

*'Including the efficacy of land ice changes in deriving climate sensitivity from paleodata'*  
by L.B. Stap, P. Köhler and G. Lohmann.  
Submitted for potential publication by Earth System Dynamics

## REPLY TO THE COMMENTS BY THE REVIEWERS

Color coding:

Black – comments by reviewers

Green – reply by authors

### Reviewer #1

In this paper the authors try to address a matter of importance — that concerning the efficacy of the different radiative forcing — and which is directly relevant to the ongoing efforts by various modelling and proxy analysis groups to estimate the planet's Equilibrium Climate Sensitivity (ECS). I like the idea of the paper and I am quite sure the paper will be accepted, but I feel there is need for clarity and additional analysis before the paper is in publishable form.

We thank the reviewer for a careful examination of our work. We are pleased that the reviewer likes the idea of the paper. In the revised manuscript, we have largely followed the provided comments to improve the clarity of the paper, and we have included additional analysis, as described below.

### **Points of broadest significance**

**Definition of the efficacy factor:** This paper builds upon the work by Hansen et al. 2005 and by PALEOSENS members (2012), but the way the authors introduce the efficacy factor in equations (8) and (9) is different from those employed in these other works. For example, according to the PALEOSENS approach, equation (9) should be expressed as (See sample calculation in PALEOSENS supplementary materials section B.2):

$$S_{[CO_2,LI]}^\varepsilon = \frac{\Delta T_g}{\Delta R_{CO_2} + \Delta R_{LI}} = \frac{\Delta T_g}{\Delta R_{CO_2} + \varepsilon_{LI} \Delta R_{CO_2}}$$

This says that the efficacy of the radiative forcing from land ice changes,  $\Delta R_{[LI]}$  is related to the equivalent radiative forcing from changes in  $CO_2$  through is a fractional parameter  $\varepsilon_{[LI]}$ . This is what the efficacy is meant to serve: to help assess the radiative forcing from non-greenhouse gas sources by relating it to the better constrained forcing from  $CO_2$ . But the way the authors are using  $\varepsilon_{[X]}$  is quite strange and it doesn't make sense to me. It doesn't appear to be a typographical mistake. The climate sensitivity world is already overflowing with numerous different formulations and I think there should be a very good reason (and which should be made extremely clear in the paper) to define an existing concept differently.

We would like to argue that the way we have implemented the efficacy factor in our approach is the most natural extension to (our) earlier studies. Indeed, the PALAEOSENS approach, which we have used so far, employs:

$$S_{[CO_2,LI]} = \frac{\Delta T_g}{\Delta R_{[CO_2]} + \Delta R_{[LI]}}.$$

Mind though: no superscript  $\varepsilon$  here, because efficacy differences are not considered.

In Köhler et al. (2010), radiative forcing records over the past 800 kyr of many different processes including CO<sub>2</sub> and land ice changes, were analyzed. This is not the issue we consider in this manuscript. Instead, we try to overcome the problem that the strength of the response of global-average temperature to global-average radiative forcing can be different, depending on the generating process (in this case land ice changes or CO<sub>2</sub> changes), so in general:

$$\frac{\Delta T_{[CO_2]}}{\Delta R_{[CO_2]}} \neq \frac{\Delta T_{[LI]}}{\Delta R_{[LI]}}.$$

In our opinion, the most logical approach to include this difference in efficacy is to multiply the radiative forcing of land ice changes by an appropriate factor so that the strength of the temperature response is the same as when CO<sub>2</sub> would be the generating process. Indeed, Hansen et al. (2005) compared the effects of several different processes, expressing the efficacy of these processes X as:

$$\varepsilon_{[X]} = \frac{\Delta T_{[X]}/\Delta R_{[X]}}{\Delta T_{[CO_2]}/\Delta R_{[CO_2]}}, \text{ so in our case: } \varepsilon_{[LI]} = \frac{\Delta T_{[LI]}/\Delta R_{[LI]}}{\Delta T_{[CO_2]}/\Delta R_{[CO_2]}}.$$

Our implementation,

$$\frac{\Delta T_{[LI]}}{\Delta R_{[LI]}} = \varepsilon_{[LI]} \frac{\Delta T_g - \Delta T_{[LI]}}{\Delta R_{[CO_2]}},$$

follows this approach very closely. The only difference is that we relate the effect of land ice changes (left hand side) to the effect of all processes except land ice changes (right hand side), because we calculate specific climate sensitivity  $S_{[CO_2,LI]}^\varepsilon$ , which does not account for the effect of these other processes.

In principal, it is also possible to relate the impact of land ice changes on global temperature directly to  $\Delta R_{[CO_2]}$ , as the reviewer proposes:

$$S_{[CO_2,LI],alt}^\varepsilon = \frac{\Delta T_g}{\Delta R_{[CO_2]} + \varepsilon_{[LI],alt} \Delta R_{[CO_2]}},$$

where the efficacy factor in this alternative case ( $\varepsilon_{[LI],alt}$ ) relates to the one used in our approach ( $\varepsilon_{[LI]}$ ) as:

$$\varepsilon_{[LI],alt} = \varepsilon_{[LI]} \frac{\Delta R_{[LI]}}{\Delta R_{[CO_2]}}.$$

However, from the records of  $\Delta R_{[CO_2]}$  and  $\Delta R_{[LI]}$  of our dataset, we infer a non-linear relationship between these two quantities (see the figure below, included as the new Fig. 2 in the revised manuscript). This would introduce a cumbersome state dependency of  $\varepsilon_{[LI]}$ , which is avoided by our approach. This has now been elaborated upon in the revised manuscript.

