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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 #1

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The reviewer brings to our attention the lack of clarity in the manuscript with respect to exactly what the SEDGES model is and what it does. We agree that this is a very important issue and will most certainly address it in the revised manuscript. Briefly, SEDGES is not a land surface model. It simulates *some* aspects of the land surface, e.g. surface albedo. SEDGES is a dynamic vegetation model. It needs to be used in conjunction with (i.e. is "'auxiliary' to") a full-fledge land surface model.

We respond now in more detail to specific comments made by the reviewer:

`Sentence starting 'In such a framework' gives two reasons for needing a more sophisticated model.`

We will clarify our logic better in the revised manuscript. The idea here is that a model is only as good as its weakest link(s). In general, the benefit of having one component of an Earth System Model be sophisticated will be small when there are bigger inaccuracies coming from other components of the model.

`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?`

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As far as we know, the only sort of validation of SimBA is in Kleidon (2006) in which the simulation by SimBA coupled to Planet Simulator of annual means of GPP, surface temperature, and precipitation are shown. However, no comparison is made with observational-based datasets.

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P2, lines 10-15.  This really is the point at which more
information should be provided on what SEDGES actually does,
at an over-arching level, before leading in to detailed
Model Description.  What are the core additional quantities
that SEDGES generates, and that are in addition to existing
land surface models?  It is also still vague about ?model
evaluation?.  Both SEDGES and another trusted land surface
model are forced simultaneously with reanalysis data, and
certain diagnostics compared?
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In the revised paper, we will rework lines 10-15 on the top of page 2 to address the reviewer's concerns.

P2,3 Overview: Again, we will clarify better what SEDGES is and does in the revised manuscript. SEDGES *is* a Dynamic Global Vegetation Model (DGVM).

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Page 3.  Assumption that NPP/GPP=0.5.  This does feel like a
very large assumption, and particularly as thermal responses in
respiration might behave differently to gross photosynthesis.
The authors themselves appear cautious, with the caveat that
this might be accurate on very long time scales.  However, this
does mean that in comparison with other land surface models,
then only long-term averages of NPP and GPP should be compared.
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The reviewer is correct in saying that only longer-term (i.e. weeks or longer as is mentioned in the text) averages of NPP and GPP should be compared. We will add more discussion on this limitation. This point is relevant for those who are interested in *short-*

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term changes in carbon fluxes from the land surface. SEDGES might be unsuitable for such purposes if high precision is required.

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Page 4,5 The Tables are excellent, and highly appropriate for
a model development journal.  It is also appreciated that all
units are presented, and where justification of parameters is
linked to existing literature.
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We appreciate the reviewer's compliment on our tables!

In the revised manuscript, the references of "climatologies" will be changed for ecological variables (e.g. GPP) to something like "multi-year monthly means". The reviewer is right in pointing this out.

Regarding the line wrapping, we will consult with the journal editors to match what they want, etc.

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Page 7.  There is some evidence now that splitting SW radiation
in to direct and diffuse can have an influence on PAR. Is this
something the authors considered, or maybe for the next model
version?
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We believe that the reviewer here means to refer to the effect of direct/diffuse partitioning on light-use efficiency (LUE), rather than on PAR. Direct and diffuse radiation separation was considered at one point during model development, but it was deemed to be not worth the additional complexity at the time. In doing research with regards to this concern, we found two observation-based studies on the relationship between the diffuse fraction of SW radiation at top-of-canopy and LUE that control for the negative correlation between VPD (vapor pressure deficit) and diffuse SW fraction in their results (Alton et al., 2007; Williams et al., 2014). When going from conditions of predominantly direct solar radiation to predominantly diffuse solar radiation, Alton et al. (2007) finds an observed 6% to 33% increase in LUE in three forests, whereas Williams et al. (2014)

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finds an ≈17% increase in LUE in shrub tundra. The increase in LUE is apparently due to a more even distribution of PAR among the leaves, which reduces light saturation among the sunlit leaves. The distinction between sunlit and shade leaves is missing in our model's single big leaf approach to canopy radiation, which tacitly assumes a spatially-averaged light profile at each level of the canopy (de Pury and Farquhar, 1997; Monson and Baldocchi, 2014, p. 355). In a future version of SEDGES, we hope to incorporate the sunlit/shade leaf distinction. Not including it implies that, in the absence of water limitation, our model underpredicts GPP at low sun angles and under cloudy conditions (and overpredicts it for opposite conditions). We will discuss this in the revised manuscript.

If, on the other hand, the reviewer *is* referring to how the PAR fraction of incoming SW radiation varies with its diffuse fraction, then, from what we have seen in the literature, the effect is quite small: ≤0.03 variation in PAR fraction over all observed diffuse fractions (Jacovides et al., 2004; Li et al., 2010), which can be neglected for a simple model such as SEDGES.

