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point-by-point reply to Editor's comments

Thank you for clarifying the misconception! I assume that dCa is equal to $d[CO_2]$ multiplied by 2.13, but this assumption is valid only in the atmosphere well-mixed vertically. I agree that it is not clear if the atmosphere can be considered as vertically "well-mixed" on the time scale of months. Who knows? The coefficient for calculating dCa from $d[CO_2]$ may also undergo seasonal changes.

For "avoiding making any implicit assumptions that the volumetric and surface averages are identical" I added the words,

Besides, the "characteristic vertical mixing time of the troposphere is little more than month" (Bolin, 1976), and hence it is not clear if the monthly globally averaged concentration at sea surface provides a good approximation to the monthly globally averaged concentration in the whole volume of the Earth atmosphere.

Another method for estimating N_a , so-called inversion of simulated tracer transport, is not based on the assumption that the atmosphere is well-mixed vertically: mixing processes are described using an atmospheric transport model. This method is theoretically more sound, but fairly complicated.

at the line 5 on page 4, section 2.1.1. "Observations".

I also revised the section 2.1.1 to make it clear that there are three separate entities:

N_a - de-trended net exchange carbon exchange between the atmosphere and other pools;
 $N_{a,s}$ - the estimate of N_a derived from globally averaged CO₂ concentrations at sea surface;
 $N_{a,v}$ - the estimate of N_a derived from atmospheric inversions;

and as you recommended introduced briefly the TransCom and Fluxnet data here.

The revision of the section 2.1.1 makes Appendix 2 eliminates the need for Appendix 2, and so I remove it.

I change the Fig 3 and Fig 4 to compare $N_{a,mod}$ with $N_{a,v}$; The Figure 9 and Figure 5 are removed. Fig 1 is changed to compare $N_{a,v}$ and $N_{a,s}$.

Although $N_{a,s}$ does not have a central role for model comparison, I would like to pay attention to it to show that the earlier hypothesis about the nature of seasonal changes in the surface CO₂ concentrations (e.g., those made by Keeling et al., 1996) are compatible with the current understanding of global carbon cycle that allows us to tune model parameters to increase the amplitude of $N_{a,mod}$.

The text around the Eq.19 and Eq. 20 is changed as you recommended. I felt that there should be some transition from Eq.19 to Eq. 20. You gave me a good idea on how it could be written.

Other minor points are also addressed in the revised version of the manuscript.

Many thanks for your comments!