Review of the manuscript 'Using data assimilation to investigate the causes of Southern Hemisphere high latitude cooling from 10 to 8 kaBP' by P. Mathiot et al.

Anonymous Referee #2

This study investigates the mechanisms responsible for the cooling registered by proxy data at high latitudes of the Southern Hemisphere from 10 ka to 8 ka. To this end climate simulations performed with the LOVECLIM coupled climate model and constrained through data assimilation are used to assess two possible hypotheses: a change in atmospheric circulation and a cooling in the Southern Ocean sea surface temperatures. The authors conclude that both are required in order to match the reconstructions.

This is a valuable study that assesses past climate changes using a novel approach in paleoclimate as data assimilation. The experimental design is thus original and the results interesting. Nevertheless, I think several issues should be improved. Thus I recommend publication subject to minor revisions.

General main comments (see also the specific comments below):

1) The authors should try to frame better the motivation and justification of the experimental design.

2) The text requires a thorough revision to improve several minor issues; often further explanations are required.

3) The authors should try to discuss a bit more the relevance of their study, the outlook and caveats in the Conclusions.

Specific comments:

0. Abstract

C0 (P 5546): In lines 12 and 18 the magnitude of the simulated cooling in the assimilation experiments is indicated. To be able to quantify their contribution, the magnitude of the reconstructed cooling should be previously mentioned. Also, I do not understand the "However" in line 19, should it not rather be a "Thus"?

P. Mathiot and co-authors: We have added the mean value of surface air temperature change over Antarctica and over the southern ocean (mean value of all the proxy data available over Antarctica and over the Southern Ocean). The beginning of the abstract is now :

"From 10 to 8 ka BP, paleoclimate records show an atmospheric and oceanic cooling in the high latitudes of the Southern Hemisphere. During this interval, temperatures estimated from proxy data decrease respectively by 0.8°C over Antarctica and 1.2°C over the Southern Ocean.. In order to study …"

"However" has been removed.

1. Introduction:

C1 (p 5546, l 23): Here it is stated that East Antarctic ice core records show a cooling from about 10 ka to about 8 ka. First, this cooling is more notable but not restricted to East Antarctica.

P. Mathiot and co-authors: As Masson-Delmotte (2000) show that Byrd and Dominion ice cores have a deuterium signal coherent with this cooling, we have modified "East Antarctica" by "Antarctica" in the first sentence of the Introduction.

C2 : Second, Figure 1 shows only the reconstructed temperature differences. This figure is useful to locate the data used and distinguish which are used in the assimilation procedure, but the magnitude of the temperature change is of limited value without knowing the variability. Since this is the central issue of this manuscript, additionally showing some of the reconstructed time-series (as in Stenni et al. 2011) would be very helpful.

P. Mathiot and co-authors: This paper is based on snapshots (10500-9500 and 8500-7500). Consequently, a figure showing the variability of the temperature during the early Holocene will confuse the reader. Furthermore, we give an estimate of the error bar for the ice cores temperature reconstruction. This error bar (0.3°C) reflects the variability of the spatial slope coefficient used to convert δD or $\delta^{18}O$ in temperature over the 1000 y period. And as you can see in Table 3 the distance between simulation is between 1.5 times the error bar for the best simulation to 3.3 times the error bar for the STD simulation. It means that, the model-data mismatch is larger than the variability depicted in the ice core records (for the early Holocene at least).

C3 (p 5547, l 5 - p 5548 17): Here the authors are reviewing the evidence from data from high southern latitudes. I was expecting that the data mentioned here would show up later on in Table 1 and Figure 1, but this is generally not the case. I would encourage the authors to make this discussion more coherent with Figure 1 and Table 1, referring to both, specially by discussing more of the data that these 1 show.

P. Mathiot and co-authors: The data cited in the introduction have been added in the table 1 with the corresponding references and in Figure 1. A larger discussion of these data is not presented in the manuscript because the data are only qualitative and we use them only for a qualitative evaluation of the sea ice simulated by LOVECLIM. For example sea ice duration and concentration are derived from kerguelensis and F. Curta gp diatoms (Crosta et al. 2008). This proxy allows us only to evaluate if, there is less or more sea ice in 8 compare to 10 ka.

Crosta, X, D. Denis and O. Ther (2008): Sea ice seasonality during the Holocene, Adélie Land, East Antarctica, Marine Micropaleontology, 66 222-232.

C4 (p 5547, l 7): I think the reference to Kim et al. (2012) might not be correct, see below.

P. Mathiot and co-authors: DONE

C5 (p 5547, l 10): I suggest replacing "These" by "The".

