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Geoscientific Model Development Discussions

Interactive comment on "Atmosphere-only GCM simulations with prescribed land surface temperatures" *by* D. Ackerley and D. Dommenget

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Overall this is a well written paper introducing a very novel experimental design: prescribed land-surface temperature AGCM experiments. Some interesting but idealized experiments are also introduced to demonstrate that the approach gives reasonable responses. I recommend publication subject to some minor revisions, listed below.

Noel Keenlyside

Main concerns

(1) The description of the response to the NH heating, which seems not the most relevant. The study from Miyasaka and Nakamura (2005) is more relevant.

(2) I am not convinced about that there is a statistically significant response over the

SH westerlies induced by Australia heating.

Minor points

Pg2, L15, Without having read the entire paper, I find aim 2 a little hard to follow because you do not say that you prescribe the very same land surface temperature from the freely varying run, and that this implies that the experimental design does not introduce spurious effects.

Pg3, L15-20, I would have imagined that soil moisture would be a key variable to prescribe to the atmospheric model to capture the surface energy budget. I wonder what are the implications of fixing it to climatology in the 10K experiments? I think you should at least acknowledge that this might impact the results of the surface heating experiments. It might be worth mentioning here that snow cover is simulated? I wonder if you were to prescribe it, whether you would fix the deviations of CON from FREE

Pg3, L30, In my version latent heat is labelled here and in the equation as labdaE, while in the figure 1 it is LE.

Pg 8, s30, I am surprised that T1.5 does not heat further. It seems rather artificial that up to 8K temperature gradient can be formed in the lower 1.5 m of the BL. Some discussion is required of how this can be possible.

Pg 9, s20, Is there any reason to expect changes in the initial conditions should lead to a significant difference on these timescales?

Pg 10, s15, "including ACCESS" is misplaced.

Pg13, s5, Again, I am not clear why you would expect a difference between the CON1 and CON2 simulations. Memory of the atmospheric initial conditions is lost very quickly, and should be gone within a several months I think you should make this clear.

Pg 15, s5, while the arguments given seem reasonable, it seems hard to discount completely the extent of diabatic heating, which is surely greater the AMA case (as

seen in the precipitation field). I think you should be clear about this. Are the responses more comparable if they are scaled by the amount of diabatic heating?

Pg15, s30, The SH Hadley Cell should be present during JJA (i.e., strongest in the winter hemisphere). Are the changes in the SH winds statistically significant?

Pg 16, s5. It would be useful to put the anomaly surface heating into perspective. For example, could you please discuss it in terms of changes expected by the end of century? It would put the simulated responses into perspective.

S4.2.4, NA experiments. The mechanisms proposed by Brayshaw et al. (2009) are more relevant to the NH winter time circulation and the NA Storm track. I think the work of Miyasaka and Nakamura (2005) is much more relevant.

Takafumi Miyasaka and Hisashi Nakamura, 2005: Structure and Formation Mechanisms of the Northern Hemisphere Summertime Subtropical Highs. J. Climate, 18, 5046–5065.

In terms of the conclusions: (1) I think you should mention in the first bullet point "(excluding Antarctica)" , or something along those lines. Perhaps the reasons for this are not clear, and don't need to be explained as the experiments are still very interesting. (2) It is not clear to me that there really was a significant change in the SH circulation in response to Australian heating. (3) Also the explanation for the response to NA heating does not seem appropriate (see comment above).

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