Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-6-AC3, 2016

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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Received and published: 21 April 2016

Reviewer general comments: 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.

Authors' response: The authors would like to thank the reviewer (Prof. Noel Keenlyside) for his insightful, constructive and supportive review of our work. We have endeavoured to respond in detail to the comments raised and hope that we have answered those issues sufficiently.

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.

Response: Having read Miyasaka and Nakamura (2005), the authors fully agree with this issue. We have removed the original description and adjusted Figure 11 accordingly. We have also included the following paragraphs to explain the process: "Miyasaka and Nakamura (2005) show that the land-sea thermal contrast along the west coast of North America is important in causing the formation and maintenance of the Northern Hemisphere, summertime sub-tropical high pressure cell over the North Pacific. Miyasaka and Nakamura (2005) show that the increase in low-level potential temperatures from boreal spring to summer over the North American continent in July (and May) acts to increase cyclonic vorticity (cyclone stretching) over the continent, which strengthens the northerly flow along the west coast. Strengthening of the northerlies then increases the advection of polar air over the ocean, enhances evaporation from the ocean surface and encourages the development marine stratocumulus, which all act to reduce SSTs. The cooling of the air column causes subsidence (visible at 500 hPa, see Fig 8(d) in Miyasaka and Nakamura, 2005) and enhances the anticyclonic circulation (vortex compression) within the sub-tropical high-pressure cell over the ocean and strengthens the northerly flow and subsidence further.

Interestingly, the differences in circulation in Fig 11(c) are qualitatively very similar to those produced by Miyasaka and Nakamura (2005), which suggests that increasing North American surface temperatures by 10 K may result in a strengthening of the Pacific sub-tropical high pressure cell. To illustrate this further, the values of ω500 from CON1 (black solid and dashed lines) and the difference between AM10K and CON1 (coloured shading) are plotted in Fig. 11(d). The largest increases in subsidence (red shading) at 500 hPa occur over the centre and to the north of the maximum subsidence in CON1 (Fig. 11(d)), which may indicate a strengthening and northward shift of the summertime high-pressure cell. Conversely, the opposite circulation anomalies occur in the AMm10K simulation (and with very similar magnitude), which suggests that the same process may be reversed by decreasing North American land surface temperatures (also seen in in the ω500 field, Fig. 11(h)). It is therefore likely that increasing or decreasing the North American land surface temperatures in ACCESS may act to enhance or weaken the strength of the Pacific sub-tropical high pressure cell (given that SSTs in AM10K do not respond to and feedback on the atmospheric circulation in the way described in Miyasaka and Nakamura, 2005). These results therefore indicate that this version of ACCESS (with prescribed land surface temperatures) may be useful for investigating the impact of regional land-sea thermal contrasts on the location and strength of the summertime sub-tropical high pressure cells, for example."

This new description (based on the suggested literature) really showcases a potentially fascinating application of this new version of ACCESS and fits much better with the results presented.

(2) I am not convinced about that there is a statistically significant response over the SH westerlies induced by Australia heating.

Response: We have now plotted the places where the changes in the winds are statistically significant (Fig. 10), with much of the annual mean change significant. Interestingly, the strongest significance seems to be in DJF with little significant change in JJA. Further discussion on this is given below when the reviewer raises those points. Nevertheless, we feel that the line discussing the change in the SH Hadley Cell is too speculative as we do not show any evidence for this (or any other reasoning) and so we have deleted it from the text.

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.

Response: We have changed aim 2 to be: "Show that simulations with prescribed and freely varying land surface temperatures (with the land temperatures in the prescribed run being derived from the freely varying simulation in order to avoid spurious effects)

are climatologically comparable."

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.

