

In ISBE Newsletter (Supplement to Behavioral Ecology) Volume 17, issue 2 fall/winter 2005 Ken Otter, Newsletter Editor writes among others in Editorial: "- - this issue also contains an article on scientific misconduct that builds on the commentary by Montgomerie and Birkhead published in the previous Newsletter (ISBE Newsletter 17: 16-24). In his article "Improving the processing of scientific misconduct charges: an eyewitness perspective", Anders Pape Møller uses his own experience on this subject to review and recommend policies for handling accusations of misconduct. Due to the serious consequences on individuals' careers that can stem from charges of misconduct, it is imperative that judicious reviews of the allegations are conducted. I applaud Anders for sharing his experiences, and moreover for using these to analytically assess and provide recommendations for how such cases should be conducted".

The following Commentaries was published in ISBE Newsletter, Vol.17(2): 34-35.

Improving the processing of scientific misconduct charges: an eyewitness perspective.

Anders Pape Møller

Laboratoire de Parasitologie Evolutive, CNRS UMR 7103, Université Pierre et Marie Curie, Paris, France

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 have 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 the 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 judgement 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. The 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 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. In finding 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 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 behaviours 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.

Acknowledgements - I would like to thank J.W. Bradbury and K. Otter for very constructive comments and assistance.

References

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Moreno J, Mousseau T. 2004. Dedication put Møller ahead, not fabrication. *Nature* 428:695.

The following is a correspondence between me and Ken Otter

-----Original Message-----

From: Rabøl, Jørgen [<mailto:JRabol@bi.ku.dk>]

Sent: Tuesday, November 29, 2005 1:00 AM

To: Kenneth Otter
Subject: APM

Dear Ken Otter,

The following Commentaries to ISBE Newsletter. All claims can be documented in paper. I am in Australia from Nov.30 until Dec.29 on vacation. If possible I will take a look on my email, but it may be easier to catch me on the email of my wife dab@ramboll.dk Cheers J

Apart from a few corrections of obvious errors or inappropriate language the following manuscript was sent to Ken Otter, ISBE Newsletter 29 Nov. 2005

Anders Pape Møller and scientific misconduct

Jørgen Rabøl
Olesvej 12, 2830 Virum, Denmark

Anders Pape Møller (APM) never ceases to amaze: He is very intelligent; he works extremely hard and determined; he is a charming person; and he has been a great inspiration for his many students, post docs and colleagues. However, he committed very serious scientific misconduct and fraud in the article Møller, de Lope 1998. His recent contribution in ISBE Newsletter (Møller 2005) clearly show that he does not appreciate the severity of this fact.

The UVVU decision, and the reactions of the Danish Ministry of Science, the Danish Ombudsman and French CNRS

In autumn 2003, the Danish Committees on Scientific Dishonesty (UVVU) ruled that APM used fabricated data in Møller, de Lope 1998. In January 2004, the case was described in both Nature (Abbott 2004) and Science (Vogel et al. 2004) where APM declared he was not guilty and would go to the Danish Ministry of Science, the Danish Ombudsman and if necessary the civil courts to be cleared of the false accusations.

Since then, the Ministry of Science has rejected all his complaints and in April 2005 the Ombudsman – based on a careful consideration – decided not to enter the case. Presently, six months later, in spite of several claims, APM has sued neither UVVU nor me who raised the case. In all probability, APM will never do so, well knowing, or being told so by his lawyers that his personal fabrication and assembly of data he delivered to UVVU were far too easy to prove.

