

## ***Interactive comment on “Atmosphere-only GCM simulations with prescribed land surface temperatures” by D. Ackerley and D. Dommenges***

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Reviewer 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 C1 GMDD Interactive comment Full screen / Esc Printer-friendly version Discussion paper other studies. The final section makes

C1

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.

Authors’ response: The authors would like to thank the reviewer (Dr. Ian Watterson) 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.

Specific comments

Reviewer comment 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  $G_0$  is and how it relates to  $T^*$  and  $T_s$ . How does step ‘n’ relate to the final, etc?

Response: The authors accept that there appears to be a large step between Eq. 1 and 2; however, the intention was not to present the equations as such. Eq. 1 was presented just to illustrate the scheme used in the explicit calculation and Eq. 2 was intended to show where we have actually changed the code. Given that we state in the paper that we “only describe the equations that are changed... to prescribe  $T^*$ ” we have therefore removed Eq. 1 as it is not changed in the new version of the code. We have adjusted the paragraph as follows (new paragraph following new Eq. 1, which was the old Eq. 2):

“Where  $T_s$  is the temperature of the first soil layer beneath the surface at the end of the

C2

previous time step ( $K$ ),  $R_s$  is the net radiation (SW and LW) into the soil layer through the surface ( $W\ m^{-2}$ ),  $A^*$  is the coefficient to calculate the surface heat flux ( $W\ m^{-2}\ K^{-1}$ ),  $C_c$  is the areal heat capacity of the surface ( $J\ m^{-2}\ K^{-1}$ ),  $\Delta t$  is the time step length (s),  $T_{prev}^*$  is the surface temperature from the previous time step ( $K$ ), all other variables have the same definition as described above. The term  $C_c/dt(T_{prev}^* - T_s)$  represents the conductive energy flux from the first soil layer to the surface of the soil during the previous time step and is equivalent to the ground heat flux ( $G$ ). More details on the derivation of Equ. (1) can be found in Essery et al. (2004) and Best et al. (2005)."

Given that we have only indicated where the code has been changed and cited all of the necessary literature, it would be easy for another person to find these equations within the model and re-produce our results. Furthermore, as we have not changed any of the other code in the MOSES scheme it is unnecessary to include the full derivation of Eq. 1 (what was Eq. 2 in the first review) when it is available in the cited literature. Also, in order to provide more information for readers we have also included an extra references to Kowalczyk et al. (2016), Best et al. (2005) and Essery et al. (2003), which have more details on the surface schemes employed by ACCESS should a reader require more information.

Reviewer comment 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. Response: We agree fully with the reviewer that the use of 'cooler' and 'warmer' (and similar language) is incorrect and we have removed such wording from the text and replaced it with e.g. increased / decreased land surface temperature. As for the surface temperature specification and the fluxes we do not alter the fluxes and only change the surface temperature. The fluxes are allowed

C3

to respond to the surface temperature perturbation. Using the 1.5 m air temperature therefore provides an indication of whether increasing or decreasing the land surface temperature is having the desired impact on the atmosphere above it. Furthermore, given that all of the atmospheric responses are consistent with the imposed surface temperature perturbations (i.e. increased convection over the Amazon when surface temperatures are increased), this implies that the surface fluxes must be responding in a sensible way to the perturbation.

Reviewer comment 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.

Response: Again, we agree with the reviewer that we should expect no impact on the climate, which is exactly why we have included that analysis. It provides assurance to a reader in that they will be able to re-produce our results without needing the same initial conditions. It is possible (given such a significant change to the surface scheme) that starting the model from a different initial condition may result in an unforeseen drift in the mean climate state. The CON2 experiment simply shows that a user does not need the same starting conditions in order to run the simulation, which we think is an important (and reassuring) result to show. It is also appropriate to draw brief attention to CON2-CON1 in this case is given the journal (GMD).

Reviewer comment 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, QJRM),

C4

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?

Response: The cited articles (Ambrizzi, Hoskins, Karoly, Jin etc.) provide very close examples of the processes that appear in the simulations described in this paper, which is why they were chosen (despite them being pre-year 2000). It is clear from Fig. 9b that there is wave activity in both hemispheres, which corresponds with the surface temperature perturbation extending beyond the region of easterly flow. In Fig. 9c there is virtually no stationary wave activity in the summer hemisphere (NH) and only wave activity in the SH, again, where the surface temperature perturbation extends into the mean westerly flow. Finally, in Fig. 9f, the same process occurs, namely that the southern end of the surface temperature perturbation extends into the SH westerlies (and waves can be seen in the SH) whereas the northern limit is embedded well equatorward of the mean NH westerly flow. Therefore, in the cases indicated by the reviewer, all of them are consistent with the easterlies acting as a barrier to Rossby wave propagation in the climatological mean. The authors therefore stand by the presented interpretation. Nevertheless, the reviewer raises a very important point that is not considered or even discussed in our work (i.e. that the  $u=0$  critical latitude assumption is not always appropriate). The experiments here could easily be applied to run experiments akin to Schneider and Watterson (1984), Watterson and Schneider (1987) and Zhao et al. (2015) and therefore acknowledgement of these papers must be included. We have included the following paragraph at the end of Section 4.2.2 to address this:

