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Interactive comment on "Inverse modeling of SO$_2$ and NO$_x$ emissions over China using multi-sensor satellite data: 1. formulation and sensitivity analysis" *by* Yi Wang et al.

Anonymous Referee #1

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This paper presents the formulation and sensitivity analysis for the inverse modeling of SO2 and NOx emission over China using satellite data. While the authors seem to emphasize that the joint assimilation saves 50% of the computational time than assimilating SO2 and NO2 separately, the benefit of the joint assimilation should be more than that. This needs to be clarified.

In the paper, γ is introduced to balance the SO2 and NO2 terms. In theory, it is not needed if the uncertainty terms can be well quantified. The optimal value of γ is determined pretty arbitrarily. There are objective ways (such as Hollingworth-Lönnberg and NMC methods) to determine the observational errors and its covariance terms instead

of relying on arbitrary balancing.

Specific comments:

Line 215: It is surprising for the authors to choose less than 3% reduction in the cost function between two iterations as a criterion to halt the minimization. The L-BFGS-B can be very slow. Such a condition can often terminate the minimization prematurely. This needs to be changed if it is not a typo.

Lines 234-7: Is it really beneficial to balance the cost function this way? Can the SO2 observation errors be objectively determined?

Line 247: It is not accurate to say "emissions are adjusted mainly at locations where prior emissions are large". If there are non-zero emissions, the adjustments can be made. The limitation of using scaling factors is that zero-emission grid points cannot be modified.

Technical correction:

Line 161, "Terrain reflectivity less than 30°": Which angle does "Terrain reflectivity" refer to?

Line 171: GOES-FP -> GEOS-FP

Line 433: compared the latter -> compared with the latter