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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of the Swiss methane emission inventory by atmospheric observations and inverse modelling" *by* S. Henne et al.

Anonymous Referee #2

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The manuscript "Validation of the Swiss methane emission inventory by atmospheric observations and inverse modelling" by Henne et al. analyze an ensemble of atmospheric inversions of the Swiss methane emissions using continuous CH4 measurements from a ground based network of 4 to 6 sites. The study highlights the consistency between the results from the different inversion set-up and the SGHGI inventory. Finally, it attempts at interpreting the spatial and temporal distribution of the inverted emissions, linking it to the different types of sources and processes underlying the CH4 emissions in Switzerland.

In general the manuscript is very well structured and written. The inversion system and

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the experimental framework seem robust, and the results are highly encouraging for the use of atmospheric inversions as a mean to verify and potentially improve the GHG emission inventories. I do not see any major issue in this manuscript, which I strongly recommend for publication in ACP.

I still have some concerns regarding the text and the results as detailed below.

General comments:

- There is a series of minor issues related to the "baseline treatment". The definition of the "baseline" and of what is targeted through the optimization of the "baseline" is unclear. The term baseline is not really self-explanatory. The definition of the baseline as "conditions without recent emission input" (l. 186) is vague even though it connects it to the concept of model boundary conditions. The different configurations of the state vector for the baseline are not really justified by a characterization of this baseline. Why could it be assumed to vary between the sites that are often quite close to each other compared to their distances to the boundaries of the inversion domain ? Why could it have a full 3D structure in space ? The impact of including the baseline in the state vector and the sensitivity to its configuration is important for such a regional configuration. When the inversion is allowed to optimize a different baseline for each site, it can easily make attribution errors between the baseline and the emissions, as illustrated by the results from experiments S-ML (there is a beginning of discussion on this topic at lines 459-462). Most of the inversion tests use low prior uncertainties in the baselines, which could limit the influence of the baseline on the inverted emissions, but which, on the other hand, may prevent from fully accounting for the uncertainties in the boundary conditions of the inversion domain. The baseline should thus be better defined and discussed, its weight on the inversion and the corresponding attribution errors should be better characterized.

- I feel that the authors go too fast in rejecting the assumptions that the positive corrections applied by the inversion in the north-east of Switzerland could be artifacts. The

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fact that those corrections could balance (through attribution errors or due to a lack of flexibility) the imperfect inversion of the baseline for north-east winds is quite ignored while this is a likely explanation. Furthermore, I do not agree with the sentences at line 908 "it seems unlikely that the same systematic bias would be inherent to both meteorological inputs" and at line 22 "which rules out an artefact of the transport model and the inversion system". Both meteorological forcings have a limited spatial resolution (which can be critical in this area) and errors inherent to FLEXPART would apply to both FLEXPART-ECMWF and FLEXPART-COSMO. Section 4.3 fails to identify a significant missing source. Therefore, my feeling is that the discussion attempting at linking the corrections in the north-east to specific processes in section 4.3 is a bit too long and a bit too ambitious. It could be shortened and the text should acknowledge that some limitations of the inversion system could explain such corrections. What is the weight of these corrections in the national budget of the emissions from the inversions ? Considering that this is connected to a sector underestimated by the SGHGI, or to an artifact of the inversion, considering that the inversion decrease the emissions from agricultural lands compared to the SGHGI, and given that SGHGI is a purely bottom-up estimate, it seems that the fit between the national budgets from the inversion and from SGHGI could be seen as a coincidence without such a weigthing.

- Several components of the inversion configuration need to be presented in a more explicit way and earlier than they are presently: the temporal resolution at which the emissions are estimated (it should be explained in section 2.3, and anticipated at the end of section 2.2 at lines 246-. . .), the transport modeling and inversion spatial domain (what are the COSMO-7 domain and Western Europe in section 2.2 ?). Section 2.3 should confirm the inversion period (Mar 2013-Feb 2014) even though it is implicitly defined near the end of section 2.1. Much of the model vs. data analysis are lead without stating whether they are based on 3-hourly timeseries with or without the data selection applied to the data assimilation (using 12:00-18:00 and 0:00-6:00 time windows only, and filtering the data according to the wind speed: line 77 states that it is used for the inversion, but we do not know whether it is used e.g. for the REBS analysis

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few lines later). Typically, the text does not say precisely what is plotted in figure 3 and 4 in terms of temporal resolution and temporal selection.

Minor comments:

- line 5 vs line 9 vs line 20: you should introduce the "reference" prior emissions at line 5

- line 9 and elsewhere: use error (or uncertainty) covariance instead of covariance by itself; use something more self-explanatory than "baseline treatment" at line 10 ?

