

## ***Interactive comment on “Identification of the cloud base height over the central Himalayan region: Intercomparison of Ceilometer and Doppler Lidar” by K. K. Shukla et al.***

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### **1. Summary:**

The authors present a method to derive cloud base height (CBH) from the profiles of signal to noise ratio (SNR) of a Doppler wind lidar (DL). They compare results with CBH from a ceilometer (CM) and with satellite data. Although such a comparison could be valuable the paper misses a lot of possibilities to set results in relation. It does not answer questions like "what are key differences of the retrievals", or "when and why do results deviate from each other". The description of the DL-CBH retrieval is at key points only vague and difficult to understand. The results are interpreted in a very optimistic way as 'good agreement' although large differences can be observed.

C1

Argumentation is in many cases not straight forward but runs in circles around and is full of commonplaces where one would expect details about the observations.

Especially the statement that cumulus clouds are connected to surface processes is repeated several times without investigating when this is the case in the underlying data set. As the Doppler Lidar provides vertical velocity this could be done with e.g. the methods described in Schween et al. (2014), O'Connor et al (2010) or Harvey et al. (2013).

Response: We greatly appreciate the detailed review by referee #3. We have tried to address all the issues that they have raised to the maximum extent possible. These changes are implemented/modified here and also in the text. We hope these changes adequately address all the concerns raised.

### **2 Comments in detail:**

2.1 Title: The title of the paper is misleading: it states that it deals with data from a site in the central Himalaya. Data stems from the ARM mobile facility deployment at Manora Peak (1958m above mean sea level) during the Ganges Valley Aerosol Experiment (GVAX). The final campaign report (Kotamarthi 2013) locates the site in the "foothills of the Himalayan mountain range". A look into a map shows that the site is indeed at the very south-western edge of the Himalayan mountain range but not in its center. Nevertheless the location is interesting as it lies in the sub-tropics under the influence of the Indian monsoon. The site itself provides also somewhat a challenge because it is situated on top of a mountain and local orography might influence cloud formation. But this is not even mentioned in the paper.

Response: Title of paper is modified by considering the relevance of the study and also field campaign report (Kotamarthi, 2013). Importance of site in introduction section-1 (Page-3, Lines: 5-9) and in site details in section-2 are also clearly discussed in the revised manuscript.

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2.2 1. Introduction: One would expect that a paper describing a new method would discuss existing comparable methods. In this case this would be e.g. CBH retrieval methods based on gradients in the backscatter profile as e.g. described in Martucci et al. 2010, threshold based methods as in Van Tricht et al 2014, multisensor, approaches as in Cloudnet (Illingworth et al. 2007), or visibility based concepts as is done by the Vaisala Ceilometers (see Vaisala Oyj (2002) or Morris (2012)).

Response: We have now considered all the suggested references described by different retrievals of the cloud base height in the introduction section (Page-2, Lines:15-20; Lines:29-36) in the revised version of manuscript.

### 2.3 2. Observational site, instrumentation and methodology

The climate information is given as maximum and minimum temperatures (no average, no precipitation) for the months March to May and December to February (why in this backward order?). Unfortunately, this excludes the months October and November during which two of the 6 presented cases occurred. Interesting would be also a discussion of the CBH statistics from Singh et al. (2016) (this paper shares one of the Co-authors).

Response: We have modified the sentences included with all the above details and Singh et al (2016) is cited and also added in the text in the revised manuscript (See Page-4, Lines:09-16).

Singh, Narendra, Solanki, Raman, Ojha, N., Naja, M., Dumka, U. C., Phanikumar, D. V. Sagar, Ram, Satheesh, S. K., Moorthy, K. Krishna, Kotamarthi, V. R. and Dhaka, S. K.: Variations in the cloud-base height over the central Himalayas during GVAX: association with the monsoon rainfall. *Current Science*, 111, 109-116, 2016.

### 2.4 2.1. Total sky imager, TSI

Page 4 line 3: "...we have processed the raw cloud images of the TSI."

