

We would like to thank referee #2 for the detailed review and his/her helpful comments and suggestions. Following are the replies (**in black**) of the authors to the referee comments (**in blue**):

General comments:

The authors present the SDPRM model, which will be used in an inversion system. The model is simple and based on existing concepts. The objective of this technical note is to present the model and show that it can simulate the carbon surface fluxes on a global scale with a small number of parameters.

The used methods and data are described clearly. The results are presented less clear, the relation with the objective is at times difficult to understand.

After reading the paper I am not convinced that the model is capable of simulating the carbon fluxes. This impression is probably due to the presentation of the results; see below for my specific comments. The uncertainties and sensitivities of the simulated fluxes and model parameter are missing, which will be important to know when the model is used to provide a-priori fields. These are derived using different drivers, but there is no concise conclusion in the paper about them. The analysis of the climate limitations seems not to be related to the objectives of the paper. The relevance of this analysis should be explained.

I suggest improving the results and discussion section by addressing the different possible sources of uncertainty. The results are already in the paper, but a logical structure will improve it, and will make it much easier to understand why the different analyses have been done. Also the English needs to be carefully checked; below I have mentioned a few suggestions, but not all.

Author Reply: The main intention of the manuscript is to technically present and assess SDPRM in the “forward” sense, leaving the assessment of the uncertainties and how to best use it in an atmospheric inversion for a follow-on paper. We agree with the referee that the current material cannot yet tell how realistic the flux estimates. However, from a modeling point of view, different modeling approaches, using different assumptions, will produce different estimates of sources and sinks of carbon (temporally and spatially). Validations and verifications of such models are difficult task on a large scale due to difficulties in scaling up small-scale measurements (also, see summary on Section 3.2.3). Therefore, it is hard to claim which one is better since each of model has its own limitations as it is discussed in the introduction and the discussion. In this technical paper, we tried to present the simplified equations governing the flux calculation in SDPRM and evaluate the flux estimates by comparing SDPRM with two independent modeling approaches (inversion of CO₂ and process-based models). These comparisons were meant to ensure that the model can capture consistent flux pattern that is simulated by the other two approaches. This is because using incorrect flux pattern can seriously distort the inversion calculations (Kaminski and Heimann, 2001).

In addition to that, we carried out the climate sensitivity analysis also to ensure that the simplified governing equations of SDPRM make sense in a process understanding, not just as a statistical model.

Specific comments

1: I suggest taking out the references from the abstract.

Author Reply: Done.

2: P 15128, L 12-14: “The estimated . . . models.” This is the main point. But the abstract does not really convince me of it. For instance, how is the analysis of the climatic controls related to this? Does it show anything about the quality of the simulations or uncertainties?

Author Reply: please see author reply to the general comments. But to make it clear, we simplified the governing equations of SDPRM that calculate individual processes (GPP and Reco) and impose spatio-temporal structure to the land fluxes based on the spatio-temporal information from the driver data. Therefore, the climate controls analysis is meant to ensure that the simplified governing equations of SDPRM still make sense in a process understanding.

We modified the text to make this clear.

3: P 15129, L 2-3: can you add some references?

Author Reply: Sentence is modified.

4: P 15129, L 6: isn't there more recent literature than this?

Author Reply: Sentence is modified.

5: P 15129, L 18: also here there is more recent literature available.

Author Reply: we added (*Baldocchi, D., M. Reichstein, D. Papale, L. Koteen, R. Vargas, D. Agarwal and R. Cook, (2012), The role of trace gas flux networks in the biogeosciences, Eos Trans. AGU, 93(23), 217.*)

6: the introduction jumps from one subject to the next, I would suggest helping the reader by adding a few lines explaining why you are mentioning them at the start of an paragraph.

Author Reply: text is modified as suggested.

7: P 15130, L 20: this cannot be the only reference here.

Author Reply: we added another two references.

8: P 15131, L 1-2: delete “some”. Aren't net fluxes and NPP the same thing?

Author Reply: modified in the revised version. They are not the same since NPP equals GPP minus autotrophic respiration (R_a), while Net Ecosystem Exchange (NEE) equals Reco (autotrophic and heterotrophic respiration) minus GPP.