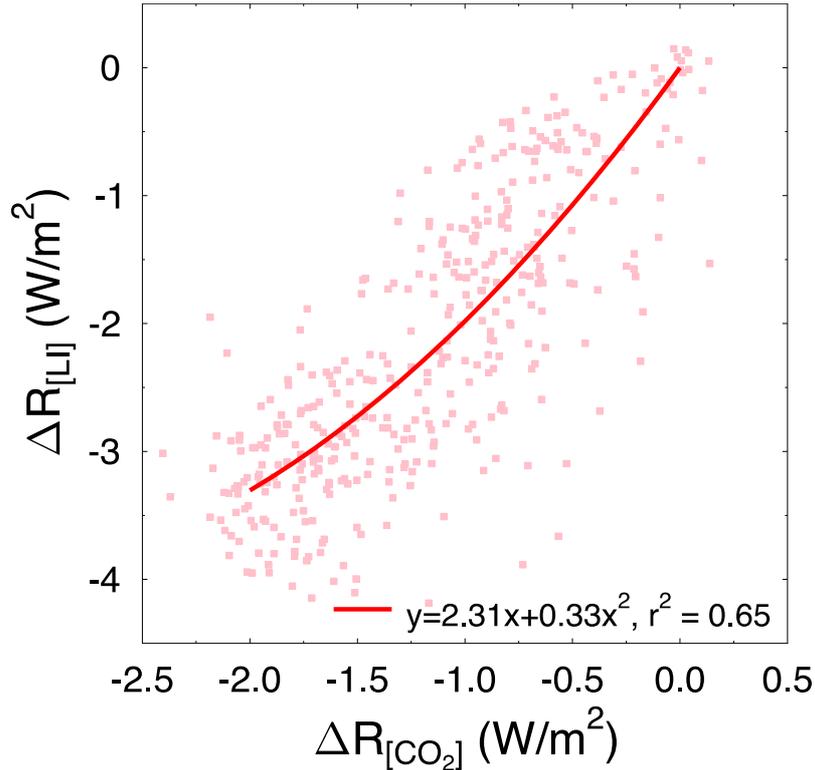


Figure: Relation between radiative forcing anomalies caused by CO<sub>2</sub> changes ( $\Delta R_{[CO_2]}$ ) and land ice changes ( $\Delta R_{[LI]}$ ) from the proxy-inferred dataset (pink dots). The red line represents a second order polynomial least-squares regression through the scattered data.

Furthermore, for clarification we have split the method section (Section 2.1) into three parts in the revised manuscript, describing 1) the PALAEOSENS approach used in earlier studies, 2) our main refinement: the inclusion of the efficacy of land ice changes, and 3) a small refinement that unifies the dependent variable in cross-plots of radiative forcing and global temperature anomalies.

**Regarding the sample calculations from CLIMBER experiment:** The authors try to apply their new formulation to compute  $S_{[CO_2,LI]}^\varepsilon$  from their CLIMBER data and compare it to  $S_{[CO_2,LI]}$  that they have previously found. Using

$$S_{[CO_2,LI]} = \frac{\Delta T_g}{\Delta R_{[CO_2]} + \Delta R_{[LI]}}$$

the authors found  $S_{[CO_2,LI]}$  to be 0.54. This formulation uses  $\Delta T_g$ ,  $\Delta R_{[CO_2]}$  and  $\Delta R_{[LI]}$  all of which are available from their CLIMBER models (and shown in Fig 1). Their new formulation  $S_{[CO_2,LI]}^\varepsilon$ , after substituting for  $\varepsilon_{[LI]}\Delta R_{[LI]}$  from equation (11) into equation (9) reduces to

$$S_{[CO_2,LI]}^\varepsilon = \frac{\Delta T_g - \Delta T_{LI}}{\Delta R_{CO_2}}$$

in which all the terms are again derived from their CLIMBER models, the only difference from the original expression is that instead of  $\Delta R_{[LI]}$  the new expression uses  $\Delta T_{[LI]}$ . I am quite confused why the new approach using temperature from land ice changes, instead of radiative forcing due to land ice changes, (both from the same set of models), and leading to a higher inferences of S is to be favoured (a sentiment expressed at the start of page 8)?

