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Ocean Science
Discussions

Interactive comment on "Influence of initial stratification, wind and sea ice on the modelled oceanic circulation in Nares Strait, northwest Greenland" *by* Lovisa Waldrop Bergman and Céline Heuzé

Anonymous Referee #1

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This manuscript describes a superficial and failed ocean circulation modeling exercise for a region to the west of Greenland where thick multi-year sea ice is land-fast for most of the year in most years.

More successful, comprehensive, and credible models of ocean-ice interactions in Nares Strait already exist in the peer-reviewed literature, e.g., Shroyer et al. (2015, 2017) that are referenced by the authors and three more that are not, e.g.

1. Rasmussen et al. (2010, 2011) in Ocean Modeling and Atmosphere-Oceans from a

dissertation at Copenhagen University, e.g.

http://www.nbi.ku.dk/english/research/phd_theses/phd_theses_2010/till_rasmussen/Till_Anc

2. Wekerle et al. (2013) in J. Geophys. Res. from a dissertation at the University of Bremen, Germany, e.g.,

https://epic.awi.de/34179/1/Dissertation_Wekerle_Oct2013.pdf

3. Grivault et al (2018) in J. Geophys. Res. from a dissertation at the University of Alberta, Canada.

The present manuscript does not contain a single result or conclusion that is not already included in the above body of literature. Furthermore, the present work confuses many issues that are well observed and documented. Let me focus on just two major short-comings even though there are many more. These relate to ocean volume flux and sea ice implementations that perhaps explain why model results fail to relate to ocean or sea ice physics in Nares Strait:

The authors correctly observe that the volume flux through Nares Strait ranges from 0.71 to 1.03 Sv (interannual averages), but they discard these estimates as too uncertain, because of a short 6-year record. Yet, the numerical experiments are run for only 274 days long (page-2, line-27) and use a volume flux of 0.2 Sv (page-5, line-28) that is imposed via an upstream boundary condition. To me this contradiction explains why model results do not compare against from many years of mooring data. Failing to take this as an indication of model failure, the authors merely state that "we could not reproduce the results of Rabe et al. (2012) who found that the geostrophic velocity in Nares Strait varies with the ice state ... because our model became unstable when we attempted to include thick sea ice." (page-9, line-29)

This brings me the second major and fatal failure of this manuscript that relates to the sea ice as modeled and observed. It is well known and documented for 140 years that (a) Nares Strait is a conduit by which some of thickest Arctic sea ice leaves the

Arctic Ocean and (b) the sea ice cover is immobile for ∼9 month a year in most years, see Kwok (2005, 2009) and Ryan and Muenchow (2017) for observational details from decadal data sets on sea ice coverage, mobility, and thickness. And yet, the authors are forced to run their models with ice initially between 0 and 1 m thin, because "... tests with higher values created unrealistic sea ice built up and caused the model to become computationally unstable." (page-3, line-21). A similar argument may apply also to sea ice velocities (page-3, line-15) that are kept artificially low. As a reviewer, I conclude that the sea ice model does not work in the present implementation. Prior ice-ocean models of Nares Strait listed above did not have this problem and compare more favorable to observations both qualitatively and quantitatively. Perhaps the authors should consult with sea-ice modelers before they release and distribute potentially faulty model results.

I have many additional issues with this manuscript that all relate to a naive reading, discussion, and understanding of the published literature (modeling and observations). The manuscript speculates excessively without much insight in either ocean or ice physics to "explain" model failures that are not recognized as such. One example of such speculation relates to the freshwater discharge through Nares Strait. On page-1, line-4 a 50% increase from 40 to 60 mSv (40,000 to 60,000 mˆ3/s) for the entire Canadian Archipelago, published values and time series exist for Nares Strait from moored instrumentation which vary from 40 to 50 mSv on average. Basal melting below Petermann Glacier is mentioned (page-2, line-7) as an additional source of freshwater flux that is not included in any models, however, the contribution of this large outlet glacier is about 2 mSv which is negligible relative to the 40-60 mSv freshwater flux.

There are too many (11) model experiments without a single one properly explained at the level that allows proper review. It appears that the boundary conditions control everything, yet pressure is never mentioned even though it may impose a dominant forcing as it does in observations. How do you define a volume flux (mˆ3/s) at a point as done in Figure-4. Salinity errors shown in Figure-10 frequently reach 0.5 to 1.5

psu within the top 30-50 m of the water column in (almost) all model experiments. Is it possible that mixed layer observations of active ice melt in the summer of 2015 are represented poorly by the model? Does the model ever have time to adjust to a steady state? Why use static metrics such as freshwater and heat content when more meaningful dynamic metrics such as freshwater and heat flux are available from published mooring observations?

I recommend to reject this manuscript without possibility of a resubmission, because excessive model instabilities, inconsistencies, and errors are more likely to explain discrepancies between prior published model results and observations that new physics. The impact of this manuscript on the community of oceanographers is negative as it may set a new low bar on the smallest publishable unit.

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