General Comment:

This study extends MEGAN v3.2 beyond conventional PFT-based representations by developing a species-resolved BVOC emission inventory for 234 dominant tree species covering over 90% of China's forest area. By integrating forest inventory data, MODIS products, and emission measurements from 184 studies, the authors establish a localized MEGAN framework and a compound-specific emission factor database for isoprene, monoterpenes, and sesquiterpenes. The study further evaluates two afforestation scenarios and demonstrates that species selection can substantially alter both the magnitude and chemical composition of BVOC emissions. These results provide improved biogenic emission inputs for ozone and BSOA modeling and useful guidance for coordinating afforestation with air-quality management.

The overall framework and experimental design are sound. However, several methodological details, including parameter definitions, dataset suitability, and figure annotations, require clarification and standardization. I therefore recommend acceptance after minor revision.

Specific Comment:

1. In Equation (1), the canopy production and consumption correction parameter (ρ) is introduced only with a nominal definition, without any calculation rules or supporting literature; since this parameter is central to BVOC canopy loss correction, the omission of its specification undermines model reproducibility, and the authors should provide the calculation formula for ρ (or its parameterization scheme) and cite the original reference(s) that define this term, or explicitly state that the parameter is adopted directly from MEGAN v3.2 without modification and provide the appropriate citation.
2. In Equation (3), the canopy environment coefficient is specified only to the extent that $\gamma = 1$ under standard conditions, with no fixed numerical value or justification for the chosen setting; as this coefficient serves as the baseline for all environmental activity factors, its value must be reported to ensure reproducibility, and the authors should clearly state the fixed numerical value adopted in this study, indicate the source from which it is derived (e.g., MEGAN default or literature), and briefly justify its appropriateness for the Chinese forest context.
3. In Section 2.2.1, the authors describe the operational procedure of equally splitting mixed-stand polygon areas among co-dominant species but do not explicitly acknowledge that this simplification introduces estimation bias; since China's forest inventory does not provide species-level stocking fractions for mixed stands, equal-proportion splitting is a pragmatic compromise, yet without prior disclosure of this limitation, readers may interpret the results as absolute rather than as the best available estimates, so the authors should explicitly state in Section 2.2.1 that the equal-proportion assumption is adopted due to the lack of species-level area ratios and briefly note that this approach may introduce allocation bias, while also adding a dedicated statement in the Discussion (Section 6) addressing this uncertainty and its potential implications for the emission estimates, consistent with the existing discussion of tree age and stress-response uncertainties.
4. In Section 2.2.2, the specific leaf area (SLA) data used to convert emission rates into emission factors are drawn from the European FloraVeg.EU database, but the authors do not justify the suitability of this database for Chinese tree species; given that leaf morphology and phenology differ substantially between European and native Chinese species, the use of substitute SLA data may introduce systematic errors on the order of 5–10%, so the authors should add a

justification statement clarifying that where domestic SLA measurements for Chinese species are unavailable (which applies to the majority of the 234 species), SLA values from congeneric species in East Asia were prioritized, and when no East Asian congeneric data existed, values from European congeners were adopted, while also acknowledging that this substitution may introduce a systematic bias of approximately 5–10% in the derived emission factors and considering adding a sentence in the Discussion to acknowledge this source of uncertainty.

5. In Section 2.2.2, the "species measurement → congeneric mean → family mean" three-tier substitution rule for species lacking observational emission data is described only in text; given that this substitution hierarchy is a core procedure for constructing the emission factor database for 234 species, the purely textual description reduces readability and reproducibility for other researchers who may wish to adopt or adapt the database, and the authors should add a concise supplementary table (e.g., Table C0 or in Appendix A) that visually summarizes the priority hierarchy of the three-tier substitution rule, with example cases illustrating how values were assigned for representative species, which will greatly improve the transparency and reusability of the database.
6. In Section 2.2.2, the procedure used to combine emission rates (K values) from multiple literature sources is not sufficiently clear. The manuscript does not indicate whether canopy-scale flux measurements from Chinese forest ecosystems were prioritized over leaf-level chamber measurements, or whether domestic studies were given preference over international sources. Because canopy-scale observations are generally more representative of local environmental conditions, the authors should explicitly define the data-selection hierarchy. Measurements from Chinese forest ecosystems should be given the highest priority, followed by leaf-level measurements for Chinese tree species, and then by international studies and the default MEGAN database. When multiple values are available at the same priority level, the arithmetic mean should be used unless an alternative choice is clearly justified. Clarifying these rules would improve methodological transparency and facilitate independent replication.
7. In Section 2.3.1, the 78 Mha potential afforestation area is presented with its total area only, but the authors do not specify the underlying data source(s) nor the screening criteria (e.g., slope constraints, ecological redlines, pre-existing native vegetation, land-use competition) used to define this potential area; since this boundary directly determines the emission increment magnitude under the A&R scenarios, the lack of transparency reduces the credibility of the scenario analyses, and the authors should fully document the delineation criteria and data sources used to define the 78 Mha potential afforestation area, explicitly referencing the original methodology from Xu et al. (2023) and including, at minimum, the base datasets used, the exclusion criteria applied (e.g., slope > 25°, existing cropland, urban areas, ecological conservation zones), and any additional filters relevant to the Chinese afforestation policy context, which will fully anchor the scenario design in the peer-reviewed literature and enable proper interpretation of the emission increment estimates.
8. In Section 2.3.1, the BIO (biomass-maximization) scenario is described in terms of its species selection logic (carbon storage optimization), but its ecological limitations are not mentioned; without an explicit caveat that this scenario is optimized solely for carbon storage at the expense of biodiversity conservation, soil erosion control, and other ecosystem services, policy-oriented readers may misinterpret the BIO scenario as the preferred afforestation pathway, and the authors should add a brief sentence immediately following the BIO scenario description, such

