**Atmospheric
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Discussions

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Interactive comment on "CarbonTracker-CH₄: an assimilation system for estimating emissions of atmospheric methane" *by* L. M. Bruhwiler et al.

Anonymous Referee #2

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This discussion paper describes an inverse modeling system for atmospheric CH4 ("CarbonTracker-CH4") and its application to analyze emissions of atmospheric CH4 during 2000-2010. The presented CarbonTracker-CH4 is based on the well-established CarbonTracker-CO2 (using the same Ensemble Kalman Filter (EKF) method and the same transport model (TM5)), adapted to atmospheric CH4. The paper contributes to the scientific discussion about the trends and (inter-annual) anomalies of atmospheric CH4 during the past decade. While the subject of the manuscript is well within the scope of ACP, I have several general comments, which should be addressed before publication in ACP:

(1) Various recent studies investigated the recent increase (2007) of atmospheric methane, including 2 studies [Bergamaschi et al., 2013; Houweling et al., 2013] which

used the same transport model (TM5) and analyzed the same period as the presented discussion paper. I would recommend to describe the recent studies in some more detail in the introduction, to motivate better the current study, and explain better the novel elements and added value of the presented CH4 inversion.

(2) There are several limitations of the presented modeling system, which require a more detailed analysis / discussion:

(a) the CarbonTracker-CH4 aims to estimate 10 different terrestrial emission processes for each of the 12 continental (TRANSCOM) regions. Separating these processes, however relies not only on a dense measurement network, but also on accurate knowledge of the spatial emission distribution for each of these processes. For many processes, however, the chosen EDGAR inventory has simplified methods for the spatial disaggregation of emissions per country (e.g. based on population density). Also it should be mentioned that the use of large predefined regions (here 12 continental and 1 ocean regions) may lead to significant aggregation errors [Kaminski et al., 2001].

(b) the inversion uses many regional and coastal monitoring stations (e.g. SGP, HPB), which are probably not very represented by the model, given the coarse model resolution of 6x4. In this context, the strategy to estimate the "model-data mismatch" errors should be explained in more detail.

(c) to analyze trends and anomalies it is essential that the inversion uses consistent settings during the analysis period. Why does CarbonTracker-CH4 use operational meteorology, and not the ERA-INTERIM re-analysis? What is the impact of the increase in vertical resolution in the ECMWF model (increasing from 60 to 91 vertical layers in 2006) and the concomitant change in vertical resolution chosen for TM5 (25 -> 34 layers)? Beside consistent meteorological fields and model settings, also consistent observations are essential. For some of the used sites, however, measurements started only after 2000 (e.g. SGP). It should be analyzed / discussed, how the derived trends (e.g. for fossil fuels from temperate North America (Fig. 11)) could be influenced

by the different temporal coverage of observations.

(d) the authors mention the critical importance of the smoother window (which is set to only 5 weeks in the current study), e.g. on page 2180 "As pointed out by Bruhwiler et al. (2005), a smoother window of at least 3 months is likely to make maximal use of remote network sites" - it is a pity that this is not further analyzed in the present study.

(3) Global and zonal averages (section 3.3): It should be explained better, how exactly the global and zonal averages have been constructed (e.g. are they based on all sites listed in Table 2 or only on remote background sites ?). I would recommend to include also the analysis of the extra-tropical southern hemisphere (e.g. adding a corresponding set of plots in Fig.7). I would consider the discussion of global and zonal averages basically as an extension of the discussion of residuals (section 3.1), therefore section 3.3 could be moved directly after 3.1. The wording in the abstract and conclusions ("demonstrate the diagnostic value of global or zonally averaged CH4 abundances") suggests that this is a novel way to analyze the results - I would rather consider this a standard approach to evaluate the performance of the assimilation.

(4) Comparison to aircraft profiles (section 3.2). Although the authors state that aircraft profiles are available from 17 sites, they show only the comparison for 2 sites (THD, DND). I would recommend to analyze the aircraft profiles in some more detail including all available sites.

(5) High northern latitudes (section 4.1). The authors define the "high northern latitudes" as aggregation of the TRANSCOM regions "Boreal North America", "Boreal Eurasia" and "Europe". The latter, however, includes also central and southern Europe (including the Mediterranean countries). It would be more consistent to consider here only the northern part of Europe (even though only the emissions of the whole "Europe" region are optimized), or alternatively include "Europe" in the "Northern Hemisphere mid-latitudes"

Specific comments:

abstract, page 2176, lines 9/10: "...a result consistent with previous;" I assume previous studies are meant ?

page 2178, line 12/13 " It contributes about half the radiative forcing of CO2, 0.48±0.05 Wm−2 in 2010": according to the cited http://www.esrl.noaa.gov/gmd/aggi/aggi.html, the radiative forcing of CO2 was 1.791 Wm−2 in 2010.

