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Interactive comment on "Comparing the cloud vertical structure derived from several methods based on measured atmospheric profiles and active surface measurements" by M. Costa-Surós et al.

Anonymous Referee #1 (Received and published: 21 July 2013)

GENERAL

The paper compares the performance of different methods to detect the presence of cloud layers from radiosonde measurements. The authors make own proposals to improve one of the techniques. The paper is a pure methodology paper. The authors do not present any new scientific findings about the atmosphere. Nevertheless, the paper can be considered for publication, because a comparison and thorough investigation of the discussed methods is valuable. I recommend that the editor checks again if ACP is the appropriate journal for such an investigation.

We thank Referee 1 for his/her positive appreciation of this work. Please find below the answers of your questions and comments. We hope that the changes that we will introduce in the paper based on the referees' comments surely would improve the study, and would make it suitable for publication in Atmospheric Chemistry and Physics, which is, in our opinion, the most appropriate journal among those edited by the European Geophysical Union.

SPECIFIC COMMENTS

1) The authors use the ARSCL data product delivering cloud properties. They state that active remote sensing of clouds is only done at very few places in the world. What are the other places? Cloudnet provides a rather sophisticated product of vertical cloud distribution as well and should be explicitly mentioned here (Illingworth, A. J., and Coauthors, 2007: Cloudnet. Bull. Amer. Meteor. Soc., 88, 883–898).

The other ARM locations, apart from Southern Great Plains (SGP), in Lamont (OK), that combine data from active remote sensing to produce the Value Added Product ARSCL are (more information in http://www.arm.gov/) North Slope Alaska (Central Facility, Barrow, AK) and Tropical Western Pacific (Darwin, Australia; Manus Island, PNG; Nauru Island).

Also, as the referee points, the Cloudnet sites provide a sophisticated product of vertical cloud distribution that will be mentioned (with the suggested reference) in the final version of the paper's introduction, provided that the paper is accepted for publication. There are three Cloudnet observing stations (Cabauw, the Netherlands; Palaiseau, France; Chilbolton, United Kingdom).Each observatory is equipped with a large suite of active and passive remote sensing instruments (a Doppler cloud radar, a near-IR lidar ceilometer, and a dual-wavelength microwave radiometer) accompanied by standard meteorological instruments (more information in: http://www.cloud-net.org/). Technically, radiosondes are not launched regularly at any of CloudNET sites. However, there are national met service radiosonde sites very close to Cabauw (de Bilt) and Palaiseau (Trappes), which routinely launch twice a day (11Z and 23Z). There is also a site very close to Chilbolton (Larkhill) which also launches at least two radiosondes per day, and often more; typically at 05Z and 11Z.

So, although we could obtain similar data from other database such as CloudNET we used those from the ARM SGP because they are launching routinely four RS per day very close to the ARSCL instruments. We will explicitly state that the study of CloudNET data could be a very interesting future work.

2) Some general weaknesses of the radiosonde methods have to be discussed. These points may have already been addressed in the cited papers describing each single method. However, as the current paper is a kind of review, some more discussion is needed. In particular, the paper should deal with the following issues:

- Radiosondes often perform poorly at low temperature and high relative humidity >80%. The usually observed biases seem to be considered in the threshold values of the different methods. However, the development of these methods partly started in the early 1990s. Since then, new radiosondes have been developed and the measurement of relative humidity has been improved. The authors do not discuss at all on which type of radiosondes their study and the previous approaches are based. How does the error in relative humidity affect the retrievals? Wouldn't it be necessary to adapt the threshold values depending on the applied radiosonde type?

The methods explained in the study are empirically based and the differences between methods could depend on the radiosonde used. The thresholds fixed in every method are intrinsically related to the radiosonde used. We have found that the methods performance seems related with the evolution of the methods itself. In other words, the better performance is found with the latest method presented in the literature. This may be linked to the better performance of the radiosondes used in each study: Wang et al. (2003) found that Vaisala radiosondes perform better in comparison with others like Sippican. This result from Wang et al. (2003) will be mentioned in the introduction.

As the reviewer suggests, we will specify the kind of RS used by every method in the Data and methodology section, if we have this information from the original work. PWR95 and WR95 use RAOBS data, without specifying RS brands or models. Similarly, CE96 use the CARDS dataset and mention the VIZ radiosonde. DS99 use aircraft measurements. Finally, MNS05 and ZHA10 use different models of Vaisala RS (RS80-15LH and RS92 respectively).

