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Atmospheric Chemistry and Physics Discussions

Interactive comment on "New particle formation in the western Yangtze River Delta: first data from SORPES-station" *by* E. Herrmann et al.

Anonymous Referee #2

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While new particle formation (NPF) is already considered a universal phenomenon, additional characterization is needed in under-represented locations in order to effectively represent NPF in models. The manuscript by Herrmann et al., "New particle formation in the western Yangtze River Delta: first data from SORPES-station," reports on 5 months of data on air ions and size distributions from the Yangtze River Delta in China. I am challenged to come up with a justification for publishing this paper. There is no attempt to place it into context with the growing number of studies that have already been published on NPF in China (an informal poll on Web of Science shows 26 in the last decade). Instead the authors attempt to explain the phenomenology of the events, making crude assumptions and leaving out many details that dilute the usefulness of the conclusions. Below I enumerate my major concerns. I urge the authors to consider these points, but I feel that a major re-write would be needed in order to make

this study relevant amongst the now-crowded field of NPF manuscripts that are coming from China.

In my comments below I precede each with the referenced page and line number.

1. 1456, 10: What does this mean "new CCN per event"? A CCN is a nucleus that forms a cloud (so even under idea conditions this term should be "potential CCN." However, in order for a particle to become CCN a set of conditions must be met that depend on size, composition, and ambient supersaturation. None of these details are given in the abstract or in the main text (1463, 28).

2. Almost all of the figure captions are terse and miss important information. Many do not reference individual panels, which needlessly confuses the reader when references are made in the text. In Fig 3, units are missing. In Fig. 4, it is not even clear what is being plotted. This looks like data from the AIS, but there is no mention of this in the caption or the text. Fig 5 does not state whether these are positive, negative, average, or total ion concentrations in the caption or the text. Fig 6 states that these are daily cycles but not whether they represent averages – the plots of global radiation seem too ideal for actual real-world radiation data (e.g., places where there are clouds). Fig 7 is missing units. In Fig 8, it's not clear what each dot represents (the legend seems to imply it's an event average but there are too many points).

3. In Sec 4, AIS measurements are presented throughout the text with no distinction between positive and negative ions, or if the data represent the average.

4. 1463, 10: the authors should also note that NPF could have occurred aloft, mixing down to the site.

5. 1463, 25: was J2 calculated from J6? What parameters were used in this calculation, since it's impossible really to know if the population of particles at 2nm has something to do with the population of particles at 6nm (thus one does not know the temperature, pressure, and other parameters that existed during nucleation).

6. 1464, 15: Any accumulation of relatively long-lived species that occurs at night will decrease during the day due to boundary layer lifting. That small clusters peak at night is interesting, however. A better plot (actually this comment applies to all diurnal plots in Figs 5 and 6) would be to show the percentiles also on the plot (e.g., using shading).

7. Sections 5.1 and 5.2 are of limited usefulness in my opinion. First of all, I find it strange that the Petaja et al. parameterization was used for estimating sulfuric and not that of Mikkonen et al. Mikkonen's derivation was done with a more comprehensive data set (as opposed to one location, the boreal forest, for the Petaja derivation). The Mikkonen derivation accounts for the codependence of [SO2] and CS. Finally, most importantly, there are no location-specific factors in the Mikkonen parameterization, so an actual value can be calculated. Otherwise this discussion (and by association that of Sec 5.2, which uses the h2so4 data from 5.1), is of pretty limited value, as it just shows the dependencies of Eq 1.

8. Another comment on Sec 5.2 is that it seems pointless to compare J6 to local state parameters like radiation, RH, etc. Nor does it make sense to calculate the L parameter for this site. The reason for this is that nucleation is not shown to occur at this location. I cannot understand why, therefore, it is useful to do these calculations, nor what their potential value may be. Note that the original L parameter was presented in McMurry and Friedlander (1979), not for Atlanta as stated on 1466, 24. Also many details are missing in describing the calculation of L. For example, how are primary particles removed from the calculation?

9. 1469, 26: this sentence that ends "... radiation is by far the most decisive factor.." implies causality that has not been shown in the data.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 1455, 2013.