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Ocean Science Discussions

Interactive comment on "Assimilation of sea-ice concentration in a global climate model – physical and statistical aspects" *by* S. Tietsche et al.

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We would like to thank the anonymous referee #1 for providing a thoughtful assessment of our work, which will certainly lead to a much improved version of the manuscript.

General comments

We are fully aware that there are severe limitations of "Nudging" as a data assimilation method, and that more sophisticated assimilation methods in general provide more consistent state estimates. Nevertheless, if nudging provides a state estimate that is good enough for the application at hand, we think it can be a useful alterna-

tive to full-fledged assimilation methods, because its implementational complexity and computational costs are orders of magnitude smaller. In our case, the application is the initialisation of sea ice in a global climate model for seasonal to decadal climate predictions. In several subsequent studies, the method described here has been successfully used to initialise the sea-ice state for predictions (e.g. Tietsche et al. 2012, Clim. Dyn., in revision). In response to the referee's comment, in a revised version of the manuscript we will add a paragraph at the beginning of Section 3 that stresses the limitations of the nudging approach. Furthermore, we will revise the abstract and conclusions to avoid the impression that the simple method described is "universally successful".

In response to the referee's second general comment we will add to the manuscript some analysis how the assimilation affects non-sea-ice surface fields like the Arctic surface air temperatures (SAT), Arctic sea surface temperatures (SST) and salinities (SSS), and mixed-layer depths (MLD). By looking at the assimilation error in these non-sea-ice surface fields, the limitations of our simple assimilation scheme become apparent. We discuss these new results further below in this authors' comment, giving them a separate section together with the corresponding figure R1 (attached to this comment).

When analysing non-sea ice fields in response to the referee's comments, we realized that another change to the manuscript is necessary: Assimilation performance for SSS and MLD is very poor if SSS is adjusted as described in the discussion version of the manuscript. Without the SSS adjustment, assimilation performance for SSS and MLD is much better, while assimilation performance for sea ice is very similar. Therefore we decided to drop the SSS adjustment in a revised version of the manuscript. This means that the CAT and PMT curves in Figures 1–4 as well as the numbers in Table 1 will slightly change. However, all previous interpretations of and conclusions from the experimental results remain the same.

To avoid making the manuscript overly long, we will follow the referee's suggestion to shorten other parts of the manuscript. We will drop Appendix B. We still think that applying the assimilation to the ice energy balance model provides useful insight, so we will keep Section 6.4. However, we will merge the model description and discussion of its properties (Sections 6.1–6.3) into Appendix A. After appropriate adaption of Section 6.4 it can stand on its own as an explanation for the different assimilation performances.

In summary, we plan the following revisions to the manuscript addressing the referee's general comments:

1. Add a paragraph stressing the limitations of our simple assimilation method to the beginning of Section 3
2. Add a discussion of the assimilation performance for several non-sea-ice fields to Section 4.2
3. Move Sections 6.1–6.3 into Appendix A and drop Appendix B
4. Revise abstract, conclusions, and the main text where necessary, to caution the reader of the inevitable limitations of our simple method.

Specific comments and suggestions

p2404, l20: Point taken, will revise.

p2405, l28: Point taken, will revise.

p2410, eq7: We feel that the relation between the gain and the error statistics is too involved to be discussed already at this early point of the manuscript. We think the presentation of the manuscript is more clear and easy to follow the way it is now.

p2412, Sec. 3.3: As the referee correctly points out, our scheme does not conserve heat. However, since the heat content of the surface ocean is determined by

sea-surface temperature and sea-ice volume, and since in the revised version of the manuscript we show that the simulation of both is improved by the data assimilation, we can conclude that simulation of heat content of the surface Arctic Ocean is improved as well.

As to the conservation of freshwater, we note that sea ice is submerged in our model (see Marsland et al., 2003, cited in the manuscript). Therefore the addition of a certain volume of sea ice removes almost the same volume of freshwater from the system.