Moreover, thresholds used in every method could be adapted for every RS and site. In this context, Zhang et al. (2013) present a study in this direction which must be taken into account for future works. We have not deeply analyzed the effect of changing the threshold since our study is focused mainly on the ZHA10 method, which was developed from Vaisala RS92 radiosondes that are the most widely used RS and can be considered a reference nowadays (e.g. *Flores et al.,* 2013). Therefore we will include this warning in the paper regarding the use of the modified ZHA10 method: “Note that the method was tested with RS92 Vaisala radiosoundings so it should be used with caution if massively applied it to other type of RS or to old databases.”

Nevertheless, the effect of RH error on the retrievals was somewhat tested by changing the thresholds in the ZHA10iLRnew test as compared to ZHA10iLR test. Probably, the uncertainty in the RH measurement (which we assume is represented by the change in the RH thresholds) should not affect too much cloud layer detection; the main expected effect would be differences in the retrieved cloud thicknesses.

Finally, regarding this reviewer comment we will also add this paragraph in the introduction section: “Moreover, using as a reference the Constellation Observing System for Meteorology, Ionosphere, and Climate (COSMIC) measurements, on average the data from most radiosonde types show a nighttime cold bias and a daytime warm bias relative to COSMIC (Sun et al., 2013), with a focus on the upper troposphere and low stratosphere where radiation-induced errors are greatest. Temperature biases also vary among climate regimes and brand types (see Sun et al. (2013) for the correction temperature parameters for each RS type and solar elevation angles). According with this study (Sun et al. 2013), newer sondes (introduced after 2000) have smaller biases than older sondes and appear to be less influenced by cloud effects, perhaps due to the improved sensor technology.”

Anyway, we agree with the reviewer comment in the sense that a homogenization of the radiosoundings and/or an adaptation of the thresholds would be necessary in order to create a climatology of CVS based on RS databases.

[Flores, F., R. Rondanelli, M. Diaz, R. Querel, and K. Mundnich (2013), THE LIFE CYCLE OF A RADIOSONDE*, Bull. Am. Meteorol. Soc., 94*(2), 187-198, doi: 10.1175/BAMS-D-11-00163.1]

[Wang, J. H., D. J. Carlson, D. B. Parsons, T. F. Hock, and D. Lauritsen (2003), Performance of operational radiosonde humidity sensors in direct comparison with a chilled mirror dew-point hygrometer and its climate implication*, Geophys. Res. Lett., 30*(16), 11-1-11-4, doi: 10.1029/2003GL016985]

[Zhang, J., Z. Li, H. Chen, and M. Cribb (2013), Validation of a radiosonde-based cloud layer detection method against a ground-based remote sensing method at multiple ARM sites*, J. Geophys. Res., 118*(2), 846-858, doi: 10.1029/2012JD018515]

- The radiative properties of clouds are determined by their phase and microphysical properties. Radiosondes can only measure relative humidity, they do not yield any information about the phase (liquid or ice) of the cloud which is absolutely critical especially for climate modelling. Can this deficit be met?

No, the radiosonde itself does not give information about the cloud phase. However, it is usually accepted that at temperatures higher than 0ºC clouds are in liquid phase, and at temperatures lower than -40ºC the cloud is in ice phase only (cloud glaciated region). Between 0ºC and -40ºC there is an extended range of possibilities where clouds may be in water, ice or mixed phased form (*Ahrens,* 2009). So, combining the CVS (which can be derived from RS as we investigate in this paper) and the temperature profile, information about phase could be estimated, but this is beyond the aim of our current study.

[Ahrens, D. C. (2009), Meteorology Today, an introduction to Weather, Climate, and the Environment, 549]

- Radiosondes are designed to measure values like temperature, pressure etc... It is generally agreed that these are representative for a whole modelling grid point. However, cloud cover can significantly change over some hundred meters. What is the use to have one measurement point within say 10000 km2? Why is this information as useful as the quality assured products of ARSCL or Cloudnet? I have the impression that with this method a lot of data may be assimilated into the models which are poorly quality assured.

We demonstrated that the ZHA10 method (with our development ZHA10LRnew) performs well with lower vertical resolution, but the decision of using it as an input for a model depends on the criteria of the modeler. Moreover, although CloudNET and ARSCL products are more reliable, in principle, they are also point observations that are considered statistically representative of 100 x 100 km cells in a model, often invoking an ergodic assumption. We find useful the use of RS because there are more than 700 RS launchings every day worldwide while CloudNET and ARSCL products are developed only in a few places around the world.

