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## ***Interactive comment on* “Characterisation of sub-micron particle number concentrations and formation events in the western Bushveld Igneous Complex, South Africa” by A. Hirsikko et al.**

**Anonymous Referee #1**

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This article summarizes 27 months of continuous sampling in an industrial region of South Africa with intensive mining and metal ore processing activities, coupled with informal housing with associated biomass combustion for heating and cooking. These coupled activities produce polluted air and frequent particle formation events. The instrumentation included positive and negative ion spectrometers measuring down to 0.8 nm, and a DMPS measuring down to 10 nm. Other instrumentation included ozone, black carbon, SO<sub>2</sub>, NO<sub>x</sub>, CO and wind speed/wind direction. The authors report the most frequent new particle formation events of any location reported in the literature

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(86% of analyzed days). There were some equipment malfunctions reported in the paper, but it appears they were dealt with carefully and fairly completely by the authors in data reduction.

The article is generally well written and is a good summary of the nucleation activity observed in this location.

Comments 1. There should be a summary of the gas and PM results in the paper. While page 1898, line 14 gives the citation to Venter et al. (2012) which apparently summarizes the gas phase data, it would be interesting to know the SO<sub>2</sub> concentrations, NO<sub>x</sub> concentrations, and particulate levels in general (such as during nucleation event periods, on average, 5th and 95th percentiles – basic characterization information). This could be in a short table, or in the text. This is especially important since the gaseous data is at the submitted stage right now, and is in a less common journal (the S. Afr. J. Sci.). But even if the gas data was already published in a common and open access journal, I think there should still be summary in the current nucleation-focused work.

2. This information on general gas and particle level pollution (see #1) should be in the abstract.

3. As it relates to nucleation, the use of direction analysis (does nucleation occur preferentially from certain wind directions) would perhaps add more richness to the data analysis.

4. Page 1903, line 15. Include how much data (for <3 nm, or >3 nm) is left over after the data cleaning.

5. Section 4.2 on the procedures taken to correct data for high altitude are not clear. They are likely clear to those who are experts in data inversion for AIS and DMPS, but to others a better explanation of the correction is needed. Is the high altitude data being adjusted for comparability to sea level instrumentation, or is it the other way around?

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6. In section 4.3.3 the notation H<sub>2</sub>SO<sub>4</sub> – proxy seems like it should be more compact, and not subject to the confusion that it might be H<sub>2</sub>SO<sub>4</sub> minus proxy (which is what I first thought it was).
7. A quantitative table of values for ion counts, particle counts, CS, etc. seems appropriate for the paper.
8. page 1908 line 27, the phrase ...unlike everywhere else... is awkward.
9. page 1909, line 7. More explanation or citations needed on how the authors are separating regional and local events.
10. page 1910 line 9. reword the phrase ...high small ion concentration...
11. page 1911, line 7. typo nucleation events
12. page 1911, line 11. typo missing space

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1895, 2012.

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