

Geosci. Model Dev. Discuss., referee comment RC2  
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## Comment on gmd-2021-29

Anonymous Referee #2

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Referee comment on "Modeling sensitivities of BVOCs to different versions of MEGAN emission schemes in WRF-Chem (v3.6) and its impacts over eastern China" by Mingshuai Zhang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-29-RC2>, 2021

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This paper describes differences in BVOC emissions resulting from different versions of the MEGAN model, along with the effect on ozone and other trace gases over portions of China. Differences due to the specification of vegetation types are also examined. Parameterizations developed for specific processes in air quality models, such as online treatments of biogenic emissions, continually undergo modification as new information is available to constrain the relationships used by the parameterization. So, it is not surprising that the estimated emission rates are different among the versions of MEGAN. The other conclusion is that specification of the subgrid vegetation fraction is also important, which was already shown in Zhao et al. (2016) (and noted in lines 253-254) who is a co-author on this paper.

The authors conclude that their results show there is still a large uncertain range in modeling BVOCs (line 61); however, this begs the question: What is uncertainty? In terms of the paper presented, the range of results are from different versions of MEGAN. This sort of model-to-model uncertainty might be even larger if one considers other treatments of BVOC emissions (listed in lines 88-91). But what is the value of using an older version of MEGAN when a newer version is available and has presumably been shown to perform better and/or have more physical processes represented than the older version? What is lacking here is a comparison of predicted BVOCs with observations to truly understand uncertainty. Maybe there are no observations of BVOCs such as isoprene or monoterpene in these regions for the simulation periods. Perhaps the simulations could have been done for periods when such observations are available and/or for different parts of China is needed. I would assume that at a minimum some air quality data, such as ozone and NO<sub>x</sub>, would be available to compare with the model results to indirectly assess the effect of BVOC emissions. Section 3.1.3 seems to suggest there are some observations, but they chose not to show any results. In terms of specification of vegetation, one can see differences in the vegetation classifications between the dataset

and conclude that those will result in different BVOC emissions without even running MEGAN. There should be satellite derived products that could be used to understand differences in the types and spatial distribution of vegetation between observations and datasets used by MEGAN.

In addition to chemical observations, an evaluation of the predicted surface meteorological quantities (temperature, precipitation, soil moisture, radiation) using observations would have been useful to understand how those uncertainties would influence the predicted BVOCs. A time series of observed and simulated meteorological quantities over the month-long period compared with predicted BVOC emissions would have been useful. The month-long period would also make simulating soil moisture challenging. There are satellite products that could be used to assess soil moisture variability over the region. As pointed by the authors, the major difference in MEGAN v3 is the inclusion of a drought activity factor (lines 239-240), but the importance of this factor for the 2 month-long simulations is discussed only briefly (line 486-495) in terms of seasonal changes. The authors do not say whether or not April or July of 2015 are periods characterized by drought. The authors note that the drought factor is constrained by limited observations (line 242) and may not be suitable for China (line 244). So how is one supposed to assess the seasonal impacts of this parameter compared to other parameters in MEGAN (i.e. Figure 7)? Perhaps some sensitivity simulations are needed with a range of values for the drought factor to get a better idea of its potential effect.

In summary, while I appreciate having differences in the physical representation among the MEGAN versions discussed in one paper, my issues with this paper are 1) the primary main findings have been described previously and 2) there is a lack of any observations needed to fully understand model uncertainty. Section 3.1.3 implies there are observations. If the authors choose to include only modeling analyses, the paper needs to be revised to frame the purpose of the paper better and improve Section 3.1.3 to put their results into the context of what might be observed in the real world, what is needed to assess components of that latest version of MEGAN, and what are the most missing or uncertain treatments – apart from the specification of vegetation.

In addition, the abstract has numerous awkward phrases and I pointed out those in the specific comments below. However, I did not point out other instances throughout the paper.

#### Specific Comments:

Line 37: The phrase “over East China” seems out of place and could be deleted. The first part of the sentence actually applies to many places in the world, not just East China. The authors should true to put the paper into a context for a broad range of readers whenever possible.

Lines 37-38: Change "are generally resulted from", which is an awkward phrase, to "are primarily due to".

Lines 38-39: Change "in model" to "in the model".

Line 38-40: Change this sentence to "One originates from different treatments in the physical and chemical processes associated with the emission rates of BVOCs."

Line 40: Change "from the biased distribution of vegetation types" to "errors in the specification of vegetation types and their distribution"

Lines 47-48: Change "biogenic VOCs" to BVOCs which was already defined.

Line 59: The authors use "significantly" here, but that is a vague term. Can they provide a number to quantify range?

Lines 61-62: This sentence just states that the three versions of MEGAN produced different results, which is not surprising since updates usually include new treatments based on observations or theoretical relationships. Presumably, older versions have out-of-date information and should be less accurate. But at this point, the abstract fails to say anything about how the simulated BVOCs compares to observations which is a better way to characterize uncertainty.

Lines 166-182: This section does not list the land-surface parameterization used. As noted by other studies MEGAN2.1 (called here MEGAN3.0) is coupled to the CLM land surface parameterization. The other versions of MEGAN can use different land-surface parameterizations. So it is not clear whether differences in the land-surface treatment, which will affect the surface energy budget and fluxes, will affect the BVOC emissions. While Zhao et al. (2016) found that differences due to the land-surface parameterization were of secondary importance compared to specification of the vegetation, that point on the model configuration should be cleared up. Also, I assume that meteorological predictions should be identical among the simulations? That should be explicitly stated.

Lines 512-513: This is a very general statement. Which results are the authors talking about? BVOC concentrations or some other quantities?

Lines 514-515: This statement needs to be backed up by the evidence that is not shown.

Lines 583: There can be anthropogenic isoprene emissions so how did the author separate out the biogenic source of VOC concentrations versus the anthropogenic sources?

Line 551: The authors need to state how the calculation of ozone from biogenic and anthropogenic sources are separated out. I assume this is a highly non-linear system and it is not easy to separate out these sources since biogenic emissions will influence the anthropogenic system and visa versa as emission sources mix. This comment also applies to NO<sub>x</sub> sources in lines 562-563.

Line 580: At the end of Section 3, there needs to be some discussion regarding the meteorological conditions during the simulation periods in 2015. Some the figures (6,7,9) plot differences between July and April. Presumably the seasonal changes are dependent on meteorology which might change from year to year. The authors should discuss whether these differences would be representative of other years and why.