Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-281-RC2, 2019

Geoscientific Model Development Discussions

Interactive comment on "Implementation of Yale Interactive terrestrial Biosphere model version 1.0 into GEOS-Chem version 12.0.0: a tool for biosphere-chemistry interactions" *by* Yadong Lei et al.

Anonymous Referee #2

Received and published: 9 December 2019

This study represents a new biosphere-chemistry modeling framework that simulates online, two-way interactions between surface ozone and vegetation, mainly through the linkages between stomatal conductance, leaf area index (LAI) and dry deposition. Global model-observation comparison for simulated gross primary productivity (GPP), LAI, ozone concentrations and dry deposition velocities have been conducted using a large ensemble of datasets. This work is important in laying a foundation for more in-depth future studies of biosphere-atmosphere interactions. However, as of the current form the manuscript lacks sufficient details regarding model implementation, which

I believe is important for a GMD paper. I would recommend the publication of this manuscript should the following model details are included, addressed and discussed.

Specific comments:

P6 L129: I think here and elsewhere, the units for all variables should be included in all the equations listed.

P7 L137: Carbon allocation and LAI simulations are a very important part of the modeling framework, but no details have been given. The schemes/algorithms used for simulating carbon allocation and LAI should be described.

P8 L157: Why is aerodynamic resistance not included in the calculation of ozone fluxes? The ozone simulated by any chemical transport model should be at the lowest model layer, but that should be different enough from the ozone concentration at the canopy top. Please justify. Moreover, shouldn't the ozone flux calculated here for ozone damage be consistent with the dry deposition velocity/flux calculation in GC? The internally inconsistent ways to represent ozone fluxes between GC and YIBs seem to reduce the usefulness of GC-YIBs as a coupling tool.

P8 L168: $4^\circ \times 5^\circ$ appears to be a rather low resolution. While the issue of computational expense is understandable, I recommend the authors to discuss how such a low resolution of simulations may interfere with the accuracy of simulated variables (ozone concentrations, GPP, etc.) as compared with observations.

P10 L210: While the replacement of Olson land-type stomatal resistance with YIBs plant-functional-type (PFT) stomatal resistance is mentioned, could the authors also explain how the conversion of other land-type resistances to YIBs PFT resistances was done? In general, it would be highly useful to explain how Olson land types are matched and mapped with YIBs PFTs. A conversion table in the supplement would really help.

P11 L223: YIBs simulates stomatal conductance first at the leaf level, while GC takes

in conductance at the canopy level. Appropriate scaling between the two levels should be included and discussed.

P12 L250: Four years of spin-up for LAI simulations is probably insufficient. LAI typically takes decades to stabilize, depending on the initial conditions of LAI. The authors are recommended to explain in greater detail such an issue, show whether LAI has reached a steady state in four years, and state specifically what LAI is used as the initial conditions.

P14 L306: I think the authors meant Online_GS here instead of Online_LAI.

P15 L308: I think the authors meant Online_LAI here instead of Online_GS.

P15 L324: The authors need to justify why BVOC changes resulting from LAI changes are not the dominant factor (in addition to stating the broadly consistent spatial patterns). How BVOC changes should influence the results and interpretation should be discussed in greater detail.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2019-281, 2019.