PLEASE NOTE

Reviewers' questions are in standard text.

Manuscript text is in *italic*.

Personal communication for reviewer is in **bold**.

Response to reviewer #1:

This study by Jeon et al. implemented a new hybrid Lagrangian-Eulerian model, STOPS, into CMAQ, to improve the air quality forecasting. Jeon et al. use the STOPS modeling framework with constrained PM from geostationary satellite AOD to improve the Asian dust event that occurred in South Korea on Feb 22-24, 2015. It demonstrates well how STOPS could be useful in air quality forecast, particularly for the unusual air quality events such as Asian dust transport. The merit of using STOPS is on low computational burden compared to CMAQ, which can be critical for emergency forecasting. The manuscript is well within the scope of GMD. However, the manuscript requires some revisions. Please see my comments below. In addition to those comment, I believe science writing in this manuscript should be improved, with focus on reducing the redundancy and increasing coherence within a paragraph. I have listed several places that need such improvement, but please try to improve throughout the manuscript (not limited to my list). When these comments/suggestions are addressed in the manuscript, I recommend this manuscript to be published in GMD.

The authors agreed with reviewer's comment about adding a detailed description of STOPS and in-line dust module in CMAQ v5.0.2, and additional meteorological evaluation results at each observational site. To that point, we have added a figure to briefly illustrate the basic concept of the STOPS model and also added an equation to better explain the in-line windblown dust module. Additionally, we have added the WRF evaluation results (statistics: RMSE, IOA and MBE) for all individual sites to depict a more comprehensive evaluation. Further, we shortened and revised the text so as to reduce redundancy, and have added comprehensive figures and tables for clarifying our results. Please see our responses to the specific comments.

Major Comments:

1. I encourage the authors to clarify the following point carefully throughout the manuscript. In my understanding, the STOPS model seems to be a great modeling tool, mainly due to less computational burden. It might be particularly useful when it needs to explore several possibilities. However, I don't think STOPS itself improves any air quality prediction. Also, the authors already stated that STOPS simulation results are relatively similar to CMAQ. I think the significant improvement in simulated PM10 was contributed by constraining PM10 based on GOCI AOD, not by using the STOPS model. CMAQ with the constrained PM10 from GOCI-AOD should also simulate a more accurate Asian dust. In short, I think STOPS does not contribute to "more accurate" forecasting but could help for "quicker" forecasting. If the authors agree with me, please change any relevant parts throughout the manuscript.

Thanks for the point. The authors agree that STOPS itself does not specifically improve any air quality prediction, but help for "quicker" forecasting. The significant improvement in the simulated PM_{10} was contributed by constrained PM concentrations based on GOCI AOD. Thus, we revised all of the relevant parts and some sentences throughout the manuscript to avoid any possible misunderstanding from readers.

2. I suggest adding more detailed information of STOPS in Section 2.1. It is not easy to picture what exactly the STOPS model does (why is it a hybrid Lagrangian-Eulerian model?). I found the short description on the abstract (line 21-23) and the Figure 1 in Czader et al. (2015) quite helpful, which could be added to Section 2.1. Please clarify model domain and dispersion process used in STOPS: 1) does STOPS accounts for vertical and horizontal dispersion as it transport, like FLEXPART, which means it changes the number of grids carrying by STOPS over time?; 2) does STOPS carry a couple of grids in the defined STOPS domain or STOPS moves the defined STOPS domain over time (e.g., 61x61 gridcells in Section 4.1)?

As the reviewer suggested, we added a figure similar to Figure 1 in Czader et al., (2015) in the revised manuscript to show more detailed information of STOPS.

<Figure 1 in the revised manuscript>

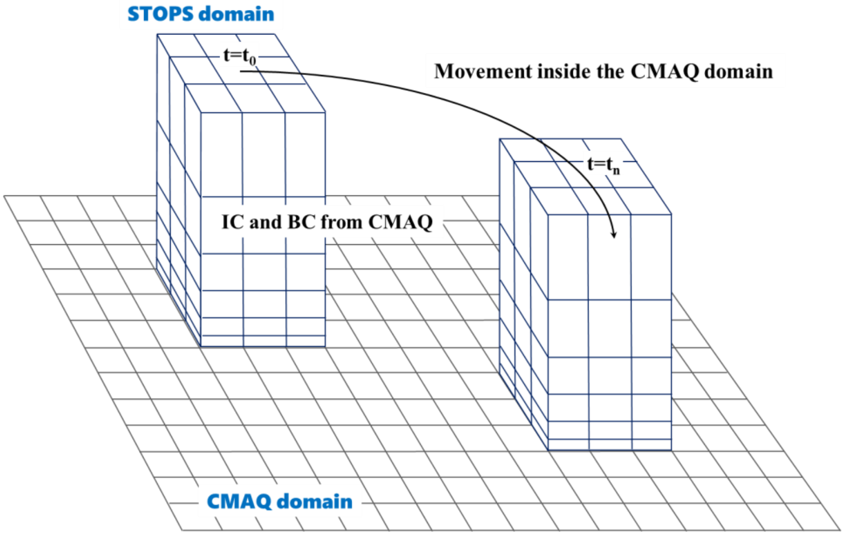


Figure 1. Conceptual diagram showing the basic structure and movement of the STOPS domain inside the CMAQ domain.

