Re-review of "*Inverse modelling of European CH_4 emissions during 2006–2012 using different inverse models and reassessed atmospheric observations*" by Bergamaschi *et al.*

I do not find the changes between this version and the original manuscript to be compelling enough to change my original stance on this manuscript. It is not clear to this reviewer that this manuscript contributes much to the current literature.

Here were my three concerns from the original manuscript:

1. I find the wetland hypothesis wholly unconvincing
2. the methods description is poor, making it hard to gain any insight from the different inversions
3. it's not clear to this reviewer that their "novel" approach to estimate bias is actually an advancement

Point #1:
The authors have added one sentence to the abstract and one sentence to the conclusions.

Point #2:
The methods description is still poor. There is still just a single paragraph in the main text describing the inversions. It is left to the reader to guess at why the inverse models obtain, in some cases, radically different emissions. Figure 2 is a good example of this. The emissions from COMET look totally different from the others (e.g., why is there a source in Northern Poland that isn't in the prior or any other model?). Figure 2 seems to only be mentioned a single time in the manuscript (in the first paragraph of Section 4.1).

In the author's response they state: *"But this [understanding the differences between top-down emissions] is actually not the goal of this study (and would require further specific modelling experiments). The objective of this study is to use the model ensemble to provide more realistic overall uncertainty estimates (from the range of the inverse models) and to evaluate the model performance by validation against independent observations."* Was this not already done in the 2015 paper by many of the same authors (*Bergamaschi et al., "Top-down estimates of European CH_4 and N_2O emissions based on four different inverse models", ACP, 2017*)? The authors have just added in a few more models and a few more years of data.

In general, the conclusions drawn do not seem to be in-line with the analysis. For example, the conclusions of this manuscript state (final paragraph): *(2) transport models need to be further improved, including their spatial resolution and in particular the simulation of vertical mixing*, aren't some of these models finer-resolution then others and include different treatments of vertical mixing? Do these models actually perform better?

Point #3:
Regarding the 3rd point, their "novel" approach to estimate bias is not particularly useful for estimating biases (as the authors claim). The difference between simulated and measured enhancements is the term that defines the model-data mismatch in the cost-function. As I showed in my previous review, $c_{\mathrm{obs}} - c_{\mathrm{mod}} \equiv \Delta c_{\mathrm{obs}} - \Delta c_{\mathrm{mod}}$. Following this, if there were no difference between the simulated and measured enhancement then the inverse model would not deviate from the prior. Stated another way, if $\Delta c_{\mathrm{obs}} = \Delta c_{\mathrm{mod}}$ then the top-down emissions would be equal to the prior.