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Climate of the Past Discussions

Interactive comment on "Evaluating the dominant components of warming in Pliocene climate simulations" *by* D. J. Hill et al.

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On behalf of myself and the co-authors, I would like to thank the anonymous reviewer for their time and comments. Although they have concerns regarding the outlined methodology, we believe these can be met by providing better clarity in the formulation of the energy balance calculations. As such the reviewer's comments should significantly improve the final manuscript.

General comments

1. Haywood et al. 2013 presents the results of both PlioMIP Experiment 1 and 2. As this manuscript is an analysis of the PlioMIP Experiment 2 simulations the starting point for this paper and hence Figure 1 and both Tables significantly overlap the

material presented in Haywood et al. 2013. We felt that, while repetition should be kept to a minimum, this paper needed to stand alone and hence a short summary section (Section 4), with associated tables and a figure, were necessary. Haywood et al. 2013 provided extensive description of the simulated Pliocene warming, but no further analysis of the reasons for the warming was presented. We feel that this analysis of the energy balance components of each of the models is a significant advance on the original descriptive paper and that some incite is gained by this analysis.

2. Clearly the description of the clear sky calculations is insufficient to give the reader an accurate view of the calculations being undertaken. To rectify this omission we will include a section making clear where the clear sky fluxes are derived from and how they do not fall into the pitfalls outlined by the reviewer. This was not clear to a reader of the original manuscript without previous knowledge of these techniques. The clear sky radiation fluxes are the integral of the radiation fluxes when the sky is clear. Hence, the cloudiness is already incorporated into the calculation by affecting the time over which the integral operates. Furthermore, in this formulation the temperature change due to cloud albedo is calculated as a separate factor within the energy balance, unlike Lunt et al. 2010 where it is indeed simply the residual. This avoids the problem of errors in clear sky fluxes adversely affecting the other components. The reviewer is right to point out that $\Delta T_{c\alpha}$ is not necessarily related to changes in the albedo of the clouds themselves, but it is the change in the overall reflection of radiation by clouds. For example the albedo of clouds could stay exactly the same, but if cloudiness increased then $\Delta T_{c\alpha}$ would be negative. This point will be made clear in the derivation of the components.

The small changes suggested in the specific comments will also be made to the manuscript.

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