As shown in Figure 1 in the revised manuscript, STOPS has sub-domain inside the CMAQ domain and it moves along with the mean wind in its domain. The vertical structure and the physical and chemical process in STOPS are exactly same as in the host CMAQ model except for the calculation of advection fluxes. CMAQ uses horizontal wind velocity (u and v) from WRF to calculate horizontal advection fluxes; while STOPS calculates the difference between a cell horizontal wind velocity and the mean horizontal velocity in STOPS domain, so it can consider the moving speed and direction of STOPS domain for the calculation of advection fluxes. Since the STOPS domain moves over time, the horizontal velocity from WRF should be adjusted based on the movement of STOPS domain. Although STOPS is Eulerian-Lagrangian based model, it is close to Eulerian model rather than Lagrangian. STOPS is almost similar to CMAQ but has small domain size. The reason why STOPS is much faster than full CMAQ is that the number of grid cells in STOPS domain is much smaller than those in CMAQ domain. We revised section 2.1 in the manuscript by adding a figure and a couple of sentences for the better explain of STOPS model.

<Section 2.1 in the revised manuscript>

STOPS has the same vertical structure and simulates the same physical and chemical *processes as CMAQ, except for the calculation of advection fluxes. CMAQ uses horizontal wind velocity (u and v) from WRF to calculate horizontal advection fluxes, but STOPS calculates the difference between a cell horizontal wind velocity and the mean horizontal velocity in STOPS domain (Czader et al., 2015), so it can consider the moving speed and direction of STOPS domain for the calculation of advection fluxes. Since the STOPS domain moves over time, the horizontal velocity from WRF should be adjusted based on the movement of STOPS domain.*

3. I agree with the authors that the main reason for the PM10 underprediction in CMAQ is very likely missing dust emissions, as the threshold friction velocity calculation indicates. However, I don't agree with the authors on how to draw a conclusion that the model meteorology is accurate, mainly because the evaluation results, shown in Figure 3, are not comprehensive. Here are more specific questions related to the evaluation. First of all, why do the authors choose averaged values of 20 sites? I'd strongly prefer to see individual site evaluations. Alternatively, the individual site evaluation can be provided in supplementary material. Secondly, given that the long-range transport of Asian dust to influence South Korea, it is important to simulate correct meteorology from source regions to receptor regions. Would it be possible to include meteorological evaluations in Chinese source regions? Lastly, I encourage including more meteorological variables (such as precipitation, if there is any precipitation event during the event).

Firstly, we added WRF evaluation results (RMSE, IOA and MBE) for 20 sites (S1-S20) in the revised supplementary document as the reviewer suggested. Table S3 in the revised supplementary document shows evenly high IOA and low biases at 20 individual sites, indicating that the simulated meteorology over Korea (receptor regions) is reasonably accurate.

<Table S3 in the revised supplementary document>

***Table S3**. Statistical parameters for the WRF simulation results during the entire simulation period (February 2015) at 20 observational sites. The location of each site is shown in Fig. 2 in the manuscript.*

Sites	*Temperature*	*Wind Speed*

	RMSE	*IOA*	*MBE*	*RMSE*	*IOA*	*MBE*
S1	*0.78*	*0.99*	*-0.08*	*1.12*	*0.97*	*0.03*
S2	*1.46*	*0.98*	*0.17*	*1.38*	*0.90*	*0.15*
S3	*2.49*	*0.90*	*-0.27*	*1.23*	*0.80*	*-0.85*
S4	*1.94*	*0.93*	*1.80*	*1.28*	*0.78*	*-0.21*
S5	*2.31*	*0.93*	*1.48*	*1.13*	*0.84*	*-0.40*
S6	*2.31*	*0.93*	*1.04*	*1.89*	*0.91*	*1.49*
S7	*2.48*	*0.96*	*-1.46*	*1.96*	*0.77*	*1.43*
S8	*2.58*	*0.93*	*-1.58*	*1.61*	*0.87*	*1.25*
S9	*1.40*	*0.94*	*1.39*	*1.19*	*0.86*	*1.12*
S10	*1.42*	*0.95*	*1.41*	*1.87*	*0.91*	*1.21*
S11	*2.02*	*0.97*	*-1.06*	*2.03*	*0.75*	*1.45*
S12	*2.70*	*0.78*	*-2.35*	*1.34*	*0.92*	*0.94*
S13	*2.11*	*0.94*	*1.24*	*1.24*	*0.88*	*0.85*
S14	*1.59*	*0.95*	*1.01*	*2.07*	*0.93*	*1.46*
S15	*2.67*	*0.89*	*-2.29*	*2.37*	*0.76*	*1.90*
S16	*1.39*	*0.98*	*0.43*	*1.59*	*0.89*	*0.90*
S17	*2.48*	*0.84*	*-1.71*	*1.98*	*0.74*	*1.36*
S18	*1.60*	*0.96*	*-1.09*	*2.64*	*0.72*	*1.27*
S19	*1.58*	*0.95*	*1.17*	*2.03*	*0.82*	*1.02*
S20	*1.12*	*0.96*	*0.98*	*1.59*	*0.89*	*0.90*
Average	*1.92*	*0.93*	*0.01*	*1.68*	*0.85*	*0.86*

