

Interactive
Comment

***Interactive comment on* “Estimates of tropical bromoform emissions using an inversion method” by M. J. Ashfold et al.**

Anonymous Referee #2

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This article describes a bromoform emission inversion study carried out using observations from two measurement sites in North Eastern Borneo. The material is definitely within the scope of ACP and the SHIVA special issue. The article is of scientific interest, they present several new findings, the conclusions are well supported, and the methodology also represents an advancement within the field of VLSL emissions. To my knowledge, they perform the first reported emission inversion for VLSL emissions that uses a mathematical technique to find the optimal emission distribution. The authors comprehensively discuss the limitations and demonstrate their effect where possible. They conclude that single measurement sites can only constrain emissions within a limited geographical region dependent on the prevailing meteorology, which has significant implications for other emission inversion studies and for studies that

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evaluate the emissions. They also clearly demonstrate the how the limited domain affects their analysis. I recommend that the article be published in ACP after having dealt with some minor comments.

General comments

I think the authors have done a good job of discussing the different methodological decisions, and have made a good attempt to demonstrate the abilities of their inversion method in section 4. However, there are some further possible methodological choices that should at the very least be mentioned as possibilities. In addition to the use of an a priori it is possible to use an a priori constraint matrix in the cost function to weight the perceived quality of the a priori. The authors could have for instance used an a priori but given it a very low weighting based on the perceived poor quality of the current emission inventories and bottom-up knowledge. It would be good to see this possibility discussed. The authors make a good attempt to explore the effects of observational uncertainties. However, another methodological approach is to include an observational constraint matrix in the cost function that acts to weight the observations in the cost function. Combined, the a priori and observational constraint matrices act to smooth the topography of the cost function thus acting to limit the number of local minima and therefore to reduce the number of local minima. This has the effect of moderating the inversion solution in order to remove its sensitivity to observational noise and to force a solution at the global minimum. This should be discussed a little bit. In fact, the authors, performed a series of different inversions with different realisations of noise on the observations, so in some way they can demonstrate the solution insensitivity to observational noise. However, I could not find a mention of the resulting standard deviation across all 25 solutions that would go some way to demonstrating that the solution is stable and insensitive to noise. If this standard deviation was added with the corresponding discussion it would strengthen the conclusions of the paper.

Specific comments

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Page 4, lines 5-10. It might help to describe the range of global emission estimates for CHBr₃. It might also help to show the range of estimates for the tropical region. The tropics are defined differently in many of the studies, but it would give the reader an idea of the uncertainties.

Page 5, lines 15-17. It might be an idea to mention the SHIVA cruise and data. I realise you have been working on this prior to the release of the SHIVA data and it is not practical to re-do the analysis with the SHIVA data, but it should be mentioned that it exists. You can then mention that those observations were not obtained during your analysis period.

Page 10 first paragraph. It might be clearer to readers if you refer to the modelled concentration as a simulated concentration at the measurement site. Also, which measurement site is this test for?

Page 10, line 13. Perhaps change "We will focus on..." to "We will only solve for...". I assume that this is what you are trying to say. Otherwise it is not clear if you are only showing results from the finest grids, or if you are only solving for the two finest grids.

Page 10, 2nd paragraph. In the inversions where you do not solve for the coarsest grids, i.e. $>4 \times 4$, what do you use to simulate the contribution to the observed concentrations at the measurement sites from those coarser grids? How does this interact with the background that you choose for air masses older than 12 days described in the next section? These points could perhaps be made a bit clearer.

Section 4. I really like the inclusion of this analysis, and I think it strengthens the conclusions in the paper. It might also be interesting to see what happens to the outcome of this analysis if the observations are degraded by noise. I realise that you perform some sensitivity tests on this in the real inversion, but it would be useful to see the point at which the inversion breaks down. The authors could even just discuss these tests in the text rather than adding another figure.

Section 5. In addition to table 2, it might be useful to add another table which summarises the setup used in each experiment (A through F). Further, the idea that there are specific inversion scenarios is first mentioned in this section. It would be better to have a summary at the end of section 3.2 that describes each of the scenarios relative to the uncertainties discussed. This would also remove the need to have as many methodological details introduced for the first time in section 5, which seems to be more about the results.

Section 5.2, 2nd para. Please can you add a more precise explanation of how the emissions were scaled? For instance, do you only scale according to the difference in ocean area in the tropical band 20S-20N? Or do you scale for the total area, land and sea?

Section 5.2. I think the attempt to up-scale the emissions to the entirety of the tropical band is worthwhile. However, I think that there should be some further mention of the limitations of this approach. Assuming that the truly oceanic emissions looked at here are associated with biological productivity, it is possible that the oceanic emissions in the region to the NE and SE of the measurement sites display an unrepresentatively large productivity compared to the open waters of the Pacific, for instance. The underlying driver of these differences are linked to the availability of nutrients in the surface waters (assuming there is a link to plankton). Since the oceanic areas close to Borneo are relatively productive, the up-scaled estimates may represent more of an upper bound. I would urge the authors to examine this issue and try to discuss it. This will therefore have some influence on the final sentence in the abstract.

Section 6. Apart from mentioning sea weeds, is it possible for the authors to include any further discussion of other causes of the derived emission distribution, i.e., are there any correlations to the presence of shallow ocean shelves, areas of upwelling, or areas close to outflowing rivers. One might expect such environments to have higher biological productivity compared to the open ocean.

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Technical comments

Page 15, line 11. Two instances of the.

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