Review of:

"Long-term impacts of global temperature stabilization and overshoot on exploited marine species" by Morée et al.

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Paper summary:

Morée et al. quantify the impact of ocean warming and deoxygenation on the habitability of the ocean by 46 exploited marine species until year 2500. The authors consider temperature stabilization scenarios and overshoot scenarios, with temperature in the latter case reaching a maximum before decreasing and stabilizing. Ocean habitability is calculated using the aerobic growth index and environmental conditions simulated in transient simulations conducted with the GFDL ocean-atmosphere general circulation model. Results demonstrate that only around half of the total habitat loss is realized when target warming levels are reached. Habitat loss continues in the decades to centuries after that. Species adaptation may lower the overall habitat loss.

General comment:

The manuscript represents a welcome departure over previous work. It is clearly written and illustrated with figures of excellent quality that efficiently convey key information supporting the text. The methods require lots of attention and are not always easy to read, but this is for the better since the setup is described in detail and I do not think this aspect can be improved without being detrimental to the scientific content of the manuscript. The results are also very well organized, starting with describing the overall changes in aerobic growth index, then the underlying environmental / climatic drivers, before focusing on changes in habitability at the species level and finishing with an analysis of the impact of adaptation, overall constituting a very informative and comprehensive work. I encourage the publication after very minor revisions and provide suggestions of improvements below.

Please note that I was not able to try and run the code, which was not provided with the manuscript (with a simple placeholder found in the 'code and data availability' section).

Main comments:

1. Selection of the 46 species. It would be beneficial to provide a short description of the species selected in the Methods: what are these species, and how were they selected? A reader being not familiar with previous work cited would better understand the context of the study.

2. Selection of the climatic scenarios. Similarly, it may be instructive to tell a bit more about the choice of the stabilization scenarios. How were the magnitude and time scales of temperature change selected? It may be good to discuss these numbers with regards to the IPCC scenarios. Regarding the overshoot scenarios, it may be interesting to discuss the plausibility of the time scale

of overshoot and subsequent stabilization: is the time scale plausible, based on previous work that did not force temperature changes but mechanistically simulated them, e.g. using sediment-enabled earth system models of intermediate complexity? Such discussion would better frame the context of the work and hence strengthen the study.

3. Infinite migration potential. An implicit assumption of the approach is that studied species can always populate the regions of the ocean where oxygen supply is sufficient for them. In reality, we might imagine that some species would not be able to keep up with changing environmental conditions due to limited migration potentials, which may impact the calculated habitability and reversibility and, in some extreme cases, maybe even lead to extinction. The effect would be strongest for species exhibiting important, transient reductions in habitability, such as Thunnus atlanticus after Fig. 5b, and for species exhibiting low migration potentials. I would encourage the authors to briefly discuss this potential limitation in section 4 (discussion and conclusions).

4. Species limitation towards the cold edge. My understanding is that the aerobic growth index suffers of the same limitations as the metabolic index with regards to its incapacity to mechanistically limit the extension of the species in the high latitudes, where [O2] is high and temperatures low. This would bias the distribution of the species (and the latitudinal diversity gradient). Is that the case? Was instead some empirical limitation added like Deutsch et al. (2022; https://doi.org/10.1126/science.abe9039) did? I think this limitation should be shortly discussed, just for the reader to be aware of this aspect.

Minor and technical comments:

- Lines 73–74: what is “lithogenic material” referring to, here?
- Lines 138 and 139: Here we read 296 years, while 269 appears elsewhere (e.g., caption of Figs. 2, 5, A2), please check. Also, I did not understand how this duration was established, would it be possible to expand on / clarify this point on lines 136–140?
- Lines 183–184: is there any technical reason for not just using the 3D-dimensional model output?
- Line 210: This section should be called “Results”
- Line 215: would it be possible to also provide rough equivalents of pO_2 values (in mbar) in terms of oceanic [O_2] (in micro mol / kg or similar), for reader more familiar with these units?
- Line 225: reference to panels (g,h) is missing.
- Line 228: please revise (“species’ their”).
- Line 272: “drivers”
- Caption of Fig. 2: since AGIrel is a difference as such, wouldn’t it be more rigorous to write “as the difference AGIrel between…”
- Line 295: “increased ventilation and reduced biological oxygen consumption”. This combination, although well demonstrated based on the figures, is counter-intuitive. Is it because increased convection induces the advection of low-nutrient waters coming from the low latitudes, similar to Rae et al. (2020; https://www.science.org/doi/10.1126/sciadv.abd1654)?
- Line 406: what does ”reversibility timescales” refer to?
- Lines 414–415: “our findings align with variable species responses seen in distribution shifts”. If this statement is supported, I think it would be a great addition to expand a bit on this. Otherwise, I would suggest deleting this sentence which, without additional details, is not very convincing.