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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Towards a satellite – in situ hybrid estimate for organic aerosol abundance" *by* Jin Liao et al.

Anonymous Referee #1

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This study aims to use the information in HCHO satellite observations, along with a model estimate of the vertical distribution, and in situ HCHO:OA relationships, to estimate surface OA concentrations over the United States. This is an interesting concept, though it is challenged with many uncertainties and assumptions and the manuscript is a bit unfocused. Here are the major issues that I have identified:

1. The title is vague and somewhat inaccurate. The estimate involves satellite, in situ observations AND a model, the "hybrid" should be described as such. Given that the results presented are US only, "over the continental United States" should be specified in the title. And finally, I would recommend that the authors modify the title to include "formaldehyde" so that it is clear that this is not an estimate based off of satellite AOD.

2. What are the implications of the statement on lines 88-89 that "isoprene SOA is

not the dominant source of SOA in summer" for the GEOS-Chem simulation in that region? The study refers several times to the GEOS-chem simulation for the SEUS being "extensively validated" (lines 52, 87, 239) but seems to refer to isoprene SOA. Does the simulation capture the non-isoprene components of SOA as well?

3. The fire analysis (inset Figure 2) is not very convincing given the small number of points. Why did the authors not include data influenced by fire from other regions in their analysis? In particular, the agricultural fire analysis appears to rely on very few points and the discussion of those results in lines 324-326 and 344-346 should be tempered.

4. The relationship between OA and HCHO seems to break down as you increase NOx (it's not all that strong to begin with, see point 7). The correlation coefficient of 0.44 implies under high NO2 less than 20% of the variability of OA can be explained by HCHO. An OA estimate based off of this weak relationship does not seem credible. Also: was NO2 not measured during CalNex and DC3 (Lines 176-183)? Why are these campaigns not parsed for NOx?

5. A key part of this analysis is the conversion of HCHO column to surface using the GEOS-Chem model. Are the AMFs used in the OMI product consistent with the eta's used here? How does the model treat boundary layer mixing? Are the results sensitive to this? Why do the authors only show SEAC4RS profiles of HCHO? How does the model perform for DC3 and CalNex? Lines 455-456 discusses the potential for BB plumes to impact the vertical profile; this could be investigated with the DC3 data.

6. The South Korea analysis is a null result and adds little to the paper. I suggest that the authors eliminate Section 6.7 and earlier analysis of KORUS-AQ data. They could re-cap in the Conclusions that a similar analysis was attempted for South Korea but was precluded by low HCHO from OMI.

7. Need to develop a more detailed and quantitative discussion of uncertainties. Several potentially large uncertainties are discussed on lines 501-508 and Section 7. The

authors should strengthen and consolidate this discussion to outline all the assumptions and potential impact on their analysis. In particular, I note that the correlations between HCHO and OA in the in situ observations are actually not all that high to begin with (in the case of SEAC4RS HCHO only explains 35% of the variability in OA). Also many assumptions are based solely on the SEUS:

a. That OA accounts for a large fraction of submicron aerosol (lines 277-278). What about in other regions of the US?

b. That the vertical profile of OA matches the total aerosol (line 284) – this has not been shown outside of SEUS and the potential for dust, nitrate, smoke plumes to alter this should be discussed

c. Applying the OA:HCHO relationship from SEAC4RS for the entire US. The authors tested using data from the LA Basin as well, but how would OA (and HCHO) differ in the Midwest and NEUS? In Section 6.2 the authors discuss the ability of the OA estimate to capture IMPROVE observations – how good is the estimate outside of the SEUS, where observations were used to construct the HCHO:OA relationship?

MINOR

1. The abstract is missing a few key descriptors: that the analysis is performed for summertime only, that the estimate of near-surface OA is for the United States, and that that estimate also relies on the GEOS-Chem vertical profile of HCHO.

2. Lines 155-166: how do ISAF and DFGAS measurements compare during SEAC4RS?

3. Line 174: specify what temperature was assumed for STP (273 and 298K are both used)

4. Line 180: correct "for daytime NO"

5. Section 2.3: The spatial resolution of the products and and details of gridding and

averaging are missing.

6. Lines 273-277: This sentence is awkwardly phrased ("can estimate the fraction of OA" and then the sentence says it can't do this. . .). Also why is MISR discussed here and later (lines 525-531) when it is not used in this study?

7. Line 280: need to define Aext in the text

8. Line 294: insert "near-surface in situ"

9. Line 298: define "BB" in text

10. Line 402/Section 2.4: The emissions are not described in the model description section.

11. Section 6.1: Are daily HCHO columns converted to surface concentrations using daily etas and then averaged to monthly values, or is the analysis performed using monthly means for everything?

12. Line 488-9: A correlation coefficient of 0.56 is pretty modest. When only 31% of the variation is captured it is not accurate to state that the estimate "generally captured the variation" of the observations.

13. Lines 537-538: The authors state their goal in using the extinction data here at the end of the Section. I suggest that the authors explain more clearly at the start of Section 6.3 why they are exploring extinction.

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