

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.

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Received and published: 5 November 2018

We thank the reviewer for the review and constructive comments in the manuscripts. In general, significant changes were made in section 3 as advised by the reviewer. The section is relatively concise now, less descriptive and pertains mostly to the flight periods. The changes mainly include less description for section 3.1.1 and 3.1.2; re-organization of flight results in section 3.1.3 and better presentation using multiple sub-headers. Significant changes in section 3.1.3 include removal of Figure 3 into the supplement and replacing Figure 3 with Figure S7 (from the supplement as suggested by the reviewer). Please see the (track change and clean copy) to identify the specific

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changes.

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

AC: Included. Please see line 59

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

AC: It is a portable TPS3 model from Meteolabor. The instrument detail is added. Please see line 143

- 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?)

AC: We agree with the reviewer's comment about the relevance of section 3.1.1 for the study. Hence, we have edited (shortened) first 2 paragraphs and included only information which is relevant to the study. However, we have kept the synoptic description as it is. We believe the information provided in the synoptic description is useful and relevant to interpret the results.

Please see the paragraph (line 198 to 209) for the shortened version of the meteorological description and now mainly refers to meteorology during the flight 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.

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AC: The authors also agree that the relation between AOD and PM is “not so straightforward” and the citation from Bondville, Iowa may not be appropriate. We would like to avoid such casual statement especially in the current paper which has a different focus or scope. Since we have made significant changes to Sub-chapter 3.1.2. (in light to reviewer’s next comment and also suggested by reviewer #2), most of the descriptive part of 3.1.2 is moved to the supplementary information (See supplementary section S7). Only a short summary is presented in section 3.1.2 (Please see line 241-252). The sentence describing the AOD and PM relation is also removed in the text in the supplementary.

- 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?

AC: Thanks for sharing your concern. We also agree with your comment that section 3.1.2 is too descriptive and is not related to the test flight measurement directly. Therefore we have moved the long descriptive (including Figure 3) into the supplementary information (see section S7 in the supplementary). Only a short synopsis is presented which describes general aerosol properties in the Pokhara Valley (Please see line 241-252).

In addition, we have also included the general aerosol properties, and synoptic meteorology observed specifically during the flight period using AERONET and synoptic readings at the Pokhara regional airport (Please see line 257-267).

As also suggested by reviewer #2, Figure S7 is moved from the supplementary infor-

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mation to the main text replacing Figure 3 (as suggested by the reviewer in a comment later) which is relevant to the description provided from line 257-267.

- 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.

AC: Thanks for this critical comment.

First, we provide exact date and times of all the flights in the main text (Please see line 273, 276) and also provide Table T1 in the supplementary

We have tried to provide a merged analysis (by discussing all the measured parameters). I think the organization of information flow now aligns chronologically with the test flights (F1-F5). Further, to organize this chapter, we have also broken down the chapter into 4 sub-chapters which are:

3.2.1. Diurnal variation in the profiles (please see line 289-337) 3.2.2. Nature of absorbing aerosols in the Pokhara Valley (please see line 337-356) 3.2.3. Comparison of satellite-derived vertical profiles over Pokhara Valley with aerial measurements

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3.2.4. Role of synoptic circulation in modulating aerosol properties over Pokhara Valley - 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. AC: We do concede that the elevated polluted layer in F2 could be due to the evolution of the boundary layer or the transport related to the mountain valley winds. Therefore we have corrected our presumption that the elevated layer of F2 is long-range transport. Only flight F1 is indicative of elevated polluted air mass (Please see line 321-326) and we have avoided including F2 as an indication of polluted air mass without any further evidence (Please see line 327-336).

Lines 394-396: The authors draw a conclusion that sharp rises in total aerosol concentration 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 α_{880}). 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.

AC: Thanks for pointing this weakness in our deduction related to the agriculture burn. We agree that the deduction was not well supported by absorption values (at 370 and

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880 nm), and number concentration is not a unique and useful indicator of an emission source. The authors would like to point out the condition at which these measurements were taken and hopefully, the reviewer could get a sense of why we came to that seeming deduction.

Agriculture fire was occasionally observed near the difficult terrain in and around the Pokhara Valley. It wasn't a big fire, so the plume disappeared as it rose higher. The sampling team (including the first author) tried to fly as close to the terrain to capture the plume (challenging flight safety sometimes). Fly through the plume was possible (for few seconds), it wasn't possible to hover around it and multiple transects along the plume was also not considered safe. The CPC could capture some pockets of plumes (sample time: 1s) while the aethalometer data (sampling time: 2 min) probably couldn't capture the plume.

