Biogeosciences Discuss., 11, C3751–C3755, 2014
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Biogeosciences
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Interactive comment on "Using atmospheric observations to evaluate the spatiotemporal variability of CO_2 fluxes simulated by terrestrial biospheric models" *by* Y. Fang et al.

Anonymous Referee #2

Received and published: 25 July 2014

General Comments: (Overall Quality of Paper)

This is mostly well-written paper describing a novel approach to the recently oft-pondered problem of reconciling multiple constraints to understand the uncertainty in the magnitude and patterns of ecosystem CO2 fluxes. Because of the need to evaluate land surface models in order to make more reliable estimates of future change, this study is valuable for its contribution of an alternative, useful and complementary method of comparison. The approach is notable for its emphasis on the seasonal and spatial variability in fluxes, which may at the current state-of-the-science be a good balance for model evaluation between more commonly used comparisons of flux magnitudes that are either regionally aggregated or compared at specific sites.

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Interactive Discussion

Discussion Paper

Though the progress made in the application here is still modest, it can be considered an important step toward bridging the gap between a relatively sparse network of atmospheric concentration measurements and highly uncertain gridded flux simulations. The lack of insight provided here into the reasons behind differences in the performance of particular models is mostly for reasons beyond the scope of this study. This point is touched on somewhat in the discussion, but it could be better emphasized that the objective of the paper is (or should be, in this reviewer's opinion) to demonstrate a method that can make use of atmospheric observations to evaluate model performance from a new perspective – rather than a detailed study of particular modeling structures themselves. In other words, this reviewer's eyes typically glaze over at the use of "made up" data, but in this case the synthetic experiments were much more informative than the "real" ones.

The background and impetus for the study are well described and justified in the introduction. Other than the aforementioned unnecessary strain in an attempt to explain differences in individual model performance, the discussion is solid and the conclusions sound. The section that needs some attention is the methods; it could use more clarity in explanation for not only the individual steps but also how they all fit together. Otherwise, this is a nice paper overall and an important contribution. It could be improved relatively easily by the authors giving it another run through and taking opportunities to focus on the value of the approach and what it can (and can't) tell us about reconciling models with observations. At the same time, interpretation of the "real data" experiments is still useful and necessary, but it should be couched more as a demonstration or proof-of-concept with respect to the types of analysis that could be done with the methods, rather than a futile attempt to say something about particular model structures of specific models performing better or worse on certain indicators. Again, the reasons why are not necessarily the responsibility of the authors here, but instead they can take this opportunity to call out these issues and challenge the modeling community to address them going forward.

Specific Comments: (Individual Scientific Questions/Issues)

Section 1

Overall a good set up of the issues and good justification for the work presented here. The fifth paragraph (9218: 16 – 23) could use a little more explanation to help convey the importance (along with the caveats and uncertainties) of the observations and inverse modeling to this study. How is this done, and why? Some additional detail here would balance better with the more lengthy discussion of TBM issues above. One suggestion would be to have an explicit statement of objective(s) in there somewhere; perhaps at the top of the 6th paragraph (9218: 24), e.g. "what is needed is a method that. . ." This where you can make clear that the emphasis of this paper is on the value of this method.

The 7th paragraph seems a bit unconventional; it is not common to have this kind of "table of contents" paragraph. Not that there is anything wrong with non-conformity when needed, it's just that this isn't.

Section 2

Figure 1 should be referenced in the first sentence (9220: 18 – 20) to illustrate the locations of the towers in relation to the biomes. This could also use a quick note in the text about how well distributed (or not) these towers are across biomes. On the figure itself, consider different symbols for the towers and/or color schemes for the biomes; the tower locations are difficult to see.

There is a general problem in this section in terms of passing off potentially important details to previous publications. For example, 9220: 22 -23 talks about processing and sub-selection. Can this be expanded to two sentences with a little more explanation of each, at least within the context of this study? How many observations are in the sub-sample? Is that important or an issue to deal with in the model selection? There could also be a little more explanation on the inverse method, for non-experts. What is

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this doing and what is the product?

Fossil fuel emissions impacts are pre-subtracted (9220: 2 – 3), but not fire emissions (like CarbonTracker)? Is this an issue that might impact your analyses? Similarly, there is no discussion in section 2.3 about how the models define NEE and which components are included or not. For example, CASA-GFED includes the impacts and emissions from fire while SiB does not. Is this important?

Section 3

This section overall was very difficult for a non-expert (admittedly not as fluent in matrix algebra as one would like) to grasp what was happening even after several re-reads. Could this explanation improve with more clarity in text and/or a visual rather than a reliance on equations? Using words like "true fluxes" (9221: 17) can become confusing without more explicit definition. The most troublesome was the seeming mismatch in resolution of a model based on 'biome-month' to predict "true" fluxes at 1x1 and 3-hrly resolution. This is so key to the whole thing that it probably should be spelled out and/or illustrated more clearly and in more detail.

On the other hand, it was curious that the BIC was defined / described in such detail (rather than just saying what it means for model selection here) compared to the relative lack of explanation of inverse methods, transport models, the derivation and character of the predictor and dependent variables, etc.

Section 4

The synthetic and real data case studies are well defined, explained and justified, and Figure 2 is helpful to see the linkages. A sentence or two that gives the basic jist on how the mismatch errors and residuals were synthetically generated would be helpful here (9225: 2 – 25), rather than leaving it all to the Supplement.

Section 5

This section is well-written, clear and does well highlighting the key aspects of this

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study. The synthetic experiments show that this method works for what it is designed for, and it can provide useful and complementary information in combination with other established approaches. That they highlight the biomes where (and why) CO_2 fluxes are not well constrained lends further credence to the need for additional observations in sparsely sampled biomes.

Section 6

The more interesting results of the "real data" experiments are when general insight can be drawn from the performance of the models as an ensemble. While the conclusions drawn here seem well-founded (e.g. better in the growing season and in particular biomes that are probably better-studied), the potential reasons for the differences in performance of the individual models (e.g. "capturing seasonal variation", "internal structures") are vague and purely speculative. This paper would be just fine without this inter-comparison piece, in fact it's removal or strong, caveated de-emphasis would improve the paper with respect to clarity / proper focus on the method and less on anything specific about these four (of many) particular biospheric models.

Technical Corrections: 9217: 1, remove "the" in "the carbon cycle science" 9218: 29, remove the "n" in "North American" 9219: 26, change to "...gives a more realistic..." 9220: 2, should CarbonTracker be cited here?

Interactive comment on Biogeosciences Discuss., 11, 9215, 2014.

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