

Interactive comment on “Investigation of the correlation between odd oxygen and secondary organic aerosol in Mexico City and Houston” by E. C. Wood et al.

Anonymous Referee #1

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This paper presents an analysis of data from Mexico City in 2006 and Houston in 2000. In particular, an examination of the correlation between Ox and SOA is shown, and expectations of the correlation based on Ox and SOA production is given. The authors conclude that $P(\text{SOA})/P(\text{Ox})$ can serve as a test of improved formulations for production of SOA.

Overall, I believe the paper presents a point of view that is consistent with current understanding of ozone and SOA chemistry. I would like to see the authors take the analysis further and speculate on the aspects of the problem that would bring the observations and numerical studies into agreement. I also would like to see some realistic

C1248

propagation of error estimates for calculated quantities. Finally, I believe that some of the derivations should be included in the supplemental material rather than just taken on faith by the reader.

Some more specific comments:

Page 3551. Near end of last paragraph. It is stated that OOA accounts for 64% to 95% with higher percentages at more rural sites. This implies that OOA might be half of the OA in Mexico City. Was this taken into account? Also, how was OA quantified in these estimate? It appears that OOA was used later in the paper, assuming OOA is approximately equal to OA, but the discussion in the paragraph at the top 3552 calls this assumption into question.

Page 3552. Second paragraph. The analysis is done for VOCs up to carbon number of 12. But, it is likely that the amount of SOA from larger hydrocarbons is significant. Estimates of the abundance of C13-C20 species should be made and their impact assessed. This includes estimating the abundance of products from these species that might be further oxidized (e.g. C13-C20 aldehydes and ketones). Together these could be very important since although their concentrations are decreasing with carbon number, the SOA yields are increasing. The last part of the paragraph is very good – if this could be done it would be a big boon for understanding this chemistry.

Page 3556. The FPEAK parameter has not been defined in this paper. It should be at least briefly described.

Page 3557. The statement that “correlation of OOA with the secondary species Ox and NO_z suggest its use as a proxy for SOA is appropriate” is not really justified. If the authors wish to demonstrate that the abundance of OOA is proportional to SOA, perhaps they could do this. It has not been done in this version, as I read it.

Page 3558. Near the bottom. So linear regressions were performed on the OOA vs Ox scatter plots to obtain slopes. How were the regressions done – standard least

C1249

squares (likely not appropriate) or some distance minimization technique? What are the uncertainties in the derived slopes? What are the uncertainties in the individual OOA and Ox values? And at what confidence level (1 sigma, 95%, etc.).

Page 3559. Near the top. It is stated that NO₂ accounted for a significant fraction of Ox, but this isn't apparent in Figures 1 and 2. Perhaps state the range and average values of the fraction. Do the slopes derived depend on NO₂/Ox?

Page 3560. It is stated that the air in the residual layer appears to highly aged. I suppose this is a relative statement, since it probably not as aged as air transport over the Gulf of Mexico. In any case, aged air likely contains a higher abundance of VOC oxidation products than fresh, making the analyses performed in this paper even more uncertain.

Page 3561. Paragraph below 4.1 label. Here the assumptions of this analysis are stated: long lifetimes of Ox and SOA, both are formed from VOC oxidation (yes, but. . .). Thus "it is reasonable to expect" that their concentrations should be correlated. When I first read this statement, it seemed sensible. After some thought and calculations, I am now not so sure. Some "VOCs" as defined (which includes CO, CH₂O, as well as alkanes, alkenes and aromatics) produce O₃ but not SOA, whereas other VOCs produce both O₃ and SOA. This means that correlation Ox and SOA is simply showing correlation between the VOCs that make O₃ and the VOCs that make SOA. I took individual PSOA and POx values, such as shown in Tables SI-2 and SI-3, and found that they are almost uncorrelated ($R^2 = 0.062$ for one case). This explains at least in part, I think, why there are widely varying values for PSOA/POx.

Page 3562. Equations (8) and (9). The "alternative formulation" for P(Ox) is shown without reference or derivation. I suggest adding one or more references here, and performing a derivation in the supplemental material. The equality of these two formulations involves some assumptions that are not readily apparent, and have not been explicitly stated in this paper. Some simplifying assumptions are discussed at end of

C1250

this page and the top of 3563. One is the assumption that HO₂ only reacts with NO. It would be good to give some range of NO values observed, and give a percentage of HO₂ reacting with NO versus other processes (HO₂+HO₂, HO₂+RO₂, possible HO₂ loss on aerosols).

Page 3563. There is a discussion of how F is determined. I infer, but it is not explicitly stated, that the value of F for each species is the average of the F for each of the peroxy radicals formed. This should be added to the text. From the F values in Table SI-1, I infer that HO₂+NO is greater than 99% of the HO₂ loss, and that peroxy nitrate formation is unimportant (CH₃CHO value for F is 1). The latter is in contrast with the observation of significant fraction of NO_y as PAN measured on the C-130. At the end of the page, the problems with the Y_i values are mentioned. How much does the uncertainty in these issues affect the calculated P(SOA)?