Nonetheless, we have removed the speculative deduction from the manuscript. In Section 3.2.1. we do not insinuate anything about the agriculture burn to the spikes seen in the particle number concentration.

- 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.

AC: The absorption coefficient (during the calculation of AAE) is shown in the unit of m^{-1} and specifically mention it in line 344.

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

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AC: Sorry, it was a typo. We have edited the sentence. See line 308-309 for the changes.

- Lines 411 forward: the absorption ångström 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? AC: We looked the spectral distribution of the absorption coefficient (unit m^{-1}) across the wavelength and saw the relation (in log scale) is linear. See the supplement section where we have shown an example plot from the flight data (Figure S11 in the supplementary information; absorption: Y-axis, wavelength: X-axis). Therefore assuming linearity, we thought the two-point slope between 470 and 880 is a good approximation in determining the AAE value. However, to avoid further confusion and objection from the reviewer, we recalculated the AAE value using the regression fit. Please see line 339-342 for the changes in the text related to the calculation of AAE. Also, see the changes in Figure 4 Why 500 m bin? There is no scientific basis for choosing 500 m height resolution to represent the AAE data. The rationale for the using a 500 m was based on the following consideration. 1. Aethalometer sampling rate was sluggish (2 min) which can't provide a 100 m resolution value (also depends on the aircraft vertical climb (120-157 meters/min)). 2. In some flights, especially in the lower elevation (see figure), we would have one data point for every 100-200 meters

However, we have presented the data at 100 m height resolution. Please see Figure 4 for the changes

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?

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AC: The rationale for using CALIPSO was: 1. To compare the profiles generated by aerial measurement to satellite retrieval values (in this case, extinction value) 2. To highlight the changes in the regional air quality using a combination of satellite, and meteorology.

Specific to CALIPSO, we think the CALIPSO data outside the flight days are also indicative of a regional haze or haze condition or high pollution episodes occurring in the broader region which was also indicated by the synoptic meteorology in Figure 3 (previously Figure S7), and in Figure S6. MODIS AOD (in Figure S8 in the supplementary) also showed the high AOD in the region prior to 6th May and a relatively cleaner atmosphere from 6th May onwards.

We have made some changes to the CALIPSO as requested by the reviewer. For instance, we have removed one extinction profiles (only 4 extinction profiles are shown in the figure and they are in local time, not in UTC time). Also, see the changes in Figure 5.

- 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?

AC: Sorry about the discrepancy. Now the figure is in local time (Nepal standard time). Now the discrepancy is fixed. See the changes in Figure 5.

- 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.

AC: Thanks for pointing this out. We think we never tried to be certain about the linkage

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between the observed elevated polluted and the biomass and forest fires, it was more of a speculative argument. There is certainly a dust transport along with biomass forest fires (as indicated by CALIPSO).

We have included a few sentences stressing that the biomass burning aerosols could be one of the many other reasons. Please see line 426-429 included to stress this concern.

- Supplementary figure S7. This figure is Suphotometer 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?

AC: We moved Figure S7 to Figure 3. The previous figure 3 is in supplementary as Figure S7. Figure 6 (trajectory data) is merged with the active fire data from Modis C6 collection (Figure S9) and the revised Figure 6 now contains trajectory and active fire count (green+gray circles) for each of the flight days. The green dots are represented by fire radiative powers (frp values) which are in megawatts in Figure 6.

Technical comments: - Line 44: "The intrusions (in the form of a trough) of the cold and humid air mass from the mid-latitude (_ 40-50_ N) a shift in the direction of synoptic air mass entering Himalayas." Unclear sentence, a verb missing?

AC: Corrected. In order to make the abstract short and concise, we have removed the sentence.

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

Figure S4: what is the red arrow?

AC: Removed.

Please also note the supplement to this comment:

<https://www.atmos-chem-phys-discuss.net/acp-2018-95/acp-2018-95-AC1->

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supplement.pdf

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

**An overview on the airborne measurement in Nepal,-part 1: vertical profile of aerosol size-number,
spectral absorption and meteorology**

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Fig. 1. Final_response_to_RC1

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Fig. 2. Revised_manuscript

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