Secondly, the authors agree that the meteorology in source regions (China and Mongolia) is also important for the simulation of long-range transport of Asian dust; hence the data need to be evaluated. This study showed accurate meteorology only in receptor regions (Korea) not in source regions (China and Mongolia) due to the limited availability of the data. As the reviewer indicated, uncertainty in meteorology (particularly in source regions) could be one of possible reason for the PM_{10} underestimation. We have added the requisite description regarding the uncertainty in meteorology in section 3.1 in the revised manuscript.

<Section 3.1 in the revised manuscript>

As shown in Fig. 4 and Table S3, meteorological fields such as temperature and wind speed over receptor regions (Korea) showed close agreement with observations, even during the Asian dust period. It suggests that the underestimated PM_{10} concentration was likely due to the uncertainty in meteorology over source regions (China and Mongolia), and/or faulty *estimation of dust emissions for the CMAQ simulation. We attributed the main reason for the PM_{10} underestimation to poorly estimated dust emission because CMAQ showed poor performance only during the Asian dust event days.*

Finally, there were several challenges in obtaining observational data in China and Mongolia. Also, the surface data for this study provided only temperature and wind variability. For these reasons we could not include meteorological evaluations in source regions and evaluation results for other factors such as precipitation.

4. Please provide a brief description of the CMAQ dust emission parameterizations used in your forecast modeling. It will help readers to understand what the underpredicted threshold friction velocity affects to dust emissions.

As the reviewer suggested, we provided a brief description of the CMAQ in-line windblown dust module as shown below.

<Section 3.1 in the revised manuscript>

The module calculates the vertical dust emission flux (F) by following formula described by Fu et al. (2014).

$$F = \sum_{i=1}^{M} \sum_{j=1}^{N} K \times A \times \frac{\rho}{g} \times S_i \times SEP \times u_* \times ({u_*}^2 - {u_{*_{ti,j}}}^2)$$

where i and j represent the type of erodible land and soil, K is the ratio between vertical and horizontal flux, A is the particle supply limitation, ρ is the air density, g is the gravitational constant, S_i is the area of the dust source, SEP is the soil erodible potential, u_* is the friction velocity, and $u_{*_{ti,j}}$ denotes the threshold friction velocity.

Also, we added a sentence describing the importance of threshold friction velocity on the calculation of dust emission flux to better explain the reason for the underestimated dust emission from the CMAQ in-line module.

<Section 3.1 in the revised manuscript>

Several studies (e.g. Choi et al., 2008; Fu et al., 2014) have reported that the threshold friction velocity plays a key role in the calculation of dust emission flux because the threshold *can determine the possibility of the lifting of dust particles.*

Minor comments:

1. <Title>: I'd suggest changing a title. What about "Computationally efficient air quality forecasting tool: implementation of a hybrid Lagrangian-Eulerian model into CMAQ v5.0.2"?

The authors agreed to change the title as the reviewer suggested. However, we added a phrase "for a prediction of Asian dust" to emphasize that this is a case study for an Asian dust event. Also, we used "STOPS model" instead of "a hybrid Lagrangian-Eulerian model", because we thought "a hybrid Lagrangian-Eulerian model" is too generic to be used in the title. In conclusion, we changed the title of this study as "Computationally efficient air quality forecasting tool: implementation of STOPS model into CMAQ v5.0.2 for a prediction of Asian dust".

2. <Abstract> : I'd strongly suggest re-writing this section. Overall abstract seems to sound quite redundant. Please consider taking the suggestions below.

Page 1; line 17-19 – Please consider moving this to the end of Abstract and either delete or modify this phrase ("for a more accurate prediction of Asian dust event in Korea"): see the major comment above.

Page 1; line 20-21 – I'd suggest deleting sentence. It is mentioned in line 31-33.

Page 1; line 24-27 – Please consider deleting this as well. Next a few sentences basically say the same information. Having this sentence, it sounds too redundant.

Page 1; line 29-31 – I'd suggest modifying this. The following is my suggestion: "The underestimated PM10 concentration is very likely due to missing dust emissions in CMAQ rather than incorrectly simulated meteorology as the model meteorology agrees well with the observations."