P. Mathiot and co-authors: DONE

C6 (p 5548, l 15-16): Please replace "explained" by "explain". Also "and potentially providing" does not fit here, please rephrase. Finally, I would not make a new line at the end of this paragraph since precisely the aforementioned climate simulations are explained next.

P. Mathiot and co-authors: DONE, the new sentence is:

"Based on transient climate simulations, mechanisms responsible for Holocene climate variability have been investigated. Using …".

C7 (p 5548, l 17-21): The first sentence of this paragraph does not make full sense since the verb is missing; please rephrase. Also, I understand that Renssen et al. 2005 concluded that the long-term cooling could be explained by "the combined effects of local orbital forcing and the long memory of the system", with no need to resort to north-south teleconnections. Now a different perspective is taken, possibly in the light of new results. The authors should explain in more detail why this is the case and what was not fully answered in Renssen et al. 2005 to explain the need for the present work. Finally, I recommend joining this paragraph with the one above.

P. Mathiot and co-authors: The sentence have been rephrased *"…Renssen et al. (2005) showed that the long-term SH high latitude temperature trend during the Holocene (9 ka to present) can be explained by a combination of a delayed response of the Southern Ocean – Antarctic climate to local orbitally-driven insolation changes, modulated by the memory of the system. …".* Unfortunately, simulations from Renssen et al. (2005) do not cover all the early Holocene and their study does not take into account the LIS deglaciation and the implied teleconections..

C8 (p 5548, l 23): Please suppress "the" before "both".

P. Mathiot and co-authors: DONE

C9 (p 5548, l 28): This sentence is unclear. I assume you mean similar to the bipolar seesaw mechanism invoked for the last glacial period. Please rephrase. Also, I think it would be worth referring here to Shakun et al. (2012): Shakun, J. D., P.U. Clark, F. He, S.A. Marcott, A.C. Mix, Z. Liu, B. Otto-Bliesner, A. Schmittner, and E. Bard, "Global warming preceded by increasing carbon dioxide concentrations during the last deglaciation", Nature, vol. 484, pp. 49-54, 2012.

P. Mathiot and co-authors: We have changed "*large scale bipolar seesaw*" by "*bipolar seesaw mechanism*" and added the reference.

C10 (p 5549, l 2-5): It would be helpful to elaborate a bit more on this last sentence referring to Renssen et al. (2010) to specify that this mechanism could override the effect of the bipolar seesaw mechanism.

P. Mathiot and co-authors: We have added precisions about the advective teleconnection: *"Such bipolar seesaw mechanism inducing austral warmth may be driven by the impact of the final Laurentide meltwater flux on the Atlantic Meridional Overturning Circulation. Additionally, changes in the intensity of convection in Labrador Sea could also influence high Southern Latitudes through advective oceanic connections (causing then delayed temperature changes of the same sign in both hemispheres, Renssen et al. 2010) and could overwhelm the effect of the bipolar seesaw in the case of shut down of the Labrador Sea deep water formation. This could ultimately dominate the impacts of local insolation changes and drive Southern Ocean climate evolution (Renssen et al, 2010)."*

C11 (p 5549, l 7): I suggest suppressing "This".

P. Mathiot and co-authors: DONE

C12 (p 5549, l 13-15): This paragraph states what the specific goal of this manuscript is, and which are the hypothesis that are investigated. First, I suggest merging with the paragraph below. Second, the motivation given in the text for an atmospheric circulation change hypothesis seems weak. The only previous reference is a change in the Southern Ocean westerlies leading to colder circumpolar deep water (CDW); is this what is meant? I suggest making the link more clear explaining what type of atmospheric circulation change is investigated and why.

P. Mathiot and co-authors: The atmospheric hypothesis is motivated by the explanations given by Schevenell et al. (2011) and McGlone et al. (2010). Schevenell et al. (2011) explain large parts of the temperature signal observed on the west side of the Antarctic Peninsula by the relation between the intensity of SWW and upwelling of CDW. McGlone et al. (2010) suggest that temperature change observed in Campbell Island could be explained by an equatorward migration and a strengthening of the SWW over Campbell Island and, consequently, an increase in poleward meridional heat transport. These two mechanism are cited in the introduction. Consequently, we only refer to the two studies in the new paragraph:

"Using data assimilation in an EMIC, we aim to test the ability of two different hypotheses to explain this cooling: either a change in the atmospheric circulation as suggested by McGlone et al. (2010) and Schevenell et al. (2011), or an oceanic cooling caused by a change in the local fresh water flux (fwf)."

C13 (p 5549, l 25): Please correct "it is admit".

P. Mathiot and co-author: DONE

C14 (p 5549, l 28): Suppress "s" in "Models".

