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Interactive comment on "The imprint of surface fluxes and transport on variations in total column carbon dioxide" *by* G. Keppel-Aleks et al.

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We thank the reviewer for her/his review, and address her/his comments sequentially below.

The authors have examined the meridional gradients in CO_2 column abundances, estimated from ground-based remote sensing measurements, to assess the constraints that these data may provide on terrestrial sources and sinks of CO_2. The CO_2 column data from the Total Carbon Column Observing Network (TCCON) are new and the analysis presented in the manuscript provides an understanding of the scales on which variations in the CO_2 surface fluxes are manifested the data. The manuscript also provides insight as to how we can exploit this information to improve flux esti-

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mates in inverse models. It is well written and the analysis is innovative. I therefore recommend publication of the manuscript after the authors have adequately addressed my comments below.

1) Since CO_2 is long-lived, it makes sense to use potential temperature (theta) as a dynamical coordinate in which to analyze the variations in CO_2. However, I don't fully understand how theta is being used here. The discussion suggests that it is being used to help assess meridional displacements that contribute to variations in $\langle CO_2 \rangle$. Indeed, the caption for Fig. 8. states that "$\langle CO_2 \rangle$ and theta covary because both have strong north-south gradients and variations arise from advection across these gradients." However, much of the large-scale synoptic motions in the free troposphere are adiabatic so transport is along theta surfaces and not across the theta gradients. There is transport across the theta gradients in the lower troposphere, where diabatic effects are large and there is strong mixing, but it is not clear if the analysis is being restricted to these transport processes in the lower troposphere. In Fig. 6 the authors presented a case where there is a 5 ppm increase in CO_2 between 5-9 km within a few hours due to the passage of a frontal system. If this frontal lofting was along isentropic surfaces (as was probably the case), there would be little change in the theta of the displaced air parcels. It is, therefore, not clear to me how the use of theta in this case would help characterize the meridional displacement of the air parcels that contributed to this enhancement. Using theta has the potential to simply the interpretation of the CO_2 changes, but the authors need to better explain what the theta variations mean so that we can better interpret the correlations between $\langle CO_2 \rangle$ and theta. For example, what are the transport processes that are reflected in the theta variations, and how does one relate the theta variations in the lower troposphere to what is happening throughout the column?

The covariation in $\langle CO_2 \rangle$ and θ is due to deformation by large-scale atmospheric eddies acting on both fields. We agree with the reviewer that we are not looking at transport

of $\langle CO_2 \rangle$ and θ across gradients. The wording in Fig. 8 was an error, and we have changed the caption for Fig. 8 to read '$\langle CO_2 \rangle$ and theta covary because both have strong north-south gradients and variations arise from advection of these gradients". We agree with the reviewer that some types of advective transport will not be captured by our approach; for this reason, we had investigated the use potential vorticity (PV) as a dynamical tracer. The relationship between $\langle CO_2 \rangle$ and PV was not as clear as the relationship between $\langle CO_2 \rangle$ and θ. Despite these limitations, with a large-enough ensemble of data, we are able to determine the actual gradient using this method (Keppel-Aleks et al., 2011). We have clarified some of these points in the discussion of the meridional gradients.

2) In the same vein, there is little discussion of the possible impact of diabatic effects on the analysis. To isolate the $\langle CO_2 \rangle$ and theta variations the authors used a bandpass filter to restrict the analysis to frequencies between 3–21 days. However, one could imagine that a heating/cooling rate of about 1 k/day (which is not unreasonable) in the lower free troposphere could result in a large change in theta over a 14–21 day period, which would be interpreted incorrectly as a significant latitudinal displacement. It would be helpful if the authors could explain why we can neglect diabatic effects on theta on timescales toward the longer end of the 3–21 day range.

We agree with the reviewer that diabatic effects on θ complicate its use as a dynamical tracer. We picked θ at 700 hPa to be above the boundary layer and therefore to minimize diabatic effects due to latent heating. The bandpass filter of 3–21 days was also determined empirically to remove seasonal trends in θ. It is true that diabatic effects will modify θ and that the use of θ as a tracer is formally justifiable only when the eddy timescale is shorter than the radiative timescale. Empirically, however, θ can be used as a tracer outside this time range. Based on our data, we do not see meridional displacements that are unphysical large; at each of our sites, the meridional displacements observed are within 1500 km (a typical midlatitude eddy length scale). This may

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be due to the fact that radiative tendencies that cause variations within the 3–21 day period can be of either sign.

