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Interactive comment on "Inverse modelling of European CH_4 emissions during 2006–2012 using different inverse models and reassessed atmospheric observations" *by* Peter Bergamaschi et al.

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Summary/General comments:

The manuscript presents 'top-down' optimized methane emissions for Europe for the 2006-2012 time period. A new, harmonized 18 site-monitoring network is used with seven inverse models and four experiments. Optimized emissions are reported (and are overall consistent between top-down and bottom-up), biases are assessed using aircraft data, and the inference of a non-negligible wetland source is intimated. Overall

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Discussion paper

it is interesting and important work to pursue. It is not easy to use this many different model/inverse approaches to one regional question, and this can potentially provide substantially more information and understanding for how to best quantify fluxes with atmospheric observations. This paper is well-placed in ACP.

We thank the reviewer for the very positive overall evaluation of our study.

However, there are a couple important gaps that need to be addressed before I can recommend publication. Most importantly, the description of different models and inverse methods is somewhat lacking, this should be a central element of this work, and this needs to be improved before I can recommend publication

The inverse modelling system are described in the supplementary material, summarizing the main elements of each system. Furthermore, all seven inverse models are described comprehensively in separate specific papers. Nevertheless, we will include some further details in the description of the models.

Major comments:

Models/Inverse methods: There is limited discussion of the different models, and specifically, of the inverse methodology being employed by each model. I understand much of this is referenced to various previous publications, and the supplement does go through each model independently, but it is important for the reader to see more comparative details in this manuscript to be able to understand the differences between models/inversions and possible nuanced causes. A succinct but clear description in its own section of the different inverse approaches used and the subtle "expert-user" choices made to define the inversion would be essential. For example the prior uncertainties and correlations lengths, which are defined differently in the different inversions, could be rather impactful on the results. How were these different priors chosen, and how important is this choice? The authors have conducted multiple experiments – they need to better convey to the reader the differences between the inversions and experiments so we can better assess the meaning of similar/different results. In many ways *this could be one of the biggest contributions of this paper.*

The specific settings of the individual inverse models are indeed largely "expert-user" choices. For many models the sensitivity of derived emissions on these settings were investigated in more detail (and described in the papers of the individual inverse modelling systems). E.g. for TM5-4DVAR different spatial correlation lengths (between 100 and 300 km) were analyzed [Bergamaschi et al., 2010], showing an overall only very small impact on the derived emissions. In the present study, the philosophy was to prescribe only the basic settings for the inversions, such as a priori emission inventories, observational data sets, and inversion time period. The main objective of this study is to use the model ensemble to provide more realistic overall uncertainty estimates (from the range of the inverse models), rather than investigating the sensitivity of individual inversion results on specific settings of the individual models. Given the large fundamental differences of the different inverse models (e.g. grid based inversion in TM5-4DVAR compared to optimization of larger pre-defined larger regions and different land-ecosystem types in the TM5-CTE (ensemble Kalman filter), it would not be possible to apply fully consistent settings in the different models. The different inversions of this study investigate the impact of the different sets of stations and the use of 'a priori' information. The different settings for the 4 inversion experiments are summarized in Table 2 and described in section 3.1.

Sensitivity of network to domain: Western Europe has the highest density of observation sites, and measurement density (and sensitivity to emissions) falls off rapidly in other regions of Europe. Given this, how appropriate is it to lump the entirety of the domain together? I'd like to see a little more discussion of the sensitivity of the network and therefore dependence of prior/assumptions in some of the domains. Another way to consider this question is how many regions can the network distinguish, and how do these regions compare with geopolitical domains? This impacts my next point.

Indeed the available stations are not evenly distributed across Europe, and the observational coverage is relatively sparse in southern Europe and Scandinavia. The fact that inversion S3 yields similar estimates for the emissions of Northern and Southern Europe (for most models; however lower estimates for NAME) compared to the other inversions (which include the detailed emission inventories as a priori) suggests that nevertheless the limited observations provide also some constraints on the total emissions from these sub-regions. We did not perform specific sensitivity experiments in this study, but we will include some more discussion of the network coverage (and the limited observational constraints in southern Europe) in the revised version.

Importance of wetlands: I'm not sure if from this analysis alone the authors can conclude substantial wetland source are or are not required to match observations. The largest prior wetland estimate (and seasonality) is in Northern Europe, where there are few observation points and the inverted seasonality is actually smaller than WETCHIMP models. When aggregating all of Europe together, it would appear the added emissions and seasonality from WETCHIMP is helpful in bringing bottom-up and top-down closer together – but given this point of spatial/seasonal errors in the Northern Europe domain I'm not sure this overall improvement is indicative of a better representation or coincidence where the inversion finds large seasonality in other regions of Europe where WETCHIMP models do not expect significant wetland sources. I would think the authors should tone done the statement of wetlands importance in the abstract, and also would like to see further defense of the seasonality signal observed and attribution that it must be wetlands.

Indeed the spatial distribution of wetlands in Europe in the WETCHIMP ensemble is not fully consistent with the results from the inverse models and most inverse models (except TM5-CTE) show a smaller amplitude of the seasonal variations in Northern Europe than the mean of the WETCHIMP ensemble. Nevertheless, the WETCHIMP ensemble estimates significant wetland emissions also in western / southern / eastern Europe (2.5 (0.4-5.1) Tg CH4 yr-1; see also our reply to reviewer 1) and the seasonal cycles derived by 4 models (TM5-4DVAR, TM5-CTE, TM3-STILT, and LMDZ) are broadly consistent with the range of seasonal variations of the WETCHIMP ensemble (although indeed the amplitude of the mean seasonal cycles of WETCHIMP are smaller for western / southern / eastern Europe). We fully agree that the uncertainties of wetland emissions remain very high (as directly evident from the very different spatial distributions of the individual WETCHIMP inventories (see Figure 4S)). This has been mentioned in the text, but will be further emphasized in the revised version. Also inversion S3 (which was performed without using detailed bottom-up inventories as 'a priori'), shows significant seasonal cycles in derived emissions (for EU-28 and all European subregions (but relatively small in southern Europe)), which confirms that the derived seasonal cycles are driven by the observations (and not by the a priori emissions). We also agree that uncertainties remain in the attribution of the seasonal cycle to wetlands, since some anthropogenic sources may also exhibit some (smaller) seasonal variations (see also our reply to reviewer 1). We will emphasize the caveats of our wetland hypothesis more clearly in the revised version (including the abstract).

References

Bergamaschi, P., et al., Inverse modeling of European CH4 emissions 2001-2006, J. Geophys. Res., 115(D22309), doi:10.1029/2010JD014180, 2010.

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