However, many of his colleagues cannot believe that APM did indeed invent the data, and the journal Oikos, in which the Møller and de Lope paper was published (and later withdrawn) still supports APM. At least in part, so does the French CNRS (APM's present employer). In the autumn 2004, the CNRS on their home-site published – in French – a statement of which the most important point was that *“it is not possible to establish formal proof of intent to commit scientific fraud on the part of Anders Pape Moller concerning the research published in the article, which was withdrawn from the journal Oikos. Lacking the material evidence necessary to establish innocence, the committee was equally unable to reach this conclusion”* (English translation by

APM in a letter to the Danish Ombudsman). According to APM there was a break into his computer and data files were stolen resulting in the deletion of the material evidence, namely the data-set delivered by Jette Andersen (JA, the technician who measured the leaves). Furthermore, the original leaves (deposited in the private home of his family in Denmark) were thrown out by accident in 2000 and therefore could not be re-measured.

The investigation by CNRS is superficial and biased in not demanding any data from UVVU, thus neglecting to render possible a demonstration that the data submitted to UVVU by APM was indeed selected and made up by APM (assembled, because he did make use of some of the data measured by JA, while other parts of the data-file were pure fabrications made by APM). APM claims that all of the fabricated data was produced by JA (and this author?), but according to Møller, de Lope 1998 “*J. Andersen did the heroic task measuring all the leaves*” and later on “*The leaves were measured - - - by J. Andersen, who was unaware of the origin of the leaves. Measurements were therefore done blindly with respect to the predictions under test*”. This means that without knowing the research hypothesis she fabricated a data file by pure chance that APM accepted as nicely supporting his to her unknown research hypothesis, Furthermore, APM claims that JA and I used another set of fabricated data to instigate the UVVU case against him. Clearly, these are not real world scenarios.

Comments to Møller (2005)

As remarked by A. Richard Palmer (home page, 2005) APM was not cleared by the CNRS: “*Moller’s innocence was presumed, not demonstrated*”. Now, the so-called independent committee of wise men considering the case on behalf of the CNRS was absolutely not independent as only APM and supporters were involved in the treatment. Furthermore, both the committee and CNRS never went deeply into the material and the facts and never appreciated that APM was not only responsible for but also the author of the data-files he submitted to UVVU.

APM and the anonymous “*independent observer*” never “*re-measured the original leaves*” after they were measured by the technician (JA). (APM did report to UVVU that he and a student in 2000 re-measured a sub-set of 25 of the original leaves). The 25 measurements of JA (mean right/left leaf width) reported by APM can be found in her data-file that was submitted to Oikos and later to UVVU. The latter is the data-file APM claims was fabricated by us with the sole purpose of starting the false accusations against him (though how could he know any data from this file?). Furthermore, according to APM the leaves were left with his family in a remote part of Denmark. So the student and APM would have to go to Denmark, before the leaves by accident were thrown out. If the leaves were not yet discarded, how could the student and APM measure the same leaves as did JA? The (normally) five leaves per tree were placed in envelopes marked with the tree-numbers only. Single leaves were not numbered. Thus no one should wonder that the correlation coefficient (r) between the leaf widths by APM and JA was not significant and close to 0. On the other hand, r between the independent measurements of the student and APM was remarkably high (above 0.999) suggesting an almost telepathic connection between the two parties.

It is not true that no former co-author of any APM paper has complained or questioned his data. Among others Pomiankowski did so (Vogel et al. 2004, Abbott 2004).

The original data-file by JA (the one submitted by JA and me to Oikos and UVVU) could be traced back to the date 29 march, 1996 (confirmed and validated by the University of Copenhagen). JA personally submitted this data-file and a paper version to APM in April, 1996, along with the measured leaves. According to APM he never received that data-file nor its paper version, but another file with false data, which then was used for the calculations in Møller, de Lope 1998.

Shortly afterwards there was a break into his computer and the data-set disappeared for ever, except for (according to APM) some papers containing not the original raw data measurements per leaf but only some calculated means per tree.

The sub-committee of three persons investigating the case on behalf of UVVU consisted of two biologists and not – as claimed by APM – by one, only. The third member was a professor in statistics.

