Geosci. Model Dev. Discuss., 8, C1713–C1715, 2015
www.geosci-model-dev-discuss.net/8/C1713/2015/

Geoscientific Model Development Discussions
Open Access

GMDD
8, C1713–C1715, 2015

Interactive Comment

Interactive comment on "Implementation of an optimal stomatal conductance model in the Australian Community Climate Earth Systems Simulator (ACCESS1.3b)" *by* J. Kala et al.

Anonymous Referee #1

Received and published: 13 August 2015

This is a fairly straightforward manuscript. Various of the authors have previously developed a new stomatal conductance model, obtained PFT-specific parameters for the model (g1), implemented the stomatal conductance model in the CABLE land surface model, and tested the CABLE model in offline (uncoupled) simulations. Here, the authors conduct climate simulations and document the impact of the stomatal conductance model on the simulated climate.

1. My biggest concern is that the authors oversell the importance of their stomatal conductance model. I agree with their assertions that their model has a theoretical basis (in contrast to other more widely used empirical models) and that alone is a significant step forward. However, the authors repeatedly state that the model improves

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

the simulated climate, while ignoring the fact that the model enhances temperature biases in many regions of the world. This is seen in the abstract, where the authors tout the improved simulation in boreal regions of the Northern Hemisphere during summer, but do not mention the degradation of the simulation in much of the rest of the world. Similar generalizations are made in the discussion and conclusions. Figures 2 and 3 clearly show Tmax and Tmin warm in much of the world because of the stomatal conductance model, and this enhances biases (e.g., North America, South America). These biases are acknowledged in the results, but then are not mentioned when the authors summarize "improvements".

2. Rather than touting improvements, a more robust conclusion is that the stomatal conductance model decreases evapotranspiration and warms the surface air temperature in most areas of the world. For the ACCESS model, this improves the simulation in boreal regions of the Northern Hemisphere during summer. In other regions, the climate is degraded. A similar result is quite likely to happen if the stomatal conductance model is implemented in other climate models (i.e., reduced evapotranspiration and warmer surface air temperature). Is this indeed the robust outcome of the simulations? If so, then that is the key result and gives guidance to other modelers about the generality of the stomatal model. The fact that the ACCESS climate is improved in one region of the world during one season is not that significant or generalizable across climate models (given the existing temperature biases in ACCESS).

3. What are the reasons for the large biases in Tmax and Tmin and the diurnal temperature range? Is this related to clouds, radiation, soil water stress? How might these affect the results presented here regarding the performance of the stomatal conductance model? Might the improvements seen in boreal regions with the stomatal conductance model simply compensate for biases in clouds, radiation, etc? And likewise, the degradation seen in other regions may be because the stomatal conductance model accentuates biases in radiation, soil water, etc. The manuscript lacks a thorough discussion of biases, their causes with respect to the ACCESS model, how these af-

GMDD
8, C1713–C1715, 2015

fect the stomatal conductance model, and whether the stomatal model is improved or is simply compensating for these biases.

4. A key result of the study is buried in the discussion and is not highlighted in the abstract or conclusion. Lines 11-13 (page 5247) state that the difference between the two stomatal models relates to the slope parameter g1 and its PFT-specific values, whereas the Leuning model uses a constant g1 (distinguishing only C3 and C4 plants). This is a critical result of the study. It suggests that PFT-specific g1 would similarly change the Leuning model, perhaps so that the two models are not distinguishable in their climate effects. This statement should (i) have some supporting analysis (it is given as a statement of fact without any documentation) and (ii) is a key documentation of the stomatal model that needs to be in the abstract and conclusion (not buried in the discussion).

5. The results show a reduction in evapotranspiration and increase in Tmax and Tmin because of the stomatal conductance model. Presumably this is because stomatal conductance decreases. However, the authors never present maps of stomatal conductance to show this. A map of stomatal conductance (e.g., mid-day maximum) is essential to show readers the magnitude of the change in stomatal conductance and its geography.

6. Give values for g0. Is this the same for all PFTs? Is it the same for both stomatal models (Leuning and Medlyn)?

7. The cabon cycle and NPP are mentioned in the abstract and also in the results (Fig 8). However, the methods (section 2.3) states that carbon pool dynamics was not simulated (line 9, page 5241). What, then, is shown in Figure 8?

Interactive comment on Geosci. Model Dev. Discuss., 8, 5235, 2015.