Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-537-RC1, 2018

Biogeosciences
Discussions
Open Access

Interactive comment on "Deep-sea benthic communities and oxygen fluxes in the Arctic Fram Strait controlled by sea-ice cover and water depth" *by* Ralf Hoffmann et al.

P. Renaud (Referee)

paul.renaud@akvaplan.niva.no

Received and published: 13 February 2018

Review of Hoffmann et al. 'Deep-sea benthic communities and oxygen fluxes'. . . (Paul Renaud, reviewer)

General comments

The authors have collected a vast amount of data from regions that are not easily accessible over two years and across a range of depths in the most important gateway to and from the Arctic Ocean. Investigations of how variability in environmental and especially benthic-process data vary spatially here are carried out with a focus on the

potential impacts of sea-ice cover and water depth. The study is valuable just in terms of the data it provides in this important but understudied region. In addition, the authors provide a largely balanced discussion of many of the critical points here.

I am concerned about several issues that are not brought into the discussion (time scales of response and which variables may have meaningful causative links) or are brought perhaps too forward (sea ice cover, which is in my mind less of a driver than the water mass properties responsible for both the ice cover itself but also the productivity regime and vertical flux). Finally, I urge more caution in development of future scenarios without a better understanding of whether nutrient loads can sustain increased production in the currently ice-covered areas.

Specific comments

1. Time scales of response. Direct (and only linear) correlations between environmental parameters and O2 flux may be misleading, or non-representative, depending on when the samples are taken (and when relative to the bloom/flux phenologies among different stations). Benthic biomass/density/structure likely respond to various factors (especially food-related parameters which are often covariates of depth) in a more seasonally (or up to decadally) integrated fashion, whereas O2 consumption/C remineralization are often more responsive to food inputs on a much shorter time scale (approx. weeks) (e.g. Renaud et al. 2008 DSR II). This must be considered in your interpretation.

2. As noted in the Methods, the difference between the eastern and western Fram Strait and potential consequences for benthic processes go beyond ice cover. Advected POC/PON/dissolved nutrients and warmer temperatures on the eastern side are far greater than on the western side. Of course that is linked to why the ice is there, but in this case ice is more of a covariate and perhaps less likely a causative factors. In addition, different zooplankton and microbial communities can well lead to different 'food' deposition. This must be considered in detail if the two transects are to

be comparable.

3. Methods: it appears that most of the variables measured were only assessed from the top 1 cm of sediment. Can you provide a justification (data-based) for this? For meiofauna it is often the top 2-3 cm that contain the majority of the fauna, and for macrofauna, at least the top 5 cm, even at deep-sea depths.

4. Ice cover in the two 'regions' is essentially 70-80% vs 1-10% (heavy ice/no ice). Except for EG V (and N5 which is often excluded from analysis) there is nothing in between. How might this affect your results/interpretation? Many of the results from N5 are more similar to the LSC than the HSC stations (see comment 2 above).

5. Ice cover as the key? factor. Related to comment 2 above, have you evaluated whether correlations/differences between benthic parameters and ice cover are the strongest relationships among your data? Primary productivity, vertical flux attenuation, and essentially food supply to the sediment surface may or may not be caused by sea ice in any way. Or it could be a feature of Arctic vs Atlantic water supply that causes a 'cascade of processes' and sea ice cover may just be a covariate with limited or even no direct causative effect (hence a logic problem on p 15 l 23-25). Your discussion implies that ice is the overriding factor but I do not see where you tested for this, or if it is even possible to disentangle all these variables to isolate depth as the key factor. If you ran similar analyses but grouped stations based on water mass characteristics instead of ice cover you would find the same result.

6. P 12 l 6: unclear what water column nutrients, presented as a snapshot without context of 'preformed' (winter) concentrations add here. Bloom phenology certainly is responsible for e.g. the lower nitrate in WS vs EG. Consider removing these data. The discussion on p 15-16 and then sec 4.2 is not really based on the data collected, but more of a general pattern documented in the literature. I agree some of this should be included, but wouldn't a more extended and balanced discussion of benthic process rates and the other factors (proximal) responsible for variation in these rates be

appropriate here?

7. Nutrient supply under the ice in EG is extremely low and not expected to increase with further melting of sea ice (e.g. Mauritzen et al. 2011 Prog Oceanogr). This casts serious doubt into any scenario where increased PP due to more light is invoked.

Technical corrections/details 1. P4 l6-9: unclear sentence. Perhaps just unnecessary (same for l 14-17 as it just repeats what you have just written) 2. P4, l 24: controlling the benthic ecosystem? Be more specific, including what you mean by 'labile organic matter' (different from benthic chlorophyll?) 3. P6 l 10: if the algorithm can estimate ice cover at over 100% then couldn't values between 0 and 100 also be mismeasurements? Could there be some (automated) check to assure that adjacent pixels are 'similar', or some other way of testing for mismeasurement in this range? 4. Also, is an annual average (vs some other ice cover parameter) the most relevant measure of ice cover? 5. P 6 l 25: frozen not frosted 6. P 8 l 25ff: Was non-local mixing (i.e. non-linear profiles) observed? How was this accounted for in the O2 flux calculation? 7. P 9 l 1-5: how much of the sediment mass could be attributed to salt from the drying process? 8. P 10 l 5: consecutive not subsequent 9. P 10 l 14: 'x to zero mean and unit variance' is unclear 10. P 10 l 21: you must exclude EG II from the analysis. You cannot make the assumption and assign a value. It was fine to exclude the shallow station, and you should do the same with EG II 11. P 10 l 30: you need to indicate whether there was a different depth relationship between the two regions and then say what you did if this was (or was not) the case. 12. P 12 l 6: unclear what water column nutrients, presented as a snapshot without context of 'preformed' (winter) concentrations add here. Bloom phenology certainly is responsible for e.g. the lower nitrate in WS vs EG. Consider removing these data. The discussion on p 15-16 is not really based on the data collected, but more of a general pattern documented in the literature. 13. P 12 l 10ff (and Fig 3). Please indicate any statistical results such that the figures correspond to what is written in the text regarding comparisons between the two regions. Only statistically significant results should be expressed as 'differences' (e.g. solute exchange is likely NS but significance is implied. Also, please clarify how many stations (and depth profile) each bar represents. This has some bearing on your comments about variability between the two locations. 14. P 12 l 18: 'pelagic food supply indicating parameter in the sediment' rephrase to clarify that these are sediment values. . ..and careful about how you define food quality. Not all organisms each chlorophyll (in fact maybe few actually do). Bacteria themselves are likely food for many organisms, and phaeopigments and other OC may also be quite high quality food for others. 15. P13 l 21: 'which indicates bacterial activity and bacterial remineralisation as the major oxygen consumer' please indicate why you conclude this. Why would bacterial oxygen consumption not be reflected in DOU data. These are effectively two different techniques to measure the same thing, each with underlying assumptions. The conclusion you make regarding the ratio is not supported. 16. P 14 l 7-13: I would focus on the differences among EG and WS stations as revealed by PCA, and not individual variable correlations (which are NOT real correlations but instead are ordination-based relationships! If you want to look for correlation then run that analysis on the raw data). 17. P 16 l 17: but macrofauna biomass has a similar relationship with depth between the two ice-cover systems 18. P 16 l 18-20. Repetitive 19. P 16 l 27: a CCA or RDA would find significant relationships. . . 20. Sec 4.3: first paragraph unnecessary. 21. P 19 l 4: neither citation is in the references. Are you sure the Kortsch ref is appropriate?

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-537, 2018.