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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We warmly thank for referee for her/his positive take on our work and helpful comments.

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 aposteriori emissions using the inversion methodology presented in section 3.1 the apriori 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.

The reviewer is raising a very interesting suggestion which might have been possible if the satellite field calculations and the CHIMERE CTM run where performed within the same operational chains. However, the former are produced in an operational manner by BIRA whereas the latter by KNMI. The suggestion of the reviewer would hence require the reprocessing of the satellite data, which is beyond the scope of this paper.

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.

Thank you for this comment, indeed you are right. Figures 1, 5, & 7 have been updated accordingly.

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

Line re-phrased.

Introduction

• Wording: Sulphur dioxide / Sulfur dioxide – I have found both in the paper. Please use only one notation and check the paper again

Sulphur dioxide was kept as notation.

• Page 2, line 17: Please name sources for hydrogen sulfide

Line added in the relevant section.

• Page 2, line 23: What are 'scheduled biomass burning events'? Please clarify

Basically, the burning of croplands in order to re-plant for the new season, i.e. the agriculture sector. Line added in the relevant section.

Section 2.2

• Page 5, line 11: Are daily/monthly/fixed SO2 profiles from the IMAGES CTM used? Please clarify

Daily profiles were used, at the overpass time of OMI. Line added in the relevant section.

• Page 5, line 20: SO2 algorithm flagging: What exactly is flagged? Perhaps add a short list or example.

Wording altered.

• Page 6, line 4/5: NS,0 is not used in any equation What is meant by SCD-SCD correction? Typo: AMD precision. I guess this should be AMF precision

Thank you for being so attentive. The NS,0 does not appear in these equations, indeed. The *SCD-SCD correction* is the Slant Density minus the Slant Density correction, and the AMD precision is indeed a typo.

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 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
Thank you for this comment, indeed, we confused the terms *layer* and *level* in the text, it should be clear now. You are also correct on the depiction comment on ppb and DU, it was inadvertently plotted on the "wrong" altitude grid. The calculations were performed appropriately.

• Page 13. Line 24-26. This is not clear for me. Why did only a part of the 8414 grid cells actually provide information?

The domain studied is between 102° to 132°E and 15° to 55°N, on a 0.25x0.25° spacing, however the MEIC emission inventory covers only part of that domain, mainland China. As a result, only 8414 grid cells out of the possible 19200 can be analyzed.

• 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.

This is a very good point. We are currently working towards a companion paper which will present the comparisons between the different emission inventories for SO2 over the region, as per Ding et al., 2007. First results were presented to the scientific community during the 18th GEIA conference in Hamburg in September 2017 [presentations online here: http://www.geiacenter.org/community/geia-conferences/2017-conference]. We hence feel that adding this material to Figure 6 of this paper would make it difficult to interpret, without all the supporting material already in the companion paper.

• 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

Wording altered.