The goal of this section is to validate our refined approach by applying it to the idealized CLIMBER-2 simulations. Here, the effect of CO<sub>2</sub> is a-priori known from the results of experiment OC:

$$S_{[CO_2,LI]}^\varepsilon = \frac{\Delta T_{OC}}{\Delta R_{[CO_2]}}.$$

This result functions as the target for our approach of obtaining the sole effect of CO<sub>2</sub> changes on global temperature from the results of experiment OIC, where land ice cover and CO<sub>2</sub> levels are both varied over time. Our refined approach considers the efficacy of land ice changes:

$$S_{[CO_2,LI]}^\varepsilon = \frac{\Delta T_g}{\Delta R_{[CO_2]} + \varepsilon_{[LI]}\Delta R_{[LI]}}.$$

We calculate the efficacy factor  $\varepsilon_{[LI]}$  as:

$$\varepsilon_{[LI]} = \frac{\omega}{1 - \omega} \frac{\Delta R_{[CO_2]}}{\Delta R_{[LI]}},$$

where

$$\omega = \left. \frac{\Delta T_{[LI]}}{\Delta T_g} \right|_{\text{spec. time}}.$$

Note here that the parameter  $\omega$  is obtained from temperatures at a specific time (for instance, the LGM), constituting the assumption that  $\varepsilon_{[LI]}$  is constant in time. Therefore, the simplification that the reviewer makes by substituting equation (11) into equation (9) is not generally valid. Otherwise, the refined approach would indeed by construction always yield the target value for  $S_{[CO_2,LI]}^\varepsilon$  (apart from a negligible contribution by the synergy of CO<sub>2</sub> and land ice changes). Instead,  $S_{[CO_2,LI]}^\varepsilon$  is only matched by construction at the LGM. The comparison we make between our approach and the target, provides a quantification of the error yielded by assuming a time-invariant  $\varepsilon_{[LI]}$ , which has been clarified in the revised manuscript (see also our answer to the next general comment).

We do not favour a higher or lower value for  $S_{[CO_2,LI]}^\varepsilon$ , but the fact that our refined approach gives a quantification of  $S_{[CO_2,LI]}^\varepsilon$  that is much closer to the target, stresses the importance of including efficacy differences.

**Constant  $\varepsilon_{[LI]}$ :** The authors have talked a lot about the state dependency of  $S_{[CO_2,LI]}^\varepsilon$ , but they have barely discussed the state dependency of  $\varepsilon_{[LI]}$ , which is the bread and butter of this paper. After all,  $\varepsilon_{[LI]}$  will likely depend on state and it can be readily computed for either their numerical model or the paleo data using their equation (11) and therefore the variability can be assessed in the manuscript. The conclusion says “the assumption that the efficacy factor is indeed constant in time could be tested more rigorously using more sophisticated climate models”, but it can be tested in this manuscript using the models and data they are already employing. Furthermore, in the absence of this analysis, the usage of LGM specific  $\varepsilon_{[LI]}$  in calculations, and which is applied as a constant value to the entirety of the Pleistocene time series makes the analysis look very contrived. The reader does not know, if the results change a lot if  $\varepsilon_{[LI]}$  is derived, from say MIS5 and then kept constant for the entire interval of analysis? So the range of changes in  $\varepsilon_{[LI]}$  and the dependence of principle results on that should be included in the manuscript.

As explained in the answer to the previous general comment of the reviewer, the analysis of the CLIMBER-2 results gives a quantification of the error made by assuming the efficacy factor to be constant in time. This is now explained more clearly in the revised manuscript. CLIMBER-2 is, however, not the most advanced model around; the results are very linear (small synergy of the effects of land ice and CO<sub>2</sub> changes), and important long-term feedbacks such as dust and non-CO<sub>2</sub> greenhouse gas changes are ignored in the simulations we analyze. We therefore maintain the sentence stating that the assumption of a time-constant efficacy factor can be investigated more rigorously using results of more sophisticated models. We have moved this sentence to the section where we present and discuss the CLIMBER-2 results.

So far, we derived  $\varepsilon_{[L]}$  using data from the LGM, because this is a well-studied time slice, that we also use in the analysis of our proxy-inferred dataset. In principal, however,  $\varepsilon_{[L]}$  can be obtained using data from any moment in time. Preferably, the radiative forcing anomalies should be large to prevent outliers resulting from divisions by small numbers, making MIS5 a less suited candidate. Instead, we now include an extra analysis of the CLIMBER-2 results, where we obtain  $\varepsilon_{[L]}$  from the mean value of all glacial marine isotope stages of the past 810 kyr (MIS 2, 4, 6, 8, 10, 12, 14, 16, 18, and 20). We find an  $\varepsilon_{[L]}$  of  $0.56 \pm 0.09$  and a corresponding PI  $S_{[CO_2,LI]}^\varepsilon$  of  $0.73^{+0.06}_{-0.05}$  K W<sup>-1</sup> m<sup>2</sup>.

**Section 3.3:** A big shortcoming of this manuscript is section 3.3 which is extremely convoluted and difficult to follow. For an otherwise relatively clearly written paper, this section seems to have been put together haphazardly without the attention to detail that makes the rest of the paper readily readable. Though I have made a couple of specific comments for this section further down in my review, in general I have not been able to follow this section at all and therefore have not been able to provide the quality of feedback that I would have liked. A careful re-writing of this section by the authors is required.

We understand from the comments of this reviewer and reviewer #2, that Sect. 3.3 was not as easily understandable as we had hoped upon submission of the manuscript. During the process of revising the manuscript, we have come to the realization that this section only served as a further illustration of the importance of the effect of land ice changes that is already found in Sect. 3.2. As such, it is not essential to the main storyline of our manuscript. To improve the clarity of the paper as a whole, we have therefore decided to remove it from the manuscript.

