

We would like to thank referee #1 for the review and his/her helpful comments and suggestions. Following are the replies of the authors (in black) to the referee comments (in blue):

Referee General Comment:

The authors present a simple observation-driven model of the terrestrial biosphere. Its driving observations mainly come from the AVHRR satellite instrument. This development is motivated by carbon data assimilation applications, even though this prospect is not illustrated here. The new scheme is evaluated with NEE data from an atmospheric inversion, with simulations from a prognostic model and by expert judgment on some of the sensitivities of its inputs to its outputs. The model itself inherits from other models (e.g., MOD17) and, being very simple, its novelty appears to be very relative. However, the study has been well done; the paper is well written and as such is an interesting review of the topic. I recommend its publication provided the following points are addressed:

- p. 15128, l. 20: “According to SDPRM, the results show that temperature...” could be better phrased by “In SDPRM, temperature ...”

Author Reply: changed in the revised version.

- p. 15138, l. 10: “focuses”

Author Reply: changed in the revised version.

- p. 15139, l. 11: “the vegetation function”

Author Reply: changed in the revised version.

- p. 15140, l. 13: “In STD-inv”

Author Reply: changed in the revised version.

- p. 15141, l. 11: “does not”

Author Reply: changed in the revised version.

- p. 15142, l. 4: “more sophisticated” indeed, but also not observation-driven

Author Reply: this is a comment. We mentioned to the differences between the two models at different “related” places in the paper “i.e. Results and Discussion sections”.

- p. 15143, l. 14: IAV already defined earlier

Author Reply: definition is removed in the revised version.

- p. 15144, l. 2: climate variables are all coupled together: none is independent. It should be made clear that the sensitivities of carbon fluxes to climate that are shown are results of computations and are therefore uncertain. A few sentences should be corrected in this spirit (p. 15145, l. 3; p. 15146, l. 2, l. 16, l. 20)

- p. 15145, l. 7-8: talking about SDPRM, the link should be made first with its equations rather than with the true world

Author Reply to the previous two comments: Totally agree. We added the following paragraph on p.15143, l.15 before “Here, we performed ...”

“Fundamentally, a statistical model only reflects the statistical influence of different factors but it does not necessarily reflect a causal relationship. Nevertheless, SDPRM should still incorporate the most important biological factors. Therefore, it is worthwhile investigating whether SDPRM shows the climate sensitivity of Reco and GPP as presented by mechanistic models.”

Also, we add the following sentence on p.15144, l.20, after “... were not investigated.” *“As mentioned, SDPRM equations reflect the statistical influence of different variables but do not justify the underlying mechanisms of the influence. The quantitative conclusions of the climate sensitivity analysis are therefore uncertain and should be drawn with caution.”*

- p. 15146, l. 17: “are consistent”

Author Reply: changed in the revised version.

- p. 15147, l. 2: why is the resolution restricted to this coarse grid?

Author Reply: Basically we can run SDPRM at a higher resolution, but SDPRM compared and will be coupled to the inverse model (STD-inv) which has a spatial resolution of (4° latitude x 5° longitude). So the resolution of the model is set to the same resolution as STD-inv.

- p. 15147, l. 11: the authors should restrict their statement to the specific timespace scales that are addressed in the paper

Author Reply: changed in the revised version.

- p. 15147, paragraph starting in l. 24: the fact that SDPRM is driven by satellite data is a limitation as well that should be discussed there as well.

Author Reply: we added the following paragraph “The deficiency in the satellite-derived fAPAR data is a limitation as well and can produce large errors in the estimated fluxes for a certain region/time.”

- p. 15147, l. 27: “are specified”

Author Reply: changed in the revised version.

- p. 15147, l. 28: “the real world”

Author Reply: changed in the revised version.

- p. 15148, l. 18: to fit concentration measurements completely for good reasons, one would need a perfect transport model.

Author Reply: Totally agree, but the main intention of the manuscript is to technically present and assess SDPRM in the “forward” sense, leaving the assessment of how to best use it in an atmospheric inversion for a follow-on paper.