

## ***Interactive comment on “Regional variability of acidification in the Arctic: a sea of contrasts” by E. E. Popova et al.***

**Anonymous Referee #1**

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Popova et al. use an ocean-only general circulation model including an ecosystem module to project future ocean acidification (OA) in the Arctic Ocean under the RCP8.5 scenario. They focus on the regional variability of OA and on the effect of climate change on OA. Their main conclusions are that spatial variability is large and that climate change effects play an important role in driving this spatial heterogeneity.

The general topic of this manuscript is relevant and interesting but I have a number of major concerns regarding the applied methodology and the presentation of the results. Some of these issues are fundamental and critical so that I don't think this manuscript is suited for publication in Biogeosciences. Unless the authors can show that they are able to comprehensively address the fundamental issues described below, I recommend the rejection of this manuscript.

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Major concerns:

1. The method of separating climate change effects on OA from the direct geochemical effect of CO<sub>2</sub> uptake is fundamentally flawed. The authors claim to separate the impacts of raising atmospheric CO<sub>2</sub> from associated future climate change with a simulation where atmospheric CO<sub>2</sub> is held constant at the year 2000 value. This does not work because atmosphere and ocean are not in equilibrium by the year 2000. Therefore, the ocean will continue to take up carbon for a long time after the year 2000 until a new equilibrium is established. This contribution is included in the "climate" simulation is thus wrongly attributed to the climate change effect. Further, the choice of the year 2000 is arbitrary and not justified. If CO<sub>2</sub> was held constant at e.g 1950 or 2010 levels the results would be different. A better approach would be to keep atmospheric CO<sub>2</sub> constant at pre-industrial levels for which we can assume that atmosphere and ocean are approximately in equilibrium. Ideally, an additional third simulation should be performed, where only the biogeochemistry is affected by the CO<sub>2</sub> increase and the radiative effects are suppressed (constant climate). This third simulations could be used to check for consistency between the approaches and to address potential non-linearities in the response.

2. In contrast to recent studies of this type, the authors use an ocean-only model driven with output from a coupled Earth System Model (ESM). This is problematic because with this "offline" model there is no interaction between the simulated marine carbon cycle and the prescribed atmospheric CO<sub>2</sub>, that depends on the ocean and terrestrial carbon cycle response in the ESM simulation. This means that there are no feedbacks included and that the prescribed atmospheric CO<sub>2</sub> concentration is decoupled from the response of the ocean-only model. Thus, the simulated changes in ocean chemistry are not necessarily consistent with the other components of the global carbon cycle. Consequently, this means also that the simulated changes are not necessarily consistent with the prescribed climate change (see also specific comments below). For a study of this type that focuses on the interaction of climate change and ocean acidifica-

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tion, I would have expected that a fully coupled model is applied, or, at the very least, the implications of not doing so is discussed with the help of a sensitivity analysis or the like. It should also be stated more clearly and prominently (in the abstract) that the authors use an ocean-only model and not a fully-coupled interactive model.

3. Much of the analysis regarding drivers of OA presented here is qualitative and therefore somewhat speculative. The statement "The main mitigating factors in areas such as Arctic Ocean are the stabilisation of stratification and the increase in the net primary production." (P.2948 L.3), for example, is indeed plausible but a quantitative analysis is lacking. How much of the changes in pH and Omega can be attributed to changes in stratification and NPP? Is this a relevant contribution? These questions are not answered in the present study. Another example is the statement "...because increased fresh water input is largely compensated for by elevated rates of the net primary production." (P.2951 L.3). The authors don't provide quantitative support for this statement either. In order to do this one would have to quantify the effects of gas exchange, carbon export, freshwater input, NPP etc. on the carbonate chemistry separately (see e.g. Yamamoto et al. 2012, Biogeosci. or Steinacher et al. 2009, Biogeosci.). This is not done in the present study and a robust quantitative attribution of chemistry changes to driving factors is therefore not possible.

