

Interactive comment on “Updated SO₂ emission estimates over China using OMI/Aura observations” by Maria Elissavet Koukouli et al.

Anonymous Referee #2

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1 Summary

The manuscript presents an updated SO₂ emission inventory for China using the MEIC v.12 inventory as an a priori and generating an a posteriori inventory using OMI/Aura SO₂ observations and SO₂ profiles from the CHIMERE CTM. The updated inventory shows new source areas in SW and SE China, which do not appear in the original MEIC inventory. The results show satisfying agreement with other emission inventories.

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2 General comments

The paper is well written and all sources well referenced. The method used and the data sources are well described, however I have one specific question that I would like the authors to clarify:

In order to calculate the a posteriori emissions using the inversion methodology presented in section 3.1 the a priori emission field is multiplied by the satellite-derived SO₂ field divided by the model SO₂ field. In order to calculate the satellite-derived field from the OMI satellite observations, AMFs are calculated using an anthropogenic SO₂ profile from the IMAGES CTM. Why didn't the authors use the same SO₂ profile for the calculation of the satellite field (i.e. in the AMF calculation) AND the model SO₂ field? In this way one would exclude differences between the IMAGES and CHIMERE CTM when calculating the updated emission inventory.

3 Specific comments

Unfortunately all multiplot maps shown in the paper are far too small. This is especially the case for Fig 1,5 and 7. In order to increase the image size I would suggest to remove the lat/lon axis labels between the single maps since all show the same area. Furthermore for Fig 1, I would suggest to use a different color bar, using white as the color for zero emissions.

Abstract

In the abstract it is written that 'novel inversion techniques' are used, however a broadly used technique is used (according to the papers cited in Section 3.1) and there is no 'novel technique' presented in this manuscript. This is misleading and I would suggest

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replacing 'novel' with 'state-of-the-art' or 'broadly used'.

Introduction

- Wording: Sulphur dioxide / Sulfur dioxide – I have found both in the paper. Please use only one notation and check the paper again
- Page 2, line 17: Please name sources for hydrogen sulfide
- Page 2, line 23: What are 'scheduled biomass burning events'? Please clarify

Section 2.2

- Page 5, line 11: Are daily/monthly/fixed SO₂ profiles from the IMAGES CTM used? Please clarify
- Page 5, line 20: SO₂ algorithm flagging: What exactly is flagged? Perhaps add a short list or example.
- Page 6, line 4/5: NS₀ is not used in any equation What is meant by SCD-SCD correction? Typo: AMD precision. I guess this should be AMF precision

Section 2.3

- Page 7, line 17/ Page 9, line 29/ Fig4: There is general confusion when using the terms layer or level throughout this section. What I understood is that the model provides SO₂ vmr in ppm on nine (or eight???) levels from which SO₂ partial columns in eight (or seven??) layers can be calculated. Hence Fig 4 is not correct – you can't show the SO₂ profiles in ppb and DU on the same grid – for

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the SO₂ profile in DU the layer midpoints should be used and not the levels from the vmr

The text should be corrected accordingly:

- P.7, l 16/17: ...on nine vertical layers levels in ppb, i.e. seven vertical layers
- P.9, l 29 Fig. 4 – eight or nine levels for vmr? Please clarify!

Section 4.1

- Page 13. Line 24-26. This is not clear for me. Why did only a part of the 8414 grid cells actually provide information?
- Figure 6. One could also add the MEIC emissions for the years 2008,2010 and 2012 to the plots to get a better overview of the agreement in different years.
- Page 16, Line 16: It is unclear from the text that the increase for 2010 is wrt to the MEIC apriori inventory. Please clarify in the text

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