Clim. Past Discuss.,
doi:10.5194/cp-2016-23-RC4, 2016

Climate of the Past Discussions

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

Anonymous Referee #3

Received and published: 2 May 2016

Note: I have not read any of the other comments or reviews in the interactive review before posting this review.

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!

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. 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.

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. 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.

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-23, 2016.