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# *Interactive comment on* "Influence of meltwater input on the skill of decadal forecast of sea ice in the Southern Ocean" *by* V. Zunz and H. Goosse

**Anonymous Referee #2**

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It seems to me the major finding in this study is that imposing a large negative freshwater flux for a decade before 1980 and then reducing the flux by 1/3 or so after 1980 causes sea ice to expand. There was no need to ramp up the freshwater flux after 1980, instead the abrupt jump at 1980 caused the expansion. The paper is technically very complex, with data assimilation and variable freshwater hosing, yet the result is very basic. Previous studies have shown that suddenly turning on freshwater in ~1980 is effective at causing the sea ice to expand. The main new innovation shown here is that the same result can be gained by tinkering with the freshwater prior to 1980, so that there is a relative increase in the freshwater flux in 1980. It is pretty clear that the minor ramping after 1980 has little effect as in Swart and Fyfe (2013). However, I disagree with the conclusion in this study that "Bintanja et al (2013) is not confirmed in the present study". In my mind, this study has a strong response for exactly the



same reason as Bintanja et al. Both have an abrupt increase in freshwater flux at the start of the period of validation (e.g., 1980 in this study) that is imposed thereafter for 30 yrs. The main difference is that in this study the initial state is forced to be a high mixing state by adding a negative freshwater flux prior to 1980 and the freshwater flux is positive after 1980 in only a relative sense.

The data assimilation without freshwater gives results that are not too surprising. The ensemble can be sampled (or selected) and nudged in a way to give good agreement with observations. The much lower coupling between ocean surface and layer below in Fig 4 indicates that climate relationships change with data assimilation. In this case the two ocean layers are weakly coupled compared to without data assimilation. However, the run with data assimilation without freshwater is unable produce adequate initial conditions for the hindcast runs because it does not have the outlandish variability prior to 1980 that is key to the cases that do have expanding sea ice. Because I view the high variability as a problem, I am left to assume that the model is flawed (also not too surprising considering that CMIP5 GCMs have similar problems) either because it is lacking some key physics or forcing.

With large stochastic freshwater input added along with data assimilation, the variability becomes sufficient to send the model into a (unphysical?) state with very high ocean mixing just prior to 1980. The variability of ice and ocean skyrockets prior to 1980, when observations are too sparse to control it. If the observations were more complete, would this have been possible? The authors should address this question. It appears to me that the massive random freshwater input is selected in the resampling process because observations are insufficient to rule out these cases. I am not at all convinced it is a realistic initial state.

The authors should show the relationship between mean surface air temperature (Fig 5) and sea ice extent (Fig 2) by plotting these variables on each axis of a scatter plot. I excpect it would show that their relationship changes fundamentally after about 1980 in the run with data assimilation and freshwater forcing. A conservative view would be that nonstationary behaviour is a flaw in the model results without observations to prove it happened or a good physical explanation.

One conclusion of this study is that the initial condition must adequately represent the observed state to perform skillful predictions. Maybe this is true, but how can we be convinced this was achieved in this study? In other words, how can we be sure that unrealistic initial conditions cannot achieve skillful predictions by accident? The authors point out that the data assimilation can account for model biases, which I think means that the initial conditions might be necessarily unrealistic.

The authors only put the magnitude of the freshwater into observational perspective when they discuss the ramp rate after 1980. But they then show the ramping is irrelevant. They should also mention their typical frehswater input of 0.01 Sv equals about 300 Gt/yr, which is similar to the freshwater that was imposed in a steady or ramped fashion by Swart and Fyfe (2013) and Bintanja et al (2013). It is also a lot higher than the Grace imbalance.

The DA_FWF does not seem necessary in this study. It adds considerable complication, and slows the reader from getting at the essence of the results. I recommend the authors do another hindcast experiment where they branch from the DA_NOFWF run in about year 1960 and add a negative freshwater flux until 1980 and then reduce it substantially and abruptly for the remainder of the run. I expect the results would be just as skillful and much easier to understand. The authors than would have to decide if the DA_FWF run is useful in spite of the objections raised here. I can appreciate that DA_FWF arrives at an initial state using and objective method, while my suggested hind cast could seem arbitrary. The issue is whether the objective method has enough observations to be satisfactory.

Minor points

p3566 line 21-22 I do not understand the claim that significant predictability for the trend spans several decades, unless you are referring to a perfect model.



p3580 line 4 I think "participates to" should be "contributes to"

p3580 line 18 would be better if it said "equivalent to a melting rate of 1.4 Gt per yearˆ2". I had to get out my ruler to verify this is what was meant.

Fig 1, the spatial distribution is unfortunate for skipping the outlet of meltwater from the Ross Shelf in McMurdo Sound. Though it is probably not critical.

Table 1 would help if it had number of ensemble members indicated, especially for the hindcast runs. I didn't realize there were ensembles until I saw the shading in Fig 7.

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