P. Mathiot and co-author: DONE

C15 (P 5550, l 25): Even though this is mentioned at the end of the previous section, here I suggest stating explicitly that the simulations are snapshots or time-slice experiments for 8 and 10 ka, not transient ones (I assume). Also, what is the length of the simulations? Personally I would rather start by describing the simulation procedure as in section 2.4 and after describe the assimilation method and proxy data (sections 2.2 and 2.3).

P. Mathiot and co-authors: As suggested, we now give basic information about the experiments at the beginning of the paragraph about the forcing. The sentence added is : ***"…****LOVECLIM is much faster than many other three dimensional climate models, large ensembles of simulations can be carried out for data assimilation.*

All experiments are 400-years-long equilibrium runs (or snapshots) with constant forcing. These experiments are driven by orbital forcing …".

However we let the simulation description in 2.4. Because to explain the simulation with data assimilation we need to explain first data assimilation method and we do not want to split this section in two parts.

C16 (p 5551, l 1-2): It is unclear to me how the ice-sheets are treated: are they simply fix or are they prescribed or partially vary between the snapshots considered (i.e the Laurentide ice sheet)? Also, please write "FWF" in capital letters, here and elsewhere.

P. Mathiot and co-authors: About the ice-sheet, the Laurentide Ice Sheet is prescribed for the snapshot simulation at 10 ka and 8 ka according to the data provided by Peltier for the corresponding periods respectively. However, during the 400y simulation, the ice sheet does not evolve. We have modified the corresponding section to :

"As no ice sheet model is coupled to LOVECLIM in the configuration selected here, ice sheet topography and fwf are prescribed accordingly to the data available at 10 and 8 ka. The ice sheet topography from the reconstruction of Peltier et al. (2004) was adapted to LOVECLIM by Renssen et al. (2009) and the ice-sheet does not evolve during the 400 years of snapshot simulations.. …"

About “FWF” in capital letter, we decide to keep it in minuscule letter (we define it like this in the introduction) in order to avoid confusion between the fresh water flux and the simulation named FWF.

2. Experimental design

C17 (p 5551, l 19): I am not familiar with the data assimilation method, but, I would assume that the atmospheric streamfunction is perturbed only in the experiments addressing atmospheric circulation changes, right? This appears to be corroborated in section 2.4, but from the statement here one would infer it is part of the general strategy. Also, I understand the one-year time-step for assimilation applies to the atmospheric hypothesis only.

P. Mathiot and co-authors: Yes, you understand well. The perturbation of the atmospheric stream function as well as the “one year time step” is only for the experiments addressing atmospheric circulation change (ATM or FWFATM). For varFWF we perturb the fresh water flux instead of the atmospheric streamfunction (ATM or FWFATM) and we use a “50y time step” instead of the “one year time step” applied in ATM or FWFATM. We now clearly mentioned it in the section 2.4:

“The “best guess” fwf for LOVECLIM is estimated using data assimilation. Here, the assimilation time step is 50 years (instead of 1 year in the previous experiments). Because the response time of the ocean is much longer than of the atmosphere, a longer period is thus required to estimate the effect of the perturbation. In these experiments, the ensemble members are produced by adding a small noise to the fwf (instead of perturbating the atmospheric streamfunction as done in the previous experiments). …”

C18 (p 5552, l6): insert “the” before “original”. Also, what is the basis for the assumption of an error for the data of 0.7 C for marine and pollen records?

P. Mathiot and co-authors: DONE. The error on marine and pollen data is first based on the error given by the literature (when error are specified). For example, pollen data from McGlone et al. (2010) have an error of 1°C, for Nielsen et al. (2004) it is 0.8°C and Shevenell et al. (2011) suggest a total error bar of 2.2°C. Proxy data used for validation only have also a large range of error bar. However, we selected among the reported errors estimates in the lowest range in order to provide the strongest constrain on model results. Indeed, the larger is your error the less the particle filter constrains the model. We then decided to used a larger error on marine proxies than on the ice proxies because error mentioned in the literature are much smaller for ice core but still select deliberately a smaller error than the one specified in the literature for many marine core in order to have a constrain on the Southern Ocean. Furthermore, as mentioned in the manuscript, a reasonable change of the errors would not change qualitatively our conclusions but could modulate the amplitude of the simulated changes (Goosse et al., 2012). The manuscript have been modified like this:

"…These error bars on marine (0.7°C), pollen (0.7°C) and ice core (0.3°C) data are lower than the typical values given in the literature. This is a deliberate choice to strongly constrain the simulations with data assimilation on the Southern Ocean as well as on Antarctica. A reasonable increase of the errors would not change qualitatively our conclusions but could modulate the amplitude of the simulated changes (e.g., Goosse et al., 2012)…."

