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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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General Comments

In this paper, the authors argue that the impacts of land temperature anomalies on the atmosphere can be investigated by imposing constraints on an atmospheric GCM, in a similar way to simulations with sea surface temperature anomalies. A method of constraining land surface temperatures in the ACCESS model is presented and it is shown that the simulated climate, including the diurnal cycle, matches well the unconstrained result. A set of experiments, partly motivated by earlier studies, with land temperature in various regions changed by 10 K is then presented. These are of considerable interest and do provide a 'proof of concept'. The Discussion section then presents further results that explore physical mechanisms and make rather lengthy comparisons with

other studies. The final section makes some conclusions that seem overstated, and includes consideration of possible further experiments in unnecessary detail. In some respects, these sections go beyond the initial aim of the paper. Much of the presentation is very good and the work is potentially an excellent contribution. However, various limitations, noted below, also indicate a need for a considerable revision. Some reduction in the text could be needed, but some of the material might be better considered in a further paper in a different journal.

Specific comments (section or page-line given)

A. Prescribing land surface temperatures within a GCM could be a fairly simple exercise. In the case of ACCESS (2.2.1), the specification of surface temperature is evidently complicated, and the description given may not be well understood by a reader not familiar with the MOSES scheme. Eq 2 does not readily follow from Eq 1. It is not clear how 'surface' temperature relates to that of the first soil layer (of depth 0.1m), what G0 is and how it relates to T* and Ts. How does step 'n' relate to the final, etc?

B. Related to the specification of surface temperature anomalies should be a consideration of the energy fluxes associated with it. From P8L1 on, terms 'heating' and 'cooling' are used without explanation. Are these are the implied fluxes needed to keep a surface layer at the prescribed temperature? In any case, the surface (anomaly) must be then heating or cooling the air, which is clearly important. In fact, a warm surface might appear to be losing heat -so cooling, in that sense. Further description of these processes is needed.

C. The presentation of COM2 results (from P9L23 on) seems excessive. If the initial condition change is merely a tweak in the atmosphere, then one would expect no impact on the climate. Indeed there seems to be no statistically significant differences, so what is the interest in the CON2-CON1 results? (Presumably, they do indicate a typical pattern of random or weather-induced differences in 50y means.) A case could be made for averaging the two and using this as the base for other results. If not, some amendment and reduction in the presentation can be made.

D. Regarding the tropically forced wave-like patterns (P14), while westerlies will aid propagation, other studies have shown that non-zonal components of a background state can also aid propagation through easterlies, especially into the winter hemisphere (which seems favoured in 9 b, c and f). Early studies include Schneider and Watterson (1984, J Atmos Sci) and Watterson and Schneider (1987, QJRMS), and these are built on more recently by studies such as Zhao et. al (2015, J. Climate). Could more recent studies than the three in 4.2.2 also be considered?

E. A potentially important result of the pair of AM experiments (P16) is that despite the large amplitudes (+10K, -10K) the response seems apparently linear, differing only in sign. Could this be highlighted? In any case, some of the discussion and comparison with earlier studies seems rather speculative. Does convection really act similarly to topography (P16L25)? Indeed, is there an explicit parameterization of the effect in the model? If not, what is the mechanism?

F. Despite rather extended discussions (section 4), the comparisons of the perturbed temperature cases with earlier studies can only be qualitative –the resulting temperature anomalies are different. The conclusions (P17L15) 'clearly show.. agree with previous studies' are rather over stated. Even at P1L12,'seems qualitatively consistent' might be enough. This links to the aims of the paper, as noted above.

Minor comments

P1L19 Land temperatures also respond to the simulated weather, of course.

P3L4 Since Bi describes two (coupled) versions, the one most like the model used here could be identified (presumably ACCESS1.0, as used in CMIP5, but at reduced resolution here).

P3L7 It might be more usual to state that 'Physical processes represented in the model include'. There are explicit components, in addition to some parameters.

P3L9 'the the'

P3L17 Does 'all' include FREE?

P3L17. Does 'deep soil' mean layers 2, 3, 4? Is there flux through the bottom of 4?

P4L1 Should this be 'SF_EXCH' –as in the Figure?

Fig 2 It seems the ' ..hourly interpolated temperature field' is in the middle column. The detail in the third column is not visible and seems to create an unwieldy file. It might be simplified.

P6L1,3 Does 00:00:30 mean 30 seconds after midnight? Should the first 00: be dropped?

P6 L17, 23 (and elsewhere) 'reduce' is being used in an uncommon, intransitive way
P6L27 'PRES', but (6) has lower case

Table 1. 'Maritime Continents'?

P7L8 'at the'?

P7L9 The soil temperatures and moisture are also prescribed, it seems.

P8L4 One might doubt if the processes in the response to such large (10K) anomalies can be known, from observations. Is this magnitude chosen to improve statistical significance of responses, given some expectation of linearity?

Fig. 3 Would grid square shading, as in Fig. 4, give a clearer depiction than the interpolated lines? Some explanation of the different usage could be added.

P8L22 (and later) If the 1.5m temperature is an interpolation from the surface (subject to parameterisations) then it will be strongly constrained to the prescribed land and sea values. Temperature at the first atmospheric level would be a stronger indication of an atmospheric response. A brief comment justifying the focus on 1.5m seems warranted.

P9L5,7 'alternating' does not seem a good description here –although it is better for precip.

P9L10 lower case‘k’

P10L16 ‘be representative’

P11L11 ‘Similarly’ is odd. As before, CON2-1 is expected to be the same, but CON1-FREE is the main test.

P11L13 Presumably MSLP is an extrapolation from a surface that is now warmer, so one might expect it to be lower, even if the surface pressure is unchanged. How much of the lowering might be due to this? Is the surface pressure different?

P12L25 It seems the mean surface temperature is the same, but there tends to be more snow in CON1. How does that influence T1.5?

P13 Consistent with the earlier suggestion regarding CON1 and 2, this 4.1.2 seems unnecessary.

P13L9 Are the SSTs unchanged in Chadwick’s warmer-land run?

P13L22. Would vertical velocity closer to the centre of the moisture column (e.g. 850 or 700hPa) be an even better match?

P14L2 ‘increases subsidence over India’ is not clear.

P14L7 Often Rossby waves are excited by the latent heat from rain formation. Does this provide the ‘imposed heat sources’ that are described here? If so, does the reduced rainfall over the seas in MC10K counter the effect of enhanced rain over the land?

P16L10 ‘and increase’

Fig 11 labels are bulky –with m10K partly missing. The bars are incorrect (swapped) in a, b, e, f. Perhaps simplify, with bars combined for the pairs?

P17L21What supports ‘the local response is governed by the strengthening .. of existing circulations’?

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