

Supplement to Behavioral Ecology

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Editorial

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Commentaries

Improving the processing of scientific misconduct charges: An eyewitness perspective

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In the last Newsletter, Prof. R. Montgomerie and Prof. T. R. Birkhead (Montgomerie & Birkhead 2005) raised a number of issues concerning scientific misconduct. This is a welcome point of view that is in dire need of discussion. Recently, I was accused and found guilty of scientific misconduct by a Danish ministerial committee. Subsequently, an independent committee established by, but completely outside the French National Centre for Scientific Research (CNRS) that is my employer, and composed of scientists completely independent of any of the persons involved in the case, concluded in the fall 2004 that they had found no evidence of scientific misconduct on my behalf. Their report can be found on the web site of the CNRS. With this in mind, I would like to consider what led to the differences in conclusions between these two tribunals, and whether this process can give us insight into how accusations of misconduct must be treated in future instances to ensure that hearings are judicious.

Brief Case Background

My own case was based on accusations of publishing falsified data in a manuscript on asymmetry in oak leaves appearing in the ecological journal, *Oikos*. The majority of the data in question was collected by a research technician; although I cross-confirmed a subset of the measurements initially, the majority of the measurements used in analysis for the resulting publication were made by the technician. When accusations that the data may have been falsified were made, I and an independent observer re-measured the original leaves, and found discrepancies between our measures (which concurred) and those in the original dataset. My co-authors and I withdrew the paper from *Oikos*, and the journal editor (Prof. N. Malmer), wrote a letter to the Danish committee investigating the case stating that I had behaved honorably in this case by retracting a paper that contained information based on measurements of poor quality.

Evaluating Accusations of Misconduct

Accusations of misconduct can have very dire consequences on the career of the accused. Thus, it is very important to consider not only the case made against the accused, but also consider potential ulterior motivations of those making the accusations. If the latter appears to be potentially biased, this should be taken into account during the progression of investigation. I have published papers with over 180 co-authors (32 of these papers in *Oikos*) and shared data files with them. Not once have I received complaints or questions that could raise doubts about the integrity of these files. By contrast, all five of the persons who raised accusations against me in the press or in emails to colleagues had previously published work in which they took theoretical or experimental positions contrary to my own. They could thus be argued to have potential conflicts of interest in evaluating the case against me. Whether or not this affected their judgment is not the issue: in modern science, any potential conflict of interest is routinely used to disqualify a potential reviewer of grants or submitted publications. That same standard must be maintained for reviewers of scientific misconduct cases.

If cases are to be fully investigated, there must be a clear code of ethical procedures that are followed to ensure a fair inquiry. The case made against me was based on legal practices that would be deemed unacceptable in most civilized societies and certainly throughout modern science. First, the original case against me was based on a data file that was never shown to be authentic by independent persons, not even by the committee that was deciding whether to raise the case in the first place. The data file had in fact remained in the hands of the accuser without ever having been validated. Second, a sub-committee of three persons eventually investigated the case, and only one was a biologist. Unfortunately, the biologist was known ahead of time to have an established view on the case. Committees investigating scientific misconduct should, by default, be composed of persons that have had no recent contact or conflict whatsoever with any of the parties involved. While I requested that foreign scientists with no prior connection to me or the

case should be included in the committee to insure impartiality, this was completely ignored. In addition to the procedural problems, there has been extensive harassment of students, colleagues and friends by unsolicited emails and phone calls, a successful attempt to have my bird banding license revoked, thereby eliminating the possibility for me to continue my 35 years time series of a barn swallow population, and explicit demands that I be fired from my current position. I find this entire series of unregulated actions inappropriate. What is worse is that the lack of proper practices has established a precedent that has subsequently been applied to another case! While many behavioral ecologists have publicly expressed concern about my specific case (Alatalo et al. 2004, Moreno & Mousseau 2004), this has unfortunately not led to a general review and critique of recent procedures

Montgomerie & Birkhead proposed a simple graphical model to understand scientific misconduct. I suggest that this model is simplistic because it does not consider the social context in which accusations are produced. Scientific misconduct protocols may be only a part of a larger need for agreed on ethics in science. Promotions, tenure, hiring, and grant and manuscript review can all be fraught with ethically dubious practices. Some of the models of (scientific) inbreeding depression, nepotism, and policing in social insects seem more appropriate to achieve a proper understanding of the situation! And even within the zone of ethics related to scientific misconduct, there is, as observed by Montgomerie & Birkhead, wide variation in what is criticized and what is not. During the last couple of years I have encountered a wide range of practices that most people would agree would comprise scientific misconduct ranging from misuse of a scientist in Eastern Europe as unpaid on-site manager without giving that person full credit for his efforts, over theft of scientific equipment, elimination of the name of a co-applicant from a multi-authored grant application, to theft of intellectual property. While I agree that these behaviors are abhorrent, I would not wish for anybody charged with even these offences to experience the level of unregulated criticism and harassment that I have encountered. I thus strongly endorse the above authors' call for better

definitions of what is and is not acceptable, but I would also argue for far more explicit and fair protocols for review, exoneration, and censure throughout our community.

Recommendations

My experiences raise questions about recommendations for changes in procedures. While it may be difficult or impossible to adopt standardized international guidelines regarding investigations of misconduct, it might be possible to agree on a set of minimum requirements. In my opinion, that would include the requirement that all members of overseeing committees are completely independent and impartial with respect to the case. Second, it would also include legal support not only for the investigating body, but also for the party being investigated. Otherwise, the superior resources and legal staff of governmental agencies may determine cases rather than the actual facts. Third, many national scientific communities are so small that an impartial evaluation of a case would require involvement of impartial scientists from abroad. Finally, the sanctions imposed in particular cases should show a reasonable relationship with the degree of the offence, and not be further aggravated by unregulated actions of parties directly or indirectly involved in a case. Such non-sanctioned punishment should themselves be subject to sanctions similar to those imposed in cases of scientific misconduct.

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