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Discussions

Interactive comment on "Methane emissions from dairies in the Los Angeles Basin" *by* Camille Viatte et al.

Anonymous Referee #1

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There is a growing interest in the use of remote sensing measurements for estimating strong local sources of methane. Research is needed to investigate methods for doing this, and their performance should be assessed using different types of measurements. This study has the right state-of-to-art ingredients to do this: A large local source of methane, total column as well is in in situ measurements and a model capable of resolving the relevant spatiotemporal scales. In the end, however, I'm left with the feeling that it is more important to the authors to convince the reader that this can be done, then to objectively assess the role of different factors influencing the outcome. As a result, and as I will explain further below, differences from the prior expectation are explained without taking proper account of methodological limitations. In addition, I had difficulty to understand some aspects of the method, which should have been explained better. Revisions in these directions are needed to make this study suitable

for publication.

GENERAL POINTS

It is known that the short-term regional scale variability in total column CH4 is dominated by local sources is well as the dynamics of weather systems. The model system that has been described takes the first into account, but not the second. I mean, it represents the weather but not the corresponding regional-scale patterns in XCH4. I was looking for ways in which the boundary conditions of CH4 where taken into account, but didn't find any. As a result, the fluxes that are derived may well account for unaccounted variations at the domain boundary. In Chen et al. an upwind-downwind approach was taken, which might indeed work under ideal conditions, with a well defined wind direction. As can be seen in Figure 4, this logic falls apart for low wind speeds. It would have been instructive to plot the zero line in this figure. Unlike the 24th of January, the gradient with respect to Harvard 2 is going everywhere, most notably on the 15th. Therefore I don't understand the role of Harvard 2 is a background on days with a low wind speed and doubt that it can be used that way. The authors may argue that variations in the background may be less important on days with lower wind speed, but this could en should then be demonstrated.

Before the WRF-LEF model is used to fit the FTS data it should be demonstrated that it has a reasonable skill in simulating the observed variability. The fit in S2 doesn't look great (what are the R2 values?), which makes me wonder about the comparison with the prior model. In this context it would be very useful to compare the use of WRF-LES with the use of WRF alone. What do we gain using LES? The results may not look great, but we'd learn about where the remaining problems are.

In line 370 it is mentioned that the modelled wind speed has an error of 'only' 1 m/s increasing further in the PBL. However, on the low wind-speed days this is a very substantial fraction of the total wind speed (according to Figure 1). What is the sensitivity of the emission estimation to errors in the modelling of wind speed and direction? I'd like again to stress that it is important that we learn about what is critical in this approach. The size of the errors in wind speed and direction warrant closer inspection and a test of the potential impact.

It is clear to me why the mass balance approach is not used on the 15th and 16th of January. However, it is not clear why the WRF-LES method is not used on the 24th. This would provide a good opportunity to make a direct comparison between the two methods. It may not be the best case to demonstrate the added value of WRF LES, but as a general consistency check to include this comparison is nevertheless necessary.

Figures 4 and 5 confirm my worry about the emission pattern that is shown in Figure 12 and I wonder why it doesn't receive more attention. The pattern correlates with the configuration of the measurement 'network'. For an inversion the easiest way to fit the data is to modify the emissions in the same grid box as where the measurements are. It may either be that the measurements are very locally influenced, in which case they are not really representative of the domain that is being optimized. Otherwise the inversion may be trying to fit uncertain variables that are not part of the state vector, such as boundary conditions (see my earlier comment) or the emission distribution within 2x2km2 regions. With in situ measurements at LANL as high as 30 ppm CH4, this may well be an important factor. To me this sounds certainly like a more relevant factor to mention than the emission of a 1 cow per year power plant.

It was not clear to me why the simulated annealing approach was chosen. Since the inversion problem is linear, I don't see why its solution should be any different from the Bayesian method. If I understand well, the method was introduced to deal with the difficulty to define the B matrix. But how does simulated annealing solve this problem? Is it just efficiency at which different options for the B matrix can be tried out? If the greens functions are available, then I wonder how simulated annealing could be faster than a Bayesian inversion. This should be explained better.

SPECIFIC COMMENTS

Line 98: 'on a high-wind data'. What does this mean? Why is Chen 2016 not included in Table 1?

Line 117: In this sub section I am missing numbers for the estimated accuracy of the mobile column measurements. How realistic are the fits to the data derived later on in the light of these uncertainties?

Line 196: 'one way nested grids': S1 should provide further information about nesting. For CH4 the logical on-way nesting is from the small to the large domain. For meteorology, however, the reverse seems true. Some further explanation is needed.

Line 207: To avoid confusion about optimizing methane fluxes and optimizing meteorology it should be stated more clearly that 'data assimilation' is referring to the latter here. Line 255: What does the random draw refer to: the starting point of simulated annealing, the first guess, a random modification of the prior uncertainty? Does this modify the actual cost function that is optimized?

Line 257: 'Mean absolute error' This approach favors the setup that gives the largest freedom to the prior fluxes. This need not be the best solution, or maybe I do not understand what is meant here (see the previous point).

Line 290: 'C' io 'SC'?

Line 373: 'More relevant to this study' It is unclear why this should be the case. The difference between the two metrics represents a cancellation of wind speeds in calculating the mean. Such errors could still affect the estimated emissions, e.g. when winds with cancelling errors come from different directions.

Line 458: It is clear that the measurements in Figure 11 represent ruminant emissions. However, these samples are not representative of the air masses that are sampled with the FTS instruments. Therefore this result does not refer to the origin of the enhancements in total column CH4 discussed earlier. Confusion should be avoided on this point.

Line 579: 'The main advantage ...' This statement should be supported by evidence or a reference to other work.

Figure 1: The wind roses do not add up to 100%.

Figure 5: Please repeat that anomalies are relative to Harvard 2.

Figure 6: It should be made clear that these errors refer to errors in WRF-LES simulated wind speed and direction.

Figure 7: Mention the minimum WMO threshold value.

S1: How about the top boundary in the LES part of the domain?

TECHNICAL CORRECTIONS

Line 288: 'V is' io 'Vis'

Line 337: area of Chino in m2

Line 353: 'has' io 'have weaknesses'

Figure 8: Information on axes parameters and units should be given in the figures themselves instead of the caption. Please also add labels to the rows (as is done for columns)

Figure 9: Figure axes and legend without labels.

Title S1: 'models' io 'modes' Figures S4 and S5: Text is too small to read.

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