as: "It should be noted that the BIO scenario is optimized solely for aboveground carbon accumulation and does not account for other ecosystem services, including biodiversity conservation, soil erosion control, or water provision," which will provide balanced context and prevent misinterpretation of the scenario as universally optimal.

9. The WRF meteorological simulations are performed for the period 2018–2022, yet the BSOA evaluation uses only 2018–2020, and the truncation of the final two years is not explained; the temporal mismatch between simulation and evaluation raises questions about the experimental design logic and may confuse readers, so the authors should add a brief explanation to Section 2.4 (or Section 3.2, as appropriate) clarifying that ground-based BSOA observations were unavailable for 2021–2022 for the 29 monitoring stations used in this study, and therefore the evaluation period was necessarily limited to 2018–2020, which simply clarifies that the evaluation reflects the period for which sufficient observational constraints were available, rather than a deliberate data omission.
10. Exist-SPE uses a 5-year mean (2018–2022), whereas Exist-IND, BIO-IND, and SUIT-IND use only a single year (2019), and the authors do not explain the rationale for this difference in temporal averaging across scenarios, which may puzzle readers; the authors should add a clear explanatory footnote or parenthetical note below Table 1 (Simulation Scenarios), specifying that Exist-SPE uses the 5-year mean to align with the multi-year observational datasets used for model evaluation (BVOC and BSOA), reducing the influence of interannual meteorological variability on validation metrics, while the IND scenarios use a single year (2019) with fixed meteorology specifically to isolate the effects of species composition and forest cover changes from climate variability, thereby enabling a clean attribution of emission differences to vegetation characteristics alone, which will eliminate any ambiguity regarding the scenario logic.

Technical Comment:

1. In Figure 1, abbreviations such as EF, LAI, and A&R appear for the first time without their full expanded forms, and the authors should expand all abbreviations in the figure caption at first appearance (EF = emission factor; LAI = leaf area index; A&R = afforestation and reforestation; etc.), or alternatively, add the expansions directly within the figure elements, which will improve the figure's self-contained readability.
2. The manuscript uses the terms “species-explicit” and “species-resolved” interchangeably, with the former appearing in the title and the latter used throughout most of the main text. This inconsistency affects the clarity and coherence of the manuscript. The terminology should be standardized throughout. Since “species-resolved” is more widely used in atmospheric modeling and already appears more frequently in the manuscript, I recommend adopting it consistently, including in the title, for example: “Species-resolved modelling of forest biogenic volatile organic emissions across China.”
3. In Table C4, numerous tree species are assigned zero values for isoprene and/or monoterpene emission factors, and readers may incorrectly interpret these zero values as indicating that these species emit absolutely no BVOCs, whereas in reality, a value of zero reflects that emissions are below the instrumental detection limit under standard measurement conditions, but trace emissions may still occur under field conditions; the authors should add a uniform explanatory note beneath Table C4, with wording to the effect that emission factor values of zero indicate

that emissions were below the instrumental detection limit in available measurements and should not be interpreted as zero emission under all conditions, as trace-level emissions may occur in the field depending on environmental conditions, which will prevent misinterpretation of the emission factor database.

4. Lines 179–182 repeat the aggregation procedure. This passage should be consolidated and rewritten more clearly.
5. Line 272: avoid “While … but”.