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Interactive comment on "Can seasonal and interannual variation in landscape CO_2 fluxes be detected by atmospheric observations of CO_2 concentrations made at a tall tower?" *by* T. L. Smallman et al.

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The paper entitled "can seasonal and interannual variation in landscape CO2 fluxes be detected by atmospheric observations of CO2 concentrations made at a tall tower" by Smallman et al. presents a comparison of a multi-tracer mesoscale simulation over several years to observed concentrations from the Angus tower. The subject is highly relevant for current regional carbon budget estimates produced by inverse methods, and more specifically the study of the inter-annual variability which, to the extent of my knowledge, has not been simulated over such a long time period at such resolution

(6km). However, there are three major issues requiring significant changes in this study, and have to be addressed before considering the paper for publication. One of them is related to the simulation of "tracers". The second is related to the comparison of national scale flux signals to the atmospheric signals observed at the tower which is not related to any scientific questions. The last one is addressing the questions in the introduction, which are confusing and somehow incomplete.

Major issues: 1. The tracers, as defined in Table 1, tell a confusing story of the different fluxes and their corresponding atmospheric signals. More specifically, some tracers are representing the "uptake" from the surface. These tracers are listed in Table 1 as "non-passive" tracers, which means that these tracers can be removed from the atmosphere. In other terms, the tracer representing the uptake from a given ecosystem/place/time can be removed from the simulation. This doesn't make much sense. The description is somehow very confusing and what I try to describe here may not be true (in this case you need to correct the table 1 and explain the time lag in Figure 6). But the Figure 6 shows that this seems to be true. The explanation in Table 1 states that "a non-passive tracer can be removed from the atmosphere, as it represents a physical mass of CO2". I don't understand how a tracer representing the uptake (which is a process and not a physical mass of carbon) can have a mass and therefore be removed from the atmosphere.

Related to this problem, in Figure 6, the fact that the maximums in concentrations are delayed compared to the maximums in the surface fluxes for all the ecosystems except the crops is very surprising. There is no reason at the regional scale to observe a time lag between the surface flux uptake maximum and its corresponding maximum in atmospheric mixing ratios (unless the air has to travel a long time across hemispheres for example). I think it indicates that the "uptake" from other ecosystems is removed from the simulation by the crop surface uptake. Here, it seems that the crops remove the "uptake" tracers from other ecosystems, until harvest. Then, once the area around the tower is free of crop uptake (=after harvest), the "uptake" tracers from the forest, grassland, and other types can finally reach the tower location. Removing the "uptake" tracer is not physically realistic. The "uptake" tracers should all be added and conserved during the simulation. Or you release CO2 molecules, which are then affected by surface uptake and respiration, or you represent uptake and respiration by individual tracers which will be always conserved in the simulation, but will partly cancel each other at the end (one being positive and the other negative). But if your tracers represent different processes (uptake for example), you can't remove this process from the atmosphere. Otherwise it means that the uptake from crops can absorb the uptake signals from the forest, which is not what happens in the atmosphere. The crops will remove CO2 from the atmosphere, as well as the forest and other ecosystems.

Concerning the time lag in the maximums, here is another way to look at the problem: how can a maximum in forest uptake happening less than 100km away from the tower be observed with a time lag of 2 months at the tower? Even though there is no forest in a 5km radius around the tower, these signals will still be observed at the tower, attenuated by the diffusion of the atmosphere, and potentially super0imposed with larger uptake signals. But in any case, the extremes should happen at the same time. This is independent of the amplitude of the signals from the forest. A parcel of air in the domain is mixed vertically through the PBL in few minutes, and transported to the tower in few hours. Even if the TTA tower is mostly surrounded by crops, the seasonal maximum for other ecosystems should be identical in the mixing ratios and in the fluxes within a 50km radius. The travel time is too short to generate a difference of 2 months.

