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# *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 #3**

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This work integrates and couples together a global atmospheric chemistry model (GEOS-Chem) and a terrestrial biosphere model (YIBs) in order to investigate the feedbacks associated between the two, often separately simulated, systems. First, the authors evaluate their integrated model against observed or baseline measures of plant activity (GPP/LAI) and an example chemical species (ozone concentration). They also compare the performance of the coupled and integrated models against observed ozone dry deposition velocities, finding the coupled model an improvement. Using this coupled model, the authors then investigate the impact ozone concentration has on



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plant activity using differing sensitivities to ozone damage. Overall, this work is timely and addresses an important issue within the modeling of these systems. The description of the model and evaluation is carried-out well with appropriately supportive figures. However, the paper does not go far enough to be truly impactful and confidently useful to the community in its present form, but rather, substantial addition and expansion is required for publishing in GMD. The authors should either expand the evaluation of the model to show that coupling truly does improve comparisons or provide additional applicational evidence for the importance of such coupling to understanding biosphere-atmosphere interactions. Further specific comments and recommendations are listed below.

1) While the PM impact on plants is mentioned as an important process to consider in the introduction (lines 63-69), there is no integration description or evaluation in this paper, and no further mention until the last paragraph. Perhaps clarify the focus of the paper at the beginning to adjust expectations.

2) Aerosols are not always beneficial to vegetation if the total radiation decreases more than the enhancing effect caused by diffusion (line 64).

3) Since GC-YIBs integrates two existing models, sections 2.1 and 2.2 can be trimmed to only include the relevant equations and processes discussed in the remainder of the paper.

4) More description of the “satellite-based land types and cover fraction” (lines 122 and 229) would be useful as this is quite vague.

5) The fact that coefficient a is uncertain and can and will be varied in different simulations is not clear from the current description in line 153.

6) Much work has been done to evaluate the GEOS-Chem dry deposition scheme for ozone and understand the importance of dry deposition schemes in general (e.g. Silva and Heald 2018, JGR, Wong et al 2019, ACP) but these issues are not mentioned

here (neither sections 2.2 nor 4). Especially important to consider is lack of observations to truly constrain ozone dry deposition globally and the uncertainty over various timescales and in spatially heterogenous regions.

7) The title of section 2.5 should read “Evaluation data”, as models are evaluated, not validated.

8) Why are only 9 sites used for the comparison of ozone dry deposition velocity (lines 266, 341-355, Table 2, Figure 8)? Many more data are available as in Silva and Heald 2018.

9) Further description of the limitations and errors of both the observed LAI and GPP product should be included (section 2.5), and clarification should be made that GPP is not observed (line 271).

10) How do the simulated GPP/LAI and ozone concentrations from offline GC-YIBs compare to those values from the original YIBs and GC, respectively? Are the original model configurations degraded or enhanced by the integration and use of a common land type and meteorological driver? Are the magnitudes of these changes similar to the noted improvements seen when the coupling is turned on?

11) Line 281 attributes the GPP bias to an underestimation in the benchmark GPP for tropical rainforest. Could the differences from using a different meteorology dataset instead be biasing the model (line 283)?

12) Compared to what other drivers (BVOC emissions changes?) are dry deposition velocities the dominant driver in the change in O3 (line 324)? Try testing the impacts of the changing other drivers, rather than relying only on consistent spatial patterns (line 323).

13) Given the small sample size and scattered data (Figure 8), the statistics cited for the comparison of dry deposition velocities in coupled GC-YIBs compared to offline GC-YIBs do not provide for high confidence that the model is truly improved with the

coupling of these systems (lines 341-355). A more robust analysis should be undertaken to account for the errors in both the observed and simulated values and present the confidence with which the model could be said to truly be improved.

14) The coupling of these systems for the assessment of ozone damages to vegetation is presented as a key motivation for this study, but the differences in damage between this coupled model and previous models are not discussed (mentioned only in line 372). The discussion should be expanded to explain the differences and highlight the advantages of coupling the systems in section 3.3.

15) Other studies including Lin et al, 2019 GBC for the GFDL models have also investigated the coupled biosphere and atmosphere in similar ways with regards to ozone and are worth discussion in addition to the CESM work. If the ozone dry deposition is the chief application of the model so far, more clarity should be made in the discussion of the uncertainties that already exist in simulating dry deposition globally.

16) One way to justify the slow model speed (line 420) for the modest model improvements shown through coupling would be to expand upon the usefulness of the applications only so far mentioned in lines 428-444.

17) While supported in part at Harvard, GEOS-Chem is developed and maintained by a global community of atmospheric chemists, not one group (line 449), and should be acknowledged as such.

18) Minor grammatical issues are present throughout, especially omission of articles before nouns. (example, line 48 “from terrestrial biosphere”).

Papers cited: Lin et al., Sensitivity of Ozone Dry Deposition to EcosystemâĂŘAtmosphere Interactions: A Critical Appraisal of Observations and Simulations, https://doi.org/10.1029/2018GB006157, (2019). Silva and Heald, Investigating Dry Deposition of Ozone to Vegetation, https://doi.org/10.1002/2017JD027278, (2018). Wong et al., Importance of dry deposition parameterization choice in global simulations of

surface ozone, https://doi.org/10.5194/acp-19-14365-2019, (2019).

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