Peer Community In Ecology

Does elevated parasite richness in the environment affect daily path length of animals or is it the converse? An answer bringing some new elements of discussion

Cédric Sueur *based on peer reviews by 2 anonymous reviewers*

Charpentier MJE, Kappeler PM (2018) A reply to "Ranging Behavior Drives Parasite Richness: A More Parsimonious Hypothesis". arXiv, ver. 3, peer-reviewed and recommended by Peer Community in Ecology. https://doi.org/10.48550/arXiv.1805.08151

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In 2015, Brockmeyer et al. [1] suggested that mandrills (*Mandrillus sphinx*) may accept additional ranging costs to avoid heavily parasitized areas. Following this paper, Bicca-Marques and Calegaro-Marques [2] questioned this interpretation and presented other hypotheses. To summarize, whilst Brockmeyer et al. [1] proposed that elevated daily path length may be a consequence of elevated parasite richness, Bicca-Marques and Calegaro-Marques [2] viewed it as a cause. In this current paper, Charpentier and Kappeler [3] respond to some of the criticisms by Bicca-Marques and Calegaro-Marques and discuss the putative parsimony of the two competing scenarios. The manuscript is interesting and focuses on an important question concerning the discussion about the social organization and home range use in wild mandrills. This answer helps to move this debate forward and should stimulate more empirical studies of the role of environmentally-transmitted parasites in shaping ranging and movement patterns of wild vertebrates. Given the elements this paper brings to the topics, it should have been published in American Journal of Primatology, the journal that published the two previous articles.

References:

[1] Brockmeyer, T., Kappeler, P. M., Willaume, E., Benoit, L., Mboumba, S., & Charpentier, M. J. E. (2015). Social organization and space use of a wild mandrill (Mandrillus sphinx) group. American Journal of Primatology, 77(10), 1036–1048. doi: [10.1002/ajp.22439](https://dx.doi.org/10.1002/ajp.22439)

[2] Bicca-Marques, J. C., & Calegaro-Marques, C. (2016). Ranging behavior drives parasite richness: A more parsimonious hypothesis. American Journal of Primatology, 78(9), 923–927. doi: [10.1002/ajp.22561](https://dx.doi.org/10.1002/ajp.22561)

[3] Charpentier, M. J., & Kappeler, P. M. (2018). A reply to "Ranging Behavior Drives Parasite Richness: A More Parsimonious Hypothesis." ArXiv:1805.08151v2 [q-Bio]. Retrieved from http://arxiv.org/abs/1805.08151

Reviews

Evaluation round #1

DOI or URL of the preprint: https://arxiv.org/abs/1805.08151v1
Version of the preprint: 1

Authors' reply, 07 June 2018

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Decision by Cédric Sueur, posted 05 June 2018

Revision needed

Dear Authors, The two reviewers and I agreed that your paper is interesting an merits to be published. However, the reviewers raised the points that your answer became at some places quite emotional and aggressive. Could you please revise your paper according to the reviewers comments and I would be pleased to recommend this preprint in PCI Ecology. I stay at your disposal for any question. Best,

Reviewed by anonymous reviewer 1, 30 May 2018

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 Calegaro-Marques, 2016). Today, as the title indicates, the present article represents a reply to remarks realized, in second time, by Marques and Calegaro-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 Calegaro-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 Calegaro-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. Besides, authors do not hesitate (i) to think that assumptions of Bicca-Marques and Calegaro-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 Calegaro-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.

Reviewed by anonymous reviewer 2, 28 May 2018

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.

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.

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.

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