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

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This manuscript uses observations and a model to assess the regional details of acidification of the Nordic Seas during the industrial era through to the end of this century. The authors find that during 1981-2019, the change in surface ocean pH is larger than would be expected from the corresponding change in atmospheric CO_2. They ascribe the cause to an evolution of surface ocean pCO_2, which while remaining undersaturated with respect to the atmosphere, increases at a rate faster than atmospheric pCO_2. They suggest that the main driver of the change in pH is the DIC increase associated with ocean uptake of anthropogenic CO_2. They also find that observed pH changes may be detected down to 2000 m in some parts of the Norwegian seas. The authors further focus on corresponding changes in the saturation state of waters with respect to aragonite and corresponding changes in the aragonite saturation horizon and what those changes may mean for cold water corals. In their model, most

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cold water corals would not be exposed to waters that are undersaturated with respect to aragonite under the low-end RCP2.6 and mid-range RCP4.5 emissions scenarios. But under the high-end RCP8.5 emissions scenario, most of those corals would be exposed to such conditions, which are are unfavorable for their long-term survival.

Overall the authors have addressed an important topic, the details of acidification of the Norwegian Seas, a regional focus that has not been addressed previously. They appear to have used all the best data available for this assessment, thanks to the many coauthors with observational expertise in the Norwegian Seas. Also included are coauthors who are experts in using the chosen model routinely to assess ocean acidification and related aspects of ocean biogeochemistry. The Abstract and Introduction (sections 1-3) generally establish the need for this study, the Methods section appears to provide sufficient detail except for the final subsection, and the Results and Discussion sections reveal much effort being devoted to the analysis.

Yet despite these positive aspects, there is also much room for improvement.

CONCERNS in order of importance

(1) Unfortunately, there seems to be a complete lack of understanding of what a pH change actually means. Although pH offers a convenient way to represent the hydrogen ion concentration, its log scale means a pH change actually represents a relative change in [H^+], not an absolute change (Kwiatkowski and Orr, 2018). That relative change is unlike the change in any other CO_2 system variable, all being absolute. Focusing only on pH and not [H^+] can give a completely wrong impression, e.g., as in this manuscript when it is used to compare changes at different depths and at different locations (Fassbender et al., 2020). Looking only at pH change, as in the manuscript, we cannot know what part of the change is due to a change in [H^+] and what part is actually due to differences in the reference [H^+], the starting point. The manuscript neglects this key point entirely, not even mentioning hydrogen ion concentration. Remedying this problem will require major revisions.

(2) Projections with only one model are unreliable. Model projections are hard to publish nowadays without using multiple models and for good reason. One model can give very different results from others. A range of models provides an estimate of model uncertainty, and the model mean typically performs better than any given model. Because the ocean component of the NorESM1-ME model relies on a dynamic isopycnic vertical coordinate, we might expect it to have very different results in simulated deep-ocean anthropogenic carbon concentrations relative to most other CMIP models. Modeling centers such as the one where some of the authors of this manuscript are associated seem to now have access to and experience working with the CMIP5 or CMIP6 models. All analyses in the current manuscript need NOT be repeated with all models. But it will be needed to show at least where the NorESM1-ME model is situated relative to other Earth system models, in terms of the depth distribution of anthropogenic carbon concentrations (and perhaps also [H^+] and Ω_{Ar}) in the different regions of the Norwegian Seas.

