This paper explains the impacts of including an improved stomatal conductance scheme in the CABLE land surface model on climate biases in the ACCESS global climate model. The new stomatal conductance model is based on global observations with PFT-specific values for the parameter g1, and it has been documented in a few previous papers. The large biases in the climate model are still present with the new stomatal conductance model, but at least this revision shows some improvement. For example, due to lower ET the daily Tmax is increased, and the bias in the annual maximum Tmax is reduced. This manuscript is well written, and the results are clearly presented. It clearly represents an improvement in both the land surface model and in the coupled modelling system.

We thank the reviewer for this summary; we agree that this captures our paper.

I have a few questions for clarification and some recommendations to improve the paper.

General comments/questions for the authors:

1. My first question relates to the impact of the model on Tmin. Mechanistically, what causes the changes in Tmin? Is it small differences in gs, or is it due to residual effects of the higher daytime temperatures (i.e.: changes in boundary layer height or turbulence due to the changes in the surface energy budget)? I would expect stomatal conductance to be 0 overnight since there is no assimilation. What is the magnitude of the g0 term in Equation 3?

For all simulations using the Medlyn stomatal conductance model, g_0 = 0 mol H_2O m^{-2} s^{-1}, meaning that stomatal conductance goes to zero under low light and, importantly, high VPD conditions. By contrast, in the Leuning stomatal conductance model, g_0 = 0.01 and 0.04 mol H_2O m^2 s^1 for C3 and C4 species, respectively. We acknowledge that this information was accidently omitted from our original manuscript and we now add this information at the end of section 2.2:

"Additionally, for the MED scheme, g_0 = 0 mol H_2O m^{-2} s^{-1}, meaning that g_s goes to zero under low light and importantly, high D conditions. By contrast, in the default LEU scheme, g_0 = 0.01 and 0.04 mol H_2O m^{-2} s^{-1} for C3 and C4 species respectively."

With regards to what causes the increase in Tmin, we suspect that this has to be due to the residual effect of higher Tmax during the day and this is made clearer in the discussion:

"We suspect that the increase in T_{MIN} is a residual effect of the increases in T_{MAX} and TXx during the day as differences in g_s are likely to be minimal at night-time"

2. I also have questions regarding the impact of the changes on NPP shown in Figure 8. The authors state that the differences in NPP between the models is due to changes in precipitation, but shouldn't there also be a first-order effect from the different approaches to gs? In CABLE, is the stomatal conductance equation solved iteratively so that the original A is impacted by the gs? If this is true, the relationship between the primary productivity and gs needs to be explained, and I would like to see more explanation for the changes to NPP. If this has been addressed in one of the previous papers implementing this stomatal conductance model, that could be mentioned here instead of a full explanation in this text.

The reviewer is correct, CABLE does iteratively solve A and g_s together, and this is made clearer at the end of section 2.2:

"Finally, we note that for both schemes in CABLE, A and g_S are solved iteratively."

We agree with the reviewer that this text in relation to NPP was unclear and gave the impression that changes in NPP related principally to changes in precipitation via ET. We have amended this text to:

"The reduction in g_s also results in an associated reduction in NPP across the Boreal forests in JJA (Fig. 8b) and in the tropics in both JJA and DJF (Fig. 8). This reduction in NPP is also a result of a change in precipitation across these regions, which results from a reduction in ET due to g_s. There is also an increase in NPP in JJA in the region to the north and east of the Mediterranean, consistent with an increase in ET in this region (Fig. 6f) using MED."

3. The authors state in the Discussion that the changes 'first and foremost' result in changing the ET, and this causes the changes in temperature and precipitation. So why not lead in the results section with the impacts on global ET?

We agree. We have moved this text to the second paragraph and moved the longer text around the advantages of this Medlyn model to the end of the Discussion. We have retained the first paragraph of the discussion in its former position since this acts as a summary of our paper.

4. It would be useful to provide more explanation of some of the overall biases in the model. For example, at line 14 on Page 5243, it is stated that the biases in Tmin over North America in JJA are due to clouds – can this be elaborated upon?

