Modeling on the drought stress impact on the summertime biogenic isoprene emissions in South Korea

Jeong et al., 2024

This manuscript investigates the potential to constraining biogenic isoprene emissions under both normal and drought conditions by incorporating drought stress into the modelling with GEOS-chem. The study highlights that previously implemented algorithms used for the southeastern United States are not directly applicable to the South Korean peninsula. Instead, the authors utilized the IFDMB framework to derive an empirical equation tailored to the South Korean region, significantly enhancing prediction accuracy under both normal and drought conditions. Furthermore, this approach also improved the prediction of other secondary pollutants under these conditions. Throughout the study, the methods and results convincingly highlight the scientific significance of this work, offering substantial findings that underscore its value for modeling applications. Also, this research provides valuable insights to enhance model predictions in regional air quality (AQ) contexts.

I would recommend a list of points and concerns which needs to be addressed.

P1. Throughout the manuscript, the authors use absolute/percentage number in the text, making it difficult and complicated for readers to follow every detail. They should refine how these results are presented. For example, they could utilize the unused white space in the spatial plots (panels) to display relevant values or add more tables with columns showing results before and after implementing the algorithm. Overall, improving the readability of the manuscript would be a substantial value addition.

P2. It is assumed that a drought index is used solely to identify drought conditions. Why were ETDI or DEDI chosen here when other drought indices are also available? Some insight on this would be valuable because DEDI is essentially an agricultural drought index best suited for short-term developing droughts, although it can also capture longer-term drying conditions (Narasimhan and Srinivasan 2005; Singh et al., 2024). Consequently, its application to vegetation---especially forests with deeply rooted trees---may not detect short-duration events. It is assumed that the developed and tested algorithm would be applicable regardless of the drought's duration and severity. Adding some discussion on this would be helpful.

P3. This study demonstrated that the algorithm developed and proven effective over the SE US are not equally effective over the SK region. Authors can pull some insights over the possible reasons behind this. Is it due to different types of vegetation over these regions or any other factors at play? As mentioned in P2, if the emission response is moderated by different vegetations types, then improvements claimed by the new empirical algorithm may be dependent on the types (hydrological or meteorological) and severity of drought, and thus be more region-specific (e.g., algorithms over the southeastern U.S.).

P4. Authors has reported the results % change across the manuscript. Authors should also provide some estimate of uncertainties over the region. I.e sensitivity of new approach.

P5: The authors should verify the calculations for the percentage change in emissions reported in Table 1 (e.g., isoprene emissions for the standard case, and HCHO). It appears that the percentage change between normal and drought conditions was computed using values with more decimal precision than those shown in Table 1, which only displays two decimal places. Consequently, the percentage changes in Table 1 differ by about 1–2% from the values one would obtain using the tabulated data. The authors should ensure consistency and revise these figures throughout the manuscript after cross-checking.

P6. Figure 5 and its description (section 4.2) is really confusing and hard to keep track when previous figures are referred. I assume in figure 5, row 1, panel 2 (drought) should have the IFDMB? Also, Fig 5c shows the box-whiskers for HCHP different approaches for all GEOS-chem simulations. how many? What would be the significance of these bar/statistics? This needs some efforts to make it more explanatory.

P7: Line 418: “presented in table 2.” Table 2 is missing in manuscript. This is vital.

Specifics: -

1. Line 10: - I suggest authors to rephrase this sentence to frame the importance of reducing the uncertainties in these emission context to SK region instead of directly stating the effort has not been made in SK region. This will better abstract the requirement and gap in knowledge in context to the region.
2. Line 40: - “some studies” sounds vague here, better cite them here. Also, in line 43 if “some previous studies” refers to the cited in the end of sentences (line 46), author can rephrase this like “recent studies….”
3. Line 112: “factor of 1.28*.. “sounds like a sudden introduction of something important. Presumably this comes from the Shen et al., 2019; Wang et al 2022b. I suggest a rephrasing of this sentence for better connectivity and explanation.
4. Line 326: “fig 3.c”. please check figure 3 if “c” is marked there?
5. Line 324” “geographical characteristics over the South Korean” please elaborate the context of geographical condition here. It is related to vegetation or climatic features.

References:

Narasimhan, B. and Srinivasan, R.: Development and evaluation of Soil Moisture Deficit Index (SMDI) and Evapotranspiration Deficit Index (ETDI) for agricultural drought monitoring, Agricultural and Forest Meteorology, 133, 69–88, https://doi.org/10.1016/j.agrformet.2005.07.012, 2005.

Singh, R., Tsigaridis, K., Bull, D., Swiler, L. P., Wagman, B. M., and Marvel, K.: Mount Pinatubo’s effect on the moisture-based drivers of plant productivity, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2024-2280, 2024.