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Atmospheric Measurement Techniques Discussions

Interactive comment on "Shallow Cumuli Cover and Its Uncertainties from Ground-based Lidar-Radar Data and Sky Images" *by* Erin A. Riley et al.

Anonymous Referee #1

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Dear authors, I believe your manuscript "Shallow Cumuli Cover and Its Uncertainties from Ground-based Lidar-Radar Data and Sky Images" reports on a very interesting and relevant topic which is reconciling cloud cover estimates made from various sensors during continental ShCu conditions which are known to be challenging to observe. Please find below a number of comments I have about the manuscript.

Positive points

o Figures are legible and have appropriate font size.

o Sentences are clear and properly structured.

o I think that the authors description of the many sensor and algorithm upgrades will add value to the literature since these are often difficult to track down and are poorly documented.

o A great novelty of this article includes comparing temporal estimates of cloud cover – from radar, ceilometer and MPL - to domain estimates of cloud cover – from TSI - trying to assess the impact of field of view.

Major comments

o Confusing uses of the terms "cloud fraction" and "cloud cover" and other derivatives

- The term "cloud fraction" is most commonly used to represent the amount of clouds present at different levels in the atmosphere and is most often presented as a profile. Here, where the authors effectively refer to the projected area of clouds at the surface, I think the term "cloud cover" would be more appropriate.

- Here, it would also seem appropriate to introduce two district cloud cover concepts: 1) "Domain cloud cover" which would be the number of cloudy pixels in each TSI image relative to the total number of pixels in each TSI image. This definition would be closest to what a large-scale model would simulate/report and is what we would ideally like to measure (i.e., "truth") and 2) "Temporal cloud cover" which is the number of cloudy pixels in time series relative to the total number of observations collected over a defined time period (e.g., in ceilometer or radar time-series over 30-min or in a TSI pixel over 15-min).

o The first goal stated by the authors is very similar to work undertaken by Kennedy et al. 2014. The authors first goal reads: "1) Have significant changes in the observations of ShCu cover occurred at the SGP site due to instrumental and algorithmic upgrades?". While I can appreciate that the current work tackles an extended dataset, to further add value, I wish it also went into more details about what are the exact algorithm changes or sensor upgrades responsible for the observed differences. For

instance:

- Given what Kennedy et al. 2014 stated about the radar: “sudden change in CF occurs around the time the radar was upgraded, suggesting that this decrease is tied to hardware sensitivity or scanning strategy changes.” The current study could quantify the additional amount of cloud detected solely by the radar sensitivity increase following the change from the MMCR to the KAZR.

- Given what Kennedy et al 2014 reported about the MPL: “Addition of the MPL increases the 14-year average CF by 9 %, mainly through an increase in optically thin high clouds year-round, and mid-level clouds during the summer months.” In the current study, what can the authors say about the relative importance of such a sensitivity boost relative to the number of MPL false classification for ShCu. What exact changes were implemented following 2011 to improve the MPL cloud mask and can you recommend any further algorithm modification which could help mitigate the number of false ShCu detections?

o I think the second goal of the authors should take the forefront as it tackles something that remains poorly documented in literature. As it reads in the manuscript, the authors second goal is: “2) what is the impact of FOV configurations on hourly and sub-hourly observations of ShCu cover?”. Taking it a step further I would be curious to know:

- Can narrow-field of view sensors be used to estimate a cloud cover representative of a domain? and if so under what circumstances (e.g., strong horizontal wind, high cloud cover, ect.)?

