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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 Anonymous Referee #3

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To begin with, we thank the reviewer for raising numerous important issues in his or her critique of our paper. In particular, we hope that in our response, our discussion on the constant ci/ca in our model is useful to others in the land surface modeling community, too.

`My major concern is that the improvement of model performances between SimBA and SEDGES is not clearly demonstrated. Since SEDGES builds on SimBA, to emphasize the value of this work, the advance of SEDGES needs to be well manifested.`

We agree with the reviewer that the value of the development of SEDGES would be highlighted by demonstrating its improved performance as compared to SimBA. It is important to remember that there are basically two main versions of SimBA. The older version (Kleidon et al., 2006) has been used in several studies (which are cited in the main paper) and is included in version 15 of the Planet Simulator (Lunkeit et al., 2007). The newer version of SimBA was developed by Pablo Paiewonsky from this older version and is included in version 16 of Planet Simulator (Lunkeit et al., 2011). Pablo Paiewonsky prefers to not evaluate the newer version of SimBA that he developed, because that version had no formal evaluation and, to his knowledge, has not been used in any published study. His personal opinion is that SEDGES is a much better model. However, if the reviewer insists, we would be willing to compare SEDGES with that newer version of SimBA.

For example, the authors show particularly well-simulated GPP, but it is not clear how much SEDGES improves the representations and simulations of GPP in SimBA. In other words, whether this well-simulated GPP is due to the incorporated processes in SEDGES, or due to the original framework set up in SimBA model? The authors need to prove that SEDGES indeed improves the GPP simulation compared to that from SimBA. Maybe adding the SimBA simulations in those relevant figures is the easiest way to illustrate. It will be even better if the authors could provide a short summary of how GPP is modeled in SimBA.

The older version of SimBA has a serious problem of multiple steady states, when used in a coupled model (Dekker et al., 2010). When we recently forced the old SimBA offline using the ERA-Interim reanalysis forcing as described in our paper, we also found multiple equilibria (at the end of the 280ppm CO_2 spin-up period). One simulation started from bare soil, and the other one was initialized with a "tropical forest" level of biomass. The forest-initialized simulation is only slightly more realistic than the bare ground-initialized simulation.

Results (GPP and ET) from only the forest-initialized simulation are shown below in the two figures for the years indicated (i.e. near the end of the increasing CO_2 phase). The simulated GPP is very unrealistic (fig. 1 below); it is much further away from the observational datasets than is the GPP from SEDGES (figure 2 in the main text). The simulated ET (fig. 2 below) is generally too large due to the Manabe bucket "beta" formulation, which gives the potential rate of ET for soil wetness fractions $\geq$ 0.25. This figure can be compared with the SEDGES simulation of ET, given in figure 12 of the

main paper, which is much more realistic. The GPP figure, especially, shows that this older version of SimBA has very severe deficiencies when forced offline with the ERA-Interim reanalysis data.

Similarly, the authors state that SEDGES improves most of the parameterization of SimBA, but again it is not clear whether or not those modifications of parameterization indeed improve SimBA simulation. The lesson we learnt from current LSMs tells us that increasing the complexity not necessarily guarantees a better model performance. Therefore, I am also concerned about the trade-off between realism and simplicity. To balance this trade-off with the purpose of improving the reliability and robustness of models, the added processes or modified parameterizations need to be proved as necessary for improving the reliability of the model.

First, we disagree that the modifications need to be formally shown to be necessary for improving model reliability. Informal evaluations occurred as the model was developed, and changes were made along the way as were deemed necessary. Second, for the majority of cases in which the SimBA parameterizations were changed, the resulting increase in model complexity was negligible. As is partially stated in the introduction, there are four main increases in complexity in SEDGES relative to SimBA: separation of ET into soil and vegetative components, inclusion of aerodynamic conductance in the formulation for carbon uptake by vegetation, full coupling of photosynthesis and transpiration through interactive canopy conductance, and soil organic carbon-dependent soil albedo. Of these three, we justify the need for only the fourth (in section 2.3.1). In the revised manuscript, we will list all four increases in complexity and explain what advantages are gained by incorporating them into SEDGES.

Below, we include a comparison between the old version of SimBA and SEDGES with respect to evapotranspiration and GPP.