To address the reviewer's concerns, we tried using a 3–14 day filter, and the estimated meridional gradients are the same (within 1-2%) when using this shorter filter cutoff. This suggests that diabetic effects in the 14–21 day range do not impact our results.

3) I would suggest modifying the discussion on the NEE estimated from the column drawdown. The authors derive the expression for NEE in Eq. (5) by stating on the last line of page 7483 "if we assume advection has a negligible influence on the change in $\langle CO_2 \rangle$, . . ." But previously on page 7482, lines 21-23, the authors stated that because the mean winds are strong, "during one day the column is influenced by airmasses originating more than 700 km upwind." So in introducing Eq. (5) we already know that neglecting advection is not a valid assumption. Then on page 7490 there is the revelation that advection is actually important and the discussion is concluded with the acknowledgement that "while regional information is contained in column abundances, these region flux signals are obscured by larger-scale variations in $\langle CO_2 \rangle$ even on short timescales." It is frustrating to have to wait until this point to read this since we already know that the large-scale variations are important. Instead, the authors should state up front in deriving Eq. (5) that we know that $\langle CO_2 \rangle$ is influenced by large-scale transport, but that we can assess the extent to which $\langle CO_2 \rangle$ captures the regional flux signals by neglecting the influence of advection.

We have made wording changes to this section in the revised paper to more clarify that, although we mathematically neglect the influence of advection in Equation 5, we do expect that transport influences the column but wish to determine whether regional flux signals are apparent despite the contribution of transport.

4) In the conclusions, on page 7498, lines 21-23, the authors state that because the boundary layer (BL) data reflect a mixture of local effects and transport of free tropo-

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spheric air into the BL "it is possible to alias the large-scale component of boundary layer variability into local surface fluxes when attempting to optimize surface fluxes based only on boundary layer observations." I don't understand the point the authors are making here. The implication is that there is a problem with the boundary layer observations, when in fact the issue is that model models cannot reproduce well the small-scale boundary layer processes. As a result, incorporating $\langle CO_2 \rangle$ with the BL data in an inversion will not produce "more robust flux estimates" as the authors conclude on line 26. If an inversion model is biased relative to the boundary layer CO_2 data, incorporating $\langle CO_2 \rangle$ into the inversion will not remove the bias. The inversion will seek a compromise between the two datasets, but the bias will remain and therefore the flux estimates will remain sub-optimal.

It seems to me that the value of the $\langle CO_2 \rangle$ data is that they provide constraints on the large synoptic scales that are generally reproduced well by global models. However, using these data alone in an inversion, assuming the observation network is sufficiently dense, would limit the spatial and temporal scales on which one can estimate the fluxes. Incorporating the BL data would enable one to extend the flux estimates down to smaller spatial and temporal scales, but the poor representation of small-scale transport processes in the models becomes an issue. The only way to obtain more robust (i.e. unbiased) flux estimates using the information in the BL data is to remove the biases in the models, preferably by improving the models.

We agree with the reviewer that there is no problem inherent to boundary layer observations and that the issue with their interpretation arises from the fact that models don't faithfully simulate boundary layer dynamics. The point we are trying to make is that if the only constraint on the mass flux of CO_2 is a boundary layer concentration observation, the model will optimize fluxes based on how the concentration observation relates to the simulated mass of CO_2 in the model. Depending on how diffusive the transport model used is, the inferred mass flux can differ substantially (Gurney et al., 2002). With total column observations, the change in mass owing to surface fluxes is

well constrained. The seasonal mass flux from column observations can then be used as an additional constraint in an inversion of surface data, which would force a less biased flux estimate. We agree that improvements to transport models would contribute much to determining robust flux estimates from atmospheric data, particularly at small spatial scales. In absence of significant steps forward in the modeling of sub-gridscale phenomena, the use of a column CO_2 data sets to constrain large-scale gradients upon which smaller scale flux estimates can be harmonized using boundary layer or direct flux observations still represents an improvement.

References

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