Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-339-RC3, 2020

Interactive comment on "Nordic Seas Acidification" *by* Filippa Fransner et al.

Anonymous Referee #3

Received and published: 3 November 2020

This is an interesting and ambitious manuscript with important goals. I commend the authors on their substantial efforts in synthesising all the varied data streams (real and modelled) to put together the trend analyses, projections, and regional maps. While these synthesis figures don't deliver revolutionary new insight in a purely academic sense, they are extremely important and highly sought after in more policy-oriented applications. These results are certainly worthy of eventual publication.

I write this having also read the two existing peer reviews of this manuscript. I agree with the concerns of the other reviewers that many results are presented with either no or insufficient quantification, and/or too vague or incomplete conceptual explanation. This is my main concern about the manuscript as it is.

This review is so brief because there are not many points left to make without simply repeating the thorough work of the other reviewers. Other than the main concern noted

above, I have only a few minor additions:

Abstract: is "window to the deep ocean" the right metaphor here? A place through which the deep ocean can be observed – is that the intended meaning?

Abstract: sensitivity to OA in the Nordic Seas is not directly due to high latitude, but rather due to low water temperature?

Sections 1 and 2, and probably also 3, are very much introductory material and I would also suggest to consider combining them, as mentioned by another reviewer.

In Section 2 and Table 1, an important aspect of discussion is absent, that is about the timescale of the effects shown in Table 1. Are you showing instantaneous effects of T/S/DIC/TA increases, or effects after CO_2 re-equilibration with a constant atmosphere? Looks like it's the former – is that really appropriate, given the context?

Methods: given the relatively low temperature of your observations, why not use the Sulpis et al. (2020) carbonic acid constant parameterisation for your CO_2 system calculations?

Throughout: pH is dimensionless; pH values do not need the word "units" after them, and $\times 10\text{-}3$ can be used in place of "mpH".

Line 205: "DIC also relates to salinity" is very vague, please explain the mechanism– including timescale considerations noted above for Section 2 / Table 1. See e.g. Wu et al. (2019).

Line 424 "both" implies two options when there are three (past, present and future). I am not sure that the chain of causality is properly represented in this and the subsequent sentences (i.e. which are drivers and which are responses in terms of air-sea CO_2 disequilibrium, pH change and hydrographic conditions), please be careful with the exact phrasing here.

The request for more research at the very end of the manuscript is very unspecific and is unexpected given that the rest of the paragraph implies that all the observed phenomena have indeed been explained here.

1 References

Sulpis, O., Lauvset, S. K., and Hagens, M.: Current estimates of K_1^* and K_2^* appear inconsistent with measured CO_2 system parameters in cold oceanic regions, Ocean Sci., 16, 847–862, https://doi.org/10.5194/os-16-847-2020, 2020.

Wu, Y., Hain, M. P., Humphreys, M. P., Hartman, S., and Tyrrell, T.: What drives the latitudinal gradient in open-ocean surface dissolved inorganic carbon concentration?, Biogeosciences, 16, 2661–2681, https://doi.org/10.5194/bg-16-2661-2019, 2019.

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