Peer Community In Ecology

The global change of species interactions

***Jan Hrcek** based on peer reviews by 2 anonymous reviewers*

Adèle Mennerat, Anne Charmantier, Sylvie Hurtrez-Boussès, Philippe Perret, Marcel M Lambrechts (2019) Parasite intensity is driven by temperature in a wild bird. Missing preprint_server, ver. Missing article_version, peer-reviewed and recommended by Peer Community in Ecology. https://doi.org/10.1101/323311

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What kinds of studies are most needed to understand the effects of global change on nature? Two deficiencies stand out: lack of long-term studies [1] and lack of data on species interactions [2]. The paper by Mennerat and colleagues [3] is particularly valuable because it addresses both of these shortcomings. The first one is obvious. Our understanding of the impact of climate on biota improves with longer times series of observations. Mennerat et al. [3] analysed an impressive 18-year series from multiple sites to search for trends in parasitism rates across a range of temperatures. The second deficiency (lack of species interaction data) is perhaps not yet fully appreciated, despite studies pointing this out ten years ago [2,4]. The focus is often on species range limits and how taking species interactions into account changes species range predictions based on climate alone (climate envelope models; [5]). But range limits are not everything, as the function of a species (or community, network, etc.) ultimately depends on the strengths of species interactions and not only on the presence or absence of a given species [2,4]. Mennerat et al. [3] show that in the case of birds and their nest parasites, it is the strength of the interaction that has changed, while the species involved stayed the same. Mennerat et al. [3] found nest parasitism to increase with temperature at the nestling stage. They have also searched for trends of parasitism dynamics dependence on the host, but did not find any, probably because the nest parasites are generalists and attack other bird species within the study sites. This study thus draws attention to wider networks of interacting species, and we urgently need more data to predict how interaction networks will rewire with progressing environmental change [6,7].

References:

[1] Lindenmayer, D.B., Likens, G.E., Andersen, A., Bowman, D., Bull, C.M., Burns, E., et al. (2012). Value of long-term ecological studies. Austral Ecology, 37(7), 745–57. doi: [10.1111/j.1442-9993.2011.02351.x](https://dx.doi.org/ 10.1111/j.1442-9993.2011.02351.x)

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[3] Mennerat, A., Charmantier, A., Hurtrez-Bousses, S., Perret, P. & Lambrechts, M.M. (2019). Parasite intensity is driven by temperature in a wild bird. bioRxiv, 323311. Ver. 4 peer-reviewed and recommended by PCI Ecology. doi: [10.1101/323311](https://dx.doi.org/ 10.1101/323311)

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[6] Bartley, T.J., McCann, K.S., Bieg, C., Cazelles, K., Granados, M., Guzzo, M.M., et al. (2019). Food web rewiring in a changing world. Nature Ecology & Evolution, 3(3), 345–54. doi: [10.1038/s41559-018-0772-3](https://dx.doi.org/ 10.1038/s41559-018-0772-3)

[7] Staniczenko, P.P.A., Lewis, O.T., Jones, N.S. & Reed-Tsochas, F. (2010). Structural dynamics and robustness of food webs. Ecology Letters, 13(7), 891–9. doi: [10.1111/j.1461-0248.2010.01485.x](https://dx.doi.org/ 10.1111/j.1461-0248.2010.01485.x)

Reviews

Evaluation round #2

DOI or URL of the preprint: **10.1101/323311**
Version of the preprint: 2

Authors' reply, 25 February 2019

Dear recommender, Thanks for your reply. I have now done the last changes, uploaded the data and code online and formatted the preprint according to the PCI template.

Decision by Jan Hrcek, posted 20 February 2019

a few corrections needed

The authors have revised the manuscript according to the comments of the reviewers and myself which has improved the manuscript and it is now almost ready for recommendation. Before I recommend the manuscript, I would like the authors to correct a typo which I found during my last reading (incorrect use of "however" on line 281) and to publicly deposit the data on which this manuscript is based according to PCI rules.

