Biogeosciences Discuss., 6, C842–C852, 2009
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Biogeosciences Discussions

Interactive comment on "Improving land surface models with FLUXNET data" *by* M. Williams et al.

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Below we respond to the referee's comments, point by point.

Specific comments Abstract P2787. L7. 'fusing' is a colloquialism and should be put here in single quotes. Ideally it should be defined as well.

-We suggest that the changing our text to read "'fusing' (i.e. linking)".

P 2787. L17. Note that fusing multiple independent data provide a potential means to limit equifinality. This may not always be so if data sets are not orthogonal.

-We suggest changing the text to read "Fusing multiple independent and orthogonal data provides a means to limit equifinality."

Introduction P2788. L24. ": : :has led to a large increase: : :"

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-We agree that this change should be made.

P2789. L7. "data exploitation" is ambiguous. What does it mean to exploit data in this context? -We agree that this is ambiguous and suggest that “analysis” should replace “exploitation”.

P2789. L27. ": : :and modeler, and thus MDF." Is this sentence complete or missing part of the last phrase.

-We agree that the sentence is improved by removing the last phrase “and thus MDF”.

P2790. L1-9. Here parameter estimation and state estimation are lumped together. However, I think this is confusing to the reader. They should strictly be mentioned separately as there are different issues to consider if the problem is a state estimation problem or a parameter estimation problem.

-We agree, and suggest changes to this section including the addition of text: “If state estimation is the goal, then model states are adjusted to generate closer agreement with the observations. Further analysis can make use of these state adjustments to identify poorly represented processes and their timings. It is important to test that state adjustments are consistent with all independent observations. If so, model deficiencies can be clearly located.”

We also suggest changes to figure 1, so that parameter and state estimation are included.

P2790. L11. Delete "at hand."

-We agree with this suggestion.

P2793. L6. This is true, but it is also true that the processes may be able to be identified but cannot be parameterized.

-We agree and suggest the text is altered to read “Processes with time constants longer than the data record cannot be reliably identified and parameterised”

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P2793. L27-28. Careful consideration of data limitations is important, but it is also more than this. The previous par refers to fast and slow processes. It is important that the timescales of model processes and their observations must overlap otherwise no useful information is conveyed to the model from those data by the assimilation scheme.

-We agree with this point and suggest adding a new sentence in the first paragraph of section 6, stating: “However, the timescales of model processes and observations must overlap, otherwise useful information cannot be conveyed to the model from those data by the assimilation scheme.”

P2794. L1-2. It is important to note here that systematic errors will be identified by the multiple-constraints approach via model-data mismatch for one or more of the objective functions. This is a strength of the method. On the other hand, large random errors in observations, while increasing uncertainties of the estimated parameters, decrease the usefulness of the model-data fusion approach because the analysis errors are not much reduced from the original background (prior) errors.

-The manuscript already acknowledges the role of multiple constraints in identifying bias in section 4.4. However, the referee’s point is well made, and we suggest adding some new text to section 6.2, stating that: “Systematic errors will be identified by the multiple-constraints approach via model-data mismatch. On the other hand, large random errors in observations, while increasing uncertainties of the estimated parameters, decrease the usefulness of the model-data fusion approach because the analysis errors are not much reduced from the original background (prior) errors.”

P2794. L19. "LaPlace: : :" This is unclear and needs re-wording.

-We agree that the wording is not clear, and suggest altering it to read: “For instance, a least squares weighting is required if flux error is Gaussian, while mean absolute error is required if flux error is Laplacian (Richardson and Hollinger, 2005)”

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P2794. L23. The flux errors quoted here should be put into context. Perhaps cite their associated coefficients of variation? Same for P2795. L3.

-We think that CVs are not useful for NEE because it can be positive or negative at different time scales.

P2795. L9. ": : :bias annual estimates of net sequestration upwards." Be specific from what value to what other value. Or what is the magnitude (absolute and relative) of the bias?

-We suggest clarifying the text to read “upwards (i.e. more negative NEE”).

P2795. L20-25. Specifically what is the important distinction between measured and filled data here?

- We clarify that gap-filled data include modelling assumptions, and so complicate the MDF process. We suggest the text is updated to state that: “In general, MDF will want to use only measured, and not gap-filled, data, because gap-filling involves the use of a model and so can contaminate the MDF.”

