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Interactive comment on “Estimating European volatile organic compound emissions using satellite observations of formaldehyde from the Ozone Monitoring Instrument” by G. Curci et al.

Anonymous Referee #2

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Review of Curci et al.

The authors present an analysis of OMI HCHO columns over Europe and apply the data to better constrain VOC emissions from that area. The subject matter is suitable to ACP and the analysis is generally solid. Aside from some minor grammatical issues throughout the writing is good. I recommend publication after the following comments are addressed.

General / scientific comments.

19702, L27. You should discuss the potential effects of using α -pinene as a surrogate for all monoterpenes. In effect you're assuming that all monoterpenes have a HCHO

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yield equivalent to that of α -pinene.

19703, L10-20. This is a nice discussion of the uncertainty in the HCHO yield, but it seems that this uncertainty needs to be propagated through the error analysis of section 4.3. Section 4.3 claims to investigate uncertainties in the Eqn 2 terms, and the Jacobian $K = dy/dx$ is strongly dependent on the HCHO yield, right?

Figure 1 seems to show much more spatial smearing for OMI versus the Chimere output. For instance, the high HCHO values north and east of Valencia. Why is this?

19705, L15-16, "the mean uncertainty of HCHO columns is $9.1E15$ ". Not clear what you mean by this, please clarify. Is this the averaged SE for the domain based on aggregating the single-retrieval errors that you described earlier (which range from 50-105%)? Or..?

19707, L3, "Observed HCHO concentrations over the SE Mediterranean.." Based on Figure 1, it seems that this discrepancy applies across the entire Mediterranean, not just the SE. Also, clarify, observed by whom? Not clear if you're talking about OMI here (i.e. Fig 1) or some other observations.

19708, L17-18, "The model bias over land is always within the estimated OMI uncertainty". Clarify what you mean here ... standard error for aggregated scenes? The uncertainty on an individual scene? Also, as phrased this statement seems to call into question any analysis that follows ...

19709, L1-2. "The model over the Iberian Peninsula under (over) predicts..." This is not clear to me from Figure 1 since the difference plot for July seems to be blue or white everywhere over the Iberian Peninsula. Are you referring to a different month? In any case clarify timeframe.

19709, L9-13. "production ... largely controlled by photochemistry ... main loss ... reaction with OH and photolysis" A bit of an odd statement since isn't this the case for HCHO no matter where or when you look?

19710, L7-9, "... also suggested by OMI yearly cycle shown in Fig. 1". State what in Figure 1 suggests this.

19714, L16 and L22. How valid is this assumption that the observational error covariance and the error covariance matrices are diagonal? To the extent it is not likely to be valid, need to discuss what effect this will have on your inversion results.

19714, L17-22. The specification of the error covariance has a strong impact on inversion outcome. Need to elaborate on why this comparison of two biogenic emission models is a good approach for estimating it. E.g., are the Derognat and MEGAN models derived from independent (versus overlapping) sets of field observations? Why is it not a better approach to estimate this error based on a typical level of agreement between MEGAN model predictions and actual field-measured fluxes (e.g. eddy covariance)? It's not clear from the manuscript, but are you assuming the error is equal to the straight difference between the two inventories? I.e., in places where the two happen to agree (perhaps b/c they used the same field observations for that ecosystem type) then the a-priori error is very small? Please clarify this.

19716, L22. The fact that R with respect to OMI improves seems like a foregone conclusion since you have adjusted the emissions based on OMI.

19717, L17-18, "since it is strictly related to intrinsic instrument characteristics." Not a true statement. The AMF depends on prior information including the HCHO profile shape and aerosol distribution, and radiative transfer through partially cloudy scenes, as you already pointed out. I question the claim that the uncertainty in estimating epsilon is negligible.

19718, L22-23. "We conclude that our Bayesian top-down estimate ... is robust against choice of the a-priori." I'm not sure this is a fair statement. Comparing the a-posteriori isoprene panels in Figs 5 and S2 I can see some regions that appear to change strongly with the a-priori. For instance, Greece, parts of the UK, and the northeast portion of the domain (Poland/Baltics). The domain total stays fairly similar but that doesn't mean

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you're not sensitive to the a-priori.

Minor / technical comments.

There are minor grammatical mistakes throughout (missing or incorrect articles, numbers not subscripted, etc). Correct prior to publication.

19701, L1: change to "are estimated to contribute"

19704, L20, change to "compute normalized HCHO distributions for the AMF using the ..."

19706, L6 "Lagrangian model calculations . . ." this sentence doesn't really seem relevant.

Fig 2, HCHO in ug/m3? Change to ppb.

19709, L19, "higher values over the sea", I think you must mean higher relative contribution over the sea?

19709, L20-24, re-write this paragraph to read " Variability was found to be driven", "Isoprene oxidation was estimated to contribute", etc, etc. Need to distinguish between model findings and statements of fact.

19709, L25 "Model calculations agree with description . . ." Not clear what you mean by this.

19712, L9, why two elements in the state column vector?

19712, L23, "the sensitivity of the retrieval". The term "retrieval" shouldn't be used here to avoid confusion with the actual satellite retrieval of HCHO.

19713, L20, "would only be not ill-conditioned" awkward, suggest rephrasing

19718, L4, should be Figure 3.

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