Review of Stap et al. 2018

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.

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):

$$S^{\epsilon}_{[CO_2,LI]} = \frac{\Delta T_g}{\Delta R_{CO_2} + \Delta R_{LI}} = \frac{\Delta T_g}{\Delta R_{CO_2} + \epsilon_{[LI]}\Delta R_{CO_2}}$$

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 $\epsilon_{[LI]}$. 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 $\epsilon_{[X]}$ 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.

Regarding the sample calculations from CLIMBER experiment: The authors try to apply their new formulation to compute $S^{\epsilon}_{[CO_2,LI]}$ 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 ΔT_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 $S^{\epsilon}_{[CO_2,LI]}$, after substituting for $\epsilon_{[LI]}\Delta R_{[LI]}$ from equation (11) into equation (9) reduces to

$$S^{\epsilon}_{[CO_2,LI]} = \frac{\Delta T_g - \Delta T_{LI}}{\Delta R_{CO_2}}$$

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)?

Constant $\epsilon_{[LI]}$: The authors have talked a lot about the state dependency of $S^{\epsilon}_{[CO_2,LI]}$, but they have barely discussed the state dependency of $\epsilon_{[LI]}$, which is the bread and butter of this paper. After all, $\epsilon_{[LI]}$ 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 $\epsilon_{[LI]}$ 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 $\epsilon_{[LI]}$ is derived, from say MIS5 and then kept constant for the entire interval of analysis? So the range of changes in $\epsilon_{[LI]}$ and the dependence of principle results on that should be included in the manuscript.

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 provided the quality of feedback that I would have liked. A careful re-writing of this section by the authors is required.

Scientific Comments

1. The various sensitivities are quotes in two different units throughout the paper: K per doubling of CO_2 and $KW^{-1}m^2$. 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 $KW^{-1}m^2$ or equivalently 5.6 K per doubling of CO_2" (similar to the last sentence in the conclusions section).
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.

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”.
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.
5. Last para, page 4: So is $S_{[CO_2,LI]}$ 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.
6. In the first paragraph on page 5 the authors say that they take a “further simplifying step” to more easily compare “$S^{\epsilon}_{[CO_2,LI]}$ to other specific paleoclimates sensitivities $S^{\epsilon}_{[CO_2,X]}$ 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 $S_{[CO_2,LI]}$ by the time one has computed the CO_2-equivalent temperature, they might as well have just used equation 9.
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_2 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.
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.
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.
10. Lines 15-17 page 7: the authors say they are inferring $S^{\epsilon}_{[CO_2,LI]}$ or $S^{\epsilon}_{[CO_2]}$ here but I think a bit of additional comment is required to clarify the appearance of ϵ 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 ϵ 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.
11. Line 1, page 8: “the new approach considering efficacies clearly leads to a more satisfactory result that 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.

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.
13. Line 9, page 8: For the calculation of $\epsilon_{[LI]}$ using equation 11 please provide the values of ΔR_{CO_2} and ΔR_{LI} at LGM that were used.
14. Line 15, page 8: is the mean the value "of" years 20 and 22 kya or "between" those years?
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.
16. Line 12 page 9: "We correct the induced $\Delta T_{[CO_2]}$ 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 it the motivation? And are you subtracting the global value $\Delta T_{[CO_2]}$ from ΔT_{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".
20. The figure description for Figure 3 is completely wrong. It is talking about things that are not on the figure.

Technical Comments

1. Line 2 page 1:"~~with~~ to equilibrium"
2. Line 29 page 2: "are obtained from ~~different~~ various model setups"
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 ($\epsilon_{[X]}$)~~, 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."
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"?
12. Line 5 page 5: Add comma after "Similarly"
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_2 changes we strive to deduce the same $S^{\epsilon}_{[CO_2,LI]}$, inferred above, from experiment OIC in which both CO_2 and land ice cover vary over time".
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"
22. Line 23 page 8: "Note that this scaling still assumes unit efficacy for ~~all other~~ process other than land ice changes"
23. Line 24 page 8: "Then, after ~~After~~ multiplying by"
24. Line 24 page 8: Units should be Wm^{-2}
25. Line 11 page 9: "that the ratio of the radiative forcing change $\Delta R_{[CO_2]}$ between the LGM (185 ppm CO_2) and the PI (280 ppm CO_2), to the change between the PI and $\cancel{2 \times CO_2}$ a $2 \times \mathrm{PI}$ case is
26. Line 16 page 9: "significant ~~on~~ at the 95% level"
27. Conclusions section, Lines 26, 28, 30: $\epsilon_{[CO_2,LI]}$ is a new symbol not previously defined. It seems like a mistake and the authors likely meant $\epsilon_{[LI]}$
28. The yellow star in Fig 4 is barely visible against the cyan background. Please change it to something dark, maybe black.

References

Bitz, C., Shell, K.M., Gent, P.R., Bailey, D.A., Danabasoglu, G., Armour, K.C., Holland, M.M., Kiehl, J.T., 2012. Climate Sensitivity of the Community Climate System Model, Version 4. J. Climate 25, 3053–3070. doi:10.1175/JCLI-D-11-00290.1

Chandan, D., Peltier, W.R., 2017. Regional and global climate for the mid-Pliocene using the University of Toronto version of CCSM4 and PlioMIP2 boundary conditions. Clim. Past 13, 919–942. doi:10.5194/cp-13-919-2017

Chandan, D., Peltier, W.R., 2018. On the mechanisms of warming the mid-Pliocene and the inference of a hierarchy of climate sensitivities with relevance to the understanding of climate futures. Clim. Past 14, 825–856. doi:10.5194/cp-14-825-2018

Hansen, J., Sato, M., Ruedy, R.A., Nazarenko, L.S., Lacis, A.A., Schmidt, G.A., Russell, G., Aleinov, I., Bauer, M., Bell, N., Cairns, B., Canuto, V., Chandler, M.A., Cheng, Y., Del Genio, A., 2005. Efficacy of climate forcings. J. Geophys. Res. 110. doi:10.1029/2005JD005776

Haywood, A.M., Hill, D.J., Dolan, A.M., Otto-Bliesner, B.L., Bragg, F.J., Chan, W.-L., Chandler, M.A., Contoux, C., Dowsett, H.J., Jost, A., Kamae, Y., Lohmann, G., Lunt, D.J., Abe-Ouchi, A., Pickering, S.J., Ramstein, G., Rosenbloom, N.A., Salzmann, U., Sohl, L.E., Stepanek, C., Ueda, H., Yan, Q., Zhang, Z., 2013. Large-scale features of Pliocene climate: results from the Pliocene Model Intercomparison Project. Clim. Past 9, 191–209. doi:10.5194/cp-9-191-2013

Hunter, S. J., Haywood, A. M., Dolan, A. M., and Tindall, J. C.: The HadCM3 contribution to PlioMIP Phase 2 Part 1: Core and Tier 1 experiments, Clim. Past Discuss., https://doi.org/10.5194/cp-2018-180, in review, 2019

Kamae, Y., Yoshida, K., Ueda, H., 2016. Sensitivity of Pliocene climate simulations in MRI-CGCM2.3 to respective boundary conditions. Clim. Past 12, 1619–1634. doi:10.5194/cp-12-1619-2016

PALEOSENS Members, 2012. Making sense of palaeoclimate sensitivity. Nature 491, 683–691. doi:10.1038/nature11574