

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

Anonymous Referee #1

Received and published: 30 August 2013

The paper "Estimates of tropical bromoform emissions using an inversion method" is a novel approach to derive emission estimates from a short-lived compound, emitted from the oceans in significant quantities, but with large uncertainty in magnitude and distribution of the emissions. The authors present back-trajectory calculations, dilution matrixes and several possible solutions for an inversion method for new emission estimates. It is well justified to use this approach, and discuss its benefits and limitations. The paper is well written and the modeling appears profound. The authors discuss their method and uncertainties and possible improvements mostly in detail. Some terminology which the authors have used or created should be explained a little more precisely and the inversion method should also be described a little more in detail. The authors shall refine some statements and explain at some places in the manuscript a little bit

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more of the background they used to obtain the results, which should be discussed a little more. It did not become clear why the authors did some experiments – appear redundant- or shall be better explained. The differences in the solutions (25) for one study case where not shown and the reader wonders how large they are. The question is if the differences can be somehow shown? In other words how robust is such a mean emission distribution estimate for one case study? The authors should discuss if this new method improves the information about the “real” regional emission distribution of CHBr₃ and add a little discussion about the possibility to pin down emission sites for CHBr₃ with this method. The abstract is saying ...obtains detailed estimates of the distribution, which is not followed in the final discussion. The discussion is missing a little bit of concluding highlights of this new approach and study - which are somehow buried in the text- under possible improvements. Thus I suggest to highlight the highlights and point out what is new and brilliant about this approach, before suggesting the possible improvements, which may even strengthen the paper. All in all it is a good paper already which needs minor revisions in the text and some details, which are addressed in the specific comments. Specific comments:

Page 20466 Line7: include Ziska et al., 2013 (as bottom-up approach) in the references with Liang and Ordonez Page 20467 Line 19: why did you choose 3.51 ppt as threshold? Page 20469 Line 19-22: The explanation of the generation of the “dilution matrix” appears insufficient. Since the generation of the matrix is a crucial step in the method, please describe it in a little more detail (according to Manning 2011). Page 20470 Line 6: Here also you naturally assume, that the reader can transform easily between dilution values and the number of trajectories in a certain grid box. Explain the connection between the number of trajectories and the dilution value –at least in one sentence or above. Page 20470 Line 10: What does less dilution information mean?- You have a factor- why is there less information, about what? Please revise wording or sentence. Actually, I do not understand what you want to say here with this sentence?- Possible erase or move, or revise? Page 20471 Line 8: Aren't they also uniformly distributed?- constant in space?- or do you put different emissions in each

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grid cell? Page 20471 Line 17: the content after the “ , “ is redundant since natural, or do you want to say something different ..please rephrase then. Page 20471 Line 20: Does this help for a variable compound such as CHBr3, since then you also group the emissions to a non existing mean and therewith loose information? Page 20472 Line 4: concentration in what? atmosphere, grid box, receptor site? Page 20474 Line 10ff: could you please include, which observations (3h means?) and modeled concentrations you use? Which number is behind the n? Page 20474 Line 21: ..it is known that the emissions at coastlines are more significant..and you apply this later..?! how does this fit to your statement? Page 20475 Line 23: are the 25 times independent of each other or did you make them subsequently? please clarify Page 20478 Line 14: I still do not completely understand, how the model calculates the emissions. Please explain a little more detail with equation 2. Individual solutions must yield very differently distributed emissions for each of the considered cases? Is it worth showing this? Page 20478 Line 16: How did you model these concentrations ? forward trajectories?? And is it not expected that they yield the same concentration, since they use the same input as the inversion? Page 20480 Line 16 to 22: I do not understand also in connection with table three, what you did here and what you like to show here. E.g. What is your choice of grid cells? Can you clarify this also in the text, what are the consequences of this investigation? Page 20482 Line 4 to 8: Why do you need to consider fine versus 4x4 and for what region? The whole issue with the choice of the size of grid cells for what region and what you want to show here shall be clarified throughout the text. Page 20483 Line 1: May be it would at least be nice to see what difference you got for the coastal area? Page 20483 Line 16 and 17: This is a very coarse statement- and there are planktonic sources in the open ocean- while algae are assumed with coastal macro algae and Zulu Sea is not necessarily open ocean. And didn't we expect coastal region to deliver more? Please refine. Page 20484 Line 1 to 8: Didn't Hossaini also show that the low Ziska emissions for CHBr3 matched best in the tropics? Page 20484 Line 11 to 12: How are the emissions in the receptor grid box resolved in your inversion model? This must cause a large error if they are not included? Please discuss, how

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relevant this is for the short-lived compound. Page 20485 Line 20: This could be elaborated a bit more – in the chapters before and here please try to specify this statement. Numbers in Figure 4 and Table 1 for accumulated emission from the different resolution grid cells don't match , % in Figure 4 for 1x1 is 44 %, while in Table 1 it is only 2% .(or is Egrid not for all 1x1 cells?, then please rephrase clearer. Wouldn't it be sufficient to define “ fine grid” cells always the same way..not: as in the black line, or red and orange, just e.g. fine (1x1 plus 2x2)? Too many definitions under the pictures for the same thing. . . Figure 4: what is accumulative emission sensitivity? Table 3: I do not see what this table is aiming at- in addition I do not understand what you mean with maximum grid size and total emission from grid cells (is it all cells or per cell?) area –averaged flux per cell? Can you choose a different unit to increase the numbers?

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 20463, 2013.

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