Documentation for allegations of various misconducts, 2nd ed.

Bryan Schønecker, 2015

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1.0 Introduction.

Most of the eleven allegations you are about to read concern plagiates and misrepresentation of merits in relation to four papers and a related PhD dissertation. Two allegations concern plagiates of my work and the rest are related misconducts that I believe also deserve to be subjects to appropriate *Errata*.

In brief, four of these allegations have been filed for investigation to the journal Applied Animal Behaviour Science (**AABS**) in December 2013; ten were filed to the University of Guelph (**UoG**) in spring 2014 and all eleven were filed to the University of Witwatersrand (**Wits**) at about the same time. The outcome has in all cases been in favour of the accused parties and subsequent appeals to Elsevier's ethics team, the new President of UoG, and the present Principal of Wits, have all been completely dismissed.

However, I do not acknowledge the integrity of any of these investigations and treatments of appeals because obvious conflicts of interests have been persistently ignored; because about 50-90% of received allegations have also been ignored, and because I find the accompanying justifications to be either invalid, irrelevant, or both. Since I apparently cannot rely on any of the relevant "systems" to conduct a proper investigation, I am now going to present my documentation and make the *Errata* myself

[Note July 2015: Paper B, D and E's plagiates have now been addressed in my three "Open reviews" on Researchgate. The reason for this 2nd ed. of "*Documentation for allegations of various misconducts*" is my decision to publish the original Appendix 6 and a previously deleted personal speculation/suggestion in Appendix 9].

2.0 Publications involved.

Paper A; Schønecker B (2009). "*Increased survival and reproductive success associated with stereotypical behaviours in laboratory-bred bank voles (Clethrionomys glareolus)*" Appl Anim Behav Sci 121: 55-62.

Paper B: Jones MA, van Lierop M, Mason G, Pillay N (2010) *Increased reproductive output in stereotypic captive Rhabdomys females: Potential implications for captive breeding*. Appl Anim Behav Sci 123: 63-69

Paper C: Jones M, van Lierop M, Pillay N (2008) *All a mother's fault? Transmission of stereotypy in striped mice Rhabdomys*. Appl Anim Behav Sci 115: 82-89

Paper D: Jones MA, Mason G, Pillay N (2011) *Early environmental enrichment protects captive-born striped mice against the later development of stereotypic behaviour*. Appl Anim Behav Sci 135: 138-145

Paper E: Bechard A, Nicholson A, Mason G (2012) *Litter Size Predicts Adult Stereotypic Behavior in Female Laboratory Mice*. JAALAS 51: 407-411

Dissertation: Jones MA (2012). *The causes and consequences of stereotypic behaviour in the striped mouse, Rhabdomys*. PhD dissertation. University of the Witwatersrand, South Africa. Available from this URL: http://wiredspace.wits.ac.za/handle/10539/11827

Thesis: Van Lierop, MC (2005). *Stereotypical behaviours in the striped mouse Rhabdomys pumilio: evaluating the coping hypothesis*. MSc thesis, University of the Witwatersrand, South Africa. Available through this URL: http://wiredspace.wits.ac.za/handle/10539/1737

3.0 Accused parties and relevant details.

Mathew Carl van Lierop (**MvL**). MSc (2005 - ethology) from Wits and MA (2011- environmental ethics) also from Wits. Acknowledged in MAJ's 2004 MSc thesis.

Dr. Megan Anne Jones (**MAJ**). MSc (2004 - ethology), MSc (2011 - clinical psychology) and PhD (2012 - ethology). The 2004 MSc and the 2012 PhD was from Wits. Acknowledged in MvL's MSc thesis.

Dr. Neville Pillay (**NP**). Ethologist and tenured professor at Wits. Supervised MAJ's 2004 thesis and MvL's 2005 thesis. Main supervisor on MAJ's PhD project. Member of the editorial advisory board at AABS when I sent the Editor-in-chief (now Co-editor-in-chief) four allegations in December 2013.

Dr. Georgia Jane Mason (**GM**). Ethologist. Canada Research Chair at UoG since 2004; tenured professor at UoG since 2006 and Honorary Research Fellow at Wits 2010-13. Member of the editorial advisory board at AABS 1997-2000 (the first three years of the present Co-editor-in-chief's appointment to Editor-in-chief). Co-supervised MAJ's PhD project and is also acknowledged in MvL's MSc thesis.

Allison Bechard. MSc (2009 - ethology) from UoG. GM supervised this MSc.

Dr. Anthony Nicholson. Veterinary anaesthesiologist.

NB: Information regarding memberships of AABS' editorial advisory board, and various appointments, were obtained from publicly available (online) personal CVs and web sites.

4.0 Personal perception of level of guilt among accused parties:

Innocent in allegation of plagiates: Dr. Nicholson, because his field of expertise is anaesthesia - not animal behaviour and not animal stereotypies. I would consequently not expect Dr. Nicholson to have a thorough knowledge of the relevant literature nor that he would have noted the plagiates in Paper E.

Innocent in allegation of plagiates: Allison Beschard (see my reasons in Appendix 5).

Guilty with redeeming circumstances: MvL is qualified to recognize every problem in papers B and C and must share the responsibility with their other authors. However, his 2005 thesis has been grossly exploited, and I do not consider MvL the *primus motor* behind any misconduct described in the following appendices.

Guilty and fully aware: MAJ, GM and NP. Support for this personal notion are presented in the appendices.

5.0 Summary of Appendix 1-9 and their included allegations of various misconducts.

This summary is solely for your convenience and summarizes the contents of nine appendices sent to the Principal of Wits in Autumn 2014 (you will find the original appendices below Chapter 5.1). This 2nd version of *Documentation for allegations of various misconducts* includes a personal speculation and suggestion in Appendix 9 (previously deleted and marked with **red** in the 1st version) and Appendix 6 is now the full original version sent to Wits. I have finally indicated whether AABS, UoG and Wits received a given allegation, and if so, the result based on my interpretation of received decisions and justifications.

Appendix 1:

Summary: It is my opinion that **Paper B** (2010) plagiates the idea behind a novel hypothesis I published in **Paper A** (2009). I believe my idea has been used as skeleton for two of the hypotheses presented in Paper B.

Decision (plagiarism of idea): AABS (reluctantly dismissed); UoG (dismissed); Wits (dismissed).

Appendix 2:

Summary: The main result in Paper B is presented as a novelty despite the fact that it appears in the Result section of **Paper C** (2008). Paper C is used as reference in Paper B, but not in this context. Paper B is a covert redundant publication of a major result and a "salami" paper. GM is acknowledged in Paper C.

Decision (covert redundancy): AABS (ignored); UoG (ignored); Wits (ignored).

Decision ("Salami"-paper): AABS (dismissed); UoG (ignored); Wits (ignored).

Appendix 3:

Summary: The experimental basis for Paper B and Paper C is presented in Chapter 2 of MvL's sole-authored MSc thesis which includes acknowledgements for NP (supervisor), GM and MAJ. It follows that MvL is the rightful first-author on both papers - not MAJ: that NP, GM and MAJ cannot claim "plausible deniability" in relation to MvL's thesis and that NP and GM have failed their obligation as supervisors to present good examples when co-authoring past/present students' papers.

Decisions (misrep. of merits on papers B and C): AABS (not received); UoG (dismissed); Wits (ignored).

Decision (supervisors fails their responsibility): AABS (not received); UoG (ignored); Wits (ignored).

Appendix 4:

Summary: **Paper D** (2011) is build entirely upon a variant of a pioneering experiment presented by MvL as Chapter 3 in his MSc thesis. Since Paper D does not mention MvL or his thesis with a single word, it follows that the authors of Paper D has plagiated the idea behind MvL's experiment.

Decision (plagiarism of idea): AABS (not received); UoG (ignored); Wits (ignored).

Appendix 5:

Summary: **Paper E** (2012) plagiates my hypothesis and the idea behind a third party hypothesis from 1997.

Decision (plagiate of my hypothesis): AABS (ignored); UoG (ignored); Wits (ignored).

Decision (plagiate of idea behind third party hypothesis): AABS (not received); UoG (ignored); Wits (ignored).

Appendix 6:

Summary: Wits confronted the authors of Paper B after having received appendices 1 + 2. The result was a rebuttal found "*plausible*" by the main investigator who decided to take no further action. I returned the rebuttal with my comments (Appendix 6) to appeal this decision, and also added a bonus (appendices 3-5).

Decision (my comments to rebuttal): AABS (not received); UoG (not received); Wits (ignored).

Insert:

Summary: GM posted some comments under the headline *"Plagiarism charge!"* on her lab's blog about a week after I filed appendices 3-6 to Wits. Although I did enjoy GM's vivid description of the way allegations of plagiarism used to be presented in the good old days, my main reason to resurrect this classical straw man (it was deleted after some weeks) is because GM states: "*This one focuses on Megan Jones' work*". I suggest you now re-read the summary of Appendix 3.

Appendix 7-1:

Summary: MAJ's 2012 dissertation includes a plagiate of the original version of Wikipedia's article "*Rhabdomys*", which was uploaded in October 2006 by someone using the name "Madmegs". If MAJ can prove ownership of the original upload to Wikipedia, this plagiate reduces to a covert redundant publication.

Decision (plagiate of Wikipedia): AABS (not received); UoG (not received); Wits (ignored).

Appendix 7-2:

Summary: Documentation for the allegation in Appendix 7-1. 497 words from "Madmegs" original upload can be found in MAJ's dissertation's 668-words introduction to *Rhabdomys* (75%). When allowing for simple paraphrases of "Madmegs" original, I find that the number of words in similar sentenses in "Madmegs" and MAJ's introductions to *Rhabdomys* increases to 533 (80%).

