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Earth System Dynamics Discussions

Interactive comment on "The sensitivity of the large-scale atmosphere circulation to changes in surface temperature gradients in the Northern Hemisphere" *by* Sonja Molnos et al.

Anonymous Referee #1

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Review of "The sensitivity of the large-scale atmosphere circulation to changes in surface temperature gradients in the Northern Hemisphere" by S. Molnos et al.

The authors use the statistical-dynamical model SDAM to investigate the impact of global, meridional and zonal temperature changes on the large-scale NH atmospheric circulation during boreal winter. I find this a weak and in fact disappointing paper for which I recommend rejection. I base this judgement on concerns regarding the methodology and the quality of results, as well as on editorial concerns.

Methodology:

1. The paper only very briefly describes SDAM. I do have many questions about the

model, however, that the paper misses to address even briefly. Is this a dry model, or does it have some representation of moisture and clouds? How high is the model top? Is there a stratosphere? What about topography? ... All of these are important for the circulation, and it's unclear whether or not these factors are taken into account, and if so how.

2. Temperature perturbations: are the temperature perturbations in the sense of Newtonian background relaxation temperatures, or is this the final temperature. If the latter, it seems the authors are prescribing the u-wind via thermal wind balance, and so prescribe the circulation. At which height are the perturbations applied? This is crucial given the ongoing debate on low-level versus high-level baroclinicity.

3. Circulation metrics: the chosen circulation metrics are unusual, to say the least. This is problematic as it will make comparison to other studies and models difficult, or might even inhibit such comparisons. Two examples: i) the jet stream strength is defined as the meridional average of u between 10N-80N at 9000mb (Fig.5). Why such a choice? Normally it's defined as the maximum zonal wind in the upper troposphere (for the subtropical jet) or the lower troposphere (for the midlatitude eddy-driven jet). ii) the Hadley cell strength is defined as the mass flux between the surface and 500mb. Why, and at which latitude? Normally it is defined as the maximum of the mass stream function. If the mass stream function maximum moves vertically, the the metric of the authors will be unable to take such a shift into account.

Content concerns:

1. There is very little new results in this paper that are of interest beyond the documentation of the SDAM behaviour for this specific setup. Most prominently this is reflected in the abstract, where only three out of 15 sentences are devoted to new results (lines 24-27).

2. The authors claim that they can clearly separate the impact of global, meridional and zonal temperature changes, and that previous studies were unable to do so. But

they entirely neglect the rich literature using dry GCMs that has looked at exactly this question (e.g., papers by Amy Butler, Jian Lu, Janni Yuval, and many more).

Editorial concerns:

The paper reads like a rushed and, to be honest, quite careless write-up. Most figures follow the same layout as if they were all produced with the same script, labels are missing (e.g., y-label in Fig. 5), and the choice of the colormap in the contour plots is poor. There is unnecessary line breaks in the text (e.g., see the introduction). Normally I would not mind, but this slopiness strengthens my feeling that this paper was done in a rush.

I could go on and list more concerns. I think, however, that I have made my point.

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