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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 #2

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This paper attempts to develop an estimate of the surface OA concentrations using three variables: (1) satellite HCHO column, (2) conversion factor from satellite HCHO column to surface HCHO concentrations (from a model), and (3) relationship between surface OA and HCHO concentrations derived from airborne measurements. The authors examined the relationship between surface OA and HCHO concentrations from a number of airborne measurements. They find that this relationship varies greatly among different studies (Table 1 and Figure 2). They further examine the dependence of OA-HCHO relationship on ambient NOx concentrations, and they do find that the slope gets lower with increasing NOx (Figure 3). The GEOS-Chem model was able to reproduce this relationship in the absence of wildfires. Finally, the authors use the relationship established from SEAC4RS to estimate surface OA and it compares well

with IMPROVE network. They found that adding NOx dependence or special treatment to urban cities do not change the correlation coefficient between OA estimate and IMPROVE OA. I have a few comments:

1. It seems that this relationship is largely driven by the relative contribution of POA vs. SOA to the total OA burden. As HCHO is mostly secondarily produced, this OA-HCHO is expected to have high slopes when OA emission is dominated by POA (such as wildfires shown in Figure 2). For SEAC4RS or DC3 this slope is much lower because OA is likely dominated by SOA. For KORUS-AQ or CalNex, this slope is likely driven by a mixture of biogenic and anthropogenic emissions, which falls somewhere between. It seems important to understand the contribution of POA vs. SOA to the total OA burden, before one uses this OA-HCHO to estimate OA. I am wondering if this can be investigated in their model.

2. Time dependence of OA-HCHO relationship. If indeed OA is dominated by SOA, one would expect a time-dependence of OA-HCHO relationship, as OA and HCHO are produced at different rates. How would this possibly affect their results?

3. Conversion from mid-day to daily average. The authors use the relationship between HCHO and OA derived from airborne measurements, and satellite HCHO column (overpass time is 130pm local time), to derive a surface OA, which should be the OA in local time 130pm. Then the authors compare that to IMPROVE monthly average data. It seems logical to add a correction factor from 1:30pm to daily or monthly average. The argument in Line 494-495 "ground OA in 494 the Southeast US were observed to have little diurnal variation" is not good enough.

4. The authors show in Figure 7. that different cases make little difference on correlation coefficients. Can the authors show how exactly the NOx dependence is implemented? What kind of NOx concentrations did they for IMPROVE sites (130pm or daily average)? Can the authors show how many large urban cities are treated differently in case 3 and 4? So the reader can see how many IMPROVE sites are affected by

this and understand why the effect is so small. Also the authors need to show how OA concentrations are affected as well.

5. Y-axis label of Figure 7 should be fixed.

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