

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2022-338-RC2>, 2022
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Comment on acp-2022-338

Anonymous Referee #2

Referee comment on "Seasonal significance of new particle formation impacts on cloud condensation nuclei at a mountaintop location" by Noah S. Hirshorn et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-338-RC2>, 2022

This work uses a newly developed algorithm to analyse 15 years of new particle formation (NPF) and growth events observed at the Storm Peak Laboratory, located 3210 metres above sea level. It couples the NPF events with coincident measurements of cloud condensation nuclei (CCN) concentrations to enable estimates of the role of NPF in producing CCN at this site. The paper is well within the scope of ACP, and the addition of CCN measurements to the size distribution measurements and analysis gives this work a unique look at an important issue. The paper is generally well written and organized, but I feel there is need of some clarification of details. Also, I find that the discussion needs to be broadened. Most of my initial comments are very minor. Some of comments 10-18 require more attention.

1. Line 86 – BVOCs can impact aerosol formation and growth.
2. Line 93-94 – Define Level 1, Level 2 and EBAS.
3. Line 127 – what is “ample”?
4. Line 181 – $\Delta N_8, D_{max}$ rather than N
5. Line 212 – It is unclear here why you bother with CCN for non-event days. Presumably, it is because you use this as a reference point for comparison of CCN impacted by events. The reason should be made clear here.
6. Line 213 – NPF and growth are impacted by sunlight as well as many other factors. Rather than “NPF and growth are largely impacted by sunlight, therefore...”, I suggest something like “Sunlight is generally necessary for NPF and growth, and therefore...”
7. Line 214-215 – Here you state that you “obtain four different values of CCNstart”. I assume, but may be wrong, that you mean one for each season, but not four for each event, which is how it sounds. Clarify please.
8. Line 223-224 – It is written to sound like this is novel, yet surely it is obvious that you must allow a long enough time.
9. Lines 248- 250 - Is 53% significantly lower than 56% in this case?
10. Lines 260-263 - What is the method of production of particles on non-event days, and are they truly new particles or just the result of a meteorological change, such as a developing boundary layer (since you are at a mountain site)?
11. Line 265 – I suggest changing “produce” to “indicate”.
12. Lines 266-267 - I’m having trouble understanding the apparent use of non-events as a

reference. Why is it necessary to use non-events this way. Are you assuming that days with events would be the same as non-event days, IF there was no event? Presumably, one of the triggers for the event could be that the aerosol immediately preceding the event was particularly low in number and size (i.e., low CS). Would the non-event reference be appropriate in that case? Also, it is possible that the aerosol sampled on non-event days may well have been influenced by NPF at some time in its history. Would you clarify your reasons behind this, please?

13. Line 283 - Here, the summer result again suggests the "non-event" may not be a suitable reference.

14. Line 303 - What about condensable organics? Do higher temperatures somehow inhibit growth by organics? How would your CCN number concentrations react if organics played a major role in growth of the newly-formed particles, compared with sulphate? What do you think are the main precursors leading to particles growth at SPL?

15. Lines 310-333 - You have more bursts in summer and fall that suggests H₂SO₄ is being produced. Are you suggesting that it ends up condensing on existing larger particles, thereby improving their ability to act as CCN? In the first paragraph here, you say that a reduced CS is important for your SPL observations of NPF, and that is shown in Table 2. However, then you lead off the second paragraph stating that one phenomenon is influencing NPF and CCN on event days at SPL in the summer: temperature. I feel that this is misleading without a detailed analysis of the many things that might affect NPF: including, temperature, CS, SO₂ concentrations, irradiance, available condensable species. In relatively clean environments with low concentrations of precursors (e.g., Arctic), a low CS can be to be a trigger for NPF. Higher concentrations of SO₂ superimposed on regions with a low CS will result in higher number concentrations of NPF. Higher SO₂ concentrations in regions with a higher CS will tend to have lower NPF. In other words, the CS is important, along with other factors. Unless you can present more evidence or a stronger argument that temperature is the primary factor controlling NPF here, I think the focus on one factor may be misleading and the discussion should be made a little more objective.

16. The paper would be helped by putting the present results in perspective through a simple tabled comparison with other estimates of the contribution from NPF to CCN in the literature.

17. Lines 397-398 - Scale is an important factor when comparing remote regions with areas of strong anthropogenic influence. There has been a reasonable amount of work examining NPF in the Arctic (a remote region). I think before concluding with this statement, you should look at some related literature: for example, Nieminen et al. (ACP, 2018; <https://doi.org/10.5194/acp-18-14737-2018>); Abbatt et al. (ACP, 2019; <https://doi.org/10.5194/acp-19-2527-2019>), and references therein.