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

Anonymous Referee #1

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Review of "Calculating state dependent equilibrium climate sensitivity from palaeodata" by Köhler et al.

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 Köhler 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

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sensitivity parameter".

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.

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.

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/s00382-012-1350-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.

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.

The technical issue outlined in the abstract is summarised by points 1 and 2 of the con-

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clusions. The majority of the conclusions are about implications for the interpretation of palaeoclimate sensitivity and particularly about the dataset of Köhler 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.

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.

Minor comments

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

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

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).

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

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