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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemical composition and aerosol size distribution of the middle mountain range in the Nepal Himalayas during the 2009 pre-monsoon season" *by* P. Shrestha et al.

Anonymous Referee #1

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The manuscript presents the results of aerosol physical and chemical observations at the Himalayan foothills. The Authors investigate the factors responsible for the variability in the aerosol number and mass concentrations. This paper is a nice complement of the aerosol observation studies focusing on the high Himalayan environment in Nepal and India and included in the same ACP special issue. The paper is suitable for publication after a few major revisions:

1. This paper is a good example for the very different observations that can be done when applying two distinct techniques for aerosol observation. The conclusions of the SMPS measurements are: a) that the boundary layer evolution connected with the

valley wind system is the key phenomenon controlling the aerosol concentrations at the two sites, and b) that diurnal cycles are superimposed to a background aerosol of accumulation mode particles which "are always there". On the other hand, filter analysis shows that aerosol mass concentration is controlled by the variation in the level of such background particles, which is driven mainly by the synoptic circulation. The papers seems split into two disconnect sections, and this is clearly reflected into the abstract. I strongly suggest the Authors to make more efforts for integrating these two bodies of observations. This can be done looking at the volume aerosol concentrations retrieved by SMPS observations, and trying to identify factors driving the variability between days with more precision: e.g., regional pollution increasing the background of accumulation mode particles in the middle of the day? Synoptic weather affecting the valley breezes system?

2. The comparison of the aerosol chemical composition between the two stations is challenging because of the non overlapping sampling periods and because of the different sampling protocols. However, are there any correction factors that the Authors might envisage to make such comparison more quantitative? Is it possible to provide statistics differentiating between "polluted" and "background" conditions (based on Figures 9 and 10)? In this referee's opinion, and given the dependence of aerosol mass concentrations on synoptic weather regimes, the comparison between simple averages for the two stations/periods (Table 2) does not make any sense.

3. Overall, I find the discussion of the link between boundary layer evolution and the variability of accumulation mode and Aitken mode particle concentrations quite convincing. The Authors infer aerosol transport patterns associated with the diurnal cycle of the boundary layer starting from their SMPS observations. But they should rather acknowledge the two papers by Panday and Prinn (JGR 2009) and Panday et al., (JGR 2009), discussing the boundary layer evolution for the Kathmandu area in details, and reporting diurnal cycles for CO analogous to those reported in this paper for aerosol particles. The importance of these two papers for the understanding of the aerosol

measurements reported in this study should be clearly acknowledged in Section 3.2.2, in the abstract and in the conclusions of the manuscript.

4. The Authors provide an interesting comparison between their findings and the results from observations at high Himalayan stations. However, it should be emphasized that the present study reports observations from a three weeks period whereas some of the past studies (e.g., Carrico et al., 2003) discuss climatologies and long-term measurements. Especially when dealing with the concentrations of chemical compounds, which have been shown to be affected by synoptic weather, the short measurement period may have introduced biases with respect to the average concentrations for the premonsoon season at the two sites, therefore caution must be taken when comparing with experiments involving different sampling schemes and showing a different data coverage. I do not mean that this part of the paper should be dumped. On the contrary, I suggest to the Authors to make it more circumstantiated, but to reinforce it, and to extend the comparison to most recent studies included in the same ACP special issue (e.g., Sellegri et al., ACPD 2010; Ram et al., ACPD 2010).

5. Specific comments:

- Page 15642, line 22. I would rather define the 20 nm particles as “fresh particles from local sources or formed in neighbouring areas” rather than “local or less processed background particles”.

- Page 15643, line 11. Add a reference for the times of the day when the boundary layer develops and collapses in this area of Nepal for this period of the year. - Page 15646, line 25. A WSOC/OC ratio of 0.2 is not a high fraction. See the data compilation reported by Jaffrezo et al. (ACP 2005).

- Page 15646, line 26. There are now several models available to predict the CCN activity of aerosol particles made up of both organic and inorganic components. Therefore, if the Authors believe that the WSOC fraction is high enough to significantly alter the CCN activity in this specific environment, they should provide full reasoning for that.

Otherwise, sentences like “suggest possible influence of WSOC in the CCN activation in this region” are gratuitous and should be omitted.

- Page 15647, line 8. When comparing the abundance of organic nitrogen in the aerosol from different environments, it is important to use the same metric. WSON/TN values cannot be readily compared to the contribution of “WSON compounds” (molecular mass? nitrogen mass?) to PM2.5.

- Page 15647, lines 12-15. This is another gratuitous statement, because only in specific ecosystems the supply of airborne nitrogen is a limiting factor for plant productivity.

- Page 15648, line 6. The Authors argue that the rainfall events washed out the aerosol from the low troposphere. However, this explanation can hold or not depending on type and extension of precipitation. In general, it is more frequent that in a convective atmosphere the aerosol concentrations decrease because of the ventilation of the boundary layer rather than due to the aerosol scavenging by the raindrops.

- Last line of the conclusions. What is the rational for the extension of the transport regime discussed in this paper up to the Tibetan plateau?

- The mass budget in Figure 8 is wrong: There must be overlaps between OC, WSOC and oxalate, therefore they cannot be plotted in the same pie chart.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 15629, 2010.