

Interactive comment on “Understanding the anthropogenic influence on formation of biogenic secondary organic aerosols via analysis of organosulfates and related oxidation products” by Q. T. Nguyen et al.

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Responses to reviewers' comments “Understanding the anthropogenic influence on formation of biogenic secondary organic aerosols via analysis of organosulfates and related oxidation products” by Q. T. Nguyen et al.

We kindly thank the reviewers for their thorough reviews and suggestions, and also for the time that they have spent reviewing our paper. We feel that we have been able to improve the paper a great deal trying to address your concerns and questions. Please

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find our responses to the comments below. Quynh T. Nguyen, on behalf of all authors.

Anonymous Referee #3 Received and published: 10 April 2014

Overall Comment and Recommendation

This paper sets out to examine the effect of anthropogenic emissions on biogenic SOA formation by measuring a set of organosulfates and nitrated organosulfates (also called nitrooxy organosulfates), as well as organic acids, derived from BVOC oxidation in PM1 samples collected from 2 field sites in Denmark using an LC/ESI-QTOFMS operated in the negative ion mode. The analytical measurements of these compounds are state-of-the-art and represent the community's best ability to quantitatively measure these novel SOA tracers, especially since many of these tracers lack commercially available standards. Importantly, the authors use authentic standards for the organic acids, except for hydroxy-pinonic acid (pinonic acid was used to quantify this compound). For the organosulfates and their nitrated derivatives, it is very difficult to find or synthesize many of these tracers. As a result, the authors used 3 surrogate standards (D-mannose sulfate, an in-house synthetic organosulfate standard of beta-pinene [MW210], and octyl sulfate). These 3 surrogate standards covered most of the retention time space of the LC, and thus, likely provide a reasonable approximation of the response factors for the real compounds in ESI owing to the conditions of the mobile phase being more similar to these surrogates. I should stress to the Editor that the choice of the organosulfates and nitrated organosulfates tracers is a good selection for addressing the research aim. This is true since these compounds have been demonstrated to form in the presence of acidified sulfate aerosol and in the presence of NOx (or NO3 radicals). The authors cite all of the prior laboratory studies that have examined this chemistry.

Reply: Thank you very much for your generous comments!

Since the aim of this study was to understand how anthropogenic emissions (such as sulfate, aerosol acidity, and NOx) affect biogenic SOA formation, it was unclear to me

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why the authors needed to include the anthropogenic SOA tracers in their analyses?
Can the authors clarify why these are needed?

Reply: We included the anthropogenic tracers as another probe to monitor anthropogenic influence on aerosol processes, since we performed the study at one urban and one semi-rural background site.

Furthermore, I fully concur with the other reviewers that some of the statistical analyses are not very clear or that powerful in addressing the research aim of this study. For example, Figure 2 is very hard to read and I'm not sure what to really conclude from this figure.

Reply: We have tried to increase the font size of Figure 2. In addition, we have also added a yellow color shade to highlight the high correlation among a specific group of correlating compounds. We also hope that the revised caption is now better explaining what we mean: "Figure 2. Correlations of organic acids, organosulfates and nitrooxy organosulfates in each sample throughout the sampling period (excluding those with too few measurements), expressed as correlation coefficient (R2). The results show R2 coefficient averaged from the two sites. High R2 values (> 0.5) were found among a specific group of species (yellow shade), suggesting common sources. A few other species including pinonic acid, pinic acid, phthalic acid, and OS 248 also partly followed the correlation pattern.

In addition, Figure 4 is hard to read and I agree with one of the other reviewer's comments about how to properly interpret this data for addressing the research aim, especially since this figure has very coarse modeling results. If possible, I think most figures need to be made bigger as many are very hard to read.

Reply: We have received the same comments from Reviewer 1 and 2 about our Figure 4 and 5 and the resolution of the SO2, isoprene and monoterpene emission maps. We have increased the resolution of these maps, showing the same spatial coverage. For the aspect of coarse modelling results and how do we use the maps to support our

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findings, please see our responses to Reviewer 2.

One of the challenges with SOA molecular tracer analyses (as the authors well know) is that we require the collection of filters over many hours. As a result, time resolution is sacrificed compared to the online MS methods like the Aerodyne AMS and Ionicon PTR-MS. However, the online MS methods lack the ability of measuring specific tracers due to the lack of separation. What I'm trying to stress here to the Editor is that there are tradeoffs with both sets of measurements. Certainly, the authors work should not be looked down upon for not using higher time resolution measurements, especially since suitable online MS methods are lacking for these novel but important SOA tracers that link biogenic oxidation chemistry with the presence and influence of anthropogenic pollutants. However, I think the authors need to recognize or acknowledge in their manuscript that the time resolution of their samples does limit their ability in trying to understand the effects of anthropogenic emissions on biogenic SOA.

