowingly neglecting to cite the work of others who have found similar (or very different) results	
	unwittingly neglecting to cite the work of others who have found similar (or very different) results	
	making up some data to increase sample sizes and make trends clearer	
	not collecting data double blind	
	putting book reviews and other unrefereed works on your CV as if they were actual papers	
	claiming to have addressed a reviewer's comments when you have not	
	not mentioning data that indicate that any of the conclusions of a study may be in doubt	
	applying for grants to do work that is already done	
	knowingly circumventing ethical guidelines, in a <u>minor</u> way, for animal or human research	
	knowingly circumventing ethical guidelines, in a <u>major</u> way, for animal or human research	
	unwittingly circumventing ethical guidelines for animal or human research	
	declining to review your share of grant applications	
	having your name on manuscripts based on work that you would unable to present in a seminar	
	copying small portions of other people's published or unpublished work without attribution	
	paraphrasing other people's published or unpublished work without attribution	
	submitting manuscripts that you have not read, checked the data and analyses, and/or understood the subject matter	
	reviewing a manuscript in a cursory fashion	
	deleting some data to make trends clearer	
	exaggerating or obfuscating to make a grant proposal look better than it really is	
	knowingly selecting only those data that support an hypothesis	
	unwittingly selecting only those data that support an hypothesis	
	exploiting the labors of graduate students and postdocs for personal gain	
	submitting the same manuscript to more than one journal at the same time	
	showing only the results of your 'best' experiment or set of observations	
	Publishing exactly the same paper in a different language	
	data mining to find significant results and then passing off those results as if no mining was done	
	knowingly biasing data collection in favor of a particular (preferred) hypothesis	
	unwittingly biasing data collection in favor of a particular (preferred) hypothesis	
	using grant funds for personal travel, supplies and equipment	
	rejecting a competitor’s grant application or manuscript to slow his/her progress	
	spreading unsubstantiated rumors about a competitor (designed to hurt their reputation)	
	failing to reveal clear evidence of scientific misconduct in a fellow scientist	

OTHER

Please list any other forms of scientific misconduct that you are aware of and rate them as above. Feel free to use a separate sheet or send us an email if you find that easier

DEMOGRAPHICS

To help us further evaluate the information that you have provided, please tell us some additional information about yourself by answering the following questions. Check all that apply in each case:

AGE ☐<30 yr ☐30-39 yr ☐40-49 yr ☐>50 yr **SEX** ☐female ☐male

HIGHEST DEGREE SO FAR ☐BA/BSc ☐MSc ☐PhD

CURRENT POSITION

☐undergrad ☐MSc student ☐PhD student ☐postdoc
☐Assistant Professor (or equivalent) ☐Associate Professor (or equivalent)
☐Full Professor (or equivalent) ☐Emeritus Professor
☐technician ☐research assistant ☐other [please specify__________________]

GREW UP IN (i.e. spent at least 5 years before you left home)

☐Canada ☐USA ☐UK
☐northern Europe ☐southern Europe ☐Middle East
☐Asia ☐Africa ☐Australia/New Zealand ☐Latin America

ATTENDED GRADUATE SCHOOL (for MSc or PhD) IN

☐Canada ☐USA ☐UK
☐northern Europe ☐southern Europe ☐Middle East
☐Asia ☐Africa ☐Australia/New Zealand ☐Latin America

CURRENTLY RESIDE IN

☐Canada ☐USA ☐UK
☐northern Europe ☐southern Europe ☐Middle East
☐Asia ☐Africa ☐Australia/New Zealand ☐Latin America

TOTAL NUMBER OF REFEREED PUBLICATIONS TO DATE

☐<5 ☐5-9 ☐10-24 ☐25-49 ☐50-100 ☐100-200 ☐>200

DECADE OF FIRST PUBLICATION

☐before 1950 ☐1950s ☐1960s ☐1970s ☐1980s
☐1990s ☐current decade

WHAT IS YOUR FIELD OF RESEARCH? ☐Biology ☐other Science subdiscipline
(be as specific as you like)___________________________

COMMENTS

We would especially appreciate hearing from you about this topic and/or this questionnaire; send your comments by email to montgome@biology.queensu.ca

RETURN THIS QUESTIONNAIRE TO

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