Clim. Past Discuss.,
doi:10.5194/cp-2016-23-AC1, 2016

Climate of the Past Discussions

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

Peter Köhler et al.

peter.koehler@awi.de

Received and published: 18 May 2016

Response to Reviewer #1 May 19, 2016

1.1 This paper compares methods for evaluating equilibrium climate sensitivity using as an example the datasets of radiative forcing and global mean surface temperature change of Kohler et al (2015) for climate states over the last 800 kyr. If I have understood correctly, the first method is to calculate the ratio of temperature change to forcing change, both evaluated with respect to a reference state (or regress one against the other, requiring zero intercept), and the second method is to calculate the derivative of temperature change with respect to forcing change, without need of a reference state. They refer to the quantity estimated i.e. the global surface warming per unit increase in forcing as the "specific equilibrium climate sensitivity", whereas in the literature relating to climate projections e.g. in the IPCC reports, this quantity is called the "climate sensitivity parameter".

Our reply: Unfortunately, the definition of variables is not always the same. This is especially the case for the global surface warming (ΔT) per unit increase in forcing (ΔR) which we call "specific equilibrium climate sensitivity" and which we give the notation S. The reviewer correctly noted that in the IPCC reports, this quantity is called the "climate sensitivity parameter". However, it is also given in the IPCC reports the acroynm $\lambda = \lambda_1 = \Delta T/\Delta R$. Other studies, e.g. *Dufresne and Bony* (2008) call λ the "climate feedback parameter", $\lambda = \lambda_2 = -\Delta R/\Delta T$. As can be seen $\lambda_1 = -1/\lambda_2$. To avoid confusion we wanted to get away from the notation λ and decided to stick the nomenclature brought up by the review of *PALAEOSENS-Project Members* (2012), in which $S_{[X]}$ was introduced, with X being the process(es) whose forcing is explicitely considered in the calculation. Furthermore, since $S_{[X]}$ in the context of complex climate models used for IPCC is not a simple (tunable) *parameter* of these models, but an outcome of the analysis of simulations, we find the wording "climate sensitivity parameter" confusing. Nevertheless, we will briefly mention now in the introduction the different wording used within the IPCC.

1.2 If the climate sensitivity parameter is a function of climate state, the methods give different results. One could argue that they give different quantities, though as the authors point out they can be related by integrating along a trajectory. I don't think it is clear that one or the other should be preferred. It depends on the purpose. It is important to be aware of this, of course, when using palaeo-data to constrain future projections, as the authors suggest in their conclusions.

Our reply: This is exactly our main motivation of writing this technical note: We need to be aware of the different approaches and how they might be transfered into each other. We will revise the draft for clarity and that per se no approach might be preferred.

1.3 I note that all the palaeoclimate states are assumed to be equilibria for the atmosphere-ocean system. That, they assume there is no heat storage occurring in the ocean. If there is, it has to be subtracted from the forcing in order to estimate the climate sensitivity parameter. AOGCMs suggest that the ocean takes more than 1000 years to reach a steady state after radiative forcing is changed, with everything else held constant. It may be worth discussing this point.

Our reply: Transient effects were investigated and the equilibrium assumption was checked in previous studies. In a previous analysis to which some of the authors contributed (*PALAEOSENS-Project Members*, 2012) we investigated how important the contribution of data-points from periods of abrupt climate change are for the calculation of $S_{[\mathrm{X}]}$. For that aim we filtered time series of the last 800,000 years for periods in which global temperature change was fast, e.g. faster than 0.5 or 0.1 K per 1 kyr. This would filter out millenium-scale variability, so-called Dansgaard-Oeschger event, for which we would expect that climate was indeed not in equilibrium, as suggested by this comment. However,

we concluded in *PALAEOSENS-Project Members* (2012) that within the given data sets transient effects are not important and the assumption of equilibrium is valid. Here, we investigate in detail different time series than in this previous study. However, this assumption of equilibrium conditions seems still valid, also because a lot of the time series investigated here ($\Delta \mathrm{T}_g$, $\Delta \mathrm{R}_{[LI]}$) are based on the model-based inversion of a benthic δ^{18}O stack with 3-D ice sheet models. Within such a setup, from which we took output at discrete time steps of 2,000 years, abrupt climate change connected with Dansgaard-Oeschger events are per se not included. This will now briefly be discussed just before subsection 2.1 starts.

1.4 An analogous question of whether the climate sensitivity is defined by the slope from the origin to the endpoint, or by the tangent slope, arises in consideration of AOGCM simulations, for example under constant 4xCO2, as they approach equilibrium e.g. Gregory et al. (2004, 10.1029/2003gl018747), Li et al. 2012 (10.1007/s00382-012- 350-z). In most AOGCMs the slope is found not to be constant, and is a function of state or time e.g. Andrews et al. (2012, 10.1029/2012GL051607). The reasons are probably not the same as on the multimillennial timescale, but the technical issue is similar.

