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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Increased new particle yields with largely decreased probability of survival to CCN size at the summit of Mt. Tai under reduced SO_2 emissions" *by* Yujiao Zhu et al.

Anonymous Referee #4

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This papers investigates long-term behavior of new particle formation (NPF) and associated particle growth at an elevated site. This is an important and scientifically very interesting topic, since there are quite limited number of studies about the response of NPF to SO2 emission reductions, and since the obtained results are somewhat mixed between different environments. The fact that the study is based on relatively short-term measurement campaigns made in different seasons, rather than continuous measurements over full years, limits the reliability of the obtained results, and this should be properly acknowledged in the paper. Anyway, I would support publication of this paper, provided that the authors will address the issues raised below.

The introduction of this paper is generally well written. However, it would benefit from having a more concrete list of scientific questions aimed to addressed (in addition the aim mentioned on lines 75-76) in this paper. Two other, minor issues in this section: 1) the term "functions" on line 34 does not sound correct, perhaps "mechanisms"?, 2) the statement on line 74-75 is unclear. What altitudes are authors referring to here, above the boundary layer in general or upper free troposphere? One should be more careful with this, as elevated NPF can be associated with many different things, including convective uplift, presence of clouds, mixing of different air masses etc.

Experimental methods have been described very shortly, and should be expanded a bit. How were the measurement data used in the current paper quality checked, are these data undergoing any quality assurance procedures? Did detection limits etc. cause any issues for data interpretations? Were there any serious gaps in the data during the periods chosen for the current study?

Concerning the calculation methods, the authors should explicitly mention in main text (section 2.2.1) at which size particle formation rates were calculated, and what size range the calculated particle growth rates refer to (or if the applied size range for this calculation varied from event to event). Also, definition of "NPF duration" referred to e.g. on line 298 should explicitly described. Is it the time period over which new particles are observed to appear at the smallest sizes, or the time period over which the growth of new particle to larger sizes can be followed.

Categorizing NPF event based on the maximum size that the formed particle are able to reach by growth is in principle fine. However, doing that has one important issue that should be at least mentioned, and preferably shortly discussed, in the text. Following particle growth over several days, or event over the night, from observations is often difficult because of the typically large diurnal variation of boundary layer properties (e.g. mixed layer height), and because of changes in measured air masses. This can be seen, for example, on Dec 24 NPF event shown in Figure 1a: there are at least two major discontinuities in the particle number size distribution data (apparent in sudden huge changes in particle number concentration in certain size ranges). As a result, it is highly questionable whether the particles observed to reach 217 nm actually initiated from the NPF event that took place much earlier on Dec 24. The same issues concerns the use of the term SP (survival probability). SP works fine when following the particle growth for a few hours, but becomes questionable for larger time periods. The authors should replace the term “survival probability” with something like “apparent survival probability” and discuss shortly this issue in the paper, including when interpreting the results.

The authors should be a bit more careful when using the term "trend". On line 191, for example, should there read "pattern" rather than “trend”? Multi-year trends can be season-dependent, but I suppose this not what the authors mean here. Please check out that "trend" is correctly used throughout the paper.

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