Page 1; line 32 – Please delete "we use the STOPS modeling system inside the CMAQ model, and", and please modify "we run several STOPS simulations using" to "we used the STOPS model with".

Page 2; line 2-4 – Please shorten the sentence. "The simulated PM10 from the STOPS simulations were improved significantly and closely matched to surface observations".

Page 2; line 5-9 – Please see my major comment 1.

We re-wrote the Abstract section based on the reviewer's comments. We shortened and changed the sentences as the reviewer suggested and deleted unnecessary sentences to reduce the redundancy.

<Abstract in the revised manuscript>

Abstract. *This study suggests a new modeling framework using a hybrid Lagrangian-Eulerian based modeling tool (the Screening Trajectory Ozone Prediction System, STOPS) for a prediction of an Asian dust event in Korea. The new version of STOPS (v1.5) has been implemented into the Community Multi-scale Air Quality (CMAQ) model version 5.0.2. The STOPS modeling system is a moving nest (Lagrangian approach) between the source and the receptor inside the host Eulerian CMAQ model. The proposed model generates simulation results that are relatively consistent with those of CMAQ but within a comparatively shorter computational time period. We find that standard CMAQ generally underestimates* PM_{10} *concentrations during the simulation period (February 2015) and fails to capture* PM_{10} *peaks during Asian dust events (22-24 February, 2015), The underestimated* PM_{10} *concentration is very likely due to missing dust emissions in CMAQ rather than incorrectly simulated meteorology as the model meteorology agrees well with the observations. To improve the underestimated* PM_{10} *results from CMAQ, we used the STOPS model with constrained PM concentrations based on aerosol optical depth (AOD) data from Geostationary Ocean Color Imager (GOCI), reflecting real-time initial and boundary conditions of dust particles near the Korean Peninsula. The simulated* PM_{10} *from the STOPS simulations were improved significantly and closely matched to surface observations. With additional verification of the capabilities of the methodology on concentration estimations and more STOPS simulations for various time periods, STOPS model could prove to be a useful tool not just for the predictions of Asian dust but also for other unexpected events such as wildfires and upset emissions events.*

3. <1. Introduction>

Page 2; line 18-21 - I'd suggest changing "Severe PM events … Gobi Desert" to "Dust emissions from Mongolia and Gobi Desert".

Page 2; line 23 – please change “become” to “are”.

Page 2; line 29 – Please rephrase “the numerous factors such as meteorology and emissions ... PM concentrations”. It sounds a bit unclear.

Page 2; line 21 – Add “modeling” in front of “studies”; change “described” to “shown” and delete “simulation”.

Page 3; line 31 to Page 3; line 9 – This paragraph should be rewritten in order to deliver the key point clearly, which, I think, improving meteorology and emission inventory do not help better Asian dust forecasting due to the uncertainty in dust emission modeling. Besides, please delete the last sentence (Therefore, ~): the first part is too obvious to mention, and the second part is somewhat debatable (especially “primarily”) and contradicts with “accurate meteorology” above.

Page 3; line 25 – This “(STOPS, hereafter)” should be moved above, where STOPS is mentioned in the first time.

Page 3; line 22-35 – I found this paragraph This paragraph doesn’t sound coherent. Please use present tense to state goals and objectives and past tense for methods. Please also modify the paragraph based on my major comment 1. It is incorrect to say that STOPS enhance the PM predictions.

Page 3; line 23 – Delete “simulated”; add “to” in front of “determine”.

Page 3; line 24 – Delete “particularly”, as this study focuses on Asian dust event only.

We revised the Introduction section based on the reviewer’s comments. We re-wrote some sentences more clearly and removed a couple of unnecessary sentences as the reviewer suggested.

4. <2.2 Modeling system and experimental design>

Page 5; line 4-5 – I think this sentence fits better in the end of next paragraph.

Page 5; line 10 – why do you mean by “refer to the CAPPS emissions”?

Page 5; line 18 – delete “for the simulation”

Page 5; line 18-23 – Please shorten the sentences.

Page 5; line 24 – Please remove “listed in Table 1” and list the date here.

We shortened, moved and deleted some sentences in section 2.2 (in the revised manuscript) as the reviewer suggested.

5. <2.3 In-situ and satellite measurements>

Page 5; line 29 – “referred to” to “use”

Page 5; line 36 – what is this “500 m resolution” for? Why is it different from AOD’s 6 km resolution?

Page 6; line 1 – “550 nm AOD” to “AOD at 550nm”

500 m and 6 km are the resolutions of original GOCI data and retrieved one by Choi et al. (2016) algorithm. The retrieved GOCI data with a 6 km resolution were used in this study. We corrected two phrases in section 2.3 (in the revised manuscript) as the reviewer suggested.

6. <3.1 Comparison with surface measurement>

Page 6; line 20-22 – Please define RMSE, IOA and MBE and explain what each measure indicates briefly.