Page 3564. The ratio in equation (11) can be calculated with the measured VOCs, but it is known that all of the VOCs are not observed. Attempts should be made to estimate the abundance of species not measured and to assess their impact on the calculated ratios. It is also stated (correctly) that P(SOA)/P(Ox) is not the same as [OOA]/[Ox]. It is stated that if the production rates do not change with time and if the losses are not significant, then the two ratios should "mirror" each other. Further caveats are listed (absolute Ox and SOA are not predicted). It is stated then that P(SOA)/P(Ox) comparison with $\Delta[\text{OOA}]/\Delta[\text{Ox}]$ "serves as a useful test of our general understanding". I do not disagree with this statement, but later in the paper quantitative conclusions are drawn from the comparison rather than tests of general understanding. I suggest making the assumptions here a bit more quantitative.

For a generic species, X, the time rate of change is related to production and loss rates (neglecting transport):

$$d[X]/dt = P(X) - L(X)[X]$$

if $L(X)[X]$ is small compared to $P(X)$, then

C1251

$$d[X] = P(X) dt$$

then if P(X) is time invariant over the period of observation,

$$[X_t] - [X_o] = P(X) t$$

So,

$$[X_t] = P(X)t + [X_o]$$

Or if P(X) does change, the equation describes the average P(X) over the time period. This means that $\Delta[X_t]$ depends on $\Delta(P(X)t + [X_o])$ which depends on $\Delta(P(X))$, $\Delta(t)$, and/or $\Delta([X_o])$. The ratio of two species (such as performed in this paper) is:

$$\Delta[X_t]/\Delta[Y_t] = \Delta\{P(X)t + [X_o]\}/\Delta\{P(Y)t + [Y_o]\}$$

For $\Delta[X_t]/\Delta[Y_t]$ to be proportional to $\Delta\{P(X)\}/\Delta\{P(Y)\}$, $\Delta[X_o]$ and $\Delta[Y_o]$ must be small. We know that the concentration within the basin changes during the day as a result of boundary layer growth and decay, so it appears that this assumption is not met. Equality between the ratio in the change in concentrations and the ratio in the change in production rates appears to be only valid if $[X_o]$ and $[Y_o]$ are either very small or known. This doesn't appear to be the case in this analysis. Alternatively, the ratios are comparable and the caveats given by the authors are unfounded. Either way, clarification is needed.

Page 3564 and 3465. Giving the reader quantitative information about the relative rates of various processes (in this case Ox production and loss) is very valuable. I suggest doing more of this. The impact of aerosol evaporation and wet and dry deposition, as well as heterogeneous oxidation on measured [SOA] are considered. Perhaps other sources of uncertainty in the observed and calculated should be considered.

Page 3566. Paragraph below 4.2. Impacts of changing a, kOH and F values on calculated P(SOA)/POx are considered. It is concluded that the uncertainties have small

C1252

effects on the ratio. The small impact due to kOH makes sense, since the values are in the numerator and denominator. Actually it seems that it should make no difference, since:

$$\frac{\sum 2k_i[VOC_i]y_i}{\sum k_i[VOC_i]y_i} = \frac{\sum 2k_i[VOC_i]y_i}{\sum k_i[VOC_i]y_i}$$

$$\frac{\sum 2k_i[VOC_i]a_iF_i}{\sum k_i[VOC_i]a_iF_i} = \frac{\sum k_i[VOC_i]a_iF_i}{\sum k_i[VOC_i]a_iF_i}$$

The other quantities (a and F) should have a larger impact than stated. Doubling all "a" values should reduce the ratio a factor of 2. Similarly, changing all F values between 0.7 and 1 should change the ratio by 43% (1-1/0.7). The rest of this page and part of the next discuss the Y_i values, but it is not stated that the P(SOA)/P(Ox) ratios are directly proportional to these values. In other words, if all Y_i values are doubled, it results in a doubling of the ratio. I suggest taking a closer look at the dependence of the ratio on the uncertainties in the input parameters. Also, many of the yields in Table SI-1 are listed as 0% or very small values. How large could these small and zero values be and still be consistent with the laboratory measurements? What would happen if all the zero yields were set to 0.1% or some similar small value? What are the uncertainties in the VOC concentrations?

Page 3567. Below equation (12). Alkanes with 22 carbons are described, but are chains this long used in the calculations? Indeed, Table SI-1 (and the others supplemental tables) indicate that not all species are listed. Somewhere please indicate which species are considered in the calculation. This can be done by class rather than listing each individual species. At the end of this page and the top of the next, quantitative comparison ("almost an order of magnitude lower") is made between P(SOA)/P(Ox) and [OOA]/[Ox] in spite of the earlier caveats. This needs to be justified in detail. This is the weakest part of this paper – jumping from caution to comparison without discussion and careful justification.