9: P 15131, L 6: which model? Yours or BETHY?

Author Reply: They replaced the Simple Diagnostic Biosphere Model (SDBM) introduced by Knorr and Heimann (1995) by the the Biosphere Energy Transfer Hydrology Scheme (BETHY) (Knorr, 2000).

10: P 15131, L 9: what do you mean with a “more involved process”?

Author Reply: The highly nonlinear nature of the dependences of BETHY makes the minimization of the cost function a complex task. This requires making linear approximation of calculating the derivative of the cost function. We modified the text to (a complex task).

11: P 15131, L 17-22: “By later”, this is not English, subsequent lines are also difficult to understand, please rewrite.

Author Reply: modified in the revised version.

12: P 15131, L 23: “devoted to the description of”, why not replace this with “describes”?

Author Reply: modified in the revised version.

13: P 15132, L 1-15: in this paragraph the references are again mostly more than 10 years old.

Author Reply: we added more recent references.

14: P 15133, L 6: delete “much”

Author Reply: deleted in the revised version.

15: a general comment, both past and present tense are used throughout the paper, could you improve this?

Author Reply: that is improved in the revised version.

16: P 15134, L 10: can you give a definition of x , y and later t . I can guess them, but I want to know if this guess is correct.

Author Reply: definitions are added.

17: P 15136, L 7-13: move these lines to where the variables are first mentioned.

Author Reply: they are already mentioned in their first places, but here is the declaration for how they are calculated.

18: P 15136, L 21: “R” in the eq. should be “Reco”?

Author Reply: changed to Reco in the revised version.

19: P 15137, L 1-4: can you add a reference here?

Author Reply: reference is added.

20: P 15138, L 4: delete “got”.

Author Reply: deleted in the revised version.

21: P 15137, L 4-7: why is it better? Where is this biological variation in the model? I might have missed it, but could you explain this here a bit further?

Climate and vegetation are important contributors to the spatial variability of soil respiration on a global scale. Therefore, by including the leaf area index parameter (replaced by fAPAR) which describes the vegetation structure, the model should be able to capture more of the global biological variation of soil respiration compared to models that are purely climate driven. More details are discussed in Reichstein et al., 2003. The text is improved to make this clear.

22: P 15138, L 9: replace “analysis mainly focus” with The main focus of the analysis”.

Author Reply: replaced.

23: P 15138, L 20: Figure 11 here confuses me. Could you change the order of the tables and figures in the order they are mentioned in the text?

Author Reply: order is changed.

24: P 15138, L 21: I might have missed it, but what is the box-car filter?

Author Reply: It is simply a running mean of some number to smooth the variable along the indicated axis (time in our case). For example we calculated the running annual mean of fAPAR from all biweekly values in each year.

25: P 15139, L 13: these lines give you a reason of why it is important to know what the uncertainties in your estimated fluxes are. If you cannot show them, could you at least discuss them?

Author Reply: please see author reply for the general comments.

26: P 15140: section 3.2.1 is difficult to read. Could you rewrite it? What is the main result? The word “the” is used too many times as well.

Author Reply: we re-wrote the section as suggested.

27: P 15141, L 7-10: I do not understand what these lines are doing in this results section. These belong in the discussion.

Author Reply: removed.

28: Is the main conclusion of section 3.2.1 that CfAPAR is better then VfAPAR? Because that is what you use in the following section.

Author Reply: Yes, text is improved to make this clear.

29: The figures 4-7 are extremely difficult to read (very small), and understand. From these figures I can only guess how the different models compare. These results would be must easier to understand when presented in scatter plots and tables. Could you improve this?

Author Reply: please see author reply for the general comments. However, we didn't expect that SDPRM will perfectly fit the STD-inv or the process based model due to the limitations that mentioned in the conclusions. Also STD-inv and BIOME-BGC are not the truth and they have their own limitations as well, as discussed in the introduction. But overall, and given that SDPRM is a very simple model, from the

comparison and the climate analysis, we definitely can argue that the model is capable of producing flux patterns consistent with the other two models. In the follow up paper, by coupling SDPRM to the inverse model, we expect to have large improvement in the flux estimates.

