Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2017-75-AC4, 2017

Geoscientific Model Development Discussions

Interactive comment on "Description and Validation of the Simple, Efficient, Dynamic, Global, Ecological Simulator (SEDGES v.1.0)" *by* Pablo Paiewonsky and Oliver Elison Timm

Pablo Paiewonsky and Oliver Elison Timm

ppaiewonsky@albany.edu

Received and published: 30 August 2017

authors' final response

August 30, 2017

Reviewer comments that were not addressed in our previous responses are addressed now. Also, some more specific comments on the changes are included.

With respect to reviewer #1's comments:

The wording could be tightened in the Abstract to explain how SEDGES is "auxiliary" to the land-atmosphere coupling scheme. This will make it easier to understand better the sentence "evaluate. . ..using a simple land surface scheme that is driven by reanalysis data". If SEDGES is compared extensively to another existing land surface model as ground truth, then from the Abstract alone, the reader will wonder what it does in addition.

We think that the original abstract was ambiguous in its meaning. We have updated our abstract to hopefully reduce such confusion, although we have not explicitly stated which variables are simulated by SEDGES because we feel that this is too specific for an abstract. Regardless, we thank reviewer #1 for signaling the need to revise the abstract.

P2, lines 1-9 reads as if unduly critical of existing analyses– potentially of the original model developers. Was there really no validation of SimBA?

We added a short clarification of this in the revised paper. For more clarification, see our first response to reviewer #1.

P12, 13. The terminology could be made clearer between leaf cover fraction and forest cover fraction. In some DGVMs, these could potentially be the same thing. I guess from Equation (18) this is to do with wilting of leaves and that does not appear in forest cover fraction. It also includes seasonal phenology?

In addition to adding a definition of forest cover fraction (as stated in our first response to reviewer #1), we changed the notation for leaf cover fraction throughout the paper from f_{veg} to f_{leaf}, which should hopefully emphasize that it is *leaf* cover fraction and not *vegetative* cover fraction. This also required some revisions to appendix B, in which we had, in fact, interchanged the use of leaf cover fraction and vegetative cover fraction. We also added text to indicate that LAI has a one-to-one relationship with the leaf cover fraction, and we added an additional equation to make explicit how the LAI of moist soils relates to the leaf cover fraction with moist soils. All these changes should also clear up the second reviewer's confusion with regards to LAI and leaf cover fraction.

p. 20: However I am less convinced by the need to compare against other land surface models.

With respect to this issue, that was also raised by reviewer #2, we have now eliminated the comparisons of vegetative and soil carbon simulated by SEDGES with that simulated by Earth System Models (Todd-Brown et al., 2013; Jiang et al., 2015) in section , since these carbon values depend on the simulated climate in those models (see section 5.3). The remaining comparisons have been left in.

With respect to reviewer #2's comments:

Specifically in response to the reviewers's criticism that we should show more non-GPP

results, we have expanded the section on ET and runoff, and now include a figure for each showing the full spatial climatological annual mean values for SEDGES and the reference datasets.

We decided not to make any comparisons of SEDGES with VECODE because there is not that much information to be gained from comparing global NPP and forest cover fraction. The latter is not that important of a variable, at least in SEDGES.

Also as promised in our first response to the reviewer, we clarified leaf cover fraction and made notational changes, as indicated above in our addressing of reviewer #1's comments.

Discussion part must be developed. For each matchless outputs, the authors must explain why they obtain this result and how this lack of precision can affect the outputs of coupled simulations.

As per the reviewer's wishes, we have expanded our discussion and conclusions section. Overall we discuss more critically some outstanding problems in the parameterizations on NPP- and ET-relevant processes. In so doing, we address the fixed ci/ca ratio that reviewer #3 was concerned about, and suggest potential candidates for future improvements. Furthermore, we discuss limits of the current model scheme for paleoclimate applications.

As promised in our first response to the reviewer, we included a brief discussion in the discussion and conclusions section on the expected effect of SEDGES biases on coupled simulations. Part of doing this entailed expanding the discussion in the albedo validation section.

With respect to reviewer #3's comments:

Although we had said that "we think it would be beneficial to discuss in the manuscript what the ramifications are of using a fixed ci/ca on GPP, for the situations in which it is significant", we decided, at least for now, to minimize discussion of the negative

ramifications of using a fixed ci/ca on GPP, and instead mention (in the discussion and conclusions section) that the light use efficiency approach without an explicit formulation for ci is quite common and has not been found to be less accurate in simulating GPP. However, as suggested in our first response to reviewer #3, we have included discussion on the effects of using a fixed ci/ca on transpiration (also in the discussion and conclusions section).

Other changes that were made include the following:

Throughout the paper, to be more specific, "soil carbon" was replaced with "soil organic carbon". The URL commands were used for the URL's, which are mostly in the data availability section.

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-75/gmd-2017-75-AC4-supplement.pdf

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