Response to anonymous Referee #1

We thank the reviewer for their thoughtful comments and suggestions to improve our manuscript. The reviewer comments are shown in italic text, and our response is shown below, in plain text.

General Comments:

A) 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.

We added text to the revised paper on the potential for using soil moisture measurements (see also our response to comment #13 below). Soil moisture measurements may be useful in identifying sources of error in covariance between modeled transport and surface fluxes, as this covariance may be driven by soil moisture through its effects on photosynthesis and sensible and latent heat fluxes.

B) 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.

We added explanation in the revised paper for why we needed the simplified transport model (SBLM) to address our science questions. Transport model inversions infer surface sources and sinks from temporal and spatial variability in concentrations. Covariation of surface fluxes and transport can generate spatial variability in CO_2 that transport model inversions may not be capable of correctly interpreting in terms of surface sources and sinks. Therefore we are interested in not only the covariation of surface fluxes and transport, but in how this covariation impacts the spatial concentration gradients. The simplified stochastic transport model allows us to quantify this impact on the vertical concentration gradient.

Specific Comments:

1) 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.

The revised manuscript has been clarified as suggested. By dynamics we meant soil moisture and other land-surface characteristics (e.g. LAI) that can influence both the land surface model underlying Carbon Tracker (CASA) and the atmospheric transport model. The transport model is constrained by inputs from reanalysis products that assimilate atmospheric observations. The reanalysis assimilation scheme will be influenced by the representation of soil moisture and land-atmosphere coupling in the ECWMF model (e.g. Betts et al., 2009; Albergel et al., 2012).

2) It is not clear why horizontal CO_2 advection is included along with surface CO_2 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.

This and comments (4), (8) and (11) below are related. We added motivation for including horizontal advection in Section 2 of the revised version, and clarified the input datasets for horizontal advection in Section 3.1.

Horizontal advective tendencies should be included because we are modeling the vertical CO_2 concentration gradient, which responds to both horizontal advection and surface fluxes.

We found that horizontal advection dampens vertical gradients generated by synoptic covariance of surface fluxes and transport and should therefore be included in estimates of the net effects of this covariance on vertical concentration gradients. This compensation by horizontal advection is not surprising for a heterogeneous site such as SGP, as horizontal advection tends to dampen spatial concentration gradients, and these spatial gradients are expected when different vegetation types covary with transport most strongly at different times of the year (e.g. April for peak winter wheat CO_2 uptake, and June for peak pasture CO_2 uptake at SGP).

3) 19059, 16 – Why are you using EC flux at 60m? Have you tested sensitivity of observed covariances to measurements at different levels? Surface CO_2 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.

The flux footprint (or source region) is larger at 60 m than at 4 m, and more representative of the Carbon Tracker grid scale, which includes both pasture and wheat in its domain. In fact Carbon Tracker uses the 60 m concentration measurements in its data assimilation routine. We added discussion of these footprint issues in Section 4.2.

The surface fluxes should be approximately constant through the surface layer, according to Monin-Obukhov similarity theory, though the concentrations can vary substantially with height. The differences in flux magnitude with height mentioned by the reviewer are probably more due to the fact that the 60 m flux samples a larger footprint than the 4 m flux. For example, there is pasture surrounding the winter wheat field on which the 60 m tower is located, and pasture has a different timing of peak photosynthetic uptake in the annual cycle than winter wheat. We added discussion of land surface heterogeneity and how this impacts our estimate of Carbon Tracker errors in the revised Section 4.2.

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

We added description of the horizontal advection inputs and free-troposphere observations in Section 3.1. Horizontal winds are specified from Carbon Tracker (TM5, driven by ECMWF reanalysis winds) for both the observations and CT cases, and free-troposphere CO_2 observations are taken from aircraft flask measurements made approximately once per week (as described in the revised Section 3.1).

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 the paper.

We added discussion on this topic to the results and conclusions sections, where appropriate. There is evidence that convective parameterizations overestimate land-atmosphere coupling in climate models, with dry states persisting possibly because parameterizations are unable to account for boundary layer thermals that penetrate the stable inversion layer (Hohenegger et al., 2009). There is also a smooth transition between shallow and deep convection that the parameterized models are not able to simulate (Zhang and Klein, 2010). These biases could impact boundary layer heights, as dry states with less cloud cover would tend to maintain deeper boundary layers. Moist convection also sets in too early in the diurnal cycle, in phase with solar forcing rather than in the afternoon hours, which could result in vertical transport of CO_2 that is more representative of nighttime respiration than afternoon photosynthesis.

