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Atmospheric Measurement Techniques Discussions

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 cloudbase height (CBH) from the profiles of signal to noise ratio (SNR) of a doppler windlidar (DL). They compare results with CBH from a ceilomter (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.

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), OConnor et al (2010) or Harvey et al. (2013).

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.

2.2 1. Introduction:

One would expect that a paper describing a new method would discuss exisiting 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)).

2.3 2. Observational site, instrumentation and methodology

The climate information is given as maximum and minimum temperatures (no average, no precipiation) 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).

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 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 manufacturers 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.

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 ?

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?

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 differ and both miss some important details:

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 exepct that the range dependence cancels in a SNR. Why is not the backscatter coefficient used ?

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 idenitified ? Is there a threshold for a 'strong peak' ? Why is the Newsom retrieval altered ?

page 6, line 2:

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

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

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.

2.9 3.3 CBH Retrieval by MODIS

The authors use a 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 $\simeq$ 1E4km^2) one could expect that the resulting CBH has a large uncertainty. A discussion of this is missing.

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

The method described here is somehow in an accuracy-inbalance: Determination of the dew point can be written as

Td = invEsat(RH/100*Esat(T))

with Esat(T) the water vapor saturation pressure as a function of Temperature and invEsat(e) the inverse of Esat, i.e. Dewpoint-Temperature as a function of water vapor pressure. For 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 downto -100degC). For invEsat is used in contrast hereto 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 100percent 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 dTd/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.

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 speparated axes with diferent 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(total)=P(opaque)+P(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.

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.

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.

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 analyised 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.

page 8 Line 30:

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

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 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 occurence 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.

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 corelation 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.

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: cloudbase-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 veritcal velocity (Schween et al 2014 or Harvey et al 2013) would be a great improvement.

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 can not 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.

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)"

This is a very positve 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).

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).

3 References:

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