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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemical composition of PM_{10} and PM_1 at the high-altitude Himalayan station Nepal Climate Observatory-Pyramid (NCO-P) (5079 m a.s.l.)" *by* S. Decesari et al.

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Reply to Anonymous Referee #2 We thank the Referee for the through revision and for allowing us to identify and erase unnecessary or unsupported statements in the manuscript.

Specific comments: 1. Page 25494, line 14: "are widely used" is a too gratuitous statement; appropriate literature references are needed.

REPLY: We specified the literature studies from which we extrapolated the multipliers for the organic carbon fractions.

2. Page 25495, lines 15-16: "typical for biomass burning aerosols and for "rural" aerosols in general" is another example of a gratuitous statement; it should be backed up by literature references.

REPLY: The WSOC/OC ratios fall in the range of values reported by a number of past studies performed in non-urban locations. We added references in the new version of the text.

3. Page 25497, lines 20-22: I cannot follow the reasoning here. The fact that the temporal variation of the different aerosol components is not the same has little to do with the mixing of these components. The temporal variation of the source strengths of the various components may simply not be the same.

REPLY: Accepted. We dropped this comment about the "mixing" of the chemical species.

4. Page 25500, lines 25-27: There is something wrong with this sentence.

REPLY: We corrected the syntax of this sentence.

5. Page 25502, lines 16-21: When talking about the geographical distribution of sulphate aerosols, one should consider that these aerosols are essentially secondary aerosols and that there is substantial production of fresh sulphate from the SO2 precursor gas along the transport pathway. Attributing differences between the sulphate and carbonaceous aerosol concentrations to differences in geographical and vertical distribution alone seems not justified.

REPLY: There is probably a misunderstanding here. In the statement in lines 16 – 17, we are commenting the distributions of the aerosols not of their sources. In our discussion, the secondary nature of sulphate is implicit, and for sure it is the most plausible reason for the more spatially homogeneous distribution of sulphate aerosols compared to that of carbonaceous particles. The fact that sulphate is produced essentially by secondary reactions makes its potential source area wider than that of carbonaceous aerosols, for which a greater impact from proximal (primary) sources must be expected. This leads to the final statement of the paragraph: “we hypothesize that the OC and EC concentration levels at NCO-P depends more on Nepalese primary sources than on emissions distributed over the whole Indian subcontinent”, not because the Nepalese sources are stronger than elsewhere, but just because they are closer to the sampling site. The impact of OC and EC sources at the Himalayan foothills on the concentrations recorded at NCO-P is confirmed by their marked dependence on breeze regime, contrary to what observed for sulphate. Therefore, we agree that the distinct formation processes of TC and sulphate are responsible for the differences in the phenomenologies of their concentrations at NCO-P. Superimposed to this main effect, we expect an additional, (somewhat smaller) effect from the inhomogeneous geographical distribution of the OC emissions (see e.g., Reddy and Venkataraman, Atmos. Environ., 2002), which can explain the higher OC concentrations in central Himalayan stations compared to the mountain stations of similar altitude in west India (Mt. Abu), as described in the text.

6. Page 25504, lines 5-6: It should be indicated how the mineral fraction was estimated from the water-soluble calcium concentration. Which multiplication factor was used? And what was the basis for the factor used?

REPLY: we calculated the average mineral fraction / Ca2+ mass ratio for 2006, resulting in ca. 4%. However, this part of the discussion has been removed following the suggestions of Referee 1.

7. Page 25506, lines 10-12: Also here it should be taken into account that sulphate is essentially secondary. See comment 5 above.

REPLY: we acknowledge that the term “source region” for sulphate is misleading. We rephrased this part of the conclusions.

8. Grammatical and other technical corrections: . . .

REPLY: all accepted.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 25487, 2009.