

Interactive comment on “OMI Satellite Observations of decadal changes in Ground-Level Sulfur Dioxide over North America” by Shailesh K. Kharol et al.

Anonymous Referee #1

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The authors present a trend analyses of ground-level SO₂ concentrations from OMI measurements over the US. This paper is interesting and has high potential, but, in my opinion, currently lacks sufficient contest and motivation. The flow of the paper is good and logical, although some sections are a bit too compact to my liking. I therefore suggest the paper to be accepted after major revisions, considering the text below.

Introduction.

The introduction clearly states the benefits of studying SO₂ from satellite, mentioning its role in the formation of sulfate aerosol and the effect of the latter on climate and environmental and health issues. Related previous work is adequately cited. However, the cited paper of Krotkov et al. [2016] already gives a trend analyses of OMI total

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column, over the same time period as the current paper, and furthermore indicates (for polluted areas) the close relationship between derived total columns and emissions. Although the current paper studies surface concentration rather than total column, I would like to see a more elaborate text, motivating why studying total column is not sufficient a proxy for emission trend analysis and the connected assessment of health risks. Only one short sentence is currently dedicated to the novel aspects of the paper and at first glance the overlap with previous work seems high. Please expand.

Section 2. 2.1 OMI: Concise paragraph. Line 3: 'Also, SO₂ variability...' I presume background SO₂ is meant here? Line 6: Please explain the use of the respective thresholds of 0.2 and 65 degree or give an reference.

2.3: Model information Line 24: It would be good to have a quantitative indication of the thickness of the lowest model layer, so reader not familiar with GEM-MACH can develop a feeling for what is assumed as 'surface concentration'. Along the same line, an indication of the partial column of the lowest layer with respect to the total boundary layer column is missing. The reader is referred to McLinden et al papers for AMF-related information, but I think it should be discussed to some extent in the text (here or in the next paragraph).

2.4 Estimation of ground-level SO₂ from OMI. I have the same problem with this paragraph as with the previous. A simple connection between observed and model concentration and column properties is given, adopted from literature, but no discussion is given. This would be ok in the case of an extended section 2.3 Line 2: The cited Lamsal [2008] paper on NO₂ is missing from the list of references.

3 Results and Discussion. The actual results look sound and well described. Line 26/27: Lee et al [2011] paper is missing from the list of references. Also, this paper already derived SO₂ surface mixing ratios from OMI and compared them to in-site measurements, be it only for 2006. Also this motivates a clearer description of the novel aspects of your paper in the introduction.

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