Our reply: Thanks for these suggestions. We will briefly mention these simulation results in the revised paper.

1.5 I think the technical point of the paper is sound, but I would say that it seems rather laboured, and I feel it could be written more briefly. The discussion section 3 could be incorporated in 2.1 if it refers only to the first method. However one could also remark that regression might be used to determine local slopes in 2.2, and the same issue applies that the extremes get more weight.

Our reply: We will follow this comment (a similar comment was made by reviewer 3) and we will delete the discussion section. Details so far mentioned there will be either shifted to places where issues are first mentioned or they will be deleted. However, please note that there are no guidelines yet how long a technical note should be. We believe that a lot more shortening will be difficult, since this might results in a piece of work that no longer stands on its own.

1.6 The technical issue outlined in the abstract is summarised by points 1 and 2 of the conclusions. The majority of the conclusions are about implications for the interpretation of palaeoclimate sensitivity and particularly about the dataset of KoÌĹhler et al. While these points may be fine, it seems to me that they are not really conclusions of the technical discussion. They are more of a discussion of the scientific implications for the particular case considered.

Our reply: The reviewer is correct that some of the points in the conclusion section are no true conclusions of this short technical note, but more a discussion of potential implications. However, we still believe these points should be briefly mentioned, because only after applying these different methods to some data one really see the consequences / implications. Therefore, we decided to revise the final section into something called *Discussions, Conclusions, Implications*. The abstract will now also include a sentence saying that we indicate some implications if the approaches are applied to Pleistocene data.

1.7 Point 5 in particular raises more subjects. Should one expect the climate sensitivity parameter evaluated from the palaeorecord to be applicable to the future? How should account be taken of forcings apart from CO2 and ice-sheet albedo? There is a lot of other literature about the dependence of the climate sensitivity or feedback parameter on the nature of the forcing agent, and about its dependence

on climate state. The authors mention the need to remove "slow" feedbacks to make their evaluation comparable with AOGCM evaluations, but this is inconsistent with their regarding ice-sheets as a forcing (rather than as a slow feedback), I would say. Is their quantity a climate sensitivity or an Earth system sensitivity? These are important questions, but not the stated subject of this technical note, and they do not appear in the abstract. I feel therefore that either the discussion should be restricted to the technical point, or that the scope of the paper as represented by the title and the abstract should be widened, and a fuller discussion of the implications should be included before the conclusions are reached.

Our reply: This comment refers to the last point in our conclusion section. We have a different perception of what is mentioned (or not mentioned) in that paragraph than the reviewer. We actually think that this point only briefly summarizes what would be the next logical steps, one might want to go, but we do not follow on into any details, because we also think (and briefly write) that these further investigations are indeed very interesting and wanted, but not within the scope of such a short technical note. We therefore do not think that this point should be deleted, and it might fit into the final section now better since we will rephrase its title (see also our reply to the previous comment). We will therefore briefly mention now in the abstract some implications given here. However, we also see, that some clarifications are necessary, we therefore give some in-depth responses to every of the various subpoints.

Should one expect the climate sensitivity parameter evaluated from the palaeorecord to be applicable to the future?
One motivation for palaeo studies still is to learn from the past to better predict the future. As long as climate sensitivity can be assumed to be independent of climate state, this raises no problems. With the state dependency of climate sensitivity, however, the application from knowledge gained from past climates

to presumablely different future climates is not straightforward anymore. We don't elaborate such a transfer here, but future studies will certainly investigate more details of this issue. Therefore, we believe our warning statements brought up here, that no easy transformation is possible is potentially still of interest for the reader. We also see this as a kind of final interpretation of what can be gained from our previous 2015 study. All that said, it probably would have been also a possible and elegant way to have all details of this technical note already included in this 2015 paper. However, we have to say that our analysis shown here was not that advanced and ready in 2015.

How should account be taken of forcings apart from CO2 and ice-sheet albedo? and *The authors mention the need to remove "slow" feedbacks to make their evaluation comparable with AOGCM evaluations, but this is inconsistent with their regarding ice-sheets as a forcing (rather than as a slow feedback), I would say.*
We think there exist some misunderstanding here. The concept of treating the land ice sheet albedo as a radiative forcing and not as a slow feedback is an operational decision which was widely explained in the *PALAEOSENS-Project Members* (2012). Also note, that this is already accounted for in our introduction in which we write that the land ice contribution is a correction of a slow feedback: *"Here, X (of $S_{[X]}$) corresponds to the forcing processes considered, typically changes in (sometimes also including the other greenhouse gases and), potentially corrected for some slow feedbacks such as planetary albedo changes causes by variations in land ice (LI), vegetation (VE) or dust (aerosols (AE))."*
For more details on this issue please see the section **Forcing and slow feedbacks** in the PALAEOSENS paper (2012).

% Is their quantity a climate sensitivity or an Earth system sensitivity?