## Scientific comments

1. The various sensitivities are quotes in two different units throughout the paper: K per doubling of CO<sub>2</sub> and K W<sup>-1</sup> m<sup>2</sup>. While the authors have been generally very clear about the units and about converting between them, as is the case on page 8, I do encourage them to use only one unit throughout the paper. This helps a reader to quickly compare various numbers from across the paper without having to convert the units. Alternatively, the authors could quote all sensitivities in both units, example: “so and so sensitivity was found to be 1.66 K W<sup>-1</sup> m<sup>2</sup> or equivalently 5.6 K per doubling of CO<sub>2</sub>” (similar to the last sentence in the conclusions section).

As we now explain in the method section, we express  $S^a$  in K W<sup>-1</sup> m<sup>2</sup> and ECS in K per doubling. We now convert  $S_{[CO_2,LI]}^\varepsilon$  to both quantities, and quote them conjointly.

2. Sentence spanning lines 9–10 on page 3: I don't understand what is meant by this sentence, specifically by the part "it has been shown that simulations of models that have been integrated over a few centuries are not yet in equilibrium". Perhaps rephrasing this sentence could make it clearer.

We have removed this sentence from the manuscript, since it was not essential to - and therefore distracting from - the storyline.

3. Line 10 page 3: Regarding ECS the authors say "Another way to express" but no other way has been previously mentioned until that point in the article. The ECS has only been defined up to that point. I think it makes more sense to rephrase it as "One way to express".

This sentence has been rephrased. ECS is expressed in K per doubling, and  $S^a$  in  $K W^{-1} m^2$ . They relate to each other as:  $ECS = S^a * 3.7 W m^{-2}$ . This has now been made clear in the revised manuscript.

4. Since the form of "f" is important for the rest of the paper, the authors should clearly articulate the motivations for f as given in equation 5.

Equation 5 is part of the PALAEOSENS approach that has been used so far in numerous publications, and as such is not an equation we propose here. This has been made clear in the revised manuscript by splitting the method section into three parts (see our answer to the first general comment of the reviewer). The idea of the PALAEOSENS approach was that the influence of long-term processes on global temperature is directly proportional to the radiative forcing perturbation they induce, as is now mentioned in the revised manuscript.

5. Last para, page 4: So is  $S_{[CO_2,L]}$  to be considered as an estimate of  $S^a$ ? Maybe the authors should clarify this explicitly. In the process of making this clarification the starting sentence of that paragraph will likely need to be modified to make the argument fit in seamlessly.

No, we obtain an estimate for  $S^a$  by multiplying  $S_{[CO_2,L]}^E$  by 0.64. This has now been clarified in the method section.

6. In the first paragraph on page 5 the authors say that they take a "further simplifying step" to more easily compare " $S_{[CO_2,L]}^E$  to other specific paleoclimates sensitivities  $S_{[CO_2,X]}^E$  by unifying the dependent variable". But all they have done is move the specific dependent variable  $\Delta R_{[X]}$  into the newly defined  $CO_2$ -equivalent temperature and which doesn't in any way free someone of the need to compute that forcing or to compute the efficacy factor. So I fail to see the simplification here (besides a notational one) but more importantly I fail to see the practical usefulness. For any given  $S_{[CO_2,L]}$  by the time one has computed the  $CO_2$ -equivalent temperature, they might as well have just used equation 9.

We realize now that calling this step 'simplifying' was somewhat confusing. In the revised manuscript, we have made a separate subsection describing this small refinement, which serves to unify the dependent variable in cross-plots of radiative forcing and global temperature anomalies. This makes our calculated  $S_{[CO_2,L]}^E$  more readily comparable to other

specific paleoclimate sensitivities, where more and/or different long-term processes are considered. We describe the newly introduced variable now more accurately as the global temperature change (with respect to PI) stripped of the inferred influence of processes X ( $\Delta T_{[-X]}$ ), in our case land ice changes ( $\Delta T_{[-LI]}$ ).

7. First para, page 6: In the experiments OC, and OI, which as I understand are meant to assess the effects of land ice and CO<sub>2</sub> respectively, why are the orbital conditions also varied in conjunction? It seems that the authors answer this later on in the manuscript, at the beginning of section 3.1: “since the influence of orbital variations is very small”. That comment should be moved closer to where these experiments OC and OI are discussed.

This comment has been moved to the description of the model data, as suggested by the reviewer.

8. Line 21, page 6: “ANICE was forced by northern hemisphere temperatures obtained...” Northern hemispheres temperature or temperature anomaly? I think it should be the anomaly.

ANICE was indeed forced by the anomaly. We thank the reviewer for this careful observation.

9. Page 6: regarding the discussion of the amplification factor for the Pliocene, new results coming from the revised paleo-geographic boundary conditions for PlioMIP2 (Kamae et al. 2016; Chandan and Peltier 2017; Hunter et al. 2019) that suggest that the amplification factor could have been larger. Models that were used in the previous PlioMIP and whose results were synthesized in Haywood et al. 2013 were consistently failing to produce the polar amplification that has been inferred from proxies. With the new results the polar amplification factor in the warm interval of the Pliocene is nearly the same as the amplification factor during the cold LGM. The authors should and cite the new papers add a comment/analysis regarding how the revised amplification factor for the warm interval affects their results.

In the revised manuscript, we have included an appendix, in which we analyze the same proxy-data inferred dataset, but using a constant polar amplification factor of 2.7 over the past 800 kyr ( $\Delta T_{g2}$  in Köhler et al. 2015). This is in our opinion a very interesting addition to our manuscript, but it does not affect the main results qualitatively.

