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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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The results are intriguing and actually some of the first I know of where co-located aerosol and meteorological measurements have been performed in a relatively stationary air mass situation. These are precisely the sort of measurements we need to better understand the relationships between meteorological and aerosol physicochemical processes like new particle formation. I commend the authors for designing a very nice study.

The findings are actually consistent with previous airborne aerosol measurements I made in 1998 over Philadelphia, USA. Although my previous results are not nearly as comprehensive as those presented here, at both the AAAR conference in 2000 and the EAC in Leipzig in 2001 I showed results from simultaneous measurements of nanopar-

ticle size distributions (5-50,000nm) and a variety of gasses that could potentially act as particle precursors (SO2, H2O2, Ozone, water vapor). We observed very interesting 'bursts' of the smallest particle sizes at the boundary between the developing mixed layer and the nocturnal residual layer during the late morning hours (around 10 am local time). We never observed the bursts during afternoon flights. We speculated that turbulent mixing could be playing a role, but did not have concurrent turbulence data to substantiate our claim.

One concept that still isn't clear to me: If we assume that 5nm particles could already be several hours old, given the back trajectories shown in Fig. 2, wouldn't the air mass within which formation actually occurred have been $\sim$100km away? The statement on page 13 that the similarity of the vertical profiles supports local production compared to vertical transport makes sense, but I don't see how horizontal advection can be ruled out if the atmosphere was simply strongly stratified and very stable in the nocturnal/residual boundary layer above the developing mixed layer. Furthermore, if the size distributions for diameters smaller than 20 nm suffered from poor counting statistics, doesn't this suggest that nucleation, if it did occur locally, was relatively weak, or that the nucleation occurred some distance away?

I would like to recommend reducing the amount of detail described at several points in the paper, particularly if the details are not explicitly connected with proving or disproving a hypothesis. At times throughout the paper it is difficult to follow the very detailed description of the boundary layer structure (for example most of page 12) - it would be helpful in several of these situations to either remove the discussion or end the detailed discussion with some kind of summary statement explaining why such detail is important - what is the point relative to your hypothesis that turbulence is playing a role in new particle formation?

With respect to the Richardson number discussion relative to new particle formation. Were data from other days analyzed as well to explore whether similar turbulence parameter profiles were observed without simultaneous new particle formation?

Could the vertical cloud chamber, or some other nucleation chamber at IfT be used to explore particle formation under controlled turbulence conditions?

Given that the diurnal boundary layer development process and turbulent mixing between the developing mixed layer and the residual layer occurs around the global, would the authors like to speculate on the potential magnitude of this process as a global new particle source?

Details:

Page 11 line 17 end parentheses is missing. Page 12 line 20 '..of the following thick..' is somewhat unclear - do you mean 'The lower boundary of the developing particle-laden layer...'?

In Fig. 8 I don't see the red lines for number concentration, I see black solid lines and black dotted lines.

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