We agree that a more in-depth analysis of the heat and freshwater fluxes introduced by the assimilation scheme would be useful. However, we feel that this is outside the scope of our manuscript.

p2413, l18: In the revised version of the manuscript, we will discuss the simulation of sea-surface temperature, sea-surface salinity and mixed-layer depth of the Arctic Ocean in the assimilation runs.

p2415, l12: In the revised version of the manuscript, we will discuss Arctic surface air temperature alongside surface ocean fields (see above comment). We do not speculate about the relative impact of atmospheric, oceanic and sea-ice fields on predictions, but leave this to dedicated studies. However, we see our sea-ice assimilation method as part of an assimilation scheme that also makes use of oceanic and atmospheric data, which would limit the effects of sea-ice assimilation on oceanic and atmospheric variables.

p2416, end of Sec. 4.2: We agree that this would be a very useful exercise. However, it is clearly outside the scope of the present manuscript, and could well be the subject of a follow-up study.

p2418, l13: We are by no means implying here that the climate model is perfect. We know that there are considerable biases in the model; one example for this is the wintertime Barents See bias discussed in Fig. 8. Nevertheless, the numbers presented in

Table 1 strongly suggest that – at least for the long-term average quality of the sea-ice concentration field – the choice of the assimilation method is more important than the model bias. We think that this is a valid conclusion in light of the experimental results. In the revised manuscript, we will restrict the statement under debate to the sea-ice concentration field.

p2419, l6: Section 5.2 and Fig. 5 will be substantially revised in response to the referees' comments (see comment to p2433, l21 by referee #1 and comment S.5.2, P.2418-2149 by referee #2).

p2419, l9: See previous comment.

p2420, l26: Point taken, will revise.

p2421, l28: As stated on ll. 1–6 on p. 2408, the model assumes perfect lateral mixing of heat within an ocean grid cell. Therefore, as long as there is any ice left in the grid cell, surface heating in the open-water part of the grid cell does not lead to an increase in SST, but is rather used to melt ice. That means $g_w < 0$ *does* lead to a loss of ice volume in the grid cell, although it does not lead to a decrease in ice concentration. Nevertheless, the phrase "melting over open water" is deficient; we will change the wording in the manuscript.

p2422, l6: We have looked at all months, and could have used the January/March equally well do demonstrate our point. However, we chose to show the October forcing because then the (neglected) turbulent fluxes are smaller and the simple model is more appropriate.

p2422, l24: In the manuscript we do not give details on how we diagnosed sea-ice growth rates, which might have caused the confusion. The diagnostics that we used considers only grid cells in the model where sea ice is present. As soon as there is no ice left, of course, it is meaningless to convert the surface heat fluxes into sea-ice growth rates. We will add clarification to the manuscript. Since our point here is

precisely the concentration-dependence of the growth rate, rephrasing the discussion in terms of heat fluxes would be counterproductive.

p2422, l27: Here, we are talking about the *conditional* probability density. Since the conditionality is essential, but there is no such term as "conditional relative frequency", we would like to keep that.

Usefulness of Sec. 6: We see the point that sections 6.1–6.3 make the manuscript feel very long when reading. However, we think that the discussion in Section 6.4 does add essential insight that would not have been possible without constructing the simplified model first. Nevertheless, to shorten the manuscript, we will move the model description and the discussion of growth rates to the appendix.

p2428, l19: Point taken, will revise.

p2430, l6: We attach the map from which we calculated the spatial average as Fig. R2, but do not include it in the manuscript. Please note that Fig. 10b shows the diagonal elements of the background error *covariance*, not of the background error correlation (compare Eq. (24)).

p2432, l14: We agree.

p2433, l21: We agree that the comparison to ICESat data alone is not conclusive, as both referees pointed out. We decided to enhance the analysis and also provide a comparison to Arctic sea-ice volume from the PIOMAS reanalysis (Schweiger et al, 2011) for the same time period. This substantially changes Fig. 5 and the corresponding discussion in the main text, which will be part of a revised version of the manuscript. We attach the revised Fig. 5 here as figure R3 and discuss its implications below in a separate section.

p2434, l10: Point taken, will revise.

p2434, l13: Point taken, will revise.

p2434, l15: We think that the phrase "shared with other approaches" is justified. Even modern sophisticated data assimilation methods like Kalman filters are not physically consistent in the sense that the model equations are obeyed without introducing artificial sources or sinks of matter and energy. Of course, they are much more consistent in a statistical sense.

