Geosci. Model Dev. Discuss., 8, C3141–C3148, 2015
www.geosci-model-dev-discuss.net/8/C3141/2015/

Geoscientific Model Development Discussions
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Interactive comment on "Inverse transport modeling of volcanic sulfur dioxide emissions using large-scale ensemble simulations" *by* Y. Heng et al.

Anonymous Referee #2

Received and published: 2 December 2015

Review of

"Inverse transport modeling of volcanic SO2 emissions" Y. Heng, L. Hoffmann, S. Griessbach, T. Rößler, and O. Stein

Overview:

The paper describes a method to construct emission height and rate of SO2 emitted from volcanic eruptions. The method juxtaposes large-scale ensemble simulations of a lagrangigan trajectory model and satellite retrieved SO2 indizies (AIRS) to obtain these parameters in an iterative way. The method is applied to the 2011 Nabro eruption. The final forward model simulation using these parameters is evaluated with AIRS SO2 and

compared with imagery from MVIRI IR and WV and aerosol profiles from CALIPO and MIPAS.

General remarks:

Estimating volcanic emissions from satellite retrievals of ash or SO2 is an important scientific task. The estimates are usually rather uncertain because of the limitations of the satellite retrievals and uncertainties in the transport simulation. Different approaches have been applied in the past and the one presented in this paper might be an interesting new approach.

However, the paper in its present form cannot convince the reader of the merits of the method and, more importantly, the validity of the results because of the following main points.

(i) A major omission is that the results (emission parameters) are not compared with other studies presenting SO2 emission (flux and height) estimate for the Nabro such as Theys et al. (2013).

(ii) Although SO2 and ash plumes sometimes coincide, but they do often not so. The evaluation of the SO2 emission heights with aerosol retrievals (CALIPO and MIPAS) as well as the imagery is therefore questionable. Another SO2 retrieval (IASI, GOME-2, OMI) would have been the best choice for the validation of the results with independent observations.

(iii) The choice of the AIRS SO2 index (SI) data for emission parameter estimate needs to be motivated as it might not be the most suited data set for the inversion.

(iv) The basic methodology needs to better be explained and case specific fine tuning should better be avoided. It requires clarification what additional information – apart from the AIRS SI and the ERAinterim meteorological data – was fed into the inversion approach. It appears that the method does not actually provide a quantification of the emission rates. Also, the sensitivity to ad-hoc choices such threshold for plume presence in model and observations are not sufficiently discussed.

(v) The model does not seem to include any SO_2 loss processes (chemical conversion, deposition). The literature suggest a lifetime of about one to two a weeks. This will have an impact on emission parameter estimates.

A positive aspect of the paper is the methodology the authors apply to evaluate the match between model and observations by using contingency tables. However, only the binary match (yes/no) w.r.t location seems to be tested. The approach should be developed further as the evaluation of volcanic plumes simulations can often not be done justice with simpler approaches.

Specific remarks:

Abstract

Please give numbers how much the CSI improved from the constant scenario to the your best estimate (max 32.3% - >41.2 and average 8.1%. ->16.6 %,)

P 9105

L 10: Using the SO_2 as proxy for ash and vice versa (as done in this paper) has to be done with caution. There are also examples of the separation of the two (Moxnes, et al. 2014 for Grimsvoetn). This point is of great importance for this paper as ash observations are used to validate the SO_2 emissions.

L 17: Your collaboration in research activities is not of importance for the paper. Please omit.

L 20: Please discuss the limitation of the satellite observations of volcanic SO_2 in more detail. Mention the consequences for the source inversions but also for the evaluation of the forward model runs.

L 25: Please mentioned also the NAME model, which is used by the UK met-office for ash plume forecasts.

P 9106

L 11: This does not make sense. Inverse techniques also use satellite data. You should have lists for the used observations and the applied techniques.

L 22: Please explain “Tikhonov-type regularization” or provide reference.

L22: “objective function” , please clarify

P 9107

L 1: Why only “nadir” and not limb sounders, the latter could provide better profiles

L2: Please explain the main idea of “sequential importance resampling”

L8: please explain the typical resolution of the discretization

L8: please explain why this needs massive parallel computing, what is your definition of “massive”

L8: please give approximate number of calculation required

L 14: Your method seems to have communalities with Flemming and Innness (2013) as they also use an ensemble of test tracers plumes and their match with observations to determine the emission parameters.

