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Climate of the Past Discussions

Interactive comment on “Technical Note: Calculating state dependent equilibrium climate sensitivity from palaeodata” *by* Peter Köhler et al.

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Discussion paper

3.1 This paper appears to be a comment on, or clarification of, the methods used in Kohler et al 2015. It is not clearly written and this has made the reviewing a bit tricky. I had to go back and read Kohler et al 2015 to have a clue what this is about. It transpires that Kohler et al 2015 and von de Heydt et al 2014 use different methods to calculate the climate sensitivity, and I think the point of this manuscript could be to highlight the differences in the results obtained by using the different methods. But I'm not quite sure. It is stated in the manuscript that von de Heydt et al used Approach II, but the authors do not clearly state that Kohler et al used Approach I. Although this manuscript is underwhelming, I think it may help avoid future problems and confusions by explicitly pointing out the differences in the two methods. It is a shame that this work was not included as an appendix to Kohler et al 2015!

Our reply: This paper is an extension of what is shown in *Köhler et al.* (2015), it is not a comment on, or clarification of, the methods used in the 2015 paper. When we finalized *Köhler et al.* (2015) we (a) have not had all the analysis ready that we presented here, (b) wanted to focus if there is and how to find state-dependency. We agree, that it would have been nice to have it included in 2015 already, however, it would have made that paper also a bit more complicated.
With respect to where the approaches are used: Indeed in *von der Heydt et al.* (2014) approach II has been used, but in *Köhler et al.* (2015) not the full scope of approach I has been used, but only the subsection, in which a probability density function (PDF) has been calculated out of individual data points. The reason why the full use of the equations of approach I was not yet included in *Köhler et al.* (2015) was the fact that we were aware that approach I and approach II disagreed, but we had not yet resolved how they might be related to each other (subsection 2.3 here). In the revised manuscript we will make it clearer to the reader which approach has been used previously already.

3.2 So of course I had to go back to Kohler et al 2015. In that paper, curves are fitted to temperature / radiative forcing (hereafter RT) plots. They conclude that, in some cases, nonlinear fits may be appropriate and thus climate sensitivity is state dependent. There seem to be two problems with this conclusion. The first is the statement in the introduction of Kohler et al 2015, "However, we are not aware that a difference in the response has been shown for radiative forcing from surface albedo changes (R[LI]) and CO2 (R[CO2]). Hence we combine them linearly." A different response to these two forcings was already clearly shown in Yoshimori et al (doi:10.1175/2011JCLI3954.1, 2011). Thus, since the RT curves for "LI" and "CO2" are likely to be different, finding that the RT curves when CO2 and LI forcings are both included is nonlinear does not uniquely show state dependence of climate sensitivity. Rather it more likely shows a combination of state dependence and forcing dependence.

Our reply: Thanks for this clarification and of what has been already shown in *Yoshimori et al.* (2011). We will briefly mention this aspect in the draft now. However, this comment is more related to the 2015 paper and even when it would imply that what we show is more likely a combination of state dependence and forcing dependence (and not state dependence alone) of climate sensitivity all aspects how to calculate S out of the scattered data of ΔT and ΔR which is show here, is still important.

3.3 The second potential problem in Kohler et al 2015 that is pertinent to the manuscript under review is possible over-fitting of high order polynomials. More parameters means it is easier to fit a scatter of points, and the method used to discriminate between the polynomials should take this into account. I'm not sure exactly how the authors employed the F-test, but why did they not use something like BIC (Bayesian Information Criterion) which explicitly takes into account this over-fitting problem? It seems likely that the higher order polynomials are not supported by the data. Since the authors are in the mode of commenting on their own previous work, perhaps they could also address this over-fitting issue in the manuscript under review.