We have added a significant paragraph to the Discussion. It is very hard to go beyond this level of explanation since it would require a case-by-case analysis and is clearly well beyond the scope of this paper. The added paragraph is as follows:

"The ACCESSv1.3 model is a skillful global climate model, performing at the top end in the evaluation of CMIP-5 models (Flato et al., 2013). While the model's overall climatology is very good, there are regional systematic biases that exist irrespective of whether the LEU or MED scheme is used. However, we note that MED does make the model worse in some regions. Perhaps the most serious bias in temperature is over North America in summer (Figs. 2, 3, 4). The biases in ACCESS using LEU reach 5-7°C in T_{MAX} *, 8-9°C in* T_{MIN} *and 6-8°C in TXx. Using MED makes these biases worse, by ~0.5°C for* T_{MAX} *and* T_{MIN} *and by ~1°C for TXx. Given the magnitude of the error in ACCESS, we suggest that using MED does not really make the model significantly worse since it is already very poor in this region. The poor performance of ACCESS over North America, across several model versions, has been previously reported. Bi et al. (2013) show errors of 3-4°C in the mean air temperature over North America. This does not appear to be linked with low rainfall but rather, it appears linked with an underestimation of cloud coverage over North America despite the overall tendency to simulate slightly too much cloud over northern hemisphere land (Bi et al., 2013). Franklin et al. (2013) examined ACCESS1.3's simulation of clouds but did not focus specifically on North America. They did note problems with capturing convective regimes; these showed too weak a dependence on large-scale dynamics in comparison to observations. The change from the LEU to the MED scheme would not be expected to affect large-scale dynamics or how these processes affect clouds. Small decreases in ACCESS's skill in capturing North America's climate linked with the MED scheme are very likely insignificant; if the large-scale cloud fields were simulated well MED might make a small positive impact in this region but any benefits are currently swamped by the poor cloud climatology. A similar problem exists over the Indian monsoon region where there is a very serious rainfall bias (Fig. 7). The MED scheme does not add value here because there are major systematic weaknesses in the generation of the monsoon linked with larger-scale dynamics as distinct from terrestrial processes."*

5. For the DTR results, to my eye it looks like the DTR improvements are mostly due to the increase in Tmax - is this true? If anything, it looks like the increase in Tmin in the boreal forest regions would serve to decrease the DTR.

Yes, this is correct and the text has been modified accordingly:

"Figure 5 shows little impact by switching to MED in DJF, but in JJA there are large

areas of the Northern Hemisphere, coincident with the boreal forests, where the 3–5°C error in DTR is reduced by 10–20%. This is caused by the larger increase in T_{MAX} (Fig. 2(f)) versus T_{MIN} (Fig. 3(f)) "

6. What is meant by "other warm extremes" in the Discussion, Page 5246 Line 8? Does this mean the TXx index, or something else?

Yes, we meant TXx. We have clarified this in the text.

Tables and Figures:

Figure 1 is not referenced in the text.
It was referenced on page 4, line 103.

Minor typographic comments:

Introduction, page 5238: The last two sentences of the introduction could be made clearer. For example, in the phrase "We seek to determine whether these problems, affecting these and other extreme indices…" the repeated use of "these" in this sentence makes it vague.

Resolved – we modified the sentence to make this clearer:

"These were, in part, attributed to an overestimation of evapotranspiration linked to weaknesses in the representation of land processes. We seek to determine whether the biases identified by Lorenz et al. (2014) can be resolved in part via the parameterization of g_S"

Also you could remove the "We also note that" in the last sentence of the section.

We have now deleted this sentence entirely.

Results, page 5244: In reference to the dry bias in the JJA precipitation (lines 15-20), I think this should be the region to the north east of the Mediterranean.

Yes, we have corrected the text.

Conclusions: There are only two goals discussed in the introduction and conclusions, so is the first sentence "three gaols" is a typo (or else something is missing in the paper!)?

We confirm this is a typo and the text has been corrected to:
"We had two goals for this paper."