Atmos. Chem. Phys. Discuss., 14, C8569–C8572, 2014
www.atmos-chem-phys-discuss.net/14/C8569/2014/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite-inferred European carbon sink larger than expected" *by* M. Reuter et al.

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Received and published: 28 October 2014

The manuscript by Reuter et al explores the possibility of a larger than expected European carbon sink based on a regional inversion approach. The paper is certainly of interest to the ACP community and touches an important and controversial topic, as evidenced by the previous discussions. A larger than expected Carbon sink was already observed by previous studies (Basu et al., 2013; Takagi et al., 2014) but not discussed because their analysis was focussed on the global scale. Other studies attributed those results to large scale retrieval biases and/or long-range transport errors (Chevallier et al., 2014). This study here looks at regional inversions, supposedly insensitive to large scale retrieval biases and/or transport.

A confirmation of previous results is thus an important finding even if some of the results here may not yet be entirely conclusive. As pointed out by Peter Rayner, error analysis of the findings here is inherently complex and prior errors and/or biases hard to quantify. The same holds for satellite retrievals, for which inferred posterior errors a) don't fully reflect observed errors and b) can be spatially correlated, which would also depends on the state of surface-atmosphere scattering. Neglecting these can be as counter-productive as stating that satellite data is too inaccurate for source/sink inversions.

Nevertheless, I think the authors went to great length to analyse the European Carbon sink and the manuscript should be published after some important caveats are made more clear and after careful consideration of comments posted by inverse modeling experts.

My biggest concern is (still) the fact that the satellite data mainly reduces uncertainties in the growing season, in fact for less than 6 months of the entire annual cycle. As the authors acknowledge:

A relatively large fraction of satellite observations are made within this period because of advantageous solar zenith angles and cloud conditions. The poor sampling during the dormant season does not allow for a larger error reduction and it cannot be excluded that CarbonTracker underestimates respiration and/or decomposition within this period, which would result in a weaker annual average sink. However, it should be noted that this is, in principle, accounted for by error propagation into the uncertainty of the annual averages assuming that the a priori fluxes in the dormant season are unbiased. Due to the lower activity of the biosphere during the dormant season, the a priori flux uncertainties in this season are smaller, which is consistent with results from an ensemble study of global inversion models showing the smallest inter-model spread in this season (Peylin et al., 2013).

I think this is still somewhat simplistic, exactly for the reasons as stated by Peter Rayner.

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Simply taking the CarbonTracker uncertainties as uncorrelated errors and trusting their error-bars may be misleading. I think the paper would raise less eye-brows if the focus would be on "growing season uptake" and not the annual sink, which is a mix of months constrained by satellite data plus CT filling in for the remaining seasons. The reason for less inter-model spread in the dormant season may also just be related to the fact that most models use similar simple parameterizations of ecosystem respiration, which is somewhat simpler than photosynthesis in the growing season.

To discuss some of the figures in that regard:

C1 and C2 (by the way, panel labels are missing):

Looking at TCCON (C1) as well as Caribic (C2) mismatches, both the CT and the CT with BESD increment results indicate an underestimate of CO2 in the dormant season and an overestimate in the growing season, basically jumping from negative to positive deltas from March to April. This is already an indication that the dormant season has at least as many "problems" as the growing season and they point in the opposite direction. It seems not unlikely that the annual amplitude of NEE is underestimated, which is different than the annual integral of NEE, which can be small in magnitude and can be prone to many errors. I think the caveat that the derived annual sink is derived not only from satellite data but also from flux assumptions in the dormant season by CT should be mentioned in the abstract and ideally also in the title (e.g. "Larger than expected European growing season CO2 uptake. . . "). The authors should be aware that some people only read the title and the abstract. This would not hurt the paper or jeopardize its publication. Pointing to caveats can also trigger future research, which this manuscript may instill. Hopefully, OCO-2 will help to shed further light on that aspect in the future. Regarding the comment about similar retrieval biases from satellite data: I think the various retrieval methods are indeed sufficiently different to de-couple thier potential systematic biases. Biases in RemoteC and ACOS were found to not correlate with the same geophysical variables, for instance. This might, in the long run, even call for ensemble retrievals of XCO2 to quantify error more rigorously.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 21829, 2014.

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