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Biogeosciences Discussions

Interactive comment on "Kinetic bottlenecks to chemical exchange rates for deep-sea animals – Part 1: Oxygen" *by* A. F. Hofmann et al.

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Reply to comment 1 of Reviewer 2:

In this manuscript, Hofmann et al. aim to combine physico-chemical characteristics of seawater into mechanistic equations that try to define and describe the oceanic oxygen supply potential. The authors aim to elucidate how global warming will affect oxygen limits to marine life. Overall, is a well-written manuscript that highlights the importance of temperature and pressure in oxygen supply.

We thank the referee for this positive comment. But we point out that we do not

describe here the limits to marine life - only the effect of small changes in external T and O_2 on the pre-existing rates, not any initial absolute limit or value. Overall these comments are much appreciated and help improve our presentation.

The main problem I see is that the manuscript does not attempt to include organism dependent properties.

We made a careful attempt not to focus on any specific organism since there is such a wide range of adaptations to hypoxia etc. But our paper is concerned with the effects of change resulting from ocean warming. No matter what the baseline property of any one organism is, each and every one will experience the same increase in diffusion coefficient, and loss of dissolved O_2/change in pO_2 in the external medium. Thus the paper is primarily concerned with the near universal effects of the changes in the external ocean as affecting rate processes across a diffusive interface; not with any particular organism.

To elaborate: we deliberately did not include biological species specific properties in our reasoning, since we want to descibe properties of and changes in the external medium only. However, our treatment does contain many variables and concepts (e.g., L, etc.) where organism specific values (or even model formulations) might be plugged in to calulate values of our proposed metrics that are more species specific. Individual researchers interested in a specific organism, might thus calculate and/or refine our quantities to be highly species specific. However, doing that ourselves would be not only beyond the scope of our paper but also somewhat beneath the point we want to make, since we want to describe the ocean as a changing environment that all animals have to face and compare the patchiness within this environment, that again, all organisms have to face. There is such a vast variety of organisms and adaptations, that including specific organisms in our treatment here would skew the picture towards

this particular organism which is not our intention here.

Moreover, I don't see that most oceanographers would change the use of pO_2 or oxygen content to use a more complicated metric to elucidate oxygen supply potential. Results for the Mediterranean Sea are surprising. I believe that authors should explain better these results and give an honest opinion about if the use of these metrics is appropriate for these regions that appear to gain a benefit from warming overcompensating oxygen loss.

We appreciate the comment. The Mediterranean example illustrates in some way the specific organism and specific input parameter problem. Within the envelope of organism properties, ocean conditions, and input parameters, we chose to examine, the effects of increased diffusivity beat the small loss of dissolved O_2. If we were to select another range of parameters we could "force" a different result, which, in itself, we deem not necessarily useful.

We acknowledge, however, that the weight or significance of the results for the Mediterranean is not properly expressed in discussion version of the manuscript yet. In the revised version of the manuscript, we will thus tone down the significance of these results for the Mediterranean. In particular, we will make **absolutely clear** in the revised version that we do **not** claim that ocean warming in general is **beneficial** for marine life. Results for the Mediterranean are what we find for the chosen combination of generic parameter values and they are meant to show that such a result **might** be possible for certain organisms in certain regions of the world oceans. They are by no means meant to be the universal hard truth of what is to come. We will change the discussion of these results to better reflect this.

As to whether ocean scientists will adopt this scheme is an individual choice. But the chemical problem is near identical to the estimates of air-sea gas exchange rates where the sea surface diffusive boundary layer is used. This notation and knowledge of piston velocities, boundary layer thickness, and Schmidt number dependence has been deeply embedded in the field for decades (although it also is a radical simplification of complex processes that really happen on the sea-surface air-water interface - but still expresses the general principles in a very useful way) and is a part of every graduate course. We hope that a similar evolution can occur here.

Specific comments:

The use of acronyms throughout the manuscript makes it difficult to follow, especially for authors not familiar with them.

This is indeed a valid and useful comment. We will add a glossary of acronyms, abbreviations, and defined variables used in the manuscript to the revised version.

P. 13819. L. 22-26. Paper by Gray defines different limits for different groups of organisms, not just a simple concentration limit as stated by the authors.

We could add further comment on such papers - but the abstracts etc clearly communicate a sense that a particular limit is damaging, and temperature/depth independent units are clearly used. We hope simply to add quite formally the effects of external T and P which are clearly understood.

We will, however, make clearer in the revised version of the manuscript that Gray et.

al. and other authors do not propose one "single" [O_2] value but rather a small group of values.

A review on the thresholds of hypoxia for marine communities by Vaquer-Sunyer and Duarte (2008) also shows a wide range of hypoxia thresholds for marine organisms and that the use of a single, universal threshold is not appropriate. Authors should acknowledge that previous studies point to the need to use a range of thresholds instead of a single value (Gray et al. 2002, Vaquer-Sunyer and Duarte, 2008). P. 13821. L. 4. The authors state that the use of a single concentration value as a limit provides no temperature dependence information. This can be overcome by using the values as a percent of oxygen saturation or pO2.

We thank the reviewer, but point out that in Hofmann et al. (2011) cited here we very carefully reviewed the ocean pO_2 status. Indeed, pO_2 and/or percent saturation values do include temperature effects and are "better" than mere concentration values in that respect. However, the use of percent saturation is a relative not an absolute value unless Henry's Law is invoked. We make that clear here. Furthermore, the dependence of oxygen supply on diffusivity and boundary layer effects is not included when one uses pO_2 only. Here we treat the set of ocean properties that influences supply rates. We thus claim that our treatment holds additional value over the use of pO_2 and percent saturation only.

Furthermore, as mentioned in our reply to the previous comment above, we will make clearer that authors such as Gray et al. (2002) and Vaquer-Sunyer and Duarte (2008) do indeed also favor a range of [O_2] values over one single value.

Effects of temperature on oxygen limits for marine life have been reviewed recently.

No reference to that work is presented here.

We are not sure which particular review is mentioned - we would love to have the full citation and will then, of course, acknowledge this work in the revised version of our manuscript.

There seems to be typographic errors in most equations where the "=" symbols appear with ":" before (e.g. equations 7, 9, 11, 12) and equation 14 with and exclamation symbol above the equal symbol.

We use the standard mathematical notations here, where $:=$ means "is equal by definition" and $\stackrel{!}{=}$ means "is required to be equal" (in the numerical routine we describe). We will add these explanations as notes in the text just after the first occurrence of the respective notation and we will also add these explanations to the glossary.

References

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Interactive comment on Biogeosciences Discuss., 9, 13817, 2012.