

Interactive comment on “Global source attribution of sulfate concentration, direct and indirect radiative forcing” by Yang Yang et al.

Anonymous Referee #1

Received and published: 3 May 2017

This study quantifies global source-receptor relationships of concentration, direct and indirect radiative forcing of sulfate aerosols utilizing an online chemistry-climate model but nudging it with reanalysis winds. They found that sulfate concentrations are mainly local origin in polluted regions, and their concentration efficiencies in terms of unit precursor emissions are high over arid regions with weak export. In addition, they found the indirect radiative forcing of sulfate aerosols is much larger than the direct radiative forcing. I found topic of this paper is interesting and is suitable for publication in this journal. However, substantial improvements are needed before publication. Following are the major and specific issues:

Major issues:

1. The authors should articulate the novelty or advance in science or methodology of

[Printer-friendly version](#)

[Discussion paper](#)



this study when comparing to previous works. In the introduction, the authors listed a number of similar studies. However, the authors did not describe clearly their motivations to repeat this kind of work as well as the uniqueness of their findings.

2. It is unnecessary to discuss the source-receptor relationships in detail since previous works have already reported similar results. These discussions are lengthy and should be shortened substantially (i.e., abstract, sections 4 and 5). Some figures and discussions could be put into the supporting information.

3. For the method section, the authors may divide it into several subsections (e.g., model description, tracer tagging, model configurations, ...). In addition, the parameterizations of calculating the DRF and IRF of sulfate need to be described in detail. The method used to calculate the DRF of sulfate from the tagged regions/sectors is also unclear.

4. An incremental IRF is defined in this study to quantify the indirect radiative forcing of sulfate. However, there is no validation about this calculation. As the authors mentioned, anthropogenic sources contributed substantially to the incremental IRF over oceans, but few measurements over remote oceans were used to validate their sulfate calculation. The authors may use some aircraft measurements to verify their results over those remote regions.

5. In the introduction, the authors have mentioned that numerous previous studies have examined the sulfate radiative forcing from different sources and regions. However, in the discussion section, they did not carefully compare their results to previous works. I would suggest the authors pay more attention to the difference between this study and previous works.

Specific comments

1. L139-145: the description about parameterizations and approach that were used to calculate the DRF and IRF is not very clear. Please provide more details.

Printer-friendly version

Discussion paper



2.L157: black carbon only occurs in the accumulation mode in MAM3, so the comparison is meaningless.

3.L162: Please show some details about this validation.

4.L198: It is not necessary to show the spatial distributions of SO₂ emissions from each tagged region individually. May put Figure 2 into supporting information.

5.L203-210: Need some explanations about these seasonal variations.

6.L219: Only North America is used to validate the decomposition of global incremental IRF. Since different regions may have distinct chemical composition and meteorology, and the sensitivity to regional sulfur emissions could vary significantly by region. I think the authors should validate more regions, especially those with large SO₂ emissions, e.g., East Asia, Europe and South Asia.

7.L267: Sulfate has a longer lifetime than black carbon? Need a reference.

8.L282-288: If this bias came from the retrieval algorithm, why this overestimation happened more significantly in China than other regions?

9.L294: Here the model results indicated that the export of SO₂ from China is underestimated. However, on Line 291, the authors stated that the inconsistency between simulated results and satellite observations may suggest an overestimation of SO₂ at higher altitude. In general, the transport is more efficient in the free troposphere, therefore this indicates a potential overestimation of exporting SO₂ from China. Moreover, I would suggest the authors validate total sulfur (SO₂+SO₄) concentrations and total precipitations over China and downwind region.

10.Sections 4 and 5 are too long and need to be shortened. The authors should pay more attention to the major advance (or unique findings) of this study and explain the difference between their results and previous works.

11.L423: In Table S3, why is the concentration efficiency of sulfate over MDE in SON

[Printer-friendly version](#)[Discussion paper](#)

greater than 1?

12.L522-525: The sensitivity test with a 20% reduction in regional sulfur emissions over North America indicated a large uncertainty associated with this method. Therefore, I would suggest the authors to discuss more on the uncertainties of this calculation.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-303, 2017.

ACPD

Interactive
comment

Printer-friendly version

Discussion paper

