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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical note: Characterization of a static thermal-gradient CCN counter" *by* G. P. Frank et al.

G. P. Frank et al.

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Response to the specific comments of Referee #1:

Page 2154, Lines 7-26: It is correct that the two calibration methods presented in the paper has been used by several other authors. In fact the method of comparing the CCN counter concentration to a CPC concentration was used already by Gras (1995) and Gras et al. (1996). However, the methods have previously not been described in such detail as in the present paper, except for the very recent technical paper by Snider et al. (2006). A detailed description and discussion of calibration methods can, because of space limitations, often not be published as part of a conventional research article presenting measurement results. We believe that it is very important to improve the calibration methods in order to do more accurate measurements, and this includes more detailed discussions of the methods. A technical note is suitable for this purpose.

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We also refer to our response to the general comments of Referee #1. We are aware of the additional articles that the referee refers to. These also make use of the same or similar calibration methods. However, we did not refer to these articles because of the following reasons: 1. Raymond and Pandis (2002) refer to Cruz and Pandis (1997), which we have referred to, regarding the method and validation; VanReken (2003) and Roberts and Nenes (2005) makes use of other types of CCN counters, not the SDC type CCN counter, and both articles are therefore not as relevant as the others.

Page 2155, Lines 1-2: We agree that this statement is somewhat misleading, and agree to remove it. However, it is clear that the article by Roberts et al. (2003) only present calibration with respect to number concentration, and not with respect to supersaturation. Since the CCN counter used in the present paper is identical to the one used in Roberts et al. (2003), we are very well aware of the large discrepancy in instrument supersaturation and “real” supersaturation.

Page 2155, Lines 3-6: We don’t agree with the referee, see our response to the general comments of Referee #1. However, we have modified these sentences, so that the goals and objectives are more clearly formulated, and more emphasis is put on the second two conclusions of the article.

Page 2156, Lines 8-11: We would not say that these findings are widely realized by the CCN measurement community, only because a few groups already have used similar calibrations methods. In fact the methods are not identical, and the methods described in the present paper are characterising the CCN counter in much more detail. The present paper also discusses and presents why it’s important to calibrate in such detail. The description of the calibrations in the referred papers is also not detailed enough. For example, the theoretical calculations of the critical supersaturation, using the Köhler equation, are often not well described, and this can have a large impact on the calibration results. A coming publication will present and discuss this further.

Page 2157, Lines 9-11: The understanding of the referee is correct. We will change these sentences to improve the understanding.

Page 2157, Line 9 - Page 2158, Line 27: Thanks for the suggestions! We will consider them and try to modify the description to make it easier to follow.

Page 2158, Lines 14-18: S is to a first approximation proportional to ΔT2, which means that ΔT must be approximately a factor of square root of 0.5 (≈ 0.7) lower. We will add this information to the manuscript. Thanks for the suggestion! No attempt has been made to model the temperature discrepancy. This is not a trivial task, and we consider it to be beyond the scope of this article. We accept the fact that the real temperature difference is lower then what is measured, and use the calibration to determine the actual supersaturation. Since we have experiences also with the DMT CCN counter, we know that the attempts to model the temperature differences in Lance et al. (2006) have not been perfectly successful. When calibrating the DMT CCN counter and using the model in Lance et al. (2006) (in collaboration with the group of Nenes), we still find a discrepancy. The reason for this is not yet clear. Because of the difficulties in modelling the temperature difference, we prefer to leave out the modelling work from the manuscript.

Page 2159, 15-16: The previous calibration was performed in a similar way, and is described in the PhD-thesis of Greg Roberts (Roberts, 2001). In fact, when we double checked this reference, we found that the result in Roberts (2001) was 0.076 cm-3. We will add the reference, and change the observed volume. The change of volume is made by changing the slit width in front of the laser. This is done by changing slit plates. A set of plates, all with slits of different widths, are available. When the plate is changed, a new calibration is needed. However, we expect the calibration to be stable for the same plate (same slit width).

Page 2159, 15-28: The referee makes a good point. Reasons for the lower concentration might be both coincidence effects and water vapour depletion. However, the effect of water vapour depletion should most likely be more abrupt, and not gradual, as observed. We will add a discussion about this. We will also explain why we have chosen 5000 cm-3 as the upper limit for reliability

Page 2160, Lines 3-18: The uncertainty presented in line 16 does not include this systematic bias, but only the random counting uncertainty. The bias is accounted and corrected for by the more thorough calibrations in figure 5 (i.e. the apparent increase in sensing volume at low concentrations). We will try to explain this better in the manuscript.

Page 2160, Line 19 - Page 2161, Line 10: We agree that this is probably the most important finding. We have changed Fig. 5 and present the data points instead of the fits. However, we do not agree with the referee that the evidences are insufficient. Figure 6 clearly shows a dependence of the number of droplets (picture(s) with the highest count) on supersaturation. Se also our comments to the general comments of referee #1. We have also added a discussion regarding measurements of particles with unknown composition.

Page 2161, Lines 12-23: The presented results are valid at an S of 0.72%, and in the concentration range between 32 and 4000 cm-3. The depletion effect can also be neglected at supersaturations below 0.72%, which includes most of the atmospheric relevant range, since the growth is slower at lower supersaturations, thus making the depletion slower at lower supersaturations. The discussion about this has been improved in the manuscript. In addition, a discussion related to the discussion in Sect. 3.3., about separation of the effect of coincidence and water vapor depletion, has been included. Water vapor depletion would lead to a sharp decrease in concentration during the number concentration measurements, and since this was not observed, it also supports the fact that the effect of water vapor depletion can be neglected.

Additional references: Gras, J.L.: CN, CCN and particle size in Southern Ocean air at Cape Grim. Atmospheric Research, 35, 233-251, 1995 Gras, J.L., Jennings, S.G. and Geever, M.: CCN determination, comparing counters with single-drop-counting and

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Interactive Comment

photometric detectors, at Mace Head Ireland. Időjárás (Quartarly Journal of the Hungarian Meteorological Service), 100, 171-181, 1996

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 2151, 2006.

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