Review of New particle formation induced by anthropogenic-biogenic interactions in the southeastern Tibetan Plateau, Lai et al.

This manuscript contains a suite of high-quality measurement data showing H_2SO_4, HOMs, and a PNSD from the Tibetan plateau. Frequent NPF was observed and an analysis of both the measurement data and some very impressive WRF-Chem simulations are presented giving some fascinating insights into the chemistry on a larger scale than measurements allow. The WRF-Chem simulations are possible due to some improvements to the VBS. The methodology for both the measurements and model are, however, extremely sparse. The developments to VBS are not discussed at all, neither are the processing of the measurement data. Similarly, the analysis of the mass spectral data is not given enough time. Many of the key arguments depend on the HOMs being monoterpene oxidation products, but the mass spectra are not discussed in detail. I understand that this would result in a very large paper so I think these things belong in the supplement. Once these are addressed, I very highly recommend this for publication as it is an extremely impressive paper.

General comments

1) The methodology regarding instrumentation is a little bit thin-on-the-ground. It would be nice to have more information about the equipment. What flowrates were the instruments run at? Did they share an inlet? Did the PSM run in scanning mode? What were the time resolution? How were the PTR and CIMS instruments calibrated? What about mass-dependent transmission corrections? This information can go in the supplement, but it is important.
2) The improvements to WRF-Chem are very valuable! But barely discussed. You say you updated VBS to incorporate RO_2 chemistry, including autoxidation and dimerization! Is this similar to existing work such as PRAM/autoPRAM? (1). This should definitely be discussed in detail (again, even if only in the supplement) as the outcomes of the paper hinge on these results. Also, are there plans to make these improvements available widely?
3) Similar to the previous comments, as your arguments hinge on the HOMs being monoterpene oxidation products, it would be nice if you showed them in more detail. The mass defect shows them, but you've lumped $C_{6\text{-}10}$ together. Why not colour it by carbon number? I'd also like to see DBE per carbon, and average oxidation state. Otherwise I have no idea what the HOMs actually *are*. I'd need to be satisfied that the HOMs are similar to alpha-pinene oxidation products, as many of the later arguments depend on alpha pinene lab studies.

Specific comments

Line 223: I'm not sure I understand the logic here, why only those species with C^* of 10^{-9} and 10^{-7}? Why not 10^{-8}? Surely this would avoid you having to input the factor-of-six adjustment? Also,

Line 229: Can you explain the temperature dependence function?

Line 279: Not sure Qi and Riccobono are the right references here. Maybe these two: (2, 3) as the former shows the formation of particles primarily through HOMs, while the latter shows the importance of HOMs as well as $H_2SO_4 + NH_3$ in the boreal environment a little more accurately than Riccobono. Maybe also (4) to show their role in growth.

Figure 3: This figure is great. Is it possible to include one for the whole campaign including H_2SO_4 and HOM? Also maybe use a different colour palette other than Jet (maybe Turbo or Viridis). Same for the other figures.

Figure 4c: As above, the sequence of greens is quite difficult to understand here. Also, is the choice of red + green for sulphuric acid + H_2SO_4 color blind friendly?

Figure 5b,c: I'm finding these bar charts slightly hard to read. Why do the charts start at $<10^{1}$ cm^{-3} ? It makes the actual difference quite hard to see. Why not a boxplot with a Y axis? Then we'd be able to see the min/max concentrations measured & predicted by the model, as well as the distribution, and median value.

Figure 5d: It looks like there's a factor of 2-3 difference between the point where the NAIS and SMPS cross over. Do you have a reference instrument you can correct to? If not, it's common practice to correct the NanoSMPS/NAIS to the LongSMPS. In either case, it doesn't make any difference to the conclusions of the figure.

Line 384: Do you mean "high values" rather than "certain values"?

Line 386 (and following paragraph): What about the SO_2? If that is also anthropogenic (which I'd presume it is as I doubt there's much DMS up there) then this strengthens your biogenic-anthropogenic argument.

Figure 10: Is nucleation rate here $J_{1.5,}$ J_3, or J_{10}? Also, it might be easier to read if instead of "binary" and "ternary" you put H_2SO_4-H_2O and H_2SO_4-NH_3-H_2O.

Line 461: What fraction of total $CN_{10\text{-}40}$ mass does this Biogenic organic mass comprise?

Line 462: Maybe worth considering that autoxidation rates also decrease with temperature (5)

1. L. Pichelstorfer *et al.*, Towards a mechanistic description of autoxidation chemistry: from precursors to atmospheric implications. *EGUsphere* **2023**, 1-30 (2023).
2. C. Rose *et al.*, Observations of biogenic ion-induced cluster formation in the atmosphere. *Science Advances* **4**, eaar5218.
3. K. Lehtipalo *et al.*, Multicomponent new particle formation from sulfuric acid, ammonia, and biogenic vapors. *Science Advances* **4**, eaau5363.
4. C. Mohr *et al.*, Molecular identification of organic vapors driving atmospheric nanoparticle growth. *Nature Communications* **10** (2019).
5. D. Stolzenburg *et al.*, Rapid growth of organic aerosol nanoparticles over a wide tropospheric temperature range. *Proceedings of the National Academy of Sciences* **115**, 9122-9127 (2018).