Appendix 8:

Summary: MAJ's dissertation is based on papers B, C, D; two other papers co-authored by NP and GM, and a manuscript with no assigned authors. The dissertation includes this note on page 13: "*In the multi-author work already published, I was the primary designer and executor of the experiments and was responsible for the data analysis, the initial write-up, and the preparation of the manuscripts for publication*" which basically translates into a claim of having made the entire Chapter 2 of MvL's thesis. This claim is supported in several other parts of the dissertation.

Since MvL's thesis includes the declaration that "*this dissertation is my own unaided work*", it follows that someone must have inflated his or her contributions to MvL's thesis way beyond recognition. I believe this someone to be MAJ. Considering NP's and GM's involvements in MvL's thesis it follows that they must have been aware of MAJ's true merits in relation to MvL's thesis (see chapter 5.1 below for an additional reason I find it highly unlikely that MAJ's claim is true).

Decision (misrepresentation of merits): AABS (not received); UoG (ignored); Wits (ignored).

Appendix 9:

Summary: Contains my general and specific conclusions to these allegations of various misconducts. Main conclusions are that MAJ, NP and GM have been engaged in several misconducts; that MAJ have earned her degree on false pretences, and that NP and GM have failed their responsibility as supervisors.

Decision (conclusions): AABS (not received); UoG (ignored); Wits (ignored).

Decision (failure of supervisors) AABS (not received); UoG (ignored); Wits (ignored).

5.1 Comments to summary of Appendix 8.

MvL's declaration of having made an unaided MSc thesis is dated "*March 2005*" which is presumably the month he handed it in to his supervisor NP at the School of APES, Wits.

MvL's method chapter (2.2.1) provides duration for all experimental phases, except the first, which was to capture 62 African striped mice in the wild and use them to start his colony. In brief, MvL would spend anywhere from a week to months (not specified) to capture 62 wild-living striped mice; use about 320 days to produce the experimental material for his Chapter 2 (i.e. about 10 months) and an additional 129 days to complete his following (and final) experiment, providing the material for his thesis' Chapter 3.

My best "guesstimate" is that the experimental work behind MvL's thesis' Chapter 2 must have lasted a year from start to finish; that it must have taken about a half year to produce the data for his Chapter 3, and then another half year to write his 54-page thesis, including two manuscripts, 89 references, 5 figures and 5 tables.

In short, I guesstimate that MvL must have started his MSc thesis work around March 2003.

MAJ's first MSc thesis also contains a declaration of having made the work without aid, and this declaration is dated "*December 2004*", which is presumably the month it was handed in to her supervisor at Wits (also NP). Its title is "*Social foraging in captive baboons*", which is not exactly related to MvL's study of stereotypies in African striped mice, and it includes an acknowledgement for MvL who at the time functioned as enrichment coordinator in Johannesburg Zoo and who, along with other staff members, "*enabled me to conduct this study*".

MAJ's thesis provides the information that her baseline study of two groups of baboons was conducted in April and May 2003, and that her actual experiments took place between September 2003 and February 2004. Ten months after these experiments were terminated, MAJ would hand in a thesis containing 123 pages; 159 references; 24 figures and 12 tables.

Now, MAJ's 2012 PhD dissertations contains the claim that she designed and made everything in relation to the experiment resulting in papers B and C which basically translates to everything presented in Chapter 2 in MvL's 2005 thesis. If so, I find this to be an obvious question:

- How likely is it that MAJ would volunteer to plan, carry out and analyze data from a (guesstimated) 12-months experiment targeting stereotypies in captive-born striped mice, and then write the manuscript for Chapter 2 in MvL's MSc thesis at about the same time that she would be spending eight months carrying out experiments for her own MSc thesis targeting social foraging in baboons?

My personal answer is "highly unlikely".

Appendix 1.

Plagiarism of my hypothesis associating fertility and stereotypies in Paper B.

My **Paper A** [1] formulates and presents the novel hypothesis that "stereotypy-genes" are linked with genes of relevance for fitness, more specifically fecundity ("fecundity-genes" in the following). I must emphasize that Paper A is, as far as I know, the first publication ever to even remotely hypothesize about some sort of link between genes influencing stereotypies and genes influencing fecundity.

Paper B [2] by Dr. Megan A. Jones (**MAJ**), Mr. Mathew C. van Lierop (**MvL**), prof. Georgia J. Mason (**GM**) and prof. Neville Pillay (**NP**) presents three hypotheses targeting an association between fecundity and stereotypies. I consider two of these to be more or less twisted variants of the idea behind my hypothesis.

Paper A is used as reference in Paper B on four-five occasions but not in this particular context. The authors also make it clear to the reader that they consider these three hypotheses as their very own intellectual creations (Discussion, p. 68: "*In Section 1, we suggested three possible mechanisms…*").

Below you will find two corresponding excerpts from these papers: The relevant backgrounds are marked with blue and the resultant hypotheses are marked with red.

Relevant sections from the introduction to Paper A:

"*Bank voles are well suited for studies of stereotypic behaviour since they readily develop a suite of easily recognisable stereotypies, as a role of thumb provided they are born in captivity and housed in barren cages (Cooper and Nicol, 1991, 1994; Schoenecker et al., 2000; Sørensen, 1987; Sørensen and Randrup, 1986; Ödberg, 1986). The smaller/more barren the cage and the more restricted concerning possibilities for social contacts, the more frequent are the voles developing stereotypies (Cooper et al., 1996; Sørensen, 1985, 1987; Ödberg, 1987). Large differences in individual levels of stereotypies among bank voles were first published by Ödberg, 1986, who suggested that these differences probably were due to individual genetic differences in the predisposition to interact with discrete stimuli early in life. Later experiments with selective breeding resulted in a seven-fold difference in the proportion of offspring developing stereotypies (Schoenecker and Heller, 2000) and suggests the potentials in developing lines of high/low stereotypers and search for differential gene-expression relevant to the main neurotransmitters connected with development of stereotypies (e.g. serotonin, dopamine and gamma-aminobutyric acid).*

Stereotypic behaviours of the types occurring in captivity have never been described among voles in nature, and only once among wild caught voles maintained in captivity (Schonecker, 2009). Beside the inherent problem of long-term observations of cryptic animals preferring to be under cover, other factors could explain this discrepancy. For instance insufficient time to develop stereotypies in the wild (see discussion in Schonecker, 2009), lack of frustrating or stressful experiences among young voles in the wild (Schoenecker and Heller, 2000) combined with negative selection pressure by predators. It is therefore difficult to interpret the apparent strong genetic component in captivity-induced development of stereotypies. If such "stereotypy-genes", or better "segregating units" (Bruell, 1962), influencing proneness to develop stereotypies in captivity could be identified, they must at least be fitness neutral in the wild and the reason for their presence could be because they are situated near other segregating units influencing trait(s) more important to the fitness of wild-living voles. Such traits could e.g. be related to fecundity".

Relevant sections from the introduction to Paper B:

" *Whilst stereotypies typify sub-optimal environments (Mason, 1991; Mason and Latham, 2004), highly stereotypic individuals often show better welfare than less stereotypic conspecifics housed in similar sub-optimal conditions (Mason and Latham, 2004). Apparent benefits linked with stereotypic behaviour including reductions in heart rate (e.g. calves; Seo et al., 1998), and elevated reproductive success. In farmed mink (Mustela vison), for example, stereotypic females are sometimes more fertile and experience lower pup mortality than non-stereotypic females (Jeppesen et al., 2004; cf. Svendsen et al., 2007) and, in caged bank voles, stereotypic behaviour has recently been shown to be associated with better survival, fecundity, and therefore increased lifetime reproductive success (Schonecker, 2009). The mechanisms for such beneficial effects of stereotypic behaviour are unclear. It could be (1) that both stereotypy (Mason and Latham, 2004) and increased reproductive success (Broom and Johnson, 1993; Wingfield and Sapolsky, 2003) reflect relatively low stress in sub-optimal cages; (2) that stereotypic behaviour has an indirect pleiotropic effect on reproductive success through its association with the directly selected trait of activity level or body condition, as is sometimes the case in mink (Jeppesen et al., 2004); or (3) that being of a fecund genotype predisposes animals to stereotypic behaviour, through genetic or epigenetic means, the latter perhaps influenced by litter-size mediated variations in the maternal care received in infancy (Priestnall, 1972)."*

Comments to the relevant background information in Paper B:

The authors mention three of four peer-reviewed papers in total, specifically addressing stereotypies and fertility (see Appendix 2 for an explanation regarding the 4th paper). The three references provided by the authors leads to the conclusion that "*Schonecker, 2009*" (i.e. Paper A) is the only previous study showing an association between stereotypies and increased fertility in absence of a known confounder (i.e. weight).

Paper A was published four months before Paper B and is also the only one to offer a causal hypothesis for this association. It must consequently have been carefully read by the authors, as also indicated by the (somewhat perfidious) way it is used as reference in other sections of Paper B.

Comments to Paper B's three hypotheses:

The first hypothesis is what I would call a pseudo-hypothesis where an observation is presented as a hypothesis. Assuming that "A" is a reflection of "B" is the same as assuming that "B" causes "A", so the authors are in fact proposing that relatively low-stress in sub-optimal environments results in increased fertility and development of stereotypies. The authors attempt to repair their hypothesis in the discussion but still manage to dismiss it with ease. I suppose the real purpose of this hypothesis is to nurture the belief that the following hypotheses are also made by the authors.

