

Interactive comment on “Including the efficacy of land ice changes in deriving climate sensitivity from paleodata” by Lennert B. Stap et al.

Anonymous Referee #2

Received and published: 15 January 2019

My initial thoughts on seeing this paper were very positive in the sense that, given the uncertainty of information in the paleorecord, and the difficulty of using state of the art models to make very long runs, all progress in the area of better defining climate sensitivity as it relates to past climates is worthwhile.

My optimism remained through the first parts of the paper, but by the end I have to admit that I am lost and really do not understand what the authors are trying to do and what they have discovered.

The authors introduce a variable, $DTe[CO_2 - equiv]$ but do not explain why this is useful or interesting. What I would have done is take equation (9), replace X with LI and then explore all the elements of that equation. This would show us how S varies with DRLI and DR CO_2 , as well a DTg and one could consider how much of the

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state+forcing+efficacy dependence of $S[CO_2]$ is accounted for by considering land ice with and without considering efficacy. I can see that Figures 2 and 3 represent some kind of sensitivity-like variable, but I cannot grasp its meaning. Basically, $DTe[CO_2 - equiv]$ is not, as you suggest in equation 14 simply a function of DR CO_2 but also depends on Tg and DRLI. I hope that the remedy is a better explanation of the reasons behind the derivations in section 1 and also better explanation of the insight that you gain from the results.

Other points.

P1L10 "Recently, it has been shown that simulations of models that have been integrated over a few centuries are not yet in equilibrium, and from longer climate simulations a higher ECS can be deduced (Knutti et al., 2017)."

This needs rephrasing. It has been well known since before dynamical oceans were included in climate models that the equilibrium time of the ocean is of the order of thousands of years. Since the invention of the AOGCM, ad-hoc methods have been introduced to try to estimate equilibrium climate sensitivity without running the models to equilibrium. What recent work has been doing is assessing the accuracy of such approximations.

P2L23 "likewise as several earlier studies"

-> "as in several earlier studies"

Eq(10) This equation suggests to me that $DTg - DT[LI] = DT[CO_2]$. Maybe I misunderstand, but it seems to me that $DTg = DT[LI] + DT[CO_2] + DT[X] + Z$, where $DT[X]$ is the influence of all the other forcings and Z represents cross terms (ie nonlinearities).

P3L5 "Similarly as in the old approach," Not English

Eq(13) Looks like a minus sign between "CO2" and "equiv".

P5L12 "A functional relationship between $TE[CO_2 - equiv]$ and $R[CO_2]$ ($T[CO_2 - equiv]$)

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= $g(R[CO_2])$) can be obtained by least squares regressions of higher-order polynomial to the scattered data of these variables."

It is not clear which variables are "these variables".

Sections 2 and 3

I think the paper order should be 2.2.1, 3.1 then 2.2.2, 3.2. The way it is presented is just confusing. Present the whole of the simple modelling case and then move on to the data-based case.

P7L22 "and again fit a second order polynomial to the scattered data of T" [CO₂-equiv]"

Which experiment?

P8L12 "Similarly as before" Not English

Table 1 State which paper each "published ECS" comes from.

I prefer to write reviews before reading what other reviewers have posted, as I feel I will be too easily influenced, so I did not read the other reviewer's comment until now. I am encouraged to see that the other reviewer also found the paper very difficult to follow. This increases my optimism that there is hope that with better explanation in critical areas, and reorganisation to improve the storyline, that the paper may become both comprehensible and publishable.

Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2018-88>, 2018.