(3) The description of the decomposition of the drivers (namely the equations in section 4.4) is weak and incomplete.

a) Eq. (1) comes from Takahashi et al. (1993) and is fine except that the authors will need to replace the Greek delta δ with the correct partial sign ∂ in all the partial derivatives. This is not a major problem, just the convention of multivariate calculus. The δ is used for something else (inexact differential). Please don't confuse them.

b) Eq. (2) is added by the authors but is unnecessary. That equation comes from Metzl et al. (2010), who expanded each partial derivative in Eq. (1) to get at so-called "known quantities". Such complexity is no longer necessary because all of the partial derivatives in Eq. (1) are now easy available as precise quantities in "derivnum", an add-on package to CO2SYS-MATLAB (Orr et al., 2018). See

https://github.com/jamesorr/CO2SYS-MATLAB

The simpler choice, just deleting Eq. (2), is preferred and avoids unnecessary com-

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plexity that can lead to mistakes in implementation. For instance, the authors four definitions that immediately follow Eq. (2) are ambiguous because they are missing key parentheses. Hopefully their actual code is less ambiguous. There is no longer any need to introduce all these extra terms.

c) Eq. (3) should be recast in the same pattern as Eq. (1), i.e., replacing fCO_2 with $[H^+]$. It should not be cast in terms of pH (as in the current manuscript) for reasons mentioned in (1) above. The partial derivatives of $[H^+]$ are also available in derivnum. That routine is called with the same arguments as CO2SYS, with one argument added in the beginning to specify what the user wants to take partial derivatives with respect to. This further move towards simplicity will avoid the old-fashioned complexity that is now in the manuscript. Furthermore, this change will help avoid misinterpretation of what changes in pH mean.

d) An equation is missing in Section 4.4 concerning the freshwater Taylor-series decomposition, results of which are presented in Fig. S8. With that equation, the appropriate citations need to be given, starting with Lovenduski et al. (2007). For the associated salinity normalization, the authors must also specify their choices of the regional salinity references and if those remain constant or change with time. Furthermore, the authors will need to mention why they generally seem to prefer to use the older, less complicated decomposition from Takahashi et al. (1993).

e) Another equation is missing in Section 4.4 concerning what the authors call “pHperf”. Currently that term is mentioned in the short final paragraph of section 4.4, where the authors attempt to describe how they compute “the pH change in seawater that perfectly tracks atmospheric CO_2”. Unfortunately, the current description does not tell us exactly what the authors have done. For instance, in the calculation of pHperf, do the authors use *i)* the actual atmospheric pCO_2 as the reference value along with the atmospheric pCO_2 change or *ii)* the oceanic pCO_2 as the reference value, to which they add the change in atmospheric pCO_2?

The importance of this question is illustrated with a simple example. Suppose atmospheric pCO_2 is at 400 μatm and oceanic pCO_2 is at 300 μatm. Although a 1 μatm change in pCO_2 starting at 300 μatm produces only a 0.7% greater change in $[H^+]$ when compared to starting at 400 μatm, the corresponding change in pH is 30% greater in the former relative to the latter. The reason is that a change in pH represents a relative change in $[H^+]$, i.e., relative to the $[H^+]$ of the starting point. These numbers slightly depend on the other reference conditions, which I have arbitrarily set to T=2°C, S=35, ALK=2300 μmol/kg, nutrients=0. If the authors have used approach *(i)*, the results will be wrong. The authors should be able to resolve this issue by using approach *(ii)* and by adding an equation and improving the text to avoid ambiguities.

A related minor question: Do the authors actually use atmospheric xCO_2 (ppm) or do they first convert that to atmospheric pCO_2 (μatm), making corrections for water vapor pressure and atmospheric pressure?

(4) The section on cold-water corals is too cursory. The authors' analysis of the change in the aragonite saturation state to which cold-water corals are exposed has potential, but the authors devote only one rather short paragraph to describing the results, which are presented in one figure. They authors also neglect to clearly attribute previous studies that have attempted the same type of exercise using model projections and cold-water coral positions. Additionally, the data set used in the manuscript for coral positions is not cited adequately, and the authors do not give enough information about their procedure for extracting the saturation state from the model. For instance, is the model sampled at the depth of the coral (as provided in the data base) or is the depth taken to be that of the model's bottom depth at a coral's latitude and longitude? More discussion of results and the addition of uncertainties from a multi-model analysis would seem critical.