The second hypothesis basically states that "stereotypy-genes" have an indirect effect on other genes (due to the expressions "*pleiotropic effect" and* "*selected traits*") important to fertility because stereotypy-genes are associated with other such fertility-promoting genes. As I see it, the authors have found an alternative way to present my hypothesis and intend to cover this up by the confusing reference to an "*indirect pleiotropic effect*". Keeping the "*through its association*" is, albeit, a dead giveaway.

The third hypothesis states "*being of a fecund genotype predisposes animals to stereotypic behaviour*". The rest of the sentence is insignificant ("*through genetic or epigenetic means*") and the main purpose with the reference to Priestnall's paper could be to further the impression that proper reference has been given. Incidentally, Priestnall's paper does not mention stereotypies at all.

At this point I would like to emphasize that my hypothesis can be expressed as: "Stereotypy-genes" are linked with "fecundity-genes". I in fact used the words "*situated near*" which covers "besides". That means an alternative version could be: "A stereotypic genotype predisposes animals to relatively increased fertility due to close linkage between stereotypy-genes and fecundity-genes", without loosing or adding information.

Switching the order of these genes would result in this formulation: "A fecund genotype predisposes animals to stereotypic behaviour due to close linkage between "fecundity-genes" and "stereotypy-genes"", again without loosing or adding information.

Bottom-line is that the authors basically hypothesize that "Fecundity-genes increases both fertility and proneness to develop captivity-induced stereotypies". They might as well have hypothesized that "Stereotypy-genes increases proneness to develop stereotypies and predisposes the carriers for increased fertility". If linkage is strong between fecundity- and stereotypy-genes, my original hypothesis will be reduced to the authors' version, because such genes will in reality most often be transferred to the offspring as a unit.

Alternatively, the authors could present the hypothesis that "Fecundity-genes are linked with stereotypy-genes", due to the word "*predisposes*". In this case they only need to re-arrange my associative hypothesis 180 degrees to arrive at their hypothesis.

Conclusion: The idea behind my hypothesis has been used as a "skeleton" on which the authors have constructed their two final hypotheses. The concept of plagiarism includes the use of ideas by others without proper credit.

Personal opinion: Claims of intellectual ownership in the discussion supports my notion of deliberation. Furthermore, according to Paper B (but see Appendix 2), my Paper A is the first paper ever to demonstrate an association between stereotypies and relatively increased fecundity not to be explained by a confounder. Paper B pretends to be the second paper to show such an association and it was published a mere four months after Paper A. I consider the odds against the notion that the authors can use Paper A as reference 4 (or 5) times without also noting its hypothesis to be astronomical. **Note**: The point with this exercise is to show relatedness between my original idea and their plagiates because, as demonstrated in Appendix 5, it is really easy to make "new" hypotheses from whatever suitable skeleton provided by previous creators.

Appendix 2.

The main result of Paper B was published two years earlier in Paper C.

The purpose of Paper B (2010) is to present a supposedly first peer-reviewed demonstration of an association between relatively increased fecundity and stereotypies in striped mice. However, as I have only recently noted, MAJ, MvL, NP had in fact already published this main message two years earlier in **Paper C** [3].

Here is the relevant excerpt from the Results chapter of Paper C **[**square bracket added by me**]**:

- "*Of the 15 pairs established per treatment, 93% each of the S♀–S♂ and S♀–NS♂ treatments, 80% of the NS♀–NS♂ treatment, and 73% of the NS♀–S♂ produced litters (Table 3). Treatment was not a significant predictor of reproductive success (Wald* ***[****Chi square with 3 degrees of freedom - my text****]*** *= 3.13, p = 0.372) or of the number of litters produced (F3,47 = 1.04, p = 0.384). However, treatment significantly influenced the total number of offspring produced ($F_{3,47}$ = 8.82, p < 0.001): treatments in which the mother was stereotypic (S♀–S♂, S♀–NS♂) produced about twice the number of offspring than treatments in which the mother was non-stereotypic (NS♀–S♂, NS♀–NS♂; Table 3)*".

I used Paper C as a reference to stereotypies in striped mice in Paper A but had at that time not noticed that Paper C *also* included a demonstration of significantly increased fecundity among stereotypers. The reason for this regrettable oversight on my behalf is that the authors do <u>not</u> discuss this finding at all but just mention it briefly like this in the Discussion:

- "*Our findings, in combination with previous work in Rhabdomys showing that the tendency to develop stereotypy is related to the occurrence of stereotypy in the biological mother (Schwaibold and Pillay, 2001), point towards a genetic basis for stereotypy <u>as well as a positive relationship between stereotypy and reproductive output</u>. However, the larger maternal than paternal …*"

This positive relationship between stereotypies and fertility is <u>also</u> not incorporated in the Conclusion, nor is it mentioned in the Abstract or indicated in the Title of Paper C.

Considering the importance "*Pillay and his team*" would later attribute to this discovery [4], I find it noteworthy that the authors behind Paper C at the time decided to "downplay" this detail so hard in this in effect pioneering manuscript. I also find it odd that the reviewers did not notice these omissions because the journals instructions for authors clearly state: "*All results presented in the Results section should be discussed (if they do not warrant discussion, they do not warrant inclusion)*".

GM has evidently been well aware of the results in Paper C since she is mentioned in its Acknowledgement: "*We are grateful to Georgia Mason for much helpful discussion*".

Conclusions:

1) The authors have made Paper B on the basis of a barely detectable result from Paper C and then "beefed" it up with additional data related to growth and inter-litter intervals, and all harvested during the same experiment (see Appendix 3).

2) Paper B is a hybrid between a "covert redundant publication" and a "salami publication".

Personal opinion: Since Paper B solely uses Paper C as reference to stereotypies in striped mice, it is my opinion, that the authors deliberately chose to cloud the fact that about half its results had been published two years earlier in Paper C. I personally <u>suspect</u> that … [**suspicion deleted**].

I personally believe this paper should be retracted and consider anything less of an *Erratum* unacceptable.

Appendix 3.

Replacement of a rightful first-author on Paper B and Paper C with a secondary author.

This is MvL's MSc thesis:

- Van Lierop, MC (2005). *Stereotypical behaviours in the striped mouse Rhabdomys pumilio: evaluating the coping hypothesis*. MSc thesis, University of the Witwatersrand, South Africa. Available through this URL: http://wiredspace.wits.ac.za/handle/10539/1737

MvL's thesis includes a reference to "*G. Mason (pers comm.)*" on page 6 and this is an excerpt from his acknowledgement section (p. V): "*I would like to extend my huge thanks to Prof Neville Pillay who expended considerable time and energy discussing many extraneous topics, while keeping me focussed on my work and dedicating much time to reviewing this document. Your guidance, support and friendship are very much appreciated.*

I thank my many long suffering colleagues at the Johannesburg Zoo for their time and input, as well as Megan Jones, Dr Georgia Mason and Dr Rob Young for their assistance and willingness to discuss numerous ideas and theories."

MvL's thesis includes two experiments and both are presented in the form of manuscripts. The first manuscript is presented in **Chapter 2** under the headline "*Fitness consequences of stereotypic behaviour in the striped mouse Rhabdomys pumilio*".

Chapter 2 replicates a previous experiment by Schwaibold & NP [5] addressing inheritance of proneness to develop stereotypies in striped mice The main difference being that MvL used all four possible combinations of stereotyping and non-stereotyping parental mice (the same "unique" design I used in 2000, [6]) and includes data showing that stereotypers are relatively more fertile.

The methods presented in Chapter 2 are similar to those of Paper B. The results from MvL's fertility-study, however, differ slightly from those presented in Paper B. The reason appears to be that MvL relates the number of litters, pups, etc. to the total number of experimental pairs in the four breeding combinations whereas Paper B relates the same data to the number of reproducing pairs.

The methods presented in Chapter 2 also match those of Paper C, which presents the result from MvL's experiment with heritance of stereotypies and also makes a first presentation of the discovery that stereotypers have relatively increased fertility (see Appendix 2).

The acknowledgement section in MvL's thesis includes this sentence: "*Approval for this study was granted by the Animal Ethics Screening Committee of the University of Witwatersrand (AESC: 2003/23/2A and 2003/24/2A)*".

The only difference to similar declarations in Papers B and C is the deletion of "*and 2003/24/2A*".

Finally, MAJ' 2004 MSc thesis includes a reference to a manuscript (p. 86: *"van Lierop et al, in preparation"*). What manuscript by "*van Lierop et al.*" was MAJ referring to in her thesis? Paper C from 2008, or perhaps what could have been an alternative version of Paper D (see Appendix 4)? This is MAJ's thesis:

- Jones, M. A. (2004). Social foraging in captive baboons: implications for enrichment. MSc thesis, University of the Witwatersrand, South Africa. Available through this URL: http://wiredspace.wits.ac.za/handle/10539/215

Conclusions:

1) Papers B and C are based entirely on the experiment presented as Chapter 2 in MvL's MSc thesis.

2) MvL is the rightfull first-author on Paper B and Paper C - not MAJ.

3) MAJ, NP, and GM cannot claim "plausible deniability" in relation to MvL's thesis.

Personal opinion: I find it intellectual dishonest to claim/accept a first-authorship on a paper where the *primus motor* in all relevant aspects are someone else. Co-authors have a responsibility to prevent unjust substitutions and especially so when they, as in the case of NP and GM, are co-authoring papers with past and present students to whom they are obligated to present good examples.

Appendix 4.

MvL's idea for an experiment has been plagiated in Paper D.

Chapter 3 in MvL's 2005 MSc thesis has the headline "*The expression of stereotypic behaviour in striped mice raised in barren and enriched environments*".

MvL use striped mice to test the effect of shifts between two different cage environments on subsequent proneness to develop stereotypies. His experiment was pioneering in the sense that it was the first of its kind to use African striped mice. MvL did not publish this experiment in a peer-reviewed journal - it is only described in his thesis, which appears to have been made publicly available in 2006.

