

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2021-619

Anonymous Referee #1

Referee comment on "Land use and anthropogenic heat modulate ozone by meteorology: a perspective from the Yangtze River Delta region" by Chenchao Zhan and Min Xie, Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-619-RC1>, 2021

Comments on "Land-surface forcing and anthropogenic heat modulate ozone by meteorology: A perspective from the Yangtze River Delta region" by Zhan and Xie

General comments: This manuscript aims to examine the impact of different land cover (LC) data sets and anthropogenic heat on meteorology and air quality as well as on one of the most populated areas in the world. The manuscript is clear and well written. Despite that, I think this manuscript needs considerable improvement to be considered a novel and meaningful contribution to the existing literature on the subject.

Major issues:

Although the authors discuss in detail the impact of land cover, there are a lot of previous studies which study the impact of land cover datasets on meteorology and air quality. Therefore, this manuscript is just confirming what has been seen in previous papers (I would also like to mention Jiang et al., 2008, Xuemei et al 2009, Park et al., 2014, Fu and Liao, 2014). USGS is an outdated dataset, and the resulting model outputs are somehow expected, as the authors stated in line 427 "Obviously, higher O₃ was produced in the MODIS_noAH, indicating that urban expansion will increase surface O₃ concentrations." Since the authors did not perform a long-term analysis to see the impact of urbanization (MODIS LU) vs pre-urbanization (USGS LU) on meteorology and air quality, but rather a short-term simulation in which they evaluate the model against measurements, I do not see why they are using an obsolete dataset for the present-day simulation. It would be nice to analyze the impact of MODIS vs From-GLC_2015.

On a related note, the changes in predicted ozone are solely driven by changes in meteorology due to the use of different LC datasets. Have the authors considered updating the input MEGAN2 file for their simulation to match the LC data? The distribution of PFTs from the standard MEGAN2 input data are those from 2001. Therefore, the changes in the BVOCs emissions are exclusively influenced by changes in the meteorology since the PFTs remain unchanged throughout the simulations.

Specific comments:

Line 101: "the" instead of "The"

Line 152: delete etc

Line 184: briefly describe how the AH fluxes have been added

Line 217: why are you using 88 hours for spin-up?

Line 238: define adverse weather conditions

Line 251: define Meiyu since the reader might not know the local name of a frontal system

Lines 255-261: It seems that the authors made these statements using only a visual analysis of the data depicted in Fig.4. It would be nice to see the correlation between meteorological variables and MDA8 O3.

Line 300: Have the authors consider using the "topo_wind" option to turn on the surface wind correction?

Figure 7: please use filled dots showing the measurements instead of purple dots if the measurements are available. It is difficult to spot the lines AB, CD, and EF.

Figure 8: Add the modeled PBLH on the plot to sustain the sentence "The maximum O₃ production was in the middle of the boundary layer (~800 m) instead of at the surface"

Line 363: "we" instead of "We"

Lines 473-499: Some sentences belong to the introduction, or they are repeating the same information presented in the introduction

Jiang et al., 2008, doi:10.1029/2008JD009820

Xuemei et al. 2009, DOI:10.1007/s00376-009-8001-2

Park et al, 2014, <https://doi.org/10.5194/acp-14-7929-2014>

Yu and Liao, 2014, <https://doi.org/10.3402/tellusb.v66.24987>