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Interactive comment on "Global inverse modeling of CH_4 sources and sinks: An overview of methods" *by* Sander Houweling et al.

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Response to Anonymous Referee 1

We would to thank the referee for the time and effort spent to help improve our manuscript. The structure of this document is as follows: Referee text's are in Italic font, answers are in roman, modifications to the text are in bold font.

General comments Paper reviews the current state of methane flux inverse modeling. The historical prospective is also presented. The paper is well written and can be published after rather minor revisions.

Detailed comments. Page 10 Line 11. Authors refer to Monte Carlo application of the variational approach as a method of choice for uncertainty estimates and note that it is

computationally demanding. It should be mentioned that Meirink et al (2008b), see Eq. 8, presented an analytical method for uncertainty estimates, that uses singular vectors retrieved during a single run of iterative optimization process, instead of multiple runs required in randomization approach.

This method that the referee refers to is mentioned a few sentences earlier (line 5), where we call it 'methods to approximate the Hessian of the cost function'. As explained in the text, they are problematic for OSSEs, which require uncertainty estimates at the resolution of the model grid. As mentioned in Meirink et al (2008b) the convergence of the method is scale dependent. In our experience it doesn't really work at the grid scale, which is why we mention alternative methods including the Monte Carlo method.

To make a clearer link between the method of Meirink et al (2008b) and 'methods to approximate the Hessian of the cost function' we added a reference as follows: '(i.e. the inverse of the posterior covariance matrix, **see Meirink et al, 2008b for details**)'

Page 11 Line 6. It is difficult to understand how the use of radiative transfer model in inversion in place of using retrieved profile and averaging kernel matrix would make analysis simpler. The problem of altitude dependence of observed signal, which is different between carbon dioxide and methane, is not going away after incorporating retrieval process in inversion.

The problem that is addressed here doesn't concern the altitude dependence of the signal, which indeed doesn't change, but rather the inconsistent use of a priori profiles in the retrieval and the sampling of the chemistry transport model. As discussed in Chevallier et al (2015) satellite retrievals make use of a priori constraints, which are much looser than justified by the transport model. In the coupled approach, there is only 1 a priori profile; the one that corresponds to the a priori transport model. Hence this inconsistency does not exist anymore.

Page 15, Line 5. Authors write: "Measurements of the vertical profile of CH4 may further improve the separation between surface sources and atmospheric sinks." This

appears as overstatement. As authors admit in the same paragraph, the OH sink-related gradients in troposphere are too small to measure.

This caveat in line 7, which the referee mentions here, is given to avoid what the referee is worried about; overstating the potential of vertical gradient measurements. Therefore we do not agree that we are overstating, because we do already what the referee expects us to do. Nevertheless, we'd like to mention the use of vertical profile information, because it should be investigated before we conclude that no useful constraints on OH can be derived. We do not agree that sink related gradients are too small to measure (see our answer to referee 2).

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