P2798. L1-25. I find Fig 5 difficult to read. Axes and legends are almost illegible making it difficult to see what the author’s are referring to. I understand the importance of wavelet decomposition for examining model performance across multiple time scales, but I find the text about OWT and the wavelet half-plane diagram (Fig 5 e & f) difficult to relate to the rest of the text. This could be removed with little loss of message from the manuscript.

- We suggest the text describing multi-scale validation using wavelet decomposition is made more concise to correspond to a revised figure, as the reviewer recommends. Specifically, Figures 5c and 5d are removed as these are not central to the point under discussion. We suggest the text in 5.1 is adjusted to read:

“The evaluation of LSMs using traditional methods may not be optimal because of large, non-random errors and bias in instantaneous or aggregated fluxes. We suggest

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that model evaluation may be improved by quantifying patterns of C exchange at different frequencies (Baldocchi et al., 2001;Braswell et al., 2005), recognizing especially the tendency for models to fail at lower frequencies (i.e. annual or interannual time scales; Siqueira et al., 2006). We demonstrate such an approach for model improvement and evaluation by comparing eight years of measured latent heat exchange (LE) from the Tumbarumba flux monitoring site (Leuning et al., 2005) with corresponding predictions from the CABLE model (Kowalczyk et al., 2006), and a CABLE improvement that explicitly accounts for soil and litter layer evaporation (CABLESL) after Abramowitz et al. (2008). CABLESL represents an improvement in overall model fit compared to CABLE (Fig. 5a and b), but conventional scatterplots and associated goodness-of-fit statistics cannot determine the times or frequencies when the model has been improved, or how the model can be further improved. (We also note that the daily flux sums are not statistically independent). The wavelet coherence, similar in mathematical formulation to Pearson's correlation coefficient, can be used to identify the times and time scales at which two time series are statistically related (Grinsted et al., 2004), and we note that these time series can represent those of measurements and models (Figure 5c and d). Specifically, Grinsted et al. noted that wavelet coherence values above 0.7 (yellow-to-red colours in Figures 5c and d) represent a significant relationship at the 95% confidence limit. In the Tumbarumba example, variability in CABLESL is significantly related to measurements across time at seasonal time scales of approximately 4 months (102.1 days); note the band at this time scale in Fig. 5c and its near disappearance in Fig. 5d. At the same time, swaths of poor model/measurement coherence exist at shorter time scales, particularly during the years 2003 and 2005–2006. Unique events that are responsible for these oscillations in the measurement record can be identified, and the CABLE model further improved with such a complete 'picture', via the display of wavelet coherence in the wavelet half-plane (i.e. Figures 5c and d)."

P2800. L15-18. ": : :important information for subsequent steps of model structural development." Why? This statement needs explanation.

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-We suggest adding a sentence to this section stating that “These residuals help identify model capabilities, particularly those processes which are best constrained by data”.

P2800. L27. "filter techniques: : :" this is not defined and not linked with point #3 on P.2801. Thus it will be confusing to readers not familiar with Kalman Filter. Also, what other techniques are used and does model uncertainty need to be defined for these? More clarity is needed here. In addition, the text associated with point #3 is likely to be unclear for many readers. There are multiple concepts in here (batch versus sequential methods, parameter only versus parameter & state estimation, model and observation uncertainties) offered without explanation. The class needs to be stated in the dot point and the detail provided in the text.

-We suggest the text is changed to read: “For some algorithms the model uncertainty itself must be estimated (e.g. Kalman filter).” See also response to P2804 L2-9 below. We suggest that the text for #3 is changed to clarify it: “3. “Sequential” algorithms, such as the Kalman filter (Williams et al., 2005; Gove and Hollinger, 2006). These methods process data sequentially, in contrast to the first two classes that treat all observations at once (i.e. batch methods)”.

P2802. L1-23. This section is a mini tutorial on least squares/Kalman Filter estimation. I am not convinced that it adds a great deal to the section on the technical implementation of model-data fusion over what can be picked up in a text book. Perhaps this section should be placed in a text box as a primer on estimation?

-We understand this comment. It is difficult to set the right level of detail in this case, but we think we have achieved the right balance. We would be happy to have section 6.1 as a text box, if that is possible.

P2803. L12-17. "infinitely narrow priors" is the same as saying "holding these parameters constant". There are ways of assessing the maximum number of parameters that can be estimated given the information content of observations and model: e.g.

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the condition number of the Hessian indicates whether too many parameters are being fitted.

-We take this point and suggest the text is altered as suggested.

