Atmos. Meas. Tech. Discuss., 5, C910–C915, 2012
www.atmos-meas-tech-discuss.net/5/C910/2012/

Atmospheric Measurement Techniques Discussions

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

M. Reuter et al.

reuterm@loz.de

Received and published: 10 May 2012

1 Discussion

Reviewer 1 had some main comments and raised several interesting questions and gave useful recommendations which we discuss in the following. The review was profound and constructive and helped us to strengthen the paper.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

2 Main Comments

Reviewer 1: *The research presented is not particularly novel: for several years, the TCCON science team has been using a similar approach...*
Authors: As Reviewer 1 states "the TCCON approach ... is not widely available". Therefore, we cannot comment on how novel the SECM approach actually is or not. It would be interesting to see the particular similarities and differences.

Reviewer 1: *Throughout the paper, the authors use CarbonTracker (CT2010) as "truth"...*
Authors: At the beginning of the introduction we already refer to the (potential) deficiencies of CT2010. However, we now clarify that CT2010 is only considered as current knowledge and reasonable a priori estimate (but not truth): "Therefore, within this publication, we consider CT2010 (CarbonTracker version 2010) as current knowledge and reasonable a priori estimate for atmospheric CO2 concentrations."

Reviewer 1: *On page 1297, they state that "The parameters c0 and c1 are determined similarly as a00-a14 by least squares fitting to CT2010 mixing ratio profiles." This means that the vertical mixing bias in TM5 is propagated into SECM. It appears then that the authors have not validated the shapes of their profiles against data. Such data is available (e.g., HIPPO, NOAA GMD aircraft program, ACE satellite). Instead, the authors both train the SECM with and primarily validate the SECM against Carbon-Tracker.*
Authors: We consider CarbonTracker as current knowledge on global CO2 concentrations. Therefore, it would be convenient to use CarbonTracker as a prior for satellite retrievals. For this reason, we train and benchmark SECM primarily with CT2010. Sec.6 makes clear that we are primarily interested in the additional smoothing error. This means, we are mainly interested in well reproducing the current knowledge rather than in validating the current knowledge which would give the paper a different focus.

AMTD
5, C910–C915, 2012

Reviewer 1: *The shapes of the profiles could be improved if variations in tropopause height were taken into account. Although this adds complexity to the model...*
Authors: We agree that SECM's profiles are extremely simplified. This applies not only to the tropopause but also e.g. to the lower boundary layer. We wanted to keep the model as simple as possible. In Sec.6 the additional smoothing error introduced by SECM is calculated and shown to be not dominating the expected retrieval errors of any of the considered retrievals. Therefore, we expect less relevant improvements of the XCO2 retrieval errors when using more realistic profile shapes. Nevertheless, we now discuss this potential improvement 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 1: *On page 1299, the authors state that the error covariance matrix can only be a reasonable approximation of the total error if the total error is dominated by the differences between SECM and CT2010. ... Fig. 4, which suggests that the total error is not dominated by differences between SECM and CT2010*
Authors: We agree that CarbonTracker has limitations which can be larger in magnitude than the differences between SECM and CarbonTracker. We now point this out in Sec.4: "The differences between CarbonTracker and ground based FTS measurements shown in the publications of, e.g., Reuter et al. (2011); Schneising et al. (2011); Keppel-Aleks et al. (2012) indicate that this is probably not alway the case. Therefore, a more realistic estimate of the total covariance structure could be determined by either deriving one covariance matrix from a comparison of SECM vs. truth or by combining two covariance matrices from a comparison of SECM vs. CT2010 (shown here) and a comparison of CT2010 vs. truth (similar to the work of Eguchi et al., 2010)".

Reviewer 1: *i) It appears that the SECM overestimates summer CO2 at the*

AMTD
5, C910–C915, 2012

northern hemisphere sites of Bialystok, Bremen, and Park Falls. At these locations, the phasing of the seasonal oscillation also seems different, with the spring drawdown and the fall increase too "slow" compared to the data. ii) At Darwin, the curvature of the SECM time traces does not agree with the data well.

Authors: We discuss these points now in detail in Sec.5: "Despite the overall good statistical agreement one can find some small but systematic deviations at some of the TCCON sites. At Bialystok, Bremen, and Park Falls one can find less pronounced seasonal amplitudes resulting in a too slow spring drawdown and fall increase. At Darwin, the curvature in the SECM time series does not agree well with TCCON. However, the seasonal cycle is less pronounced here and differences become more apparent. The reasons for these deviations can be found in the simplicity of SECM but also in shortcomings of CT2010 (e.g. Reuter et al., 2011; Schneising et al., 2011; Keppel-Aleks et al., 2012)."

Reviewer 1: *It would be helpful if the authors broke down how well the SECM compares against CT2010 in different regions, rather than simply aggregating 10,000 randomly chosen global profiles to give the statistics at the end of section 2*

Authors: Done. We added the follwing discussion to Sec.2: "The standard deviation of the difference, referred to as standard error in the following, amounts to 0.99ppm in total, 1.15ppm in the northern hemisphere (30^{o}N-90^{o}N), 1.06ppm in the tropics (30^{o}S-30^{o}N), and 0.92ppm in the southern hemisphere (90^{o}S-30^{o}S)."

Reviewer 1: *Could the same SECM parameters also reasonably give priors for 2012, or will the model need to be re-trained with data up to 2011?*

Authors: We updated the discussion of this point within the conclusions: "The TCCON comparison goes one year beyond the fitting period 2003-2009. As we found no obvious problems in 2010, we conclude that SECM is also (at least to some extend) able to extrapolate into the future. In the case of extrapolating into a farer future or past, it would be advantageous to replace the linear increase of Eq.1 by an

AMTD
5, C910–C915, 2012

exponential term. This, however, could require a longer fitting period to produce stable results. Additionally, one could think of rejecting the time dependency of the seasonal amplitude (a08 and a13) when extrapolating from a short fitting period".

3 Minor Points

Reviewer 1: *The authors state that the SECM can be used as a quick check for obvious retrieval errors...*
Authors: We rejected this statement.

Reviewer 1: *Reversed C's in TCCON*
Authors: Done.

Reviewer 1: *The authors conclude with a statement that "it is remarkable how well a simple empirical equation depending only on date and latitude can reproduce atmospheric CO2 concentrations". I think the authors are somewhat overselling their model here, and need to keep in mind that CO2 is quite well mixed and that it is variations of order 1-2% in atmospheric carbon dioxide from which seasonal and net carbon fluxes are inferred.*
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

AMTD
5, C910–C915, 2012

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.