I think the authors results could be used to answer this question. For instance, while the biases the authors focus on in the abstract and in the main text are between the ceilometer and TSI as estimated using a 17-year long dataset, the 1 to 1 correlation between ceilometer and TSI estimated cloud cover reported in Table B3 suggest the cloud cover statistics estimated by the two sensors converge rapidly such that ceilometer point observations could be representative of the TSI domain observations on short

timescale. The 1 to 1 correlation also improve using hourly instead of 30 min time windows. Would the authors say that this implies that hourly cloud cover estimates from ceilometer are equivalent to hourly TSI domain cloud cover? What is the optimum averaging time period where temporal cloud cover become equivalent to domain cloud cover? Would that depend on horizontal wind speed or cloud organization?

o Did the authors consider the effect of horizontal wind speed in the comparison of domain cloud cover and temporal cloud cover? I would expect that higher horizontal wind speeds advect clouds more rapidly such that, under higher wind speeds, shorter time periods of narrow-field of view measurements would be required to capture the CF observed by the wider field of view TSI.

o The spatial analysis of TSI cloud mask is very interesting but I had to read the article twice to understand where is fits in with the other cloud fraction definitions. What would make this clearer for me would be to state that the TSI “lane by lane” cloud fraction estimates are effectively temporal cloud fraction estimate (and not FOV or domain cloud fraction estimates) and that each lane can be interpreted as a time series observed by a narrow field of view sensor. I would also perhaps bring information about this lane by lane methodology and information about the radar wind profiler to section 4.3 where you describe your tool.

o Clarifications regarding the impact of insects on ShCu top detections

- Multiple studies have reported that the presence of insect hinders the radars ability to accurately detect cloud top. I think more information is needed here about how insect contamination is handled in ARSCL both pre and post 2011 where the authors hint that the MPL stopped being applied in the boundary layer. This could offer an alternative explanation to the changes in radar-lidar CF post 2011 where the increase in radar detected cloud top could be due both to the KAZR being more sensitive than the MMCR and to the KAZR insect filtering having changed such that more insect returns are misclassified as cloud tops. If both effects are in play, then I would like to

see their relative importance quantified.

o The idea of compensating bias introduced on Page 8 “introduction of compensating errors using the cloud top height criteria in the updated merged lidar-radar product.” needs clarification.

- If I understand correctly the hypothesis is that in the 2000-2010 period the MPL was overly sensitive to aerosols leading to a CF overestimation while the MMCR was underly sensitive to cloud leading to a CF underestimation hence the compensating bias.

o There are gaps in the literature review

- Beyond the few studies cited on Page 2 line 30, others before have attempted to assess the representativeness/reconcile multiple cloud fraction measurements (e.g., Dr. Mariko Oue work with scanning cloud radar or Dr. Steve Schwartz work with photography or Dr. Wei Wu work with ISCC).

- Some references are missing for the bibliography (e.g., Tatarevic and Kollias, 2015)

- Some references are to meeting abstracts rather than to the published journal articles (e.g., Lamer et al. 2017 abstract work has since been published in GMD)

- Some references are miscited (e.g. Chandra et al., 2013, Zhang and Klein, 2010, 2013 and Lamer and Kollias, 2015 do not show any model-observation comparison).

o Although I understand that there are many ways to organize a methods section and that we all prefer to receive information in different sequences, for me, the layout of the data and methods section was confusing.

- I would rather the authors merge the data and methods sections which go hand in hand and preface such a section stating what quantities they are after 1) Identification of cumulus cases, which requires cloud top height and cloud cover estimates 2) Temporal cloud fraction, which will be obtained from ceilometer, ceilometer+MPL, and ceilometer+MPL+radar cloud base height time series 3) Domain cloud fraction, which

will be obtained from TSI using different angular domains 4) Providing context using horizontal wind direction and TSI lane-by-lane decomposition

o Clarification are needed when it comes to ShCu case selection

- Page 4, Line 3: Which observations are used in the ARM Shallow Cumulus data product to identify ShCu cases?

- Table 1 somewhat helps in understanding which portion of the cloud field is of interest. However, I do not see a reference to Table 1 in Sect. 3.1. where I would expect it.

- Figure 2 shows cloud fractions ranging from 0 to 1, do you consider overcast conditions to be ShCu?

- Page 4, Line 8: Why did you chose to: “additionally extending the start and end-times by 1 hour each.”. This could include periods presenting deeper clouds or cloud aloft.