Evaluation round #1

DOI or URL of the preprint: **10.1101/323311**
Version of the preprint: 1

Authors' reply, 14 February 2019

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Decision by Jan Hrcek, posted 28 September 2018

revision needed

First, I would like to apologize for how long it took me to make the decision – it was difficult to get reviewers in the summer. After considering two reviews and my own reading of the manuscript I ask you to revise the manuscript. Both the reviewers and I think that it would be a valuable contribution if you address the comments. I especially appreciate the temporal scale of the data and the strength of the evidence which comes with it. Currently the manuscript is narrowly focused on temperature effect on the parasites. In addition to reviewer comments, I would like you to explicitly consider aspects of host-parasite dynamics in the manuscript in addition to temperature. Could you test if there is evidence for dependence of the parasite population on performance or population size of the host in previous year, or in some other way relate parasitoid dynamics to host dynamics?

Reviewed by anonymous reviewer 2, 04 August 2018

Summary: This study describes how local temperature affect blowfly abundance in blue tits nesting in a Mediterranean habitat based on a survey over 18 years. Authors convincingly show that environmental temperature is a potent mediator of parasite abundance, both within breeding seasons across years, and between different years.

General comments: I congratulate the authors on a fine contribution that will interest a range of ornithology and ecology scholars. The ms is well-written and clear for the most part, data were appropriately collected and analyzed, and conclusions follow. All in all, I found this neat paper, and have relatively few further recommendations offer. I have made note of some more itemized issues that you may wish to address. I hope you will find these useful.

Minor comments: 1. Line 20: Please italicize species names.

1. Lines 23-24: I think it would be easier to understand this effect should you express it as temperature differentials, e.g. what is a high "previous summer temperature".
2. Lines 26-28: Sure, but blowflies are hardly range restricted as is?
3. Lines 29-32: This should be revised for clarity.
4. Line 54: What is the rate of activity? Movements per hour?
5. Line 87: You need to be more specific at this point – "physiological performance" is both vague and subjective.
6. Line 119: Which other breeding attempts would there be in the net boxes?
7. Line 120: It would suffice to say you visited boxes to determine start of breeding. References to Julian day are superfluous.
8. Lines 123-125: It would be prudent to state dimensions here.
9. Line 130: By "wing plumage color" you mean that you checked for a molt limit between the primary coverts + alula and the greater coverts?
10. Line 133: Because these females could not be caught?

11. Line 166: “Statistical analyses”
12. Lines 196-197: Were temperature indices for the different 3-month periods correlated?
13. Lines 210-215: How influential was the one very warm Fango summer for these results?
14. Lines 223-226: You could downplay this effect I think. It was prudent testing for it, but it is not essential for the Discussion to keep it in. Also, out of interest, if the loss argument holds where would the nest parasites otherwise have disappeared, and why?
15. Line 227: Thermal dependence is an awkward term, please swap for something less ambiguous.
16. Line 232: Why would you expect it to be?s
17. Line 239: I agree insofar that thermal limits to development are as likely in blowflies as in other insects. However, given the vast distributional range of the Calliphoridae, I would be careful with drawing broad conclusions about taxon-wide temperature tolerance.
18. Line 241: Summer heat, or warmer summer temperatures? This is an important distinction.
19. Line 247: “High” is rather subjective a term in this context.
20. Lines 241-250: This reasoning is in analogy with a recent study that manipulated nest temperature in blue tits (Andreasson et al. J Avian Biol 2018) found that nestlings in heated nests had higher body condition and suggested this could have been a result of increased parasite mortality at high environmental temperature.
21. Lines 271-277: This gets a bit repetitive.
22. Lines 281-285: You should have the data to test this?
23. Line 296: There are evidence for similar effects also in homeotherms, which you might consider acknowledging here.
24. Lines 291-307: I am not convinced by this reasoning, as there are already blowflies at latitudes considerably colder than at your study sites. I am not sure how much this adds to the ms.

Reviewed by anonymous reviewer 1, 17 September 2018

Dear Editor, I found this MS very interesting and mostly well analysed and written. I have some, mostly minor queries intended as constructive with the aim to improve the paper. As detailed to authors, I think that one of my queries, that concerning making explicit the percent variance explained by two interesting factors, should be addressed ‘mandatorily’ as its dissection and eventual discussion may throw light on the role of two additional factors -genetic (bird host) and environmental (nest identity)- in this host-parasite system. I hope this review may be useful for the editorial team to reach a recommendation. Best regards.

- Title: this may be a matter of different personal taste but, in my view, only one of the two adjectives (wild, passerine) should remain in the title.
- L. 49. ‘relevant’: to me, this adjective is dubious in this context and raises the question what ‘irrelevant host-parasite systems’ would be to the authors. My advice is to change wording here.
- L.51-61. in my opinion, the stated rationale falls short of being complete in a host-parasite framework by only dealing with the (ecto) parasite life histories part and ignoring any, theoretical at least, dynamic response (e.g. immune responses, behavioural changes) on the part of hosts. That, is, what I am asking for here is some background on host (bird) dynamics in relation to the purported responses of parasites to climate change.