Appendix A – Usefulness of simplified model: The advantage of the simplified model is that we can numerically investigate the impact of the data assimilation on the local ice-energy balance (Sec. 6.4), and thereby provide a simple explanation why the performance of the assimilation methods differs. See also above.

Appendix B: We will drop appendix B, as both referees feel that it does not add much insight.

Discussion of Figure R1

Because there are no sufficient observations of the ocean fields that the model results could be validated against, we will restrict ourselves to the identical-twin experiment. We consider the seasonal cycle of the Arctic-Ocean field RMS error of the different runs (Eq. (12)) in complete analogy to Fig. 2 of the manuscript.

The Arctic Ocean SST error (Fig. R2(a)) resembles the Arctic Ocean sea-ice concentration error. Without assimilation, the RMS error is about 0.3 K in winter and about 1 K in summer. All assimilation methods reduce this error, with PMT performing best. For the Arctic Ocean SSS field, all assimilation methods lead only to a small reduction of the RMS error of 0 to 0.1 PSU starting from 0.6 to 0.7 PSU as the level of natural variability (Fig R2(b)). This demonstrates that assimilation of sea-ice concentration with the simple approach that we employ is not very successful in constraining other variables in the climate model. The MLD (Fig. R2(c)) shows even less positive impact due to the assimilation. While assimilation with the PMT method reduces MLD error

over the winter months, it even increases MLD error over the summer months. Finally, for Arctic SAT we find a slight improvement between September and March, but little effect in Summer months (Fig. R2(d)).

Discussion of Figure R3

In a revised version of the manuscript, we will change Fig. 5 to compare the northern-hemisphere sea-ice volume from direct ICESat thickness measurements, from the PIOMAS reanalysis (Schweiger et al., 2011), and from the different model runs performed for the present study. There are differences between the ICESat and PIOMAS ice volume estimates of up to 3000 km^3, which are, nevertheless, compatible with the uncertainties quoted by Schweiger et al. (2011) and Kwok and Rothrock (2009) (cited in the manuscript). Note that conclusions about the assimilation performance differ depending whether one compares to ICESat or to PIOMAS data.

It is evident that the no-assimilation model run has too low ice volume throughout. All assimilation methods bring the ice volume closer to both the observational snapshots from ICESat data and the time series of reanalysed ice volume from PIOMAS.

In November 2007, sea-ice volume was anomalously low with respect to the years before both in ICESat and PIOMAS data. This relative anomaly is captured well by the CAT and PMT, but not by CMT and the no-assimilation run. Although the CMT run is often as close to the PIOMAS estimate as the other two methods, it is further away from the ICESat estimates. Moreover, the CMT method overestimates the seasonal cycle of sea-ice volume by 3000-4000 km^3. All this indicates that the CMT method is less skillful than the other two method, which are within the error bars of both PIOMAS and ICESat volume estimates.

References

A. Schweiger, R. Lindsay, J. L. Zhang, M. Steele, H. Stern, and R. Kwok. Uncertainty in modeled Arctic sea ice volume. *J. Geophys. Res.*, 116:C00D06, September 2011.

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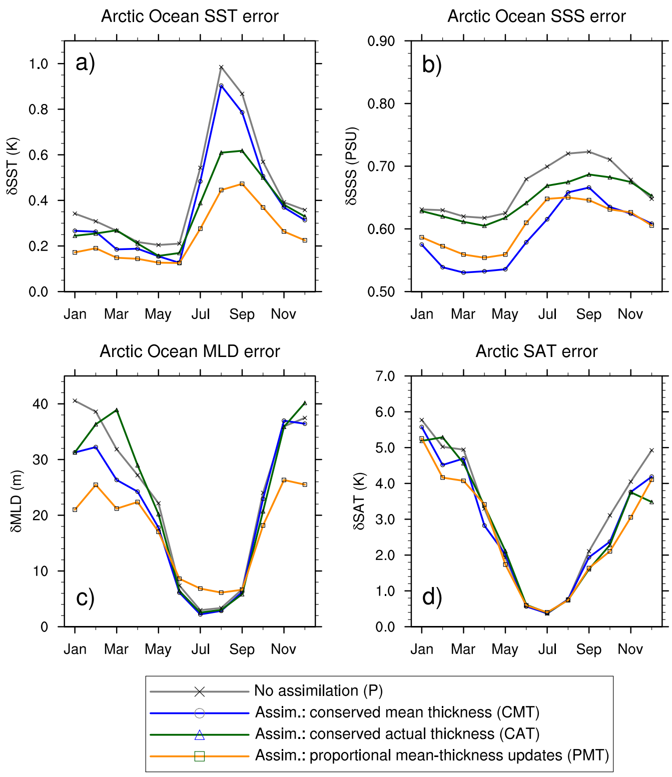