Response: The authors agree with the reviewer but in this instance we decided to prescribe the soil moisture in order to constrain the surface as much as possible. This was to prevent the soil moisture from responding to the imposed temperature perturbations, which could have induced extra feedbacks. Nevertheless, removing such a constraint would be a sensible development of these simulations and also would be easy for any other users of the code to undertake. We have actually repeated the AMA10K experiments with and without the soil moisture constraint as part of our next phase of development and we have included the figures below. FIG 1: (a) Difference in MSLP (hPa) between the AMA10K and CON1 experiment and (b) for AMA10K – CON1 with freely evolving soil moisture. FIG 2: (a) Difference in precipitation (%) between the AMA10K and CON1 experiment and (b) for AMA10K – CON1 with freely evolving soil moisture. In both experiments there is a reduction in MSLP over Amazonia; however, the stationary wave-like response in FIG 1(a) is non-existent in FIG 1(b). The reason for this is clear in FIG 2 as when we prescribe soil moisture in (a) we get increased precipitation (i.e. increased convection, which can then cause the wave formation) whereas in (b) the precipitation is lower and suggests a weakening of deep convection (i.e. less impact on the large-scale circulation). It is likely that the moisture in the soil is evaporated away and therefore the local moisture source for rainfall in the Amazon is reduced. The reduction in MSLP is likely to be caused by increased dry convection (i.e. a dry heat-low) from the surface heating once the moisture has evaporated. If we extrapolate out globally, then it is likely that the circulation response in the ALL10K simulation would also be weaker than presented in our work here if the land-based tropical convective centres are sensitive to the local soil moisture content. We have included the following in the future work list as it acknowledges the impact of the soil moisture constraint: "…, which could have an impact on the modelled climate. For example the global circulation response in the ALL10K experiment may not be as strong once the local moisture supply from the land-based convection has been evaporated away." With respect to the snow coverâĂŤwe have included "…(and snow cover is not prescribed)" in Section 2.1 to point out that we do not prescribe it. Prescribing the snow remains an area of future development and would be useful for other experiments but is beyond the scope of this paper.

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

Response: Changed in Fig. 1 to be λE.

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.

Response: Looking at the surface energy balance equation, heat may be lost (or gained) from the surface through long-wave emission, conduction/convection into the air (sensible heating), evaporation and conduction into the ground (ground heat flux). If surface temperatures are increased, and all four methods for re-distributing that heat are equally important, then only one quarter of the extra energy will be available for sensible heating above the land surface to increase T1.5. The long-wave emission will distributed over the whole atmospheric column, latent heating does not increase the air temperature until that energy is re-released in condensation (above the surface) and the ground heat flux is directed into the soil. Furthermore, the values of T1.5 are calculated by interpolating between the surface and the lowest model level. Therefore, the T1.5 response also depends on the global atmospheric response to the increased land

surface temperatures, which will be relatively weak as we are only warming ~33% of the global surface (the other 67% of the globe has unchanged, prescribed sea surface temperatures).

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

Response: There is no reason why we would expect there to be a difference but we think it is important to show this so that future users of this model know that there is no impact on their simulations from not using the same starting field that we used. There is a chance that changing the initial conditions could lead to drift in the simulations (for example, the build up of snow in CON1 relative to FREE could have been sensitive to the initial conditions). Given that no such drift occurs in CON2 and the scope of the journal (model development), the authors feel that it is worth confirming this point.

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

Response: The text has been rearranged.

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.

Response: We do not expect a difference and it is to confirm that nothing unexpected happens when the initial conditions are changed. To make this clearer in this context we have adjusted the end of the second sentence of 4.1.2 to read:

"… shows that this model setup is reliable for other users to perform idealised simulations without the need to use the same initial conditions as this study."

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?

Response: The reviewer makes a good point here and this needs to be accounted for in the text. We have included the following sentence to cover this at the end of the paragraph referred to: “Nevertheless, the larger areal extent of the diabatic heating (and higher precipitation amounts) in AMA10K relative to MC10K is also likely to be an important factor in the different wave responses between those two simulations.” Given this is a proof of concept paper and that the explanation given qualitatively ties in well with the results in the cited literature, a more in-depth look at the size of the diabatic heating region is unnecessary in this case. Nonetheless, it is something that a future user could look at in more detail given these initial sensitivity studies.

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?

Response: The changes in the winds are significant in DJF but not JJA. We have therefore removed the line that included reference to the Hadley Cell circulation changes as it is speculative at best.

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.