- line 8 vs 12 vs 15: you should use the same designation for the inventory, otherwise the reader can believe that you speak about 3 different inventories

- line 11: I feel that "independent character" is a bit strong, even though I do not contest the robustness of the system. You still have large sensitivities to e.g. the prior uncertainty in the baseline which is a critical component of the system. Furthermore, here, you do not necessarily speak about the national budgets only, and the sensitivity of the spatial distribution of the emissions to the inversion set-up is also significant.

- line 58-59: "Methane emissions from individual sources are much more difficult to quantify than anthropogenic emissions of CO2" it's a too general statement to be true

- l. 71-72 : this raises some questions; inventories of CH4 emissions should be built on such an upscaling of site / process scale measurements or on highly uncertain emissions factors, so why would this be too difficult ? or why were the numbers from these specific studies difficult to upscale (because the sampling was too small ?) ?

- line 75: the inversion delivers but does not "combine" an optimal estimate of the emissions

- line 92 : evaluate instead of validate ?

- l. 95 give an idea about this high resolution

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- l.114: you do not target the Swiss Plateau in the following, but rather the national budgets which is thus something different (by 30%)

- l. 185, the justification for using nighttime data at JFJ and SSL goes too fast and should explain why the situation regarding the PBL and the local sources is different there compared to the other sites (otherwise it could seem in contradiction with lines 182-183)

- l 185-198: this paragraph is not fully clear (is the baseline an annual mean, or a series of 60 day averages ?) and will generate some confusions in the following, where the baseline will rather be computed as an interpolation of "baseline nodes" and where only the REBS analysis for JFJ will be used. You should find a way to avoid this little issue either here or later when describing the inversion protocol. "reflecting different degrees of variability and frequency of air masses not influenced by recent surface contact and emissions." is not really clear.

- l. 206: why the spatial resolution of ECMWF is degraded outside the Alpine area ? for computational issues ?

- l. 208-210 : can you explain the role (on the top of the analysis from MeteoSwiss) and horizontal resolution of COSMO ?

- l. 214 "time-inverted" was not mentioned before

- Table 1: the particle release heights are given for FLEXPART-COSMO only, not for FLEXPART-ECMWF unless the release heights at JFJ and SSL for the latter are based on the ground height in COSMO ? (the paragraph on the release heights is not fully clear about this).

- l .244: for the mountaintop site JFJ during nighttime only ?

- l. 258-259 : you speak about sensitivities, not about sensitivities multiplied by the emissions, so you should not write lines 257-259 (or you should speak about mi,j times Ei,j and defining Ei,j correctly)

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- l. 279-280 should be improved

- l. 284: can you give more details about the domains that were tested ? it could be important for the discussion on the baseline and on the corrections in the North East of the Alpine area.

- l. 327 : you will correct the baseline differently for each site, so why using the REBS analysis at JFJ site as a prior for all sites ? see the main comment regarding the lack of characterization for the baseline which could help justify the way it is accounted for in the inversion state vector and the way priors and prior uncertainties are assigned to it.

- l. 303: "the vector f gives the fractional contribution of the region to each inversion grid cell" something is inverted in this sentence and it could be said better in order to mention that it is related to the surface area (see l. 303 vs. l. 306-307). l. 306-307: I am not sure to understand, you will thus derive xk in terms of emission per capita instead of per m2 to be consistent ? then will it impact the results ? The whole paragraph should be clarified.

- l. 317: the letter f is already used for eq. (7), you should use another letter to avoid confusion ?

- Equation 8 and associated parameters at lines 322-323: you do not discuss and justify this modeling of the spatial correlations. Ignoring the independence between the different types of sources of methane in such a definition could raise problems. The 14-day timescale for the errors on the baselines, which could miss the signal associated with synoptic events, is also given without justification. The set-up of the prior and observation/model error covariance parameters cannot be perfect but it could be supported by few explanations.

- l. 321: if you use the vector 1, you will include emissions from all countries, while you aim at comparing it to the Swiss budget (line 323). Could you confirm that you scale

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uncertainties outside Switzerland based on the scaling factor derived in Switzerland ?

- l. 330 add "at a given site"; and speak about the spatial correlations between errors on baselines at the different sites (some justifications may be needed). Line 333: then you apply the uncertainty diagnosed by the REBS for the annual baseline to the uncertainty in 5-day baseline nodes ? or does the number given by the REBS mean something else ?

- the paragraph regarding the model errors is quite confusing and should be improved; it is difficult to understand what exactly means "a constant contribution while the third term represents an uncertainty contribution relative to the prior simulation of above baseline concentrations". Through these computation aren't you attributing part of the prior uncertainty to R ? Lines 342-348 go too fast and are unclear. "Residual" is not a self-explicit term.

