Response to the specific comments and corrections of the reviewers
(Italic: comment of reviewer, bold: our reply)

We greatly appreciate all very helpful and insightful comments by the reviewers.

Executive Editor (Comments to Author (shown to authors):
Dear authors,

in my role as Executive editor of GMD, I would like to bring to your attention our Editorial version 1.2:

https://www.geosci-model-dev.net/12/2215/2019/

This highlights some requirements of papers published in GMD, which is also available on the GMD website in the 'Manuscript Types' section:

http://www.geoscientific-model-development.net/submission/manuscript_types.html

In particular, please note that for your paper, the following requirement has not been met in the Discussions paper:

- *"The main paper must give the model name and version number (or other unique identifier) in the title."*

Please add a version numbers for MITgcm/ECCO in the title upon your revised submission to GMD.

Yours, Astrid Kerkweg

As suggested, we include version number in the title.

Reviewer #2 (Comments to Author (shown to authors):
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).

We agree that in the previous version of the manuscript, it was not clear how the cost function is defined. We revised the manuscript to state that we use a cost

function following Forget et al., 2015. The weights for potential-temperature and salinity observations are prescribed as a function of depth and are estimated based on the standard deviation of the simulated properties in the model domain (Lines 107-111). Following suggestions from the reviewer, we calculated r, RMSD, and normalized standard deviation before and after optimization. We also include a Taylor diagram and add discussion (Lines 243-249).

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?

One drawback of the adjoint method is that model uncertainty cannot be calculated directly when obtaining optimized parameters. One component of uncertainty can, in theory, be calculated by obtaining the second derivative of the cost function but this would involve an unrealistic amount of computations. For ECCO-v4 (which has a grid cell count similar to that of our higher-resolution regional domain), the dimension of the state vector at each time step is greater than N=11 million. Updating the state and its covariance would require running the model N+1 times at each time step, as described in Wunsch (2018). More practical and less formal ways of obtaining uncertainty for ocean state estimates

are discussed in Wunsch (2018) but their application is beyond the scope of the current manuscript.

Carl Wunsch (2018) Towards determining uncertainties in global oceanic mean values of heat, salt, and surface elevation, Tellus A: Dynamic Meteorology and Oceanography, 70:1, 1-14, DOI: 10.1080/16000870.2018.1471911

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.

We agree that this is a very good suggestion but it would require a substantial amount of additional work — the adjoint-model-based ocean state estimate presented herein was achieved after ~5 years of development and computations. Instead we revise the manuscript in Lines 276-277 to suggest this as future work.

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 searate figures or sentences rather than referencing figures from other papers.
We do not include observations as these sections are obtained from different locations and can not be compared as done in Figures 4 and 5. We instead include a few sentences to indicate that detailed model-data comparisons were

conducted in Nakayama et al., 2017 (Lines 136 and 143-144).

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.

We revised the manuscript as suggested (Lines 157-158).

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 whose 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.

We revised the manuscript as suggested (Line 183).

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.

We revised the manuscript as suggested. We now include percent increase in Table 5, as these sensitivity experiments tend to increase the cost compared to the control (CTRL) experiment.

Figure 12: indicate the depth in a-c.

Done. Depths are indicated in the figure caption.