

This paper analyzed the application effect of fused satellite AOD products in improving NO_x and PM emissions in Northeast Asia. The methods used for the emission constraining suggested in this study seem reasonable, and the difference in results according to the combination of satellite data seems clear. The contents of this paper are expected to be sufficiently meaningful in the field of emissions inverse modeling, so it is judged to be worth publishing in this journal. However, it is believed that revising some of the following matters before publication will help improve the quality of the paper.

Minor comments:

1. Lines 242-245: There seems to be an insufficient explanation of how you modified the primary PM emission. How did you distribute the AOD concentration to each PM species in Table S2?

2. Section 2.6: Since the distribution(location) of the ground observation sites used for evaluation is not shown, it is difficult to interpret the results, so it would be good to add them to Figure 1. Whether the observation sites are evenly distributed throughout the domain or intensively distributed only in a specific area is very important to the reliability of the evaluation results.

3. Table 1: The constrained emissions based on GOCI-AHI AOD were found to have significantly reduced error compared to that based on AHI AOD (MAM season). What are the causes of this improvement? Figures 2 and 3 show a clear difference between AHI AOD and GOCI-AHI AOD in South China (MAM season) area. Then, was the improvement of NME also seen in the area? First of all, it seems that the distribution of AERONET observation sites used in the evaluation should be illustrated, and the contents should be fully explained based on that.

4. Lines 370-374: I know what the author is trying to explain, but these sentences seem to need

to be supplemented. Other gas-phase substances, SO₂, and NH₃ have not been adjusted, so it may be difficult to accurately understand the impact of gas-phase substance emission adjustment. Also, this paper does not accurately define the primary PM emission, so the explanation needs to be more specific.

5. The manuscript does not show the accuracy comparison between the constrained results using GEMS-AMI-GOCI2 AOD and GOCI-AHI AOD. Is the reason why this result is not shown because there is no period during which all satellite data exist at the same time? For me, it looks that the adjusted result using GEMS-AMI-GOCI2 AOD shows the highest accuracy. I think the main reason why the GEMS-AMI-GOCI2 AOD showed the best results is the number of AOD records increased in the mixed data. Am I right? And is there any other reason? It would be nice to add a more detailed explanation of the main reason for improved results.