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# *Interactive comment on* "Could the Pliocene constrain the Equilibrium Climate Sensitivity?" *by* J. C. Hargreaves and J. D. Annan

**Anonymous Referee #1**

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This paper derives a relationship from an ensemble of AOGCM simulations of the mid-Pliocene between tropical temperature difference wrt present (Tp) and equilibrium climate sensitivity (S, ECS), and applies it to an estimate of Pliocene tropical SST to derive bounds for ECS. I think that it is useful to attempt to do this in principle, but I am not convinced by some aspects of the method. Consequently I am not confident of the results obtained.

General comments

My first two comments are reservations about the relationship at line 90

S = alpha Tp + C + epsilon

which is fitted by linear regression of the models.

(1) Firstly, no comment is made at line 90 about C, and it later turns out that C is substantial compared with S. I would expect that if ECS is zero, both global mean and tropical temperature change wrt present will be near zero. Tp might not be quite zero because there is unforced variability in any simulation, and there could also be local effects of heterogeneous forcing. The general idea of climate sensitivity is that forcings cause a global response. Many experiments with GCMs show that the pattern of response in a given model is fairly constant while the amplitude depends on the magnitude of the forcing. I am sure the authors know this. In sect 4.4 they briefly discuss this problem. They suggest that the tropical temperature change could be zero for a non-zero ECS. But that contradicts their own expectation at line 67, where they note that in LGM simulations there is large Tp with large S. The latter is also suggested by Fig 1, where the tropical response is greater than the global mean. If Tp were near zero it would not contain any information that could constrain ECS i.e. it invalidates the assumption of the method. If C is omitted, the line is constrained to pass through the origin, and the conclusions will be substantially modified.

(2) Secondly, the authors argue that S should be treated as the dependent variable and Tp the independent in a regression. This seems surprising. In an ensemble of models simulating climate change, the T change in a small region will generally have a larger fractional spread than the global T change. Because of this, and because ECS refers to a global energy balance that determines global T change, I think it would be more natural to make S the independent variable. Alternatively TLS could be used, as the authors say, given an independent estimate of the ratio of the uncertainties in S and Tp. However, I would suggest that the treatment of the scatter entirely as an additive epsilon is not appropriate anyway. As I said in the first point, many results show that a given GCM tends to have a fairly constant pattern of T change, but the pattern is model-dependent. Thus, we might expect the ratio of tropical to global T change to be a model-dependent ratio R, and Tp = S R F/F2x, where F is the Pliocene forcing and F2x the 2xCO2 forcing used to define S. There is probably additive noise as well, but at least part of the scatter in Tp versus S is due to R, which is a multiplicative factor. I

suggest that the effects of the spread of R and S in the PlioMIP ensemble should be considered separately.

(3) I am also concerned that Tp in the models appears to be systematically larger than the proxy estimate. The authors comment on this in Sect 4.1. Isn't this a serious problem? It might mean that the proxy data is wrong, the BCs used for the AOGCM experiments are wrong, or that the models are wrong (they might produce the wrong R, for example). In any of these cases, the method is compromised.

(4) The treatment of the uncertainty of the Pliocene tropical SST estimate seems inadequate. The authors have derived a tropical-mean annual-mean by interpolation and integration from the proxy dataset, which was presumably rather sparse initially as well having uncertainties on the data. I don't find it satisfactory to state simply that the uncertainties are not known and therefore make some fairly arbitrary choices, since the final result depends substantially on this.

Specific comments

13. I don't think it helps to call it "Charney". I would recommend omitting that. If the authors mean that certain things are included and others are not, it would be better to spell them out. Throughout the text and figures, I would recommend using the phrase "equilibrium climate sensitivity" (or ECS). The phrase "climate sensitivity" alone is rather vague. It might mean the climate sensitivity parameter (in K per W m-2).

24. Why do the ice sheets particularly make it a challenge? Ice-sheets give a global forcing which can be taken into account in estimating ECS.

30. Similarly, why is it an advantage that CO2 was higher in the mid-Pliocene? I am not arguing against this or the previous point, but I think they should be justified.

33. More is needed here to explain what ESS is and why it is different from ECS, so that the reader can be clear what is new about the present paper.

52. Should it be "constrain" rather than "predict"?

91. I think alpha is not completely unknown as it should contain the ratio of the forcings.

98. Are the two PlioMIP experiments for the same climate conditions? They have to be consistent, because of the use of the SST from Experiment 1.

109, 225. If the AOGCMs are not run to equilibrium, the ratio of Tp to global T might not be characteristic of equilibrium. The suppressed warming in the Southern Ocean in Fig 1 might indicate they are not equilibrium. This could bias the results.

112, 124, 200. It would be useful to quantify the various forcings, so that we can appreciate how they compare with the CO2 forcing. Either CO2 has to be overwhelmingly dominant, or we assume that climate sensitivity is the same for all forcings (that's the usual assumption, although not completely accurate, as the authors later note).

133. Please quantify the correlation and test its significance.

139. What is the precise definition of "the tropical region"?

217. Please give some references for this "commonplace" method.

219. Increasingly recognised by whom? References please.

223. Andrews et al. use abrupt4xCO2, not 1pctCO2.

227. Although the surface temperature trend may be small after 500 years, it can go on for a long time, and thus global T change may be substantially short of its eventual value. See e.g. Li et al (2012, 10.1007/s00382-012-1350-z).

256. What is uniformitarianism?

257. There are many earlier references for this e.g. Joshi et al. (2003, Clim Dyn), and more recently e.g. Shindell (2014, Nature Climate Change).

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