

Interactive comment on “Drivers for Atlantic-origin waters abutting Greenland” by Laura C. Gillard et al.

Anonymous Referee #2

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I think the set of experiments that were carried out in this study is very nice and that it could potentially be useful to understand the waters surrounding Greenland and what mechanisms are important for changes in troughs around Greenland. However the way this paper is presented resembles a bit an observational study where one is limited to a few hydrographic sections and needs to hypothesize of why some of these observations might be what they are. There is no attempt to understand the reported results and to explain mechanisms that are responsible for these results, at least within the context of this model. One example is the seasonality in heat flux - we see plots of seasonal signal from different sections, but there is no advance in understanding why this seasonal signal is there. There is also no validation of this model (there are a few hand wavy remarks of how something agrees, but no actual comparison). An-

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other example is weak EKE in the NE and NW - it is hypothesized that it might be because of sea ice - but there is no attempt to quantify this in any way. It feels as if the authors forgot they have the whole 4-D model output that probably contains the answers. In summary, I think these experiments suggest that responses to different forcing changes around Greenland might be regionally different, and that is useful (and it would be more useful if there was some model validation with observations). However, this paper could do much deeper than that, and actually try to help understand reasons for these differences. One option it would be to focus on less experiments in a bit more depth.

Some (still major) comments are listed here, the more detailed comments are in the attached pdf.

1) Model

*There is essentially no model validation. While there is a very nice description of the regional oceanography in the introduction, the reader is left wondering what the actual ocean circulation and hydrography looked like in this model.

*There is no justification for the spin up time, which was seemingly ad-hoc chosen to be 2-years. A lot of emphasis in the discussion is on 2004-2007 increase of oceanic heat flux towards Disko Bay. However since only first 2 years of the simulation were discarded - the first analyzed year is 2004, the year on which so much emphasis is placed.

*There is no discussion of the sensitivity of the offline Lagrangian particle tracking to the time step used for the integration. It seems that the integration was carried out on the 5-day model output which seems large (e.g. Koszalka 2013 (JPO) found that for a 2km horizontal resolution they need a 15 min time step).

*There is no discussion of how modeling choices may affect the results, e.g. the way meltwater is introduced.

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2) Description of experiments

*The experiments need to be described more clearly. At this point there is introduction, pieces of experiment setup, and discussion all mixed up. I would suggest to first describe the LowRES in full detail. Then go to individual experiments. Say what question the experiment attempts to answer. Review the aspect of the base case that you will change. Then describe how the change will be carried out. And also how the evaluation of differences will be done.

*It is not obvious that downscaling the HighRes simulation will reduce to LowRes simulation, because the geometry was generated using different approaches. If that is not the case, this may have implications for the comparison of the two simulations, when the effect of horizontal resolution is explored.

3) Organization

*Background is mixed up in results and introduction. It would be useful to include a background section before the results, that describes the three sectors that the analysis will be focused on.

4) Figures vs. their description

*I found myself disagreeing to some extent with the description of the shown figures enough times to bring it up in the major issues (examples in the minor comments). I would suggest the authors to make sure that the text and the figures are consistent.

5) References As far as I can tell, some of the references in the paper are attributed to what the referenced work actually shows and discusses. Examples are in attached pdf.

6) Section 3.2 shows there is a seasonal variation of heat flux across the selected section, but it does not provide any explanation of how this seasonality is introduced. As far as I am concerned the claim that it has to do with distance from source (it is not actually specified here what the source is) is sound, however there is no analysis at all

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in the manuscript, that would support this claim. Some questions that I think would be worth investigating are:

How is the seasonality introduced in the water properties? Is it by the same mechanism at each location?

Does the model show a propagation of seasonal temperature signal?

Is it possible that the seasonal changes are due to seasonal changes in the water flow and not due to temperature? Can you separate these effects?

You have a full model of this region, not just a few observations, so it seems that you should be able to explore the observed seasonality in more depth.

7) Section 3.3

This whole section that discusses contribution of the mean flow vs the fluctuation could be significantly improved. The discussion is qualitative now, but it could be made quantitative. Rather than saying what is more or less important there could be some metric used to measure how important each component is and by how much.

8) Warming vs. increased heat flux, warm water vs Irminger water

There needs to be consistency in the terminology used here. Irminger water is never defined in the manuscript, but it is referred to all the time. Sometimes warm water is interchangeably used with Irminger water, but for example in the backward particle tracking particles are released below 30 m depth - so water that is not Irminger water is being traced as well.

There is a lot of mentioning of warming, when what is shown is increased heat flux - and that can increase even if no actual water mass increased its temperature. It would be good to be careful with terminology and interpretation again here.

Please also note the supplement to this comment:

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<https://www.the-cryosphere-discuss.net/tc-2019-84/tc-2019-84-RC2-supplement.pdf>

Interactive comment on *The Cryosphere Discuss.*, <https://doi.org/10.5194/tc-2019-84>, 2019.