

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

### **Anonymous Referee #1**

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The manuscript "Assimilation of sea-ice concentration in a global climate model – physical and statistical aspects" by Tietsche, et al. describes the design of a simple assimilation scheme (Newtonian relaxation) and its application to a coupled AOGCM. Dense sea ice concentration and, based on 3 different assumptions, derived sea ice thickness are used to correct the model's ice state. Differences in performance of the schemes are derived from physical and statistical arguments.

The presentation of the work is mostly clear and the manuscript is very well written. While this is great, the presentation style also appears overly positive and lacks the capacity for self-critical awareness: I got the feeling everything is "too good to be true". Still, I think that the manuscript is worth publishing after a revision that (also) addresses the points below.

My main concern is with the Newtonian relaxation method itself. Nudging techniques will always introduce a phase shift depending on the coupling constant, i.e. the relaxation parameter (as a simple analogue, think of a forced oscillation in any mechanics textbook). This is probably the reason why modern assimilation systems use more advanced schemes. Nudging does not take into account possible spatial correlations that maybe important in sea-ice physics, as the Arctic ice can move in large (basin wide) features at times. None of the issues associated with nudging (e.g. phase error, missing correlations, ...) are addressed in the manuscript.

Further, only the effect of the assimilation on the assimilated fields is explored (ice thickness and extent). What happens to the remaining variables of the climate model. Do they become more realistic, or does the assimilation introduce unexpected anomalies?

The presentation in the manuscript suggests that the scheme is universally successful. It would be interesting to know the limitations of the scheme. When does it break down? Why don't we all use this scheme for fixing our sea-ice models? Does it work equally well in the Antarctic, where the correlation between ice concentration and thickness is different?

I think that the manuscript could be shortened. For example, the quite useful covariance analysis in section 7 could be introduced along with the introduction of the three different methods, state expected performance, and relate to previous work (e.g. Duliere and Fichifet, 2007), rather than use it as an post-mortem explanation. Likewise, section 6 could be condensed and inserted in section 2 or 3. Now, after section 5, it felt like the beginning of a new paper.

specific comments and suggestions (mostly minor): p2404, l20 I think that "are superior to" has the wrong connotation? I'd replace it with "outperform"

p2405, l28 AOGCM -> an AOGCM?

p2410, eq7, why not discuss  $K_N$  as a "Kalman"-gain already here and relate the value of 0.1 to some background/observation error? Now it this approach looks very much "ad hoc" (in spite of your referring to section 7 and the end of 3.1).

p2412, Section 3.3. What about the conservation of heat? Your scheme provides a source of sea-ice volume and thus heat (latent heat of ice melt/formation, the cooling effect is described here). The same is true about freshwater: While the salinity is adjusted, the water volume of the system is changed (once ice is added by the assimilation, the can be melted to raise the sea-level in a free-surface model). Would it make sense to discuss the magnitude of this extra heat and fresh water source and relate it to the, say, seasonal cycle of heat flux and fresh water flux?

p2413, l18: the state of the ocean is not discussed in this manuscript, unfortunately.

p2415, l12. What happens to other parameters in the ESM? How does the atmospheric and the oceanic trajectories change with the different nudging techniques. Is it relevant at all? I assume that for predictions (after the observations have stopped coming in), the state of the slowly varying ocean and and the fast atmosphere are more important than the re-analyzed state of sea-ice.

p2416, end of 4.2. What happens if neither data nor model are perfect? The twin-experiment configuration can be used to explore this more realistic case. Data fields could be perturbed with some random uncertainty, model biases could be introduced by using different internal parameters, e.g. vertical diffusivity in the ocean/atmosphere, different ice strength parameters in the sea-ice model, etc. This exercise would be very useful to demonstrate the robustness of the scheme (or equally useful: its limitations).

p2418, l13: I think it is too early for this conclusion, in fact, I don't see how one can draw a conclusion that implies that a climate model is perfect (ever). What happens to the heat content in the system by adding/removing sea ice? That should introduce a bias in the assimilated run, when it is used for prediction.

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p2419, l6: or deficiencies in the assimilation scheme? p2419, l9: and by the free run as well. p2419, ll10: I would move this paragraph to the beginning of 5.2

p2420, l26: ... by dividing by/with the ... p2421, l28: how can we have melting over open water? Growth rates over water are only effective, if they are positive to form ice. Therefore, while  $g_w$  may be negative, it should not have an effect on ice concentration (as is correctly implied by (6)), it also shouldn't change the ice volume ( $h_m$ ). So the last statement of the paragraph should say that with increasing ice cover, there is more melting (more of  $g_i < 0$ ), I think.

p2422, l6, why not  $LW_{down} = 170 \text{ W/m}^2$  (the Jan, Mar values)? Do you need to do that because the turbulent atmospheric fluxes are neglected? p2422, l24: this is really confusing: while the argument about the growth rates implied by heat fluxes is probably correct, the actual growth rate (here melting) can only be applied to ice (without ice, there's no melting). How can ice melt more, the less there is? Please rephrase in terms of heat flux or clarify otherwise. p2422, l27 and Caption: Fig7: probability -> relative frequency

p2423, l9: can be due to -> can be caused by

Is Section 6 useful? All of this reasoning would have been possible without the simplified model. This section certainly has a lot of potential for shortening: I would remove the simplified model altogether, as it does not provide essential insight that wouldn't be possible without it.

p2428, l29: for clarity I'd write: submatrices of dimension  $pxp$

p2430, l6, how does 0.7 emerge, if the central Arctic (a large area) is below 0.5 and given figure 10b? p2430, l8-9, I don't yet see this justification.

p2432, ll14: I would turn it around and say, that only optimal methods with good background error covariances should be used in data assimilation.

p2433, l21. To me the results suggest that the adjustment of ice concentration works,

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but for ice thickness the scheme is not very successful. In three cases the free run is close the observations, but only in two cases the assimilated run is close to observations (or even within error bars!!). Why give the wrong impression here that it worked?

p2434, l10: not really "independent", but "another" p2434, l13: "automatically build in" -> the information of the model equations is not used in the analysis step p2434, l15: "shared with other approaches": nowadays, big emphasis is given to physically consistent assimilation schemes, examples are Kalman filters (of various flavors) and variational techniques. Purely statistical interpretations (optimal interpolation, etc.) are no longer state-of-the art.

Appendix A: I am not sure what the advantage of the simplified model is. See above  
Appendix B: p2438, l4: admittedly,  $P^*$  is a tuning parameter, but I have never seen values this low (5000). Is there a plausible explanation why the sea-ice model needs these low values (typical values that I encountered range from 15000 to 30000). p2438, l13: also for small velocity shear, the stress tensor dominates because of very small  $\Delta$  p2438, l22-25: I cannot follow: how can the ice advection react strongly in very thick and compact ice? There is hardly any drift velocity possible under these conditions. Or do you mean driven by the gradient of  $P$ ? Please clarify (or drop this appendix, as it does not add too much of insight)

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