2. The comparison of the national scale land surface uptake to the observed uptake at TTA (comparison of fractions) is missing a scientific justification. This comparison should include the influence of the tower footprint at the surface. Assuming that TTA observes surface flux signals from the entire domain does not make sense (which is explained in the introduction but surprisingly not considered later on). The tower has a specific footprint (which can vary significantly as a function of time) and this

footprint is clearly not as large as the UK (or the simulation domain). This question has been addressed in several papers (some of them being cited in the introduction) and the analyses here do not provide much insights. The analysis (sections 4.2 and 4.3) should be modified to include some information about the footprint of the tower (i.e. backward plume simulations) or at least should approach the footprint by considering the climatology of the wind across seasons, or use some existing calculated footprints for similar towers (Mace Head?) and apply it as a mask on the surface fluxes. But the analysis as presented here does not bring any relevant information. An atmospheric mixing ratio corresponds to a specific footprint at the surface at a given time multiplied by the corresponding fluxes. In addition, the analysis is also affected by the "uptake" of "non-passive" tracers as explained in 1. Your conclusions may change once you solve the issue of the uptake of the "uptake". Forests and other ecosystems may show a larger contribution.

I would recommend to perform some additional simulations to evaluate the spatial extent of the tower footprint, which can be used to understand the contributions from the different ecosystems. The footprints could also be used to correct the problem described in 1., without re-running WRF. By combining the surface fluxes with the tower footprints, the contributions of each ecosystem to the observed atmospheric signals can be calculated. But if this suggestion represents too much work for this paper, the climatology of winds and the use of similar footprints from other tall towers would suffice, as long as some information about the tower footprint is introduced in the analysis.

3. The questions 2 and 3 of the introduction are not correctly stated. The TTA will detect some seasonality and some IAV. The question is to define the area covered by the tower. In addition, I don't think you need to run WRF over 3 years to answer this question. If the average surface footprint of TTA over one month is not covering your domain, you can argue that TTA cannot capture the seasonality and IAV of Scotland's terrestrial ecosystems. Re-phrase your questions to explain more clearly your objectives.

General comment about this study: You should consider additional questions, which could greatly improve your study, and not very demanding considering the large amount of results at hands. I recommend that you extract additional information from your simulations. More statistics would be very informative. Specific wind climatology could be used to evaluate the boundary inflow from CT (when the wind blows from the East), or at night when the measurements are above the stable PBL. Is there no flux tower available in the area that could support your analysis? 3 years of simulated atmospheric signals offer a lot of possible avenues. The paper would really benefit from more analysis of the WRF-SPA results, e.g. detection of flux anomalies, or specific events (droughts?)... The current analysis is limited mostly to monthly means and correlations. Comparison to CT results would be relevant for the evaluation of the atmospheric model resolution and its impact on the mixing ratios. These short analyses do not require any additional model runs, and would be more relevant than comparing the tower signals to an artificial domain which is not scientifically relevant.

Technical comments:

14302-4: "realistic" is unclear. Re-phrase. 14302-9: "realistic transport" The CO2 mixing ratios do not provide any information about the accuracy of the transport, or the source/sink distribution and magnitude. Both could be wrong and produce reasonable mixing ratios. 14302-12:13: Contradict the earlier statement that fluxes are appropriate. Harvest is a critical component of the regional carbon budget in the area. 14302-15:17: This simply implies that these ecosystems are out of the tower footprint. You could have found that with a simple footprint model. 14302-20: confusing. You mean "over-represented compared to the national fraction of cropland" 14303-4: "fraction" instead of "proportion" 14303-6: "... a net land-atmosphere surface exchange...". And do you mean that the terrestrial models are not able to constrain the regional carbon budget due to large uncertainties? 14303-8: "plant functional type" is not a driver. They represent a set of model parameters. 14303-11: "...further complexity to simulate/represent the ecosystem processes." 14303-16: what type of observations?