Van Lierop's experiment:

Animals:	Two groups of 40 F2. Weaned age 20 d. Housed in same-sex kin groups until age 25 d.
Schedule:	Group I: Enriched cages age 25-85 d. -> 2 days video-> Barren cages c. age 87-147 d.
	Group II: Barren cages age 25-85 d. -> 2 days video- > Enriched cages c. age 87-147 d.
	Two days video after the experiment (about age 147-148 d.)

Paper D [7], authored by MAJ, GM and NP, was published in 2011 and tests if striped mice maintained in enriched cages for the first roughly 2/3 of the experimental period, and thereafter is housed in barren cages, differs in proneness to develop stereotypies compared to striped mice maintained in barren cages for the entire duration of the experiment. In other words, Paper D is the second publication in the history of science to present a test of this type of experiments on African striped mice - the first was MvL's thesis.

However, Paper D includes no reference to MvL's thesis and no personal acknowledgement of MvL.

Paper D's experiment:

Animals:	Two groups of 24 and 36 F1.Weaned age 22 d. Housed in sibling groups until age 30 d.
Schedule:	Group I: Enriched cages age 30-170 d. -> Barren cages age 171-240 d.
	Group II: Barren cages age 30-240 d.
	Three days video between days 161-170 and three days video between days 231 and 240.

Conclusion: Paper D presents a variation of an experiment presented in Chapter 3 of MvL's MSc thesis but does not include any reference to this thesis, or any acknowledgement of MvL personally. The concept of plagiarism includes un-credited appropriation of ideas originally presented by others. Paper D consequently plagiates MvL's idea for his, in a sense, pioneering experiment.

Personal opinion: Both experiments have their pros and cons when compared to each other and also supplement each other nicely. The authors should of course have paid due credit to MvL's work instead of erasing the footsteps they so obviously were following.

An *Erratum* with reference to MvL's thesis would only be most appropriate, even in case MvL should have granted permission to follow his lead without due credit. In fact, I must insist on such an *Erratum*.

Appendix 5.

Paper E plagiates my hypothesis associating fertility and stereotypies and a hypothesis by Würbel & Stauffacher associating low weight at weaning and later stereotypies.

This is an excerpt from the Discussion (p. 410) of **Paper E** [8] by GM and two other authors (Ms. Allison Bechard - one of GM's former MSc students - and Dr. Anthony Nicholson):

- "*The cause-and-effect relationship between litter size and stereotypic behavior inherent in our original hypothesis, if correct, would then be consistent with the corpus of work showing lasting behavioral effects of the early social and physical environment (for example, social isolation[25] and enrichment[3,21,34]).*

  The hypotheses we offer assume that developing in a larger litter causes later effects on adult phenotypes. However, our data were merely correlational, and perhaps increased stereotypic behavior and large litter sizes covary for completely different reasons. In particular, maybe highly stereotypic dams have bigger litters and their offspring then inherit this behavior. For example, in both African striped mice and farmed American mink, offspring with stereotypic parents are more stereotypic as adults than are offspring of nonstereotypic parents.[11,16,26] Furthermore, stereotypic mothers of these species have higher reproductive outputs, including larger litter sizes.[13,15] "

- Note the omitted reference to Paper B (Paper E's reference #15) after the first underscored sentence. Paper E presents what appears to be a new hypothesis because of missing references to its origin (First disclosed in Paper A, then plagiated in Paper B, and now plagiated again).
- Note the missing reference to Paper C (Paper E's reference #16) following the last underscored sentence.
- Note this little "twist": Paper E offers the seemingly novel hypothesis that "*maybe highly stereotypic dams have bigger litters*" and then refers to Paper B for experimental support of this hypothesis - the very same paper also proposing an essentially similar hypothesis as possible explanations for its experimental outcome.

I believe Paper E's "*original hypothesis*" (first line in excerpt) also deserves a comment so first I would like to show what Würbel & Stauffacher, 1997 (Paper E's reference #35) have to say in their introduction:

From Introduction to Würbel & Stauffacher, 1997 [9]:

"*Laboratory mice are commonly weaned at 20 days of age. Weaning is associated with increased exploratory behaviour (Würbel et al. 1996) and attempts to escape the cages (Terranova & Laviola 1995; Würbel et al. 1996). These patterns are the source behaviours from which laboratory mice develop stereotypies subsequent to weaning (Würbel et al. 1996). … [snip…*

In the present study we investigated these relationships by manipulating weaning age and weaning weight of male ICR-mice. Because of a higher risk to fitness both precocious weaning and lower body weight at standard weaning age should be associated with higher stress levels and more attempts to escape the cage. In a final step we examined whether differences in the estimated risk to fitness derived from physical condition at weaning determine individual differences in adult stereotypy performance".

Now the Discussion in Würbel & Stauffacher, 1997: "*This supports our hypothesis that not only weaning age but also weaning weight affects the motivation to escape the new cage and to return to the mother*". In short, Würbel & Stauffacher, 1997, hypothesize that ICR mice that are light when weaned will be more prone to develop stereotypies than the heavy ones. Their central predictive variable in relation to adult stereotypies is weight when weaned.

This excerpt is from Paper E's introduction and describes the authors' "*original hypothesis*":

"*Together, these findings suggest that individual mice from large litters may be at increased risk of later stereotypic behavior because they receive less maternal care in infancy and because they weigh less at weaning*". *… … [snip…*

"*We predicted that due to the expected negative influence of litter size on development, mice from larger litters would be smaller at weaning and adult stereotypic behavior would be greater in mice from larger litters or mice that weighed less at weaning. We also predicted that delayed weaning would reduce stereotypic behavior and that pups from larger litters might particularly benefit from this practice*".

In short, Paper E from 2012 hypothesize that ICR mice from larger litters will be more prone to develop stereotypies (because of the logical and common observation that such pups are lighter on average than pups from small litters), and do so without the slightest credit to Würbel & Stauffacher for their original hypothesis.

Since I happen to have read Würbel & Stauffacher's 1997 hypothesis (as also demonstrated in Paper A), and Paper E's logical extension from 2012, I will for the sake of argument demonstrate how easy it is to make a "new" hypothesis on said basis:

- "New" hypothesis: "ICR mice from litters where the mother has been starved (e.g. a restricted diet of 60% of estimated daily caloric intake) while lactating will be more prone to develop stereotypies as adults than ICR mice from same-sized litters where the mother has been fed *ad libitum* with all kind of high-caloric food-types following parturition".
- Next logical extension could be the "new" hypothesis that pups from mothers which have been on a restricted diet from the time the mated with the male will be more prone to develop stereotypies as adults that pups from mothers fed *ad lib* from that time.
- Yet a logical extension could be the "new" hypothesis that pups from light mothers, that has also been on a restricted diet right from their own weaning, will have the relatively highest prevalence of stereotypies.

It is indeed very easy to make your very own and new hypotheses if fellow researchers have just provided a suitable skeleton to build upon. The hard part is to get the idea, and even if an original hypothesis is not formulated as "*We, the authors, hypothesize that…"* or "*Hypothesis:…"* I maintain that experienced scientific authors certainly should know exactly what is right and what is wrong in such cases.

Paper E uses Würbel & Stauffacher, 1997 [9], and their related 1998 study [10] as references #35 and #36, respectively. However, quite analogues to the way Paper B used Paper A as reference, Paper E only refers to Würbel & Stauffacher's papers in relation to their results - not in relation to their accompanying hypotheses.

Conclusions:

1) Paper E plagiates not only my hypothesis but also at least one of the two paraphrased and modified plagiates presented in Paper B.

2) Paper E should have paid proper credit to Würbel & Stauffacher, 1997, for having supplied the basis for its "*original hypothesis*". Again, the concept of plagiarism includes un-credited appropriation of ideas originally presented by others so Paper E also plagiates Würbel & Stauffacher's hypothesis.

Personal opinion: GM's co-authorship of Paper B and evident involvement in Paper C suggests that GM has been solely responsible for the plagiarism of my hypothesis. GM has previously published lengthy and thorough reviews of the literature in relation to animal stereotypies and she most certainly were aware of Würbel & Stauffacher's hypothesis long before co-authoring Paper E (see e.g. this 2004 paper [11]).

I have actually read Ms. Bechard's 2009 MSc thesis [12] and although it refers to Würbel & Stauffacher's hypothesis I doubt that she would have ..[**subjective reasoning deleted**]..recognized Paper E's "original hypothesis" as a plagiate. Since Paper E do not refer to my Paper A at all, and given that GM was also Ms. Bechard's supervisor on her MSc thesis, I further doubt that she would have realized the plagiate of the idea behind my hypothesis, nor have had the inclination to question this contribution to the manuscript.

I also doubt that Dr. Nicholson, a veterinary anaesthesiologist at the University of Adelaide, would have had the necessary background to notice said plagiates, so I believe GM introduced these plagiates personally.

Also, I assume that.. [**assumption deleted**]. This detail supports my lingering suspicion that.. [**suspicion deleted**].

I must insist on a proper *Erratum* to Paper E with reference to both Paper A's hypothesis and to Würbel and Stauffacher, 1997, for their original hypothesis relating low weight in weaned ICR-mice with a high risk of subsequent development of stereotypies in adulthood.

Appendix 6.

Comments to the authors' rebuttal.

The authors' rebuttal consists of five paragraphs, a final declaration, and signatures. MvL has not signed the rebuttal. Below follows my rebuttal to the authors' rebuttal (**RR**).