“Overall, the circulation responses to both of these tropical surface temperature perturbations are consistent with the results of Hoskins and Karoly (1981), Hoskins and Ambrizzi (1993) and Jin and Hoskins (1995). Nevertheless, there are cases where the cross-equatorial meridional flow can allow Rossby wave propagation through easterly flow (as discussed in Schneider and Watterson, 1984; Watterson and Schneider, 1987; Zhao et al., 2015). For example, Zhao et al. (2015) show that wave sources in

C5

the summer hemisphere can excite wave activity in the winter hemisphere if the meridional flow is from the summer to the winter hemisphere. Therefore, the idealised GCM with prescribed land surface temperatures in this study is likely to be useful for running similar experiments that address all of these features (where easterlies do and do not act as a barrier to wave propagation).”

Reviewer comment 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?

Response: The authors agree that it is worth noting the linear response and we have included the following in the first paragraph: “The atmospheric responses to the  $\pm 10$  K surface temperature perturbations over North America also appear to be of almost equal and opposing sign in each respective simulation, which suggests the circulation and precipitation respond in a linear way to the different surface temperature conditions.”

With respect to the drag caused by convection—tone of the other reviewers brought new literature to light that helps to elucidate the processes at work as a result of the AM10K and AMm10K experiments (different from those stated). We have removed the last two paragraphs in Section 4.2.4 and replaced them with a new discussion (and we have also updated Figs 11d and h). The new literature provides a basis for a different set of fascinating experiments that could be run with this new version of ACCESS to investigate the impact of continental heating on the sub-tropical high-pressure cells. The new literature is therefore more appropriate to discuss the AM10K and AMm10K experiments (and not the original discussion).

Reviewer comment F. Despite rather extended discussions (section 4), the compar-

C6

isons 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.

Response: The authors accept that there is speculation attached to the discussion section; however, we feel that such speculation is warranted in order to showcase the gaps in the literature where this model may be useful for increasing scientific understanding of atmospheric processes. The aim was not just to produce a new version of the model, but to provide several examples (and suggestions) of how it can (or could) be used. We feel that such speculation may encourage others to use this new model setup over a broad range of applications. Nevertheless, the language used is inappropriate given that the results are mainly a qualitative comparison of known physical processes. We have therefore taken the reviewer’s advice and changed some of the language in Section 4 (such as “clearly shows” to “is consistent with”) to reflect this.

Minor comments P1L19 Land temperatures also respond to the simulated weather, of course. Response: We accept this but it does not change the point of the sentence, which is that land temperatures are not normally prescribed and so can respond (through the atmosphere) to the prescribed SSTs.

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). Response: We have adjusted the text to refer to this setup being more akin to ACCESS1.0.

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. Response: Changed as suggested.

P3L9 ‘the the’. Response: corrected.

C7

P3L17 Does ‘all’ include FREE? Response: Yes, it does include FREE. This is done in order to be sure that the FREE soil moisture is consistent with those in the prescribed experiments.

P3L17. Does ‘deep soil’ mean layers 2, 3, 4? Is there flux through the bottom of 4? Response: yes, this does mean layers 2, 3 and 4 and there is zero flux boundary condition at the bottom to ensure energy conservation. The text has been updated to state, “. . . and deep soil temperatures (i.e. on all four levels described above). . .”.

P4L1 Should this be ‘SF\_EXCH’ –as in the Figure? Response: Yes, corrected.

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. Response: The figure has been simplified and the missing text is now visible.

P6L1,3 Does 00:00:30 mean 30 seconds after midnight? Should the first 00: be dropped? Response: Yes, corrected as suggested.

P6 L17, 23 (and elsewhere) ‘reduce’ is being used in an uncommon, intransitive way. Response: We have considered the use of ‘reduce’ and have altered the manuscript to account for this (i.e. remove and replace).

P6L27 ‘PRES’, but (6) has lower case. Response: Corrected.

Table 1. ‘Maritime Continents’? Response: Corrected.

P7L8 ‘at the’? Response: Corrected to “as the”.

P7L9 The soil temperatures and moisture are also prescribed, it seems. Response: We have included “. . . uses prescribed, climatological soil moisture, deep soil temperatures, SSTs. . .” for the FREE description in Section 2.3 to reflect what is said in Section 2.1.