APM claims that one of the sub-committee members “*was known ahead of time to have an established view of the case*”. This is not true. When the case started, APM mildly protested against the inclusion of this member in the sub-committee on the grounds that this author might find him an unfair disadvantage since he was a former teacher and later colleague of APM. When the sub-committee in 2002 delivered their recommendation to UVVU, APM now violently protested against the member whom he claimed was against his (APM’s) science for political reasons. Furthermore, this member should have been involved in a serious APM-family tragedy. The member strongly denied these allegations.

It is true that the Zoological Museum in Copenhagen informed APM that they cared too much about their bird ringing data to cooperate with people who fabricate data. However, they also patiently explained to APM that he could re-apply for his ringing license at a higher level. So it is not true that APM was prevented from continuing his 35 years time series of a barn swallow population.

Finally, APM is right when suggesting that the procedures need to be changed in order to cope more efficiently with scientific fraud. APM himself was favored by the fact that the Danish UVVU made their decision known only to the complainant (me) and the complainee (APM). Furthermore, the ruling was in Danish only. In its annual report, UVVU describes its cases in much detail, but again in Danish only and without mentioning the names of the persons cited for scientific misconduct. Fortunately, APM himself at an early stage insisted that his correspondence with the sub-committee and UVVU be in English. Therefore, the important enclosures (such as the detailed recommendation of the sub-committee to UVVU which was attached to the UVVU ruling), are available in the English language. In fact, the UVVU ruling is made widely known only because I informed the then director of the then Zoological Institute, University of Copenhagen, who published the decision (without enclosures) on its web-site. After a few months, the ruling was removed. However, anyone interested in all the important facts of the case (including the sub-committee recommendation and the different data-files) may find these on www.dannaborg.dk in the section Jørgen. Here the file, Overview1, summarizes the case until Feb.2004.

Other misconducts by APM?

An important question is whether Møller and de Lope 1998 is APM’s only case of scientific misconduct. Personally, I am convinced that he has for many years adapted and fabricated his data whenever necessary and possible. However, this may be difficult to prove, but strong indications are easily found.

On such example follows.

Fluctuating asymmetry and Dutch elm disease

Møller 1999 (Tables 1 to 3) investigated 39 pairs of elm trees of which all 78 trees in the first year (1993) were healthy, i.e. not attacked by Dutch elm disease. In the course of a four year period a

single tree in each pair developed the disease. According to Møller 1999 “the disease is still relatively uncommon at the Danish study site with less than 5% of all elm trees being infected”. I.e. less than 5% were infected when the investigation was finished in 1997. The 39 pairs of trees was a sub-sample in a sample of 43 pairs (or perhaps a larger sample of 43 pairs + 25 extra pairs = 68 pairs) of trees (Møller 1995). APM succeeds in attaining 39 pairs consisting of one healthy and one diseased tree. If 5% of the trees in the population developed the disease in course of the period, the following frequencies of non-diseased (0/0) pairs, one tree diseased (1/0), and two trees diseased (1/1) should be expected: 0.9025, 0.095, and 0.0025. respectively. In a sample of 43 or 68 pairs this means 4.09 or 6.46 1/0 pairs, respectively. Møller 1999 got 39 pairs! A plausible explanation is required.

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From: Kenneth Otter [<mailto:otterk@unbc.ca>]
Sent: Tue 29/11/2005 6:09 PM
To: Rabøl, Jørgen
Cc: Kenneth Otter
Subject: RE: APM

Dear Dr. Rabøl

Thank you for your commentary, however, we are not considering publication of any further material on Scientific Misconduct at the Newsletter, and we are not willing to open the debate of Anders guilt/innocence here. I agreed to publish the report of Anders only as it points the way to how such cases need to be