Does this mean that the authors made their own retrieval or do they use the retrieval

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of the manufacturer? In every case there is missing a description how the differentiation between opaque and thin clouds works (as far as i remember one can adjust parameters in the manufacturer's retrieval. Is this done here?), and how cloud cover is determined (just counting pixels or by weighting parts of the image differently?). I see that some description appears below in section 4 "Results and discussion" which would rather belong here. And this description is also not complete.

Response: Yes, the retrieval method of the manufacturer is utilized and fine tuning for adjusting parameters is done based on our location preferences. The detail description and references followed to determine the cloud cover is given in Morris, (2005). Description part is also modified in the revised manuscript considering all the suggestions (Page-5, Lines:07-13). "The TSI sky filter thresholds are defined as follows: Clear/Thin determines the ratio of thin cloud cover to clear sky. Thin/Opaque determines the ratio of thin cloud cover to opaque cloud cover. The values are assigned upon initial configuration of the TSI by adjusting each ratio to match the cloud in observed images". The description is moved from the "results and discussion" section 4 (Page-5, Lines: 05-07) to the section 2.1.

### 2.5 2.2. Doppler Lidar

There is missing Manufacturer and type of the instrument. Does it direct Doppler measurement or is it a multi-pulse Lidar?

Response: The manufacturer details are now added as a separate Table (See Page-19) with full technical specifications of the instrument.

### 2.6 2.6. MODIS Obviously a data product from Modis is used: which version, any reference how it works and how accurate it is?

Response: MODIS level-3 (MOD08\_D3.051) data for the CBH retrieval is utilized and the references (Kishcha et al., 2007 and Platnick et al., 2015) describing the MODIS data details are added in the revised paper (See Page-6, Lines:29-30; Lines:32-33).

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Kishcha, P., B. Starobinets, and P. Alpert (2007), Latitudinal variations of cloud and aerosol optical thickness trends based on MODIS satellite data, *Geophys. Res. Lett.*, 34, L05810, doi: 10.1029/2006GL028796.

Platnick, Steven, Michael D. King, Kerry G. Meyer, Gala Wind, Nandana Amarasinghe, Benjamin Marchant, G. Thomas Arnold, Zhibo Zhang, Paul A. Hubanks, Bill Ridgway, Jérôme Riedi.: MODIS Cloud Optical Properties: User Guide for the Collection 6 Level-2 MOD06/MYD06 Product and Associated Level-3 Datasets. Version-1, [http://modis-atmos.gsfc.nasa.gov/\\_docs/C6MOD06OPUserGuide.pdf](http://modis-atmos.gsfc.nasa.gov/_docs/C6MOD06OPUserGuide.pdf), 2015.

#### 2.7 3.1. Cloud statistics from the DL

This is the new algorithm presented and evaluated in this paper. If I understand it correctly it is the same as described in an ARM report by Newsom et al. (2015) but the description here and in Newsom differs and both miss some important details:

Response: Yes, Newsom et al., (2015) algorithm is followed and the estimation methodology of CBH is described in detail in the revised version (See Page-7, Lines:3-5;7-22) of manuscript and also included Figure 4 (See Page-24) for better clarity to the reader.

Page 5 line 32:

"... by detecting the heights of sharp spikes in the range corrected SNR."

Accordingly, the method does not use the profile of the backscatter coefficient but instead the signal to noise ratio (SNR). How is this SNR defined? Is it the ratio between peak power in the Doppler spectrum and noise at larger Doppler shifts? Or is it the ratio between average backscattered signal and standard deviation in the return signal of the multiple pulses? Why must the SNR be range corrected? - I would expect that the range dependence cancels in a SNR. Why is not the backscatter coefficient used?

Response: SNR is defined as mean signal power in the Doppler spectrum divided by mean noise power (Rye and Hardesty, 1997)). The attenuated backscatter is estimated

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by using SNR profile by considering the telescope function and the detailed description about the methodology is given by Hirsikko et al., (2014).

Page line 33:

"...the DL uses a narrow Gaussian filter..."