4. The authors address regional variability in the Arctic but do not mention temporal variability. Previous model intercomparison studies suggest that different models show quite distinct responses in terms of seasonal variability. I think it would be good to also discuss seasonal and interannual variability in the context of this paper, particularly with respect to the occurrence of monthly mean undersaturation presented e.g. in Fig. 5.

5. The description of the applied methodology is not sufficient. There are conflicting statements and generally there is too little information on the modeling approach and on the separation of climate and geochemical effects (see specific comments below).

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6. Some problematic terms are used throughout the manuscript:

(i) The authors use terms as "the model predicts" or "the predicted onset". I find this problematic and I suggest to change "predict" to "project" because the results depend on the chosen emission scenario. See e.g. [http://www.wmo.int/pages/themes/climate/climate\\_projections.php](http://www.wmo.int/pages/themes/climate/climate_projections.php) for more information on the distinction between 'projection' and 'prediction'.

(ii) The authors use the term "climate change feedbacks" to describe the effect of climate change on ocean acidification. I don't think this term is appropriate because I fail to see where the feedback loop is. Increasing CO<sub>2</sub> alters ocean chemistry and affects climate. Changing climate may have an effect on ocean acidification but I don't see where ocean acidification feeds back to atmospheric CO<sub>2</sub> or climate change (at least in the setup of this study). I therefore suggest to use terms like "effect of climate change" or "is modified/amplified/attenuated by climate change" in order to avoid the term "feedbacks".

Specific comments:

P 2938, L 2: "...negatively impacting...": This is still ongoing research. I suggest to formulate this a bit more carefully e.g. "...with potentially negative consequences for...".

P 2938, L 5 and P2941, L 2: "full description of the carbon cycle": I don't think "full" is appropriate here. A model is by definition not a full or complete representation of nature. Please change this to "comprehensive description" or something similar.

P 2938, L 6: "response of...to changing climate in the Arctic Ocean.": More importantly it is in response to increasing atm. CO<sub>2</sub>, which is missing in this statement.

P 2938, L 10: Strictly speaking, RCP8.5 is a concentration pathway and not an emission scenario, i.e. the radiative forcing (GHG concentrations and other agents) define the scenario and not the emissions.

P 2938, L 13: Typo: "ocean-atmosphera" => "ocean-atmosphere" P 2938, L 14: "was

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prevented from increasing beyond the flux calculated for year 2000.": This is first a complicated formulation, and secondly not consistent with the description at P. 2941 L. 10. Setting an upper limit for the ocean-atmosphere flux as described in the abstract is not the same as holding constant atmospheric CO<sub>2</sub> as mentioned in the introduction.

P 2938, L 15 and throughout the manuscript: "climate feedbacks" is not appropriate (see major comment 6 above).

P 2938, L 22 and throughout the manuscript: "predicted" is not appropriate (see major comment 6 above).

P 2938, L 22: Undersaturation with respect to aragonite or calcite?

P 2940, L 4: I don't think that Houghton et al. (2001) is an appropriate reference for projected atm. CO<sub>2</sub> on CURRENT emission rates.

P 2940, L 27 and P 2941, L2: The abbreviation GCM is often used for Atmosphere-Ocean General Circulation Models. OGCM should be used here to clearly indicate that an ocean-only model is applied here.

P 2939, L 30 and throughout the manuscript: Please consider changing the abbreviation for dissolved inorganic carbon from CT to DIC, which is more commonly used.

P 2941, L 3: Are concentrations or emissions from RCP8.5 prescribed? Please include more information about the model forcing in the method section.

P 2941, L 10: This method of separating "CO<sub>2</sub> and climate" should be described in more detail and justified in the methods section.

P 2941, L 20: Please provide more information how the ocean model is forced. Which output from the ESM is used exactly? Is atmospheric CO<sub>2</sub> simply prescribed from the ESM output or is it also affected by the atmosphere-ocean fluxes simulated by NEMO? In either case the atmospheric CO<sub>2</sub> is not necessarily consistent with the simulated climate change (if NEMO affects atm. CO<sub>2</sub>) or ocean chemistry (if it does not). This

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caveat should be discussed. What are the sources of freshwater input? How are carbon content and alkalinity of river runoff calculated or specified? Does the ESM account for glacier melt?

