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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Helicopter-borne observations of the continental background aerosol in combination with remote sensing and ground-based measurements" *by* Sebastian Düsing et al.

Anonymous Referee #1

Received and published: 11 September 2017

Düsing et al. (2017) describe a closure study using the airborne ACTOS payload. Their goal was to evaluate the extent to which ground-based measurements were representative of vertically-resolved (airborne) measurements. They focused on aerosol optical properties measured or inferred by ground-based lidar, which includes backscatter coefficient, CCN number concentration (CCN-NC), and particle hygroscopicity. Overall, the measurements and analysis presented in this manuscript are of a high quality and I have only a few scientific comments. My major comment would be that the presentation of these results can be significantly improved before publishing.

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Discussion paper

The conclusions of this manuscript are currently lost in an excess of detail which is presented without clearly signalling the takehome message. As a prime example, the goal of the paper is not stated until the third paragraph of the abstract. As another example, section 4.3.1 titled "case study of flight 14b", begins with a review of basic flight statistics but goes on to perform an evaluation of the lidar backscatter backscatter coefficient data. The latter is clearly the main goal of the case study, and the reader needs to be informed of that by changing to a more descriptive title. Especially for a long paper which addresses multiple topics, it is important to provide a clear manuscript structure. The multiple topics discussed here include lidar, CCN and aerosol optics, which will probably attract readers from a variety of backgrounds who will each want to read only one of those topics. It would be better to summarize the flight statistics in a dedicated results section followed by sections focused on the take home messages, like "In situ versus lidar measurements of Bext", "Vertical profiles of aerosol hygroscopicity", etc. These are just examples.

My other major criticism is that the explanations of some observations are too speculative. On line 20 of page 18, the authors argue that the Mie calculations underestimated the backscatter coefficient because the upper cutoff of the inlet system was 2 microns. When an argument like this is presented, it should be backed up by hard data. For example, add a statement like "Using flight 14b as an example, we calculated that, at 5 microns, only 2 particles per cubic centimeter would be required to close the gap between the Mie calculations and the lidar measurements."

Another example of an incomplete or fragmented argument is on line 1 of page 20. The authors first speculate that aerosol hygroscopicity from the CCNC was influenced by supermicron particles, not measured by the ACSM. A few sentences later, they state that the CCNC hygroscopicity is only valid in the size range of the derived critical diameter. These two statements contradict each other because the critical diameters will be less than a micron.

My recommendation would be that the authors rewrite the entire text to be more focused on the take home messages and minimize speculation. The manuscript would then be greatly improved. However, I do not consider the manuscript unpublishable in its current form.

Other comments

-Page 10, line 15-23. The authors state that "aerosol particles consists of a core surrounded by a shell" gave "the best agreement between modeled and measured hemispheric backscatter coefficients for Melpitz" and cite Ma et al (2014). I was not aware of that conclusion, so I consulted the cited paper. I do not see anything about "best agreement" in Ma et al. In Ma et al, Table 2 suggests that no conclusions could be made about mixing state in their work, and the authors did not make any such conclusions. Mixing state assumptions do not appear to be important at Melpitz.

-Page 17, line 30. It is not correct to delete negative values because they are unphysical. A negative value is only unphysical if it comes along with a confidence interval less than its magnitude. Otherwise, it is the same as a zero. On the other hand, if a negative value is still negative after considering the confidence interval, it means the confidence interval is too small. In this case, it is definitely misleading to delete negative values, which are now telling you that there are fundamental problems in the calculation. If the authors have systematically deleted negative values, this would explain the overall high bias of the lidar results.

-Page 21, line 15. "This study shows that the aerosol type dependent intensive property of the LR" "leads to uncertainties in particle light extinction profiles". This is a strong conclusion that I did not personally see demonstrated in this manuscript.

Page 22, line 4. The variation of Qbsc with x does not prove that a precise calculation of Qbsc (total) requires a very precise determination of the PNSD. The Qbsc (x) will be smoothed out when integrating over a size distribution.

Minor comments

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Discussion paper

-The abstract is far too long. Although ACP does not enforce abstract length requirements, this abstract reads more like a thesis abstract than a manuscript abstract, it doesn't communicate the manuscript's conclusions effectively.

-The ACSM measures organics which vaporize at 600 degrees C. This can include water insoluble material such as hydrocarbons emitted by traffic. I am not criticizing the data analysis, only the language.

-On line 18 of page 2, the aerosol-radiation interaction radiative forcing is quoted as -0.35 W/m2 without mentioning the uncertainty range (-0.85 to +0.15). It is the uncertainty range and not the value which is important here.

-Page 3 line 18 please provide a citation for the GAW network.

-Page 3 line 34 onwards, this paragraph seems out of place. Are you describing a shortcoming of airborne measurements, or describing the methodology necessary to compare in situ and remote sensing data? Some guiding words are missing.

-Page 4, line 3, please state explicitly which two of the challenges.

-Equation 1 is missing the dynamic shape factor. Please include it. I understand that you assume it is part of your assumed density, but it still needs to be included.

-Page 18, line 7. Why three times the standard deviation? This would imply a confidence interval of 99%. There is no point to asserting such a high confidence interval, when modeling assumptions have been significant.

-Page 19, line 24. "mainly the non-observed size range in the PNSD". How do you know this is the main cause?

-Page 21, line 8, "complex behaviour" is not a satisfying explanation.

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