

Interactive comment on “Retrieval of methane source strengths in Europe using a simple modeling approach to assess the potential of space-borne lidar observations” by C. Weaver et al.

Anonymous Referee #1

Received and published: 7 September 2013

This paper aimed at evaluating the required measurement precision of CH₄ from space for estimating surface emissions and/or emission changes from European region. They have set up an inversion system consisting of 262 division over the western europe. The paper is generall well written for reading. However, I found there are lack of information on the model set up, evaluation criteria, analysis method etc. I have listed several of my concerns below, and recommend the authors conduct additional experiments for addressing the systematic bias in measurements from space. Eventhough the work is important for setting targets for the future satellite missions, I feel a major

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revision is needed before publication of this article in ACP.

Detailed comments:

P.19563, Line# 10ff: A bit concerned about this fundamental assumption. The daily variations are likely to be caused by local emissions or synoptic transport as revealed by the Transcom continuous experiment for CO₂ as well as CH₄ simulations later using Eulerian chemistry-transport model (Patra et al., JMSJ, 2009).

That means, to be able to derived emission information one need to know the transport perfectly at hourly timescales and at very fine resolution. I have my own reservation whether or not 0.5x0.5 longitude-latitude boxes are good enough for that.

Additionally, flexpart may not be a good model for the species that have long lifetime and are globally well mixed.

P.19563, Line#26: How good is this 150m of emission height approximation, when you have sites respresenting wide variety of sites. For example the air column above Jungfrauoch site, at 3580m, can be very thin comared to the column above Mace Head.

P.19563, Line#27: If you have assumed 20 days lifetime for the CH₄ particles, why again this OH chemistry? The OH chemistry with instanteneous lifetime of longer than 3-4 years in the mid-high northern latitudes should have non-measurable impact on the forward simulations.

P.19564, Line#2: What is the rationale for having a model top up to 400 hPa - is it the typical height of the tropopause over your model domain or the experimental design for measuring CH₄ column below 400 hPa? Please clarify.

P.19564, Line#6: may be 'i-th' is better.

P.19565, Line#2: I think it is not the effect of horizontal resolution, more important probably is how the meteorology is paremeterised in Flexpart. Also, I am not con-

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vinced by the methodology applied for correlation analysis. Looks like you correlating concentration at hourly time intervals, please correct me if I am wrong. I would like you to separate diurnal and daily variability and then make the correlation analysis, as in Patra et al. (2009). That will enable you to understand the Flexpart transport quantity better, in my opinion.

P.19566, Line#22: Ideally yes, you want trajectory to travel over all boxes, but what about employing covariance between boxes with spatial correlation?

P.19567, Line#3-4: I am surprised that a good agreement is claimed here with MACC! Can you be quantitative, say for spatial pattern or strength?

P.19567, Line#27: What is the connection here with tulip industry? Was there a link suggested by someone? Give reference or else delete

P.19568, Line#5ff: Lacks discussion, please discuss the role of the baseline determination on your results

P.19568, Line#2: How? Column up to 400mb is only about 60% of the airmass, and recent analysis show that CH₄ column above the tropopause is also important for variability in total column CH₄.

P.19568, Line#2: What about systematic bias, which is what the most dangerous for source/sink inversion (Patra et al., JGR, 2003), and GOSAT inversions struggling hard to deal with the retrieval bias (e.g., Basu et al., ACP, 2013; Maksyutov et al., ACP, 2013).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19559, 2013.

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