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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of turbulence-induced new particle formation in the residual layer" *by* B. Wehner et al.

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General comments:

The paper by Wehner et.al. provides an excellent presentation and discussion of a challenging experiment focused on observation of new particle formation and related dynamic and thermodynamic parameters through profiling within the residual boundary layer. The extensive set of measurements along with the highly detailed and integrated data analysis made in this study are adequate to define many heretofore undetermined aspects of the NPF process, particularly its relation to mixing processes. Gas and condensed phase chemistry observations are notably lacking, as is admitted, but were beyond the scope of the experiment. All in all this paper presents a well designed experimental plan and describes the results in terms of a well defined case study of

turbuence-induced particle formation.

Specific comments:

Page 338 Line 23.

"It should be emphasized that lidar backscatter signals may will not be well correlated with the total particle number concentration because the signal strength is mainly controlled by backscattering of accumulation and coarse mode particles." "May not" is understating the situation. If there is new particle production, then the premise must be that the number-size distribution shifts as you demonstrate in figure 5. That the magnitude of the lidar backscattering moment of the aerosol distribution seems to be associated with NPF, at a size range where the backscattering must be orders of magnitude lower, implies that there is layering of the transported aerosol, particulate plus gas phase, yielding gradients amenable to non-linear mixing effects and NPF.

Page 343 Line 7.

"Even though this increase of turbulence is not very strong, the correlation of increased Eta t and increased N in L1 and L2 is remarkable."

Without some numbers, R-squared for example, its hard to say that the correlation is remarkable. I agree that by ocular analysis there is a correlation and that with a limited data set in a rapidly varying set of mixing layers it is hard to do rigorous, convincing statistics. If the statistics were calculated they should be presented even if the outcome is less than robust. If they were not calculated they should be.

The arguments based on Eta in fig 8 are tenuous. The layer boundaries as defined by [N] generally match the altitude boundaries that would be indicated by Ri and Eta but the altitude values are subjective.

Line 18.

"From our observations it is not possible to determine the exact age of the small par-

ticles but typically the growth rate can be estimated to a few nm per hour (Kulmala et al., 2004) which results in a particle age of one or two hours or so which is also an indication for our hypothesis that these particles were formed inside these layers."

Doesn't Kulmula's growth rate depend extremely strongly on condensable gas phase concentration such that the age could be as short as minutes up to the "two hours"? Without any particular knowledge of the gas phase concentrations that might have existed in this residual layer, but from the trajectories and your description of "polluted air masses", higher precursor concentrations and shorter times would seem probable. Admittedly the relatively low [N] implies that the air mass was not highly polluted.

Page 344 Line 16.

"This short time scale corroborates our hypothesis that the rapid increase of ultrafine particles observed by NAIS is due to vertical mixing of the particles observed earlier by ACTOS in the residual layer, rather than new particle formation at ground level." While I agree with this discussion point, I fail to see mention of the original hypothesis in the introduction that is referred to here.

Similarly, the words "remarkable" and "suspect" where they occur should be replaced with more scientific terms.

Table 2.

Would it be informative to add Ri and Eta to the table for the level legs or are these parameters not valuable except as profiles?

The values of [N8-20], [S] and [V] are not mentioned quantitatively in the discussion in the context of NPF. Particularly S, and the relatively high values of S, are worth adding.

A useful addition to the discussion would be guidance of what observations could be added to better define future experiments of this type or how "re-interpretation" of earlier ground-based observations could be done.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 327, 2010.