First paragraph: "*It is unfortunate that Dr Schoenecker has decided to complain to you about his concerns rather than approach us directly. We suggest that he submits a manuscript to Applied Animal Behaviour Science (AABS) to raise his concerns, which is the normal process for such matters to debate scientific thought/approaches. Our papers (2008, 2010) consider different questions and were peer-reviewed independently and accepted for publication in AABS*".

RR: I have all the time considered it an exercise in futility to approach the authors directly given the seriousness of these allegations. Also, I do not consider allegations of various scientific dishonesties to be suitable subjects for a "Letter to Editor".

It is only necessary to ask two simple questions to test Paper B's alleged covert duplication of a main and pioneering result in Paper C: It is possible to read from the "Result" section of both papers B and C that stereotyping striped mice are more fertile than non-stereotyping dittos? Does Paper B from 2010 refer to Paper C from 2008 in relation to this result? The answer is evidently "Yes" and "No", respectively. Paper B consequently includes a covert duplication of a pioneering result published in Paper C and it is irrelevant for the matter at hand that Paper C *also* happens to include results outside the scope of Paper B.

Regarding the independency of this particular peer-review process: Dr. Per Jensen has been the sole Editor-in-Chief of AABS since 1997 (online CV) and must have approved both papers for publication, just as he personally has approved all my AABS publications in the period 2000-09. GM served as a member of Dr. Jensen's editorial board at AABS in the period 1997-2000 (online CV) and NP is presently a member of said board (AABS' website). I believe the correct term is: "a potential conflict of interests".

Second paragraph: "*We strongly oppose all allegations that we plagiarised his work, deliberately or mistakenly or that we misrepresented ourselves and/or twisted his words. In fact, we cite his work regularly in our publications, including the ones with which he takes umbrage, and moreover referenced his 2009 work in the sentence preceding the set of 2010 hypotheses that he now disputes*".

RR: The question is not whether it is possible to plagiarize somebody if you cite their works on a routinely basis because any student can do that. The core of the matter is "context" and the right question to ask from my point of view is: How do the authors use Paper A as reference in their Paper B?

1st reference to Paper A: "*in caged bank voles, stereotypic behaviour has recently been shown to be associated with better survival, fecundity, and therefore increased lifetime reproductive success (Schonecker, 2009). The mechanisms for such beneficial effects of stereotypic behaviour are unclear. It could be (1) that*…" and then follows the three hypotheses.

Context: As reference to a prior and most relevant finding in another rodent specie.

2nd reference to Paper A: "*Using Rhabdomys as a model, we assessed the relative reproductive success of stereotypic and non-stereotypic individuals. We set up four treatment groups, formed from combinations of non-stereotypic and stereotypic mothers and fathers. This design makes our study unique since we could assess the relative contribution of maternal and paternal stereotypic behaviours to reproductive output (cf. the recent work in bank voles examining only the maternal contribution; Schonecker, 2009), and thus test the prediction that both male and female stereotypic striped mice would reproduce more successfully than non-stereotypic mice".*

Context: To promote the "uniqueness" of their 2x2 prospective experimental design on my (retrospective) Paper A's expense. The authors do not inform the readers that Paper C used the same simple design and also "*assessed the relative reproductive success of stereotypic and non-stereotypic individuals*".

3rd and 4th (or "and 5th" depending on how you count) reference to Paper A: "*The recent retrospective analysis of Schonecker (2009) showed that stereotypic bank vole mothers, similar to striped mice, have reduced latencies between pairing and the birth of their first litter but, in contrast to Rhabdomys, Schonecker (2009) found no difference in litter size or number of weanlings between stereotypic and nonstereotypic dams. Moreover, the study showed that the pups from stereotypic females sometimes experience higher pre-weaning pup mortality - replicating an effect observed in previous work on bank voles (Ödberg, 1987; Sørensen and Randrup, 1986)*".

Context: Although Paper A fails to show two of the three associations disclosed by the authors it did after all get one thing right and even managed to "*replicate*" a rather un-surprising finding made over two decades ago. No reason to pay too much attention to the differences in experimental designs and animal species…

Paper B contains no more references to Paper A, and as mentioned in the previous Appendix 1, the authors underscores their intellectual ownership of all three hypothesis in the first paragraph on p. 68 of Paper B.

I also find this paragraph confusing because given the authors oppose all allegations, it follows that they maintain that they conceived their hypothesis all by themselves. I therefore fail to see why it should matter to the authors where, exactly, in the manuscript they happen to refer to my paper, and how they can use such an irrelevant detail as an argument. From their point of view, that is.

Third paragraph: "*To quickly address his main complaint that we plagiarised his 2009 hypothesis in our 2010 paper. Upfront, in the Introduction, we gave three possible reasons for why stereotypic behaviour has beneficial effects. Dr Schoenecker is concerned with our second and third hypotheses, and believes that each is a paraphrase of his hypothesis that stereotypy and fecundity genes are closely located and hence transmitted together. He appears to have misinterpreted our hypotheses, as (2) suggests pleiotropic effects and (3) genetic or epigenetic effects and thus not genetic linkage per se (his so-called novel hypothesis). He doesn't, however, seem to be arguing that we omitted to include his hypothesis but instead that we included it and failed to acknowledge his ownership of the idea*".

RR: The meaning of words "*pleiotropic*" and "*epigenetic*" are easily found using Google and I believe Appendix 1 indicates that I actually do understand the meaning of the authors three hypotheses.

I will try to make it clear this way:

- I know of no published attempt to formulate a genetically based hypothesis for any association between emergence of stereotypies and effects on fertility before Paper A was published in autumn 2009. Hence the novelty of my hypothesis which the authors question by using the phrase "*his so-called novel hypothesis*". The authors must either accept its novelty or present proof to the contrary.
- Paper C from 2008 was, just as Paper B, build entirely upon MvL's 2005 MSc thesis but do not include anything remotely similar to my hypothesis.
- I guess/estimate that MvL must have made his experiments around 2004/5 and his thesis do not include anything remotely similar to my hypothesis.
- GM and MAJ were both acknowledged in MvL's thesis and NP was his supervisor (see Appendix 3). I suppose the authors did not manage to come up with a hypothesis like mine in the four years between MvL's experiments and the publication of Paper C because if so, I can only assume they would also have included it in Paper C (and also discussed the result).
- I therefore assume the authors makes the case that they created the two disputed hypotheses at some point in time after Paper C was published in 2008 and before one of them could read Paper A.

If that should be the case I must consider the odds against the notion that not one of the authors should have noticed a "competing" hypothesis in Paper A to be astronomical. Competent researchers and prolific authors such as GM and NP would surely have noticed.

In summary, the authors should of course have included my hypothesis with proper reference to paper A and then they could have presented their contributions. Instead they pretended they developed all hypotheses without the slightest inspiration from Paper A and erased Paper C's pioneering contribution to their key-scope from history. Also, describing the arguments in Appendix 1 is not the same as rebutting its contents.

Fourth paragraph: "*In our Discussion, we again were not necessarily advocating for his hypothesis or speaking to it directly; we were saying that one explanation for increased reproductive success in stereotypic striped mice could be genes but in combination with something else (as moms have more input than dads) which actually argues against two closely located genes for fitness and SB separately, as he advocates, and instead for the pleiotropic or epigenetic effects that we suggested in our original hypotheses (2) and (3). Thus we mentioned that there might be one gene that for example makes animals more active and thereby leads to both stereotypic behaviour and increased fitness - hence a pleiotropic effect*".

RR: I am not disputing that the authors have formulated all three hypotheses in Paper B. I simply maintain that they have used my hypothesis as inspiration and basis for further treatment resulting in two of these three hypotheses, and done so without proper acknowledgement to the original source. The authors disagree.

Bottom-line is that plagiarism might sometimes be verbatim copy-paste exercises without proper credit to the original intellectual creator, and other times plagiarism is the acquisition of someone else's idea to be polished up a bit, perhaps with some added details, and then served as one's own intellectual creation. For the sake of arguments I created three "new" hypotheses based on inspiration from Würbel & Stauffacher, 1997, and Paper E, in Appendix 5: It took me longer time to write the first down that it took me to construct them all.

It does not matter at all how much explanatory power a plagiarized original hypothesis turns out to possess so it is quite irrelevant for this allegation whether Paper B eventually concludes in favour of one, or the other, or none, of the proposed hypotheses. In that sense, plagiating a scientific hypothesis is no different from plagiating e.g. the design of an expensive, good-looking, but also particularly uncomfortable armchair by Hans J. Wegner.

Final paragraphs:

"*In summary, we believe that Dr Schoenecker is using a straw man argument and appears to be trying to cast doubt on our character and scientific integrity by splitting hairs over our phrasing of hypotheses.*

We do not want to enter into third-party debates on this issue.

RR: I seems to me that the authors rebuttal, besides expressing adamant denials of all allegations, also seeks to present me as a person of questionable decency, limited cognitive powers, poorly developed sense of fair play and perhaps even suffering from an irrational compulsion to smear the authors' reputations.

I believe this type of straw man argument is considered a classic. Instead of focussing on the substance of these allegations the question raised is "would it be wise to take the words of such a person over the words of tenured professors?" I think not, and down goes the straw man. Incidentally, I note that the authors, for all practical purposes, do not address the 2nd allegation in their rebuttal (covert duplicity).

To conclude, I personally just want proper *Errata* to be published in relation to Paper B, -D, and -E and, as mentioned in the beginning of this RR, I see no point in having a direct debate with the authors.

Insert

This is my original download of a posting by GM on her lab's blog.

Animal Behaviour & Welfare in the Mason Lab

Plagiarism charge!

