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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The covariation of Northern Hemisphere summertime CO_2 with surface temperature at boreal latitudes" *by* D. Wunch et al.

Anonymous Referee #1

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This paper describes a very detailed but highly interesting analysis of interannual variations in summer CO2 drawdown at high latitudes. The main result is a relatively high climate sensitivity which can be explained from a combination of dynamical transport patterns and ecosystem respiration variations. The authors argue convincingly against other plausible mechanisms. The paper presents many interesting avenues for follow-up research, and provides a range of interesting numbers that others can use to compare data, and models, to. I fear my review is not very useful for further improvements to the paper, but this is simply because of its current excellent state already. I therefore fully support publication of this work, after some minor revisions:

Major comment:

The use of GoSAT in this study does not add any value to the analysis, nor any extra credence to the results. The only use I see for it is to confirm the wide spread nature of the XCO2 drawdown variations, which could also be simply stated without the description of GoSAT in section 3, or its mentioning in the abstract. I suggest to remove the details about the GoSAT XCO2 and its relatively shallow (to the high standard of the rest of the paper) analysis. This would also save one table, and one figure (7).

Specific comments

Page 10264, line 20: I got a bit lost here in the positive/negative logic in this sentence, could you please describe which variable causes which effect and use decreases/increase in uptake/release of carbon?

Page 10268, line 15: I do not think the biosphere fluxes in Carbontracker are balanced as fires and regrowth are calculated from the same vegetation pools.

Page 10270, line 7: Did you also try this with the Mauna Loa CO2 growth rate? Using the global value might increase some of the signal attributed to the polynomial terms because inclusion of the SH dampens growth rate variations that originate on the NH, and are thus seen first and most strongly at Mauna Loa.

Page 10271, line 25: What is the rationale for not including 2000-2003 in the mean?

Page 10271, line 29: Considering the strong increase in biomass going into the tropics, what would be the effect of shifting the 30N boundary of your analysis further south? This is similar to the possibility that some of the observed/simulated anomalies originate in the tropics and are transported into your domain, thus not representing high-latitude sensitivity to temperature.

Page 10272, Line 14: There must be some surface flux data to back this IAV up with measurements of the quantity you are most interested in. Have you tried to find this?

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Page 10274, line 15: I guess this is similar to my suggestion above. How would you account for this transport, and how would it influence your sensitivity? I know these questions cannot be answered easily, but maybe some discussion can be added.

Page 10672, line 23: Perhaps an analysis of d13C might help to reveal such drought related influences, but one would have to revert back to surface flask samples again.

Page 10277: I was a bit surprised that the authors did not try to provide some more context for the calculated climate sensitivity of uptake in boreal regions. This topic is currently debated actively for the tropical regions, in the context of climate simulations and the ability of the latest IPCC models to capture recent COâĆĆ growth variations (Cox et al., 2013). Perhaps an interesting addition to the discussion could be attempted.

Page 10289: The large negative values of parameter a0 for SIB need some explanation

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 10263, 2013.