conducted in the future - it was strongly edited to reduce the defensive nature that Anders originally felt necessary to take. Having spent nearly two years at the University of Copenhagen shortly after Anders left, I had to concur that it would be difficult for Anders to receive an impartial hearing in Denmark, as there was a great deal of animosity towards him personally following his departure. The Danish council, in my opinion, was negligent in not having greater representation from international scientific community, as that done by the CRN. That latter panel was less 'forgiving' than Anders claims, finding neither evidence of willful misconduct, nor evidence against it, and censures Anders for not being more diligent. However, by pointing out in his article that this report is available for public scrutiny, ISBE readers can make their own decisions on the details of Anders' case. Regardless, it is imperative that punishment fits the crime in these cases; regardless of what I think of how Anders deals with data, the punishment imposed by the Danish review of his case was, in my opinion, out of keeping with the severity of the offense and the efforts made to rectify it. Based on this, I felt it important to publish the points that Anders raises to create an agreed-upon format for dealing with and setting censure levels on allegations of misconduct.

I appreciate your perspective on this matter, but I will not be accepting any commentaries that merely dwell on this one case without providing solution to the larger issue of dealing with allegations of misconduct. Such material would more likely be used by the executive council in their development of a panel to create such guidelines.

Sincerely,

Ken Otter, Editor, ISBE Newsletter

-----Original Message-----

From: Rabøl, Jørgen [<mailto:JRabol@bi.ku.dk>]
Sent: Tuesday, December 06, 2005 5:06 PM
To: Kenneth Otter
Subject: RE: APM

Dear Ken,
I will answer you later in more details.
J

The following is an email send by Ken Otter to me on 7 Dec. 2005

Jørgen,

I look forward to hearing your reply.

I do feel it fair to warn you, however, that I won't have much lee-way about accepting any commentaries about Moller's case - the ISBE council has decided that they will not engage in a debate about this or any specific case, only on general issues. The council were involved in the decision to publish and provided editorial feedback on the two articles the Newsletter has published on this topic - both articles were allowed only because they covered larger issues

than arguments for or against any one case. The second article (Moller's) was allowed only because it was felt it added a component missed in the first article, the discussion on due process. Both articles were allowed to draw upon specific cases only so far as to make their argument, and both were very heavily edited by myself and at least two other members of council. Moller's article in particular was edited through several rounds and accepted only after substantial changes had been made based on both my own and other's requirements. Both I and the council feel that this topic has been sufficiently covered in the Newsletter that we do not feel that there is room for further constructive discussion; at this stage, the discussion of how to proceed will be better decided by an international committee that can prepare recommendations for standardized policies on what constitutes scientific misconduct, and how to adjudicate cases once accusations have been laid.

Again, I look forward to receiving any replies from you on this matter, but the discourse will likely have to be conducted at an unofficial level, as the official stance of the society is the one I outline above.

Ken

Ken Otter, Associate Professor (Biology)
Ecosystem Science & Management Program
University of Northern British Columbia
3333 University Way, Prince George, British Columbia V2N 4Z9
phone - (250) 960 5019
fax - (250) 960 5851
<http://web.unbc.ca/~otterk/index.html>

16 jan. 2006 the following email was send from me to Ken Otter:

Hi Ken,

Back from Australia the following comments. Scientific misconduct seems on its way as a worldwide epidemics as it appears from the recent reports from Korea and Norway.

Cheers J

A slightly changed version of the article was attached plus some **Further commentaries** after having re-read the Commentary by APM and the emails from Ken Otter

The most important of these commentaries were:

The APM commentary for a first consideration appears reasonable but with the knowledge that APM is a serious misconductor one feels the hypocritical undertone and can only guess how pathetic it was as a manuscript before Ken Otters improvements and changes.

APMs memory of what the Oikos editor Nils Malmer said and did is not very precise. Malmer forced APM to retract the fabricated paper and dictated the retraction word by word. Later on, Danish UVVU contacted Malmer with the purpose to compare the data files of APM send to UVVU with the data files send to Oikos (APM also send data to Oikos said to be measured by

himself). However, according to Oikos these data could not be the ones behind the table values. Therefore, Malmer forced APM to retract the paper. Malmer also prevented an open debate in Oikos on Møller & de Lope (1998) by withdrawing a critical Forum-commentary of Jette Andersen and me.

