

Interactive comment on “An empirical model simulating long-term diurnal CO₂ flux for diverse vegetation types” by M. Saito et al.

Anonymous Referee #1

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BGD Saito review

General comments Saito et al. model eddy covariance-measured flux data from a variety of North American ecosystems using an empirical approach with two steps: 1) three parameters of the four-parameter nonrectangular hyperbola were fit to eddy covariance data (the curvature parameter theta was set to 0.9); 2) subsequent parameter variability was modelled using standard formulations. Reasonable fit was found for a variety of ecosystems.

I find the approach interesting, but I have a number of specific comments listed below, and some general concerns. Foremost is that the model, as described, is not independently validated. Parameters were generated for different ecosystem types, and the

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testing against data that was used in part for generating the said parameter sets. How well do the parameters determined for individual ecosystems model flux at different ecosystems? This would be a powerful test of the approach and lend support to the generality that vegetative physiognomy is a logical means to separate the climate-flux relationship.

I'm curious to know why the results for grasslands do not hold given that work with the nonrectangular hyperbolic model has been concentrated on grassland ecosystems to date (Gilmanov et al. 2003). The authors mention the role of grazing and mowing. How does the model match up during relatively static ecosystem conditions?

As a side note, I agree that employing daytime data to estimate RE is a sound approach even if the preferred technique uses nighttime data (Reichstein et al. 2005).

Specific comments Introduction Page 4002 Line 26: Discuss why parameters should vary. Fung et al. didn't succeed in adjusting, rather they noticed that using the adjustment provided a better fit. A fully mechanistic explanation of this phenomenon is lacking.

4003, 27: Some colloquial wording, and the narrow footprint is subjective.

4004, 17: Write tundra ecosystems instead of tundras.

4004, 26: The application of the GSOD is new to me, please provide the basic background and some specifics of its application here.

4005 20: Fixing the theta parameter at 0.9 diminishes its usefulness. The rectangular hyperbola is the non-rectangular hyperbola for $\theta = 1$. Is this then the pseudo-rectangular hyperbola? Why was 0.9 chosen? Admittedly, convergence problems may result from attempting to fit this parameter at high frequencies, but testing the assumption of $\theta = 0.9$ would be useful and will impact results.

4005 24: Other papers familiar to the authors suggest that least absolute deviations rather than least squares is the appropriate cost function for fitting model parameters

to eddy covariance data. This will make fitting a four parameter model more difficult because it decreases the topography of the parameter space. Was LAD tested?

4006 L 5: Be consistent with abbreviations. Choose either Pmax or beta.

4006, 21: Why is equation 4 an increasing function of VPD?

4006, equation 2: Multiplicative reduction functions have little empirical or mechanistic basis despite their wide application in ecological modelling (noting the Leibig8217;s Law formulation in equation 5). Is there evidence that Pmax (i.e. beta) follows this functional response?

4007, 28: The correlation of Pmax and alpha may simply be the result of poor model fit when using the least-squares cost function [see the appendix in (Palmroth et al. 2005). Plot for example the degree of correlation between Pmax and alpha against r2 of the model fit.].

4008. equation 8: Why is this relationship expressed as a fraction of NPP? The original (Lloyd and Taylor 1994) reference does not do this.

4009, 9: Close parentheses.

4010, 5: l8217;m confused about this passage. NPP can be, but need not be, estimated using mean annual temperature or precipitation. These models usually fit poorly if timing is important, and it usually is.

4010, 12: If slope is the same at the plant-level, what is it, and is there a relationship with LAI? It would be interesting to test if there is evidence for this relationship being universal.

4011, 6: 8216;thence8217; is not in common usage.

4011, 25: (Wilson and Baldocchi 2000) argue that there is seasonal parameter variability due to leaf age and N. (Katul et al. 2003) argue for a fundamental relationship between hydrology and parameter variability. List these examples and others rather

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than the text, which is obvious.

4012: Is soil moisture or water deficit potential explanations for the savanna and grassland results?

4013: The description of model fit is largely qualitative.

4013, 25: The uncertainty in measurement here is almost certainly due to instrument self-heating (Burba et al. 2008) if an open-path gas analyzer is used at this site. (I do not have the resources to check this as I write.)

4104, 10: More work could be done on quantifying when, post-disturbance, grasslands can be effectively modelled.

4104, 16: Add species composition to the list of factors to test.

4104: 24: Again, the model comparison is qualitative. Focusing on examples of poor model fit here and elsewhere does not lend confidence to the approach.

Figure 1: Plot this figure with P_{max} on the abscissa as NPP is related to this variable, an ecosystem characteristic, rather than the other way around.

Fig 2: Write 8216;grasslands8217;, 8216;one standard deviation from mean8217; and 8216;Duke Forest8217;.

Fig 7: The fit is often poor at extremes, exhibiting sometimes strange curvature. Is the VPD model (equation 4) responsible?

Figure 8: This site is impacted substantially by the sensor heating effect described in (Burba et al. 2008). What were the air temperatures during the period, and is there evidence that the sensor was heated by solar radiation to be substantially above air temperature?

References: Burba, G. G., D. K. McDermitt, A. Grelle, D. J. Anderson, and L. Xu. 2008. Addressing the influence of instrument surface heat exchange on the measurements of

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