Fig. 1. The average point-wise root-mean-square error in (a) Arctic Ocean SST, (b) Arctic Ocean SSS, (c) Arctic Ocean mixed-layer depth, and (d) Arctic SAT. Shown is the climatological error for each month of

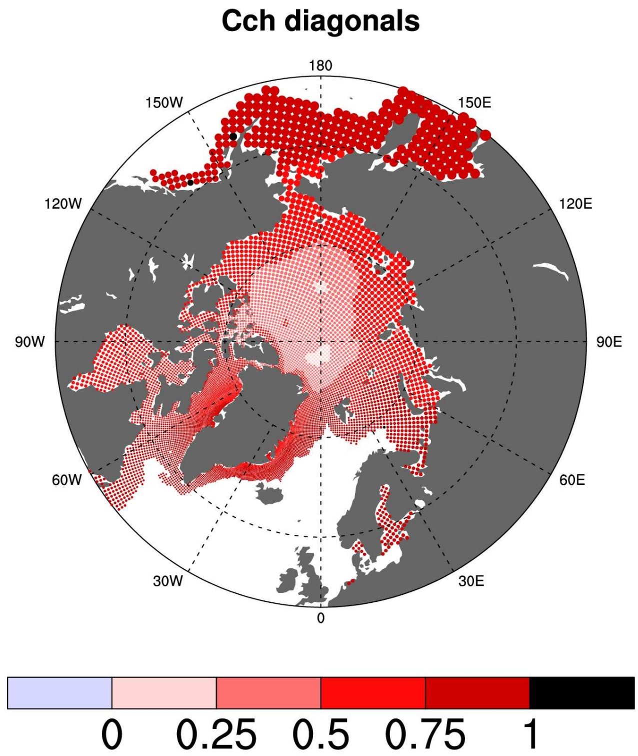

Fig. 2. Correlation between sea-ice concentration and mean sea-ice thickness at the same grid point diagnosed from a long model run with Eq.∼(24) of the manuscript.

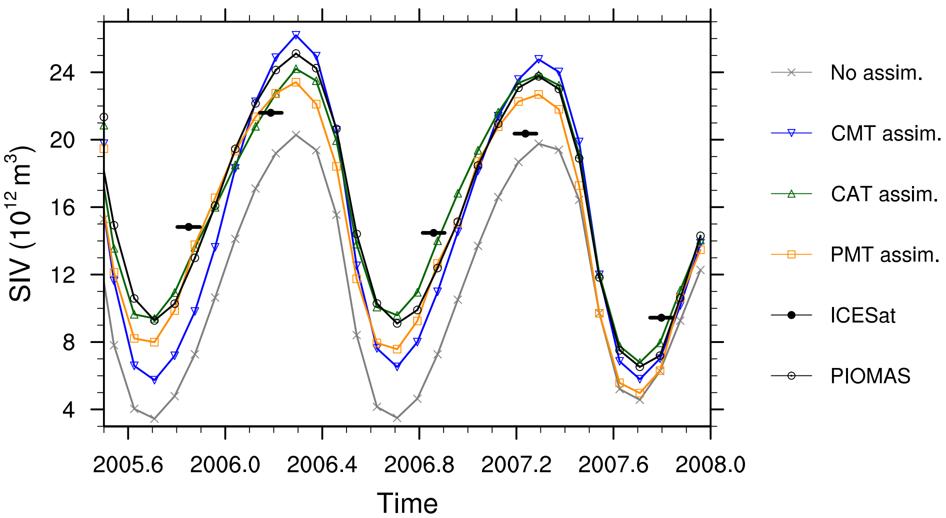

Fig. 3. Comparison of northern-hemisphere sea-ice volume from our model runs with ICESat observational estimates and PIOMAS reanalysis estimates.