Dear members of the TheoBio group,

I have performed a serious act of scientific misconduct by stealing the following article from the Newsletter of the “Ethologische Gesellschaft”. Originally, this article has been published in the newsletter of the International Society for Behavioral Ecology. I find its content interesting and relevant for all scientists, and I hope that some of you will fill in the questionnaire on scientific misconduct developed by Bob Montgomerie and Tim Birkhead (**http://biology.queensu.ca/~montgome/sm**).

Franjo

REPRINTED from the Newsletter of the International Society for Behavioral Ecology,

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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 nano-scientists, 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?

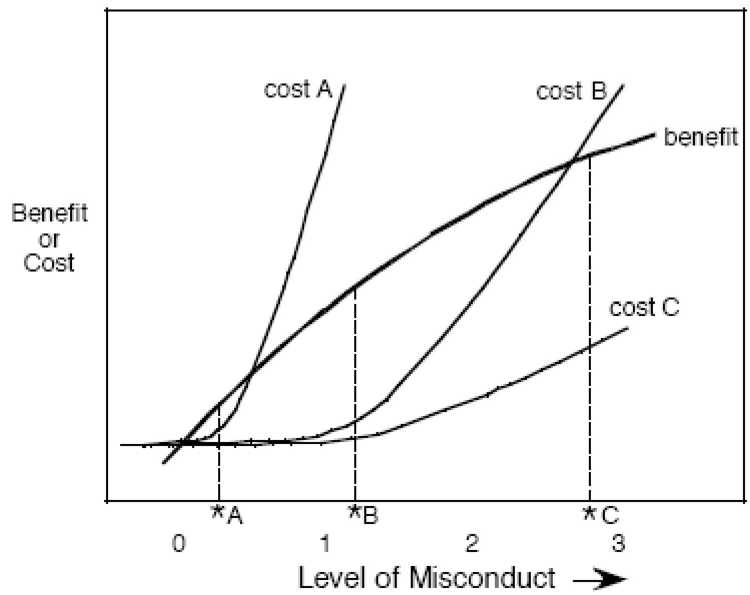
While some practices clearly constitute scientific misconduct, other aspects of the scientific enterprise are considered to be wrong, fraudulent and morally reprehensible by some scientists but not others. Moreover, there is a wide range of opinions about the severity of various forms of misconduct and 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 perception 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 actual 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 online 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.

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SCIENTIFIC MISCONDUCT QUESTIONNAIRE

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' unpublished 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 minor way, for animal or human research	
	knowingly circumventing ethical guidelines, in a major 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	

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

R Montgomerie, Department of Biology, Queen's University, Kingston, ON K7L 3N6, Canada; or by email to montgome@biology.queensu.ca