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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

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Reply to Referee #2

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To being with, we thank the reviewer for taking the time to comment on our paper and provide us with feedback.

`I think it will be better to pay more attention to understand how far SEDGES can be simplified before losing efficiency in coupled simulation.`

We disagree that this is necessary.

The reviewer then suggests that we compare and contrast SEDGES and its assumed framework (i.e. SEDGES and the type of land surface model that it presupposes that it forms a part of) with other second generation land surface models. The intention behind this suggestion is reasonable and this sort of comparison might be useful to some readers, so we will include a couple of sentences on this comparison. Note that the SEDGES model framework is not a second generation land surface model. Rather, it has attributes that are specific to both the first and third generations of land surface models, as described by Pitman (2003) (note: we assume that the reviewer is referring to the review by Pitman (2003), instead of Pitman et al. (2005), which we could not locate).

The reviewer says that we must answer the question of how we deal with the trade-off between simplification, precision, and robustness. Prentice et al. (2015), in fact, suggest that robustness tends to be lost as land surface models increase in complexity. As we mention in our response to reviewer #3, most of the parameterization changes that are made to SimBA (on which SEDGES is based) do not significantly increase the complexity. However, when introducing the changes that do appreciably increase the complexity, we will provide some discussion on the aforementioned trade-off in the revised manuscript.

In response to the reviewer's comments on the lack of clarity as to what exactly SEDGES is, i.e. to what extent it is a model that is built from scratch as opposed to an improved version of SimBA, we will make this clearer in the revised manuscript.

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But if the authors will add features to SimBA, they increase
the model complexity, they do not create simple model.  The
authors need to clearer this part !
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In response, even though it is more complex than SimBA, SEDGES is still a simple model. One cannot say that it qualifies as "intermediate complexity". We will make this clear in the revised manuscript.

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The authors argue that the GPP was better simulated by SEDGES
than other state- of-the-art LSM models but strangely, authors
only show this comparison for GPP. Is there something they
don't want to show for ET, LAI, soil carbon, albedo ...etc ?
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Two things here:

We did not go as far as saying that GPP was overall better simulated by SEDGES than by state-of-the-art land surface models, except for the 20 year trend in GPP, in which SEDGES does surprisingly well.

It is not true that we only showed this comparison for GPP. We did some comparison

with the state-of-the-art LSM models for vegetative and soil carbon in sections 5.2 and 5.3, respectively. While it is true that we could make more comparisons between SEDGES and those state-of-the-art models with some of the other variables (ET and LAI), reviewer #1 questions the need for such comparisons, and reviewer #2 also states the following:

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In addition, a better comparison was to test their results with
models of the same level of complexity like second generation
LSM, SimBA, VECODE, ENTS. A comparison with far more complex
LSM like ORCHIDEE, JULES, and CLM4CN are not relevant in this
context because these LSMs are not developed just to provide
information to GCM but also to understand global dynamics
of vegetation across centuries that SEDGES are not able to
simulates.
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As a compromise, perhaps it would suffice to include an extra two figures showing the spatial annual averages of ET and runoff in SEDGES and how these compare to their reference datasets. We feel that more comparison with LAI is not needed, but would agree to it if the reviewer insists that we do so.

With regards to comparisons with other models of similar complexity, the third reviewer has already requested that comparisons with SimBA be made, and we did in our response. We can show such results for (the 2007 version of) SimBA in an appendix, perhaps for surface albedo, GPP, and ET. It is not worthwhile to put these results in the main text. SimBA has very severe problems when forced offline, i.e. when not coupled to Planet Simulator. See our response to reviewer #3 for ET and GPP figures. In the revised manuscript, we will add some qualitative comparison with ENTS (Williamson et al., 2006) for soil carbon, vegetative carbon, annual mean evaporation. Some comparison will be made with (offline-forced) VECODE (global mean NPP in Cramer et al. (2001) and tree fraction in Brovkin et al. (1997)), which are apparently the only published offline evaluations. We are hesitant to compare other (spatially-varying) eco-

logical variables (maximum LAI, NPP, biomass, and soil carbon), as shown in Brovkin et al. (2002), when VECODE is coupled to CLIMBER-2, because the simulation of those variables depends not only on VECODE, but also on the simulation of climate variables by CLIMBER-2, which is an Earth system model of intermediate complexity, and may thus deviate too far from the actual climate for a fair comparison with reanalysis-forced SEDGES.

To clarify, SEDGES is a dynamic vegetation model, so it certainly can be used to understand some important dynamical aspects of vegetation across centuries, in contrast with what the reviewer states. We will make this clear in the revised manuscript.