GHGs? 14303-23:26: re-phrase 14304-2: They can detect anomalies, but are these systems able to quantify accurately the fluxes? 14304-16: remove "to" 14305-6:24: this paragraph is not part of the introduction. This should be moved to the method section. 14306-questions ii and iii: these questions are incomplete. Which ecosystems are you referring to? Many papers have shown the seasonality observed at tall towers. What do you mean by interannual variability? Same question for "seasonality". 14307-3: Mesoscale models cannot be used below 1km resolution during daytime without breaking some of the assumptions made in the turbulence schemes. The parameterization of the turbulence assumes that the turbulence is smaller than the grid dimensions. During daytime, an eddy can easily grow beyond 1km of diameter. The model would simulate well-mixed conditions within an eddy, which is physically unrealistic. 14307-18: isn't the default land cover map from USGS? MODIS has been recently implemented. What version are you using? 14308-5: please add a reference for this dataset. 14309-1:2: Why should the inflow/outflow be zero? CO2 is not a conservative physical quantity within your domain. You could have more or less CO2 depending on the inflow/outflow. Your domain is an opened box connected with large scale conditions. 14309-16: "The WRF-SPA simulation is comprised of two grids in two-way nesting mode:..." 14309-26: the driver data are at 1 degree resolution whereas the coarse grid of WRF is at 18km. This is more than a factor of 5 between the two, which is the maximum for grid nesting. An intermediate grid would have been preferable (52km resolution for example). 14310-7: please add a reference or if not available, explain more precisely how the measurements were calibrated. 14311-5: "A larger positive bias..." 14311-7: This means that the seasonality in SPA is incorrect then. The abstract should highlight this problem which is not unique but important for future studies using the SPA model. 14311-14: could you confirm this result with some data? Flux towers could be used to evaluate the performances of the SPA model. This evaluation is subjective and does not provide much information. 14312-1:20: why do you compare to the national land cover fractions? Is this tower supposed to represent the entire country? This question has been already treated in several studies. Comparing the ecosystem contributions at the tower and over the country will certainly lead to different fluxes and atmospheric signals. State your question more clearly and explain why this comparison is scientifically relevant. Section 4.3: Is it still a comparison between TTA signals and surface fluxes at the national scale? 14312-24: "…in forest at the national scale…" 14313-1:3: Why should the two be the same? Are you comparing national scale to TTA? 14313-4:9: Unclear. Please re-phrase. 14313-13:16: How are these two quantities (temperature and PAR) used in SPA? Are the correlations not due to the formulation of photosynthesis and respiration? Explain more. 14314-1: You haven't evaluated your transport errors, the boundary inflow, and the anthropogenic emissions which could significantly affect your results. This conclusion uses "likely" which assumes that the uncertainty has been evaluated. 14314-1: You didn't assess the performance of WRF-SPA, but only the capability of the model to represent the observed variability. WRF and SPA could be both incorrect. 14315-1:3: You should evaluate the results using flux towers over similar ecosystems. "Broadly realistic" doesn't mean anything. Please provide numbers and quantify the errors. 14315-21: "its net surface uptake" 14315-24:26: The sentence is confusing. At night for example, the footprint of the tower can be much smaller because of the low wind speed. In this case, the strength of the vertical mixing is very low whereas the influence is very local. There is no direct relationship between vertical mixing and the extent of the footprint, but instead a combination of wind speed, stability conditions, and vertical exchanges (if the PBL grows for example, the air from the Troposphere is entrained in the PBL and dilutes the signals at the surface). Section 5.3: the conclusions are likely to change once the "uptake" tracer is conserved. 14317-14: "Caveats" = discussion? Conclusions: The questions should be re-formulated. Keywords are missing as "national scale fluxes". There is no "bias in ecosystem". I guess you mean that the distribution of ecosystems within the footprint of the tower is different than the distribution of ecosystems at larger scales (national?). This question is not scientifically justified. Instead, you can compute the footprint of the tower directly and define the spatial extent of the footprints across seasons.

Table1: A passive tracer has a common definition which is different than the definition

used here. This term needs to be changed. "interacting tracer" could be an option. "Passive" means that the tracer does not react with other atmospheric components. This term means "chemically non-reactive". Figure 2 (caption): " Panel (a) shows…is driven mostly by…" re-phrase Figure 5 (caption): "over- or under-represented" compared to national scale fluxes? Explain in the text. Figure 6: This figure illustrates the physical problem with your non-passive tracers. The forest signals are visible once the crops have been harvested, creating a time lag in the maximum. This would mean that the depletion of CO_2 due to forest uptake is removed by the crop uptake. Whereas the signals from the forest can be much smaller and so difficult to observe at TTA, there are still observed. Removing the forest removal of CO_2 (uptake) is not physically consistent.

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