3) The method of Minnis et al. was developed at ARM SGP Central Facility and uses "empirical parameterizations". Later it is stated that it had to be adapted for Arctic conditions. Again, for climate modelling general globally applicable methods are necessary.

- Can this method meet these criteria?

Obviously, the more general the method is the more applicable to global models (around the world and its different climates) the method will be. An empirical method like MNS05 could be applied everywhere, but it would perform much better at sites with similar conditions and climate to the data used to create the method. Therefore, this kind of methods is usually adapted to particular conditions. Specifically, Jin et al. (2007) discussed the feasibility of applying a RS based cloud detection parameterization (MNS05 method) over sea ice in the southern Beaufort Sea, and suggested some changes in the method to meet the criteria of Arctic conditions. Jin et al. (2007) affirm that since MN05 is developed in a more polluted continental setting, it is not suitable for detecting cirrus clouds at the colder temperatures, and pristine air, of the Arctic. They speculated about the different freezing mechanisms between cleaner (Arctic) and more polluted (continental) super-saturated water vapor air masses. So, finally the method was adapted to Artic conditions by modifying the threshold Pcld (probability of cloud occurrence) to 65% (instead of 67%, which was the Pcld in SGP).

We also recalled in the conclusions that it would be of interest to extend our analyses to other sites, databases and long periods. In this sense, it would be interesting to test the need of changing the thresholds to obtain better results under other atmospheric conditions (climates, sites).

[Jin, X., J. Hanesiak, and D. Barber (2007), Detecting cloud vertical structures from radiosondes and MODIS over Arctic first-year sea ice*, Atmos. Res., 83*(1), 64-76]

- Page: 14418, line 19: "RH values must be converted to RH with respect to ice when temperature is less than -20 ºC". Does this mean that the method assumes ice-only clouds at temperatures lower than -20 ºC? This should be checked thoroughly because there are liquid layers observed well below -20 ºC (e.g. *Zhang et al.,* 2010).

As the referee says, it's known that liquid layers could occur at temperatures lower than -20ºC. But this is how the MNS05 method works, and we restrict our analysis to explaining and applying the method as it was presented in the literature. The authors (Minnis et al 2005) state that "…converted to RH with respect to ice at temperatures less than or equal to 253 K, the midpoint between the freezing point and the homogeneous ice nucleation point."

[Zhang, J., H. Chen, Z. Li, X. Fan, and L. Peng (2010), Analysis of cloud layer structure in Shouxian, China using RS92 radiosonde aided by 95 GHz cloud radar, *J. Geophys. Res., 115*, 1-13]

4) The "total agreement" parameter shouldn't be used like it is. The percentages of "perfect agreement" and "approximate agreement" are summed up making the two different values effectively equal. Another parameter must be found to characterize the performance of each method. One could for example use the "perfect agreement" value alone, because it is out of question that missing one complete cloud layer could be an issue for modelling of any kind.

The reviewer is right, we understand the concern and we deleted the "total agreement" values from tables 4 and 6 and from the discussion to avoid confusions to the reader. In fact, this question is also raised by referee 2 (Major comment 7). Of course, the presentation of results and their discussion will be changed accordingly.

5) The statistical errors of the results ("total agreement") should at least be estimated based on the number of cases used.

The confidence interval (CI_m) for perfect agreement of each method has been calculated and it will be included in the results (Table 4 and 6).

The formula used to calculate the CI_m to a 95% confidence is:

$$CI_m = \frac{1.96 \cdot d_m}{\sqrt{n}}$$

where dispersion d_m is:

$$d_m = \sqrt{\frac{PA_m \cdot \left[1 - \frac{PA_m}{n}\right]^2 + (n - PA_m) \cdot \left[0 - \frac{PA_m}{n}\right]^2}{n}}$$