Reply: We have added to the Method section (section 2.1) where the sampling interval (12 h) was detailed the following acknowledgement: "While this 12 h sampling interval allowed us to sample sufficient volume of the tracer compounds, the coarse time resolution complicates investigation of any short-term chemistry or transport episodes".

A recent paper by Lin et al. (2013, ACP) tried understanding the effects of SO2 and NH3 plumes on biogenic SOA formation. In order to do this, high-volume PM2.5 samples were only collected during the day when certain levels of SO2 (high or low SO2) and NH3 (high or low NH3) were measured at the site. To do this, Lin et al. (2013, ACP) connected an SO2 monitor and NH3 monitor to their high-volume PM2.5 samplers. In the case of SO2, when SO2 was below 0.25 ppb one of the paired high-volume PM2.5 samplers turned on to collect PM2.5 under low-SO2 conditions whereas the other high-volume sampler would turn on when SO2 was higher than 0.5 ppb. These thresholds were determined from a historical analyses of SO2 (and NH3) measured at this particular field site. From this prior study by Lin et al. (2013, ACP), for low-SO2 and high-SO2 samples collected on the same days, there was a statistically significant en-

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hancement found for isoprene-derived SOA under high-SO₂ conditions, whereas with low-NH₃ and high-NH₃ samples collected on the same day there was no statistically significant difference observed between the isoprene-derived SOA tracers. The point I'm trying to make is that even though the time resolution with high-volume PM2.5 samplers can limit our understanding of plumes on biogenic SOA formation, when used in this conditional sampling manner one can try to more directly probe the effects of these pollutants on biogenic SOA formation with these sampling methods. Naturally, I'm not saying to the authors that they need to redo their entire study this way, but it is certainly something to think about in the future with these lower time resolution methods.

I do think the measurements currently presented here are needed in the published literature, as many locations around the globe have not measured these important indicator compounds for the effect of NO_x and SO₂ on biogenic SOA formation. I only worry that the integrated sampling approach with time being the constraint and not the concentrations of SO₂, NO_x, sulfate, or nitrate make it difficult to tease out the effect. I understand though that this is why the authors resorted to using HYSPLIT analyses to see how upwind conditions may have affected the formation of these compounds. Certainly transport from upwind locations is going to be another factor, so when comparing the measurements of NO_x, SO₂, etc. obtained at that particular field site with the biogenic SOA tracer measurements, one may not find a direct correlation. However, this certainly doesn't mean that the anthropogenic pollutants didn't have a role in the biogenic SOA formation upwind or during transport of the air mass to the particular field site.

Reply: Thank you very much for having suggested interesting ideas on how we could improve sampling in the future. We thought it could be interesting to try this at some point. We could also combine and have a high volume sampler collecting all the time as baseline, and another at such as low and high SO₂ / NH₃ as in Lin et al. (2013) or other conditions. We will keep this in mind for future studies.

With major concerns above, the other reviewer comments, and also my specific comments below, I must recommend that this manuscript be accepted with major revisions noted. I think a careful consideration of all of these comments is needed before full publication is considered. I think this manuscript will be suitable for ACP once these comments and suggestions are considered by the authors.

Specific Comments:

1.) Abstract: You mention that the organosulfates and nitrooxy organosulfates account for about 0.7% of the PM1 mass. Can you also include here or somewhere in the manuscript how much of the OM mass?

Reply: Unfortunately OC or OM has never been measured for those samples. So we could only provide percentage contribution to PM1.

2.) Section 2.1 - Field Sampling Sites: Can you clarify how many total samples were collected for each site?

Reply: We have added this sentence to the end of the Field sampling sites section: "In total, 64 and 37 filter samples (including both day and night samples) were collected at HCAB and Risø, respectively."

Also, were the quartz filters you used in the PM1 samplers pre-baked? If so, please clarify the exact temperatures used and for how long. If not, are you worried about potential artifacts?

Reply: We used quartz filter grade QR-100, which was pre-baked at 1000°C for 2 hours by the manufacturer to reduce organic contamination. The filters were kept in sealed bag until use.

Were field blanks collected? If so, how often and were they used to correct the actual samples?

Reply: This following sentence has been added to the Method section 2.1: "A field blank was collected in between every 12 samples or less, which showed concentrations

below detection limit for the organic acids, organosulfates and nitrooxy organosulfates. The field blanks were however used for correction of concentrations of inorganic ions."

