

Summary

This ACPD article characterizes the amount of organosulfates (OSs) and nitrooxy organosulfates (NOS) through ESI-Orbitrap and HPLC-MS during a field campaign near Beijing, China. By using standards or surrogates, the manuscript breaks down organosulfates into isoprene-derived OSs and monoterpene derived OSs, and shows isoprene-derived OSs dominated the total OSs.

By analyzing inorganic aerosol composition, acidity, and liquid water content, the author concludes that due to acid catalyzed chemistry, the production of isoprene-derived OSs was strongly correlated with the acidity of the particles, which was governed by sulfate percentage in secondary inorganic aerosols.

The monoterpene-derived NOSs and isoprene-derived NOSs were measured mainly by the HPLC-MS and their concentrations were used to correlate with the ambient NO_x concentration. Monoterpene NOSs were greatly enhanced during night time due to high NO_x concentration.

As written in the manuscript, through measurements of OSs and NOSs, this study describes the interaction between biogenic emission and anthropogenic pollutants. The data of this kind are valuable and fits into the scopes of ACP. Overall, the manuscript is sound and after addressing the following issues, it is suitable to be published on ACP.

Comments

Line 145: This part was not very clear. How did the author obtain the monoterpene NOSs for quantification? Was it synthesized or commercially available? Please illustrate in detail.

Line 239: The manuscript describes the strong correlation between GAS, LAS, and HAS with isoprene OSs. Then the author concludes that “isoprene or its oxidized products as potential precursors of GAS, LAS and HAS.” The logic here is flawed. Isoprene OSs are correlated with sulfate concentration. Therefore, it is very much likely that GAS, LAS, and HAS are just correlated with sulfate concentration. The correlation of GAS with isoprene OSs does not mean isoprene oxidation products may be precursors of GAS, LAS, and HAS. I suggest changing this part to: “They also showed strong correlations with isoprene OSs (Table S2), suggesting GAS, LAS, and HAS can be potential tracers for organosulfates.”

Line 250: The author raised a very interesting point here. The Southeast U.S. has an isoprene OSs concentration of 165 ng cm^{-3} (Rattanavaraha et al., 2016). The isoprene concentration in Beijing is only 5 times lower than Southeast U.S, but the isoprene-derived OS is 10 times lower. The average RH in Beijing is also lower than Southeast U.S. Maybe the author should provide this evidence to further support the statement that organic coatings and their phase can play an important role.

There are a few important references that I would suggest adding to the manuscript:

Line 69: I suggest adding Shrestha et al., 2014; Zhang et al., 2015 to provide more evidence for how RH and LWC affect aerosol viscosity.

Line 66: I suggest adding Riedel et al., 2015 to show acid-catalyzed reactive uptake reactions in forming isoprene-derived OA.

Line 250: I suggest adding Riva et al., 2016 to show the effects of pre-existing organic loading on isoprene-derived SOA formation. Riva et al. was the first to show the effect of OA on isoprene SOA formation, and Zhang et al. 2018 was the first to quantify such effects.

Line 164: Oxford comma is suggested here.

Line 371-372: Are there any evidence to show that isoprene NOSs are formed via similar pathways (or multiphase reactions) as isoprene OSs? To my knowledge there is limited experimental evidence to show the isoprene NOSs are formed through multiphase reactions. The author should provide more evidence to support the argument.

References

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