

Geosci. Model Dev. Discuss., referee comment RC2
<https://doi.org/10.5194/gmd-2020-421-RC2>, 2021
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Comment on gmd-2020-421

Anonymous Referee #2

Referee comment on "Development of adjoint-based ocean state estimation for the Amundsen and Bellingshausen Seas and ice shelf cavities using MITgcm/ECCO" by Yoshihiro Nakayama et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-421-RC2>, 2021

This manuscript presents the results from the adjoint-based data assimilation/ocean state estimation for the Amundsen and the Bellingshausen Sea where ice-shelf-ocean interactions are important. The strength of this work lies in that it is the first to attempt the data assimilation in the regions since the similar effort using Green's functions, by overcoming the limitation of this low-dimensional estimation approach. The manuscript is generally well-written and represents a substantial contribution to the modeling communities. However, my first major concern is that despite the importance of serving as the first adjoint-based state estimation effort in these regions, the authors did not go into much detail on model skill assessment, which appears rather descriptive by simply comparing the model outputs to observations. Other major comments are concerned with the lack of the discussion of optimized parameters and sensitivity experiments to varying model initial conditions/parameter guesses.

1. Model skill assessment

Model skill is discussed with regard to the model-observations misfits and the model improvement capturing key oceanographic characteristics by the optimized simulation compared to the initial (unoptimized) simulation. However, much of this discussion appear descriptive, and I would suggest that the authors calculate a series of skill metrics for a more quantitative model skill assessment. For example, what is the formula of the model cost function, and how were the components (e.g., means, weights, standard deviations of the assimilated data types) calculated within? I would recommend that the authors examine the univariate model metrics (e.g., r , RMSD, the reliability index, the average error or bias, the average absolute error, and the modeling efficiency; in Stow et al. 2009, J. Mar. Sys. 76:4-15) and add the Taylor diagrams showing r , RMSD, and the normalized standard deviation, before and after optimization (one set based on the unoptimized simulation and the other based on the final optimized simulation).

2. Discussion of optimized parameters

It is nice to see the in-depth discussion of possible causes of model-observation mismatch, the limitation of the adjoint-optimization methodology, and the utilization of limited

observational data. As the authors mentioned in the manuscript, I see that the important contribution of this work to the broad modeling community is to provide a better set of model parameter estimates for the AS and BS regions in future global ECCO optimizations. I would suggest that the authors report the optimized parameter values with uncertainty ranges (assuming those are calculated from the inverse Hessian matrix/approximation) and whether these values make sense scientifically. Also, the current modeled fields do not have any uncertainties, perhaps because only 1 optimization experiment was performed. If the uncertainties of the optimized parameters can be calculated, how the random perturbations within the range of the optimized parameters can impact the modeled fields?

3. Sensitivity tests to varying initial conditions/parameter guesses

It is great to see that the authors added several sensitivity trials to test the relative importance of air temperature, precipitation, and winds in better matching the region-specific, nonlinear processes. However, I wonder if starting in another place in parameter space would lead to a significantly different local minimum of the cost function. I understand that the time and effort of conducting even 1 optimization experiment (20 iterations for this study) can be significant, so I would not suggest that the authors do a large number of new optimization from different initial conditions/parameter guesses, but still would like to see a reasonable number of trials to ensure the robustness of the optimized model solution presented in the study.

Other comments:

Figures 4-5 do not show observations but consistently referenced in the sentences discussing the model-observation misfits. I would suggest that the authors include the detailed characteristics and patterns of the observational data as separate figures or sentences rather than referencing figures from other papers.

Line 142: I am not convinced that the figure shows much better agreements to the sea-ice observations in the optimized simulation. For September, how much of overestimation at iteration 0? Closer to observation after optimization by how much? Please be more quantitative.

Line 168: The fact that heat and salt transfer coefficients changed at iteration 11 conflicts with line 166. Please rephrase this section. The authors discussed the reason why those parameters had to change in lines 116-118 in the Methods section, but these all should consistently appear together in the methods, or in the results.

Line 190-197: Instead of providing the absolute cost function values (not very meaningful without the full presentation of the model-observation misfit calculations) please calculate the percent reduction in the cost function. Also, I am not sure why Table 5 is needed. The summary of what changed as part of sensitivity experiments can be directly stated in the discussion, and maybe with the cost function reduction reported in Table 5.

Figure 12: indicate the depth in a-c.