Regarding the extensive use of footnotes: we will try to reduce these in the revised manuscript by putting them back into the main text and/or omitting them as appropriate.

`P11-14. These variables are the more novel parts of this land surface model, and it might be appropriate to re-iterate this point? That is, components more associated with carbon stores than the fluxes.`

Again, in the revised manuscript, we will make clear that SEDGES is a DGVM that simulates only some aspects of the land surface, etc.

`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`

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appear in forest cover fraction. It also includes seasonal phenology?

In the revised manuscript, we will add a short definition of forest cover fraction in section 2.2.9 to make clearer what it is. There is no seasonal phenology with regards to forest cover fraction.

P17, Section ?How to Couple SEDGES?. Unfortunately at this point in the paper, I am again confused as to exactly what SEDGES is, given that it needs the variables listed lines 24,27. The issue here is that some of these components do not uncouple? For instance, if SEDGES predicts LAI, then altered LAI will adjust transpiration, in turn affecting soil moisture content. So soil moisture content cannot be regarded as a pure input? I am happy to accept that I might not have fully understood the direction the paper is taking, but this could be made clearer. I can see that there are hints of this discussion around the middle of page 18.

Some variables such as soil moisture content (W_{soil}) and (total) ET are to be simulated outside of SEDGES, i.e. by the land surface model that SEDGES is coupled to. However, the simulation of such variables will use SEDGES output, which in this case would be the surface wetness factor, C_w (section 2.2.5) and also (possibly) the soil water holding capacity, W_{max}, for the case of W_{soil}. The important thing is that there be compatibility between how these variables are simulated by the land surface scheme and what SEDGES presupposes for a land surface scheme (e.g. the simplified mosaic approach and neglect of canopy interception). We will make these things clearer in the revised manuscript. ET is not input into SEDGES, but rather PET. A source of the confusion here has probably been what the reviewer indicated above: that in the current manuscript, it is not sufficiently clear what SEDGES is and does.

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P18. Related to the point above, in Equation (29), is ETsoil
derived from SEDGES? If so, then Wsoil becomes a diagnostic,
rather than an independent forcing.
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First of all, there is a typographical error on line 22 of page 19. Instead of "ET", it should be "PET". We will correct this in the revised manuscript. Secondly, what is done with ETsoil is admittedly confusing at the moment. ETsoil is only calculated explicitly when there is snow cover present, because, in this scenario, a distinction needs to be made between sublimation from snow and sublimation from the soil. ETsoil is therefore SEDGES model output (and thus soil water content, W_{soil}, is diminished by it in the land surface model that is coupled to SEDGES). The revised manuscript will explicitly include ETsoil as an output variable, and we will introduce it earlier, probably in section 2.2.5, around equation 13, rather than in section 4.

What might also be confusing the reviewer is that equation 29 in section 4 only describes the simple hydrological model that we used as part of our forcing of SEDGES offline with the reanalysis data, and the calculation of W_{soil} in that equation is not part of the actual SEDGES model. Perhaps we should make this more clear in the revised manuscript?

W_{soil} is not a diagnostic. As we say on line 14 of page 19, W_{soil} is a prognostic variable. It needs to be simulated outside of SEDGES and inputted into SEDGES. Regardless of how it is formulated to change, W_{soil} will depend on SEDGES output (as we describe above). However, W_{soil} also depends on the hydrological scheme of the land surface model that SEDGES is coupled to (and thus includes whatever processes are involved in runoff generation in that scheme).

Note that what is input and output for SEDGES versus what is input and output for the whole scheme of forcing SEDGES with reanalysis data differ. Again, maybe we should make this more clear in the revised manuscript...

On line 20 in section 4, there is a typographical error. W_{frac} should instead be W_{max}.

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We will correct this in the revised manuscript.

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

We understand the concern that a comparison of SEDGES to other models is not in all instances valuable or at least not necessary. However, we believe that it is important to give the reader some ideas how SEDGES compares with other model present-day simulations. In particular, one could argue that such information is important in any case: if SEDGES were an outlier model for some specific simulated variables or processes then the reader should be made aware of this situation. Or when SEDGES is similar to other model results, this could be useful in cases when the observational database is known to have large uncertainties.

We are open to suggestions regarding this issue. Reviewer #2 has indicated a preference for not comparing SEDGES with state-of-the-art land surface models, but reviewers #2 and #3 would like to see SEDGES compared with SimBA, if not also other low-complexity models. It seems very reasonable to us to at least reduce the number of comparisons with state-of-the-art models in the revised manuscript, i.e. restrict the comparisons to just the most essential variables and/or eliminate the comparisons to vegetative and soil carbon simulated by Earth System Models (Todd-Brown et al., 2013, Jiang et al., 2015), since these carbon values depend on the simulated climate in those models.

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