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# *Interactive comment on* "Estimation of biogenic volatile organic compound (BVOC) emissions in China using WRF–CLM–MEGAN coupled model" *by* Lifei Yin et al.

**Anonymous Referee #1**

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Yin et al present an emissions inventory for some BVOC across China, using the BVOC emissions model Megan and WRF-CLM. A number of previous studies have already presented emission inventories for China, as also mentioned by the authors. While their approach is somewhat different, I cannot get overly excited about the analysis as it is currently presented. It seems methodologically largely sound (but see my questions below); but apart from providing yet another BVOC emissions map there is little novelty in the study. Isoprene and MT emissions are largest from forests, most of the MT come from conifers; spatial patterns thus depend on weather as well as land cover, temporal patterns on weather and LAI âĂŤ all of this has been found in many other studies before and the results are no surprise given the main BVOC algorithms that basically



vary standard emission factors (which depend on the PFT) with weather.

I would think that at least some simulation experiments that investigate emission changes historically and/or in future, in response to climate change, CO2 and/or land use change would be needed to warrant publication of the work. Given that the model system is in place this should not be too much effort and would make the paper somewhat more interesting.

While I agree that high spatial resolution is an advantage when estimating BVOC emissions I do not follow the need to use a coupled WRF-CLM model version. This would have been needed if the authors wanted to do surface-atmosphere feedback experiments. But at the moment, all they do is to drive an BVOC emissions model with simulated high-resolution weather. This could have been done in offline experiments just as well, especially since the vegetation in this study is prescribed from MODIS.

Lines 68-70: This statement is plain wrong. There are several approaches published in which authors have incorporated BVOC emission algorithms into ecosystem models that calculate also leaf gas exchange, and vegetation dynamics. See e.g. Unger et al., ACP 2013; Pacifico et al., ACP 2011, doi:10.5194/acp-11-4371-2011; Arneth et al., ACP 2007; Schurgers et al., Biogeosc. 2009. And there's presumably others. The authors will need to do their homework more thoroughly. And since the authors didn't even use the dynamic vegetation module of CLM but prescribed land cover and LAI their claim that the study presented here is so much more 'accurate' (see line 79) than previous estimates seems overstated.

MEGAN defines canopy-level emission factors for multiple compounds. What is the observational evidence, if any, for compounds such as myrcene, ocimene, sabinene etc? Where and what types of have canopy-scale measurements been made that would in fact support the value specified? And if such measurements are scarce/non-existent, what justifies their use in a large-scale inventory?

Guenther et al. in their 2012 paper claim that they describe how leaf age fraction is estimated in section 2.4 of their paper, but section 2.4 has no mentioning of leaf age calculation at all (neither has any other section in their manuscript). The authors need to describe how leaf age (new, growing, mature and senescing categories) can be differentiated (realistically) from a remote sensing product such as MODIS.

Worden et al (ACP, 2019, https://doi.org/10.5194/acp-19-13569-2019) use MOPITT CO to infer isoprene emissions. While they don't present numbers for China, I wonder if it would not be worthwhile to approach these authors to find out if an emission map for China could be obtained in order to compare with the simulations shown here.

The section 'uncertainties' is somewhat thin. Nothing in it is wrong but it doesn't provide a lot of substance and the section reads a bit as an after-thought at the moment. Needs more concrete examples and/or even example simulations. And the section 'conclusions' is in fact merely a summary section, which could be removed, as the text contains a lot of repetition of what has been said elsewhere in the manuscript.

Scientific papers should be apolitical. Yet I note that Taiwan (Republic of China) is shown on the maps presented. Moreover, while not all of the islands in the South China Sea are shown on these maps (the Paracel islands and Pratas seem to be included) there are several occasions in the paper in which the authors state 'the small islands in the South China Sea are not included', implying that these should be counted. These islands are contested territory and the political status of Taiwan is also a non-consensual one. The authors write about emissions from China, and not emissions from the People's Republic of China, but in the day-to-day use of China the term is synonymous with PRC. I am concerned that misunderstanding might arise from this, and therefore suggest to restrict the reporting of numbers and maps to mainland China only.

A number of more minor issues: Line 36 – sounds as if global BVOC emissions are known to be 1150 Tg C a-1, which is not the case. There are huge uncertainties and no global observations. Revise. Line 50 '…that cannot be reproduced well in canopy



environment models'. Unclear what is a canopy environment model. Lines 62, 66 and possibly elsewhere: hydrological cycle (not hydrologic cycle).

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