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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimating the size of a methane emission point-source at different scales: from local to landscape" *by* Stuart N. Riddick et al.

Anonymous Referee #2

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General comments

This is an interesting paper discussing observations of atmospheric methane at different scales (near-source, middle-distance, and landscape), and their value for quantifying a methane emission point-source (using specialized estimation methods). My main problem in this paper is the conclusion that the landscape inverse modeling approach can be used to identify point sources. The inversion method lacks details and the discussion is somewhat superficial. I think OSSEs would be required to determine the ability of observations at the landscape scale to constrain emission hotspots.

Specific comments

P6, L9-10: "The standard deviation of the lateral (σy, m) and vertical (σz, m) mixing ratio distribution are calculated from the stability class of the air (Pasquill, 1974)." So

what are the values for the standard deviation used in this paper?

P7, L19: "This allows for any potential bias due to highly uncertain observations to be accounted for." I don't see how the bias would be accounted for.

P9, L14-15: "A statistical filtering technique separated methane mixing ratios at each site into. . ." What is this statistical filtering?

P9, L16: Why "18th percentile"? Why not 10th or 25th?

P9, L21: "For a more detailed description of the measurement sites and the InTEM setup please refer to Connors et al. (in prep)." I think more details about the InTEM setup should be given. For example, what prior constraints or regularization do you use? This is crucial for an inversion.

P12, L4: ". . .using near-source measurements are 453 kg hr-1 in June/July 2015. . ." I thought the near-source measurements cover only two days? This looks like two-month data.

P12, L15-20: Table 4 shows the lowest emissions month is in April (111 kg/hr). I am not very convinced that seasonality is due to temperature. Does stability class in the Gaussian plume approach play a role?

P12, L33-34: I am not convinced by this conclusion. See my general comments.

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