Page 6; line 26-29 – Please see the major comment 3.

Page 6; line 30-36 – CMAQ dust emission modeling should be explained before this result. Please add the brief description in method section.

As the reviewer suggested, we added brief description of the statistical parameters used in this study (RMSE, IOA and MBE) in section 2.3 (in the revised manuscript).

<Section 2.3 in the revised manuscript>

The following statistical parameters were used for the evaluation of the performance of WRF and CMAQ simulations: Index Of Agreement (IOA), Mean Bias Error (MBE) and Root Mean Square Error (RMSE). These are defined as:

$$IOA = 1 - \frac{\sum_{i=1}^{N}(P_i - O_i)^2}{\sum_{i=1}^{N}(|P_i - \bar{P}| + |O_i - \bar{O}|)^2}$$

$$MBE = \frac{\sum_{i=1}^{N}(P_i - O_i)}{N}$$

$$RMSE = \sqrt{\frac{\sum_{i=1}^{N}(P_i - O_i)^2}{N}}$$

where N is number of data points and P_i *and* O_i *denote CMAQ-simulated and observed concentrations, respectively.*

We also added the WRF evaluation results at individual sites in Table S3 (in the revised supplementary document), and a formula for dust estimation used in CMAQ in-line dust module in section 3.1 (in the revised manuscript) for the better explanation. Please see our responses for question 3 and 4.

7. <3.2 Comparison with satellite-based observation>

Page 7; equations 4-6 – It looks like empirically derived method. Does the method by Roy et al. (2007) tested over the Korea as compared to more theoretical-based (Mie theory) optical properties? Is it reasonable to use it for Korea? Also, why isn't there no water uptake by organic aerosol [OM] in Eq 5?

Figure 4 – It is good that the CMAQ AOD field shows removed areas with GOCI bad pixels. However, it would be also helpful to present CMAQ AOD without removing any areas in the supplementary materials. It could show what GOCI might miss in those areas.

Page 7; line 32 – delete "the same results"

Page 7; line 34 – Do you actually mean "PM precursor" or "PM and its precursor"? If it is indeed specifically "PM precursor", please provide further explanation. Next sentence about meteorology should be re-considered (see major comment)

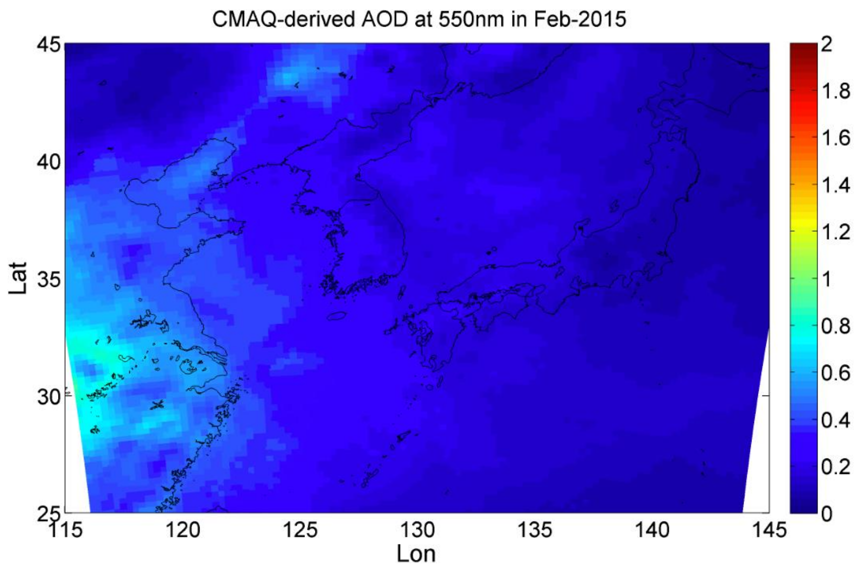
Page 8; line 3 - please add year: Feb 22-24, 2015. Please make the same corrections throughout the manuscript, if possible.

Page 8; line 15-16 – please change "the high amounts of dust particles" to "the high dust concentrations".

Page 8; line 19-20 – This should be modified with my major comment 1. I'd suggest changing to this: "We use STOPS to explore how to improve PM10 simulation."

Firstly, the empirical method used in this study has successfully been tested in East Asia (Park et al., 2011; Song et al., 2008) which is preferred to the Mie theory in this region. This is mainly because of the fact that aerosols properties including size distribution have not been precisely characterized in this region to allow us to use the Mie-theory extinction coefficient calculations. This issue was partly discussed in two mentioned papers. The OM hygroscpocitiy are highly uncertain, and to best of our knowledge it has not been parameterized yet. It should be mentioned that the significant portion of dust particles is NH_4NO_3 and SO_4^{2-}, therefore OM concentrations are not strongly prominent.