page 2178, line14-15 " it is 25 times more efficient per mass" I assume that the GWP is meant (defined as the time-integrated RF due to a pulse emission of a given component, relative to a pulse emission of an equal mass of CO2 (IPCC AR5)) ? Add reference.

page 2178, line 9. "increase" -> "to increase"

page 2179: at which temporal resolution are emissions optimized (weekly / monthly ?) ?

page 2179, lines 13-17: from the listing it is not clear which are the single emission processes that are optimized (e.g., is "oil and gas production" one process or two separate processes ?).

page 2181, lines 1-2: check sentence

page 2183, lines 4-5, "prior flux estimates of Bergamaschi et al. (2007)": In the cited paper 2 different wetland inventories were used - which of these has been used in the present study ?

page 2183, lines 16 / 19: add references for the estimates of CH4 emissions from oil and gas production, and from coal mining.

page 2184, line 4: Why has the old EDGARv3.2FT 2000 inventory been used (and not newer versions, which may have better spatial disaggregation for some processes) ?

page 2184, line 27: add reference of CH4 emissions from rice agriculture. How is the seasonal variation of CH4 emissions from rice agriculture modelled (EDGAR provides

only annual total emissions) ?

page 2185, section 2.7: which GFED version has been used ?

page 2185, lines 23 and 24: add references for the CH4 emissions from ocean and seeps in shallow coastal waters.

page 2187, lines 3-4, "Krol et al. (1998) estimated that the uncertainty in globally averaged OH is $\pm 10\%$." reference is missing. Specify, if the quoted uncertainty refers to multi-annual average or yearly average.

page 2187, lines 5-6: add reference for stratospheric sinks.

page 2188, line 16 (and Table 2): How are the prescribed model-data mismatch errors estimated ?

page 2193, line 25, " 81 ± 41 TgCH4 yr-1" check given uncertainty, seems not consistent with uncertainties shown in Fig 8 for the 4 categories (would be useful to give also the uncertainty of total emissions in Fig. 8).

page 2194, line 9, " sufficient to reduce uncertainty by over 75%", seems not consistent with uncertainties shown in Fig 8 for the 4 categories

page 2194, lines 23-24 "...estimates of Bloom et al. (2010). They find that only 2% of global wetland emissions come from the high northern latitudes" . They define the Artic region as ">67°N", which is quite different from the definition in the current paper (see also general comment (5)), and may explain a significant part of the differences (see also Fig 3B in Bloom et al. (2010)).

page 2195, line 6-7, how is the first value given for the anomaly (4.4 Tg) calculated ? Relative to the 2000-2010 average ? How is the "maximum summer anomaly" calculated / defined ?

page 2195, line 10, "103 Tg CH4 yr-1", please check number, seems not correct

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page 2195 / 2196 and Figure 9: While the Arctic temperature anomalies in 2007/2008 had been discussed in previous papers [Dlugokencky et al., 2009], it would be interesting to extend here the analysis of correlation between CH4 emissions and temperature to the entire 2000-2010 period.

page 2196, lines 13-14, " while fossil fuel and agricultural and waste emissions are distributed mainly in populated areas of Europe" these are mainly from mid-latitudes (see general comment (5))

page 2197, lines 17-18, " High latitude emissions of CH4 from agriculture and waste are significant only for Europe" same comment as previous

page 2197, line 26, "Walter et al. (2007)" : reference is missing

page 2198, lines 1-6, " CH4 is stored in ice hydrates.... The estimates of Shakhova et al. (2010) estimate the size of this source to be 10 TgCH4 yr-1 for the Arctic Ocean". Shakhova et al. (2010) do not claim that the CH4 emissions which they estimate for the East Siberian Artic Shelf region are due to CH4 hydrates.

page 2200, line 5, "EDGAR (version 4.3)", To my knowledge version 4.3 has not yet been released. Please check version number.

page 2200, line 9, "Fig. 5": I assume this should be Fig. 6 ?

page 2201, line 18, " EDGAR emission estimates are too high": do you mean "emission estimates" or trend in emissions ?

page 2206, line 16, " agree well with observations, generally to within 15 ppb". Should be specified more accurately - do you mean average bias here ?

Figure 5: labels at x-axis are missing

Figure 6: "The growth rate is computed by taking the first derivative of the average mole fractions shown in the top figure": I assume that the concentrations are first de-seasonalized ?

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References

Bergamaschi, P., et al., Atmospheric CH4 in the first decade of the 21st century: Inverse modeling analysis using SCIAMACHY satellite retrievals and NOAA surface measurements, J. Geophys. Res., 118, 2013.

Houweling, S., et al., A multi-year methane inversion using SCIAMACHY, accounting for systematic errors using TCCON measurements, Atmos. Chem. Phys. Discuss., 13, 28117–28171, 2013.

Kaminski, T., P. J. Rayner, M. Heimann, and I. G. Enting, On aggregation errors in atmospheric transport inversions, J. Geophys. Res., 106(D5), 4703-4715, 2001.

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