-l. 356: the selection of the data should remove this problem of the simulation of the diurnal cycle in the PBL ? furthermore, such an error sounds like a "bias" while 0.5 days is relatively short

- l. 362-363 : even if these assumptions were true, the set-up of the inversion would still be highly uncertain, which explains most of the sensitivity tests. What do you mean by "uncorrelated residuals" ?

- l. 385: could you define the seasons (the corresponding months) here ?

- l. 385 the text could mention here or even before (when defining the temporal resolution of the state vector) that the emissions may actually have significant variations in time (even though it will be discussed in section 4)

- l. 389: here as in section 2.5, you describe some new theoretical components faster than in section 2.3 while it could require the same level of detail (especially when presenting the extKF). I suggest to (1) generalize section 2.3, (2) detail the configuration for the base inversion (3) present the sensitivity tests by detailing each corresponding

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perturbation to this base inversion using the mathematical framework of 2.3.

-l. 390: 90 days is nearly one season, so the inversion might have a limited ability to increase the seasonal variations. Do you know whether decreasing this temporal correlation would have significantly increased the seasonal variations that are analyzed in 3.2 and discussed in section 4 ?

- Section 2.5.3 is a bit short given that many of the inversion parameters, concepts and underlying assumptions change when switching from the base method to extKF. And the text should explain which type of uncertainties are targeted by this sensitivity test. This section should better characterize what is the state vector for each (3-hourly ?) analysis of the sequential algorithm. What is the time resolution of the corrections to the emissions here ? L. 396: what is the tendency of the baseline value ? Are there some consistencies between the B and R matrices used here and that used in the base inversion ? Do you need the ML method to set-up these parameters (maybe switch the sections 2.5.4 and 2.5.3)? l. 403-405 is impossible to understand; how Q is set up ?

- L. 408: based on the national emission uncertainty as estimated by SGHGI SGHGI provides the best knowledge of the uncertainty in SGHGI, not of the uncertainty in the prior estimates (MAIOLICA and EDGAR) used here

- L. 415-416 : residuals and differences sound like shortcuts to me

- L. 427: why ? in theory, the model/obs errors should be similar in the base and extKF inversions, even if the changes in the state vector can impact part of the model error (e.g. the aggregation errors related to the resolution of the state vector)

- l. 435 : who reports to UNFCCC ? FOEN and thus MAIOLICA would be consistent with SGHGI ?

- l. 445-446: rewrite, it's hard to understand.

- Sections 2.5.5 and 2.5.6 define some critical components of the base inversion. So, in section 2.5, we are not just looking at perturbations to the base configuration. This

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supports my earlier suggestion to start giving a general framework for the inversion which could apply to both the base and sensitivity cases, and then to present the base and sensitivity configurations.

- L. 465 and 469: "initial" may be confusing here

- L. 463-465: I would rather discuss the link between the baseline and the boundary conditions (which can be derived from large scale transport models) earlier when defining the baseline.

- L. 473 to 485: shouldn't the baseline rather be discretized in space over the inversion domain boundaries ? The concept of a 3D discretization of the baseline looks strange (see the general discussion about the baseline), but it's balanced in this inversion test by the fact that the same baseline is applied to all sites, and by the coarse resolution of the grid for this 3D baseline.

- L. 481-482: it is surprising to see that this level is at 3000magl. What is the typical PBL height during the afternoon in winter / summer ? One could have thought that most of the sensitivity for afternoon data lies in the PBL. Of note is that it could make sense to select a vertical separation close the PBLH.

- l. 504-505: you do not explain this mapping so this is not straightforward

- l. 517, 519: this seems more frequent than induced by these sentences. Oct 2013 is an example comparable to March April 2013.

- L. 525-526 : we do not know (in the text; the figure gives the answer) if this prior baseline is a REBS analysis of the prior simulation, or the prior of the baseline state variable which should be equal, for all sites, to the REBS analysis at JFJ

- L. 527 and 543: Figure 4 does not necessary indicate a considerable improvement of the fit to the data after inversion. However, the inversion relies on corrections to the emissions and baseline at very low time resolution, so it has a limited potential for increasing the fit at high temporal resolution, which could explain why the posterior cor-

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relations hardly exceed 0.6. So would not it be more relevant to check the correlations between the model and the observations at a lower temporal resolution ?

