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Interactive Comment

Interactive comment on "Influence of medium range transport of particles from nucleation burst on particle number concentration within the urban airshed" *by* H. C. Cheung et al.

Anonymous Referee #1

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The manuscript describes measurements of particle number concentration (PNC) at three sites in and near Brisbane, Australia. The three sites represent different pollution environments - urban, roadside and semi-urban. The authors discuss diurnal and weekday-weekend patterns of PNC, its dependence on the local wind direction, correlation with different co-pollutants and inter-correlation between the sites. The influence of regional nucleation events on PNC at the three sites is also discussed. The findings reported in the manuscript are fairly interesting, though not surprising, because similar diurnal and weekday/weekend patterns have been reported at other locations by different research groups. I have several serious concerns regarding the measurements and data interpretation, which are described below.

Measurements:

There is practically no information on the experimental setup. What was the length of the sampling tubing, its material, the sampling flow rate? These are important, because PNC is very sensitive to diffusional losses in the ultra-fine range.

PNC at the QUT site was measured with an SMPS. The authors state that it consisted of an electrostatic classifier (TSI 3080), but do not specify whether a long or a short DMA was used. What was the sheath flow rate? How confident are the authors in the 4-110 nm size range they claim for the SMPS, when the CPC that they use (TSI 3781) has a nominal lower size limit of 6 nm?

The SMPS size range used in this study puts under question comparability of PNC measured with the SMPS and PNC measured with standalone CPCs used at the other two sites. Were there no particles larger than 110 nm at any of the three sites? These larger particles would be counted by the CPCs, but missed by the SMPS. Could this be part of the reason for the observed lack of correlation between the three sites?

Were the SMPS and the two CPCs intercompared side-by-side? Without such data there is very little value in any inter-correlation of PNC measured with these instruments.

Data processing and analysis:

If I understand the 2nd sentence of section 2.4 correctly, SMPS channels with less than 1 cm-3 were discarded. This would be a wrong approach to treat SMPS data. Did you mean the integrated count (i.e. all channels combined)? How much did each of the quality control criteria contribute to the total 28% data removal?

I find it hard to understand the rationale that the authors use for attributing PNC to different sources. For example, in the abstract they state that “morning traffic exhaust emissions . . . contributed 5.5% and 5.1 5 (???) during the week respectively”. This attribution seems to be based solely on the fraction that the morning rush hour con-

ACPD

11, C15556–C15559, 2012

Interactive Comment

centrations represent of the total cumulative daily PNC. The origin of this attribution is not explicitly discussed in the text, but becomes apparent upon examination of Fig.2,5, and 7. Unfortunately, this approach is simply naïve, because PMC is a combination of contributions from different sources, including the background. At the very least, the background contribution needs to be subtracted to estimate the traffic contribution. If the background has a diurnal pattern, as the authors seem to suggest (due to photochemical activity and new particle formation), this task becomes very problematic. A similar problem is found in the attribution of higher PNC observed in NE wind sector to industrial emissions (section 3.3). I do not understand how the authors can distinguish industrial emissions from nucleation events associated with that wind sector, which apparently happen very often (p. 32975, last sentence).

The explanation at the end of section 3.4 that the authors offer for the absence of a nucleation event on 9 Sep 2009 at one of the sites (WOO) is less than convincing. The authors try to explain the absence of the event by a PM10 concentration that was higher by 30% than on the date when nucleation was observed at all three sites. First, it is doubtful that a 30% increase in PM10, which is a very poor parameter for condensational sink, would have such a dramatic effect to switch off the nucleation. Second, if the event was regional, then particle should be also forming upstream of the site and should have been still arriving at the WOO site. Yet, apparently, it was not happening. The only explanation could be a rapid coagulation scavenging of the small particles by the larger ones. However, given very low total number concentrations (a few thousand per cm3), this seems highly improbable. Maybe the authors could provide an order of magnitude estimate of the time scales of coagulation losses?

Minor comments:

p. 32966, line 9: replace "5" with "%".

p. 32966, line 2: Remove "alteration"

p. 32966, line 2: I doubt ultrafine particles contribute to visibility degradation - they are

ACPD

11, C15556–C15559, 2012

Interactive Comment

in the Rayleigh scattering regime.

p. 32967, line 19: The were several studies observing regional nucleation events in urban areas, such as Atlanta and Pittsburgh. Those studies were more comprehensive than the current one and need to be cited here.

p. 32969, line 5: How "heavy" was the traffic flow? Provide traffic count.

p. 32970, line 6-7: The TEOM measurement have been already mentioned in the previous paragraph.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32965, 2011.

ACPD

11, C15556–C15559, 2012

Interactive Comment