Secondly, as the reviewer suggested, we made a figure showing the CMAQ AOD without masking of GOCI bad pixels (Please see the figure shown below). However, it does not entirely match with previous figure (Figure 5-(b) in the revised manuscript) because bad pixels in GOCI were not filtered out for the calculation of monthly mean AOD from CMAQ. Although the below figure shows the CMAQ-derived AOD over whole areas in the modeling domain, it cannot be directly compared with figures in Figure 5 (in the revised manuscript). For this reason, the authors decided not to add the below figure to the supplementary document to avoid unnecessary argument.



Lastly, we revised all the sentences in section 3.2 as the reviewer suggested.

8. <4.2 PM_{10} forecasting using STOPS>

Page 9; line 6-8 – This sentence is unnecessarily long. Please remove “that is, the … failed”.

Page 9; line 8-9 – This should be rephrased, esp. “the most recent and accurate input data”. It makes me think about meteorology, emissions, initial and boundary conditions. If the constrained PM10 derived from GOCI AOD is only read in the first time, it is considered initial concentration and thus “input data”. However, the way you used the constrained PM10 derived from GOCI AOD in Section 4.2.2 seems more than initialization and close to nudging.

Page 9; line 13-18 – Please remove this part. This is out of placed and doesn’t have much new information, in my opinion. If the authors want to make a point that the CMAQ with constrained PM using GOCI AOD is less desirable as a forecasting tool due to their long simulations, perhaps do it elsewhere (maybe the end of the paragraph).

Page 9; line 18 – what do you mean by “dust core”? center of dust storm?

Page 9; line 26- do you actually mean “on the STOPS domain”? Perhaps it is “on the STOPS results”? Also, perhaps “would be diminished” is better than “would be mitigated”?

We removed and revised a couple of unnecessary sentences and confusing phrases as the reviewer suggested (Section 4 in the revised manuscript).

9. <4.2.1 Satellite-adjusted PM concentrations>: This section is particularly confusing. Please re-write them and use figure or diagram to help readers to understand the method.

Page 9; line 31 – Please remove “To provide ~ AOD into account,” and clarify “at the beginning of the updated forecast”.

Page 9; line 34 – Perhaps “as a constraint” is correct?

Page 10 – Isn’t the second paragraph better to move?

Page 11; line 2-3 – Fix line break 4.2.2.

As the reviewer suggested, we re-wrote section 4.1 (in the revised manuscript) to better explain the method we used for PM constraining. We added a figure, which briefly describes entire procedures of the new PM forecasting using STOPS with GOCI-derived AOD data.

<Figure S3 in the revised supplementary document>

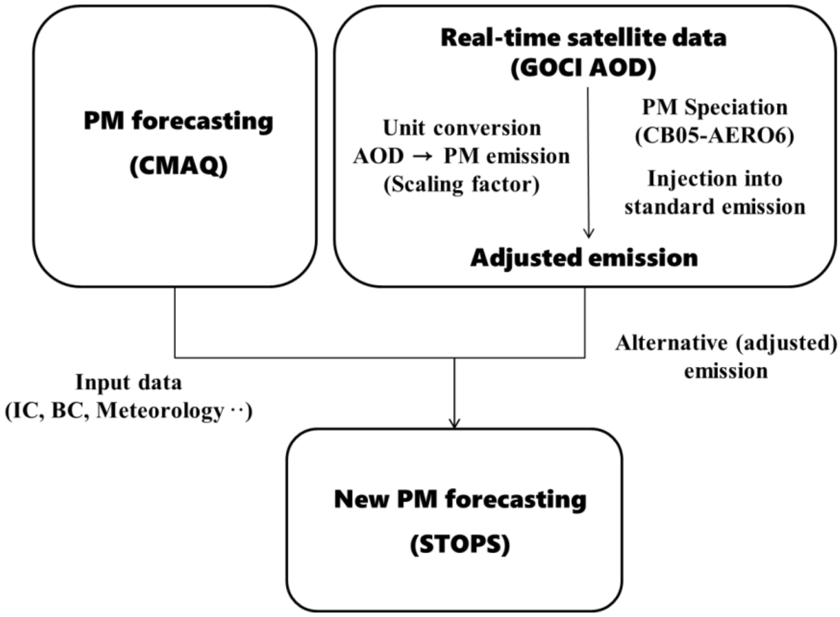


Figure S3. *Schematic flowchart describing the procedures of the new PM forecasting by STOPS using the real-time AOD data from GOCI.*

10. <4.2.2 Enhanced PM10 forecasting using STOPS>

Page 11; line 22 - why did you said “were assumed to”?

Page 11; line 29-30 – please shorten to “Figure 8 exhibits clear…”

Page 11; line 27 – please add “, shown in Fig. 8,” after using STOPS

Page 11; line 32 – please change to “because of the poor dust emission modeling in CMAQ”.

Page 11; line 36~ - Isn’t this already mentioned in Line 30?