7 **04** 2014

Prompted to post this by an email Joe (https://med.stanford.edu/profiles/compmed/joseph-garner) has just had (http://masonabwlab.wordpress.com/2014/04/07/sex-ratio-madness/). In the old days such things would have arrived in the post, stamps all wonky on the envelope, written in green felt-tipped capital letters on tissue paper. This one focuses on Megan Jones' (http://masonabwlab.wordpress.com/2012/06/28/megan-jones-gets-her-phd/) work, so my colleague Neville Pillay (http://www.wits.ac.za/academic/science/apes/staff/academicstaff/p received this (though one's apparently been sent to UoG too).

The charge (all 5 pages of it – below is just a snippet) is that we plagiarised a hypothesis, but covered our tracks with such devilish care and cunning that the plagiarism is hard to spot. Hmm, or …. maybe it's just not there at all??

The third hypothesis states "*being of a fecund genotype predisposes animals to stereotypic behaviour*". The rest of the sentence is insignificant ("*through genetic or epigenetic means*") and the main purpose with the reference to Priestnall's paper could be to further the impression that proper reference has been given. Incidentally, Priestnall's paper does not mention stereotypies at all.

At this point I would like to emphasize that my hypothesis can be expressed as: "Stereotypy-genes" are linked with "fecundity-genes". I in fact used the words "*situated near*" which covers "besides". That means an alternative version could be: "A stereotypic genotype predisposes animals to relatively increased fertility due to close linkage between stereotypy-genes and fecundity-genes", without loosing or adding information.

Switching the order of these genes would result in this formulation: "A fecund genotype predisposes animals to stereotypic behaviour due to close linkage between "fecundity-genes" and "stereotypy-genes"", again without loosing or adding information.

Bottom-line is that the authors basically hypothesize that "Fecundity-genes increases both fertility and proneness to develop captivity-induced stereotypies". They might as well have hypothesized that "Stereotypy-genes increases proneness to develop stereotypies and predisposes the carriers for increased fertility". If linkage is strong between fecundity- and stereotypy-genes, my original hypothesis will be reduced to the authors' version, because such genes will in reality most often be transferred to the offspring as a unit.

Alternatively, the authors could present the hypothesis that "Fecundity-genes are linked with stereotypy-genes", due to the word "*predisposes*". In this case they only need to re-arrange my associative hypothesis 180 degrees to arrive at their hypothesis.

(http://masonabwlab.files.wordpress.com/2014/04/screen-shot-2014-04-06-at-9-01-09-pm.png)

Appendix 7-1.

MAJ' introduction for her PhD dissertation plagiates an article in Wikipedia.

I became aware of MAJ' PhD dissertation after stumbling upon the following announcement, uploaded by GM on her groups webpage at Wordpress.com (announcement dated the 16th of May 2012) [13]:

"Megan Jones' PhD thesis is approved! (And with flying colours)

Megan Jones, the PhD student I co-supervise in South Africa with Neville Pillay (http://www.wits.ac.za/academic/science/apes/staff/academicstaff/pillay/7019/profnevillepillay.html) had her thesis approved today! It used the African striped mouse, Rhabdomys, as a model for understanding the developmental risk factors for stereotypic behaviour. One of the three examiners described it as "wonderful" and "an examplary piece of work", while one of the others judged that "the thesis falls within the top 5% of PhD theses that I have examined or supervised". Very nice: well done Megan!!!!!".

This is MAJ' PhD dissertation:

- Jones, M. A. (2012). *The causes and consequences of stereotypic behaviour in the striped mouse, Rhabdomys*. PhD dissertation. University of the Witwatersrand, South Africa. Available from this URL: http://wiredspace.wits.ac.za/handle/10539/11827

After reading MAJ' brief introduction to her study subjects (striped mice; genus *Rhabdomys*) I decided to see if Google might provide me with some more information regarding these rodents. The first link to appear after I had typed in "Rhabdomys" was this one to an article in Wikipedia:

http://en.wikipedia.org/wiki/Rhabdomys

The present version of this article informs the reader of various aspects of striped mice. However, using the "View history" button in the upper right-hand corner reveals that a user named "Madmegs" created the original article on The 11th of October 2006. It is possible to both read the original text and see how it differs from the present version by pressing "Cur". This is the URL to the original by "Madmegs":

http://en.wikipedia.org/w/index.php?title=Rhabdomys&diff=601806902&oldid=80871829

Appendix 7-2 shows the relevant excerpt from MAJ' dissertation, the original entry from Wikipedia, and finally two versions of a hybrid text where text from Wikipedia's original article has been underscored and fitted upon the excerpt from MAJ' dissertation. The first hybrid version is conservative allowing for no paraphrases whereas the second hybrid version is more in line with common sense, treating insignificant additions and paraphrases as written by the original author "Madmegs".

The underscored sections evidently dominate both hybrid texts and constitute about 75-80% of the lyrics. MAJ appears to simply have copied the original, rearranged some sections, paraphrased some words, deleted some older references, added some new references, and finally added a couple of sentences here and there.

Conclusion: MAJ's dissertation plagiates a publicly available article from Wikipedia ("*Rhabdomys*").

Personal opinion: I am inclined to believe that MAJ wrote this original entry using the pseudonym "Madmegs". If MAJ can provide sufficient proof of this alternative, it follows she has included a covert duplicate publication in her dissertation and not plagiated another author.

Appendix 7-2

Documentation for MAJ's plagiate of the original Wikipedia article "Rhabdomys".

The following excerpt is from the introduction on pp. 10-12 of MAJ's 2012 PhD dissertation.

START EXCERPT:

"***STUDY SPECIES***

Population biology and socio-ecology

The striped mouse, genus Rhabdomys, is a small (40-50g) muroid rodent that is widespread and abundant in the southern African subregion (De Graaff 1981; Willan & Meester 1989; Pillay 2000a, b, c; Skinner & Chimimba 2005; Schradin & Pillay 2005; Ganem at al. in press). It has a brown pelage with a lighter underbelly, and is characterized by four dark stripes on its dorsal surface running from head to tail (Skinner & Chimimba 2005). There is some regional (and potentially species and subspecies; see below) variation in morphology. Striped mice from the southwestern regions of southern Africa are slightly larger than those from the more northern regions, and those from the xeric western areas have a paler coat and longer tails than do striped mice from the mesic eastern regions, which also have a more yellow-brown colouration (Pillay 2000a, c). There is no distinct sexual dimorphism in the genus (Skinner & Chimimba 2005).

Unlike most rodents, the striped mouse exhibits a diurnal, bimodal activity pattern, with activity concentrated around mornings and evenings, and reduced during the midday period (Schumann et al. 2005; MacKay 2011). Its omnivorous diet, ability to survive without water provided its food has a minimum water content of 15% (Willan 1982), and extreme plasticity in habitat preference are likely reasons for its wide (if discontinuous; Brooks 1982) distribution throughout southern Africa (Rambau et al. 2003; Ganem at al. in press).

Striped mice are seasonal breeders, and are reproductively active from spring to autumn in moist eastern grasslands (Willan & Meester 1989) and in spring in arid western regions (Schradin & Pillay 2003) of South Africa. After a gestation period of 22-23 days, free-living females give birth to approximately five pups; captive females have slightly larger litters (e.g. 7.2 ±1.8; Pillay 2000). Pups begin to consume solid food at 10 days, leave the nest from 12 days, and weaning occurs around 16 days of age (Brooks 1982). Sexual maturity is reached between approximately five to six weeks (range 34-90 days; Brooks 1982). Females have an inter-litter interval of approximately 23-30 days (Pillay 2000).

Striped mice in the arid western regions of southern Africa have a flexible social organisation and mating system that appears to be shaped primarily by population density, and secondarily by resource (particularly food and cover) availability and thermoregulatory requirements (Scantlebury et al. 2006; Schradin et al. 2011). In arid habitats (Succulent karoo; Schradin & Pillay 2005a, Schradin et al. 2011), striped mice can be described as territorial, group-living, solitary foragers that display biparental care (Schradin & Pillay 2004). In mesic grassland habitats (e.g. Kwa-Zulu Natal Midlands; Wirminghaus & Perrin 1993; Pretoria Highveld; Brooks 1974; Zimbabwe; Choate 1972) and semi-succulent thorny scrub (e.g. Eastern Cape; Perrin 1980a, b) striped mice are solitary, with females rearing their litters on their own. Both sexes maintain territories that overlap the territories of the opposite, but not the same, sex (Schradin & Pillay 2005). However, males from both mesic and xeric populations display parental care in captivity (Schradin & Pillay 2005), suggesting a plesiomorphic occurrence in the mesic populations, since the desert-living form represents the putative ancestral form (Rambau et al. 2003).

Taxonomy and phylogeny

Striped mice have two different karyotic forms (2n = 28 and 2n = 46; Taylor 2000). Based on this finding and on the analysis of mitochondrial DNA (Rambau et al. 2003), as well as evidence of divergent behavioural repertoires among populations (e.g. courtship behaviours; Pillay 2000b; Pillay et al. 2006), Rambau and colleagues (2003) suggested that Rhabdomys, previously considered a monospecific genus containing the single species R. pumilio, be reclassified as two species: R. pumilio (the social form that occurs in xeric habitats; 2n = 48), and R. dilectus (the solitary form, found in mesic areas, that comprises two subspecies R. d. dilectus, 2n = 46, and R. d. chakae, 2n = 48); this taxonomic grouping has been corroborated by studies of the mate recognition system of the taxon (Pillay et al. 2006) as well as by their different environmental niches (Ganem at al. in press)."

END EXCERPT.

This is "Madmegs" original article on *Rhabdomys* (all underscored).

