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Interactive comment on "The detection of carbon dioxide leaks using quasi-tomographic laser absorption spectroscopy measurements in variable wind" *by* Z. H. Levine et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper describes a model-based retrieval approach for the detection of gas leaks based on laser absorption spectroscopic measurements. It describes the methodology and then presents several simulations and first results using actual measurements.

The paper is well written and its scope fits well into AMT. However, the paper has some shortcomings listed below, which must be addressed before publication. The principles of the used approach are well described, however, the relevant details on how the described likelihood is optimised is lacking. The simulation studies exhibit several artefacts, which should be discussed in the textual description. Most important,

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the results based on the experimental data are not validated against an independent measurement.

SPECIFIC COMMENTS

page 12304, line 5ff:
The notation of functions could be clearer. It is not clear why "greek L" is a function of n while L is a function of f and n. It might be better to introduce μ as function of f earlier and than describe L already as conditional probability. It would also be helpful to defined greek P(f|n) = P(n|f) * P(f) / P(n) and derive the cost function greek L to maximise. It is mentioned in the text that a flat a priori distribution is assumed, but the implication P(f) = const could be made clearer.

page 12307, line 12:
You propose to validate your approach by evaluating experimental measurements. It is highly unusual that you first present the evaluation of actual data and then proceed with a simulation study. Reversing the order of presentation would be very helpful, as the first reviewer pointed out. With experimental data, you may only demonstrate that your method does not work, i.e. you may falsify things. So without a controlled experiment (where, e.g., CO2 is released artificially at a known position) or at least a secondary, reliable source of information about the point and strength of leakage or at least information on whether a leak was present at all you demonstrated not much.

In that line, information about the actual source is missing from this paper, which is necessary to validate that it worked with actual measurement data. The publication by Dobler et al. suggests the use of an artificial subsurface source, which in contrast to a natural source should be perfectly known. If such a source was also used here, please indicate its characteristics and location.

Certainly, the presented Figures 3, 4, and 5 should be discussed in more detail and the peculiarities should be explained (see also comments to Fig. 4 in that regard).

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page 12308, line 24:
Please explain the cause of the cell-like structure and why it goes away at 10 m altitude. The distribution of CO_2 at those altitudes and varying the wind speeds might be helpful here.

MINOR REMARKS

page 12303, line 8:
depreciated? perhaps deprecated

page 12305, line 3:
How are the absorption cross sections determined? Are temperature- and pressure dependence negligible?

page 12305, line 7:
How is the contribution to the derivative relevant? Constant terms — or better terms not depending on f — do not influence the likelihood and can thus be neglected in the optimisation.

page 12305, line 18:
Is f_0 really unique? Usually such inverse problems are ill-posed, which often imply the non-uniqueness of the solution unless some kind of regularisation is employed.

page 12306, line 21:
What about the derivatives with respect to x/y? What algorithm is used for the optimisation, e.g. steepest descent, Gauss-Newton, L-BFGS...?

[*page 12308, line 26:*
The methodical section describes that you alternately optimise for Q and position. This suggests that no optimisation for position is performed, but that just Q would be optimised for all positions in a semi-brute force manner. Please elaborate the employed methods in the method section.

page 12309, lines 12ff:

This part about your minimisation should be moved to the method section.

page 12309, lines 17ff:
For a non-linear problem, the solution of such an algorithm will usually only find local minima such that the choice of the starting value for the position of the leak determines the solution. The topology of the likelihood for the 1-m case certainly suggest that Newton-type optimisers will have a hard time here and that techniques like simulated annealing or basin-hopping might be used; however these do not guarantee to find the optimum everytime. It would be advantageous if you could smoothen your topology in some manner. The paper does not assume a discretisation so I assume that every-thing is done analytically, which certainly does not help with the well-posedness of the problem. Did you investigate into regularisation techniques such as discretisation to make the problem more well-posed?

page 12321, Fig. 3+4:
Why are there local minima along the line-of-sights of the measurements? Fig. 5 looks more like one would expect the likelihood for one measurement and the likelihood for two such structures pointing in different directions stemming from measurements for different wind directions should — naively — look like the multiplication thereof. How does the superposition of many measurements result in those cells that seem to relate to the light path pf the measurements? Further, why are there these secondary maxima in Fig. 3 at x=-190? Fig. 5 does not show a maximum there. Faulty measurements, or measurements where the model assumptions do not hold might be the cause of this? Could you please explain this result?

page 12323, Fig. 6
Interestingly this figure resembles quite a lot Figure. 5 that was generated by a single scan. Is the elongated shape determined by the position of the measurements or by the prevalent wind speeds?

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 12297, 2015.