What means narrow in meters or bins? How is this value motivated? Convolution of the signal results in a smoothed profile. Which is of advantage for the following calculation of the first derivative? Newsom et al (2015) use simply the maximum of the smoothed profile. Here a pair of adjacent strong positive and negative peaks is searched, which must enclose a zero intercept. I guess one may find several in a single profile. How is the best candidate for CBH identified? Is there a threshold for a 'strong peak'? Why is the Newsom retrieval altered?

Response: The Newsom retrieval method has been altered for our requirement. As stated in the revised (See Page-7, Lines:7-22) manuscript, the magnitude of the extrema in the  $d(r2SNR)/dr$  profile must exceed 0.1 km and this filters out many potential peaks and dips. We then look for the lowest peak with a corresponding dip that's between 2 and 15 range bins above the peak. If one exists, then the maximum value of  $r2SNR$  between the peak and the dip. This determines the (lowest) CBH. Alternatively, we have simply find the maximum value of the entire  $r2SNR$  profile, but we found that method results in too much false detection. The derivative technique described in this paper works better because it helps to suppress the range-dependence of the background signal.

Page 6, line 2:

"Additional checks are applied to minimize false detections by rejecting temporally isolated peaks."

How this Additional are checks done (thresholds, range of comparison etc.)?

Response: The additional checks to remove false detection are now described in more

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detail in the revised (See Page-7, Lines:7-22) manuscript. CBH estimates from profiles immediately before and after the current profile are compared and rejected if both differences exceed 1km then the current CBH estimate.

#### 2.8 3.2. CBH retrieval by using CM

The Basic Ranging equation (from LIDAR=Light Detecting and Ranging) is also valid for the Doppler-Lidar described before and one may assume that the interested reader knows the LIDAR principle. More interesting would be here a description of the principle of the CBH determination in the Vaisala instrument. According to the Vaisala manual it is based on a visibility threshold. The Vaisala-CBH typically lies above the maximum in the backscatter coefficient profile as e.g. can be seen on this page of the CEILINEX intercomparison: <http://ceilinex2015.de/special-topics/test>. Insight in the retrieval would probably help to understand differences in the resulting CBH.

Response: We have used the visibility threshold for the detection of cloud base height (Väisälä Oyj (2002) or Morris (2012)) and these references are included in the revised manuscript (See Page-7, Lines:35-37; Page-8, Lines:1-3).

#### 2.9 3.3 CBH Retrieval by MODIS

The authors use constant liquid water content (Hess et al. 1998) which is a global average for clean air cumulus clouds. The liquid water content in Cumulus clouds typically increases with height (water condensates from rising air) and should depend on many parameters like strength of the updraft or temperature. The constant value used here is a global average with a corresponding large uncertainty. One could compare it with values derived with the remote sensing instruments suite of the AMF. Due to this and the rather large grid of the Modis retrieval (1x1deg ' 1E4km2) one could expect that the resulting CBH has a large uncertainty. A discussion of this is missing.

Response: We have now included a detailed discussion on observed differences in the derived CBH with MODIS, DL and CM. However, in the present manuscript main point

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is to show the capability of DL in CBH estimation. A comparison with CM and other ground based and satellite instruments is only to ascertain that how good our CBH estimation with DL is able to represent CBH with CM and MODIS and to our surprise, values with different instruments are matching well in most of cases. However, minor discrepancies observed in some of the cases can be understood by considering the complexity of the location presented and DL CBH estimation done for the first time (Page-11, Lines:23-25).

#### 2.10 3.4 Lifted condensation level estimation by using surface MET and RS datasets

The method described here is somehow in an accuracy-imbalance: Determination of the dew point can be written as  $T_d = \text{invEsat}(RH/100 * \text{Esat}(T))$  with  $\text{Esat}(T)$  the water vapor saturation pressure as a function of Temperature and  $\text{invEsat}(e)$  the inverse of  $\text{Esat}$ , i.e. Dew point-Temperature as a function of water vapor pressure. For  $\text{Esat}(T)$  is used here the Goff-Gratch (1946) equation which is recommended by the WMO as the most accurate one (especially for very low temperatures down to -100degC). For  $\text{invEsat}$  is used in contrast here to the inverse of the Clausius (1850)-Clapeyron (1834)-equation which is rather inaccurate as it does not consider the temperature dependence of the vaporization energy  $L$ . As a result, TD at 100 percent relative humidity will not be equal to air temperature.

