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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.

Anonymous Referee #2

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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-season 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 car-

bon 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.

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. 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.

Minor suggestions:

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

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

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

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

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