C1253

I would like to see an estimate of including SOA formation due to oxidation of VOCs by O₃ and NO₃. It may be small, according to current understanding, but a statement justifying leaving it out of the calculations is needed.

The discussion of differences between the calculated and observed ratios seems irrelevant given the incorrect assumption that they should be quantitatively comparable (unless I am missing something). At a minimum, some uncertainties for the observed ratios should be given.

Page 3572. I do not understand the statement (made here for the second time) that using the observed Mo results in an upper limit for SOA, and I also do not understand the relevance to a 3-D model. As I understand it, nothing in this analysis had anything to do with a 3-D model. The statement seems strange to me and perhaps needs some clarification.

Page 3573. It is stated that oxidation of POA is another source of OOA that is not included. Can you estimate what the magnitude of its impact?

Page 3573 (bottom) and 3574. This discussion of missing VOCs is good. Of course, it is not the degree of discrepancy, but the product of the concentration of the missing species times their OH rate coefficient times their SOA yield that would help increase P(SOA). The OH reactivity could have relatively small differences, but P(SOA) from those missing species could be large.

Page 3582. Last sentence of manuscript (before Acknowledgement). The authors recognize that the reactivities of SOA and O₃ forming precursors are different. This recognition should be incorporated into the paper. For this reviewer to better understand the calculations presented in this paper, I performed some calculations using an augmented version of Tables SI-1 and SI-2, as well as a simplified calculation with 4 species. I present these calculations to help the authors understand my quandary with this paper.

C1254

I was able to reproduce the values for P(SOA)/P(Ox) presented in the paper. I also added reactions of alkenes with ozone and reactions of NO₃ radicals. It appears to me that these probably don't make a huge difference unless the SOA yields are much higher than the corresponding yields from the OH reactions. Addition of small yields from alkene oxidation increases the ratio. Addition of large alkanes at 10 ppbv total concentration and 20% average yield increases the ratio significantly (up to about 120). Are these estimates within the uncertainty of field observation and laboratory studies? That is what the authors need to include in this paper.

A simple model with 4 species (CH₄, CO, VOC1 and VOC2) is instructive to understand the dynamics of the problem. The first three species generate Ox but not SOA according to their concentrations, kOH, and F values. The fourth species generates Ox and SOA. I assumed an [OH] of 1E6 so that absolute rates can be calculated. For "background-like" conditions (CH₄=2000, CO=200, VOC1=1, VOC2=1 ppbv), the P(SOA)/P(Ox) ratio is 38. Increasing the CO to 2500 (like MC) reduces the ratio to 5. Obviously, increasing VOC2 increases P(SOA) and the ratio. After exploring a range of conditions, I noticed that the ratio correlated strongly with the fraction of OH that reacts with VOC2. There are slight differences in the ratio changing the relative amounts of CO and VOC1, but overall large ratios such as observed in this study (160 ug/m³/ppmv) are only possible if VOCs with reasonably high (10 percent) SOA yields are the primary reactants for OH (>90%). If the yields are smaller, then the fraction of the OH reactivity needs to be even higher to get large ratios. There are maximum values the ratio can be. For standard CH₄ and CO (1800 and 100 respectively), if Y(VOC2) is 1%, then P(SOA)/P(Ox) cannot be larger than about 18. The maximum scales with the yield assumed. The value of P(SOA)/P(Ox) scales with the fraction of OH that reacts with VOC2, but not linearly. I found a quadratic that is concave down fit the data pretty well (Figure R1).

Calculations such as these might be useful to the reader to better understand this system. It also appears to me that the P(SOA)/P(Ox) ratio doesn't say so much about

C1255

the mechanism of SOA formation as the relative reactivity of the SOA precursors and their SOA yields. It does seem to indicate that there are atmospheric species with relatively large SOA yields that are not being accounted for. It might be useful to include the fraction of OH reactivity due to SOA forming species in the calculations in this paper. Regarding the derivation of the formulas in equations 8-11, I found that it must be assumed that HO₂+HO₂, HO₂+RO₂ are insignificant peroxy radical sinks, and that photolysis of VOCs are insignificant peroxy radical sources (this was discussed in the paper) for the derivation of P(O_x). I must admit less familiarity with SOA formation, including the use of equation (12), but the calculations shown appear to be correct.

In summary, I think this is a good paper, but needs some clarification in a few areas. I hope the authors can do this to make it better understood by the readers.

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C1256

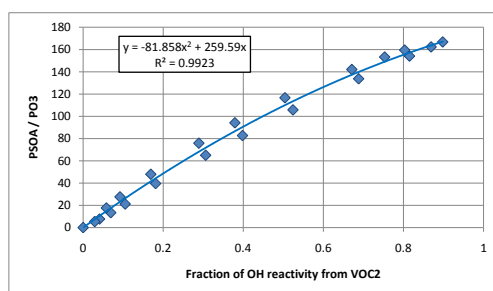


Fig. 1.

C1257