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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Sensitivity of Biogenic Volatile Organic Compounds Emissions to Leaf Area Index and Land Cover in Beijing" *by* Hui Wang et al.

Anonymous Referee #1

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This manuscript describes a modeling study of biogenic VOC (BVOC) emissions from Beijing China. Since BVOC emissions are important for determining atmospheric composition and chemistry and are not well understood, this original study has the potential to contribute to the scientific understanding on this significant topic. The manuscript is difficult to understand in many places but that could be addressed with a thorough language editing.

The authors apply the MEGAN model, driven with WRF meteorology as is typically the case for MEGAN simulations. The most valuable part of the study is the investigation of the two of the main drivers of BVOC emissions: meteorology and landcover. For

meteorology, the authors compare temperature to observations and report a bias of cool temperature simulated by the model that is likely because the model does not adequately simulate the impact of the "urban heat island" on temperature. This is one of the more interesting results of the study and is a topic that the authors could potentially explore further with a more detailed examination and discussion of the canopy and leaf temperature simulations. For landcover, the authors compare different satellite based datasets. They do not compare with any in-situ observations so it is a sensitivity study with limited insights regarding accuracy and uncertainties.

The six main conclusions of the paper are listed in the conclusions section:

Conclusions #1 to 3 and #6 relate to the total emissions and the contribution of individual seasons. This would be of more interest if the study included some comparisons to BVOC emission measurements, so we have some idea if the emission results are correct. Since the paper does not include any observations of BVOC emissions, the MEGAN predictions of Beijing emissions should not themselves be the major focus of the manuscript. The current manuscript text (in the conclusion and elsewhere) devoted to describing the MEGAN model results (totals, seasonal and spatial variations) is too long and should be provided only as a brief description in the manuscript, and could perhaps be included in more detail in a supplemental section.

Conclusions #4 (LAI) and 5 (PFT) are the potentially more interesting contributions. However, there are several issues regarding the results and associated conclusions.

Page 11, line 24/25 states that MODIS LAI led to a 17.4% decline of total BVOCs compared with baseline in this study, because of the relatively big mask area in the MODIS LAI product. This is not a reasonable comparison. The mask indicates that no data is being provided for the masked region so it doesn't make sense to compare them. The default MEGAN LAI data on the MEGAN website replaces the MODIS LAI in the masked region with values based on an interpolation from the surrounding region. You could use this or some other approach but it is misleading to indicate that the

MODIS LAI is lower as indicated in the conclusion section and elsewhere (e.g., Figure 3).

Page 11, line 27/28 The statement, “Generally, the uncertainty of LAI is limited under the MEGAN model frame”, is unclear but seems to suggest that because the GEO and GLASS LAI data products are similar that means that LAI uncertainties do not contribute substantially to MEGAN BVOC emission uncertainties. This is not necessarily the case as it probably just shows that the two datasets are based on a similar approach (with similar errors).

Regarding conclusion #5, and the PFT comparison in general, the authors apparently consider only the relative contribution of PFTs to the vegetation covered regions and do not consider the differences in total vegetation cover. I assume this is the case since the PFTs in table 3 add up to 100% but I expect the vegetation cover in Beijing must be less than 100%. How does total vegetation cover differ between the three landcover databases? In addition, the conclusion #5 reports the PFT cover differences but does not provide any insights on which is the most accurate, how uncertain they are, and what the implications are for modeling. For example, how important is it to get the relative PFT correct in comparison to getting total vegetation cover correct or accounting for the variability of emission factors within each PFT (i.e., not all broadleaf trees have the same isoprene emission factor).

Finally, it is evident that the modeling exercise described in this manuscript generally supports the results and conclusions of a similar study by Ren et al. (http://dx.doi.org/10.1016/j.envpol.2017.06.049) for the same region (Beijing) that covers the same topic more thoroughly. The Ren et al. paper is not referenced in this manuscript which is not surprising since it was only recently published. However, it is important that the authors do compare with and discuss the results and conclusions of the Ren et al. paper and consider whether (and how) their manuscript adds any new information to the existing scientific literature.

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