

***Interactive comment on* “Baseline Evaluation of the Impact of Updates to the MIT Earth System Model on its Model Parameter Estimates” by Alex G. Libardoni et al.**

Anonymous Referee #1

Received and published: 1 May 2018

Review of:

Baseline evaluation of the impact of updates to the MIT Earth System Model on its model parameter estimates

By:

A. G. Libardoni, C. E. Forest, A. P. Sokolov, and E. Monier

Submitted to: Geoscientific Model Development

General Comments:

Printer-friendly version

Discussion paper



This is a well-written and interesting paper documenting new updates to the MIT Earth System Model. In particular, the authors highlight how updated forcings and changes to the land surface model influence model parameter estimates of equilibrium climate sensitivity and the transient climate response (TCR).

This is a useful paper that is well-suited for GMD. However, it is a bit brief and lacks sufficient depth for GMD. In particular, I think the paper falls a bit short in providing enough details about the model "development", which is key for this journal. I recommend adding more description of the model and key changes since the last version (right now the descriptions of the model and forcings are limited to two dense paragraphs), or perhaps a simple summary of the model lineage and history and/or recent results from the authors' companion papers? Additionally, it would be nice to see more explanation about what causes the differences between model versions and connections with broader climate problems (e.g. probabilistic projections) beyond the reporting of the model sensitivities... such as more description of the importance (or lack thereof) of ocean heat uptake and aerosol forcing, and how new model parameter estimates compare to previous versions. I provide additional specific comments and suggestions below:

Specific Comments:

P1, L10: "absent an increase" is odd wording. P1, L12: This statement seems redundant to line 4-5 P1, L14-15: What causes these shifts? P1, L15-16: So if the land surface model has limited effect on temperature evolution, is it updates to the forcings that cause the differences in climate sensitivity estimates? It's not entirely clear what points the authors are trying to convey here. I suggest tightening up the abstract to highlight the significance.

P7, last paragraph: The authors raise interesting, but somewhat contradictory, points. They state that reducing the number of diagnostics from 3 to 2 has little impact on model parameter estimates, but then go on to state that CS estimates are lower when

[Printer-friendly version](#)[Discussion paper](#)

using 2 diagnostics. Why are the results insensitive to the upper-air diagnostic? Also, the constraint on K_v is not clear. Is there any update since what was shown their previous work (e.g. Libardoni and Forest 2011)? I suggest adding more details to these points to help the reader.

P8, L10: Can you show a plot of the ECS pdf for IGSM and MESM for comparison?

P9, L3-4: How do these new estimates of net aerosol forcing compare with other recent estimates?

P10: L14: I'm a little unclear how ocean diffusivity fits in with the analysis. Why did the old ensemble cut off high values of K_v ? It is also relatively insensitive to the model updates compared to aerosol forcing and equilibrium climate sensitivity. Why is this? I recommend the authors streamline the results and discussion sections to include a summary of key points about each model parameter, the constraints and model sensitivities, and physical reasoning for the differences.

P12, L7: Why choose a third-order polynomial here? Is there sensitivity in the fits to the functional form? Would you expect similar results in terms of model differences using a 2nd order polynomial?

P12, L24-25: The authors state that the shift towards higher transient climate response is driven by higher climate sensitivity in MESM, but there is not enough explanation in my opinion as to why there is a larger CS in MESM compared to previous versions, how they compare (e.g. posterior distributions), and to what extent the updated forcings play a role.

P12-13: The conclusions provide a nice summary of the paper's key points. I suggest expanding the results section to include more in-depth discussion along these lines.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-54>, 2018.

Printer-friendly version

Discussion paper

