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Biogeosciences Discussions

Interactive comment on "Shell dissolution observed in *Limacina helicina antarctica* from the Ross Sea, Antarctica: paired shell characteristics and in situ seawater chemistry" *by* Kevin M. Johnson et al.

Anonymous Referee #1

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Review for Biogeosciences, manuscript BG-2016-467 "Shell dissolution observed in Limacina helicina antarctica from the Ross Sea, Antarctica: paired shell characteristics and in situ seawater chemistry" by Johnson KM, Hoshijima U, Sugano CS, Nguyen AT, Hofmann GE

General comments

This manuscript aims at describing in situ carbonate chemistry conditions of the thecosome pteropod Limacina helicina antarctica in the Antarctic Ross Sea paired with their shell condition and provide some base data on species life history traits and abun-

dance. The motivation for this study comes on the one hand from the growing evidence of pteropods as sentinel organisms of ocean acidification, the fact that the study region (McMurod Sound) is proposed to experience aragonite undersaturation by the year 2030 during winter, and on the other hand from the lack of ecological knowledge on pteropods in the Ross Sea. Calculated aragonite saturation states ranged from 1.16 to 1.24, and hence were never below 1. Roughly 63% of investigated shells showed some degree of degradation (of the protoconch area). A main conclusion put forward in the abstract is that "shelled pteropods of the Southern Ocean are experiencing aragonite under-saturation events in the present-day that lead to a majority of individuals with shell dissolution".

Generally, I appreciate this type of approach looking at the in situ state of pteropods related to prevailing biogeochemistry and acquire more ecological data important to describe life history traits very much. All of this hasn't been done before in the Ross Sea (to my knowledge) and we are lacking knowledge on thecosomes in light of ongoing ocean change. However, the present manuscript has some major shortcomings and I don't think it can hold what it seems to promise at the beginning. Most importantly, I think the conclusion in the abstract mentioned above is not supported by the data because the authors don't know anything about the recent carbonate history of the pteropods but only assume that pteropods lived at under-saturated conditions and this being the reason for the dissolution patterns found. Further shortcomings are: 1) The shell dissolution was analyzed only in the protochonch area of the shells. The protoconch is the embryonal/veliger stage and oldest part of the shell. In many species it is naturally shed or broken off from the permanent adult shell (Lalli and Gilmer 1989). I.e. it is the area where damage due to any kind of reason first becomes apparent. No characterization of dissolution of the rest of the shell was done which makes it rather impossible to compare these data with other published work. In fact, the younger part of the shells (juvenile) were apparently not(?) affected by degradation (Fig. 6) indicating pristine shells, thus rather suggesting life at super-saturated Ωaragonite conditions. 2) No attempt was made to relate biogeochemistry data to dissolutions patterns found,

i.e. shell dissolution results were analyzed isolated from prevailing pH, Ωaragonite or pCO2. That way it is not possible to draw any sound conclusion whether the described dissolution patterns have anything to do with carbonate chemistry and even more so with potential changes due to anthropogenic impact. However, as presently prepared, the manuscript "pretends" that results proved a connection between shell dissolution and in situ seawater chemistry. I don't think it allows for such conclusion. 3) Abundance and shell size data were collected but apart from the fact that they were collected and presented in the results, the reader is left alone with these data. Certainly it is not possible to deduce a whole life cycle from such data collected over a rather short time interval (6 weeks), but still it is possible to draw some conclusions and compare with current knowledge. What do these (precious indeed!) data tell us?

For these reasons, unfortunately, I cannot recommend the present manuscript for publication. I would really like to see a resubmission including an amended data set (with more elaborate shell analyses for example) and data analyses (linking shell dissolution to prevailing carbonate chemistry), if possible, and free of unfounded speculations.

Some minor comments:

- Introduction: Actually, the Arctic Ocean is expected to be first in temporal/spatial undersaturation events, not the Southern Ocean.

- M&M: Nutrient data are lacking in the carbonate chemistry calculations. Should be included as they affect results.

- Hoshijima et al. 2016: Is this an accepted manuscript in J Exp Biol? Couldn't find it?

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