10. Lines 15-17 page 7: the authors say they are inferring  $S^{\varepsilon}_{[CO_2,LI]}$  or  $S^{\varepsilon}_{[CO_2]}$  here but I think a bit of additional comment is required to clarify the appearance of  $\varepsilon$  in these sensitivities. These are after all inferred from experiment OC in which  $\Delta R_{[LI]}$  is zero, so the meaning of land-ice radiative efficacy  $\varepsilon$  is not strictly defined. This is probably hair-splitting over notation but I think it is best to be as clear as possible since the climate sensitivity literature is already overflowing with (sometimes sloppily used) notation.

As the reviewer rightly points out, in the case of  $\Delta R_{[LI]}=0$ ,  $\Delta R_{[LI]}$  and  $\varepsilon_{[LI]}$  have no effect on  $S^{\varepsilon}_{[CO_2,LI]}$ , so  $S^{\varepsilon}_{[CO_2,LI]} = S^{\varepsilon}_{[CO_2]} = S_{[CO_2,LI]} = S_{[CO_2]}$ , which is now indicated in the revised manuscript.

11. Line 1, page 8: “the new approach considering efficacies clearly leads to a more satisfactory result than the old approach.” In the present form this sentence implies that for some reason the numerical value 0.74 is more satisfactory than the older value of 0.54. I am

not sure if that is defensible or even that the authors themselves meant to imply that. I think the authors meant to say something like “the new approach is more flexible/accommodating/physically accurate than the old approach”. Please re-phrase this accordingly.

This has been rephrased, because calling the results ‘more satisfactory’ could let readers believe we have a certain preference for a lower or higher result, which we of course do not have. We meant to say the new result ( $0.72 \text{ K W}^{-1} \text{ m}^2$ ) is much closer to the target value of  $0.74 \text{ K W}^{-1} \text{ m}^2$  than the result of the old approach, stressing the importance of including efficacy. This has been clarified in the revised manuscript.

12. Lines 13–15, page 8: The authors have presented two results which lead to opposite conclusions. This needs to be addressed here directly instead of referring the reader to another publication. While the issue may have been more thoroughly assessed in Köhler et al. 2018, a brief comment should also be provided here so that the reader grasps the discordance in the author’s results at a bare-minimum level without having to read up another paper.

A brief explanation of this result has been included in the revised manuscript.

13. Line 9, page 8: For the calculation of  $\varepsilon_{\text{[LI]}}$  using equation 11 please provide the values of  $\Delta R_{\text{[CO}_2\text{]}}$  and  $\Delta R_{\text{[LI]}}$  at LGM that were used.

The LGM values ( $\Delta R_{\text{[CO}_2\text{]}} = -2.04 \text{ W m}^{-2}$  and  $\Delta R_{\text{[LI]}} = -3.88 \text{ W m}^{-2}$ ) are now provided. Upon including them and redoing the calculations, we realized we made a small mistake in the calculation of  $\varepsilon$  in the former section 3.2, and the corresponding  $S^{\varepsilon}_{\text{[CO}_2\text{,LI]}}$ . This has been corrected in the revised manuscript. We thank the reviewer for letting us double-check our calculations.

14. Line 15, page 8: is the mean the value “of” years 20 and 22 kya or “between” those years?

The temporal resolution of this dataset is 2,000 years, so we have values for 20 and 22 kyr ago. In that sense, it is indeed the mean ‘of’ these times. This has now been clarified.

15. Line 16, page 8: “The specific paleo climate sensitivities we find here are generally higher than calculated by the old approach” But the new sensitivity calculated is 1.39 which is lower than that by the old approach which was 1.66.

The new sensitivity of  $1.39 \text{ K W}^{-1} \text{ m}^2$  (revised to  $1.45 \text{ K W}^{-1} \text{ m}^2$ , see our answer to scientific comment #13) holds for the LGM, and should be compared to  $0.93 \text{ K W}^{-1} \text{ m}^2$  obtained by the old approach. The PI sensitivity of  $1.66 \text{ K W}^{-1} \text{ m}^2$  of the old approach should be compared to our new PI sensitivity of  $2.45 \text{ K W}^{-1} \text{ m}^2$ . This has now been clarified in the revised manuscript.

16. Line 12 page 9: “We correct the induced  $\Delta T_{\text{[CO}_2\text{]}}$  of all individual models for this ratio” I don’t follow.

17. Line 15 page 9: At this point I am lost. Why are you doing that regression? What is the motivation? And are you subtracting the global value  $\Delta T_{\text{[CO}_2\text{]}}$  from  $\Delta T_{\text{NH}}$ ?

18. The ECS given in Table 1 for the CCSM4 model is different from that usually cited. Bitz et al. 2012 using the NCAR-CCSM4 and recently Chandan and Peltier, 2018 using a related UofT-CCSM4 have deduced the ECS to be 3.2. The value in Table 1 is lower than that. Where did the authors get this from? Haywood et al. 2013 also use CCSM4 ECS (from Bitz et al) of 3.2. Do the numbers for the other models need to be checked as well?

19. The authors should cite all the original experiment design papers for the PMIP3 experiments listed in Table 1. This can be done readily by adding a new column to the table called "References".

Answer to points 16 to 19: Section 3.3 has been removed from the manuscript, see our answer to the fourth general comment of the reviewer.