P2804. L2-9. The issue of model errors in the Kalman Filter has been remarked on previously and so this section partly repeats. The text should be made consistent throughout the MS. L7-9 will be unclear to those not familiar with KF and exploration of posterior distributions. What is the "careful inspection" that the user does?

-We suggest the repetitive sentence is removed (L2-4), and the remaining text on KF is shifted to the start of section 6.1. We make a link to section 6.6, which discusses the issue of inspection in more detail.

P2804. L23-29. Sentence repeated.

-We remove this sentence.

P2804. L28-P2805. L1. What does it mean to "relax" the model? How is this done? In Figure 7 what are the codes down the left hand side (Presumably flux tower site codes)?

-As stated in the text, "relaxing" is analogous to "estimating the initial conditions" - full details are provided in the referenced paper. However, for clearness, we propose to rephrase P2804, L26-29 to: "In ecosystems far from equilibrium, model parameter uncertainties and biases can be avoided by relaxing the steady state assumption, i.e. optimizing the initial conditions of C pools (Carvalhais et al., 2008), which reduces model estimates uncertainties in upscaling exercises."

The flux tower names are now on the figure.

P2805. L7-10. What does "independently" (in quotes) mean here? What does it mean that "compromises" are required from data and model? I think you mean that tradeoffs among objectives of a multiple constraints problem is needed so that no single objective

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is exactly satisfied?

-The quotation marks should be removed. Our point is a relatively simple one, so we suggest our text is updated and simplified to state that: “Modelled soil pools should be consistent with measured pools”.

P2806. L6-8. Its not just more information that is required , but new data possessing information that is orthogonal to the information already contained within the existing data. This will lead to reduced posterior uncertainties (ie greater variance explained).

-We suggest the word “orthogonal” is added to this sentence.

Interactive comment on Biogeosciences Discuss., 6, 2785, 2009.