Our reply: Interestingly, when preparing the paper which was now published in 2015 we first only investigated if a 2nd order polynomial would better fit the data than a linear approach. However, one of the first reviewer, who commented on that manuscript, even before it was submitted to *Climate of the Past*, suggested that we should test if even higher order polynomials might better fit to the data. We therefore followed this idea ever since and tested which order of polynomial best fits the data. For that aim we used 2 different approaches, one based on Akaike's information criterion (AIC), (e.g. see *Wilks*, 2006), the other based on F-test. However, we found that the F-test is more conservative than AIC, implying that when relying on F-test the order of the polynomial was for some cases smaller than when relying on AIC. We therefore restricted all statistics to F-tests only. Since we used two different statistical methods and finally used only the more conservative one we think we are not overfitting the data. This will be mentioned now briefly.

3.4 The main thrust of the manuscript under review is that, in the case of a nonlinear polynomial in RT space, a different result for calculation of the gradient of a curve will be obtained depending on which position on the polynomial you start from. This is obvious. However, the point that I think the authors are making is that Kohler et al 2015 calculated all their values of R relative to a particular state, (Ro,To) and then calculate Sensitivity (S) as (T-To)/(R-Ro) whereas Von de Heydt et al calculate the tangent to the RT curve. These two methods result in a different function for S. The authors state that Approach I (Kohler et al 2015) is the most robust approach, but it is not clear why, and it seems to me that representing S as dT/dR (Approach II of von der Heydt et al) is more generally our scientific goal. The authors state that Approach II "is more readily comparable to climate model results". This is an odd statement as it really depends on the climate model experiment. It would be possible to exactly reconstruct Approach I using a climate model, and indeed a common suite of experiments (0.5CO2, 2xCO2, 4xCO2 starting from try control state), are a version of Approach I.

Our reply: Approach I was not in full depth followed on in *Köhler et al.* (2015) (see our reply to comment 1 of the reviewer above). Approach I was in our view the more obvious or most robust one because the results following this approach are in agreement with the climate sensitivities S calculated for each individual data point (one time step), while results based in approach II disagree with results based in individual points and only converge to those based on approach I when the intergral given in Eq 11 is calculated (see Fig 3b of manuscript under discussion). The important point is that when following the local slope (approach II) to calculate S one needs to calculate the integral given in Eq 11 ($S^{\text{model}} = \frac{1}{\Delta R_2 - \Delta R_1} \int_{\Delta R_1}^{\Delta R_2} S^{\text{local}} \, \text{d}\Delta R,$), and not stop at Eq 5 ($S^{\text{local}} = \frac{\delta \Delta T}{\delta \Delta R}$). The reviewer is right that simulations can be designed which follow approach I, therefore the comment "that Approach II is more readily comparable to climate model results" will be deleted.

3.5 I don't understand the point of the Discussion section. It isn't a discussion of the previous sections, but another comment on a different part of the analysis. It seems to be comparing two completely different things. One thing is the fitting of a curve to the scatter of points in RT space, which results in the calculation of a functional relationship between S and T. The second thing is taking the scatter of points in the RT space and turning them into a distribution in R (or S) space, which indicates how often the earth system has wandered into different parts of

RT space in the paleoclimate record.

Our reply: What previously has been the discussion section is either shifted to other sections or deleted, because being a rather inimportant statement. See also comment 5 of reviewer 1 and our reply to it, pointing in the same direction.

References

Köhler, P., B. de Boer, A. S. von der Heydt, L. S. Stap, and R. S. W. van de Wal, On the state dependency of equilibrium climate sensitivity during the last 5 million years, *Climate of the Past*, *11*, 1801–1823, doi:10.5194/cp-11-1801-2015, 2015.

von der Heydt, A. S., P. Köhler, R. S. van de Wal, and H. A. Dijkstra, On the state dependency of fast feedback processes in (paleo) climate sensitivity, *Geophysical Research Letters*, *41*, 6484–6492, doi:10.1002/2014GL061121, 2014.

Wilks, D., *Statistical methods in the atmospheric sciences, second edition*, *International Geosphere Series*, vol. 59, Academic Press, 2006.

Yoshimori, M., J. C. Hargreaves, J. D. Annan, T. Yokohata, and A. Abe-Ouchi, Dependency of feedbacks on forcing and climate state in physics parameter ensembles, *Journal of Climate*, *24*(24), 6440–6455, doi:10.1175/2011JCLI3954.1, 2011.

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