3.) Section 2.2 - Extraction: Can the authors clarify exactly what they mean by 90% v/v acetonitrile? Do they mean 90% of acetonitrile in water? I think they mean this.

Reply: Yes that was what we mean. We have revised the sentence as follows: "...was extracted in 75 mL of a solution consisting of 90% (v/v) acetonitrile and 10% (v/v) milli-Q water"

One minor concern I have is that acetonitrile is not the best solvent for removing isoprene-derived SOA constituents. Typically methanol is used. I realize that the authors use acetonitrile to prevent any methylation of SOA products, but this tends to be fairly low for most compounds. Are most of the quantified compounds corrected by some known extraction efficiency in 90% v/v acetonitrile? This wasn't state in the current text. If this was done, how was the extraction efficiency determined?

Reply: This comment is absolutely right, and we have used acetonitrile for such reason. However we did not correct for extraction efficiency.

4.) Section 2.4 - Extraction and analysis of ions: Since the authors measure the ions from the filters by IC and then determine the concentration of H⁺ from charge balance, why didn't the authors consider using the E-AIM Model to estimate both aerosol pH and liquid water content (LWC)?

Reply: Thank you for your suggestion. We have been trying the E-AIM model at: <http://www.aim.env.uea.ac.uk/aim/model4/model4a.php>

However we find it challenging right now to employ the model due to two reasons: (1) the lack of information on organic compounds as model input; (2) if we ignore the organic compound input, we also sometimes experienced the issue of charge not being balanced with the model.

So for the time being, we would keep our discussion of charge balance and acidity

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at the current simple level, and work on improving this in future studies. If you have experience with using the E-AIM model, we would very much like to discuss this further.

5.) Section 2.5 - Measurement of particle number size distributions: The authors stated that the CPC was serviced by TSI for ensuring correct counting efficiency. What is their gold standard for this? This is always a mystery to me from TSI and I'm not sure I've ever been convinced by their answers. Furthermore, did the authors check the DMA's sizing performance with PSLs of known sizes from time-to-time?

Reply: TSI is checking the correct operation of the CPC. We do not know what exactly they do there in TSI either. However, we also do comparison of different CPCs on regular base in our lab with ambient air. We were unable to regularly check the DMA's sizing performance with PSLs. However, one of our systems – (they are all the same type) was checked for spheres some years ago and was doing o.k.

6.) Phthalic Acid: Is this really a good tracer to use? I say "good" since phthalic acid is everywhere (on surfaces, gloves, etc.). I wonder how much of this is on your lab and field blanks? My lab tends to avoid using this as a tracer since it tends to be on our blanks.

Reply: This is a very valid point. We however included phthalic acid as we did not observe any "suspicious behavior" in the concentration trend of the species. In fact, higher concentration of phthalic acid was also found on days of concentration maxima from other species. As can be seen in Figure 2, concentrations of phthalic acid showed a high correlation degree of the long-range transported group, therefore we believe that it is unlikely that the artefacts interfere with the overall results.

7.) Isoprene OS: The authors are likely well aware that the isoprene epoxydiol (IEPOX)-derived OS tends to be the most abundantly observed in prior lab and field studies (Surratt et al., 2007, 2008, 2010; Lin et al., 2012, 2013). I see it is not the most abundant, but it is not the smallest. This made me wonder about how well the acetonitrile solvent you use to extract the filters is removing the IEPOX-derived organosulfate from

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the quartz media?

Reply: We have added a few lines in text discussing this, as pasted below:

"OS 216, which was suggested to derive from the reactive uptake of isoprene epoxydiols (IEPOX) in the presence of acidic sulfate aerosol (Surratt et al., 2010; Lin et al., 2012), tends to be the most abundant organosulfate observed in prior laboratory studies and field studies in the southeastern US (Surratt et al., 2010; Lin et al., 2012; Surratt et al., 2007b; Lin et al., 2013; Surratt et al., 2008a). In this study, a moderate mean concentration of OS 216 of 5 ng m⁻³ at HCAB and 3.2 ng m⁻³ at Risø was observed. It should also be noted that we used acetonitrile as extraction solvent instead of methanol as used by Surratt et al. (2007b, 2008, 2010) and Lin et al. (2012, 2013), which could have affected the extraction yield of OS 216 in the present work."

8.) Nitrated OS: The authors seemed to find an enhancement of Nitrated OS species from BVOCs at night, even though they found no correlation with nitrate. I'm not surprised there was no correlation of the nighttime NOS species with nitrate, especially since prior work has shown that NO₃-initiated oxidation can produce NOS species in the lab (Surratt et al., 2008; Ng et al., 2008). Have the authors considered calculating the NO₃ nighttime production from their NO₂ and O₃ measurements and seeing if there is a correlation with this variable instead? That might be more useful. The authors should be aware of recent work from Ron Cohen's group (Rollins et al., 2013, JGR) that found NOS species can be correlated with total particulate organic nitrate overnight.

