10th January 2020

Dear Editor,

You notified us on January 2, 2020 that we were encouraged to review and resubmit a revised version of our manuscript entitled “Stoichiometric constraints modulate the effects of temperature and nutrients on biomass distribution and community stability”.

We believe that the revised version satisfactorily addresses the few remaining comments (see our detailed point-by-point explanations below). If you have any questions, please do not hesitate to contact us. We really appreciate your assistance in this matter.

We would be grateful to you for reconsidering this manuscript and we hope that you would approve this new version. We look forward to hearing from you at your earliest convenience.

With kind regards,
Arnaud Sentis on the behalf of all co-authors.

Decision by the handling editor (Dr. Elisa Thebault).
Dear authors,
Many thanks for your thorough revision of the manuscript. There are only a few remaining minor comments to address from one of the reviewers before recommendation.
best wishes, Elisa
Our response: We thank the handling Editor for the positive comments on our manuscript. As recommended, we have modified the manuscript (see below and the blue font text in the main text).

Reviewer 1
I enjoyed reading the revised version of this manuscript from Sentis et al. I was impressed by the authors response to my queries. I have two more important points that I hope will address my remaining concerns along with some extra line comments. My two main concerns are (#1) the extinction threshold interpretation and the (#2) the assumption of e^ns = e_max. I've really enjoyed reviewing this paper--the authors are exploring an important and exciting topic in my opinion.
Our response: We thank reviewer 1 for the positive comments on our manuscript. We have corrected all the typos (see text in blue colour font in the main text) and addressed the two main concerns (see below for more details).

Reviewer 1
Line Comments:
Line 34-35: Can you state the direction of the effect rather than referring to a generic change?
Our response: We now state the direction (lines 36-39).

Reviewer 1
Line 42-160: I would argue for a further reduction in the text here to focus in on the key mechanisms your model addresses. For example, you discuss (e.g. Line 97, 116) drivers and effects that you model is not currently considering. This is my opinion, but not a binding request if the authors feel this extra content is important.
Our response: We decided to keep this content as we think that it enlarge the scope of the introduction by highlighting how stoichiometry can be important for other drivers and effects such as CO_2 and biogeochemical cycles. This could be interesting for a more general audience.

Reviewer 1
Line 90: Extra citation
Our response: removed.

Reviewer 1
Line 128-132: This is exactly what I had in mind. It makes it clear to me upfront what your contribution is. Very helpful!
Our response: Thank you.

Reviewer 1
Line 173-175: I would argue this justification is extra, since a Type II functional response already an expected part of the RM model and you have already argued why you are using the RM model (Lines 141-144). Don't remove it on my account if you have another reason to keep it in, but it might help with flow.
Our response: We removed this justification.

Reviewer 1
[Main Concern #1] Line 331-333: Why did you decide that the extinction threshold you set in the methods was not the right metric? You conclusion, that the RM model has a higher probably of extinctions across your gradients, is based on assuming the extinction threshold doesn't matter. It would help me understand your claim if you state why we should not consider the extinction threshold. I think you need one more step here to make it clear why you are using an extinction threshold and what it means.
Our response: We apologize for the lack of clarity. We did not mean that the extinction threshold is not the right metric. The aim of comparing the simulations without threshold (i.e. assuming that population does not go extinct even at really low biomasses) to the simulations with the threshold was to assess how many of the extinctions are driven by population fluctuations that bring the biomass densities below the threshold. We clarified this point both in the methods (lines 288-290) and the results (lines 320-323 and 339-341).

Reviewer 1
Line 344-347: I really like the addition of this proof. I have a concern about one step (see comment Line 922).
Our response: Thank you.

Reviewer 1
Line 397-415: I suggest moving this to the methods section.
Our response: We agree that this section could go the methods section. However, we decided to keep it in the results section as it better fits with the flow of the manuscript as we first need to explain the results before we can explain how we investigate the dynamic effects of stoichiometric constraints.

Reviewer 1
Line 402-405: OK, I follow your explanation now. Thanks for the extra clarity!
Our response: Thank you.

Reviewer 1
Line 466-479: I really like this paragraph.
Our response: Thank you.

Reviewer 1
Line 490: A good citation here would be Menge et al. (2012: PLoS One). I think relaxing this assumption would be a really exciting addition to this framework, but I agree that it is beyond the current scope.
Our response: We added Menge et al. (2012)

Reviewer 1
[Main Concern #2] Line 922: You make the assumption that e^ns = emax in your proof. This assumes that somebody fitting the RM model would parameterize e^ns as the maximum possible assimilation efficiency rather than the expected assimilation efficiency given the standing heterogeneity in resource quality. I do not think this is a reasonable assumption. Both a literature parameterization of e^ns and a model fit to empirical data (assuming the parameters are identifiable), should settle on a value for e^ns that is the average value for a population experiencing heterogeneity in resource quality. This would mean that e^ns < emax.
Our response: We apologize for the lack of clarity. One underlying assumption of the RM model is that resource stoichiometry is not limiting and do not influence conversion efficiency. An additional assumption that we make is that the value of the conversion efficiency is taken for a consumer feeding on a high quality resource, and as the reviewer points out, e^ns = emax. This assumption is in line with most previous studies using the RM model assuming a value of *e* of 0.4 for an herbivore feeding on a plant and of 0.85 for predator feeding on an animal prey (Yodzis & Innes 1992; Binzer *et al.* 2012; Fussmann *et al.* 2014; Uszko *et al.* 2017). This values corresponds to conversion efficiency for high resource quality (Peters 1983) However, these values can be much lower when the resource is of poor quality (i.e. when there is a stoichiometric unbalance between the consumer and the resource nutrient: carbon ratio) (Elser *et al.* 2000; Elser *et al.* 2007). In other words, the consequence of stoichiometric constraints is to lower the values of conversion efficiency from the RM model. We clarified this point both in the main text (lines 220-224 and 233-234) and in the proof (lines 932-938).

Reviewer 1
Line 940: Can you provide the year for the Yodzis paper in Table S1 and add it to the reference list?
Our response: we modified the reference for Peters 1983 and added the reference to the list.

References

Binzer, A., Guill, C., Brose, U. & Rall, B.C. (2012). The dynamics of food chains under climate change and nutrient enrichment. *Philosophical Transactions of the Royal Society B: Biological Sciences*, 367, 2935-2944.

Elser, J., Sterner, R., Gorokhova, E.a., Fagan, W., Markow, T., Cotner, J. *et al.* (2000). Biological stoichiometry from genes to ecosystems. *Ecology Letters*, 3, 540-550.

Elser, J.J., Bracken, M.E., Cleland, E.E., Gruner, D.S., Harpole, W.S., Hillebrand, H. *et al.* (2007). Global analysis of nitrogen and phosphorus limitation of primary producers in freshwater, marine and terrestrial ecosystems. *Ecology Letters*, 10, 1135-1142.

Fussmann, K.E., Schwarzmüller, F., Brose, U., Jousset, A. & Rall, B.C. (2014). Ecological stability in response to warming. *Nature Climate Change*, 4, 206-210.

Uszko, W., Diehl, S., Englund, G. & Amarasekare, P. (2017). Effects of warming on predator–prey interactions – a resource-based approach and a theoretical synthesis. *Ecology Letters*, 20, 513-523.

Yodzis, P. & Innes, S. (1992). Body size and consumer-resource dynamics. *The American Naturalist*, 139, 1151-1175.