Page 7, eq 8 (Lifting condensation Level)

The Espy equation used here assumes a constant vertical gradient of the dew point  $dT_d/dz$ . There are other more accurate formulas as e.g. discussed in Lawrence (2005) or used in Stackpole (1967). Later in the text (page 9 line 16) it is stated that the method of Stackpole (1967) is used leaving the reader in uncertainty what the method in use was.

Response: We have calculated dew point temperature estimated by using equation (11) given in Lawrence (2005). However, in order to avoid confusion, we have now modified the sentences for reader's clarity in the revised manuscript (Page-9, Lines:2-

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3).

#### 2.11 4. Results and discussion

How where the 6 cases selected? Looks as if it is simple one case per month. Are these the only cloudy days? What was the synoptic situation? In Fig 3 are shown time series of the cloud categorization on separated axes with different scaling for thin (black, left axis) and opaque clouds (red, right axis). It would be more convenient to show time series of Popaque and the  $P(\text{total})=P(\text{opaque})+P(\text{thin})$  to get an idea how much of the sky is obscured by clouds and to circumvent the wrong attribution of opaque clouds as thin clouds visible in fig 1.

Response: Indeed, it was very difficult to choose cases in winter because maximum no of days are clear sky. However, we could find some cloudy cases during daytime but unable to get simultaneous datasets with other instruments for other more cases. Hence, considering all the above criteria we could find only 6 cases in those 4 months. We have also presented synoptic conditions of the observational period and modified the TSI description as suggested by the reviewer in the revised manuscript (Page-4, Lines:19-27). As suggested, Ttotal cloud cover with TSI is plotted and shown in Figure-1 in separate file.

Page 8, lines 12-20:

"To classify the thin and opaque clouds, we have performed the red-green-blue ... are given in Slater et al., (2001)."

belongs rather to section 2.1 TSI (methodology). After reading this text it is not clear whether Koehler (1991), Slater et al (2001) or Long et al. (2006) (cited on the same page above) has been used for the categorization.

Response: We have moved the suggested text in the section 2.1 in the revised version of the manuscript. We have also removed the sentences with Koehler (1991) and Slater et al., 2001 in the revised manuscript which creates the confusion to the reader. The

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detailed description about the classification is given in Long et al., (2006) which is used by manufacture of the TSI also (Page-5, Lines:4-12).

Page 8, line 21:

"The dominance of opaque clouds is clearly seen from the figure 3(a-f) during afternoon..." I do not agree: there are indeed more clouds in the afternoon in fig3 a, b, c, but not in the other three cases. Additionally, fig 7 indicates that cloud vertical velocity is only in one case significantly higher around noon.

Response: We meant to say here that opaque clouds are more as compared to thin clouds and we have modified the sentence in the revised manuscript (Page-10, Lines:5-6). Page 8, Line 26... "The development of convective clouds in the lowest part of ABL is due to the presence of convective thermals ..."

It would be great if this would be analysed with this data set: The Doppler lidar provides vertical velocity profiles and it could be clearly detected whether the observed clouds are due to rising thermals.

Response: We have checked the vertical velocity to confirm the rising thermals and found that the updrafts are more dominant for all cases during daytime.

Page 8 Line 30:

Stull and Eloranta 1985 and Zhang and Klein 2013 are missing in the references.

Response: We missed it somehow and now included both the references in the revised manuscript (Page-18, Lines:1-2, Lines:22-24).

Page 9, paragraph following line 3, discussion of figure 5:

"... we have also found that the frequency of occurrence of clouds is higher during afternoon."

