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**Interactive
comment**

Interactive comment on "Evaluation of regional isoprene emission factors and modeled fluxes in California" *by* Pawel K. Misztal et al.

Anonymous Referee #1

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The importance of accurately estimating isoprene emissions for simulating photochemistry and aerosol formation in chemical transport models is well recognized. Yet, after two decades, the uncertainty of isoprene emission estimates still hovers around a factor of two. If the results from this study are upheld, the bias will have been significantly improved, although there is still considerable variability between model and observations for specific events. This paper builds off efforts to reduce this uncertainty by analyzing "measured" airborne isoprene fluxes over a range of ecosystems (from low emitters to higher emitters) over a large portion of California. The usefulness of this paper hinges strongly on the airborne flux technique introduced by Misztal et al. (2014). Overall, the results of the paper should be of interest to the ACP reader community, and with modifications, the paper is recommended for publication in ACP.

It was unclear why three models (BEIGIS, MEGANv2.04, and MEGANv2.1) were eval-

uated with integrated ecoregion fluxes, while one model (CARB) was evaluated with footprint data. Because of the uncertainty in the size and shape of the footprints, it is recommended that the evaluation focus on all four models versus the integrated flux data across each ecosystem. Because of the importance of trying to establish reliable airborne derived footprints for a variety of other trace gases (such as CO_2, CH_4, and N_2O), maintaining the exploratory footprint section would be of interest to the community.

Specific comments:

Abstract: I am not a fan of abstracts written with paragraphs and references, but that is my personal preference. Suggest consulting ACP style guide to see if abstract follows protocol.

p.3, line 25: Since the CARB model was "improved" using the CABERNET measurements, evaluating it versus the other models using CABERNET measurements doesn't seem fair. Was any of the data set held out for an independent evaluation?

p.4, lines 6-11: Use of the WRF model to provide temperature and PAR is mentioned. And it is mentioned that the temperature data are compared against observations. How reliable is the PAR data? Because of aerosol effects, we have found that PAR from WRF can easily be overestimated by 10%.

p. 4, line 14: Sentence that begins with "BEIGIS" is fragmented and should be edited.

p. 5: Fundamentally, isoprene emissions from all four models depend on the normalized isoprene emission fluxes assumed for quercus. Either in this section or in the supplemental materials, please summarize the normalized isoprene fluxes assumed for each model, whether they are assumed at the leaf, branch, or canopy level, whether fluxes vary by quercus species, and any references to support the flux values. Similarly, isoprene fluxes depend on quercus leaf biomass densities. How do these vary among the four models? Perhaps, one way of comparing the three/four models would

be a table summarizing the attributes of the models that account for the differences in estimated isoprene fluxes. The narrative as currently written only provides a superficial insight into model differences.

p. 5, line 19 and other locations: In most situations, "which" should be preceded by a comma.

p. 5, section 2.1.4: Building on the "p. 5" comment above, more detail is needed to adequately describe the CARB hybrid model. Also, the sentence that begins "This regional model most closely agreed with the measured fluxes . . ." is confusing. My first reaction was why was a conclusion offered before the analysis and results sections. Then, I realized that the model had already been calibrated using the study data. This calibration needs to be clearly described here. All in all, this section is awkwardly written and needs to be reworked.

p. 8, section 2.4.1: The results of the paper rest on how the raw airborne data are converted into basal emission factors. The introductory paragraph mentions that more methodological details are provided in the supplement; however, these details are missing! These details are essential, but I didn't see them in the supplement. Lines 17-24, by themselves are insufficient, for convincing me of the accuracy of the airborne measurements in estimating basal fluxes. If possible, an estimate of errors associated with each of the six steps would be very helpful.

p. 9, lines 6-7: Clean up the font and subscripts for "dx0.5", "h", and "zm".

p. 13, line 7-8: Provide references and/or analysis to support the role of landcover in heterogeneity and inaccuracy in overestimates/underestimates. Perhaps uncertainties in the aircraft data and their translation could be a contributor?

p. 13, Section 3.3.2: In some of the tables and plots, emission amounts are used. Recommend sticking to fluxes. These could be area weighted to provide a perspective on the relative importance of different ecoregions.

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p. 13, line 10: Add "," after "model".

p. 13, line 11: Remove "Supplement".

p. 13, line 20: After "woodlands", recommend showing specific ecoregion ids in parentheses.

p. 13, lines 22 and 25: Either ecoregion "7o" or "7a" is incorrect.

p. 13, line 23: Add comma after "emissions".

p. 13, lines 22-28: Given that high isoprene fluxes were measured over areas that seemingly have low isoprene emitting vegetation, have the authors considered the possibility of anthropogenic sources – such as petrochemical related facilities, like tire manufacturing?

p. 13, line 28: Add comma after "Overall".

p. 15, lines 2-7: While not necessarily a recommendation, this paragraph reads like it was extracted from a project report submitted to the state of California. Its style isn't consistent with most scientific journal articles.

p. 15, line 21: Be more specific on "other recently available tools".

Figure 1: Consider removing the portions of the flight tracks where flux data were unavailable.

Figure 2: "GAP's" should be defined. Plots c and d should be reversed to match caption.

Figure 3: The text should discuss the apparent underestimate of WRF for max temperature. There appears to be ∼2 deg C underestimate, which can strongly influence the Guenther estimates above 30 C.

Figure 4: Consider showing only those tracks with flux data.

Figure 7: This figure is confusing. It seems to imply a continuous range of footprint

sizes from 0 to 3500 km? Perhaps (if I understand correctly) it should be re-titled to read something like "distance along flight track". Also, why not use flux values rather than emission rates? I assume that 4 kg/hr = 1000 ug/m2-hr. For those portions of the flight track showing significant over- and under-estimates, why not be more specific with location in California and type of ecoregion(s)?

Figure 8: As mentioned earlier, recommend strongly that all four models be compared together. It is confusing to move from ecoregion averages to footprint emission amounts.

Supplement 1: As mentioned earlier, having more descriptions of the four models would be useful. Also, more details on how the basal emission fluxes were derived from the airborne measurements would be very helpful. See "p.8, section 2.4.1" comments above.

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