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Moreover, the simplifications also need to prove as reasonable.
For example, the ratio of ci/ca in Equation (6) is considered
as a constant, but has been shown that the optimal stomatal
behavior allows ci/ca decreases with VPD, increase with
temperature (e.g.  Prentice et al.  2014, Medlyn et al.
2011, Lin et al.  2015).  The variation in ci/ca seems
quite important in terms of capturing the spatial pattern
of GPP (Wang et al.  2014).  This simplification needs a
justification.
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We agree with the reviewer that it would be preferable to have variable ci/ca in our model. In a future version of SEDGES, we hope to include this feature. Its incorporation into SEDGES is actually quite difficult because the simple relationships between ci/ca and VPD that are derived by Medlyn et al. (2011) and Prentice et al. (2014) are incompatible with the SEDGES framework. Medlyn et al. (2011) and Prentice et al. (2014) assume a Fick's Law of Diffusion transmission between the leaf and the outside air. In order to have consistency in moisture and CO_2 fluxes between the land surface and atmosphere, the land surface scheme also needs to use a diffusive exchange between outside air and leaves, or at the least, reasonably approximate such diffusive exchange. Diffusive exchange of moisture and CO_2 is not used in the SEDGES framework. Instead, transfer occurs from surface to atmosphere through a bulk aerodynamic formulation. This formulation only approximates the diffusive scheme when canopy resistance (r_c) greatly exceeds aerodynamic resistance (r_a). As is said in the main text, in early versions of SEDGES (coupled to Planet Simulator), it was found that this condition ($r_c >> r_a$) was frequently violated.

It should be mentioned that Pablo Paiewonsky has been extremely interested in incorporating variable ci/ca into SEDGES and did in fact include its dependency on VPD in an early, unpublished version of SEDGES, but later realized that it was not theoretically justifiable and thus removed it.

It should also be noted that even though ci/ca decreases with VPD (Morison and Gifford, 1983), it is nevertheless conserved in the real world under many conditions, including a wide range of different light levels, CO_2 levels, and nutrient levels (Wong et al., 1979).

While we view this constant ci/ca as a limitation of the model, we think that the reviewer overstates its importance with regards to capturing the spatial pattern of GPP. Our GPP results with SEDGES suggest that a fixed ci/ca is adequate for this purpose. Also, the paper that the reviewer cites to justify the importance of variable ci/ca on GPP, Wang et al. (2014), shows that the effect of including the RuBP-regeneration limited rate (which has the ci dependency) has a much weaker impact on the spatial pattern of annual GPP than do the two key variables: incoming light levels and foliage cover, whose variation are indeed incorporated into SEDGES.

Moreover, LUE (light-use efficiency) models using remotely sensed data have been successfully used for many years, but typically lack explicit ci dependence (e.g., Yuan et al., 2007). (Granted, though, ci dependence may be implicit in productivity's dependence on VPD in some of those models). More specifically, with regards to the VPD dependency of ci/ca and its subsequent effects on GPP, as we describe below, dependency of GPP on VPD in SEDGES does occur by way of the water-limited rate, GPPW, i.e. by hydraulic transport limitation. In this sense, productivity limitation by VPD still occurs in SEDGES, albeit through a different mechanism than by ci/ca reduction. With regards to the temperature dependency of ci/ca and its subsequent effects on GPP, colder temperatures do result in lower ci (Prentice et al., 2014; Lin et al., 2015), which, in isolation, reduces GPP in the Farquhar model of photosynthesis (Farquhar et al., 1980). However, this effect is substantially offset by the decrease in the CO_2 compensation point with lower temperatures (Prentice et al., 2017). On the other hand, 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, and also the ramifications for simulating transpiration.

Having a constant ci/ca is likely to play a more important role in transpiration (by way of affecting the water use efficiency) than (at least directly) in GPP. This is because, when $r_c >> r_a$ (i.e. using the diffusive approximation for heuristic purposes), transpiration is (unless it is occurring at the maximum rate in our model) proportional to VPD*GPPL/(1 - ci/ca), where GPPL is the light-limited rate of GPP. As such, changes in ci/ca (whose values are typically closer to 1 than 0 for C3 plants) have greater relative impact on transpiration than they do in the equations for either RuBP regeneration-limited or Rubisco-limited photosynthesis (at least for non-extreme conditions) in the Farquhar model (Farquhar et al., 1980).