L18: please motivate the choice of the AIRS SO_2 data. There other data sets e.g. from UV instruments such as OMI, GOME-2 or IR like IASI

L 20: please mention your evaluation with CALIPO and MIPAS aerosols

P 9108

L 6ff: please provide reference for mid-point method, and the approach to simulate diffusion (Markov model is a very general term). Why do you distinguish between “atmospheric diffusion” and “turbulent diffusion” ?

L 8: Please explain what chemical conversion processes or removal process of SO_2

are considered. If not this may have important consequences for your results.

L 9-13: Why is the detail on the parallelisation of importance here? Most of the atmospheric models require high amount of parallelism. Perhaps omit.

P 9109

L 1: Is the 6 h time resolution of the ERA-interim data good enough for trajectory calculations. Is this a limitation of your modelling?

L 20: What is the possible range of SI, please explain how a quantitative information can be obtained from this index.

P 9110

L2: How is the limitation to this height range affecting your results?

Section 2.3

The evaluation of the final SO2 forecast should use observations that represents the model result, i.e. SO2. Therefore please evaluate with SO2 retrieval from IASI or UV instruments such as GOME-2, OMI etc. You may use the CALIPO data or the imagery as secondary test for the injection height but you can not rely on them entirely.

P 9111

L11: It is not clear how this quantification of the emission flux is achieved if only the match in location is testes with CSI

L25: What is the strength of the emission pulse for each of unit simulation?

P 9112

L6: why “hidden”

L12: Please make clear at what point the observations are used

P 9113

L 9: It is not clear how you obtain the threshold of 0.1%. Is the SO2 mass the mass of the ensemble unit simulations? What is the correspondence to the observed values. Or is it just a match between yes/no etc.

L 14: 4 DU is already a strong volcanic SO2 signal. I could imagine that the choice of this threshold is important for the final outcome of your simulation. Please clarify.

P 9115

L 10: The choice of the split point seems important for the results. Is it just a heuristic choice. Please explain in more detail. How universal is a split point of 48 h. Please mention your sensitivity study in section 4.5

L 17: subdomains of the emission column?

P 9116

L 11: How does the number of Clarisse et al. (2012) compare with your estimate.

P 9117

L 4: the AIRS data are only available for the respective overpasses. Please specify which orbits (times) have been used for the for the emission update. How does the temporal resolution of the data impacts the results.

P 9117

L 15: Does this simulation use a constant and uniform (in the vertical) emission flux? If yes say so.

P 9118

L 1: Please say that you only test the match in space and not the SO2 total column value.

P 9119

L 3: But this all depends on the arbitrary choice of your split point.

L 14: It is not clear where this number comes from. How did you obtain the total SO2 burden? Please clarify.

L 20: Is the total emission obtained with product and mean rule the same or not. Please give numbers. The plot suggest different total emissions for the two cases.

P 9120

L 5: Why are they an underestimation?

L 23 Figure 7 is not clear. Why is the diagram above the imagery? Perhaps two panels are better.

P 9122

This whole discussions should perhaps be moved upward. In it is present from there are no real conclusions, which split point is best. Is it case specific etc.?

L 25 Please discuss which one is better. Are there any recommendation for the split point choice – or not. If not it is perhaps not necessary to include this section in the paper.

P 9123

L4: “equal-probability strategy” explain that this is the uniform and constant emission scenario.

L 4: Do all emission scenarios have the same total or do they differ? Please clarify.

L 7: Are the number of “false alarms” (model = yes, obs = false) and “misses” ” (model = no, obs = yes) more or less the same in the three model runs. Or does one type dominate? This would be important additional information. Please try to show also time series of these components of the CSI.

L 16: On average 16% hit is perhaps not so overwhelmingly high.

P 9123

L 8: Please clarify how the total emissions are assed and what the input data are and what your ad-hoc choices are.

L 9: Please say more clearly what the “equal probability assumption” is.

L 12: Make clear that this evaluation with the imagery is only qualitative.

L 16: Please give numbers how much the CSI improved for the three scenarios

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