Again i do not agree: the cloud frequency is only higher during afternoon hours in cases

C10

e and f (Feb.8 and Mar.14) all other cases show either a decrease or complex patterns. Beside this it would be interesting to compare the 'cloud occurrence frequency' from the Doppler Lidar which relies on the point measurement directly above the site with the cloud cover from the TSI which covers a larger area.

Response: We have now plotted the monthly mean diurnal variation of cloud occurrence frequency by using Doppler Lidar which is given below in the Figure. From figure, it is clearly seen that it is showing different nature in forenoon and afternoon in all the months. During month of October and November, initially it is higher and decreasing trend up to 20.5 hrs and then showing increasing trends. The occurrence of clouds is more in the afternoon in comparison to forenoon during December. In January, it shows higher in forenoon and in the rest of day varies between 30-60%. It is showing increasing and decreasing trend in February and magnitude varies between 30-50%. As Doppler Lidar is giving first cloud base height. Therefore, we have compared the frequency of occurrence with the Singh et al., 2016 and found that an overestimation in all months (October (~3%), November (~ 2%), December (~7%), January (~ 5 %), February (~ 2 %) and March (~ 6 %)). The overestimation of cloud occurrence frequency by Doppler Lidar varies between ~ 1-8 % which could be due to the different techniques and estimation method (Figure-2 in separate file).

Page 9, paragraph following line 11, discussion of figure 6:

Fig. 6 shows the temporal evolution of the CBH from the two instruments together with LCL (fig 6). Differences are visible and can be large (e.g. fig6a after 13.5h LT: diff. = 500m or fig 6f before 12.5: diff>500m) but are not discussed. Instead it is stated that "there is a strong correlation between the CBH observed by the DL and CM for all cases." (Line 13). Similar for the differences between CBH and LCL: they are in the order of several hundred meters but are not discussed.

Response: We have observed an overestimation of the CBH by using DL in comparison to CM which could be due to different retrieval techniques and technical specifications

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of both the instruments. Similarly, we have also observed the difference between LCL and derived CBH in some cases but they are reasonably in good agreement in most of the cases. We have modified the revised manuscript considering all these suggestions (Page-11, Line:6-9).

Page 9 paragraph following line 27, discussion of fig. 7: "From figure 7(a-f), it is clearly evident that the updrafts are dominant due to the diurnal evolution of convective ABL during daytime over the site."

To me this is not clear: cloud base-vertical-velocity (vv) is in most cases decreasing during the day and also shows negative values even in the afternoon (e.g. case a, case c). Again: an investigation in terms of the state of the Boundary layer considering the whole profile of the vertical velocity (Schween et al., 2014 or Harvey et al., 2013) would be a great improvement.

Response: We have now modified the revised manuscript for better clarity by taking boundary layer vertical velocity into account (Page-11, Lines:13-14).

Page 9, paragraph following line 34, discussion of fig. 9:

Error bars in Fig 9 are standard deviations of CBH over the whole day ignoring that CBH shows a distinct diurnal course and thus cannot be seen as an uncertainty for the time of the satellite overpass. It is stated that the agreement with the MODIS derived CBH is good. But as far as i see there are four MODIS CBH values of which only two are good in terms of the overestimated error bars. In my opinion this is not a good agreement.

Response: We have now modified the revised manuscript by clearly stating the similarities and discrepancies observed for each case (Page-11, Line:23-25).

Page 10, paragraph starting with line 4, discussion of fig 10:

"It is noticed that the CBH estimated by the DL is well correlated ( $R^2 = 0.76$ )"

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This is a very positive view on the figure. It would be interesting to see parameters like root mean square error (RMSE), bias and the parameters of the linear fit shown in the figure. The plot shows that differences can be large (up to 500m), that in many cases CBH (DL) > CBH (CM) and that there is a systematic overestimation by the DL compared to the CM when CBH is below 300m. It would be important to discuss these differences in terms of the parameters which are used for the estimate (SNR versus backscatter coefficient), the method used (height of maximum versus visibility) and the state of the boundary layer (stable, turbulent, convective).

