**Biogeosciences
Discussions**

Interactive
Comment

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

Anonymous Referee #1

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This paper gives a clear overview of the principles and techniques involved with using eddy covariance flux data to improve land surface models. It is not an exposition of the range of model data fusion techniques, rather a summary paper surveying literature relevant to LSM Fluxnet integration. Thankfully, it does not gloss over deficiencies/criticisms of either eddy covariance data or land surface model ability, in fact provides sound advice on the common pitfalls in interpreting their combination in the model-data fusion process. I found it to be well written, articulate and appropriately comprehensive.

As such, I only have minor concerns and a few errata (in no particular order):

1. The referencing was a bit sloppy - I only had to go as far as the first author in the references section to find inconsistencies. This included both the relationship between what was cited in the text and what was printed in the references as well as the de-

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tails about particular papers (authors, years etc). This clearly needs rechecking and amending.

2. Page 2796-2797: the sentence over these pages should perhaps end with "how environmental factors influence key model parameters" rather than "how environmental factors influences of key model parameters".

3. Page 2802, line 10: "combination of different kind of data" should read "kinds of data".

4. Page 2804, last paragraph: a sentence is repeated.

5. Page 2806, in the section addressing equifinality: I would agree that additional data sources can help to constrain unidentifiable parameters, but what should also be mentioned is that including several cost functions that have different diagnostic signatures can often help as well, without the need for additional data.

6. I am not convinced in Figure 9 that "it is clear that turnover rates of foliage... were well constrained by NEE data" The final PDF essentially covers prior range and the real value is not in the histogram bin with the highest frequency - could this just be a lucky pick?

7. I would also caution in Section 6.6 that cost function residuals approximating a Gaussian distribution are not necessarily a sign of good approximation. Consider, for example, a constant model (=0) approximating a zero mean sinusoidal process (not really Gaussian, I understand, but evenly distributed about the mean, nontheless).

8. I heartily applaud the authors' insistence that model parameters must be fixed in time - the ecological community has taken some time to recognise this axiom.