- Table 1. Why is a minimum cloud base height threshold applied to most CF estimates? If insects and clutter have been properly filtered from the radar data, I cannot see why this could be necessary.

- Table 1 What is the value of estimating a CFtot if the cases discussed are purely single layer ShCu? Shouldn’t all cloud observed have tops and bases below $\sim$3 km?

o Pertinent information missing in the various definitions of cloud fraction

- Page 3 Line 15: “Appendix A contains pertinent information for their application”. I believe all pertinent text should be in the main text and the appendix should be reserved for details. I am especially wanting to know how the ARSCL reports cloud top height when both the radar and MPL are used (in the 2000-2010 period) since this is very relevant to the sensitivity versus insect detection compensating effect.

Minor comments:

1) In the future, when submitting articles for review, please number the lines continu-

ously rather than restarting the numbering process on each page.

2) The abstract is very “number focused” and could benefit from including more “conclusions”, for instance, the abstract does not provide information about which sensor upgrade had the largest impact on the cloud cover estimates or about the fact that cloud field organization (e.g., cloud streets) parallel to the horizontal wind direction can create large biases between narrow and wide field of view cloud cover estimates.

3) Page 1 Line 22: What is meant by “mean cloud cover”

4) Page 2 Line 2: Missing some “the”

5) Page 2 in a few places. I would suggest using the word “variability” instead of the word “changes” when referring to the cloud field

6) The acronym for the Ka-band ARM Zenith Radar should be entirely capitalized (i.e., “KAZR” not “KaZR”)

7) Page 2 Line 18: What do you mean by “consistent”?

8) Page 2, Line 18: Zhang and Klein 2013 used 13 years of ARSCL data, the sentence as you have it constructed is somewhat misleading as it suggests that they used 20 years of data. It would be more appropriate to state that previous studies have used ARSCL (cite here) and this data record is now reaching 20 years in length.

9) Page 2 Line 23: Following my suggestion above “Areal cloud cover” would become domain cloud cover.

10) Page 3 Line 1: Given that the radar can be affected by insects, I would avoid using the word “reliably”.

11) Page 4, Line 4: Shouldn’t “ShCu cloud coverage” read “ShCu cloud periods”?

12) Figure 2 Panels a and b are missing a legend

13) Figure 2 c and d and all figures of this style are missing colorbar labels

14) Page 4, line 31: “This method has the advantages of low missing data due to multiple instruments used and limits the vertical extent of clouds.” Please rephrase. Using a cloud top detection criteria does not “limits the vertical extent of clouds”.

15) Page 9 line 4: Add “altitude” after “1.5 km”

16) Page 2, Line 29 “In addition, long-term averages of CF obtained from merged ceilometer-MPL data tend to be larger than FSC (Boers et al., 2010; Qian et al., 2012; Wu et al., 2014; Kennedy et al., 2014), indicating a potential consequence of instrument-dependent cloud detection differences.” Could this difference not also be attributable to FOV differences? If so, please add this caveat.

17) Page 1 Line 19 “We demonstrate that CF obtained from ceilometer data alone and FSC obtained from sky images provide the most similar and consistent cloud cover estimates: bias and root-mean-square difference (RMSD) are within 0.04 and 0.12, respectively.”

According to your analysis of the impact of the “Field Of View (FOV)” performed by comparing the two TSI FOV, the averaging period of the narrow field of view sensor can affect the RMSD between cloud cover obtained by the narrow and wide FOV. Am I correct to understand that this result also applies to the comparison between the ceilometer or any other “beam” observation (e.g., radar, MPL) and the TSI? If so, I think the statement above should include information about the averaging time period used for the ceilometer in this comparison.

18) Page 8 line 24: “Though a number of differences exist, the incorporation of MPL data below 3 km in the initial cloud top height retrieval algorithm between 2000-2010 but not the updated algorithm likely has a large impact (see Sect. A.4 for more details).”

I think it would help the reader to explain if an overestimation or an underestimation is expected and why?

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