EGUsphere, referee comment RC2
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Comment on egusphere-2022-436

Anonymous Referee #2

Referee comment on "Satellite-derived constraints on the effect of drought stress on biogenic isoprene emissions in the southeastern US" by Yuxuan Wang et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-436-RC2, 2022

This study employs the GEOS-Chem CTM (driven by MEGANv2.1 biogenic emissions) along with OMI HCHO measurements and ground-based flux measurements to derive constraints on the isoprene emission response to drought stress in the southeast US. The authors then implement an updated drought stress parameterization in MEGAN to investigate the impacts of drought-associated isoprene emission reductions (as compared to the baseline MEGAN implementation) on air quality.

I found this paper to be interesting and generally well-written; it is a nice application of a long-term satellite data record to advance our understanding of isoprene emission processes and improve our ability to simulate those processes in models. However, I think the paper needs more discussion and evaluation of the uncertainties associated with NOx biases in the GEOS-Chem simulations used here, and how they might impact interpretation of the results. I also think additional space should be devoted to more explanation of the potential impacts of the bias correction applied to the OMI HCHO data, and the other data adjustments that are performed. Specific recommendations are listed below. After these revisions I would recommend publication.

Specific comments:

- Line 45-47: Travis et al. (2016) showed that NEI2011 emissions are biased high in the SE US, and Kaiser et al. (2018) thus applied a 60% reduction in NEI2011 anthropogenic NOx sources (other than power plants) to account for this in their OMI HCHO-based optimization of isoprene emissions over the region. Have the authors applied similar NOx adjustments in their simulations here?

- Line 127: Do the authors have thoughts as to what type of uncertainty is introduced by using a single bias correction factor for the long-term OMI HCHO record used here? The 1.59 factor was derived by Zhu et al. (2016) with respect to aircraft measurements taken in a specific summer (2013), however, I wonder if temporally-varying biases are possible given that the HCHO background likely changes with time.
- Line 149: Why not just sample the model at approximately the time of the OMI overpass? It seems like this would be more robust than scaling the daily mean data by a single conversion factor everywhere and at all times. Can the authors discuss potential uncertainty associated with this assumption?
- Line 205-207: While I agree with the authors that isoprene is probably the dominant "missing process", is there any literature that discusses changes in other factors (e.g., biomass burning, mixing height, etc.) during SE US drought that the authors can point to here? I see in Fig. 4 that they ruled out changes in anthropogenic VOCs, which is a helpful addition.
- Lines 219-221: Could domain-mean temperature also be added to Fig. 4? If the more severe drought time periods are also warmer, then the increase in isoprene emissions is not really surprising despite the LAI reductions, given the strong exponential dependence of those emissions on temperature.
- Line 224-228: I don't typically think of the SE US as a low NOx environment; however, even if it were, I think the buffered response is misrepresented as described here. It simply reflects the fact that HCHO is less sensitive to OH variability because it's loss to photolysis still occurs at low OH. However, the HCHO yield from isoprene varies as a function of NOx (and, thus, OH), so any NOx bias in the model can lead to an HCHO bias. Even after their NOx emission adjustments as discussed above, Kaiser et al. (2018) demonstrated that spatially-varying NOx biases lead to biases in the modeled HCHO column in the SE US. Have the authors evaluated the model NOx in their study region? I think a comparison to OMI NO2 would be a very nice addition to this paper, and would strengthen the argument that the overestimate of HCHO in the model is due to emissions and not a bias in the formation rate of HCHO.
- Line 302: The discussion here is confusing—the authors talk about "downscaling" the GC emissions for comparison to MOFLUX, but what they've actually done is scale them up by 1.42, correct? It sounds like this number represents the mean MOFLUX-GC relative bias during N0?
- Figure 6b: I find it very difficult to see the direction of change between Nostress_GC and MOFLUX_stress_GC in this scatterplot. Suggest either having two panels or maybe adding the MOFLUX_stress_GC predictions as an additional line in Figure 6a.
- Lines 326-345 and Figure 7: I think these LAI-normalized HCHO vs temperature curves are a nice way to show the data, but I'm not clear as to why the model predicts an increasing temperature dependence as drought severity increases in the model. Isn't the temperature dependence a fixed function for each plant type in MEGAN? Does the variation reflect the temperature "memory" effect on emissions that presumably increases during drought (i.e. I think MEGAN actually accounts for the previous two weeks' temperatures or something along those lines?) or is it something else that's changing (such as an increase in clear, sunny days as the drought progresses, which would increase PAR)? Can the authors discuss this a bit here?
- Lines 382-383: Can the authors discuss a bit more why they think the MOFLUX comparison doesn't do a good job representing drought stress dependence across the SE US? Is there something unique about the Ozarks ecosystem that doesn't apply across the region more generally?
- Figure 10: I wonder if it would make more sense for the third column to be OMI_Stress_GC – OMI HCHO to demonstrate the improved agreement?
- Section 5: These results are interesting and a nice addition to the paper, but I'm not sure they demonstrate applicability over regions outside the SE US. Consider perhaps restricting the comparisons just to that region? Also, I think a discussion of how the authors deal with NOx biases in the model would help in the interpretation of these results.

Technical comments:

- Line 27: By “non-uniform” I think the authors mean “non-linear”?
- Line 28: “trend of increase” is awkward. Do you mean “increasing trend”?
- Line 67: Consider editing the beginning of this sentence to “With wide spatiotemporal coverage, satellites provide…”
- Line 72: I would add “e.g.” before the citations here, as this is far from an exhaustive list.
- Line 79: Change “the monthly” to “a monthly”
- Figure 2: I would label the panel titles and the axes with the same names used in the text (GCHCHO_Nostress and OMHCHOd). Also, the units should be denoted as “molec cm^{-2}” instead of “mole cm^{-2}” for this and all other figures.
- Figure 3: I would label the GEOS-Chem panels (b and c) as “Nostress_GC” since this is how it is denoted in the text.
- Figure 4: Label the curves corresponding to GEOS-Chem as “Nostress_GC” to avoid confusion.
- Line 147: Change “simulating” to “simulated”
- Line 170: “regions” should be “region”
- Line 193-194: This sentence is awkward. Consider revising.
- Figure 7: Label the curves corresponding to GC as “Nostress_GC”
- Line 331: “sensitives” should be “sensitivities”

References:

Kaiser, J., et al. High-resolution inversion of OMI formaldehyde columns to quantify isoprene emission on ecosystem-relevant scales: application to the southeast US., Atmos. Chem. Phys., **18**, 5483-5497, doi:10.5194/acp-18-5483-2018 (2018).

Travis, K. R., et al. Why do models overestimate surface ozone in the Southeast United States? Atmos. Chem. Phys., **16**, 13561-13577, doi:10.5194/acp-16-13561-2016 (2016).