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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Biases in regional carbon budgets from covariation of surface fluxes and weather in transport model inversions" *by* I. N. Williams et al.

Anonymous Referee #1

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General Comments

The manuscript "Biases in regional carbon budgets from covariation of surface fluxes and weather in transport model inversions" written by I. N. Williams et al. examines the impact of the synoptic rectifier on boundary layer co2 gradients. Using a simplified boundary layer transport model applied to assimilated and observed forcing data in the Southern Great Plains region, the authors show that model estimates of covariance of surface flux and vertical mixing are too high at weekly and longer time-scales, leading to biases in boundary layer concentration gradients which appear to be amplified during wet years. The authors use a simple Kalman filter to quantify the impact of surface flux

estimates.

Although the authors barely scratch the surface of what I find to be a major implication of the results, this study represents an important contribution to our understanding of how coupling of the water and carbon cycle at different time scales impacts carbon budgets within the boundary layer, and provides a diagnostic for assessing potential climate change impacts on these coupled interactions. In particular, I find it critically important that Carbontracker overestimates covariation between PBL depth and surface fluxes during the anomalously wet year of 2007. From this finding, the authors make what I think is the most important conclusion of the study, that surface flux inversions would benefit most from improved simulation of soil moisture, as this will affect both surface fluxes and transport.

I think this paper is strong as is but would be greatly enhanced by more discussion (abstract, intro, and conclusions) about these water/carbon cycle effects. The timing of this study is fortuitous with respect to the launch of the Soil Moisture Active Passive (SMAP) satellite next summer, which is expected to provide global coverage of soil moisture, and it would be nice to see the authors discuss how they think such measurements might benefit a study like this. The authors spend much of the paper developing and applying their stochastic approach for quantifying the impact of the synoptic rectifier – I hesitate to comment here because I lack expertise in statistics, but somehow the justification of running the simplified transport model with and without this stochastic estimate of covariation is lost on me. It may be helpful to clarify this in the introduction. Besides these and a few other science questions below, I think the paper is well written, results are robust, and the findings address several key carbon cycle issues. I therefore recommend this paper for publication in ACP after some minor revisions.

Specific Comments

Abstract

1. 19052, 12:13 – Please clarify or be more specific about "improved simulations of

dynamics". This statement suggests mixing dynamics but my impression from your conclusions is you mean better simulations of soil moisture at low frequencies, which will indirectly improve dynamics.

2. It is not clear why horizontal co2 advection is included along with surface co2 exchange in the co2 flux term, F, if you are primarily interested in covariation of vertical forcing terms. Indeed, this term is removed later in the paper to demonstrate the importance of covariation of surface exchange with h and E. Either remove this term completely or provide better motivation.

3. 19059, 16 – Why are you using EC flux at 60m? Have you tested sensitivity of observed covariances to measurements at different levels? Surface co2 flux should be stronger closer to the surface, and I suspect this will give higher covariances in Fig. 2 and CSD's in Fig. 5, in which case the synoptic rectifier bias in CT won't be so high, particularly in 2007.

4. Section 3 - How are CT and observed horizontal winds specified? How about CO2 observations in the free troposphere?

5. Section 4.1, 19064, top paragraph – Poor representation of sub-grid vertical mixing by moist convection is probably a major factor driving the synoptic rectifier bias. Since this is tied to the covariance of water and carbon cycles at high and low frequency time scales, please discuss in more detail throughout paper.

6. 19066, 11:13 – Is the small co2 gradient at SGP due to this particular location, or is it systematically low in CT? Did you look at CT over the range of sites discussed in Stephens et al. [2007]?

7. 19067, 1 – You mention that covariance of surface flux & BL height is twice as important as covariance of entrainment and BL height. Is this surprising? Why should we expect E & h to covary?

8. 19067, 17 – comparison to gravity wave dissipation is confusing. Please either

clarify how this is tied to the paper, or dismiss.

9. 19069, 6:10 – You say assimilation of higher frequency data is needed, but this contradicts an earlier statement that CT assimilates flask and tower data (see 19061, 11). Please clarify

10. 19069, 21 – either use the extreme case (0.3 ppm) or average (0.1 ppm). Its hard to justify using a value between the mean and extreme, unless it's the mode.

11. 19069, 27 – Does 1-sigma F mean surface flux or surface flux + advection?

12. 19070 – First conclusion is already being done with CT. Please clarify.

13. Conclusions – Would assimilating column data (e.g., TCCON, GOSAT, OCO-2, etc.) help reduce flux estimation errors due to the synoptic rectifier? It might be possible to test this by rearranging Eq 1 to obtain a prognostic equation for column CO2 and then rerunning the stochastic BL model with TCCON data at Lamont and CT data sampled using the TCCON averaging kernel. In either case, please speculate in the conclusions on the impact of column data.

Technical Comments

1. Figure 5 a:c – legend for synthetic CT-TM5 forcing needs to be dashed

2. There is an imbalance between the number of "unshown" figures (3) and the number of supplementary figures (2). The unshown figures also sound more important to me than the supplementary figures, although its possible that all can be dismissed. So, please either remove the supplementary material and add another unshown figure to the main text, or insert the unshown figures into the supplementary material.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19051, 2013.