Response to comments by Anonymous Referee #2

We would like to thank the Anonymous Referee #2 for the constructive comments on our manuscript to improve it. We have replied to each point in turn below and will consider these carefully into the revised manuscript. Note that the referee's comments are in bold.

1. This manuscript uses three different approaches to developing biogenic emissions in the Seoul area, then uses those emissions estimates in a chemical transport model. The model is run with a fine grid over the area of interest. They perform a model evaluation, and also show how differences in ozone correspond to differences of emissions. It is not apparent that there is a demonstrable difference in the performance of the model in a statistical sense. All of the performance metrics are similar.

We agree to the referee's assertion that all of the performance metrics are similar. However, it should be noted that the main objective of the performance evaluation section is not to determine which one of the three different PFT database should be preferred. The main objective of this performance evaluation is to check whether our CMAQ simulations (with different PFT scenarios) can provide us with reasonable O3 concentrations in our study domain although these are a limited number of comparisons. As the result, the CMAQ predictions follow the observations reasonably well (Line 22 of Page 24945 - Line 23 of Page 24947).

We feel that the title of section 3.4 ″Impact of PFT distribution differences on model performance" convey somewhat misleading implication to readers like "There must be great differences between the model performance metrics." Therefore, to avoid misleading implication, the title of the section 3.4 will be altered to "Performance check of CMAQ predictions."

In the section 3.4, we described that most monitoring sites (121 sites in 148 sites) were located at urban sites (Lines 19-20 of Page 24946). Thus, we can expect that the little difference in those performance metrics does not necessarily mean the same impacts of the different PFT distribution scenarios on CMAQ O3 predictions. To support this, we presented noticeable deviations of hourly CMAQ O3 predictions with different PFT scenarios in our modeling domain (Figure 8 in the original manuscript). We further indicated that different PFT distribution scenario make large differences with increasing ambient temperature (Figure 9 in the original manuscript).

As we responded to the referee #1, we will clearly address the point that KORPFT can be used one of the PFTs to support biogenic emission and ozone air quality modeling. In addition, we will add some discussions about the benefit of using local detailed sources of PFT distribution data for biogenic emission and ozone air quality modeling. As shown in Figure1, KORPFT finely well provide the PFT distribution information, whereas CDP and MODIS have notable omissions of the PFT distributions at some islands, some costal city areas off the Incheon Metropolitan Area, and some Seoul Metropolitan Area. These omissions of PFT distributions can cause the biogenic emission missing implying the BVOC reactivity missing.

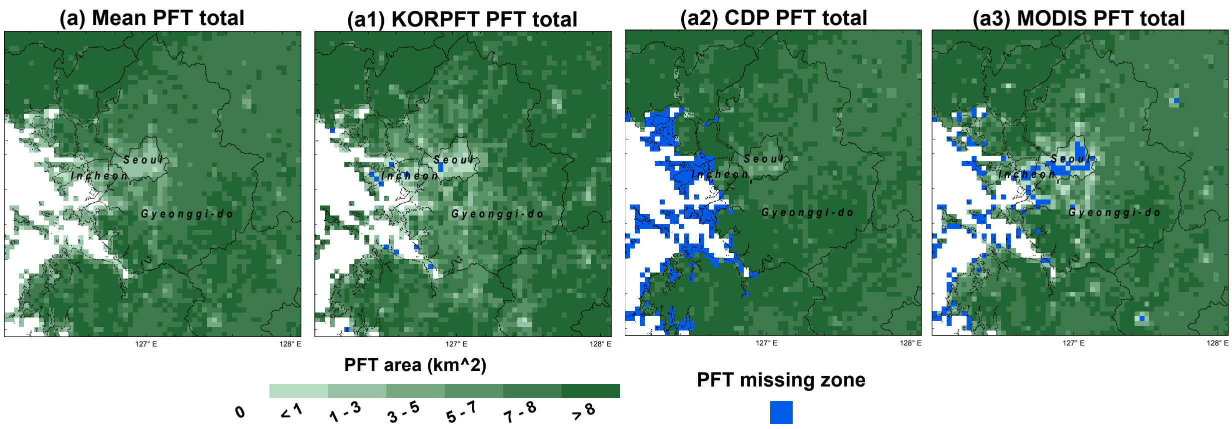

Fig 1. Spatial distribution of the PFT area for each PFT scenario. The mean spatial distributions for PFT total (i.e. sum of BT, NT, SB and HB areas) were derived by averaging the three different distribution data sources (i.e., KORPFT, CDP and MODIS).

