Biogeosciences Discuss., 8, C4935–C4938, 2011
www.biogeosciences-discuss.net/8/C4935/2011/

Biogeosciences
Discussions

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

Anonymous Referee #3

Received and published: 16 December 2011

The authors have examined the meridional gradients in CO2 column abundances, estimated from ground-based remote sensing measurements, to assess the constraints that these data may provide on terrestrial sources and sinks of CO2. The CO2 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 CO2 surface fluxes are manifested the data. The manuscript also provides insight as to how we can exploit this information to improve flux estimates 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.

Specific Comments

1) Since CO2 is long-lived, it makes sense to use potential temperature (theta) as a dynamical coordinate in which to analyze the variations in CO2. 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 <CO2>. Indeed, the caption for Fig. 8. states that "<CO2> 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 CO2 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 CO2 changes, but the authors need to better explain what the theta variations mean so that we can better interpret the correlations between <CO2> 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?

2) In the same vein, there is little discussion of the possible impact of diabatic effects on the analysis. To isolate the <CO2> 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.

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 <CO2>, . . ." 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 <CO2> 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 <CO2> is influenced by large-scale transport, but that we can assess the extent to which <CO2> captures the regional flux signals by neglecting the influence of advection.

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 tropospheric 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 <CO2> 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 CO2 data, incorporating <CO2> into the inversion will not remove the bias. The inversion will seek a compromise between the two datasets, but the bias will remain and there-

fore the flux estimates will remain sub-optimal.

It seems to me that the value of the <CO2> 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.

Interactive comment on Biogeosciences Discuss., 8, 7475, 2011.