Reply: We have included analysis of NOS and NO₂ instead. Correlation was found between total NOS at Risø (rural background) and NO₂, however we did not find any correlation at the urban curbside site HCAB, even with the significantly higher concentration species NOS 297. NO₂ was included into the current Figure 3 and 5, and also discussed in text. We have also added the Rollins et al. reference, the correlation between NOS and organic nitrate that they found over nighttime is interesting!

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9.) Page 2466, Lines 14-16: Please be sure to provide citations to the published literature for this statement.

Reply: We have provided a couple of references to the text accordingly.

10.) Table 2: The authors should be aware that even though OS 155 and OS 170 have been found to be derived from isoprene SOA formation, glyoxal and methylglyoxal have other sources (including aromatic oxidation). Thus, I think some caution is warranted when using these OS tracers for biogenic SOA enhanced in the presence of acidic sulfate aerosol. Further, Frank Keutsch's lab has shown that these compounds are formed from glyoxal and methylglyoxal and their exact structures are glycolic acid sulfate and lactic acid sulfate, respectively (Galloway et al., 2009, ACP; Olson et al., 2011, ES&T).

Reply: We are grateful that you have brought this to our attention. We have removed the notation that glyoxal and methylglyoxal are only from isoprene in Table 2. In addition, we have also updated the suggested structure, source and reference for the OS 156 and OS 170.

For the OS 200 compound I had a few notes. First, the 2-methylglyceric acid derived OS (which is from isoprene) has been recently shown to form from methacrylic acid epoxide (MAE) uptake onto acidified sulfate aerosol (Lin et al., 2013, PNAS).

Reply: We have added a note to Table 2 further specifying this formation pathway via MAE of OS 200.

Furthermore, I'm curious to know if the authors observed another OS 200 isomer associated with MBO oxidation in the presence of acidified sulfate aerosol (Zhang et al., 2012, ES&T; Zhang et al., 2014, ES&T Letters)?

Reply: This is a very interesting comment. We have double checked and realized that we might have overlooked this additional OS200 (which we termed OS200_2), which could be from MBO, eluting right after the other OS200_1. We have tried to quantify

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it. However in most cases it was below detection limit, but it was also present in those days that we think are long-range transported. We have updated our table and results to include this organosulfate accordingly.

Lastly, for Table 2, I noted that the authors correctly state that the OS 216 compound comes from isoprene oxidation. However, more specifically, this has been shown to be derived from the reactive uptake of isoprene epoxydiols (IEPOX) in the presence of acidified sulfate aerosol (Surratt et al., 2010, PNAS; Lin et al., 2012, ES&T).

Reply: Thank you for the note. We have made a note in Figure 2 specifying the formation of OS 216 referring to the references accordingly.

11.) Table 3: Please subscript the "0" for NOS 331's formula, as well as for NOS 343's formula.

Reply: Thank you very much for having noted such typo! They have been corrected accordingly.

12.) Table 4: Should you include in this table the total % of PM1 and OM mass identified?

Reply: We have already included the temporal variation of PM1 into the (ACPD) Figure 7 (which is now Figure 6 in this current revised version). For time series % contribution of OS and NOS, we feel that inclusion of such information would confuse rather than improve the article. However we have added a few words as follow to section 3.5 to specify the % range: "It was estimated that organosulfates and nitrooxy organosulfates contributed to 0.1 - 1.0 % (approximately 0.5 % on average) of PM1 mass at HCAB and 0.4 - 1.5 % (approximately 0.8 % on average) of PM1 mass at Risø."

OM mass has unfortunately not been measured in these samples.

13.) Correlation with sulfate and NOx: I find it interesting at the curbside site you see a fairly moderate correlation ($R^2 = 0.6$) of total OS concentration with sulfate, especially since prior work has shown that these compounds form in the aerosol phase from

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BVOC oxidation products that partition onto acidified sulfate aerosol. Should this be more directly pointed out in the abstract or conclusions? This is sort of buried in the main text. Also, did the authors carefully explore the effect of NOx on all tracers? This was unclear to me. From prior work, some of the organic acids from monoterpenes have been associated with the presence of NOx.

Reply: Thank you for your suggestion. We have added sulfate to Figure 3, and show the correlation with organosulfate, and discuss it more explicitly. We have also found a correlation of NOx with total nitrooxy organosulfates at Risø (which has now been added to Figure 3), and also discussed in text.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 14, 2449, 2014.

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