As for the figures, the variability of the model output is crucial rather than fitting each point of the data to the other models which also requires making a matrix scatter plot which will increase the number of figure as well. Therefore, figures should rather stay as it is, but will be provided in high resolution for the final publishing process

30: P 15142, L 15-24: I do not understand the reasoning in these lines. Could you rewrite them? Why is this important?

Author Reply: this is to explain why SDPRM might underestimate the amplitude of the seasonal cycle of Reco in some region compared to BIOME-BGC.

31: P 15143, L 3: The results in figures 4-7 do not convince me that the model is capable enough.

Author Reply: the figure will be provided in larger size in the final publishing process.

32: P 15143: can you add an explanation of why section 3.3 is needed to address the objectives of the paper?

Author Reply: we added to the text : “*Fundamentally, a statistical model such as SDPRM 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*”.

33: P 15143-15144, from L 21: this section should be moved to the methods.

Author Reply: given that this section is another analysis and different from the method part, and also it uses the fluxes from SDPRM-CfAPAR, It should rather stay as it is.

34: P 15145, L 11-12: delete the lines “On the other hand. . . 2

Author Reply: deleted .

35: P 15145, L 21-23: there is no need to repeat the results from the table in the text. What is the main finding?

Author Reply: the finding is already mentioned in the text: VPD is a dominant control on GPP where water is severely limited. The text is revised as suggested.

36: I suggest to rewrite the results, shorter and clearer, with explanations of the analyses are needed to address the objectives.

Author Reply: The text is revised as suggested.

37: P 15147, L 13: “. . . to some extent, is mainly. . .” yes or no? Is NEE driven by climate, or not? What is the percentage explained by the climate variables?

Author Reply: actually as it is mentioned also in the manuscript that both climate and vegetation changes are important contributors to NEE. But IAV of fAPAR has some variations that may not be caused by vegetation variation (discussed in manuscript), and accordingly NEE will be affected by these variations. By removing the IAV of fAPAR that is used to drive the model, NEE estimates are more consistent with flux estimates from STD-inv. this indicates that in our model the interannual variability of NEE is likely to be insensitive to the vegetation changes but is mainly driven by climate. The climate sensitivity analysis shows the percentage explained by each climate variables globally and regionally. We revised the text to make it clear.

38: P 15147, L 14-16: why have tested these sensitivities?

Author Reply: see the author reply to 32: P 15143.

39: P 15147, L 27: this is the first time PFT versus global parameters are mentioned. What is shown in the figures? Or is this the difference between CfAPAR is better than VfAPAR? Is this the main result? I am confused now.

Author Reply: here, we are talking about the limitation of the model and what sources of uncertainties in the model which can be improved in the follow up study. Also we mentioned to the parameters in the description of the equations and how they are defined (globally or per PFT).

40: Can you end with the main findings? And restructure this last section?

Author Reply: we believe that the structure of the last section makes sense this way since it presents the following points in order;

- 1- What we did
- 2- Results
- 3- Limitation of the model with comments on how to be improved
- 4- Outlook

Table 2: where do the numbers in this table come from?

Author Reply: reference is added.

Table 4: I might have missed it, but is this table mentioned in the text?

Author Reply: it is mentioned in the appendix. Label will be changed in the revised version.

Fig 4-7: see above. These figures are difficult to interpret. Could you combine them in scatter plots and a table?

Author Reply: figures will be provided in high resolution for the final publishing process.

Fig 8: How does this figure compare with Fig. 2 in Beer et al. Science (2010). There is no overlap in the red zones in the 3 panels, they could be combined in one figure.

Author Reply: Actually the patterns have a good agreement with the results from Beer et al. (2010) even though we are using different climate data (they use CRU and ECMWF ERA-Interim). For the figures, we prefer not to combine them since there are some areas are limited by both radiation and temperature (i.e. Northern hemisphere).

Fig 9: the 2 panels of this figure could be combined, they are exactly the same, only the colour scales are different.

Author Reply: to keep consistent with Figure 8, we keep them split.

Fig 10: When this figure is not mentioned in the text (I could not find it), please remove it.

Author Reply: now, we mentioned to it in the text, it was requested by one of the referee before the manuscript appears in the discussion.

Fig 11: this should be figure 3.

Author Reply: figure order is changed