Page 12; line 32 – Remove “changed” in “To verify the changed horizontal”

We removed some unnecessary sentences, and revised all of the addressed phrases and sentences as the reviewer suggested in order to reduce redundancy (Section 4.2 in the revised manuscript).

11. <Summary>: Please revise the summary section if it is subject to the major comments.

Page 13; line 22 – “but with” to “but used”

Page 13; line 24 – add comma between “dust events” and “we”

We revised the Summary by considering all of the changes in each section in the revised manuscript.

12. <Table & Figures>

Table 2 – “Without Dust Events” to “Without dust events”

Figure 1 – It is hard to find the site location. I was able to find only 17 sites. Can you use color symbol for sites?

Figure 2 – It would be nice, if the dust event days were shown in the figure.

Figure 6 – Does white space shown in the map represent for very low AOD or does it also include areas with missing pixels? Just in cases missing areas should be shown in white.

Figure 7 – Please double check the caption. It says standard and constrained CMAQ runs, while “constrained CMAQ run” is never discussed in the main text.

We corrected “Without Dust Events” in Table 2 (in the revised manuscript) to “Without dust events”, changed Figure 2 (in the revised manuscript) by adding 3 missing sites and using color symbols, and marked the Asian dust event days in Figure 3 (in the revised manuscript). Also, we revised caption in Figure 7 and 8 (in the revised manuscript) for the better explanation.

<Figure 2, 3, 7 and 8 in the revised manuscript>

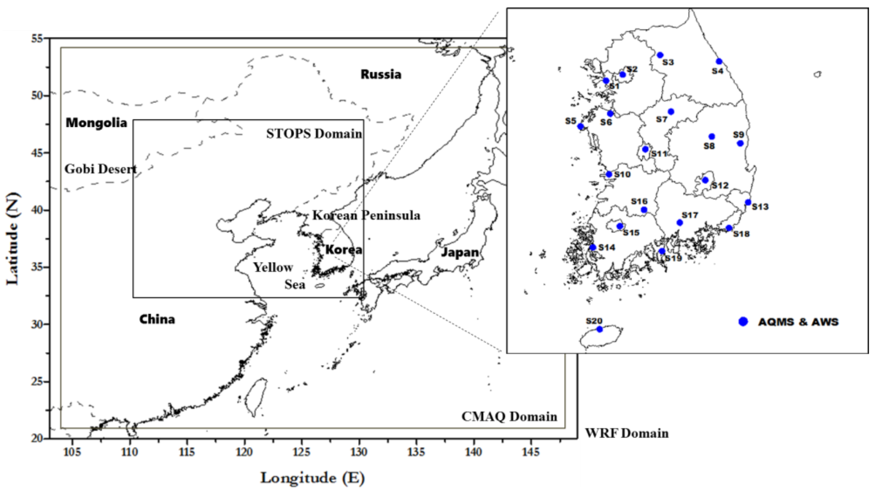


Figure 2. *Domains for the WRF, CMAQ and STOPS modeling. The right panel shows the location of the air quality monitoring stations (AQMS) and automatic weather system (AWS) sites used in this study.*

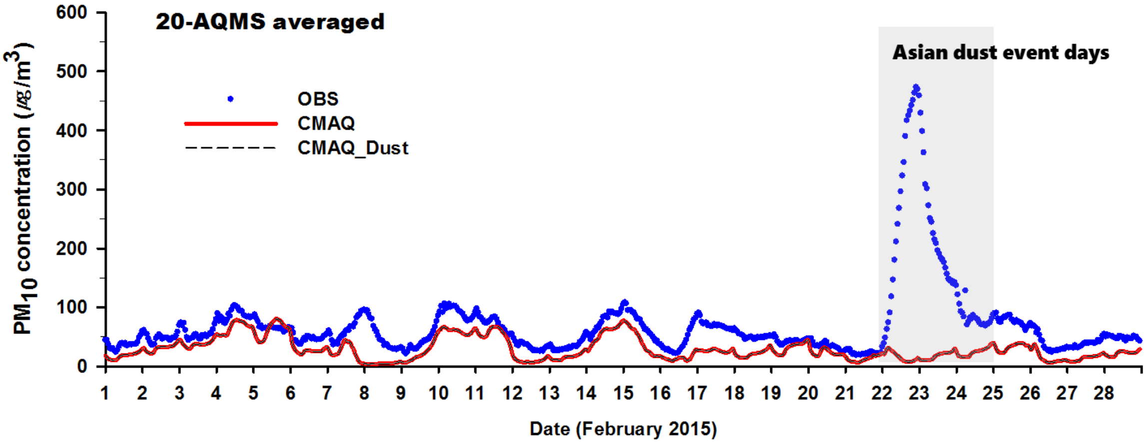


Figure 3. *Time series of observed (OBS, blue dots) and simulated (CMAQ: red line, CMAQ_Dust: black dashed line) PM_{10} concentrations in February 2015. The values are averaged values for 20 AQMS sites: CMAQ_Dust is closely coupled with the standard CMAQ modeling results (red line).*