2.1 "Due to the short photochemical lifetimes of BVOC (e.g., isoprene ~2h) (Atkinson and Arey, 2003), the emitted BVOC from local biogenic emission sources (i.e., PFT area distributions) immediately affect the levels of local surface O3 concentrations rather than move over long distance." Should be re-examined in this case.

We agree with the referee. The assumption "the emitted BVOC from local biogenic emission sources (i.e., PFT area distributions) immediately affect the levels of local surface O3 concentrations rather than move over long distance" may not proper to current our modeling experiment. To address referee's concern, we have re-conducted spatial regression analysis and this result will be added into the revised manuscript.

Based on the results of the reanalysis, the section 3.3 will be re-written and a paragraph in the concluding section (Lines 17-20 of page 24954) "Multiple regression analyses (CMAQ δO3 results vs. δPFT variable datasets) with the different PFT scenarios suggested that the region-specific PFT distribution dataset (i.e., KORPFT) provides better explanations for the relationship between PFT, BVOC emission and surface-level O3 changes." will be reworded.

Furthermore, using a regression equation we tried to present the effect of the data missing problem (i.e., BVOC emission missing due to LAIv missing) on the CMAQ O3 predictions by (line 16 of page 24948 – line 9 of page 24950 in the original manuscript). We feel that this is not sound approach. For the revision, we have carried out CMAQ simulations with new biogenic emissions estimated by MEGAN with new LAIv input data and investigated the effect of data missing on CMAQ O3 predictions. We made the new LAIv input data by filling in each nearest neighbor LAIv value for each missing grid cell. This new result will be added in the revised manuscript.

2.2. In particular, the way I see they are using the deltas in the regression analysis, they are using each grid independently in their regression analysis, i.e., they use the deltas for each grid (this should be made very clear) in the regression equation, thus the very large number of data sets.

We thank referee for this comment. The referee is right to point out how we used data for the regression analysis. We will make it clear in the revision that we perform spatial regression analysis using each gridded dataset. The main objective of this multiple regression analysis was to show that changes in O3 concentrations at the modeling grid cells are influenced by the changes in PFT distributions at the modeling grid cells. We did not consider spatial dependence of each gridded value for dependent variable (i.e., delta O3). In the reanalysis, we have considered this problem.

2.3. There are a number of questions about this. First, such a statistical analysis, and in particular, to infer much in terms of differences and in terms of the significance, should account for the very strong correlations between the deltas in adjacent girds and at adjacent times.

We did not consider the spatial collinearity of explanatory variables (i.e., delta PFTs). In the reanalysis we have considered this problem. The new results will be presented in the revision.

2.4. Second, a lifetime of 2 hours, and a wind velocity of, say, 3m/s, leads to a distance covered of about 11 km or about 4 grids. The emissions do not fully, or even mostly, react in the grid in which they are emitted. At 3 m/s, on average, the emitted isoprene will be out of the cell in about 10 minutes.

Like the referee's comment, due to the relatively high wind speed (mean wind speed at 10m = 3.7 m/sec) over our finer modeling grid system (3km × 3km), isoprene emissions generally do not fully react in the grid in which they are emitted. In the reanalysis we have considered the spatial dependence of each gridded value for dependent variable and the spatial collinearity of explanatory variables. The new results will be presented in the revision.

3. The paragraph beginning with "Meanwhile, we consider that the KORPFT is the representative sources of PFT data..." is not clearly stated (do you mean the "most" representative?), highly speculative and either the authors should delve in to this further or remove it. The next paragraph is likewise speculative and should be removed.