Response: The observed differences between the CBH DL and CM around 500m in some cases can be attributed to an overestimation of CBH by DL in comparison to CM. These differences may arise due to different methodologies for the estimation of CBH with both the instruments. It should also be noted that minor discrepancies are observed, however, while considering the complexity of the location presented and DL CBH estimation done for the first time. ABL also plays an important role in the formation of cloud and all the cases are during convective boundary layer condition and it this may be one of the reason behind the overestimation with the DL in some cases (See Page-11, Line:6-9 and Page-13, Lines:15-21).

One further comment:

At several places CBH and cloud cover are named microphysical cloud properties which in fact are rather macrophysical (see e.g. [http://glossary.ametsoc.org/wiki/Cloud\\_microphysics](http://glossary.ametsoc.org/wiki/Cloud_microphysics)).

Response: Microphysical is modified to Macrophysical in the revised manuscript (Page-2, Line:3).

3 References:

Harvey, et al. 2013, "A method to diagnose boundary-layer type using Doppler Lidar, Quat. J. Roy. Meteorol. Soc., 139/676, 1681-1693, doi:10.1002/qj.2068, 2013.

C13

Illingworth, A.J. et al. 2007, "Cloudnet - continuous evaluation of cloud profiles in seven operational models using ground-based observations"

Kotamarthi, VR 2013, "Ganges Valley Aerosol Experiment (GVAX) Final Campaign Report", DOE/SC-ARM-14-011, <https://www.arm.gov/sites/amf/pgh>

Lawrence 2005, "The Relationship between Relative humidity and the Dew point Temperature in Moist Air", BAMS pp225

Martucci, G., Milroy, C., and O'Dowd, C.: Detection of cloud-base height using Jenoptik CHM15K and Vaisala CI31 Ceilometers, J. Atmos. Ocean. Tech., 27, 305-318, doi:10.1175/2009JTECHA1326.1, 2010

Morris V.R.: Vaisala Ceilometer (VCEIL) Handbook, 1 DOE/SC-ARM-TR-020, 2012.

Schween et al. 2014: Mixing layer height retrieval with ceilometer and Doppler lidar: from case studies to long-term assessment, Atmos. Meas. Tech., 7, 3685-3704, doi: 10.5194/amt-7-3685-2014.

Van Tricht, K., et al., 2014: An improved algorithm for polar cloud-base detection by ceilometer over the ice sheets, Atmos. Meas. Tech., 7, 1153-1167, doi: 10.5194/amt-7-1153-2014.

O'Connor et al., 2010, "A Method for Estimating the Turbulent Kinetic Energy Dissipation Rate from a Vertically Pointing Doppler Lidar, and Independent Evaluation from Balloon-Borne In Situ Measurements", J. Atmos. Ocean. Tech., 27.

Please also note the supplement to this comment:

<http://www.atmos-meas-tech-discuss.net/amt-2016-162/amt-2016-162-AC4-supplement.pdf>

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-162, 2016.

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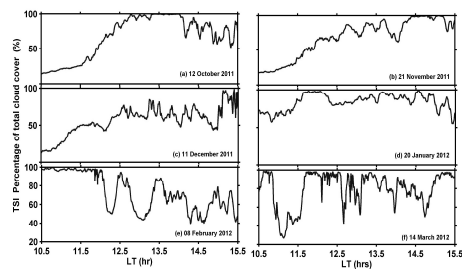


Fig. 1.

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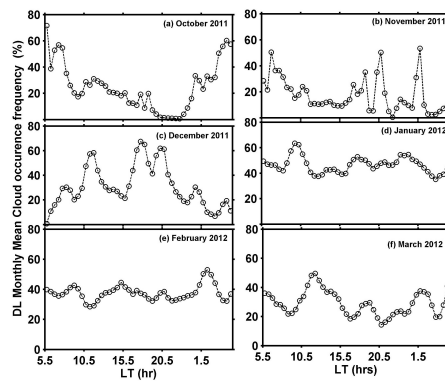


Fig. 2.

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