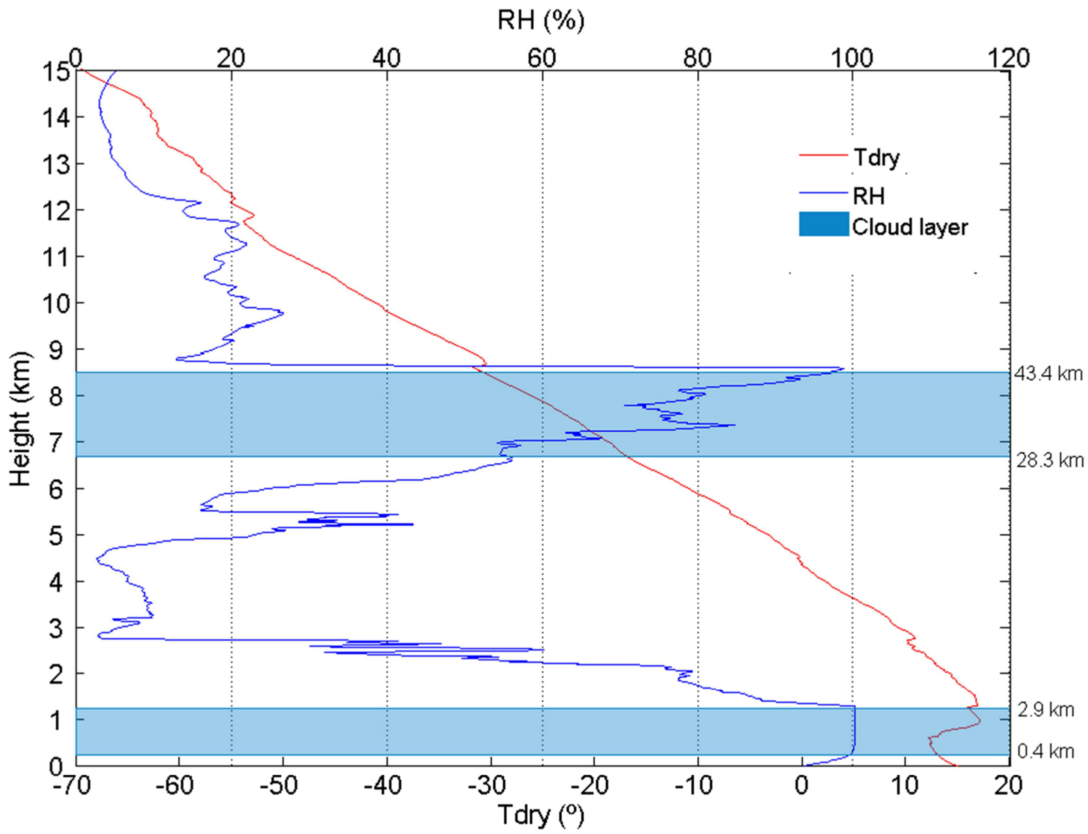
and PA_m is the number of cases of "Perfect Agreement" for each method, and n is the total number of cases.

The inclusion of this confidence interval analysis helps to distinguish among the methods' performance and supports our general conclusions (see new tables with results in the answer to Major comment #1 of referee #2).

Additional questions/hints:

- It would be interesting to see a comparison case study between the ARSCL product and a radiosonde launch at the very beginning of the paper.

A new figure (Fig. 1) will be added and commented in the introduction to show a particular case study.



New "Figure 1". Temperature (Tdry (º), in red) and relative humidity respect to water (RH (%), in blue) profiles above ground level from the radiosonde on October 5 of 2009 at 23.23h at SGP. Blue shading represents the cloud layers as detected by ARSCL. The values (on the right) related to every cloud layer boundary indicate the sonde horizontal distance from ARSCL site in kilometers.

- The authors justify the need of this paper by the need of improving climate predictions. ("The cloud vertical distribution ... is an important characteristic in order to describe the impact of clouds in a changing climate.") Is this really the most important goal? If the climate would not be changing, would this research be unnecessary?

The reviewer is right and, honestly, we must say that this is not the main objective of the study. The objective of the study is purely methodological, as explained in the introduction: "the main objective is to find the best approximation to the real CVS obtained from methods based on radiosonde profiles with a comparison between estimations produced by ground-based active instruments and to reveal the strengths and weaknesses of the methods used."

Our work has an interest in the frame of climatic studies, whether or not the climate is changing. Therefore, we have changed the sentence that the referee mentions, which now is: "The cloud vertical distribution... is an important characteristic in order to describe the impact of clouds on climate."

- It is important to understand the tropics, however measurements are rare. Could the methods presented here fill this gap? Can you estimate how well your methods will perform under tropical conditions?

