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

J. C. Hargreaves and J. D. Annan

jules@blueskiesresearch.org.uk

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At the outset, we aimed to explore the outputs of PlioMIP to investigate how best to use these. It is well known that the variety of slightly different definitions of what is meant by equilibrium sensitivity can give rise to different answers. This ambiguity is an additional source of uncertainty that could have been discussed more clearly in our manuscript, although we do not think it has a substantial influence on our results as the different approaches generally give similar results. In the two cases noted: for IPSLCM5A, the value of 3.4C quoted in Haywood et al appears to match closely to the 2xCO2 value of 3.47C presented in Table 1 of Dufresne et al (whether the small difference may be due to a rounding error, or some other source of variation is not clear). Thus, we see no basis for changing this value. As for GISS, we were aware of the range of values that had been generated, and discussed this with the relevant co-authors on the Haywood

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Discussion paper

paper. We are happy to confirm that the value presented in Haywood et al was in fact the deliberate choice of these authors. However it is arguable that the lower value of 2.3 would have been more appropriate, depending in part on what method other authors used for their sensitivity values (which is not always clear in the literature). We did test changing to the lower value, and it made very little difference to our result with correlation still being significant at the 95% level. Thus we didn't think it appropriate to over-rule the choice made in Haywood et al. In fact, even when changing both the GISS values to 2.3 and the IPSL value to 4.1 as suggested in your comment, the regression is still significant at the 5% level, contrary to your assertion.

We agree that it is a concern that the observations are towards the lower end of the model range. However, when uncertainties are considered, there is substantial overlap. There are reasons to believe that the MPWP forcing may be on the high side, and changing this would improve the match, as discussed in the manuscript. We are certainly not concerned that the models do not cover the full range of uncertainty allowed by the observational analysis. GCMs are constructed so as to obey a large number of physical principles and (unlike simple energy balance models) their sensitivity is an emergent property that cannot be arbitrarily selected. There may be very good reasons why no-one has yet built a reasonable climate model with a sensitivity much less than 2C, for example. Given the uncertainties in proxy interpretation and forcing, we would be reluctant at this point to confidently assert that most models have warmed unrealistically. Of course, all models are inevitably wrong and each one will either be too warm or too cold compared to reality. The purpose of the regression is not to select which model or models are "correct" but rather to estimate where on the y-axis a perfect model could be expected to lie, if the relationship found across the existing ensemble holds. Our choice of conditional form “could” in the title of the paper, and caveats stated throughout the manuscript, were quite deliberate.

Regarding the non-zero intercept, we agree this is an interesting issue and thank you for raising it. We agree that it would be natural to have anticipated a priori that the

regression would pass close to zero, and this does raise the issue of whether it should be constrained to do so. On the occasions that we have presented this work, we have discussed this issue with other climate scientists, but have not arrived at a clear physical explanation for the non-zero intercept. Thus we think that it would be useful to include further discussion and the calculation as an alternative result and plan to do so in the revised manuscript.

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