To convince others that SEDGES is a good LSM for coupled simulations, the authors must test SEDGES with a coupled simulation and check if the simplifications/modifications they made, have an impact on the GCM outputs. Otherwise, a least, authors must write a couple of sentence to explain why this test was not done and when they plan to realize this essential step. When I read the conclusion part, I have the feeling that the authors are convinced that SEDGES are already validated for coupled simulations and no more tests are needed : " In conclusion, we feel that SEDGES provides a new viable and computationally efficient alternative to currently implemented terrestrial vegetation/ecological models, [...] "

The reviewer raised a major concern that our first paper on the newly developed SEDGES model should also include an evaluation within a coupled climate model (or Earth System Model). We share the reviewer's concern that a stand-alone evaluation against present-day observations or in comparison with other models of similar complexity is not sufficient to make firm statements on how SEDGES performs in a coupled mode. Pablo Paiewonsky has been working with PlaSim-SEDGES and carefully analyzed the behavior of SEDGES inside that model for present-day climate (pre-industrial

climate conditions). However, we feel that an extension of the evaluation of SEDGES within ONE coupled climate model (namely PlaSim) already would add substantial amount of additional results (and discussions). Therefore, it would be better placed in a separate paper, along with an application of the coupled model for a specific research objective.

We are working on a second paper that will provide a detailed analysis of the PlaSim-SEDGES model. The results are part of Pablo Paiewonsky's PhD dissertation, which is going to become publicly available in August 2017.

As the reviewer knows very well, too, in a coupled system the cause-effect relations more often than not are difficult to assess. It could be therefore misleading if a poor evaluation of some variables that are numerically part of the SEDGES code is presented here, without going into an equally detailed evaluation of the physical part of the coupled system. For brevity we just highlight here, that the coupled model simulation, indeed shows some critically important model biases, for example, the equivalent of a grassland/shrub vegetation zone within the boreal forest zone of Siberia. Without careful study of the climatic conditions, and potential vegetation-climate feedbacks, there is no substantial value to adding the coupled-model evaluation into this paper, which has the ultimate purpose of introducing the model to the community at large.

[Moreover, we have identified a number of potential sources of coupling-issues that are intrinsic to PlaSim. For example, not all coupled models will have the same biases arising from low resolution (unresolved mountain topography), deficits in regional vegetation-climate feedbacks due to the use of a slab ocean model, and other physical parameterizations related to radiative fluxes and cloud processes, all of which can impact the results of the SEDGES component in PlaSim.]

In conclusion with regards to this concern, we hope that this paper with its stand-alone model description/evaluation of SEDGES can raise interest in the SEDGES model, and motivate modelers to couple our model with other GCMs or EMICs in near future. We

will revise our discussion and conclusions section accordingly and add a sentence that makes clear that the performance of SEDGES in a coupled system will need careful evaluation.

Moving on to the reviewer's comments on LAI, since the reviewer shows some confusion with respect to the variables and processes, it is possible that other readers will, too, and so we will revise the description to clarify the connections between (and roles of) LAIm, LAI, and fveg. fveg is leaf cover fraction, not forest cover fraction. LAIm is not directly translated into fveg, but rather fvegm (leaf cover fraction for moist soils). The reviewer questions the need to compute LAI at all. The final LAI does not need to be computed, as is said in the text in section 2.2.8. LAIm *does* need to be computed because it is directly used to compute fvegm, and thus, in conjunction with soil moisture limitation, fveg. We thank the reviewer for his or her point that it is dangerous to validate/evaluate SEDGES against observed LAI, when the observed LAI (from MODIS) was also used for calibration. In the revised manuscript, we will include some discussion on which datasets were used both for calibration and evaluation.

The reviewer further requests that we explain, for each lack of match between simulated output and reference dataset, why the mismatch occurs and what problems the lack of model precision could create in coupled simulations. We feel that we very often explain the reasons (or possible reasons) behind the mismatches between SEDGES and the reference datasets (used for evaluation) in our paper and that this is done adequately. To address the reviewer's other concerns, in the revised manuscript we will gladly try to anticipate conditions in which the model parameterizations would be expected to yield very inaccurate and/or untrustworthy results (particularly for non-modern conditions) and delve a bit into the ramifications of such model imprecision for coupled simulations, when we feel that they could be important. Such limitations are often not foreseeable from model performance under modern conditions (which is what is examined in the paper), but could be relevant for paleoclimate studies or if feedbacks in the coupled system amplify model imprecision in the offline mode.

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