To address referee's concern, we will remove this paragraph and the next paragraph.

4. The section starting with "Recently, the Korean Ministry of Environment (MoE) promulgated the Special Act on Metropolitan Air Quality Improvement (SAMAQI) ..." and ending with "... the SMA municipal government has to establish the implementation plans (IPs) for air quality attainment for those pollutants across the SMA (Ministry of Environment, 2012a)." contains little scientific content and should be removed. As is, it detracts from the article.

We thank referee for this comment. To address referee's concern, this section will be removed in the revised manuscript.

5. The grammar is in need of much attention, e.g.,: The sentence with "provide important implications to air quality supporting groups" is quite awkward. "Secondly, the MIR scale based OFP calculation may cannot" "obtained were simply rough ones and it may cannot represents" "A global chemistry transport modeling with MOZART-4 (Model for Ozone and Related Chemical Species, version 4) reported"

We thank for the referee's comments on the grammar of our text with some detailed list. We will pay attention to the grammar of our text in the revised manuscript.

6. Support and clarification is needed for "As an example, the MEGAN BT was calculated by 80% x [broadleaf evergreen trees+broadleaf deciduous trees]"

This sentence will be supported and clarified in the revision as followed:

As an example, geographically distributed pixels (a pixel size = $500\text{m} \times 500\text{m}$) for MODIS broadleaf evergreen trees and broadleaf deciduous trees were reclassified to the broadleaf trees (BT). Based on the IGBP description about the vegetation covered fraction for the broadleaf evergreen trees and broadleaf deciduous trees (Strahler et al., 1999) (i.e., "Lands dominated by woody vegetation with a percent cover > 60%"), we assumed that the BT covered percent values for every reclassified pixel range 60% and 100% (mean BT covered percent = 80 %). Therefore, we simply assigned mean BT covered percent, 80 %, to every BT pixels when we develop a gridded $PFTF_{BT}$ database (500m ×500m spatial resolution) for MEGAN.

7.1 In summary, the manuscript does not really support the use of one approach versus another.

We thank referee for this comment and agree to the referee's view. It is hard to say that one PFT is more accurate (or reliable) than others based on the contents of manuscript. As we responded to the referee #1, we will update the manuscript with a clear view that KORPFT can be used one of the PFTs to support biogenic emission and ozone air quality modeling. We will keep this view clearly throughout the revised manuscript.

The main aspect we wished to address from our manuscript is that the use of different PFT distribution data need much caution for O3 air quality modeling (or forecasting) in complicated urban atmospheric condition including meteorology and emissions because it can provide temporally and spatially far different O3 prediction results. To support this aspect, we have synthesized and addressed our results showing that the spatial difference of vegetation data of each PFT scenario (e.g., Figure 3 in the original manuscript) affect the results of the MEGAN BVOC emission modeling (e.g., Figure 4 in the original manuscript) and consequently affect the results of CMAQ O3 predictions (e.g., Figures 8 and 9 in the original manuscript).

To our knowledge, no study has been conducted in any Asian mega city areas with a focus on the impact of different sources of PFT data on biogenic emission and ozone predictions. Therefore, we believe that our results can provide implications to atmospheric research community members who have interest in serious air pollution problems in Asian mega cities.

7.2 The statistical analysis, including the use of p-values and the F-statistics, needs to account for the very strong correlations between values in different grids and at different times.

We agree with the referee. The new results considering the spatial dependence of gridded values and spatial colinearity between explanatory variables will be presented in the revision.

7.3 Inter-grid transport of both the primary emissions and secondary products will be very large.

As the referee expect, inter-grid transport of both the primary emissions and secondary products will be very large due to high wind speed over finer modeling grid system.

7.4. The performance evaluation finds little difference.

Please refer our response to the comment 1.

References

Strahler, A., Muchoney, D., Borak, J., Friedl, M. A., Gopal, S., Lambin, E., andMoody, A.: MODIS land cover product algorithm theoretical basis document (ATBD) (Version 5.0), 1999 (http://modis.gsfc.nasa.gov/data/atbd/atbd_mod12.pdf).