START ARTICLE:

"*Rhabdomys is a small (40-50g) muroid rodent that is widespread and abundant in the southern African subregion (De Graaff, 1981; Willan & Meester, 1989; Pillay, 2000a, b, c; Skinner & Chimimba, 2005; Schradin & Pillay, 2005). Unlike most rodents, the Striped mouse exhibits a diurnal, bimodal activity pattern, with activity concentrated around mornings and evenings, and reduced during the midday period (Perrin, 1981; Schumann et al., 2005). Its omnivorous diet, its ability to survive without water provided its food has a minimum water content of 15% (Willan, 1982), and its extreme plasticity in habitat preference are likely reasons for its wide (if discontinuous; Brooks, 1982) distribution throughout southern Africa (Rambau et al., 2003).*

Rhabdomys is a seasonal breeder and reproductively active from spring to autumn (Willan & Meester, 1989; Schradin & Pillay, 2003). After a gestation period of 22-23 days, free-living females give birth to approximately five pups; captive females have slightly larger litters (e.g. 7.2 ±1.8; Pillay, 2000). Pups begin to consume solid food at ten days, leave the nest from twelve days, and weaning occurs at around 16 days. Sexual maturity is reached at approximately five to six weeks (range 34 – 90 days; Brooks, 1982). Timing of sexual maturity, as well as dispersal age, depends on environmental factors (e.g. resource availability), social cues (e.g. the presence of older, reproductively active animals), as well as the animal's developmental history (e.g. weight at weaning; sex-ratio of litter; Mason & Latham, 2004). Females have an inter-litter interval of approximately 23-30 days (Pillay, 2000).

Rhabdomys has a flexible social organisation and mating system that appears to be shaped primarily by resource (particularly food and cover) availability and, secondarily, by population density. In arid habitats (e.g. Namib; Krug, 2002; Kalahari; Nel, 1975; Nel & Rautenbach, 1975; Succulent karoo; Schradin & Pillay, 2005a, b) Rhabdomys can be described as a territorial, group-living, solitary forager that displays biparental care (Schradin & Pillay, 2004). In mesic, grassland habitats (e.g. Kwa-Zulu Natal Midlands; Wirminghaus & Perrin, 1993; Pretoria Highveld; Brooks, 1974; Zimbabwe grassland; Choate, 1972) and semi-succulent thorny scrub (e.g. Eastern Cape; Perrin, 1980a, b) animals are solitary, with females rearing their litters on their own, and both sexes maintain territories that overlap the territories of the opposite, but not the same, sex (Schradin & Pillay, 2005a). However, males from both mesic and xeric populations display parental care in captivity (Schradin & Pillay, 2005b), suggesting a plesiomorphic occurrence in the mesic populations, since the desert-living form represents the ancestral form (Rambau et al., 2003).

There is some regional variation in morphology. Striped mice from the southwestern regions of southern Africa are slightly larger than animals from the more northern regions, and animals from the xeric western areas have a paler coat than do mice from the mesic, eastern regions (Pillay, 2000a, c). In addition, there appear to be population level differences in personality – an animal's characteristic and consistent style of behaviour (Reif & Lesch, 2003) – and stress-sensitivity (personal observations; suggested by Reuther, 2000), although these differences have yet to be investigated empirically.

Two karyotypic forms of Rhabdomys (2n = 28 and 2n = 46) have been detected. Based on this finding and on the analysis of mitochondrial DNA, as well as evidence of divergent behavioural repertoires among populations (e.g. courtship behaviours; Pillay, 2000b; Pillay et al., 2006). Rambau et al. (2003) suggest that R. pumilio be reclassified as two species: R. pumilio (the social form that occurs in xeric habitats; 2n = 48) and R. dilectus (the solitary form, found in mesic areas, that comprises two subspecies R. d. dilectus, 2n = 46, and R. d. chakae, 2n = 48)."

END ARTICLE

Hybrid version 1: Literary copy-paste.

This is a hybrid version where "Madmegs" original (underscored) is fitted onto the excerpt from MAJ' dissertation. In between I have added a single sentence (in bold) to point to an odd difference between the dissertation and Wikipedia's article (I have not bothered to find out if which alternative is true).

"***STUDY SPECIES***

Population biology and socio-ecology

The striped mouse, genus Rhabdomys, is a small (40-50g) muroid rodent that is widespread and abundant in the southern African subregion (De Graaff 1981; Willan & Meester 1989; Pillay 2000a, b, c; Skinner & Chimimba 2005; Schradin & Pillay 2005; Ganem at al. in press). It has a brown pelage with a lighter underbelly, and is characterized by four dark stripes on its dorsal surface running from head to tail (Skinner & Chimimba 2005). There is some regional (and potentially species and subspecies; see below) variation in morphology. Striped mice from the southwestern regions of southern Africa are slightly larger than those from the more northern regions, and those from the xeric western areas have a paler coat and longer tails than do striped mice from the mesic eastern regions, which also have a more yellow-brown colouration (Pillay 2000a, c). There is no distinct sexual dimorphism in the genus (Skinner & Chimimba 2005).

Unlike most rodents, the striped mouse exhibits a diurnal, bimodal activity pattern, with activity concentrated around mornings and evenings, and reduced during the midday period (Schumann et al. 2005; MacKay 2011). Its omnivorous diet, ability to survive without water provided its food has a minimum water content of 15% (Willan 1982), and extreme plasticity in habitat preference are likely reasons for its wide (if discontinuous; Brooks 1982) distribution throughout southern Africa (Rambau et al. 2003; Ganem at al. in press).

Striped mice are seasonal breeders, and are reproductively active from spring to autumn in moist eastern grasslands (Willan & Meester 1989) and in spring in arid western regions (Schradin & Pillay 2003) of South Africa. After a gestation period of 22-23 days, free-living females give birth to approximately five pups; captive females have slightly larger litters (e.g. 7.2 ±1.8; Pillay 2000). Pups begin to consume solid food at 10 days, leave the nest from 12 days, and weaning occurs around 16 days of age (Brooks 1982). Sexual maturity is reached between approximately five to six weeks (range 34-90 days; Brooks 1982). Females have an inter-litter interval of approximately 23-30 days (Pillay 2000).

Striped mice in the arid western regions of southern Africa have a flexible social organisation and mating system that appears to be shaped primarily by population density, and secondarily by resource (particularly food and cover) availability and thermoregulatory requirements (Scantlebury et al. 2006; Schradin et al. 2011).

[Note that MAJ swaps the original primary reason ("Ressource") for the secondary reason ("population density") as compared to both the original and present version]

In arid habitats (Succulent karoo; Schradin & Pillay 2005a, Schradin et al. 2011), striped mice can be described as territorial, group-living, solitary foragers that display biparental care (Schradin & Pillay 2004). In mesic grassland habitats (e.g. Kwa-Zulu Natal Midlands; Wirminghaus & Perrin 1993; Pretoria Highveld; Brooks 1974; Zimbabwe; Choate 1972) and semi-succulent thorny scrub (e.g. Eastern Cape; Perrin 1980a, b) striped mice are solitary, with females rearing their litters on their own. Both sexes maintain territories that overlap the territories of the opposite, but not the same, sex (Schradin & Pillay 2005). However, males from both mesic and xeric populations display parental care in captivity (Schradin & Pillay 2005), suggesting a plesiomorphic occurrence in the mesic populations, since the desert-living form represents the putative ancestral form (Rambau et al. 2003).

Taxonomy and phylogeny

Striped mice have two different karyotic forms (2n = 28 and 2n = 46; Taylor 2000). Based on this finding and on the analysis of mitochondrial DNA (Rambau et al. 2003), as well as evidence of divergent behavioural repertoires among populations (e.g. courtship behaviours; Pillay 2000b; Pillay et al. 2006), Rambau and colleagues (2003) suggested that Rhabdomys, previously considered a monospecific genus containing the single species R. pumilio, be reclassified as two species: R. pumilio (the social form that occurs in xeric habitats; 2n = 48), and R. dilectus (the solitary form, found in mesic areas, that comprises two subspecies R. d. dilectus, 2n = 46, and R. d. chakae, 2n = 48); this taxonomic grouping has been corroborated by studies of the mate recognition system of the taxon (Pillay et al. 2006) as well as by their different environmental niches (Ganem at al. in press)."

Literary plagiarism quotient = 74.4%

(497 words ("Madmegs")/668 words (total, excl. titles and my note)) * 100)

Hybrid version 2: Literary copy-paste and simple paraphrases.

The previous hybrid text includes no paraphrases of "Madmegs" original text. Allowing for simple paraphrasing is more in line with common sense and this is the result:

"*STUDY SPECIES*

Population biology and socio-ecology

The striped mouse, genus Rhabdomys, is a small (40-50g) muroid rodent that is widespread and abundant in the southern African subregion (De Graaff 1981; Willan & Meester 1989; Pillay 2000a, b, c; Skinner & Chimimba 2005; Schradin & Pillay 2005; Ganem at al. in press). It has a brown pelage with a lighter underbelly, and is characterized by four dark stripes on its dorsal surface running from head to tail (Skinner & Chimimba 2005). There is some regional (and potentially species and subspecies; see below) variation in morphology. Striped mice from the southwestern regions of southern Africa are slightly larger than those from the more northern regions, and those from the xeric western areas have a paler coat and longer tails than do striped mice from the mesic eastern regions, which also have a more yellow-brown colouration (Pillay 2000a, c). There is no distinct sexual dimorphism in the genus (Skinner & Chimimba 2005).

Unlike most rodents, the striped mouse exhibits a diurnal, bimodal activity pattern, with activity concentrated around mornings and evenings, and reduced during the midday period (Schumann et al. 2005; MacKay 2011). Its omnivorous diet, ability to survive without water provided its food has a minimum water content of 15% (Willan 1982), and extreme plasticity in habitat preference are likely reasons for its wide (if discontinuous; Brooks 1982) distribution throughout southern Africa (Rambau et al. 2003; Ganem at al. in press).

