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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport of regional pollutants through a remote trans-Himalayan valley in Nepal" *by* Shradda Dhungel et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper by Dhungel et al., 2016, provides a first characterization of the variability of ozone and equivalent BC at a measurement site located in a Himalayan valley. Until now only sparse continuous measurements are available in the Himalayas region. Thus the data presented in this work can be considered of high interest for the advancement of knowledge about SLCF (short-lived climate forcers) variability in the Himalaya and about the emissions and atmospheric processes able to affect them. Unfortunately, it is not clear from the paper, which data coverage is available over the whole (2.5-year long) investigation period.

However, the paper suffers of major deficiencies that prevent publication in this current form. Indeed, the paper only provides a basic characterization of typical seasonal and

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diurnal variability of O3 and BC: only a tentative attribution of the observed variability in terms of valley wind regime. No information about the role of synoptic-scale transport variability is provided. The data analysis is basic and lacking of statistical analysis. The possible impact of open fire emissions in the IGP and Himalaya foothills should be better assessed by carrying out a systematic analysis. At least, the three case studies presented in Figure 6 should be better explored (as an instance by investigating them by using air-mass transport modeling and a better use of satellite data) and extended (e.g. no information is provided about the frequency by which the three "regimes" were observed over the whole measurement period). The occurrence of open fires is a typical feature of the pre-monsoon season in the Himalaya foothills. Why the transport of fire emission along the valley is observed only in a few cases? Which are the factors triggering the transport of open fire emissions?

Some previous works already extensively investigated the role of thermal wind circulation and open fire emission in affecting atmospheric composition in Himalaya (e.g. Bonasoni et al., ACP, 2010; Dumka et al., ACP, 2015; Lüthi et al., ACP, 2015; Xu et al., ACP, 2015; Raatikainen et al., Atmos. Env. 2014; Hyvärinen, ACP, 2011a,b). It should be great if the authors can discuss their results as a function of these previous investigations even clarifying the scientific advance of their study in respect to these previous works. As an instance, the diurnal behaviors of BC and O3 are significantly different from those observed at other Himalayan site (e.g. NCO-P, or Naintal ,see Bonasoni et al., 2010; Dumka et al., ACP, 2015) which reports eqBC increase from early morning and peaking in the afternoon. The authors should better motivate these differences. Finally, as also admitted by the authors, a not negligible influence on the observed behaviors could relate to local emissions (see the BC peak observed in the morning). It should be important (and interesting) that this local contribution is isolated and quantified before discussing eqBc and O3 variability.

Moreover, I cannot able to find along the paper a real proof about the transport of pollution from IGP to TP: the paper only presents observations inside the Himalayas

valley, thus the export of this pollution to TP is just a speculation at this stage. . .

Finally, I strongly suggest a language revision by a native-speaking English person.

SPECIFIC COMMENTS

Pag 3, line 101. Extensive investigation of the role of valley wind system in favoring the transport of SLCFs to Himalayas was presented by Bonasoni et al., 2010 and reference therein. These researches can be profitably cited at this point of Introduction other than reporting the (rather dated) works from Alpine region. Also this work can be profitably cited: Quantification of topographic venting of boundary layer air to the free troposphere. S. Henne, M. Furger, S. Nyeki, M. Steinbacher, B. Neininger, S. F. J. de Wekker, J. Dommen, N. Spichtinger, A. Stohl, and A. S. H. Prévôt. Atmos. Chem. Phys., 4, 497-509, doi:10.5194/acp-4-497-2004, 2004.

Pag. 4, line 133: actually, the intrusion of the haze is not so visible from Figure 1a.

Pag. 4, line 138: the description of the valley orientation is difficult to follow. Some of the described features (e.g. Eastward orientation at Jomstom) cannot be captured by Figure 1. I would suggest to add to Figure 1 a more detailed map of the measurement site.