P8L4 One might doubt if the processes in the response to such large (10K) anomalies

C8

can be known, from observations. Is this magnitude chosen to improve statistical significance of responses, given some expectation of linearity? Response: The value of 10 K was chosen to maximise the atmospheric response to the imposed temperature changes (i.e. so they would be obvious). Given that the atmospheric responses compare well (qualitatively) with those within the literature we are confident that our model is doing what we expect (even though the perturbations are exceptionally large, the atmospheric response is physically realistic). Ultimately, the experiments show how the model we have developed could be used in the wider scientific community.

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. Response: The difference is primarily due to aesthetics and does not require explanation in the text. We have looked at using grid-square shading for temperature and the figures appear less 'busy'; however, the current figures show interesting features more clearly (Fig. 3), such as the stationary Rossby waves, and so we are going to leave Fig. 3 unchanged. The precipitation plot, given the nonlinear colour bar, works better as grid box shading.

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. Response: The 1.5 m temperature provides the 'first check' on whether our setup is correct, as it is so close to the surface. If the 1.5 temperatures do not respond as expected then we know instantly that we have done something wrong. Furthermore, given that the atmospheric responses to each of the surface temperature perturbations are also what we expect (e.g. increased convection over the Amazon), evaluating the temperatures on the first level of the atmosphere would not add more insight beyond what is already presented.

P9L5,7 'alternating' does not seem a good description here –although it is better for precip. Response: The anomalies do alternate between positive and negative (and are

C9

clearer than the precipitation ones) so we would prefer to keep that description.

P9L10 lower case 'k' Response: Corrected.

P10L16 'be representative' Response: Corrected as suggested.

P11L11 'Similarly' is odd. As before, CON2-1 is expected to be the same, but CON1-FREE is the main test. Response: Agreed, but this is important in the context of the new model developed here. It re-iterates that the initial conditions (as expected and desired) are unimportant.

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? Response: We accept that the surface pressure may be unchanged despite a lowering in MSLP; however, given the circulation response shown in Fig 8 (i.e. ascent primarily over the land and descent over the ocean) it would seem likely that the surface pressure is also reducing due to the higher land surface temperatures in the ALL10K simulation. The MSLP field, in this case (Section 3), provides a simple illustration of the impacts of the surface temperature perturbation experiments and are not key to the interpretation. The main interpretation is in Section 4 (and Fig 8) where the circulation changes are shown.

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? Response: We state that, "... snow melt is prevented earlier in CON1 than FREE and so snow amount are, on average, higher in CON1 during the cold season, which causes T1.5 to be systematically lower", which should answer this point.

P13 Consistent with the earlier suggestion regarding CON1 and 2, this 4.1.2 seems unnecessary. Response: Given the brevity of this section and that this is a model development paper, we think this section should remain to show any potential future

C10

users of this model that the initial conditions they use do not matter. Again, while this is expected, it should be shown for completeness.

P13L9 Are the SSTs unchanged in Chadwick's warmer-land run? Response: Yes. They only change the solar constant and use prescribed SSTs.

P13L22. Would vertical velocity closer to the centre of the moisture column (e.g. 850 or 700hPa) be an even better match? Response: We would expect areas with increased deep convection to extend through 500 hPa, which is why this field was chosen to compare with the precipitation. Furthermore, given the very high pattern correlation (-0.69), it appears that the 500 hPa omega field is useful for explaining the changes in precipitation.

P14L2 'increases subsidence over India' is not clear. Response: Changed to, "... results in positive differences in  $\omega_{500}$  for ALL10K relative to CON1 over southern India, which would suppress precipitation."

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? Response: Yes, the increased convection over the Maritime Continent is from the increased surface temperatures (i.e. imposed heat sources). The authors see how this language is a bit vague so we have been more explicit and changed the text to, "... propagating away from the imposed tropical heating sources (Gill, 1980), which in this case are from increasing surface temperatures by 10 K and the resulting increase in latent heat release (inferred from the increase in precipitation, see Figs. 4(d) and (e))." There does not seem to be any reason why the reduced rainfall over the sea would counter the effect of enhanced rainfall over the land. Given that there is an increase in ascent over the islands (as indicated by the increased rainfall) from increasing the surface temperatures, then there should be subsidence in the region surrounding that ascent (suppressing rainfall). Such an impact can also be seen in the AMA10K simu-

C11

lation, and to a large extent in the ALL10K simulation, where ascent is enhanced over the land with subsidence over the ocean.

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? Response: Changed the text as suggested and corrected the colour bars. The figure titles are a bit bulky but we feel they are necessary to make it easier to know what each panel is showing without having to re-read the caption.

P17L21What supports 'the local response is governed by the strengthening .. of existing circulations'? Response: We agree that this statement is vague and therefore unnecessary. We have removed it.

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C12