20. The figure description for Figure 3 is completely wrong. It is talking about things that are not on the figure.

Figure 3 and its caption have been corrected.

## Technical comments

We are very grateful for these technical comments by the reviewer. We have implemented all the suggestions, except where indicated.

1. Line 2 page 1: "~~with~~ to equilibrium"

This sentence has been rewritten completely.

2. Line 29 page 2: "are obtained from ~~different~~ various model setups"

This sentence has been rewritten completely.

3. Line 16 page 3: "In this case, the ~~average~~ global paleo temperature anomaly with respect to the pre-industrial (PI) ~~average~~ ( $\Delta T_g$ ) is"

4. Line 17 page 3: "that are typically neglected in ~~the~~ climate simulations".

5. Lines 3-4 on page 4 incorporating the phrase "the calculated paleoclimate sensitivity" in the current form refers to some specific and as yet undefined sensitivity. It's best to rephrase it as "If, for instance, only the most important slow feedback in the climate system, namely radiative forcing anomalies induced by albedo changes due to land ice (LI) variability are taken into account, then one can correct  $S^p$  to derive the following specific paleoclimate sensitivity."

6. The sentence on line 5, page 4, appears as a sharp interruption to the logic train before and after that sentence. It should instead be placed at the end of that paragraph and rephrased as "~~An overview~~ A synthesis of ~~different values~~ estimates of  $S_{[CO_2,LI]}$  ~~for~~ from both ...."

7. Line 15 page 4: "~~e.g. because~~ because, e.g."

8. Line 18 page 4: "through efficacy factors ( $\epsilon_{[LI]}$ ), ~~which demands~~. This requires a reformulation"

9. Line 20 page 4: "to clearly distinguish ~~them~~ the sensitivities from ~~the former ones~~ those of the PALAEOSENS project, in which the radiative forcing of the different processes ~~had identical weights~~ were assigned identical efficacies."

This sentence has been rephrased to:

'This serves to clearly distinguish these newly-derived sensitivities from those of the PALAEOSENS project in which efficacy was not taken into account, implying that identical radiative forcing of different processes leads to identical temperature changes.'

10. Line 25 page 4: "by land ice changes ( $\epsilon_{[LI]}$ ), using ~~a slightly different definition than~~ the following formulation which is based on, but modified from Hansen et al. (2005)"

11. Line 22 page 5: The sentence "CLIMBER-2 combined a 2.5 statistical-dynamical..." seems something is missing after 2.5. Did the authors mean "2.5 degree"?

Corrected to '... 2.5-dimensional ...'

12. Line 5 page 5: Add comma after "Similarly"

Corrected to 'As before, ...'

13. Line 3 page 5: "leaving 217 data points as indicated in Fig 1c,d."

14. Sentence beginning on line 18 page 7: change it to something like "For our first attempt at compensating paleoclimates sensitivity for slow processes other than CO<sub>2</sub> changes we strive to deduce the same  $S^{\epsilon}_{[CO_2,LI]}$ , inferred above, from experiment OIC in which both CO<sub>2</sub> and land ice cover vary over time".

This sentence has been rewritten as:

'Now, we apply our approach to the results of experiment OIC, in which both CO<sub>2</sub> and land ice cover vary over time, with the aim of deducing the sole effect of CO<sub>2</sub> changes on global temperature.'

15. Line 23 page 7: "Between .... ~~there are~~ some ~~outlying values caused by~~ outliers resulted from division ~~of~~ by small numbers (not shown on Fig. 2b)."

16. Line 29 page 7: "...is more linear than that ~~of~~ between ..."

17. Line 31 page 7 "~~in the simulated domain~~ through the entire 5 million year interval."

18. Line 11 page 8: "~~Similarly as before (Köhler et al., 2018), we detect~~ Similar to Köhler et al., 2018, we too detect"

19. Line 15 page 8: the value of  $\Delta R_{[CO_2]}$  should be -2.04

20. Line 15 page 8: "the LGM value (~~here taken~~ taken here as the mean...)"

21. Line 21 page 8: "we first scale ~~them~~ it by a factor"

This sentence has been removed.

22. Line 23 page 8: "Note that this scaling still assumes unit efficacy for ~~all other~~ process other than land ice changes"

This sentence has been corrected as suggested by the reviewer, and replaced to the method section.

23. Line 24 page 8: "Then, after ~~After~~ multiplying by"

This sentence has been removed.

24. Line 24 page 8: Units should be  $\text{Wm}^{-2}$

This sentence has been removed.

25. Line 11 page 9: “that the ratio of the radiative forcing change  $\Delta R_{\text{CO}_2}$  between the LGM (185 ppm  $\text{CO}_2$ ) and the PI (280 ppm  $\text{CO}_2$ ), to the change between the PI and ~~2-x  $\text{CO}_2$~~  a 2 X PI case is

26. Line 16 page 9: “significant ~~on~~ at the 95% level”

Answer to points 25 and 26: Section 3.3 has been removed from the manuscript, see our answer to the fourth general comment of the reviewer.

27. Conclusions section, Lines 26, 28, 30:  $\epsilon_{[\text{CO}_2, \text{LI}]}$  is a new symbol not previously defined. It seems like a mistake and the authors likely meant  $\epsilon_{[\text{LI}]}$

28. The yellow star in Fig 4 is barely visible against the cyan background. Please change it to something dark, maybe black.