APM is much concerned about that “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”. “Finally, the sanctions - - - non-sanctioned punishment should themselves be subject to sanctions similar to those imposed in cases of scientific misconduct” (in the **Recommendations**) rises from the same stand and such points of view are of course to be expected by a misconductor who wants to carry out his fabrications, filtrations etc. under so much protection as possible.

APM on the whole is very worried about that referees of grants and scientific papers and people in committees considering scientific fraud should have had even the slightest critical/negative touch with the person /accused in consideration. This means that if I only once in a personal discussion with APM was a little critical about say fluctuating asymmetry then I could never be a referee on one of his many papers, I could never be in a committee investigating APM for fabrications, and if I dared to sue him for scientific fraud I should myself be investigated and accused with the same eager and intensity as him. APM clearly means that he personally should be allowed to select people off if he for some real or imagined reason feel they are against him. Back in the world are now only people positive and fond of APM and in such an ideal world he would never have problems being cleared for his fabrications and with approval of his many low-probability papers.

Finally, in the email to Ken Otter 16 Jan. 2006 I send the following as an attachment:

Dear Ken,

I read the two emails carefully (and once more the Newsletter commentary by APM and your editorial) and took my time to (re)consider the situation.

The hard facts are that when editing/improving the commentary of APM and rejecting the commentary of mine you very significantly improved the respectability and apparent innocence of APM. So now APM largely accepted by two important scientific journals and French CNRS has no need to go to the Danish court system (nor Strasbourg) to be cleaned (he will claim that he never had or will have a fair trial in Denmark).

The cardinal is back in his position in the Catholic Church. Perhaps a little weakened, and very probably he will never be Pope. *After a very fine, detailed examination of the facts and a meticulous search for evidence, a committee of four wise angels has found that it is not possible to establish formal proof of intent of paedophilia on the part of the cardinal (the other possibility was intent of gerontophilia on the part of 450 choirboys). Lacking the material evidence necessary to establish innocence, the committee was equally unable to reach this conclusion. In such a situation, by heavenly legal principle, it is the presumption of innocence that must be applied.*

Congratulations.

I have some further comments to your emails.

You were in Copenhagen for two years shortly after APM left and of course there was some talking, wondering and bad feelings about his behaviour and sudden run away to Paris in 1996. However, you cannot seriously mean that this had any effect on the work and decision of UVVU starting four years later. Furthermore, all three members of the ad hoc committee came outside the University of Copenhagen.

I was a near colleague of APM in 1994 to 1996, and was the one who submitted a debate article to the journal of Oikos, and later on asked for an UVVU investigation. I also read much of the correspondence between APM on the one side and Oikos, UVVU, Danish Ministry of Science and the Danish Ombudsman on the other side, so I have insight in the mentality of APM from an other side than his friends and scientific colleagues. I am not a medical doctor or psychologist so I shall refrain from making a diagnosis but he seems unable to appreciate that he very often lies and fabricates. At least he thinks he has all the rights to do so. Therefore, he also appears very convincing in his argumentation and defence.

You write the CNRS report "is available for public scrutiny, ISBE readers can make their own decisions". This is not true. The basic material upon which the report is resting is not available for scrutiny. Furthermore, the committee of so called wise men only used material submitted by APM and CNRS and it is a farce and a scandal to designate the work of this committee independent.

You write that it is "difficult for Anders to receive an impartial hearing in Denmark". Does this also apply to the Danish court system if APM sues UVVU or me?

You write that "the punishment imposed by the Danish review of his case was - - out of keeping with the severity of the offense and the efforts made to rectify it". Do you really mean that there is any excuse and forgiveness for data fabrications? Come on Ken. This weird person submitted fabricated data to UVVU (as realized even by CNRS) and as demonstrated in the JAISBE document send to you (and also by the ad hoc committee of UVVU) this material could only be fabricated/assembled by himself.