Interestingly, this question was already asked by the handling editor after submission and we repeat and extend our point of view on this issue:
The terms *Earth system sensitivity*, *Charney sensitivity* and even *climate sensitivity* estimate the mean global warming as a response to a doubling of CO_2 concentrations, so units of all these quantities are "^oC" or "K". We here follow the more general approach which in detail is here and elsewhere (e.g. PALAEOSENS paper in 2012 in Nature) calculating temperature change per radiative forcing change. Therefore, our result variable is in detail labelled *specific* equilibrium climate sensitivity S (see beginning of abstract), which then is expressed in units "K W^{-1} m^2", or as pointed out by the reviewer *climate sensitivity parameter* within the context climate simulation results used for the IPCC. Truely, Earth system sensitivity (or ESS) is a special case of S. To calculate ESS the specific equilibrium climate sensitivity S is already multiplied by the radiative forcing caused by a doubling of atmospheric CO_2 concentration. In detail, the connection between both is established in the PALAEOSENS 2012 paper. We like to state our calculations in the more general formulation of the nomenclature put forward by the PALAEOSENS group in their Nature review paper, also because its definition is more strict and, if properly followed on, confusions are avoided more easily. Furthermore, since we never go that final step to calculate any temperature change out of S because we believe that due to the non-linear character of the S this is not straight-forward possible, we believe sticking to the nomenclature as used so far is correct and more general and should not lead to confusion. However the distinction to ESS will now briefly be mentioned in the introduction.

1.8 Minor comments

p1 line 10. "One prominent approach". Please could references be given.

Our reply: References given now (e.g. *Rohling et al.*, 2012; *von der Heydt et al.*,

2014; *Köhler et al.*, 2015; *Martínez-Botí et al.*, 2015).

p2 line 7. "Results point more and more in the direction". Again, please could references be given.

Our reply: References given now (e.g. *Crucifix*, 2006; *Hargreaves et al.*, 2007; Yoshimori et al., 2011)

p4 line 12. I don't think radiative forcing is ever "absolute"; it is always referred to some climate state e.g. in IPCC reports to pre-industrial (c 1750).

Our reply: In a climate model absolute radiative forcing can be calculated and we therefore think that our calculations in section 2.3, which rely on absolute radiative forcing should be included here. In principle it would be possible to reduce this section to calculations based on relative changes in radiative forcing only. However, such a shortening would unnecessarily miss some of the insights given here.

p5 line 4. I think it should be "mean local slope".

Our reply: We think "local slope" is correct here.

References

Crucifix, M., Does the last glacial maximum constrain climate sensitivity?, *Geophysical Research Letters*, *33*, L18,701, doi: 10.1029/2006GL027,137, *Crucifix* (2006); Crucifix2006; Peter; gotit; awi; AGCM; radiative forcing; LGM; today; 2xCO2; modelling; intercomparsion, 2006.

Dufresne, J.-L., and S. Bony, An assessment of the primary sources of spread of global warming estimates from coupled atmosphere-ocean models, *Journal of Climate*, *21*, 5135–5144, doi: 10.1175/2008JCLI2239.1, *Dufresne and Bony* (2008); DufresneBony2008; Peter; gotit; awi; today; review; RF; feedback analysis; IPCC AR4, 2008.

Hargreaves, J. C., A. Abe-Ouchi, and J. D. Annan, Linking glacial and future climates through an ensemble of gcm simulations, *Climate of the Past*, *3*, 77–87, *Hargreaves et al.* (2007); HargreavesAbe-OuchiAnnan2007; Peter; gotit; awi; modelling; paleo; radiative forcing; zu Masson-DelmotteKageyamaBraconnot2006, 2007.

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.

Martínez-Botí, M. A., G. L. Foster, T. B. Chalk, E. J. Rohling, P. F. Sexton, D. J. Lunt, R. D. Pancost, M. P. S. Badger, and D. N. Schmidt, Plio-pleistocene climate sensitivity evaluated using high-resolution co_2 records, *Nature*, *518*, 49–54, doi:10.1038/nature14145, 2015.

PALAEOSENS-Project Members, Making sense of palaeoclimate sensitivity, *Nature*, *491*, 683–691, doi:10.1038/nature11574, 2012.

Rohling, E. J., M. Medina-Elizalde, J. G. Shepherd, M. Siddall, and J. D. Stanford, Sea surface and high-latitude temperature sensitivity to radiative forcing of climate over several glacial cycles, *Journal of Climate*, *25*, 1635–1656, doi:10.1175/2011JCLI4078.1, 2012.

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.

Yoshimori, M., Hargreaves, J. C., Annan, J. D., Yokohata, T., and Abe-Ouchi, A.: Dependency of Feedbacks on Forcing and Climate State in Physics Parameter Ensembles, Journal of Climate, 24, 6440–6455, doi:10.1175/2011JCLI3954.1, 2011.

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