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Atmospheric Measurement Techniques Discussions

Interactive comment on "Cloud base height retrieval from multi-angle satellite data" *by* Christoph Böhm et al.

Anonymous Referee #2

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The authors describe a novel algorithm for the retrieval of cloud base height (CBH) from MISR satellite measurements. Global information on cloud base height is important for many applications and the retrieval approach is interesting and promising. However, the manuscript is not sufficiently convincing in demonstrating the reliability of the new CBH product. Below are a number of major issues to be addressed before this manuscript may be suitable for publication.

General comments

The algorithm is tuned with METAR observations over the U.S., i.e. extratropical land surface. How representative is this for ocean surfaces and other climate zones, where different cloud types prevail? To show the skill in other regions, some comparisons with independent measurements elsewhere would be required.

More information on the success rate of the retrieval algorithm is required to evaluate how useful it is. Statistics of the number of samples n_s are given but these are only absolute numbers, not (fractional) success rates. For example, Table 3 indicates that n_s is between 3059 to 7772 depending on R_fv. A rough calculation based on 1510 ceilometers, a MISR revisit time of 6 days, and a cloud fraction of 50% would potentially yield around 45,000 cloudy collocations. This suggests that in only 10% of the cloudy cases, a valid CBH retrieval is obtained. Is this correct? Such statistics, accompanied by the relative occurrence of different causes of retrieval failure, need to be provided, also for the global plots, to evaluate the applicability of the method.

The calibration of the algorithm is done for z_base smaller than 3000m, because of the limited range of the ceilometers. However, for the global composites an upper threshold of 5000m is used. It is unclear whether this extrapolation outside the range for which the product has been trained, is valid.

The global maps in Figs. 9 and 10 are hard to interpret because upper limits of z_base and z_top have been applied. What does the median of a distribution cut-off at some value tell us? I'm also confused by the description of Fig. 9, which says that the ITCZ is clearly visible with higher z_base and z_top. In the plots a brown band can be seen, but these are lower rather than higher values. Can you explain? Is it also possible that the results in these multi-year median are biased to certain cloud types? For example, in the stratocumulus (Sc) areas west of the continents, cases with closed Sc will probably not yield a valid retrieval, while for open Sc z_base can be retrieved, so that the end result will be biased to the latter.

The authors define percentile (P) values of the MISR lowest cloud layer z distribution to obtain z_base and z_top. For z_base one would expect P=0 because z_base should be lower than any MISR-derived cloud-top height. The chosen value P=15 is motivated by the noise in MISR z, which makes sense. However, for z_top I do not understand the chosen value P=95. All MISR z values are actual estimated cloud top heights. The logical way to aggregate these is to average the individual z observations or take the median. In other words, a value P=50 seems natural. The choice of P=95 should thus be motivated.

Cloudsat is, especially in combination with Calipso, arguably the most accurate source of cloud base height (as well as cloud top height) information from space. Surprisingly, Cloudsat is not mentioned at all (except for two remarks in the context of the Desmons et al. paper) in the manuscript. At the least, Cloudsat should be discussed, and it would also be good to make some comparisons with this instrument, even if direct collocations with MISR supposedly only occur at high latitudes.

Specific comments

P1, Abstract: The abstract does not include any information on the cloud types the algorithm is applicable to. In the manuscript this information is also too limited. Does the method work for cirrus, or for deep convective clouds?

P2, L8: Stephens et al. (2002) is mainly about Cloudsat. It's not the appropriate reference for CALIPSO.

P4, L4: Is 'aftward' correct English?

P5, L6: 'measurements': what is measured?

P5, L9: Is the value 5000 ft correct? It seems such a big jump from 100 ft and 200 ft in the two lower height categories, respectively.

P5, L10: This suggests that bins is the same as clusters, which is not the case, I assume.

P8, L1-3: This paragraph looks out of place here. Suggest to move it somewhere else.

P8, L7: Suggest to replace 'the estimated' by 'a typical'.

P8, L15: what are 'z pixels'?

P8, L25-26: Does this mean that the case in Fig. 4 is not included in the statistics?

Isn't it a bit strange to present a case study that is not part of the selection applied furtheron?

P10, L5: Fig. 4 includes only one 'h_gap'.

P10, L13-14: The second layer detected by MISR has a base height between 5000 and 5500 m a.s.l. The ceilometer detects layer base heights at 853 m, 2286 m, and 7010 m a.g.l. None of these seems to match with the second MISR layer. Can you explain?

P12, Fig. 6: I assume this figure is for N=10 and R_fv=10 km. Can you add this to the caption for completeness?

P12, L6: N=10 seems a relatively low number and one could wonder whether P=15 makes sense for such a small N. Can you comment?

P13, Fig. 7: Can you add a color bar? Is the scale linear or logarithmic?

P14, Table 4: Do you have any explanation why 2007 has 30% more valid retrievals than 2008?

P14, L5: Certainly the different measurement geometry (point over time versus circular area instantaneous) can cause differences. But why would this lead to a bias, and why to a negative bias of MISR in particular? Can't you tune the overall bias to near zero by increasing P?

P15, Fig. 8: I'm not sure how useful this distinction in two geometrical thickness classes is, in particular because this thickness is based upon the MISR retrieval itself.

P15, L3: The termination of the z_base range by the threshold height relates mostly to the lower thickness class, so it would be better to write: 'The smaller E for clouds with a smaller Delta z ..'. P16, L6: Sentence ends unexpectedly.

P17, L1: The sampling size is in Fig. 9c.

P19, L3: effect should be affect.

P19, L32: A mean difference of 500 m is quite large relative to the retrieved z_base in Fig. 11, which appears to vary between 800 and 1200 m for the selected region. Is it reasonable to assume that the model can simulate a reasonable seasonal variation of z_base if it has such a large bias?

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