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

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The concept of using knowledge about conditions during the Pliocene to constrain the equilibrium climate sensitivity (ECS) is attractive. Here the method involves comparing observations of tropical sea surface temperature (SST) warming during the mid-Pliocene Warm Period (mPWP) with that simulated by coupled atmosphere-ocean general circulation models (AOGCMs), and using the relationship in those AOGCMs between the simulated warming and their ECS values to infer an estimated range for ECS in the real climate system.

Unfortunately, there seem to be some serious problems with the results derived in this manuscript. In the light of them, I am not convinced that the results presented can be viewed as robust.

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Lack of a significant relationship when inaccurate model ECS values are corrected

The method used requires that correct model ECS values are used. The ECS values in Table 1 have, except for FGOALS-g2, been taken from Haywood et al (2013). In two cases, they differ significantly from those given in the appropriate authoritative source, and appear to be wrong.

Haywood et al state the ECS of GISS-E2-R as 2.7–2.9 K; Table 1 and the regression analysis use the mid-point value of 2.8 K. The most authoritative source for the GISS-E2-R ECS is Schmidt et al (2014), which states:

"The climate sensitivity [Charney et al., 1979] for each model configuration is calculated using the q-flux model (with a maximum mixed layer depth of 65 m to reduce computation time) to estimate the climate response to 2xCO2. The NINT, TCAD and TCADI ModelE2 versions have sensitivities of 2.7°C, 2.7°C, and 2.9°C, respectively. . . Estimates of the coupled ocean model sensitivity . . . are 2.3°C, 2.3°C, and 2.4°C and 2.5°C, 2.4°C, and 2.5°C, respectively for the three physics-versions and the two ocean models (R and H)."

It appears that Haywood et al, who cite the submitted version of Schmidt et al. as their reference for GISS-E2-R, have confused the ECS of the mixed layer version of GISS ModelE2 with that of the coupled model. The standard version of GISS-E2-R is NINT; there is no indication that the TCAD or TCADI version was used for the PlioMIP experiments. Accordingly, the relevant ECS value per Schmidt et al. 2014, which should be used for this study, is 2.3 K. Note that Table 9.5 of AR5 WG1 gives the estimated ECS of GISS-E2-R as 2.1 K, but this is derived using the standard estimation method of regressing over the first 150 years of the abrupt4xCO2 CMIP5 experiment. According to Gavin Schmidt's presentation at Ringberg2015, for GISS-E2-R this regression method underestimates its true ECS value of 2.3 K.

Also, Table 1 and the regression use the ECS value for the IPSL-CM5A model given by Haywood et al of 3.4 K. However, Dufresne J L et al 2013, the reference cited by

Haywood, states:

"While the climate sensitivity of IPSL-CM5A-LR (ΔT_s^e(2CO2) 4.1K) lies in the upper part of the sensitivity range of the CMIP3 models, the sensitivity of IPSL-CM5B-LR (ΔT_s^e(2CO2) 2.6K) falls in the lower part".

The 4.1 K ECS value for IPSL-CM5A is supported by the abrupt4xCO2-based regression lines in their Figure 24a, and the related values of 4.10 K for IPSL-CM5A-LR and 4.12 K for IPSL-CM5A-MR given in their Table 1. Note that Dufresne et al. state in their conclusions that "The equilibrium climate sensitivity of IPSL-CM5A and IPSLCM5B are drastically different: 3.9 and 2.4 K, respectively.", which is not quite the same. It appears that, in the final published paper, that wording was inadvertently not updated from the wording in the same sentence in the conclusions section of the submitted manuscript, to reflect the revised values given in the first sentence quoted and in their Table 1. In the submitted manuscript, the values appearing in the first sentence quoted, earlier in their text, and in their Table 1, were 3.9 K and 2.4 K.

Hourdin et al (2013), discussing the IPSL-CM5B model, likewise state that: "The climate sensitivity of the new IPSL-CM5B model is thus much smaller than that of the previous IPSL-CM5A model. For a doubling of CO2, the temperature increase is approximately half of that for a quadrupling of CO2, i.e. around 2.7 K for IPSL-CM5B and 4 for IPSL-CM5A".

The estimated ECS for IPSL-CM5A in AR5 WG1 Table 9.5 is 4.1 K.

Clearly, 3.4 K is not the correct ECS value for IPSL-CM5A; the most authoritative value is 4.1 K, from Dufresne et al (2013).

