

Interactive comment on “An overview on the airborne measurement in Nepal-part 1: vertical profile of aerosol size-number, spectral absorption and meteorology” by Ashish Singh et al.

Anonymous Referee #2

Received and published: 29 June 2018

General comments:

This paper present first ever vertically distributed aircraft measurements of atmospheric aerosols over Pokhara Valley in Nepal. The Himalayan region is generally polluted but only few detailed measurements of the pollution exists. Nepal, being situated in Central Himalayas, is a desirable region for conducting such measurements, in order to understand the sources and transport of aerosols in the region. Therefore, the manuscript is of definite value and well suited for ACP. The measurements seem to be well conducted, and complemented with appropriate data from ground based AOD-measurements as well as satellite and model results. There are few issues with the

C1

paper making it lack focus and therefore at times difficult to follow. These issues are mostly minor, and can be addressed with a reasonable effort. I give suggestions below how the authors could improve the paper. I recommend the paper to be published with these minor revisions.

Specific comments:

- Line 63: to add references about new particle formation in the Himalayas, please see Neitola et al. ACP 2011.

- Line 146: which instrument was used to measure meteorological parameters?

- Paragraph 3.1.1. general meteorological features are presented. While these are nice to know, the authors should consider if they give any increased value for the present paper; especially when values outside the measurement period are given. Furthermore, many values are presented without a proper reference to the averaging period (annual, seasonal or for the measurement period?)

- Line 267: " AOD and ground-level PM generally correlate well (Green et al., 2009)". Although the authors soften this statement in the following sentences, I would be very careful to write this, and many people would certainly disagree. Consider replacing the word "well" with something like "to a certain extent". Furthermore, whether association is better with PM_{2.5} or PM₁₀ is very much dependent on the measurement location, and drawing this conclusion from Illinois (Green et al. 2009 cited) would not prove a wide enough dataset.

- Sub-chapter 3.1.2. I believe this sub-chapter is somewhat too long given the context of the paper, presenting a seasonal analysis of sunphotometer-related products. A somewhat similar analysis and conclusions has been made in the Xu et al. (ACP 2014) paper, although with a shorter measurement period. Similarly to the analysis of the meteorological parameters, the authors should consider whether the analysis brings any additional value to interpret the main measurements (vertical distribution of

C2

aerosols during May 2016) of the paper. Perhaps an analysis focused more on the measurement period could be presented?

- Chapter 3.2. This Chapter presents the core data measured in the project. As written for the moment, the presentation of the results is following a somewhat mixed logic, and I believe some restructuring could be done to make this chapter more readable. First of all, I'm missing the exact dates and times for the 5 flights (F1 to F5) conducted. Second, it would be very useful to identify, if some of the flights were conducted during the same day (as written, F1 and F2 were the morning and evening flights if the same day). Third, I would change the ordering of describing the flights – at the moment for example, the authors write about F2 results before the F1, although it would make more sense to follow the chronological order of the flights, and try to deduce especially what is occurring between flights taking place during the same day.

- At the moment each measured parameter is discussed separately. I would suggest to make a more merged analysis based on the individual flights, in accordance with the previous comment.

- The same Chapter 3.2. includes all the analysis for satellite data and back trajectories. I suggest utilizing more sub-chapters for these.

- Lines 368 – 377. The authors talk about an elevated polluted air mass, first appearing in the morning (Flight F1) at 3000-3500 m a.s.l., and in the afternoon (Flight F2) at 2500-3000 m a.s.l. I am not sure, if these are the same polluted layers. On the contrary, the diurnal evolution of the boundary layer (and / or mountain valley winds during the afternoon) should elevate the aerosol even higher during the afternoon, I would suspect that the morning polluted layer is something else (perhaps long range transported?). The layer which is clearly visible in F2 should then reside at a lower altitude in F1, perhaps around 1500 m a.s.l. Knowing the exact time of the morning flight would give more indication if the polluted layer would already have elevated during this flight.

- Lines 394-396: The authors draw a conclusion that sharp rises in total aerosol con-

C3

centration at 1500 m a.s.l. during F2 and <2000 m a.s.l. during F5 are due to agriculture fires. I am not sure how this can be deduced from total particle number concentration alone. The aethalometer data should shed some light in the issue, as biomass burning results in an elevated absorption at lower wavelengths compared to 880 nm (i.e. a higher absorption \AA ngström alpha). For the altitudes given by the authors, such elevated absorption at 370 nm is unfortunately not evident. There are some cases where absorption at 370 nm are elevated, namely F1 2000-2500 m a.s.l., F4 2200-2300 m a.s.l., and F5 2000-2300 m a.s.l. These are interesting cases and could warrant more attention.

- Line 399. The authors give the absorption measurement results with ngm-3. This is ok for the 880 nm (typically denoted as Black carbon concentrations). However, for 370 nm, this unit is typically denoted as “apparent black carbon” by the instrument manufacturer. As of, this value does not have any physical meaning, it is rather an indicative measure of absorption in relation to BC. I would encourage the authors to use the absorption coefficient (unit m^{-1}) calculated through MAC for presenting absorption data in the manuscript.

- Line 403: the authors write “during the first two afternoon flights”. I thought only two of the flights were conducted during the afternoon?

- Lines 411 forward: the absorption \AA ngström exponent is calculated. Why did the authors choose to take only a two-point slope of the 440 nm and 880 nm measurements, and not a linear regression fit over the whole wavelength range? Both approaches may be used, but I would like to hear their reasoning for this. Further, why did the authors choose to average this data in 500 m bins – was the data too noisy?

- Line 432 onwards: the authors should consider how much the CALIPSO measurement bring added value for the main objective of this paper. Certainly overpasses during 5th and 7th May should be presented with respective overpass times. Are measurements outside the flight days relevant?

C4

- Figure 5: why are the data presented in UTC? This creates much confusion when trying to compare against the vertical flight measurements. Moreover, in the left panel, there is a result for 8th May, while in the right panel for 7th May – where does this discrepancy come from?

- Lines 487-489: the authors conclude that the elevated polluted layer would be due to biomass and forest fires in North India and western Nepal. While this can certainly be one of the reasons, it is unlikely to be the only one, as the biomass burning aerosols should certainly yield higher absorption ångström values (absorb more at lower wavelengths) than observed.

- Supplementary figure S7. This figure is Sunphotometer AOD from the flight period, and in my opinion, relevant to be in the main text rather than in the supplementary.

- Figures 6 and S9. Consider overlapping these and presenting in the main text?

Technical comments:

- Line 44: “The intrusions (in the form of a trough) of the cold and humid air mass from the mid-latitude ($\sim 40\text{-}50^\circ \text{N}$) a shift in the direction of synoptic air mass entering Himalayas.” Unclear sentence, a verb missing?

- Lines 485-486: please remove “prior to which the synoptic air masses were north westerly.” As this was already mentioned earlier in the sentence.

- Figure S4: what is the red arrow?

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-95>, 2018.