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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Terrestrial carbon sink observed from space: variation of growth rates and seasonal cycle amplitudes in response to interannual surface temperature variability" *by* O. Schneising et al.

O. Schneising et al.

oliver.schneising@iup.physik.uni-bremen.de

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We would like to thank the reviewer for the constructive comments, which helped to improve the manuscript. Below we give answers and clarifications to all comments made by the referee, which are repeated in italics.

This paper analyzes variations of CO2 growth rate and seasonal cycle amplitude derived from the 9-year SCIAMACHY data record in comparison to that from the CarbonTracker (CT) inversion analysis, and correlates these CO2 variations with growing sea-

son surface temperature deviations. The SCIAMACHY and CT generally agree well and the CO2 correlations with temperature appear robust at the hemispheric scale. The results confirm that the modeled temperature dependence of respiration and net ecosystem exchange is broadly accurate and consistent with expectations and, further, that this correlation suggests a positive warming feedback in which the terrestrial carbon sink diminishes with climate warming. The paper is very well composed with clear graphics, concise text, and solid analysis and logic. The topic is interesting, important, and the analysis is unique in its breadth and length using satellite data. I certainly recommend it for ACP. I feel, however, that the implication for longer-term projection of climate-biosphere interactions is not warranted and the authors should consider modifying that part of the discussion.

The problem is trying to infer long-term trends from correlated deviations in a relatively short time series. The mechanistic forcing is not necessarily the same. If we think about the biosphere response to temperature rather like the atmospheric CO2 time series, with annual perturbations around a longer-term trend, what is tested here is the response to the temperature fluctuations, not the biospheric trend, which is the crucial climate feedback response. The fact that the CT biosphere model correctly simulates the temperature/flux response simply reflects the fact that the diagnostic model (CASA) has been well-tuned to simulate the current biosphere and its response to weather. It really doesn't inform us about the ability to project the future biosphere, which might have very different plant populations and carbon content. The authors acknowledge this to some extent in the conclusion section. I would prefer that they leave it at that and remove or modify the speculation in lines 13-14 in the abstract and lines 20-21 in conclusions. I fully agree with their bottom line that we need continued vigilance of CO2 from a comprehensive, multi-scale monitoring system including satellites.

The mentioned text passages are rephrased along these lines to better discriminate between derived decadal-scale results and potential longer-term projection uncertainties.

The abstract now reads: "...This suggests that the terrestrial carbon sink is less efficient at higher temperatures during the analysed time period. Unless the biosphere has the ability to adapt its carbon storage under warming conditions in the longer term, such a temperature reponse entails the risk of potential future sink saturation via a positive carbon-climate feedback..."

Then conclusion section is changed to: "...The identified correlations suggest that the efficiency of the terrestrial carbon sink decreases with warming. Under the assumption that the corresponding underlying mechanistic forcing of the current biosphere is not only transitory, this could result in a future sink saturation as a consequence of a positive carbon-climate feedback. However, the derived decadal-scale temperature response does not necessarily allow reliable longer-term projections of climate-biosphere interactions, because the future biosphere might have different carbon content due to divergent plant populations or thermal adaption. Moreover, climate has several different effects on the biosphere and a potential feedback and its strength depend on..."

It might be useful to do the growth rate analysis separately for tropics and SH/NH temperate if the data are sufficient. Seasonal balance of GPP and respiration are quite different in the topics so this might help to better identify mechanisms.

The growth rate analysis for the tropics does not provide as robust results as for the hemispheres (below the 90% significance level). The derived covariations are also considerably smaller (about 0.3 $\mathrm{ppm\,yr^{-1}\,K^{-1}}$) for the tropics (good agreement between SCIAMACHY and CarbonTracker). Therefore, we decided to maintain the original splitting.

Another interesting derivative would be to analyze seasonal minima and maxima separately rather than peak-peak amplitude. How much does wintertime respiration contribute to variability? Annual growth rate is the integral not the amplitude.

In consistence with the finding that the fluxes during the growing season dominate distinctly the variability of the annual fluxes, the correlations and the absolute values

of the covariations (good agreement between SCIAMACHY and CarbonTracker) are significantly larger for the minima (with warm season temperature anomaly) than for the maxima (with cold season temperature anomaly). Hence, the contribution of wintertime respiration to the interannual variability is small, as already indicated by the fluxes. This is consistent with the fact that there are no significant correlations and no covariations differing significantly from zero between the seasonal cycle amplitude and the cold season temperature anomaly. This is in line with the interpretation that the variation of the seasonal cycle amplitudes is essentially driven by the variability of the biospheric carbon uptake during the growing season (drawdown from maximum to minimum), which is made more clear in the revised version.

Minor suggestions

Delete or greatly diminish statement on P. 22741, lines 10-14, unless there is some objective reason to exclude that data point.

The statement is deleted in the revised version.

Insert 'atmospheric' into 'larger ... growth rates' on P.22743, line 16 so we know it's not plant growth.

Done.

Has this been done with flask/globalview data? How does it compare?

The quantitative comparability of surface flask/globalview data and column-averaged satellite data is limited, in particular when looking at seasonal cycle amplitudes or atmospheric growth rates. Therefore, the presented comparison with column-averaged mole fractions of CarbonTracker, which assimilates surface flask data, was chosen. The seasonal cycle amplitudes of surface data are larger than for column-averaged data. Surface and column-averaged growth rates differ because of age of air and variability of the surface growth rate with time; in other words: growth rates of the past "mix in" at high altitudes. The altitude dependent sensitivity of the satellite (averaging kernels) also needs to be considered. This is achieved via weighting of height layers when integrating the CarbonTracker profiles and is not possible for surface data. Therefore, the analysis has not been performed with flask/globalview data.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 22733, 2013.