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Biogeosciences Discussions

# *Interactive comment on* "Kinetic bottlenecks to chemical exchange rates for deep-sea animals II: Carbon dioxide" *by* A. F. Hofmann et al.

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## Reply to reviewer #2

In the manuscript by Hofmann et al. the authors aim to combine physico-chemical characteristics of seawater into mechanistic equations that define, describe and predict how climate change will impact the boundary layer environment of marine animals. This manuscript specifically aims to evaluate the impact of increasing $CO_2$ concentrations in the world's oceans on marine organisms ability to export $CO_2$. In general, I believe that this manuscript is well written, the language is fluent and precise and the overall presentation well constructed and clear. The topic is within the scope of



BG. The manuscript basically addresses a relatively understudied topic concerning the importance of the factors controlling $CO_2$ within the diffusive boundary layer around marine animals in an ocean changing in temperature and $CO_2$.

*Thank you very much, we appreciate the comment. We believe the work is indeed timely given the rise in concern over rising oceanic $CO_2$ content.*

My field of expertise is not in biology, but I believe the inclusion of some organism specific properties would be useful. The use of one example for a species with well-known kinetic mechanisms would be useful and help to bring the subject to life.

*Life in the oceans is immensely diverse. Organisms exist in all shapes, sizes, with all kinds of mechanisms and adaptations. Organisms can survive in many an environment, also extreme environments due to respective adaptations. When we started investigating kinetic limitations posed by boundary layer transport through a layer of external medium around organisms, we did try to focus on a representative, well studied organism. This, however, was not at all well received by the biological community, as we realized in many talks that we gave on the subject and previous versions of the manuscript that we showed to biologists. The unanimous tenor was that given the huge diversity of organisms out there, it is virtually impossible to find one proper example species, as always large groups of organisms will be misrepresented. This is a valid point.*

*Including a long list of several example species would result both in a treatment that is too long for one single paper and would also not be in line with our goal with this paper of delivering metrics that can describe the ocean as a habitat and compare various* *oceanic regions in a generic, non-organism specific way. What we want to describe here are ocean physico-chemical processes and conditions that are independent of any given organism, yet that every organism has to face and find its own limits within. We thus deliberately choose not to investigate what happens inside an animal, but only what happens outside an animal.*

*Real-life gas exchange for organisms might be limited by transport trough the gas exchange tissue of the animal, or by the diffusive transport of the respective gas through the diffusive boundary layer around an animal (see also revised Section 2.1.1 of this manuscript). The former is highly dependent on the organism considered, and the latter is not, it is a quality of the medium surrounding any organism. The true flux, however, can never be greater than the maximum flux through the boundary layer around the organism (see also comments to reviewer #1 and Section 2.1.1. of the revised version of this manuscript). Thus the maximal flux through the boundary layer constitutes an "upper boundary" for the true flux, that is depending on the oceanic environment that any organism faces and that thus can be used to describe, compare, and manage various oceanic regions. This is the subject of this paper and its companion paper - deliberately without any species specific properties or mechanisms.*

*Therefore, we will not include more organism specific properties in a revised version of the manuscript. However, every researcher can plug in species specific values or even model descriptions at his or her leisure in our treatment and thus generate more species specific values for our properties, doing that ourselves, however, is beside the scope and the point of this current paper.*

Additionally, a more in depth discussion on ocean acidification would be helpful as well.



*While a true in-depth discussion of ocean acidification would be beyond the scope of this paper, we will, however, add a short paragraph on ocean acidification and its relation to the subject of our paper to Sectoion 1 of the revised version of the manuscript. Additionally, we will include more references to papers with in-depth discussions of ocean acidification and its history.*

The concept of "enhancement factor" is interesting but a broader discussion would be useful.

*We added a paragraph after the initial definition of EF in Section 2.1.2. that discusses the EF concept, its history, and its application. It notes that EF and similar concepts of treating CO2 reactivity in the DBL have a long history and gives respective citations, it stresses the fact that EF is defined to also describe cases that are not in (reaction) equilibrium, and it gives typical values of quantities involved to illustrate the orders of magnitude for typical cases. (see also reply to respective comment of anonymous reviewer #1).*

The results for the Mediterranean Sea seem to be the most surprising. The authors should explain these results more in depth. Are these results appropriate and what one would expect for the region in question?

*The Mediterranean has been included in our comparison, because it is different to all other stations in one key variable for our defined quantities: temperature. Also different alkalinity values and thus a different state of the seawater carbonate system (and influence on EF) make the Mediterranean results stand out from the rest of* *the stations. This "outlier" character of the Mediterranean was expected from the beginning and was included to show the influence of natural variability on the quantities defined here.*

*We will add a paragraph in section 2.7. of the revised manuscript to detail and discuss the role of the Mediterranean in the lineup of our example stations.*

The treatment of diffusion and $CO_2$ reactivity as two separate steps does not seem to be useful. Since $CO_2$ is highly reactive in seawater, considering only the purely diffusive $CO_2$ export flux sheds no light on the question that the authors propose to answer. It would be appropriate to discuss the limitations of this approach.

*Here, we follow established practice in this field. For example Emerson (1975) uses this same approach to illustrate the enhanced invasion of CO2 into a lake by comparing to the reaction free case (we will explicitly include in the revised version of the manuscript that we follow Emerson (1975) with this approach). We show it applies near identically for enhanced export of* $CO_2$*. This is ideal for teaching the principles and we feel it is helpful. It also helps for the comparison with unreactive* $O_2$*.*

*Additionally, one has to note that we do not treat diffusion and reaction as two separate (and temporally distinct) steps. We only derive the final equations (that do contain diffusion and reactivity together - which is, indeed, the only sensible and "realistic" way to look at* $CO_2$ *export) in a two-step fashion to foster understanding (we will explicitly state in the revised version of the manuscript that only the derivation of the equations is done in a two-step fashion). Since all final results do include both diffusion and reactivity, there is no limitation to this approach at all. The hypothetical (!)*



*"unreactive" case is just occasionally shown to illustrate the importance of considering* $CO_2$ *reactivity.*

*We will make this clearer in the revised version of the manuscript:*

- *everywhere where "results" from the EF=1 (i.e. the hypothetical "unreactive" case) are discussed (before: section 3.1., in the revised version: appendix) it will be made even clearer that this is a hypothetical case that only serves as a comparison to the sensible "real" EF values*
- *We removed the EF=1 case from the* $RP_{CO_2}$ *plots (previously: fig 2, in the revised version: Fig 8 in the appendix), to avoid misunderstandings about the hypothetical nature of this EF=1 case.*

## References

Emerson, S. (1975). Chemically Enhanced Carbon Di Oxide Gas Exchange In A Eutrophic Lake A General Model. Limnology and Oceanography *20*, 743–761.

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