(5) The writing needs improvement. Getting through this manuscript was not easy. Although there are few if any errors in English, and individual sentences generally work well, the manuscript would benefit if the authors could redouble their efforts to improve

flow between sentences. That is, connections between sentences are often rough, causing the reader to slow down and sometimes stop. Also lacking is coherence in many individual paragraphs. My recommendation would be for the authors to consult the book by J. M. Williams (Style The Basics of Clarity and Grace), and in particular the short chapter on Cohesion and Coherence. Then they could go through the manuscript trying to improve both aspects. If one cannot borrow this book from a library or colleague, older editions only cost about 10 euros. It offers the potential to dramatically improve one's writing by applyng a few basic principles.

(6) The figures need improvements. Some figures appear to have too many panels, some figures should be combined, and some figures should be deleted. There are also other issues.

a) In Fig. S6, it seems that only 3 out of the 6 regions seem to show a trend in surface ocean pCO_2 that is significantly greater (statistically speaking) than that of atmospheric pCO_2. Thus I am unconvinced by the statement that it is only the Barents Sea Opening does not follow this pattern. More care is needed when handling this subject in the revised manuscript.

b) In Figs. 3, 7, and 9, the third row of maps for Ω_{Ca} should be deleted because it exhibits the same patterns as for Ω_{Ar} in the second row, only differing by a constant. Their constant relationship could be briefly mentioned once in the text rather than wasting space in each of those three figures. Likewise, Fig. S5 for Ω_{Ca} should be deleted because it shows exactly the same patterns as Ω_{Ar} in Fig. 6.

c) In Figs. 5 and 6, the numbers given in each panel for the slope and uncertainty should be moved to a table, where it will be easier to compare numbers between regions and depth layers. The same goes for the corresponding supplementary figures (Figs S1-S4). In addition, there are often too many significant figures in the slope and uncertainty, and the number of digits is not always consistent. Furthermore, in those same supplementary figures, the slopes have the wrong units. In regards to these and other figures, when statistical significance is mentioned in the text, that should be backed up with a statement of how it was determined. Such is not the currently the case in the manuscript, but it is critical, e.g., when discussing if oceanic pCO_2 is increasing more rapidly than atmospheric pCO_2 (Fig. S6).

d) Figs. 8 and 10 should be combined.

e) Fig. 11 includes some details that might need to be deleted, and corresponding supplementary figures should also be refined. What is the rationale for including the dashed line and black stars in subsurface layers? Those layers have been isolated from the atmosphere for some time and we would not expect them to track atmospheric CO_2. Showing these details in subsurface layers will confuse the reader. Moreover, would it not be better to devote a separate figure just to the subject of ocean pCO_2 tracking atmospheric CO_2 rather than trying to squeeze that information here into a very small space? Fig. S6 fills this need well. That could be brought up into the main paper. Only the top level (0-200 m) of Fig. S6 would need to be shown as we do not expect subsurface levels to track current levels of atmospheric CO_2. I also worry about how representative the 0-200 m layer is of surface ocean pCO_2. Some discussion on that and perhaps a modified figure seems necessary.

In corresponding supplementary figures for the model (Figs. S9-S11), the authors miss the opportunity to compare the model results over the same 1981-2019 period as used for the model. By the way, why is this time span often referred to in the text as lasting 40 years; actually, it lasts only 39 years. My impression is that relative to the observations, the model is dominated even more by the change in DIC, based on the analogous plots for the previous and subsequent time periods. These supplementary figures concern the model, but readers will be confused because 'OBS' is used to designate the model result, both in the caption and in the figure itself. Please change 'OBS' to 'MOD'.

f) Fig. 12 has too much white space.

g) The supplementary figures are mentioned out of order.

These are issues that I hope the authors will be able to address with major revisions to the current manuscript. I look forward to the opportunity of seeing a revised manuscript.

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