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Comment

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

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We are grateful to Referee 1 for a thorough review of our manuscript and for the important points raised about our study. For the purposes of this initial response we would like to focus on the six Major Issues listed in the review (we agree with all Specific Comments raised by the Referee and will address them in our revised manuscript).

We are confident that Issues 4-6 can be addressed in a reasonably straightforward way by sorting out terminology, improving description of the methodology used and by performing additional analysis of seasonality in our results.

However, the best way to respond to Issues 1-3 is less straightforward and will have to involve additional runs which will potentially take us to the limit of our computational

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capabilities. As such, we would like to use the opportunity provided by the EGU Open Discussion process to post our plans for additional experiments in order that Referee 1 may be able to comment on these before we embark on a major computational and analytical task.

Issue 1. Fixing pCO₂ at year 2000 value does not separate climate change effect as the system is already out of equilibrium and the ocean continues to take up atmospheric CO₂.

On reflection, we would agree with the Referee's comment, and with the suggestion that a better approach to separate impacts would be to fix pCO₂ at the preindustrial level (1860 in this simulation framework). In addition to this simulation, we will run the third suggested sensitivity run with a constant climate but increasing pCO₂.

Issue 2. The problem could have been better addressed with the fully coupled ESM.

We agree with this comment in general, since only coupled ESMs offer the potential for representing climate feedbacks. Nonetheless, ocean-only models are still a valuable tool, largely because their computational burdens are typically lower than those of ESMs. For instance, they can be run at a higher resolution than ESMs, which can be essential for relatively small regions such as the Arctic that are heavily driven by physics. Furthermore, such models can afford a more detailed representation of ocean processes, as well as more numerical experiments that serve to improve model performance. Because of such constraints, here we trade consistency of the simulation for a better representation of the ocean physics. Neither of the two approaches is perfect, and we would be hesitant to dismiss ocean-only models in favour of more computationally expensive and (typically) lower resolution ESMs. However, we would still fully agree with the Referee's general view, and will amend the manuscript so that the limitations of our chosen approach are stated more clearly, including in the abstract and discussion.

Issue 3. The attribution of chemistry changes to the driving factors is mostly qualitative.

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This is the most difficult issue to address. In the papers by Steinacher et al., and Yamamoto et al., the budget was calculated for the Arctic Ocean as a whole which simplified treatment of the freshwater input from the rivers and melting sea-ice. In the context of our paper, such an approach will not suffice as it is regional differences that are being analysed. As such, to provide quantitative estimates for our discussion we propose to construct spatially varying budgets for all horizontal grid points for a “surface” layer of certain depth, in particular separating vertical diffusivity. How meaningful such a budget can be and what are the error bars arising from approximations is a priority uncertain and will be a subject of investigation.

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