All the colors in Fig. 4 have been changed for better visibility.

#### REFERENCES:

Forster, P. M., Andrews, T., Good, P., Gregory, J. M., Jackson, L. S., and Zelinka, M.: Evaluating adjusted forcing and model spread for historical and future scenarios in the CMIP5 generation of climate models, *Journal of Geophysical Research: Atmospheres*, 118, 1139–1150, 2013.

Haywood, A. M., Hill, D. J., Dolan, A. M., Otto-Bliesner, B. L., Bragg, F., Chan, W.-L., Chandler, M. A., Contoux, C., Dowsett, H. J., Jost, A., et al.: Large-scale features of Pliocene climate: results from the Pliocene Model Intercomparison Project, *Climate of the Past*, 9, 191–209, 2013.

Hansen, J., Sato, M. K. I., Ruedy, R., Nazarenko, L., Lacis, A., Schmidt, G. A., Russell, G., Aleinov, I., Bauer, M., Bauer, S., et al.: Efficacy of climate forcings, *Journal of Geophysical Research: Atmospheres*, 110, 2005.

Köhler, P., Bintanja, R., Fischer, H., Joos, F., Knutti, R., Lohmann, G., and Masson-Delmotte, V.: What caused Earth’s temperature variations during the last 800,000 years? Data-based evidences on radiative forcing and constraints on climate sensitivity, *Quaternary Science Reviews*, 29, 129–145, <https://doi.org/10.1016/j.quascirev.2009.09.026>, 2010.

Köhler, P., de Boer, B., von der Heydt, A. S., Stap, L. B., and van de Wal, R. S. W.: On the state-dependency of the equilibrium climate sensitivity during the last 5 million years, *Climate of the Past*, 11, 1801–1823, 2015.

Stap, L. B., van de Wal, R. S. W., de Boer, B., Köhler, P., Hoencamp, J. H., Lohmann, G., Tuenter, E., and Lourens, L. J.: Modeled influence of land ice and  $\text{CO}_2$  on polar amplification and paleoclimate sensitivity during the past 5 million years, *Paleoceanography and Paleoclimatology*, 33, 381–394, 2018.

## **Reviewer #2**

My initial thoughts on seeing this paper were very positive in the sense that, given the uncertainty of information in the paleorecord, and the difficulty of using state of the art models to make very long runs, all progress in the area of better defining climate sensitivity as it relates to past climates is worthwhile.

My optimism remained through the first parts of the paper, but by the end I have to admit that I am lost and really do not understand what the authors are trying to do and what they have discovered.

We thank the reviewer for considering our work. We are pleased the reviewer sees merit in the aim of our study. Along the helpful comments provided, we have thoroughly rewritten and restructured the manuscript to get our message across more clearly.

Most importantly we have improved the readability of the manuscript in the following manners:

- We have removed Section 3.3 from the manuscript, because this section only served as a further illustration of the importance of the effect of land ice changes that is already found in Sect. 3.2. As such, it is not essential to the main storyline of our manuscript.
- We have included a brief introduction to the method and results sections, in which we explain the aim of the section. The results sections end with a statement and discussion of the gained insights.
- We have split Sect. 2.1 (the method section) into three parts, describing 1) the PALAEOSENS approach used so far in earlier studies, 2) our main refinement: the inclusion of the efficacy of land ice changes, and 3) a small refinement that unifies the dependent variable in cross-plots of radiative forcing and global temperature anomalies.
- We have relocated Sects. 2.2.1 and 2.2.2, so that first the modelling results are introduced and analyzed straight away, and thereafter the proxy-inferred dataset is introduced and analyzed.
- We have renamed the variable 'CO2-equivalent temperature change' ( $\Delta T_{\text{CO2-equiv}}$ ). In the revised manuscript, we have more accurately named it 'the global temperature change (with respect to PI) stripped of the influence of land ice changes ( $\Delta T_{\text{[-LI]}}$ )'.

The authors introduce a variable,  $\text{DTe}[\text{CO2 - equiv}]$  but do not explain why this is useful or interesting.

The introduction of this variable, named  $\Delta T_{\text{[-LI]}}$  in the revised manuscript (see below), serves to unify the dependent variable in cross-plots of radiative forcing and global temperature anomalies. This makes our calculated  $S^e_{\text{[CO2,LI]}}$  more readily comparable to other specific paleoclimate sensitivities, where more and/or different long-term processes are considered. This step is a small refinement compared to our main refinement of including the efficacy of land ice changes. This has been clarified in the revised manuscript by splitting the method section into three parts.

What I would have done is take equation (9), replace X with LI and then explore all the elements of that equation. This would show us how S varies with DRLI and DR<sub>CO2</sub>, as well as DT<sub>g</sub> and one could consider how much of the state+forcing+efficacy dependence of S[CO<sub>2</sub>] is accounted for by considering land ice with and without considering efficacy.

This has been done in detail in Köhler et al. (2010) for the old approach (equivalent to  $\epsilon_{[LI]} = 1$  in the refined approach). The inclusion of an efficacy factor for land ice changes does not qualitatively change this analysis, it just linearly amplifies (when  $\epsilon_{[LI]} > 1$ ) or diminishes (when  $\epsilon_{[LI]} < 1$ ) the effect of radiative forcing by land ice changes. We therefore focus directly on the effect of  $\epsilon_{[LI]}$  on  $S^e_{[CO_2,LI]}$ .

