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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 #2

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The aim of the paper is to examine if the mixed layer depth from the regional ocean model configurations of Arctic is accurate enough for the estimation of the air-sea gas exchange rates. However, the authors spend majority of paper evaluating the sea-ice velocity, sea-ice concentration and temperature and salinity profiles against observations from satellite, moorings and ice-tethered profilers. I found that the motivation and some of contents in the paper are not aligned. I would not accept the paper for publication in its present form. The paper needs to be re-organized to address the following issues:

1. The motivation of the paper is to estimate the mixed layer depth from the ocean model; however, the following sections do not seem to have any connections to the

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Discussion paper

mixed layer depth estimation: 3.3.1 Geopotential height, 3.31 Vertical salinity temperature profiles and 3.6 Circulation and etc. This makes the interpretation of this paper difficult and results in many figures, which are not labeled correctly. For example, line 20 describes the sea-ice trajectory, but Fig. 10 contains color plot of geopotential and arrows, which are not described. Line 25 describes the geopotential, yet the referred figure 12 does not contain any information on the geopotential. I would keep the section 3.5 Mixed layer depth and omit 3.3.1, 3.6 and 3.7 unless there are clear connections to the change in the mixed layer depth.

2. The paper claims in the abstract "Overall, we find the course resolution model to be an inadequate surrogate for sparse data, however the simulation results are a slight improvement over several of the simplifying assumptions that are often made when surface ocean geochemistry, including the use of a constant mixed layer depth and a velocity profile that is purely wind-driven." I agree with the first claim "find the course resolution model to be an inadequate surrogate for sparse data"; however I do not see where the second claim, "the simulation results are a slight improvement over several of the simplifying assumptions that are often made when surface ocean geochemistry", is supported in the paper. The authors need to show the rate of air-sea gas exchange calculated from the mixed layer depth from 1) the ocean model results and 2) the conventional method, and discuss the difference in the estimates.

3. I am concerned with the use of salt plume parameterization in the model configuration, A1. The A1 has relatively high vertical resolution of 2 m down to upper 50 m, whereas the salt plume parameterization is tested for models having the vertical resolution of $\sim$10 m. The salt plume parameterization is known to suppress the mixing to maintain a reasonable mixed layer depth in the polar regions. The parameterization is meant for a coarse vertical resolution model ($\sim$10 m). The vertical resolution in A1 is 5 times higher, and I am concerned that the parameterization in A1 is over suppressing the mixed layer depth. In fact, Figs 11, 13 20, 21 all points to the underestimation of the mixed layer depth compared to the observations. I suggest switching off the salt plume

parameterization and evaluating the mixed layer depth in comparison to the existing model results and observations.

Minor comments

Line 10-15: It is unclear if the lateral boundary conditions are prescribed from the climatology or seasonal output from ECCO2 and JRA25. It is also unclear why the authors use output from two different models ECCO2 and JRA25. Map of the domain showing bathymetry will be helpful.

Equation 3: What is the definition of i?

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