- l. 533: you have a major part of North Western and Central Europe in your domain

- l 536: I don't really see it at JFJ

- l. 562-563: I'm not sure to understand what indicates the link between the correlation and "general flow to the site", and the link between the STD and the local processes

- l. 585: this study uses a higher resolution modeling so the scores should be better; but it also depends on the type of sites where these scores are derived (this study focuses on a complex area)

- l. 603: - 605: putting these 2 sentences together is quite confusing. In one hand you discuss the theoretical uncertainty reductions. On the other hand you have an estimate based on sensitivity studies exploring sources of errors ignored by the theoretical computation (here the uncertainty in the particle release height). Mixing these two numbers (you say "additional uncertainty range") is a bit confusing.

- L. 640, 665, 697.... the maps of posterior emissions are systematically stated to be "similar" to that of the base inversion. It is sometimes difficult to assess the level of similarity since the supplementary material does not show these maps (it already shows a lot of material). But the maps of corrections to the prior emissions by the inversion sometimes strongly differ regionally (such as S5c), S8c) S17c) vs. 2c)). The uncertainties in the inverted emissions for some specific areas may thus need to be more emphasized in the discussion sections when analyzing the spatial distribution of the emissions.

- l. 647-648: as previously discussed, the low temporal scale of the state vector in the base inversion limits the potential for increasing the correlation to 3-hourly data. extKF correcting the emissions at a much (which one ?) higher temporal resolution, it definitely has a higher potential for increasing this correlation.

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-l. 657-658: clarify and explain it earlier when presenting the extKF in section 2.5.3

- l. 678: this is not straightforward; the theoretical computation of A can account for the model performance only through the set-up of R. Then this discussion should rather be based on the comparisons of the estimates of R for COSMO vs. ECMWF.

- l. 681-684 the link between smaller posterior emissions and the diffusivity is not fully straightforward and could be better explicited.

- l. 705-708: the high corrections and uncertainty reductions for urban centers should not be too surprising: since the prior uncertainty is proportional the prior map of the emissions, and since in EDGAR urban emissions are high, the inversion will naturally apply large corrections and derive large uncertainty reductions for urban centers when using EDGAR as a prior. Does it yield urban emissions that are similar to that when using MAIOLICA as a prior ?

- l. 712: rewrite "model/observation pairs of one site" ?

- l. 748-750: the text is quite confusing and could be improved. Discussing the influence areas of the different sites and linking it to specific patterns of the corrections could feed this discussion. It raises some doubts regarding the corrections in the North East of Switzerland in the base inversion (see the general comment about it).

- l. 755: it will be critical to strongly support the assumption that the corrections driven by GIM are erroneous since biased by a high signature of the local emissions at this site. Otherwise one could also think that the "shadowing effect" impacted the results when removing GIM and that the best estimate of the national emissions from the inversions should be obtained when using all the data.

- l. 765: it is a dangerous discussion; it sounds like paradoxical to analyze the theoretical positive uncertainty reduction brought by GIM and FRU and to use it to demonstrate that they only have a local footprint, while it was stated earlier that assimilating these data increase significantly the errors on national budgets

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- l 770-774 and 995-999: this assumes that the best inversion case is the base inversion, which is contradicted somewhere else. Having similar results with one site only, despite the "shadowing effects" that have been mentioned earlier, is actually a bit preoccupying. You may have to be more careful about such a discussion. Figure 5 was already showing the dominant role of BEO. But on principle, other sites should have been necessary to ensure that the incoming ("baseline") CH4 from remote areas is well constrained and that gradients between BEO and these sites could be used to constrain the fluxes in the inversion domain without high attribution uncertainties.

- Table 5 and line 815: why don't you rather look at the inventory for the year 2013 ? your inversions apply to 2013; can you systematically use the same denomination for the SGHGI / FOEN / national inventory ?

- l. 787 : these expectations highly depend on the definition of the baseline. See the general discussion about it.

- l 800-804: such a comparison is a bit unadapted; their problem sounds like too different from yours

- l 825-840: having optimistic theoretical posterior uncertainties is not surprising since they rely on assumptions that the errors have unbiased and gaussian distributions, and that the set-up of the error covariances in the inversion system is perfect. When using the ML method, the analytical computation of the prior and obs/model variances rely on such assumptions and on the optimization of a few parameters, which correspond to a simple approximation of the actual errors. l. 837-838 are unclear.

- l. 845-846: please clarify what you mean by "referred"

- l 865: does the inversion yield such a seasonal variations for all agricultural areas or are there large areas where this does not apply ?

- in the abstract and at line 969 you should remind that the value you provide from SGHGI is for 2012

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- l. 993: I missed the link with the biosphere CO2 fluxes; the problem of the inversion of CO2 bio fluxes is quite different from that of inverting CH4 emissions and such a link is rather weak.

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