Recommender note

Dear Maxime,

Both reviewers have read your preprint and are very positive about your study. Both provide a few recommendations, mostly for clarification. Reviewer 1 also suggests that it would be interesting to repeat your analysis including an interaction between shell size and your urban metric to see if urbanisation is a factor impacting shell size and risk of nematode infection. Please can you respond the reviewer's recommendations, make the requested changes, or respond as to why you do not consider them appropriate.

Kind regards,

Alison Duncan

> Dear Alison,

Thank you and thank to the reviewers for the positive comments and feedback! Please find below our replies to each reviewer's recommendations or suggestions. (In addition to those, please note that the map in Fig. 1 has been slightly updated with a layer for the urban centres that better reflects what was actually used in our analyses. This has no other consequences for the rest of the manuscript.)

(Line numbers in replies refer to the revised version unless specified otherwise)

Reviewer 1

Main recommendation:

In the statistics section the authors present the main models containing several fixed factors but they opted to not include interactions among such factors. I understand that the authors are mainly interested in the effect of such factors and not on the interactions among them. Additionally, most factors have no significant effects (so interactions are probably meaningless). However, the authors do find a significant effect of shell size on the probability of finding encapsulated nematodes (line 182/183). Lacking the interaction between this factor and the urbanization metric I wonder if rural snails have overall bigger shells and if this increases the chance of having encapsulated nematodes. Is that the case? If you standardize the data according to size, do you still find that rural populations are more likely to have encapsulated nematodes? It may be interesting to do an extra model including only the best urbanization metric, shell size and their interactions, to check if these two significant effects depend on each other or not. Either way it might be an interesting point to add on the discussion.

Response:

> We ran a new linear mixed model analyzing shell size in response to urbanization; we found no significant effect of categorical urbanization metric on size (as a check, we ran models with the other urbanization metrics and also found no effects). This model has been added as a

new Supplementary Material (S3) and is discussed in the main text Lines 267-271, in the context of a possible indirect effect of urbanization mediated by size.

Given this absence of effect on size, given the size*urbanization interaction was not one of our initial hypotheses, and given Reviewer #2's comments about the length of the Discussion section, we did not include in the revised manuscript or supplement the extra model(s) with size*urbanization interaction suggested by the reviewer. We did run them though for our own interest: although the AICc values changed, the best model remained the same (the one including categorical degree of urbanization). That model had a slightly worse AICc than the equivalent one with no interaction (323.1 vs 322), the size*urbanization interaction was not significant, and in any case the effect of categorical urbanization remained significant, and quantitatively near-identical.

Minor comments:

Line 48: "…cross-taxon study **of** urbanization impacts…"

> This is corrected (Line 48)

Line 49: please revise the word "seemed". Is it your interpretation of some pattern observed in the cited work? Or do the authors show a tendency and not significant effects?

> We replaced "seemed" by the more affirmative "were" (Line 49).

Strictly speaking it is an interpretation of taxon-level patterns that are presented side by side, but not directly statistically compared to each other in the cited work, hence the original use of "seemed". However, the patterns are explicit enough than more affirmative wording can be used.

Line 50: please revise "small home ranges". Do you mean habitat requirements? It is not clear.

> We removed the entire sentence as it was ambiguous and not needed for the argument.

Line 108: please remove "still".

> This is corrected (Line 106)

Line 249: please change "another related species" to "a related species".

> This fragment has been removed from the text during revisions

Line 251: removing "in the species however" may improve the flow of the sentence.

> This is done (Line 245-247)

Line 285: do you mean body size or shell size?

> Shell size, as this is what was measured. This is corrected in the revised version of the paragraph (Line 284)

Reviewer 2

1. Please explain more about the number of nematodes found in the shells. The data is in supplementary but I think you could make more of nematode abundance e.g. I would like to know what the range of numbers of nematodes you found in the shells is and does that differ with location? The max I found was 101 nematodes in a snail shell...

> We have added to main text Methods an explicit mention of the overall range of abundances found in our shells (1 to 58)(Lines 147-148). We have added two elements to the Supplementary Material S2:

(i) the marginal and conditional R^2 values for the abundance model, similarly to the prevalence model in the main text. They show that although our fixed effects (urbanization, shell phenotype) do not explain it, there is still substantial between-population variation in nematode abundance.

(ii) a mention of the range of mean abundances per population, which confirms it.

2. In the discussion please change 'Phasmarhabditis elegans' to 'Phasmarhabditis hermaphrodita'.

> Thanks for catching that mistake! This is corrected (Line 296)

3. In general, I think your discussion is far too long and should be edited down a lot. I think you need to consider the fact there may not be many parasitic nematodes in cities compared to rural environments. You do touch on this, but there needs to be more.

> We have edited down the Discussion, while still adding elements suggested or requested by either reviewer. Even with the new additions, the revised wordcount of the Discussion is still down roughly 10-11% compared to the original.

We do agree that ideally we would need to add a bit more about the possibility of fewer parasitic nematodes in cities. However, unless we missed references, there are simply not that many relevant studies of nematodes parasites of land molluscs including samples from both urban and non-urban areas. We slightly expanded that part of the Discussion nonetheless, adding some more elements based on Aziz et al 2016 or Andrus et al. 2022 (Lines 232-239).

4. Did you find any trematodes or mites in the shells?

> No; this is implicit from the original Methods ("no other metazoan parasites were recorded", line 150 in the original text). This is written to be more explicit in the revised version: "we found no mites, trematodes or other parasites in any of the shells" (Lines 146-147). Non-nematodes trapped really do seem to be anecdotal "by-catches" of the anti-nematode defence system, it would appear from all the combined studies so far…

5. I find it really interesting you found no difference in the numbers of nematodes trapped in the different morphs of shells. I also found this experimentally, but there is a general increase in the number of nematodes encapsulated in shells that have 0, 1 then 3 to 5 bands, so I just wonder if there is something going with the different morphs. (Sorry nothing for you to really add to the paper there, I'm just thinking).

> Yes, we expected to find a morph effect based on the link with active infections in our previous paper, but it seems not, at least not based on the data so far.

6. Please can you add some pictures or a picture of nematodes being trapped in the shells?

> This is done, as the new Figure 2.