Pag 5, line 148: please add to Table 1 a column with measured parameters

Pag 5, line 151: actually “equivalent BC” is measured by MAAP.

Pag 5, line 158: please substitute “attenuation” by “absorption”. For O3 and eqBC, please provide indication about measurement uncertainty and QA/QC procedures.

Pag 5, line 161: please indicate the percentage of data available over the period January 2013 – August 2015. Please, remove the sentence “Measurements of carbon monoxide. . .” (no CO data were presented/discussed in the paper).

Pag 5, line 164: no winter season has been identified?

Pag 5, line 169: I would skip “about 10 meters above and”

Pag 5, line 175: I cannot understand this kind of normalization. Why did you not report actual eqBc and O3 values? You should simply report the averaged seasonal diurnal variation of O3 and eqBC obtained by subtracting averaged monthly values from hourly values.

Figure 2: I would like to see the percentiles for each single month. This would provide also information about year-to-year variability.

Pag 6, line 189: please provide references. Possible reduced domestic emissions related to less domestic heating?

Pag 6, line 197: “Seasonal variability....broad regional pattern”. I do not agree. In the IGP, BC is maximized during winter months (December- January), while in Himalayas (and also at your station) the values are higher during pre-monsoon! (see also Ratikainen et al., 2014 Atmos Env).

Pag 6, line 209: “These differences in...”. Not clear: what differences?

Pag 6, line 213: The works by Ratikainen et al., 2014 AtmosEnv can be cited here

Section 3.2: this discussion is mainly qualitative. No statistical analysis have been applied and it is difficult to discern if the observed features are statistically significant. I suggest to add a line describing the mean average values with statistical confidence level (this would help in understand if the observed peak and minima are robust features). I would add to these plots the analogous for wind direction and speed to clearly correlate wind regime with O3 and eqBc variability. In any case, the results are based just on the analysis of 3 single months of observations. A comment for taking into account the possible intra-seasonal and year-to-year variability should be added. Your measurement period is 2.5 year-long. Why you did not use all the available data?

Pag 7, line 219: “peaked in the early afternoon”. I would say “at noon”! This can be an hint for local photochemical production...

Pag 7, line 220. “Finally,...0 to 1 (Fig. 3)”, I cannot be able to understand this sentence...Maybe you would suggest that diurnal variability account for the most part of the overall O3 variability? In the case, this is a point that should be better stressed. Can you quantify it?

Pag 7, line 223: “increased rapidly following sunrise”...because later in the manuscript, you suggested that this peak can be related to local emissions, I would change with “increased rapidly in the early morning”.

Pag 7, line 225-229: Again, this sentence is not clear to me. See the same comment for ozone.

Pag 7, line 230: you should also mention air-mass transport. At a remote site, if local emissions are really negligible (I’m not totally convinced about this for your site, see your following sentence about eqBC), I would expect that the contribution by transport is the most important one!

Pag 7, line 236: the secondary peak (in the evening from 19 to 21)is visible only during the post-monsoon. Please comment. Does this peak be related with domestic emissions (e.g. domestic cooking or heating)?

Pag 7, line 239: “Up-valley...Alpine pumping”. As mentioned before, many works in Himalayas investigated the role of valleys as channel of anthropogenic pollution. Please consider them and comment your results as a function of these previous works.

Section 3.3: the expected outcome from this Section is not clear. Why did you show just 6 days of data at JSM_2, when more than two years of meteorological data are available at the “core” site where also O3 and eqBC data were available? You must show these data! Moreover, if I’m not wrong, JSM_2 is located 1000 m above the “core” site. Thus, which is the goal of showing these data?

Section 3.4: Legend is missed in Figure 6. I suspect that blue dots represent O3 but you have to add a legend! Basically, this section repeat the same concept about diurnal

variability already reported by Section 3.2. . .