Response: Looking at Figure 12.11 from IPCC AR5, Ch. 12, the surface air temperature responses we see over land in our experiments are comparable with those of the ensemble mean, end of century (2081-2100) land surface air temperature changes under RCP8.5. We have included the following at the end of Section 3.1 (T1.5 results), as it seems this is the most appropriate place for it: “Interestingly, in the experiments with higher land surface temperatures (ALL10K, AMA10K, MC10K, AUS10K and AM10K), the T1.5 responses are similar to those of the CMIP5 multi-model ensemble average for the end of the 21st century (2081-2100) under RCP8.5 (high greenhouse gas concentrations; see Fig. 12.11 in Collins et al., 2013). Similarly, the negative T1.5 anomalies over North America in AMm10K are of a similar magnitude to those simulated over

land at the Last Glacial Maximum (see Fig. 2 in Harrison et al., 2014).”

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.

Response: We fully agree with the reviewer here and have made the necessary changes to the text (see the response to the first main concern).

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. Response: We have included “(excluding Antarctica)” as suggested.

(2) It is not clear to me that there really was a significant change in the SH circulation in response to Australian heating. Response: The change is significant (annual and DJF-mean), but our explanation was very speculative with no real evidence to back up the cause (i.e. Hadley circulation changes). We have therefore removed that sentence and left the rest of the discussion about the SAM, which is more relevant.

(3) Also the explanation for the response to NA heating does not seem appropriate (see comment above). Response: We have adjusted the text and taken account of the literature suggested by the reviewer (see above).

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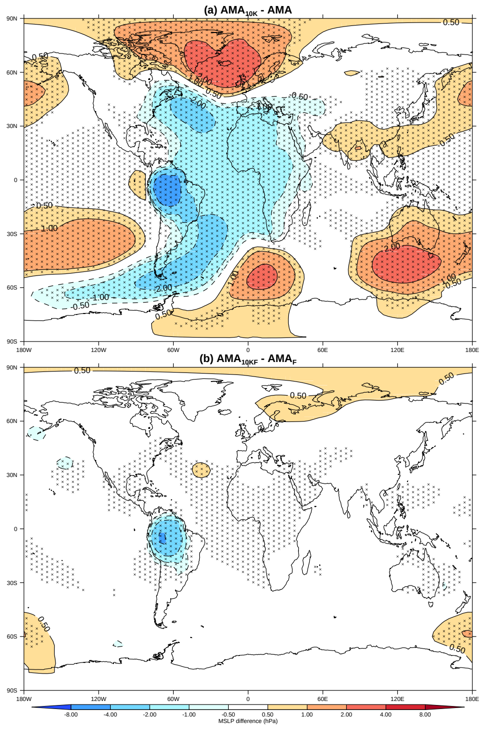

Fig. 1. The difference in mean sea level pressure (MSLP, hPa) for (a) AMA10K – CON1 (prescribed soil moisture) and (b) AMA10K – control run (both with freely varying soil moisture). In both cases the land sur

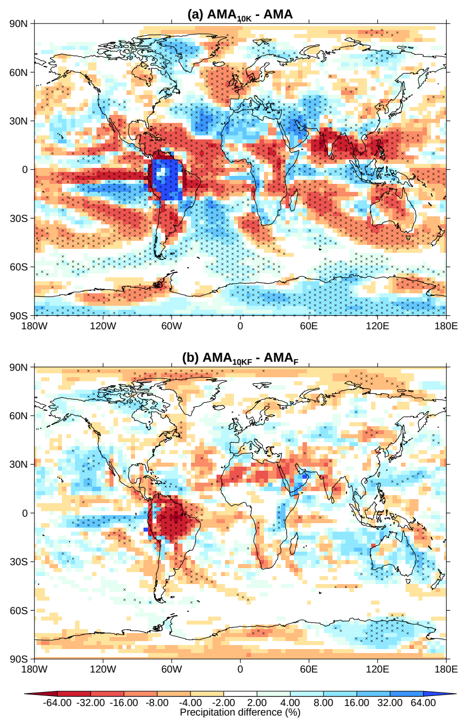

Fig. 2. The difference in mean precipitation (%) for (a) AMA10K – CON1 (prescribed soil moisture) and (b) AMA10K – control run (both with freely varying soil moisture). In both cases the land surface temperat