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Atmospheric Measurement Techniques Discussions

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Interactive comment

Interactive comment on "Bayesian atmospheric tomography for detection and quantification of methane emissions: Application to data from the 2015 Ginninderra release experiment" *by* Laura Cartwright et al.

Anonymous Referee #2

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This study investigates the performance of an inverse modelling algorithm for estimating greenhouse gas emissions from a single point source using data from a controlled release experiment. Inverse modelling methods are widely used to quantify emissions addressing a large range of scales. The advantage of the small scale investigated here is that true emission can be known, allowing a direct evaluation of the inversion performance. Usually this is not possible and the performance can only be evaluated indirectly. The outcome is rather sobering, emphasizing the difficulty to obtain reliable emission estimates despite the favorable availability of data from different types of instruments. An attempt is made to identify the most important factors limiting the performance. As explained in further detail below, it is unclear what we learn from this study that was not already known before. In part this is because the link is missing with earlier work, and how the performance that is achieved here compares with what was done in the past. It remains unclear also how well the optimized model is fitting the data and what would be needed to further improve the results. Further efforts in these directions will be needed to make this work publishable.

GENERAL COMMENTS

This study is following up on the study of Feitz et al (2018), in which not only the measurement techniques are described in detail but also different methods are used for emission quantification. That study is referenced, but it remains unclear how the method in this study relates to what was done before, and how the results compare. Besides the Ginninderra release experiment, other similar studies have been conducted in the past. To keep track of progress, and make sure the recommendations of those studies are taken into consideration it is important to make a closer connection to them and compare the performance that is achieved here.

This study arrives at the expected outcome that the performance of the inversion improves when the stability parameters of the Gaussian plume model are optimized. However, the finding that the performance of the OSSE is so much better than the results of the experiment using real data, despite using realistic settings in the OSSE, points to a significant remaining problem with the model. Given the simplicity of the Gaussian plume formulation this may not come as a surprise. Nevertheless, some further analysis of fit residuals is needed to find out what prevents the inversion from finding the right answer. Could it be as simple as the assumed wind direction or speed being wrong? I didn't find back an exact specification of the information that was used for that. How appropriate is the use of a Gaussian plume model in this experiment?

Different measurement techniques are compared, but there is very limited discussion on the best technique for inferring emissions. What would be the recommendation for monitoring leaks? It would also be an option to use data that are not used in the inversion for evaluating the optimized concentrations.

SPECIFIC COMMENTS

Page 1: Introduction section: Here I was missing some information about the specific application of inverse modelling that is studied here, among the large range of applications discussed in literature. Special for this case is the small scale of the experiment and the known location of the emission source. Usually this is not the case, raising the question for which kind of application this would be relevant (you might argue that there are easier methods to monitor emissions when the source is known).

Page 6, line 22: '... serves to scale outputs vertically ...' In the end I understood that 'vertically' referred to the y-axis in figure 2. What is meant is that Q scales the concentration enhancements. Please rephrase to make this clearer.

Page 7, line 18: I'm missing an explanation of the logic here. Please clarify the reason for quantifying the statistics of 1/U.

Page 9, line 7: '... graphically in Fig. 3..." Here the dependent variables of the optimization are introduced, but not explained. Here the meaning of tau and the omega's should be explained explicitly, and that in addition to these variables Q is estimated from the data.

Page 10, line 2: '... and one to the stability class ...'. The model error contribution to e_i is not just the stability class.

Page 10, line 3: "(variance)" does not correspond to "(increases)". This is a good example why this grammar style is better avoided.

Page 10: What justifies a windspeed independent error for windspeeds > 1 m/s?

Page 11, line 7: Is a 'point mass at zero' not just a 'point emission of zero'. If so then please change to avoid confusion.

Page 11, line 21: What is the relevance of the distribution of the inverse of the square root of the precision parameter? For a precision it would be straightforward to relate it to the numbers that are given in the same sentence. However, what is done here is more complicated for a reason that I don't see.

Page 11, line 7: 'While addressing . . . zero emission rate' Looking at equation 4, I don't see why it precludes zero as it is in the interval where the half-normal prior applies.

Page 14, figure 5: It is explained in the text why the method precludes zero as a solution to the inverse problem (see my earlier comment on that). However, I had expected the estimates to be much closer to zero when emissions are switched off. The likely reason is not the zero condition, but the accuracy at which the background concentration is accounted for. It makes me wonder why the background is not fitted as an additional unknown parameter.

Page 15, line 23: why are wind speed and direction assumed to be known?

TECHNICAL CORRECTIONS

Page 3, line 6: 'Houweling' instead of 'Houwelling'

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