I can see that Figures 2 and 3 represent some kind of sensitivity-like variable, but I cannot grasp its meaning.

Indeed, in Figures 2 and 3 we showed the main results of our manuscript: the influence of the deduced  $\epsilon_{[LI]}$  on  $S^e_{[CO_2,LI]}$ .

Basically, DTe[CO<sub>2</sub> - equiv] is not, as you suggest in equation 14 simply a function of DR<sub>CO2</sub> but also depends on T<sub>g</sub> and DRLI.

What we meant here is that we make a regression to the scattered data of  $\Delta R_{[CO_2]}$  and the variable DTe[CO<sub>2</sub> - equiv] (now called  $\Delta T_{[-LI]}$ ).  $\Delta T_{[-LI]}$  comprises the influences of  $\Delta R_{[LI]}$ ,  $\Delta R_{[CO_2]}$  and  $\Delta T_g$ . To clarify this, in the revised manuscript we have named this function *regfunc* (instead of *g*).

I hope that the remedy is a better explanation of the reasons behind the derivations in section 1 and also better explanation of the insight that you gain from the results.

Other points.

P1L10 "Recently, it has been shown that simulations of models that have been integrated over a few centuries are not yet in equilibrium, and from longer climate simulations a higher ECS can be deduced (Knutti et al., 2017)."

This needs rephrasing. It has been well known since before dynamical oceans were included in climate models that the equilibrium time of the ocean is of the order of thousands of years. Since the invention of the AOGCM, ad-hoc methods have been introduced to try to estimate equilibrium climate sensitivity without running the models to equilibrium. What recent work has been doing is assessing the accuracy of such approximations.

We have removed this sentence from the manuscript, since it was not essential to - and therefore distracting from - the storyline.

P2L23 "likewise as several earlier studies"

-> "as in several earlier studies"

This sentence has been removed.

Eq(10) This equation suggests to me that  $DTg - DT[LI] = DT[CO2]$ . Maybe I misunderstand, but it seems to me that  $DTg = DT[LI] + DT[CO2] + DT[X] + Z$ , where  $DT[X]$  is the influence of all the other forcings and  $Z$  represents cross terms (ie nonlinearities).

In this manuscript, we aim to calculate specific climate sensitivity  $S^e_{[CO2,LI]}$ , which only compensates paleoclimate sensitivity ( $S^p$ ) for the influence of land ice cover changes. To deduce the efficacy of land ice changes, we relate its effect on global temperature changes to that of all other processes combined. This is in line with our calculation of  $S^a$  from  $S^e_{[CO2,LI]}$  by multiplying by a factor of 0.64, which implies unit efficacy for all other processes than land ice changes. As stated in the text, this is a source of uncertainty to be investigated in future research.

P3L5 "Similarly as in the old approach," Not English

Corrected to: 'As before, ...'

Eq(13) Looks like a minus sign between "CO2" and "equiv".

We realize that calling this variable 'CO2-equivalent temperature change' ( $\Delta T_{[CO2-equiv]}$ ) was confusing. In the revised manuscript we have therefore more accurately named it 'the global temperature change (with respect to PI) stripped of the influence of land ice changes ( $\Delta T_{[-LI]}$ )'.

P5L12 "A functional relationship between  $TE[CO2-equiv]$  and  $R[CO2]$  ( $T[CO2-equiv] = g(R[CO2])$ ) can be obtained by least squares regressions of higher-order polynomial to the scattered data of these variables."

It is not clear which variables are "these variables".

This sentence has been rephrased as:

'Now, we quantify  $S^e_{[CO2,LI]}$  by performing a least-squares regression (regfunc) through scattered data from  $\Delta T^e_{[-LI]}$  and  $\Delta R_{[CO2]}$ .'

Sections 2 and 3

I think the paper order should be 2.2.1, 3.1 then 2.2.2, 3.2. The way it is presented is just confusing. Present the whole of the simple modelling case and then move on to the data-based case.

We have followed the suggestion of the reviewer.

P7L22 "and again fit a second order polynomial to the scattered data of  $T[CO2-equiv]$ "

Which experiment?

Here, we analyse the results of experiment OIC. We have clarified this in the revised manuscript.

P8L12 "Similarly as before" Not English

This sentence has been corrected.

Table 1 State which paper each "published ECS" comes from.

Section 3.3 has been removed from the manuscript, see our answer to the comment of the reviewer below.

I prefer to write reviews before reading what other reviewers have posted, as I feel I will be too easily influenced, so I did not read the other reviewer's comment until now. I am encouraged to see that the other reviewer also found the paper very difficult to follow. This increases my optimism that there is hope that with better explanation in critical areas, and reorganisation to improve the storyline, that the paper may become both comprehensible and publishable.

Reviewer #1 was mostly concerned about Sect. 3.3. During the process of revising the manuscript, we have come to the realization that this section only served as a further illustration of the importance of the effect of land ice changes that is already found in Sect. 3.2. As such, it is not essential to the main storyline of our manuscript. To improve the clarity of the paper as a whole, we have therefore decided to remove it from the manuscript.