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Atmospheric Chemistry and Physics Discussions

Interactive comment on "An investigation of nucleation events in a coastal urban environment in the Southern Hemisphere" *by* J. F. Mejía and L. Morawska

Anonymous Referee #1

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General comments:

The manuscript describes an analysis of a particle size distribution data set collected at an industrial site in Brisbane Australia aiming to investigate nucleation event frequencies at the area. The subject of this work is interesting, as more long-term data of secondary particle formation are needed from different types of environments. However, I think that the data set and, more importantly, its analysis is not appropriate to draw any conclusions of regional scale particle formation. Because of this, I think that the material presented in this work is not sufficient for a full paper, and its presentation is somewhat unclear in many places. I therefore think that this manuscript is not ready

to be published in ACP without complete re-thinking of presentation of this data set and possible re-submission.

Specific comments:

1. The authors should carefully describe what they mean as a nucleation event. In most of the recent scientific literature the term "nucleation event" has been used to refer to regional scale particle formation and growth that continues for hours (see e.g. Kulmala et al., 2004). I assume the "nucleation events" referred to in this work are more of a local origin, if any? One example of defining particle formation events (there are others too) is the approach by Dal Maso et al. (Boreal Environ. Res. 10: 323-336, 2005). In most previous studies nucleation events are analysed mostly based on the full size distribution, taking also into account particle growth (rather than just bursts of small particles), whereas in this study the analysis seems to be done only based on particle numbers. Therefore, if the term "nucleation event" is not carefully defined and compared to the literature use of the term, it is impossible (also for the authors themselves) to compare the results of this work to any previous studies on nucleation and particle growth.

2. I have a serious concern about the choice of the measurement site and instrumentation in the aim of investigating nucleation events: It seems like a site that is very highly affected by different kind of local emissions. This, on the other hand, makes it very difficult to assess wether secondary particle formation events take place or not - particularly as the measurements start at a 14 nm. I think it is very well possible that there are particles formed below 14 nm but they coagulate away before reaching detectable sizes. Also, it would be good to calculate the total particle surface area or condensational sinks and compare these to the concentrations in the smallest sizes to investigate a little bit the importance of coagulation losses at the site.

3. Related to the two previous points, it would be nice if the authors would show plots of the evolution of the particle size disrtibution at the times when they have (or have not)

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defined a nucleation event to take place. Also, it would be interesting to know whether any particle growth is observed - suggesting a regional scale phenomenon rather than just a local burst of nanoparticles is observed. Differentiating between regional and local scale phenomema is important since their climatic importance is likely to be of very different magnitude.

4. It is nice to have a long-term data set, but the authors should give detailed information on the exact times of the measurement periods. It should also be clearly defined what is meant by "summer", "winter", "spring" or "autumn". Also, statistics on the goodness of the data (i.e. how many full days of data were collected in total and how large fraction of the data was relaible) should be given.

5. Related to the two previous points, it is very difficult for the reader to jugde wether the mean size distributions given in Figure 2 are representative at all.

6. p. 2199, lines 18-20. The factor given in Eq. 1 is not really a correction but rather a normalization factor.

7. p. 2199, lines 21-22. I suppose the channels were logarithmically evenly spaced?

8. p. 2200, lines 5-6. I suppose there should be a Δ rather than ∇ in Eq. 3 and the following line.

9. Why are there no signs of morning and evening rush hours in the traffic intensities? Is this specific to the site?

Technical comments:

10. The figures are almost impossible to read because the size of the font is so small.

11. The map of the measurement site is confusing: again, the font size is too small, and the resolution of the image is too poor. Also, in the text the authors refer several times to a "road" and "railroad". These should be clearly indicated in the map.

12. Many of the references are not appropriate for the place where they appear. I

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suggest the authors carefully check all the references they cite.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2195, 2009.

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Interactive Comment