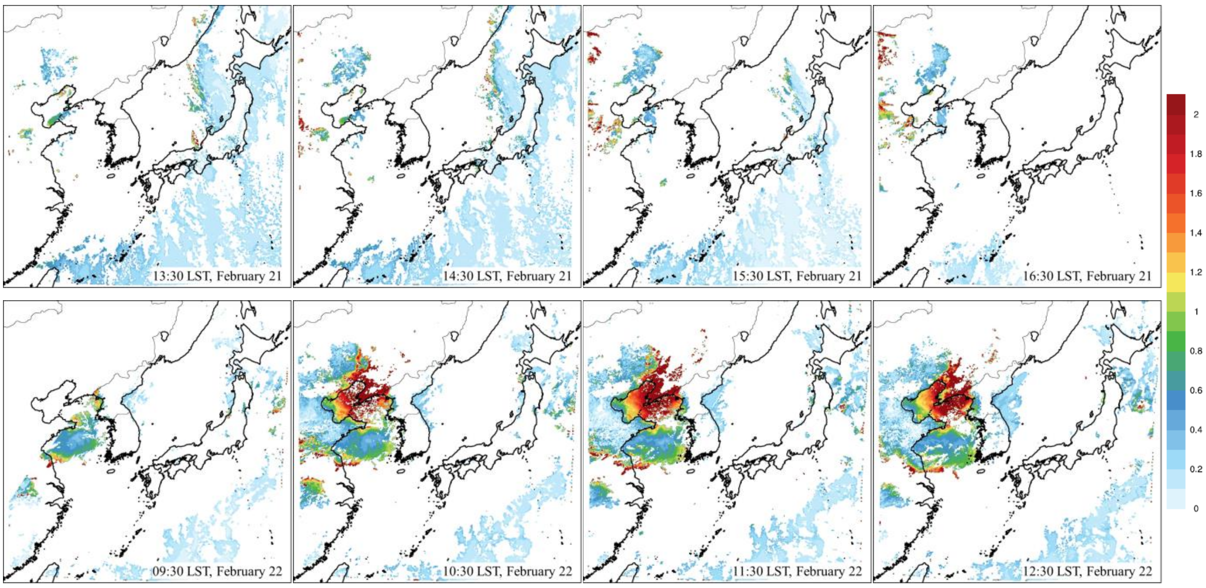


Figure 7. *The GOCI-derived AOD (550 nm) from 13:30 LST on 21 February to 12:30 LST on 22 February in 2015. The white-colored areas represent missing pixels.*

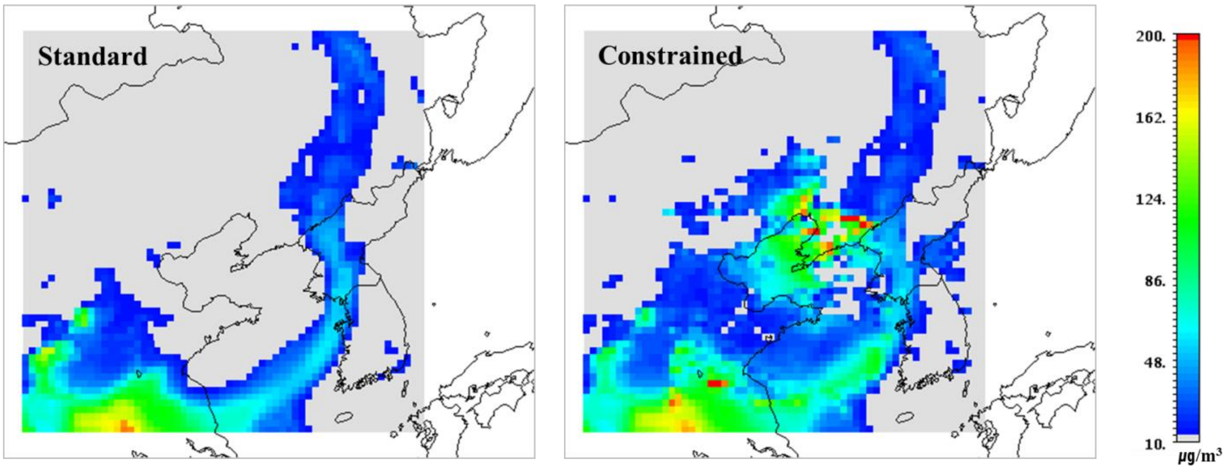


Figure 8. *Difference of the simulated PM_{10} concentrations (μg m^{-3}) between the standard CMAQ run (left) and STOPS forecasting run with alternative emission estimated according to GOCI-derived AOD (right) inside the STOPS domain at 12:00 LST on 22 February in 2015.*