So far as I can tell, the remaining CMIP5 model ECS values are close to their most authoritative estimates.

Unfortunately, when the regression is re-run with the correct ECS estimates for the two models with incorrect values, 2.3 K for GISS-E2-R and 4.1 K for IPSL-CM5A, the

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tropical SST – ECS relationship is no longer significant even at the 5% level. Using accurately digitised tropical SST changes per Figure 2, the correlation drops from 0.75 (on my calculation) to 0.59 (regression R2 0.35), with a p value of 9.3% rather than 1.9% using the uncorrected ECS values.

The lack of a statistically-significant relationship using the chosen regression model, when corrected values for model ECS values are used, appears fatal to drawing valid conclusions about ECS based on this model, even if there were no other issues with the regression model and approach used.

Realism of model mPWP simulations and extrapolation

The authors state (line 49) the rationale underlying the method as: "if the past behaviour of the models is indicative of their future behaviour in some relevant manner, then it should be possible in principle to use observations of the past to deduce which models are more reliable and hence generate a constrained forecast of the future."

Even if the regression relationship were strong, I don't think that the rationale underlying the method applies in this case. The idea logically involves ascertaining which models generate realistic results, on the basis of the tropical SST change in their mPWP simulations, before using the regression relationship to deduce what range of model ECS values corresponds to the uncertainty range for observed SST change. In the case of mPWP tropical SST change, most models lie outside the observed range, with one at its 80% point, two around its 92.5% point and the other six above its 95% point. That implies a strong probability that almost all, if not all, of the models do not generate realistic simulations of tropical SST change for the mPWP. Why, then, should one regard the relationship between tropical SST warming and ECS in models, almost all of which generate unrealistic warming, as reliable?

This problem is acute here because the relationship is extrapolated well beyond the area occupied by the models; the 5% point of the observed values corresponds to SST warming of only 0.04 times as much as that generated by the model with the lowest warming. Even the 50% point is only 0.68 times as high as the warming in that model. I do not see how extrapolating from a relationship amongst models that almost all generate unrealistic results can produce a realistic uncertainty range.

Regression with intercept methodology

In many cases where an emergent constraint approach is used the observations involved may have no direct connection to the characteristic of the model that is of concern. However, in this case the observation is of the tropical SST change arising in the mPWP, attributed primarily to greenhouse gas forcing (albeit with slow as well as fast feedbacks having occurred). There seems good reason to expect a direct, if imperfect, relationship between tropical SST change and ECS in models. If a model has an ECS of zero, the SST change should be approximately zero. The slope of the ECS–SST relationship will vary between models, reflecting inter alia differing ratios of Earth system sensitivity (ESS) to ECS and of tropical to global surface temperature changes. Nevertheless, if the relationship between model ECS and mPWP tropical SST change is informative about model ECS then would one not expect the regression line to pass within a reasonable distance of the origin? It could be argued that regressing with no intercept term may be more consistent with the physical relationships involved. The fact that the best fit line when regressing with the intercept as a free parameter crosses the zero SST change line at an ECS of over 2 K is certainly of major concern. If regression with no intercept term were used, the median ECS estimate would be 50% lower, so this issue is of first order importance. Although the issue is discussed briefly in the manuscript (section 4.4), it is not in my view adequately addressed.

Perhaps a less unsatisfactory method would be to scale each model's ECS value in proportion to the ratio of its simulated mPWP tropical SST change to the (probabilistic) observed value, generating for each model a probability density for a scaling-based estimated ECS. One would then average these PDFs over all models, perhaps downweighting those with the least realistic simulations, or use some other method of combining the individual model scaled ECS estimates. This approach would recognise that models exhibit different relationships between ECS and mPWP tropical SST change.

Robustness of a relationship found amongst a group of AOGCMs

When a similar approach to that used in this manuscript was used in Hargreaves et al (2012), in relation to Last Glacial Maximum (LGM) simulations by CMIP3 models, the issues identified in this comment were either inapplicable or of minor importance compared with in this case. Nevertheless, the strong relationship that they found in CMIP3 AOGCMs between tropical warming post the LGM and ECS did not survive into the CMIP5 generation of models (Hopcroft and Valdes 2015). That must make for caution in the much more problematic mPWP case. The robustness issue is discussed in section 4.6 of the manuscript, but I am not sure that the caveats given sufficiently reflect the great fragility of conclusions drawn in this case, where almost none of the models produce realistic simulations of mPWP tropical warming.

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