Responses to reviewer 3

We are grateful to reviewer 3 for all the work done. The comments are identifying weak points of the manuscript that will help us to improve it. Please find below our responses (in blue italics) to the comments (in black). References to line numbers, figures and tables refer to the revised manuscript, unless stated otherwise.

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?

With this phrase we wanted to refer to the strong connection between surface and deep waters through deep water formation. The introductory sentence of the abstract has been revised.

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

You are correct, the two introductory sentences of the abstract have been replaced with the following:"With prevailing low temperatures, deep winter mixing, and cold-water coral reefs, the Nordic Seas is vulnerable to ocean acidification." An explanation on the impact of low temperatures is given later in section 1.1.

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.

It is a good idea to do some merging of these sections. After some thinking, we ended up dividing the introduction into two sections; one general introduction on ocean acidification and the Nordic Seas, and one on "theoretical background".

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?

We agree that this is an important aspect that should be discussed. Table 1 shows the instantaneous effects without any gas exchange, we now make this clear in the label and when we refer to it. We think that it is important to show this rather than the effect after CO_2-re-equilibrium because the Nordic Seas surface waters are not equilibrated, and therefore somewhere in between the instantaneous and secondary effect. We now discuss this also in Section 3.1.

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?

The Sulpis et al (2020) paper was published in late July 2020, when all our analyses had been done, which explains why those parameterizations were not used. This could indeed be important for our estimates of the aragonite saturation horizon. But, looking at their figure 8, we see that the differences in the aragonite saturation state they get with the Sulphis parameterization and the Lueker parameterization, is about the same size as our uncertainty estimates (Table 2). These uncertainty estimates become even larger for past and future when taking into account the mapping error. This indicates that the results will not change substantially if switching between the two parameterizations. The Sulphis parameterization is however something that we will consider in our future work.

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

We have removed the "units" and replaced mpH with $\times 10^{-3}$ *.*

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).

This section has been strongly revised in the new version of the manuscript, and we now also discuss the importance of timescales. The phrase "DIC also relates to salinity" is no longer in use.

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.

In the revised manuscript we have put less focus on changes in pCO_2, and we have removed this paragraph. It is, indeed, not a driver of pH change, but just another indicator of a changing carbonate-system.

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.

We have removed this sentence.