

Interactive comment on “Land cover change dominates decadal trends of biogenic volatile organic compound (BVOC) emission in China” by Hui Wang et al.

Anonymous Referee #1

Received and published: 14 April 2020

General comments: This paper presented the MEGAN-simulated biogenic volatile organic compound (BVOC) emissions in China and analysed the modelled contributions from changes in land cover and climate to the BVOC emissions. The modelled variations in isoprene emissions were further linked to the HCHO vertical column. The paper is well-written and has delivered the message about the potential importance of land cover changes in BVOC emissions in China. The current format of the manuscript has been much focused on analysing the patterns simulated from the four different scenarios, but rather limited in understanding the uncertainties (e.g., uncertainties from satellite products or assigned emission factor or missing PFT) associated with the model simulation. Then when the authors linked their simulated isoprene emission with the

C1

HCHO vertical column, the disagreement of these two has been mainly attributed to the AVOC, but I would think there could be also contributions from the uncertainties in the simulated BVOCs. From the maps with simulated BVOCs, I am a bit surprised to see that the north part of China with high LAI showed very low simulated emissions, especially monoterpene. Could this be linked to the misclassification of forest type? Then in the east and/or at least North China Plain area, there is wide distribution of crops. Are crops specifically considered in MEGAN? In general, a map showing the spatial distribution of PFTs could be very useful for readers. I also think it is crucial to compare the modelled emissions with a few sites' measurement data to illustrate the performance of the model before digging into analysing the changes of the emission patterns at the national scale and further linking to the HCHO column data.

Specific comments:

P2 L5-6, please indicate at which spatial scale we can see cropland dominates the reduction of isoprene.

P2 L10, the authors mentioned that the greening in China has been linked to “maintain and expand forests”. Did they change plant species when expanding forest? And can you see this level of land use change in the MODIS PFT product?

P3 L2, suggest to delete “accurately”. You have not evaluated the modelled BVOC against the measurements.

P4L2-4, here you might need to specify where these emission factors are from? How much of these emission factors covered the measurements from China? I did a quick google search and could already see some measurement data available for different ecosystems in China.
<https://www.sciencedirect.com/science/article/pii/S1352231017302947>
<https://www.sciencedirect.com/science/article/pii/S1352231015305173>
<https://www.sciencedirect.com/science/article/pii/S0269749119346081?via%3Dihub>

C2

P4 L8, “The Cce(=0.57) is a factor to xx” what does this mean?

P4 L9, How can LAI define leaf age in MEGAN?

P4 L13, Is soil moisture used as inputs for model? If so, please clarify.

P4 L17-18, LAI is a ‘modelled’ product from other satellite products and potentially has large uncertainty in itself. I wonder if the LAI has been filtered by the quality flags before using as inputs for MEGAN and how the model deals with the LAI gap if there is no data for many 8-days? P4 L21-23, Could you list what PFTs you have in your simulations (or showing a map), and also how MODIS PFTs were reclassified to the CLM group? I think this information is important for readers to understand the spatial pattern.

P7 L18-19, the reasons why the simulated MT is so much lower than the previous estimations needs to dig in-depth. Like I mentioned early, could it be linked to the misclassification of PFTs or very different emission factors assigned? In Table 3, the modelled isoprene is very low than Li et al., 2013, can the authors describe a bit about why?

P9 L23, might need to add one or two sentence in the method section why $p > 0.9$ is statistically significant. I did not get it here.

P12 L11-12, “The lack of long-term in-situ observations of BVOC in China...” I think this might be the case for most of countries where we don’t have dataset being representative at the whole country level, but I think the authors should definitely compare the modelled with in-situ data for a few representative sites to evaluate the model performance. In China, there are some sites where you can find the ecosystem-level BVOC measurement data for comparison, like some links I provided in the previous comments.

P12 L12-18, this part should be in the method section.

P13 L5, “. . . are marked with black dots” it is difficult to see these dots though.

C3

Conclusion, it is rather lengthy at this moment and includes large section of discussion as well. Please make it more concise.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-28>, 2020.

C4