

## ***Interactive comment on “Can seasonal and interannual variation in landscape CO<sub>2</sub> fluxes be detected by atmospheric observations of CO<sub>2</sub> concentrations made at a tall tower?” by T. L. Smallman et al.***

**Anonymous Referee #2**

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General comments:

The manuscript tackles with two issues relevant for use of high tower greenhouse gas concentration data: the representativeness of a single tower to the surrounding landscape, on one hand, and the ability of the tower to detect temporal variability of the surface flux signal, on the other. The manuscript fits to the scope of the journal and its title reflects the contents. However, reading the paper I had a feeling that there were assumptions made that were neither thoroughly explained nor demonstrated in the paper. For instance, it seemed to me that most of the main conclusions were based on the

C6034

assumption that the modeled ecosystem signal at the surface is without any potential error sources even though the modeled region is in fact quite large and heterogeneous, not only from land cover, but certainly also from soil property and topography points of view. One would expect such a heterogeneous surface to introduce uncertainty to the modeled surface signal. If that is my misinterpretation rather than an assumption made, please, restructure and rephrase the text so that the main aims, assumptions and limitations are acknowledged from the beginning on. Many of the following specific comments may in fact, be similar misinterpretations and the recommendation given above will hold for tackling with them too. Furthermore, some of the specific comments reflect the fact that it was not always clear which modeled or measured variables were referred to in each occasion of terms 'simulated' and 'detected'.

Specific comments:

In the abstract as well as in the description of the model framework it has been stated that the models are coupled, furthermore it has been explained that the hydrological, carbon and energy cycles in SPA model are fully coupled but the actual way of coupling between the two models has not been explained. It seems to me that a one-way coupling among WRF and SPA takes places, where there is no feedback from SPA to the driving model, except for the inclusion of tracers in each time step (what was the time step, by the way?). If this is the case, its implications are worth of explaining and discussing more thoroughly.

Having background in flux observations and their source area problematics I find some parts of the motivation in the introduction slightly confusing. It is stated for instance that: “Despite a rapid decline in ecosystem contribution, the total footprint of a tall tower observation can cover a large area.” Isn't the rapid decline of contribution an implication of a large footprint rather than a contradiction? Related to this I wonder where the range of dominant signal of <100 km derives from. Above there is given a measure with a reference that area of 500km x 700km contribute up to 50%. Can you give similar approximate fraction of the signal originating from within <100 km as a

C6035

measure of its dominance?

Please, explain in more detailed manner how the concept of non-passive and passive tracers accounting for carbon sources and sinks, respectively, preserves the mass balance. Is the fraction of the absorbed non-passive tracers taken into account in the contribution of the passive tracers from the area of absorption? A mathematical formulation would help the reader. Under which conditions and how frequently an absorption of a non-passive tracer took place in the simulations?

Please, mention the number of years used for the analysis instead of “multi-annual” as the expression is a bit misleading. It has been stated that spin-up period from 2002 to 2005 allows for differentiation of ecosystem phenology but that was not presented in this paper and thus the statement is redundant.

Give the abbreviation TTA at the first occasion of tall tower Angus and use it from the first occasion on.

Please specify how the Griffin Forest meteorological data is suited for the carbon store spin-up – the location of the site in relation to the study region would among other specifications be a very helpful piece of information for a reader. How well does the data represent the regional variability within the domain? It is stated in the manuscript that the topography is complex, which may introduce local variability in the meteorological fields. There is on the other hand a re-analysis data used in the study – the representativeness of the site meteorological data could for instance be validated against this data.

Is it typical to call the tracers excluding land biosphere “forcings only”? In what sense those tracers are “forcings” as opposed to the “total”?

In the beginning of “Results” there are statistical values R2, RMSE and bias given as measures of performance of “total signal” in comparison to “forcings only” for different temporal averaging periods. Confusingly the set of given statistical values varies from

C6036

time scale to time scale. Please, always give the same set of statistics so that the reader can see herself in which time scales the performance improves because of the inclusion of the ecosystem signal.

In the end of 4.2 there are couple of sentences that need to be rephrased. Especially, I do not understand the sentence: “Forest and cropland show seasonality for which ecosystem is the dominant signal at TTA.”

It is not clear from the text (throughout the manuscript) and from figure captions of figures 5 and 6 what is referred to with “surface” or “total surface” in the context of simulated net uptake. I guess that is the inner domain of the model area but this should be clearly defined and named and consistently referred to throughout the text and figure captions.

What is meant by the statement that the observations “do not contain realistic information on ecosystems not adjacent to the tower”? I do not think that it contains somehow false information about those distant ecosystems either, uncertain surely but realistic as long as their contribution is above the detection limit. Please, rephrase to clarify the point.

The following sentence is even more confusing: “Interannual variation of the simulated seasonal cycles is poorly detected by TTA.” How come can one expect that the simulated seasonal cycles would be detected in the first place? Doesn't the poor 'detection' of simulated signal rather imply that some part of the model system fails to produce the correct seasonal cycle than that the tall tower fails to detect it? I probably misinterpreted the message here as well, please clarify.

There is some text in the figure captions (see especially the Figures 4, 5 and 6) that would rather belong to the “Results” than to the captions. Please, explain in the caption what is shown in the figure and the interpretation in the “Results” and “Discussion”.