P 2944, L 1: "CO<sub>2</sub> in the model reaches 950 ppm": In which model? Again it is not clear if this concentration is simulated by the ESM only or if the ocean model sees a different atmospheric CO<sub>2</sub>. It should be clearly stated if the given CO<sub>2</sub> concentration is consistent with the simulated SST also mentioned here.

P 2943, L 19: "accumulation of melt water": How do you separate melt water from precipitation or riverine input? Are melting glaciers also considered or just sea ice?

P 2943, L 28: "exchange with deeper layers with ... low Omega.": Is Omega really lower in deeper layers? There are several locations in the Arctic where the saturation state is lower at the surface than at 100-1000 meters below (Jutterström & Anderson, 2005, Mar. Chem.). Particularly at sites that are affected from freshwater input at the surface, mixing from deeper water masses with higher Omega would therefore potentially increase the saturation state. P 2944, L 14-16: The numbers given here are based on a method that is flawed (see major concern 1).

P 2945, L 12: I think this should read Fig. 3f instead of Fig. 4f.

P 2945, L 18: "As such, it is crucial to correctly estimate the time...": This sentence suggests that an argument is made why it is crucial to correctly estimate the onset of undersaturation. I, however, fail to find this argument.

P 2946, L 20-25: Please add references to support this statement, i.e. climate change leading to a reduction in nutrient supply for NPP due to increased stratification.

P 2949, L 19: I think Steinacher et al. (2009, Biogeosci.) should also be added to this list.

P 2951, L 19: "As things stand today, observed emissions are already reaching this level, suggesting the possibility that future emissions[...] could be even higher.": This is

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speculative and I fail to understand the line of argument here. Future CO<sub>2</sub> emissions depend mainly on future mitigation actions, technological and economical development, etc., and not on current emission rates. I suggest to delete this sentence, which is only marginally relevant for the presented analysis.

P 2952, L 1: This result is not consistent with McNeil and Matear (2008, PNAS) and should therefore be discussed in the light of their analysis.

P 2952, L 3: "global ocean circulation model [...] forced by the RCP 8.5 emission scenario": The model is not directly forced with the emission scenario. It requires an intermediate step with a coupled ESM. This is not clear here.

Fig 1: - Please indicate which pCO<sub>2</sub> is shown in panel a). Is it the pCO<sub>2</sub> seen by the ocean-model? Is it the same for the carbon-cycle and the radiative forcing in the ESM? - The title of panel c) is not consistent with the figure caption (ice extent vs. ice volume). Please explain what the two lines in panel c) indicate. Are really monthly-mean values shown in panel c)? At least for sea-ice extent I would expect a larger seasonal amplitude. - Please indicate in the caption what the subscript "max" means for DIN<sub>max</sub> in panel e)? - The units of panel f) are not consistent with the figure caption (GtC/yr vs. gC/m<sup>2</sup>/yr).

Fig 2: - Please describe in the caption what the red line indicates. - The difference in panel c) is only shown where the ice concentration is non-zero by 2099. The difference should include all areas that have ice cover in 2000. - I think a different color bar (e.g. bright colors at 0 to darker colors at -1) for the difference would help in terms of readability. Maybe it would also be better to show the difference as percentage of the initial ice concentration.

Fig. 3: - Again, a different color bar for the difference plots (e.g. white centered with red (positive) and blue (negative)) would help in terms of readability. This applies also to the other figures.

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Fig. 4: - Panels a/b, e/f, i/j should use the same scale to be comparable.

Fig. 5: It is not clear whether the white areas indicate waters that were already undersaturated before 2000 (I assume this is the case in the deep ocean) or remain supersaturated up to 2100 (calcite in the North Atlantic). I suggest to add a color to indicate undersaturation before 2000 A.D.

Fig. 7: These panels are very small and the space between the panels is large. Consider increasing the size of the panels by reducing the gaps between them.

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Interactive comment on Biogeosciences Discuss., 10, 2937, 2013.

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