Danish UVVU treated/considered APM independent of his position as a most prominent/significant officer in the scientific system. In spite of all his arrogance and unwillingness to cooperate he had a fair trial concentrating on the data presented in the paper and the data he submitted to UVVU. On the other hand, Oikos, and in particular CNRS and you/your journal, consider the case as an intrinsic/political affair of the established system. No one outside the system has any right or weight to sentence a prominent officer of system. The system wants to control the discussion about scientific misconduct, and in particular French CNRS cannot live with an admission that they for so long time fostered such a significant misconduct. Furthermore, all former APM co-authors are trapped in a difficult position. Finally, the system wants no uncontrollable extension of the case into a discussion of the referee system and the hypothesis testing method which both have been heavily misused for a long time.

Clearly, Jette Andersen (the drunken technician who mis-measured the leaves) and I cannot in any way accept the systems accept of APM for not being the person who fabricated/assembled the data submitted to UVVU. If APM was not the fabricator, we were. Furthermore, as already told he accuses us for using another fabricated data file for opening the case. So the APM case will go on and on until he is finally ejected and not protected by the system.

Cheers J

20 Jan. 2006 Ken Otter send the following email:

Dr. Rabol,

Thank you for the follow up messages with respect to Moller`s article in the recent newsletter. I have carefully read your arguments in both the attached coverletter and revised article, and my decision on this matter remains unchanged. However, I have forwarded your original and follow-up messages, and our revised article to ISBE President Jack Bradbury to obtain his opinion on these matter. He and I will get back to you as soon as possible.

Sincerely, Ken Otter

24 Jan. 2006 the ISBE President send the following email:

Dear Dr. Rabol,

Ken Otter has forwarded your proposed manuscript and arguments for it to me. I have to agree with Ken`s decision not to publish your contribution. The newsletter is mainly intended to be informational for Society members, even in its Commentary section. Like many fields in science, behavioral ecology has no published sets of standards for proper conduct, nor agreed upon protocols for dealing with misconduct. The first commentary by Montgomery and Birkhead made a case for discussing these issues in some appropriate (non-newsletter) venue, and they offered to collect input from colleagues, on their own, to be summarized at that other venue. There was never any intention to make the Society newsletter the relevant venue. The second commentary by Moller provided additional arguments for why more explicit protocols were needed. While you apparently feel that giving Moller any public forum is unfair to his critics, note that both articles were heavily edited to keep the focus as much as possible on the general issues.

Ken, the ISBE Executive, and I all feel that our members are now sufficiently alerted to this particular issue. In addition, we also do not feel that any Society newsletter is the place where such standards and protocols should be worked out, and it is definitely not the place to adjudicate any current case. I disagree that what was published constitutes a Society stamp of agreement on everything that it said. That has never been true of any of our prior commentaries, and it was not true with either of these recent contributions. One function of commentaries (is) to provoke interest in a topic. Our members are clever people, and when they read such an article, they know wellthat it includes opinions of the author(s). How they feel about those opinions is up to them, not the Society.

I hope this will clarify why we have declined to publish further commentaries on this topic for now. It is not, as you suggest, because the Society has taken a stand on this particular case. It has not and cannot. But our members should now be thinking about these issues in general, and hopefully, in the proper venue, clarifying protocols and standards that the community can use in future situations.

With best wishes,

Jack Bradbury

25 Jan. 2006 I emailed the following to Jack Bradbury and Ken Otter:

Hi Jack,

Thank you for your patient and inforatory email. I understand you and of course I accept your point of view, but at least Ken in his editorial to the commentary of APM should had added that APM – in his own interest and for the interest of the ecological community – should investigated by a committee outside CNRS, because as admitted by Ken the “clearance” of CNRS is not satisfying for APM or anyone.

Cheers Jørgen

Hi Ken,

As I emailed to Jack Bradbury I was lacking in your editorial comment that you said that APM should be investigated by a real independent committee outside France, because the CNRS decision is not the paper worth it is written on.

I send you the document overview¹ about the case as it evolved.

J