Striped mice are seasonal breeders, and are reproductively active from spring to autumn in moist eastern grasslands (Willan & Meester 1989) and in spring in arid western regions (Schradin & Pillay 2003) of South Africa. After a gestation period of 22-23 days, free-living females give birth to approximately five pups; captive females have slightly larger litters (e.g. 7.2 ±1.8; Pillay 2000). Pups begin to consume solid food at 10 days, leave the nest from 12 days, and weaning occurs around 16 days of age (Brooks 1982). Sexual maturity is reached between approximately five to six weeks (range 34-90 days; Brooks 1982). Females have an inter-litter interval of approximately 23-30 days (Pillay 2000).

Striped mice in the arid western regions of southern Africa have a flexible social organisation and mating system that appears to be shaped primarily by population density, and secondarily by resource (particularly food and cover) availability and thermoregulatory requirements (Scantlebury et al. 2006; Schradin et al. 2011). In arid habitats (Succulent karoo; Schradin & Pillay 2005a, Schradin et al. 2011), striped mice can be described as territorial, group-living, solitary foragers that display biparental care (Schradin & Pillay 2004). In mesic grassland habitats (e.g. Kwa-Zulu Natal Midlands; Wirminghaus & Perrin 1993; Pretoria Highveld; Brooks 1974; Zimbabwe; Choate 1972) and semi-succulent thorny scrub (e.g. Eastern Cape; Perrin 1980a, b) striped mice are solitary, with females rearing their litters on their own. Both sexes maintain territories that overlap the territories of the opposite, but not the same, sex (Schradin & Pillay 2005). However, males from both mesic and xeric populations display parental care in captivity (Schradin & Pillay 2005), suggesting a plesiomorphic occurrence in the mesic populations, since the desert-living form represents the putative ancestral form (Rambau et al. 2003).

Taxonomy and phylogeny

Striped mice have two different karyotic forms (2n = 28 and 2n = 46; Taylor 2000). Based on this finding and on the analysis of mitochondrial DNA (Rambau et al. 2003), as well as evidence of divergent behavioural repertoires among populations (e.g. courtship behaviours; Pillay 2000b; Pillay et al. 2006), Rambau and colleagues (2003) suggested that Rhabdomys, previously considered a monospecific genus containing the single species R. pumilio, be reclassified as two species: R. pumilio (the social form that occurs in xeric habitats; 2n = 48), and R. dilectus (the solitary form, found in mesic areas, that comprises two subspecies R. d. dilectus, 2n = 46, and R. d. chakae, 2n = 48); this taxonomic grouping has been corroborated by studies of the mate recognition system of the taxon (Pillay et al. 2006) as well as by their different environmental niches (Ganem at al. in press)."

Plagiarism quotient when allowing for simple paraphrases: 79.8%

(533 words ("Madmegs")/668 words (total, excl. titles and my note)) * 100)

Appendix 8.

Blatant misrepresentation of merits in MAJ's dissertation.

While downloading MAJ's dissertation I also took the opportunity to download MAJ's response to the initial evaluation by the examiners:

- Megan Jones Thesis Corrections.docx (http://wiredspace.wits.ac.za/handle/10539/11827)

"Examiner 1" had no comments at all but "Examiner 3" requested that MAJ should clarify her "*contribution towards the study concept and design*" in her revision of the dissertation.

The response was this note on page 13 in MAJ's dissertation:

" *Note: In the multi-author work already published, I was the primary designer and executor of the experiments and was responsible for the data analysis, the initial write-up, and the preparation of the manuscripts for publication. My supervisors provided standard supervisory guidance throughout.*"

The clarifying note is consistent with the following excerpts from MAJ's dissertation (p. 111-112):

"*This study had three broad objectives. First, building on previous work in striped mice and in other species (see General Introduction), I investigated the genetic transmission of SBs from both the mother and the father, and also the relative reproductive success of non-stereotypic and stereotypic individuals. The study design used in these experiments was unique and differed from those in previous studies in striped mice and other species: I set up four treatment groups, formed from combinations of non-stereotypic and stereotypic mothers and fathers, and so could assess the relative contribution of maternal and paternal stereotypic status to offspring stereotypic status and to reproductive output. I showed that SB has a strong genetic component, but that maternally mediated prenatal factors might also predispose the SB phenotype (Jones et al. 2008, Chapter 2; previously it has been shown that there is negligible postnatal social transmission of SB from dams to their pups; Schwaibold & Pillay 2001); that the reproductive output for stereotypic females, but not stereotypic males, is significantly greater than for non-stereotypic striped mice of both sexes (Jones et al. 2010a, Chapter 3); and that there is likely unintended selection for SB in captivity because the phenotype is associated with greater reproductive output underpinned by apparent genetic variance (Jones et al. 2010a, Chapter 3)".*

The clarifying note is also consistent with the way MAJ introduces her "*Motivation for the study*" (p. 10):

"*First, building on previous work in striped mice (Schwaibold & Pillay 2001), I investigated the genetic contribution to individual variation in SB performance in CB striped mice (Chapter 2, Jones et al. 2008), and explored the relationship between SB and reproductive output (Chapter 3, Jones et al. 2010a)*".

Conclusion: MAJ claims to have designed, executed and analyzed data from about half of the experiments presented in MvL's thesis. More specifically, and given a manuscript in a thesis must be considered the "*initial write-up*", MAJ makes the *de facto* claim of having made and written everything in Chapter 2 of said thesis.

Personal opinion: Based on a "*Declaration*" in MvL's thesis (p. ii: "*I declare that this dissertation is my own unaided work*"), and common sense not least, I believe MAJ must be inflating her contribution to Papers B, C, and MvL's thesis, way beyond recognition.

I further believe that the most parsimonious reason for this misrepresentation of merits is to justify MAJ's first-authorships on Papers B and C and thereby making her contributions seems that more impressive. The result of the misconducts described in Appendix 1-4, 7 and 8 was a dissertation so sparkling that the examiners could hardly contain their enthusiasm. To repeat the quote from GM's announcement ([13]):

"*One of the three examiners described it as "wonderful" and "an examplary piece of work", while one of the others judged that "the thesis falls within the top 5% of PhD theses that I have examined or supervised".*

Appendix 9

Conclusions, personal demand, - speculation and - suggestions.

General conclusions:

- MAJ, GM and NP have been engaged in several acts of scientific dishonesties in relation to MAJ's dissertation and derived papers.
- NP and GM have failed their obligation to present a good example as supervisors for MAJ.
- MAJ has evidently been awarded her PhD on false pretences.

Specific conclusions:

- The idea behind my hypothesis has been deliberately plagiated by MAJ, MvL, NP and GM in Paper B, which also happen to be a hybrid between a "covert redundant publication" and a "salami publication". Paper B presents a pioneering result from Paper C (authored by MAJ, MvL and NP) as a novelty, and adds some minor results that could easily have been included in Paper C.
- Paper B and Paper C are based entirely on the experiment presented as Chapter 2 in MvL's MSc thesis. MvL thesis was supervised by NP and includes acknowledgements to MAJ and GM "*for their assistance and willingness to discuss numerous ideas and theories*". It follows that MAJ, NP and GM cannot claim "plausible deniability" in relation to MvL's rightful first-authorship on these papers; that MAJ misrepresents her merits in relation to these papers by assuming the prestigious position as first-author, and that MAJ apparently did so with NP and GM's full knowledge and acceptance.
- Paper D (authored by MAJ, GM and NP) presents a variation of a pioneering experiment presented in Chapter 3 of MvL's MSc thesis but does not include any reference to MvL or his thesis. Paper D consequently plagiates MvL's idea for his experiment.
- The idea behind my hypothesis has been deliberately plagiated by GM and two co-authors in Paper E that also contains a plagiate of the idea behind an older hypothesis by Würbel & Stauffacher.
- MAJ's 2012 dissertation plagiates a publicly available article from Wikipedia ("*Rhabdomys*") that was originally uploaded to Wikipedia in 2006 by a person using the pseudonym "Madmegs".
- MAJ's dissertation includes this note: "*In the multi-author work already published, I was the primary designer and executor of the experiments and was responsible for the data analysis, the initial write-up, and the preparation of the manuscripts for publication*". Translation: MAJ claims to have designed, executed and analyzed data from about half of the experiments presented in MvL's MSc thesis. More specifically those presented in Chapter 2 of said thesis, which MAJ also must claim to have written personally. Since MvL's thesis includes the declaration that "*this dissertation is my own unaided work*", it follows that someone has been inflating his/hers contributions to said thesis beyond recognition. I believe that someone to be MAJ.

Personal demand: Paper B, D and E must be supplemented with *Errata* as already specified. Since MAJ, NP, and GM have allready demonstrated a most regrettable lack of intellectual honesty, I believe Wits has a clear obligation to make sure that such *Errata* are eventually published.

Personal speculation: The preceding appendix' describes an approach to ethics in publishing which is at least consistent. I believe the purpose of these misconducts has been to make MAJ's dissertation appear as impressive as possible. However, I find it hard to believe that *any* student would even consider to include such examples of academic and scientific dishonesty in a thesis or dissertation without prior accept from the supervisor(s). In short, I consider this a most relevant question: Have GM and NP in fact been *encouraging* MAJ to venture into the realm of academic and scientific dishonesty right from the start?

Personal suggestion: I suggest that the three examiners at MAJ's defense are informed and that Wits makes sure that all Head of Schools and all Deans know how to treat allegations of misconducts.

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