DATA added:

P. Mathiot and co-authors: One data from James Ross Island has been added in the paper for validation. We can't used it for data assimilation because it was published when the simulations were already done. Consequently the RMSE changes a little (some hundredths) but does not change the conclusion. Actions taken are a modification of figure 1 and a modification of the rmse in the text.

" … Some records rejected for data assimilation are kept for independent validation as well as recently released data (Mulvaney et al., 2012) that were not available to us at the time the simulations were launched (Table 1b). …"

New rmse:

Experiments	Antarctica	Southern Ocean
STD	1.01	1.26
ATM	0.45	1.04
FWF	0.74	0.77
ATMFWF	0.38	0.66

C19 (p 5552, l 10): replace "on" by "of" and rephrase "remain difficult to fully quantify"

P. Mathiot and co-authors: DONE

C20 (p 5552, l 16): separate "by" and "changes"

P. Mathiot and co-authors: DONE

C21 (p 5552, l 26): delete "are" before "through"

P. Mathiot and co-authors: DONE

C22 (p 5553, l16-18): I understand the initializations stem from two (not one) long equilibrium runs for 8 and 10 ka, respectively?

P. Mathiot and co-authors: Yes, it is right. The text has been modified in the following way: *"These simulations are initialized by the results from a long* *equilibrium run (with a duration of 3000 years) with constant forcings for 10 and 8 ka, respectively.*"

C23 (p 5555, section 2.5): This discussion is necessary but I am not sure that it belongs in this section (which by the way is already quite long); I would rather try to include it in the Introduction and possibly some discussion regarding the uncertainties in the Conclusions and Discussion section.

P. Mathiot and co-authors: The location of this section was a long debate between co-authors during the writing phase of the submitted version. We think, it is better to keep the description of the current knowledge of the fwf evolution during the early Holocene around Antarctica at the proposed location and also the related discussion, because it is important to give the leeway authorized by the literature before discussing simulations, especially results from varFWF and FWF simulations. Here, the section allows also to compare the reference forcing field used for fwf in the Southern Ocean to the current estimate of fwf evolution. Consequently, we decide to keep this section at this place with some minor modifications to highlight the leeway given by all the uncertainties on the modellised and observed fwf. The end of this section is now: **"***It is therefore difficult to faithfully assess changes in fwf due to WAIS melting between 10 and 8 ka from the existing data. The uncertainties on timing and melting rate are thus large enough to justify the study, with an Earth climate model of intermediate complexity such as LOVECLIM, of how modifications of this fwf can affect the early Holocene SH high latitude climate, and which fwf amount leads to the best consistency between the simulated and reconstructed temperature patterns. We are fully aware that all the results are probably model dependent and subject to many limitations due to the model selected resolution, physics, forcings, the data assimilation method and the target data, as discussed in more details below. "*

3. Results

C24 (p 5557 l9-25): These three paragraphs are part of the same story and should thus be merged into a single one. I understand this result agrees with Renssen et al (2010) but not with Renssen et al (2005); what is the reason for this discrepancy?

P. Mathiot and co-authors: This result agrees with Renssen et al. (2010) and not with Renssen et al. (2005) because in Renssen et al. (2005), there is no fresh water flux applied in northern hemisphere. Renssen et al. (2010) show that the impact of the injection of the fwf due to Laurentide ice sheet in the ocean overwrite the impact of local insulation and long term memory effect described in Renssen et al. (2005). Some precisions have been added in the introduction :

"Using an intermediate complexity ocean-sea ice-atmosphere model whitout fresh water flux (fwf) forcing due to ice sheet melting, Renssen et al. (2005) showed that the long-term SH high latitude temperature trend during the Holocene (9 ka to present) can be explained by a combination of a delayed response of the Southern Ocean – Antarctic climate to local orbitally-driven insolation changes, modulated by the memory of the system. … .Such bipolar seesaw mechanism inducing austral warmth

may be driven by the impact of the final Laurentide meltwater flux on the Atlantic Meridional Overturning Circulation. Additionally, changes in the intensity of convection in Labrador Sea could also influence high Southern Latitudes through advective oceanic connections (causing then delayed temperature changes of the same sign in both hemispheres, Renssen et al. 2010) and could overwhelm the effect of the bipolar seesaw in the case of shut down of the Labrador Sea deep water formation. This could ultimately dominate the impacts of local insolation changes suggested by Renssen et al. (2005) and drive Southern Ocean climate evolution (Renssen et al, 2010)."

