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Discussions

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Interactive comment on "Using CALIOP to estimate cloud-field base height and its uncertainty: the Cloud Base Altitude Spatial Extrapolator (CBASE) algorithm and dataset" *by* Johannes Mülmenstädt et al.

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In summary, I do not recommend this paper for publication in its current state. The methodology is questionable. The conclusions are highly qualified, and its not clear to me what exactly the authors have contributed to the science. In fact, its not clear to me at all why this methodology is even needed. Let me summarize my primary scientific concerns:

The reason this methodology is needed is that all existing satellite cloud base products

have significant limitations. We do not claim that our product is flawless (and in fact a large fraction of the effort behind this product is dedicated to characterizing the errors); but science is an incremental endeavor, and the product incorporates enough features beyond existing products to make it a significant advance: cloud base heights, validated against ground observations, along the A-Train, for optically thick clouds, including validated point-by-point uncertainty estimates. We address the reviewer's specific concerns below.

Its not clear why you'd need to resolve cloud base from CALIOP when CloudSat can do it. That's the whole point of the synergy between the lidar/radar.

As we point out in the introduction, CloudSat is limited in its ability to detect cloud base because (a) the droplet size and thus radar reflectivity tends to decrease towards cloud base, frequently below the CloudSat detection limit, and (b) the lowest km of the profile tends to be affected by ground clutter. We have included references to these limitations of CloudSat in the introduction, and the manuscript includes a plot and a table documenting that CloudSat cloud base estimates perform worse than Calipso estimates even absent any attempts to correct or select high-quality lidar estimates. At the suggestion of Reviewer 1, the discussion of CloudSat cloud bases has been moved upward in the manuscript; we have also expanded upon the description of the CloudSat cloud base shortcomings in the introduction. We hope that this makes it clearer to the reader why we do not use CloudSat.

Further, its not clear to me why, if you can resolve cloud base with CALIOP for an optically-tenuous clouds, why you'd need an algorithm to understand potential correlation and uncertainty, and thus who'd even use it?

As we state at the beginning of the abstract (l. 2–4 of the manuscript), in the introduction, and again in the conclusions, we are not content to know the cloud base of optically tenuous clouds, but rather want to know the cloud base of optically thick clouds. We list several potential applications in the conclusions that indicate who po-

tential users would be. As to why we would want to understand the uncertainty of a new product, we agree that not all new satellite products include a rigorous uncertainty analysis; however, we feel that this is important information to allow users to judge the quality of the product.

If this paper is going to be publishable, the reviewers needs to go back and very considerately make the case that answers these questions. In my opinion, they have not done so beyond a threshold necessary for publication.

In our opinion, the manuscript was already quite clear about why CloudSat and CALIOP without further processing are unsatisfactory for cloud base height. We have nevertheless tried to make the explanations even clearer in the revised manuscript.

Training of your dataset relative to ground-based ceilometers, as you even state, limits your application to a very small set of cloud types. The authors seem aware of this, but only speculate as to its impact.

It would be desirable to have a validation dataset for oceanic cloud in addition to continental, and we are very upfront about this in the manuscipt. That said, the range of cloud types observable over a year across the contiguous United States is not "very small". In our judgment, releasing a dataset with documented imperfections was preferable to polishing the apple forever. In particular, releasing the dataset makes it possible for others in the community to validate its performance for oceanic cloud if they are aware of a suitable validation dataset that we do not know of. (We note that such a validation exercise would be meaningful even if we did not retrain the algorithm on an oceanic dataset.)

I ask again, who is the customer for this dataset, and how will it advance any scientific interest? How was the 100 km threshold for collocating with ceilometers chosen? What is the correlation length of cloud base spatially so as to justify such a choice? What is the impact on your results if you vary that threshold? There have been efforts (Omar et al. 2013 for aerosols...JGR-A) to collocate CALIOP with ground-based sun phot-

meters. They came up with something like 1500 suitable collocations under a much more stringent set of temporal and spatial thresholds. What you're trying to do requires far more justification and scientific basis, as it goes against conventional/proven thinking otherwise.

The spatial decorrelation of the cloud base height is a good point. In principle, the algorithm learns in the training stage to give reduced weight to more distant measurements, as the RMSE increases with collocation distance, and increases the predicted uncertainty accordingly. In the revised manuscript, we have included a figure on the increase in predicted uncertainty as a function of distance (Figure 6). We find that that the lowest-uncertainty measurements do in fact come from the closest measurements ($D < 40$ km). Interestingly, 40 km is in fact the collocation threshold Omar et al. (2013) recommend (and the reason we have a larger number of collocations is that airport ceilometers vastly outnumber AeroNet stations).

We also note that we provide cloud base estimates with two collocation distance thresholds: 40 km and 100 km (stated in Section 4); the reason for this is to allow the user to make the tradeoff between increased probability that we can provide an estimate at a given location (the 100 km dataset; the smaller collocation threshold provides only approximately 1/5 as many collocations as the larger threshold) and lower cloud base uncertainty (the 40 km dataset).

I recognize that as this is a Discussions page, that the likelihood is that the authors will be afforded opportunity to respond. That's fine. I caution, however, that if this were a more standard journal, I would be recommending an outright rejection.

We are grateful for the opportunity to respond.

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