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I think it is ci (the intercellular CO2 concentration) that
really matters in CO2 fertilization.  If you consider ci
for water-limited GPP in Equation (6), why not here for
light-limited GPP?"
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As stated in the paper, the parameterization for CO_2 fertilization (Equation 3) comes from Franks et al. (2013). Pablo Paiewonsky has had a similar concern as the reviewer and agrees that a dependency on ci would make more sense, from a theoretical standpoint. The above fertilization parameterization is for the RuBP regeneration-limited rate of photosynthesis, but it makes the approximation that ci $\approx$ ca. In Pablo Paiewonsky's current interpretation, the Franks et al. (2013) parameterization is with respect to ca and not ci because it seems that ca and not always ci was measured in the CO_2 sensitivity experiments that they used to validate their parameterization. So, strictly speaking, the empirical support of that fertilization parameterization is with respect to ca, rather than ci. Regardless, the impact of using ca instead of ci in that equation is not large. For instance, if one uses ci in Equation 3 and ci/ca = 0.8, then it can be shown that there is only an $\approx$ 6% increase in the light-limited rate of GPP at CO_2 of 2000ppm as compared to using ca in Equation 3. In fact, a potentially greater source of inaccuracy in the fertilization parameterization than the above is the assumption of a constant CO_2 compensation point.


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Typically, modeled LUE (not only fAPAR) is represented with
a dependency on vapor pressure deficit, why the equation here
does not include such an effect?  Is it implicitly considered
via the coupling with water-limited GPP?
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Yes, it is correct that the effect of VPD is implicit within the water-limited GPP. Canopy conductance is limited by a maximum rate of transpiration, which, in turn, depends on the specific humidity deficit (Δq), which, in turn, is approximately proportional to VPD. Note that the effect of VPD has nothing to do with optimality, though.

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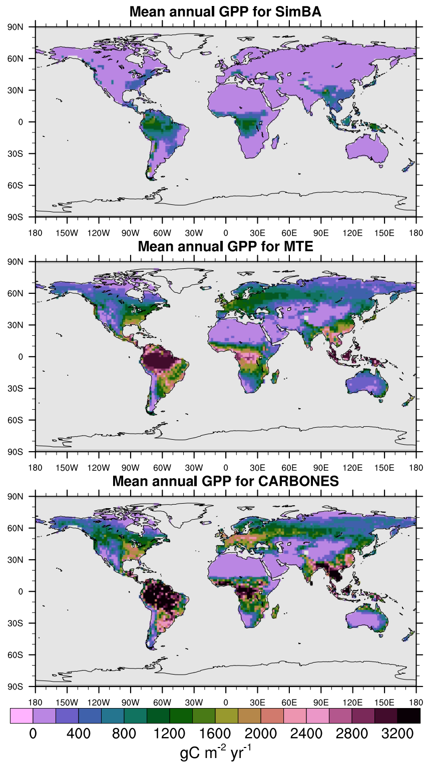

Fig. 1. Multi-year annual mean of gross primary productivity (GPP) for "old" SimBA (version 15 of Planet Simulator) and two reference datasets, MTE and CARBONES, for 1990-2009 over non-glaciated land.

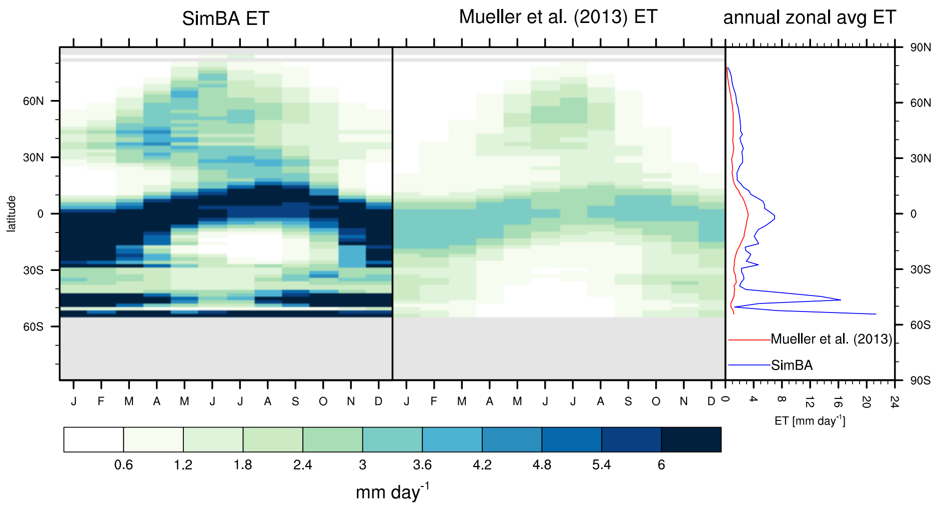

Fig. 2. Zonal monthly mean and annual mean climatologies of evapotranspiration for "old" SimBA (version 15 of Planet Simulator) and Mueller et al. (2013) reference dataset for 1989-2005 (non-glaciated land).