C25 : I understand part of the explanation could be a "wrong choice of fwf" as stated in the text, but I think the authors could be a bit more explicit.

P. Mathiot and co-authors: The example have been deleted because we already deals with this issue in this study. About mechanism linked with the local fwf or distant fwf, we briefly described the mechanism and give the best references in the introduction. Furthermore, following the suggestions of the referee 1 we have added other potential reason to explain the deficiency of the STD simulation. However, the details and the importance of each mechanism is out of the scope of this paper. The new paragraph is : *"The climate simulated in STD experiments is thus not consistent with data. This might be due to several processes such low frequency internal variability of the system not well taken into account by the model, to inadequate model physics that do not allow a correct response to the forcing, or the realism of the model forcing itself. "*

C26 : Finally, this discussion on the model-data comparison would require taking into account the variability/uncertainty of model and data, to ascertain the significance of the differences. If this is not feasible it should be explained why.

P. Mathiot and co-authors: About the significant of the result, as suggested by reviewer 1, a 99% Student test significant difference is applied on figure 2,3,5 and 6. As the large majority of the changes are significant, only minor modifications have been done throughout the manuscript.

C27 (p 5559, l3): I assume here "AMS" should be "ATM".

P. Mathiot and co-authors: DONE

C28 (p5561, l 20): Replace "increase" by "change" (ATM actually leads to a decrease).

P. Mathiot and co-authors: As change is a vague statement, we decide to modified the sentence like this:

“The changes in surface air temperature due to modifications in atmospheric circulation or due to the cooling of oceanic surface temperatures are associated with a decrease (for both simulations with reference fwf, ATM and STD) and with an increase (for both simulations with modified fwf, FWF and ATMFWF) in sea ice concentration and sea ice duration (Figure 6), the two variables for which proxy information is available.”

Conclusions:

C29 : After a big effort regarding the experimental design, the results and also Conclusions section appear quite short in comparison to the previous sections. I think the main value of this manuscript is that it provides a new means of constraining mechanisms that might have been relevant to explain past climate changes. I think the authors should try to discuss a bit more the relevance of their study, the outlook and caveats. In this line, I am not sure that the present exercise can really pinpoint the relevant mechanism. In this line, the combination of data assimilation with a perturbed atmospheric circulation and Southern Ocean freshwater appears to yield the best result in terms of the RMSE, but the experiments carried out are insufficient. For example, different magnitudes of the freshwater fluxes could provide a different impact capable of reducing the RMSE as well. I understand this is out of the scope of this paper, but addressing some of these caveats would be valuable.

P. Mathiot and co-authors: The conclusion has been largely rewritten. We now described the main results in a first section, then the limitation at the end. We have expended the discussion to take into account the limitations in method and model. About the fact that “different magnitude of the fwf could provide a different impact capable of reducing the RMSE”, the most important now is not the magnitude, because the experiment varFWF evaluates the difference model/proxy for a large range of fwf. We think the main point is more about location of fwf input. About EAIS, it is quite clear that the flux is minor compare to WAIS as said in the introduction. In NH exact location and fwf amount could have could have large impact on AMOC and water masses properties in the NH as well as in the SH via N/S teleconnection. Some words have been added to highlight this issue.

References:

C30 : I think the reference to Kim et al. (2012) is incorrect; the correct year for that manuscript is 2008. Kim et al (2012) is rather:

Kim, J.-H., X. Crosta, V. Willmott, H. Renssen, J. Bonnin, P. Helmke, S. Schouten, and J. S. Sinninghe Damste (2012), Holocene subsurface temperature variability in the eastern Antarctic continental margin, Geophys. Res. Lett., 39, L06705, doi:10.1029/2012GL051157.

P. Mathiot and co-authors: Thank you, we have added this reference.

Figures:

C31 : Figure 1 (already mentioned above in the specific remarks): as mentioned above, the magnitude of the temperature changes is of limited value without knowing the variability. Thus, and since this is the central issue of this manuscript, additionally showing some of the reconstructed time-series (as in Stenni et al. 2011) would be very helpful.

P. Mathiot and co-authors: See comments of C2.

C32 : Figures 2-3, 5: Please state differences are 10 ka minus 8 ka as done for figure 6.

P. Mathiot and co-authors: The differences plotted in figure 2, 3, 5 and 6 are 8 ka minus 10 ka. We have modified the figure 6 to be consistent with the figure 2-3, 5 and also with the description of simulation and acronym in part 2.4. The caption have been modified to clearly mention the convention of the plot (8 ka minus 10 ka).

C33 : Figure 6a: Replace REF by STD for consistence.

P. Mathiot and co-authors: DONE