

## ***Interactive comment on “Impacts of new particle formation on aerosol cloud condensation nuclei (CCN) activity in Shanghai: case study” by C. Leng et al.***

### **Anonymous Referee #2**

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Referee Comment ACP-2014-466

General Comment: The paper presents and analyzes a comprehensive measurement campaign primarily aimed at estimating the effect of small particle formation and growth on the concentration of cloud condensation nuclei (CCN). The measurement work seems quite competent and is well presented. On the condensation nuclei (CN) topic, the analysis is entirely empirical, and could have been stronger had the extensive data been used to make theoretical estimates of parameters like formation rates, growth rates, and condensation and coagulation sinks. Still, the work is solid overall and deserves full publication.

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Specific Comments: The authors do not comment on the relatively large difference in the frequency of formation events in their springtime study (27%) with that observed during winter by Du et al. of 5.4%. This seasonal preference for greater frequency during spring is seen by many others in northern hemisphere locations and probably deserves to be pointed out.

The instrumentation descriptions are quite terse. For particle size distributions, the mention of TSI 3080 is insufficient information. What was the CPC? Was the TSI software used? If so, was multiple charge and diffusion correction applied? What was the sample inlet? Were diffusion losses and impaction losses in sample lines accounted for? If all this information is in a previous publication, please cite that paper.

Similarly, the Thermo FH62C14 needs another line or two of description. It should be included in the paper that this is a beta attenuation gauge. Also, there should be some mention of the QA procedures, the detection limit, etc. Once again, if this is included in a previous paper, please cite the paper. Finally along these lines, the model number of the Vaisala visibility monitor should be included.

The authors mention the condensation sink in Section 3.2.2 and describe a method for theoretical estimation of particle formation rate. Using the method of Dal Maso et al. (2002, JGR) the authors could estimate the numerical values of the condensation (and coagulation?) sinks and strengthen their analysis.

The discussion in Section 3.2.4 describes the method for determining an estimated kappa value for the measurement period. The work seems quite good, but a detailed reading reveals a number of unanswered questions: 1) Are all the data for this section limited to the 70 hours used for the graph in Figure 10? (line 25); 2) If only 70 hours of data were used, why was only this small subset of data chosen, and how was it chosen? 3) What was the effective kappa used for the predicted N(CCN)? How does it compare to other estimates of this parameter? (There does seem to be sufficient information for the reader to calculate the effective kappa, but it just seems strange

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that the authors do not present this useful result!)

Technical Corrections: p. 18644, line 5-6: A reference is given in the text as Zhang et al., 2010. In the References section there is a Zhang, 2010; and Zhang et al., 2012 and 2013. Please clarify.

p. 18644, lines 22-24: the clause that begins “and atmospheric ammonia” is awkward and difficult to make sense of. Please reword – split into two sentences if necessary.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 18641, 2014.

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