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Biogeosciences Discussions

Interactive comment on "An empirical model simulating long-term diurnal CO_2 flux for diverse vegetation types" *by* M. Saito et al.

M. Saito et al.

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Anonymous Referee #2

We appreciate the comments. The manuscript was substantially revised as advised. Our responses are as follows.

1. To general comments

   In this study, AmeriFlux Level 2 products, which contain non-gap-filled data, were used to construct an empirical model.

   We agree with the referee's claim that the strength of the modeled relationships should be provided. We now mention this in the Materials and methods section as well as in the Results and discussion section.

We also admit that the model performance should be tested using data not used in model development. We present new Figs. 8, 9, and 12 and Table 5 to show the tests of model performance using 10 AmeriFlux and four AsiaFlux data.

The manuscript was checked by professional proofreading service.

2. Abstract, L. 10-13: How do the author define reasonable? And satisfactory? Statistical performance measures need to be included here.

   We added new analyses in Section 3 and revised the manuscript to show the model performance.

3. Abstract, L. 14: According to what evaluative measures? Again, objective performance measures need to be cited here.

   We agree with the comment and added new analyses as advised.

4. P. 4003, L. 17-19: This is not a complete sentence

   We appreciate this comments and revised the manuscript as follows; " Alternative ways of evaluating biospheric processes are therefore required for the estimation of diurnal cycles in CO_2 variability. In some cases, empirical models can fit the data more closely than mechanistic models (Thornley, 2002).".

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5. P. 4005, Eq. (1): Why use beta instead of Pmax, and gamma instead of RE in this equation?

   We agree with this comment and revised the manuscript.

6. P. 4005, L. 20: I do not understand why the authors elected to fix theta, and why they selected a value of 0.9. They cite a very old study (i.e. Gutschick, 1991) despite the fact that the NRH model has been widely applied to eddy covariance data in recent decades (See Stoy et al. 2006). More justification needs to be provided here.

   We frequently failed in the parameter fitting of θ due to convergence problem. The reason why we chose $\theta = 0.9$ is that the value of 0.9 is widely used by previous studies (e.g., Kosugi et al., 2005; Saigusa et al., 2008), and performs well in these studies.

7. P. 4006, Eq. (4): Why was this particular function chosen to the effects of VPD?

   As shown in new Fig.1, Pmax decreases with increasing VPD. To represent this relationship, we applied an empirical function, new Eq. (4), and added new Fig. 2 as an example of Eqs (2), (3), and (4).

8. P. 4007 , Eq. (5): Using a very simple, and very old, empirical model for NPP to derive a parameter for a NEE model seems quite regressive. Why not explore relationships between Pmax and site-level variables such as AMT and AP directly. Or even better, explore relationships between Pmax and physical variables such as LAI, for example,

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which is often available for eddy covariance sites or alternatively can be derived from remote sensing

We admit that the MIAMI model is very simple. But, since the MIAMI model was constructed using the data aggregated over the world, it can be applicable to different regions in North America. This applicability is the reason we used the MIAMI model, because we hope that the model proposed in this study is employed for estimate of global CO_2 flux. We also agree with the referee's suggestion that examination on relationship between Pmax and physical variables is needed. But, further research is needed to include these variables in the model. We mention this in the results and discussion section as well as in the conclusion.

9. P. 4007, L. 22-23: The authors refer to this results several times throughout the discussion, even calling it remarkable on page 10. However, non-linear optimization techniques can often produce correlation among variables that is an artifact of the regression methodology, and not a true physical correlation. The impact of optimization technique on the parameter estimates needs to be further discussed.

   We admit that the correlation of Pmax and α may, in part, be the result of poor model fitting. We now mention this in Section 3.1.

10. P. 4011, L. 25: primal should be primary?

    We appreciate this comment and revised the manuscript.

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11. P. 4012, L. 10-19: The motivation for and implementation of the triangular filter is not entirely clear. Why was a triangular filter shown? Is this simply simulating the impact of sharply changing leaf area dynamics? If so, why not use a leaf area normalization procedure instead?

    We admit that this method is not clear and confuses readers. We therefore removed this sentence and estimated NEE variations at Savanna ecosystems without a filter to simplify the model. The result was described in Section 3.2.

12. Table 1 Since mean annual temperature and precipitation are used in the model, they should be included in this table. Additionally, many sites have just one or two years of data. Did the authors attempt to ensure that these shorter study periods did not coincide with a prolonged drought? And finally, if data presented for the intermediate hardwood, mature red pine, young jack pine, and other cites for which a reference is not given in this stable, then a brief description of the sites is warranted.

    We appreciate this comment and added information on mean annual temperature and precipitation in Table 1. Since available data are limited, we constructed the model by using all available data obtained from the AmeriFlux network. Unfortunately, information is not enough to give detailed descriptions of the non-reference experimental sites.

13. Table 2: What are the numbers in parentheses?

    We appreciate this comments and revised Table 2.

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14. Figure 3: Why are data from only two sites shown here?

    The relationship between bin-averaged temperature and ecosystem respiration scaled by annual NPP in savannas was shown in old Fig. 3. But we removed this old Fig. 3 and collected the relationships at 7 biomes into new Fig. 5c.

15. Figs. 5, 6: This information is nice to see as a time series, but if these figures used to illustrate model performance, then scatterplots of the data with 1:1 lines would be nice, or if not, r2 and P values at least need to be given.

    We added r2 in Figs.

References

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