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]?

The smaller gradient at SGP is not an error in Carbon Tracker but rather a result of the seasonality of heterogeneous fluxes at this site. This point was made clearer in the revised manuscript. The 4.4 ppm referred to in our discussion paper was for an average of the Northern Hemisphere sites as reported in Stephens et al., (2007). Note that there was an error in our discussion paper and the actual value is 4.8 ppm. The SGP vertical gradient seasonal cycle is smaller compared to other Northern Hemisphere sites in part due to land surface heterogeneity, because the peak surface carbon uptake from winter wheat (typically in April) is not coincident with the peak in uptake from pasture (typically in June).

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?

We added motivation for this comparison in our revised paper. We expected E and h to covary because the boundary layer grows by entrainment, so that deeper boundary layers tend to entrain more free-troposphere air. Also, the subsidence velocity is typically an increasing function of height in the lower troposphere (e.g. Williams et al. 2011, as cited in our discussion paper), so a deeper boundary layer will tend to experience greater large-scale mass fluxes by the subsiding flow.

The relative importance of these two covariation terms is important for identifying likely sources of errors, whether those be in the transport model or in the land model and assimilation scheme (which adjusts the land model prediction). The land surface component would be a less likely source of bias due to rectifier effects if the covariation among atmospheric transport terms were more important for concentration gradients than covariation between the land fluxes and transport.

8) 19067, 17 – comparison to gravity wave dissipation is confusing. Please either clarify how this is tied to the paper, or dismiss.

As Reviewer 1 suggested, we discarded mention of gravity wave dissipation, since it is included in the horizontal advection term.

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

We clarified what we meant by “assimilates flask and tower data” in the revised paper. Yes, Carbon Tracker does use continuous tower observations and samples the model at the time and location of these measurements to create an error estimate, but this error is used to optimize scaling factors on CASA modeled land fluxes that are updated once per week. This weekly assimilation time-scale does not account for covariation that occurs on weekly or shorter time-scales, or having periods of less than 14 days in Fourier space. Adjusting the scale-factors on this weekly time-scale could introduce bias in the optimized flux estimates if the errors are due to bias in the covariation of fluxes and transport as opposed to bias in weekly mean fluxes. The effect of this covariation on the weekly optimized surface fluxes is the synoptic time-scale analogue to the seasonal rectifier effect, which can create bias in the annual mean surface flux estimates for seasonally varying but annually balanced surface fluxes (e.g. Denning et al. 1995, as cited in our discussion paper).

Another issue is that not all sites have continuous measurements. Flask data is typically collected once per week. These less-frequent flask measurements are useful for informing transport model inversions at monthly and seasonal time-scales, but cannot inform transport model inversions at shorter, synoptic time-scales.

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

We used the 2007 case (0.3 ppm) in the revised version of the paper.

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

Yes, this term was clarified in the revised text.

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

We clarified our statement in the conclusions as the reviewer suggested (see our response to comment 9 above, which is related).

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.

A discussion on column data was added to the conclusions in the revised version. The synoptic covariance examined here results only in a vertical concentration gradient, leaving the column-average unchanged. The idea behind this experimental design is that the covariation of transport and surface fluxes can generate vertical concentration gradients that may be falsely interpreted by the transport model inversions as surface sources and sinks. Column data is one way of discriminating between true surface fluxes and these apparent surface fluxes due to vertical covariation, as true surface fluxes would generate column-mean tendencies in CO_2. However there is also the issue of resolving the vertical structure of horizontal advective tendencies in the column. For example, CO_2 transported from the surface to the upper troposphere by deep convection would appear as a source or sink to the column in the upper troposphere some distance downwind of the deep convection, and this source or sink would have to be separated from true surface fluxes when using column-average data in transport model inversions. This transport is 3-dimensional, and satellite-based column CO_2 in combination with aircraft vertical profiles would therefore be most useful in further constraining CO_2 source and sinks in transport model inversions.

Technical Comments

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

This has been fixed in the revised paper.

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 it's 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.

We will add an unshown figure to the revised paper.

References

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A. K. Betts, M. Koehler, and Y. Zhang, "Comparison of river basin hydrometeorology in ERA-Interim and ERA-40 reanalyses with observations," Journal of Geophysical Research-Atmospheres, vol. 114, Jan 20 2009.

C. Hohenegger, P. Brockhaus, C. S. Bretherton, and C. Schaer, "The Soil Moisture-Precipitation Feedback in Simulations with Explicit and Parameterized Convection," Journal of Climate, vol. 22, pp. 5003-5020, Oct 2009.

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