Pag 8, line 271: “This peak occurred about an (one) hour later during the post-monsoon period”. Looking at Figure 3, this seems not true! eqBc peak at 8:00 AM during all the seasons. However, it is important to evaluate the robustness and origin of this peak. Looking at the eqBc time series reported in Figure 6, it looks that the early morning peak is related to “spiky” observations, very likely related to local emissions. This is particularly evident during post-monsoon, when these “spikes” where observed during the diurnal minima of eqBC. This feature can be of a certain interest to evaluate the local emissions to the “pristine” Himalayan environment, but I would neglect it for the analysis of transport processes affecting O3 and eqBC variability.

Pag 8, line 275: “. . ..decreasing concentration with increasing wind speeds are consistent with expectation based on dilution”. I think that the decrease on eqBC observed during midday can be associated not only to dilution in a more developed PBL of local emissions, but also to the fact that air-masses reaching the site from the lower valley are still not enriched in pollutant. . .Indeed, you observed eqBC (and O3) increase in the afternoon/evening (when both PBL height decrease and air-masses richer in pollution could reach the site from longer distance). You should roughly evaluate the distance of eqBC emissions by analysing wind speed at the measurement sites. . .

Pag 8, line 285. This detailed description of local wind regime needs a more detailed map on Figure 1!

Pag 9, line 294: “Nocturnal decupling of the boundary layer preserves the concentration of. . .”. Not clear: what do you mean with “decoupling PBL”? Decoupling from what? Section 3.4: I assume that this Section should be “Evidence of LONG-RANGE transport episodes. . .”. In this section you discuss three typical regimes of O3 and eqBC variability. However, I would like to see a more detailed description of the main features of each single regime (basically how are you able to distinguish among them?) and a systematic assessment of their occurrence and impact on eqBC and O3 variability.

E.g. which is the frequency of occurrences of these regimes on a seasonal basis? Are you able to objectively identify (by some selection criteria) the occurrence of that regimes? Can you able to compare your results with previous studies? (e.g. Putero et al., Environ. Poll. 2013)

Pag 9, line 304: actually you did not show any evidence of transport to TP! I think this is just a (reasonable) speculation. Maybe you can discuss this possibility in the conclusions Section.

Pag 9, line 310: I think that the relationship between eqBc , O3 and fire emissions in the IGB is only qualitative and deserve more analyses. At least a correlation between the temporal variation of the fire number in the IGP and the eqBc and O3 at the measurement site must be showed. Moreover, air-mass transport analysis (i.e. back-trajectories or dispersion plume) corroborating the transport towards the measurement site region should be showed (the same is valid for case B). For case B and C, eqBc is maximized during night-time, when down-valley winds are expected in a mountain valley and cleaner air-masses from upper layers should be present (add the behaviors of wd and ws!). Please, comment. Moreover, the different co-variability (correlation) between O3 and eqBC should be better investigated and commented. During case B, the diurnal variability of O3 and eqBC appeared to be minimized. Can this indicate the role of far (fires?) emissions instead of regional/local ones? The increase of daytime minima value of eqBc and O3, would indicate a build-up of pollution. Is this build-up only limited to the valley or did it extend to the foothills? Maybe time series of satellite MODIS data can help. . .

Section 3.4.2 is confused and not provides important information: thus, it can be skipped. STE discussion at this point is a little bit out of the scope of the paper. A rigorous assessment of STE contribution to eqBC and O3 variability would deserve a specific investigation. Moreover, I would note that the focus on the monsoon season is of limited interest, since it is well assessed that summer monsoon is the season during which transport of stratospheric air-masses to the lower troposphere is minimized over

the Himalayan region (see e.g. Putero et al., 2016, Ohja et al., ACPD, 2016).

The conclusions are not robust. Actually you did not demonstrate the transport of pollutant from IGP to TP but just the transport of pollution up to the valley. Your main results (seasonal and diurnal O3 and eqBC variability, influence of open fires)should be better commented in the framework of the most recent studies on the topic.

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