

Reviews

Reviewed by anonymous reviewer, 2018-05-30 11:53

Globally, I found the manuscript interesting and I think it focuses on an important question concerning the discussion about the social organization and home range use in wild mandrills. Moreover, this manuscript is well written and the readers are well guided throughout the discussion.

First at all, authors published in 2015 an article concerning new data on group composition and patterns of male migration in wild mandrills and complemented this description of social organization with data on ranging behavior and home range use (i.e. Brockmeyer et al. 2015). Following this article, another authors published, in the same journal (i.e. ‘American Journal of Primatology’), a reply calling results, and notably interpretations, into question (Bicca-Marques and Calegario-Marques, 2016). Today, as the title indicates, the present article represents a reply to remarks realized, in second time, by Marques and Calegario-Marques (2016). Indeed, authors resent the impression that the incomplete and biased depictions created confusion within original discussion as, according to them, Bicca-Marques and Calegario-Marques (2016) created a heavily distorted point of departure from their article. In this present article, authors argue their initial results and the associated point of view throughout a discussion organized mainly in four paragraphs corresponding to assumptions debated by Bicca-Marques and Calegario-Marques (2016). I found this article interesting but I’m not specialist and, with a lack of bibliographie on the subject, I doubt the depth of my criticisms considering the subtlety of the discussion. However, even if I comprehend authors’ willpower to defend their original article and associated results, I think also that the intrinsic discussion of this present article is quite aggressive.

We have revised our style thoroughly to remove any kind of aggressive remarks. In particular, we have shortened the first section and removed the final paragraph about scientific discourse.

Besides, authors do not hesitate (i) to think that assumptions of Bicca-Marques and Calegario-Marques (2016) are incomplete (e.g. lack of bibliography and arguments) and (ii) to employ the term ‘omission’ as the title for the first part of this present article. Moreover, authors end their article with a general comment underlining the fact that they did have not the opportunity to reply to Bicca-Marques and Calegario-Marques whose comments hurt original interpretations and took out the original article of its context.

In conclusion, I find this reply article brings lights on the discussion about the social organization and home range use in wild mandrills but I think that this article would has more its place in the ‘American Journal of Primatology’ which have accepted the two first articles. Although this is the first time that I review an article for the PCI ecology, I am not sure that this kind of debate (reply) represents the main objective of the revue.

We agree that publishing our reply to American Journal of Primatology is more suitable. This is the reason why we did submit this reply in the first instance to American Journal of Primatology who declined our response. We are copying the editor’s reply here (in italics):

“Thank you for your recent submission, “A Reply to ‘Ranging Behavior Drives Parasite Richness: a More Parsimonious Hypothesis’”. [...] I think it is important for you to know that AJP does not solicit responses to our papers. The paper by Bicca-Marques and Calegario-Marques was submitted independently. In addition, AJP does not publish responses, even unsolicited, as such; all submissions must contribute new ideas, data, or information. Our “commentary” category is peer-reviewed and the articles must stand

alone and not merely be responses to other articles. The Bicca-Marques paper went for peer review, went through revision, was judged to contribute independently to the literature, and was ultimately accepted.

Given these policies, we cannot accept a commentary that is primarily a direct response to the Bicca-Marques paper, without adding new information. However, if you would care to write a review article on the assumptions underlying primate parasite studies and under what conditions these assumptions are likely to be appropriate for species with certain characteristics and inappropriate for species with other characteristics (i.e. day range, group size, diet, home range area, frequency of grooming, etc.), that would be the kind of paper that we would judge as an independent contribution to the literature and be happy to send out for review. In fact, I hope that you would be interested in writing something like that. [...]"

As you can see, the alternative option that AJP proposed us was to write a review (which was definitively beyond the scope of our initial purpose).

Reviewed by anonymous reviewer, 2018-05-28 04:39

The text is a rebuttal to a critique of an earlier paper. As such, I don't believe there is need for a strong review--the response length is presumably largely at the discretion of the editor; otherwise, the authors should be allowed to rebut as length permits. That being said, I largely agree with the authors' points. However, while they are clearly laid out in the abstract, elsewhere they become somewhat more emotional. For instance, the entire 'omission' section weakens the paper and could be reduced to 2 or 3 sentences. I couldn't really care less about the authors resentments--I just want to know why their points are valid or not.

We have now shortened the entire 'Omission' section as suggested and have also removed sections' titles. As such, the section initially called 'Parsimony' now constitutes the core of the reply.

Similarly, 'the general comment' is unnecessary. Decent protocol is to send a critique to the authors, regardless of the size of the field, and I think most readers are aware of this. A minor aside saying that the critique was not given to the authors would seem to be enough.

We agree that the scientific discourse should be transparent, but that was definitively not how our reply was handled (see above). That being said, we have removed this final paragraph entirely, as suggested.

Assumption 1--there is some debate here about whether the parasites are pathogenic or not. Why does it matter? Animals will move to avoid pests whether or not they are pathogenic. Caribou (and people) will move considerable distances to avoid mosquitoes even if they have no pathogenic effect; similarly, I will avoid areas of ticks (even those I know are not pathogenic). It seems awfully narrow-minded to focus only on the pathogenicity of the parasites.

We have provided an extended discussion of this first assumption because the supposedly non-pathogenicity of the studied parasites is always seriously criticized. This is the reason why we chose to keep this entire paragraph as we think this is a crucial point deserving extended clarification.

While we agree with this reviewer's general assessment, we would like to provide a more nuanced response. Animals will, indeed, move to avoid *any kind* of pests if avoidance

mechanisms that have evolved are not *specific* to some parasites. For example, avoiding all types of feces, whatever their level of parasitism or their level of contagion, is probably a parsimonious general mechanism allowing several types of pathogens (bacteria, viruses, protozoa...) to be avoided simultaneously.

I believe the authors would be better suited to provide a short rebuttal focusing on their main points.

We have followed this general comment by shortening our reply, focusing on concise scientific elements.