Including the Tropics is beyond the scope of the present work, which we think is extensive enough. We will add a comment in the paper stating that these results need to be tested in different sites and climate regimes, such as the Tropics and the Arctic regions. It is difficult to estimate the performance of the methods under tropical conditions; the only way to assess the performance would be to repeat the present work (including obtaining and analyzing RS, ARSCL, and geostationary satellite data, all of which are available for the ARM tropical sites) for a tropical site. This should be addressed by a future work.

- Radiosondes are gradually replaced by aircraft measurements. Can the measurements of aircraft also be used instead of radiosondes? (Aircraft usually intentionally avoid cloud layers...)

The reviewer is right: the methods could be in principle applied to temperature and humidity profiles obtained by aircrafts. Moreover, the aircraft profiles are growing in number (in 2008 there were about 150,000 automated aircraft reports per day, Ballish and Kumar (2008)). De Haan et al. (2013) conclude that the accuracy of aircraft meteorological information is of good quality and can be valuable source of wind and temperature information for operational weather forecasting and assimilation in NWP models. However, aircraft tend to avoid clouds (Moninger et al. (2003): "Unlike many other sources of meteorological data such as satellites, radiosondes, and automated surface stations, aircraft can be directly affected by inclement weather – when major storms occur, aircraft often do not fly”). Thus use of aircraft data may potentially introduce a bias in the overall statistics due to biased sampling. Other matters such as the warmer temperatures usually recorded by aircrafts (Ballish and Kumar, (2008) and the trajectory of aircraft profiles (*Schwartz and Benjamin,* 1995) should be taken into account.

Some sentences about this topic will be added both in the Introductions and in the Conclusions (future work) sections.

[Ballish, B. and V. K. Kumar (2008), Systematic differences in aircraft and radiosonde temperatures. Implications for NWP and Climate Studies*, Bull. Am. Meteorol. Soc., 89*(11), 1689-1708, doi: 10.1175/2008BAMS2332.1]

[de Haan, S., L. J. Bailey, and J. E. Konnen (2013), Quality assessment of Automatic Dependent Surveillance Contract (ADS-C) wind and temperature observation from commercial aircraft*, Atmos. Meas. Tech., 6*(2), 199-206, doi: 10.5194/amt-6-199-2013]

[Moninger, W. R., R. D. Mamrosh, and P. M. Pauley (2003), Automated meteorological reports from commercial aircraft*, Bull. Am. Meteorol. Soc., 84*(2), 203-216, doi: 10.1175/BAMS-84-2-203]

[Schwartz, B. and S. G. Benjamin (1995), A comparison of temperature and wind measurements from ACARS-Equipped Aircraft and Rawinsondes., Weather and forecasting, 10(3), 528-544, doi: 10.1175/1520-0434(1995)010<0528:ACOTAW>2.0.CO;2]

- Page 14419, line 12: "agree well" - please quantify (e.g., "agree within XX percent...")

We summarized the conclusion from Zhang *et al.* (2010) with this sentence because we only wanted to do a brief comment. In the original paper (Zhang *et al.*, 2010) this conclusion is extensively detailed: “Cloud detection based on radiosonde measurements compares very favorably with WACR [W-band ARM cloud radar] image data except for a few cases where the WACR failed to detect thin clouds. The absolute differences in cloud base heights from radiosonde and MPL/ceilometer comparisons are less than 500mfor 77.1% / 68.4% of the cases analyzed. The ceilometer fails to detect upper cloud layers due to the fast light extinction at lower altitudes”.

According with the referee suggestions, we will change the sentence to say: “The absolute differences in cloud base heights from radiosonde and MPL/ceilometer comparisons are less than 500 m for 77.1% / 68.4% of the cases analyzed.”

[Zhang, J., H. Chen, Z. Li, X. Fan, and L. Peng (2010), Analysis of cloud layer structure in Shouxian, China using RS92 radiosonde aided by 95 GHz cloud radar*, J. Geophys. Res., 115*, 1-13]

- The Figures should be numbered in the order of appearance.

Maybe the reviewer means that the Fig. 6 is mentioned before Fig. 3, but in fact we presented Figures 2 to 5 in page 17 (14421) because they are so similar and we do not wanted to break the structure. However, based on Major comment #3 from the other referee, we will remove old Fig. 6, making much clearer the order of appearance of all figures. Also, we corrected a typo in page 14420 (18), line 4: Fig. 1b should be Fig. 2b.

- Page 14425, line 21: typo "de" -> "the"

This will be corrected. Thanks!