- L.77. I am familiarised with a relatively old paper by Bennett and Whitworth (Bennett, G. F., & Whitworth, T. L. (1991). Studies on the life history of some species of Protocalliphora (Diptera: Calliphoridae). Canadian Journal of Zoology, 69(8), 2048-2058) but I am unaware of a paper of (seemingly) the same authors cited as 'in press' but not included in the reference list.

- L.131. It would be better give a citation here or explaining the rationale for considering female age and no other female traits in relation to nest sanitation. Readers have to wait to find the citation/explanation later, in l. 182-184.

- L.154. If I understand well, here 'replaced' seems to indicate that nestlings had not yet fledged by the time researchers removed the nests as replacement of nests with mosses would not be necessary if chicks had fledged. For the sake of clarity, authors should be more explicit when describing the procedure.

- L.169-170. While I can understand removing predated nests from the analyses, it is plausible that nests heavily infested by blowflies are in turn more exposed to predation, due to increased begging by nestlings due to worsened nestling condition and/or a larger number of feeding visits by parents attracting predators to the nests. Hence, I think that, if possible – i.e. if blowflies could still be sampled after predation, as the number of (depredated) chicks surely is known - , some test should ideally be presented to demonstrate that the omission of those nests does not affect the results of this study; or to demonstrate that what I have just written is wrong and therefore, heavily parasitized nests do not attract predators differentially. Independently of whether these ideas/tests are included or not, I think that the identity of predators (woodpeckers, colubrid, mustelids, etc.? should be mentioned explicitly.

- L.177. Authors include both biotic and abiotic factors, so 'or' should be 'and'.

- L.182-184. Ok, but this should be better placed before (see above re: L.131).

- L.185. Authors show differences among valleys in blow fly prevalence as 'differ markedly in a range of factors' (L.190). Therefore, it would be very interesting to know whether other environmental - or even (host) genetic) - factors apart from valley affect blow fly prevalence. The statistical analyses include, as stated, female and nest box identities, in addition to year, as random factors. In my opinion, the article would improve significantly if authors 'dissect' a bit more their results and give (and discuss) the amount of variance explained by female ring and nest box location as random factors. As authors likely know, these stats can be extracted within the R stat environment, e.g. by following routines in Nakagawa, S., & Schielzeth, H. (2013). A general and simple method for obtaining R2 from generalized linear mixed-effects models. Methods in Ecology and Evolution, 4(2), 133-142, freely available here `https://besjournals.onlinelibrary.wiley.com/doi/10.1111/j.2041-210x.2012.00261.x`.

- L.195. Are laying date means corrected for female age? As the age distribution in the population may vary among years and young birds lay much later than older birds, I think it would be advisable to do so, in similar vein to your inclusion of female age in the GLMM in the former section (lines 182-184). I note that authors use a similar approximation for parasite load, when they correct for ambient temperature during nesting (legend to Fig. 3).

- L. 207. I wonder whether the nest cleaning behaviour of females is disrupted or modified by the cotton bags 'enveloping' the nest and this could affect the differences in abundance. Maybe the cotton enclosure impedes females to manipulate mosses, etc. 'correctly' to find and remove larvae and puparia? Do authors have data (e.g. videofilming) on female cleaning behaviour in those nests?

- L.241 forward. I hate to say this but...could the research itself affect blow fly mortality? Larvae and puparia are collected from nests by researchers and hence, adult flies do not emerge from the nests. I realize that the invasive technique employed is maybe unavoidable to study this system but, if all nests are 'cleaned' from blow fly propagulae, it is not hard to infer this may affect the demography of the fly population. Maybe this may be solved by stating that blow flies parasitize nests of other bird species in the area, if this is the case, that are not emptied from its parasitic contents? (I am assuming here that there no many natural holes where tits and other hole-nesters may breed and be parasitized by blow flies, but I do not know for sure).

- L.259-260. 'shortly after post-winter emergence'. When is it? Post-winter seems too loose a term having in mind that the fly will not search for bird nests to parasitize until there are hatched fledglings, as stated earlier in the MS (L.80). Please mention concrete dates if available from your study or from the bibliography.