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

Anonymous Referee #2

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The manuscript attempts to formulate generic parameterization schemes for the non-rectangular hyperbolic (NRH) model for NEE to facilitate a priori specification in coarse-scale modeling applications. The effort focuses on the seasonality of some of these parameters, though since leaf area index and other phonologic variables are not considered here, I would argue that this analysis is rather an exploration of the impact of climate on the model parameters. Their results suggest strong relationships between the initial slope of the NRH and maximum photosynthetic capacity, and they conclude that the simple empirical model described here performs well over a range of biomes.

The problems of how to specify the parameters of terrestrial ecosystem models for a wide array of biomes, and how to modify these parameters to reflect seasonal change are important questions, and while I believe that a more mechanistic treatments of

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relationships between the parameters and driving variables is preferable, the simple empirical scheme described here is a useful framework. However, the manuscript suffers from three methodological problems that need to be addressed before it is fit for publication.

First, the authors give very little details the eddy covariance data quality control procedures. They state on page 4004, lines 19-20 that only measured; fluxes were used to avoid biases associated with gap-filling. But the methods used to determine acceptable flux data (i.e. those data that were collected during near-neutral conditions in the absence of advective flows) vary significantly from site-to-site, and data availability can range anywhere from 40

Second, the authors fail to specify how they evaluated the strength of the modeled relationships, and further, did not provide any performance measurement statistics (i.e. r2, P). These must be included in any modeling exercise. Further, the authors base many of their conclusions on apparent correlation based on binned averages (i.e. Figures 2,3 and 7), despite the fact that the size on the bins has a significant impact on correlation. Either half-hourly or daily r2 values should be reported for all relationships so that the robustness of this model may be directly compared to other similar efforts.

Third, the authors do not present a true test of their model, as the same data used to train the model is used to evaluate performance. The authors either need to test model performance from additional sites not used in model development, or use half of the available data records to train the model and half to test them.

Finally, the language of the manuscript is a quite stilted and awkward at points, and any revised manucript should be carefully proofread for clarity.

Some additional comments follow:

Abstract, Line 10-13 ...The estimated physiological parameters had reasonable magnitudes and seasonal variation...but they were less satisfactory for disturbed grassland

and savanna than for forests...

How do the author define reasonable? And satisfactory? Statistical performance measures need to be included here.

Abstract, Line 14 ..The diurnal cycle of NEE was generally well predicted all year round by the model..

According to what evaluative measures? Again, objective performance measures need to be cited here.

Page 4003, Line 17-19 This is not a complete sentence

Page 4005, Equation (1) Why use beta instead of Pmax, and gamma instead of RE in this equation?

Page 4005, Line 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.

Page 4006, Equation (4) Why was this particular function chosen to the effects of VPD?

Page 4007 , Equation (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, which is often available for eddy covariance sites or alternatively can be derived from remote sensing

Page 4007, Line 22-23 ...We found that seasonal variation in the initial slope was correlated with that in Pmax...

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 pro-

duce 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.

Page 4011, Line 25 primal should be primary?

Page 4012, Lines 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?

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.

Table 2: What are the numbers in parentheses?

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

Figure 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.

References: Falge, E., Baldocchi, D., Olson, R., Anthoni, P., Aubinet, M., Bernhofer, C., Burba, G., Ceulemans, R., Clement, R., Dolman, H., Granier, A., Gross, P., Grunwald, T., Hollinger, D., Jensen, N.O., Katul, G., Keronen, P., Kowalski, A., Lai, C.T., Law, B.E., Meyers, T., Moncrieff, H., Moors, E., Munger, J.W., Pilegaard, K., Rannik, U., Rebmann, C., Suyker, A., Tenhunen, J., Tu, K., Verma, S., Vesala, T., Wilson, K., Wofsy, S. 2001. Gap filling strategies for defensible annual sums of net ecosystem exchange. Agric. Forest Meteorol. 107, 43-69.

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