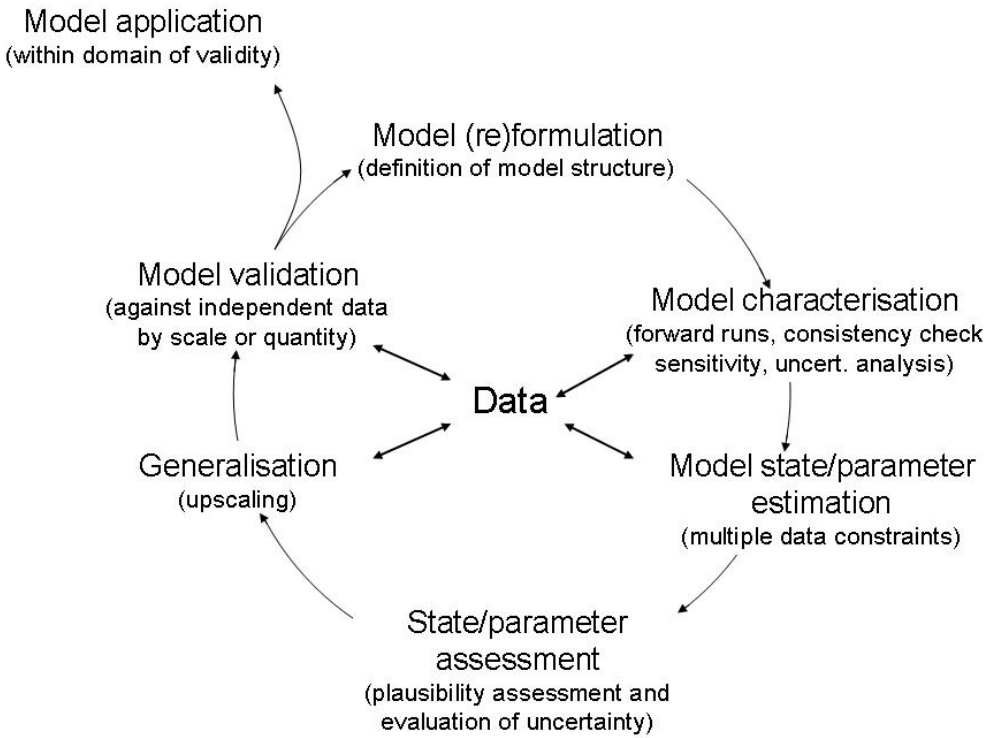

Fig. 1. Fig 1

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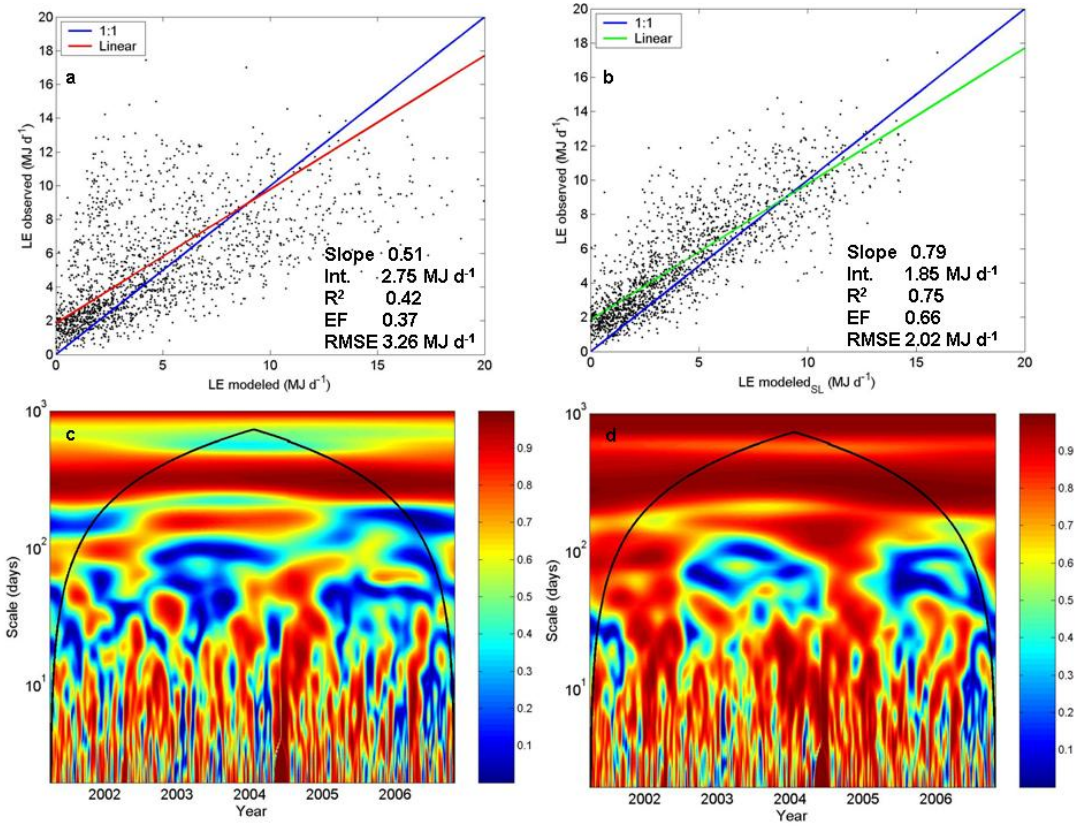

Fig. 2. Fig 5

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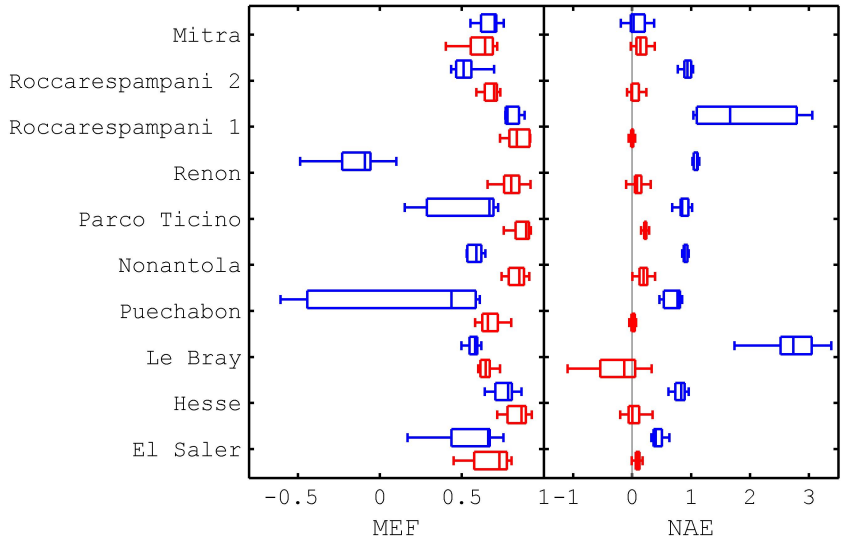

Fig. 3. Fig 7

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