Atmos. Meas. Tech. Discuss., 5, C913–C915, 2012
www.atmos-meas-tech-discuss.net/5/C913/2012/

Atmospheric Measurement Techniques Discussions

Interactive comment on "A simple empirical model estimating atmospheric CO_2 background concentrations" *by* M. Reuter et al.

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Received and published: 10 May 2012

1 Discussion

Reviewer 2 had several interesting comments and gave useful recommendations which we discuss in the following. The review was profound and constructive and helped us to strengthen the paper.

2 Comments and Recommendations

Reviewer 2: *pg 1297, line 16: There is one technical issue with the approach that could improve the value of the model. Currently, SECM uses a vertical profile shape with a fixed pressure cut-off, pt = 0.2, ... It may not be possible to modify the model to include a variable value of pt, but this source of error should be acknowledged*
Authors: Done. We now discuss this point as potential improvement of future versions within Sec.3: "Additionally, the profile shapes could be improved if variations of the tropopause height were taken into account. In more complex future versions of SECM, one could realize this by, e.g., introducing additional model parameters accounting for latitudinal and/or seasonal variations of p_t."

Reviewer 2: *pg 1301, line 23: Here, the authors note: "The smoothing error becomes less important if XCO2 retrievals are used in an inverse modeling framework because it will be removed from the retrieval within the assimilation process." I don't believe that this statement is entirely true. Smoothing errors can be made "less important" by employing an accurate, sounding-by-sounding averaging kernel in a well-conceived inverse modeling framework.*
Authors: We reworded this sentence: "The smoothing error becomes less important if XCO2 retrievals are used in an inverse modeling framework accurately employing sounding-by-sounding averaging kernels within the assimilation process.".

Reviewer 2: *Pg 1303, Line 19: I am concerned by the statement "SECM can be used to identify obvious retrieval errors (by monitoring the difference between retrieval and SECM)."*
Authors: We rejected this statement.

Reviewer 2: *Besides these points, my only concern is that the last statement in the conclusions is unnecessary, and seems more like salesmanship than science.*

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"Moreover, it is remarkable, how well (more than 94% explained variance) a simple empirical equation (depending only on date and latitude) can reproduce atmospheric CO2 concentrations." I see nothing particularly remarkable about this. As noted earlier, CO2 is a long-lived well-mixed gas. When averaged over regional scales (1000 x 1000 km), the largest known changes in XCO2, are no larger than 2-3% from pole to pole, or throughout the seasonal cycle. The authors make no attempt to model the true variability (e.g. CO2 weather), only the zonally-averaged component of the output of a model (Carbon- Tracker). In spite of this, it still takes a 17-parameter function fit its variability. I suggest that this statement be omitted.

Authors: We rejected the last sentence of the conclusion. However we would like to keep the following (similar) statement within the first paragraph of the conclusions: "In other words, a simple empirical equation (depending only on date and latitude) explains more than 94% of CT2010's variability (including, e.g., CO2 weather), i.e., of our current knowledge on atmospheric CO2 concentrations."

We agree, CO2 is long-lived and well mixed and these are the reasons for having only small variations. Therefore, 395ppm is already a rather good guess (which is indeed not so remarkable). However, the challenge of modeling and retrieving CO2 are the small variations (where all the information about sources and sinks is hidden in). We compared SECM (depending only on date and latitude) with globally distributed CT2010 values (including e.g. CO2 weather). The fact that SECM explains more than 94% of the variance means that a simple parameterized zonal average is almost everywhere on the globe a very good estimate regardless of (CarbonTracker's) CO2 weather.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 1293, 2012.