Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-501-RC2, 2018

Biogeosciences Discussions
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Interactive comment on "Evaluating and Improving the Community Land Model's Sensitivity to Land Cover" *by* Ronny Meier et al.

Anonymous Referee #2

Received and published: 24 March 2018

Review of bg-2017-501: Evaluating and improving the Community Land Model's sensitivity to land cover

Summary

The authors compare CLM modeled differences in biophysical properties between forest and open land with remote sensing estimates (MODIS-based) and additional observational data for evapotranspiration (ET). They find that albedo and average and max surface radiative temperature differences are adequately simulated by CLM, but min temperature and ET are not. They note that CLM with separate pft soil columns performs better than the default shared soil column, except for ET. They conclude that error in forest transpiration parameters/processes is responsible for the relatively poor ET performance of CLM, and make modifications to improve the ET response. These

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Discussion paper

modifications improve the ET response somewhat, and mostly in lower latitudes, but discrepancies with observations still remain.

Overall the paper is clearly written and the methods are sound. It advances our understanding of land change effects on surface properties and how to go about evaluating and estimating such effects. My main concerns are with some of the interpretations of the results, and the premature conclusion that vegetation transpiration is the main source of ET errors in the model. To make this conclusion the authors need to carry out further tests related to soil evaporation and potentially interception evaporation as well (as this may be compensating to some degree for error in soil evaporation). These are the main issues that need to be addressed for publication (more detail is provided below):

1) Complete the analysis of the sources of error. You test only things related to vegetation transpiration and not soil evaporation. Your data do not clearly indicate that vegetation is the main driver, and in fact show that soil evaporation could also be a dominant source of error. Just because your modifications for transpiration show some improvement does not mean that they are correct, because you could be over-compensating or over-fitting these parameter values.

2) Please provide a metric for quantifying the effects of the modifications. Figure 7 (and the aggregate climate zones) is not adequate for demonstrating significant improvement of the results due to the modifications.

Specific comments and suggestions:

Abstract

Introduction

Methods and data

page 6 lines 7-11: I think CLM also outputs a surface radiative temperature. Why didn't you use this?

Results and discussion

page 7 line 21: confidence in which observations? the non-outliers i assume.

page 7 line 28: be clear that it is the deciduous/evergreen trees in the model that are the source

page 8 lines 21-22 and 31-32: this statement is not supported by your data or the rest of this paragraph. while the visual pattern between the VTR and total is similar, the soil evap effects are compensated for by the interception effects, thus leaving VTR to dominate the pattern. but this doesn't mean that the soil evap is not a main contributor, especially in the tropics. and you mention the biases in the non-forest that contribute to this discrepancy as well. Figure 4 also indicates that the soil evaporation dominates the total ET pattern in the higher latitudes, which is where your modifications show little improvement.

page 9 line 8 should be referencing table 2 here

page 9 line 25: at all latitudes

page 9 line 26: same sign as delta LST

page 10 lines 5-6: if comparing for lee et al, why reference alkama and cescatti for the amplification? you should include the delta LST per degree from lee et al for a consistent comparison, and to show that these observations also show this amplification

page 11 line 5: not sure that this is the case

page 12 lines 11-22: this indicates that your hypothesis regarding VTR as the main driver of discrepancies may not be correct. while you get improvements, soil evap remains a problem, and you may even be overcompensating with the VTR related modifications

Also, while the pft level comparison with GETA looks good, the climate zone comparison is more difficult to evaluate. Aggregating to these climate zones smooths out a lot of spatial variability, and may be too coarse to adequately evaluate the modifications. can you calculate a metric to quantify the effects of the modifications? what do pixel-level correlations between the model and the obs look like? are these correlations improved by the modifications? would zonal grouping make more sense than the climate zones?

page 13 line 18: is this because you used prescribed atmospheric forcing?

page 13 lines 29-30 i am still not completely convinced of this

page 15 lines 7-8: this suggests that soil evaporation may also be a main factor

Figures and tables

Generally, why show a CI for only the modis zonal average? What about the other data and the model outputs? And is CI the best metric to depict variability here? There are many reasons for variability around the globe at a given latitude (e.g., different weather patterns, continental vs maritime), and we should not expect a zonal mean to behave like a population mean estimate that supposedly characterizes a more homogenous group.

Figure 2a: this does not appear to be the correct figure. it does not match with the averages in panel 2c, nor table 2

Figure 5 row labels do not that these are differences, which can be confusing

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