Ocean Science
Discussions

Open Access

EGU

Interactive comment on "Numerical investigation of the Arctic ice-ocean boundary layer; implications for air-sea gas fluxes" *by* A. Bigdeli et al.

Anonymous Referee #1

Received and published: 7 September 2016

This paper is interested in the ice ocean boundary layer. The long term goal is to focus on air-sea gas fluxes. These air-sea fluxes depend on water column properties, and their history. Given that such data is often lacking, the authors examine if they can use output from a coarse resolution numerical models. Air-sea fluxes are not estimated, but instead the authors compare model fields of sea ice, upper ocean properties and mixed layer depth with observations, such as from ice tethered profilers. The analysis shows the ice concentration and velocities from the model compare well with the observations. For the mixed layer depth, broad trends are comparable. But significant biases in upper ocean properties and velocities are found.

The underlying issue for this paper, determining if model fields can be used in the

estimation of air-sea gas fluxes is interesting and important. However, I don't believe the present paper does a good job of answering the question posed. Rather than taking the output from the latest and most advanced ocean general circulation models, a number of which have been extensively evaluated in the Arctic, the authors try to set up and run their own coarse resolution simulations. Beyond some specific issues about the author's experiments and analysis, I don't see the purpose of these stand-alone experiments. Given the groups doing high resolution ocean modelling have the spent the time improving and evaluating their models, I don't see why such fields aren't being obtained and considered for this analysis. Additionally, since any given model has issues, doing this analysis with an ensemble of models would give much better confidence for the observational comparisons and thus the utility of numerical model output in calculating air-sea gas fluxes.

Additionally, this is sloppily prepared manuscript. Almost all the figures are mis-numbered in the text. Most of the figure captions are too brief and lack the detail needed to clearly explain all aspects presented. The details on the figures are also not well explained in the text. A number of figures have spelling errors on the axes labels. 32 figures are probably too many as well. Also, there are some technical issues with respect to the use and analysis of the numerical model output.

Thus I can't recommend that this paper be accepted in its present form. I would suggest that the authors get high resolution model output from colleagues (ideally more than one model) and then repeat their analysis with that output. Then I think the authors may have a really interesting paper, that will be really useful for the community.

I won't bother with text comments given that text will significantly change if the paper is revised. Instead I will provide a few additional specific comments below that the authors may find useful in thinking about the topic and revising their manuscript.

Introduction: With the later focus on the Beaufort Gyre, it needs to be discussed (especially the observations that will later be used) in the introduction. The reader has

otherwise no idea of the geographical focus until much later in the paper.

Page 3, Line 1: In areas of deep convection, the mixed layer can change by much more than a factor of 2 over several weeks.

Page 3, Line 17: Why do you need to use output from a model run on a desktop? Being able to run a model is a very different task compared to running a model to produce high quality evaluated output. Just take the fields from those produced by modelling groups.

Page 4, line 14: Era40 is the output from an atmospheric reanalysis. This is not the same as a numerical weather forecast.

Page 4, Line 19: Most high resolution models now use vertical resolution of 1-3 m near the surface.

Page 10, Line 8, etc.: Do not understand how the MLD could be zero. Model tracer points are in the middle of the grid cell for the MIT model. Thus, I don't see how the MLD could less than the depth of the first layer, given M1 is based on a threshold approach.

Page 10, line 11: Might it not be useful to use a simple 1-D mixed layer model to examine directly the impact of the forcing.

Figures 26-32: I'm surprised the model and ITP velocities differ by so much. Have the model velocities been rotated from the model grid to latitude-longitude?

————————————————————

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-4, 2016.