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Interactive comment on "Multi-layer Cloud Conditions in Trade Wind Shallow Cumulus – Confronting Models with Airborne Observations" *by* Marek Jacob et al.

Anonymous Referee #1

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This manuscript uses simultaneous observations from a Ka-band radar and a visible/near-IR lidar aboard a high-flying aircraft to evaluate the distribution of cloud-top and cloud-base heights in shallow cumulus in two simulations with varying grid resolution. The study is focused on winter-time conditions near Barbados, site of the NARVAL experiment, where shallow (warm) clouds and precipitation dominate. After discussing how cloud tops may be determined from lidar and radar observations, and how cloud base might be identified in radar returns, the authors describe how synthetic observations are constructed from regional simulations with the Icon model at "storm-resolving" kilometer-scale horizontal resolution and "large-eddy model" at 300 meter resolution and a more complicated treatment of microphysics. The observations show

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a bi-modal distribution of cloud top heights in the more sensitive lidar observations, one corresponding to well-developed shallow convection and the other attributed to very shallow clouds (as diagnosed by the small difference between the lifting condensation level and cloud-top height) which is better reproduced in the higher-resolution configuration. Sorting results by liquid water path illustrates a range of model errors with roots in some combination of microphysical and dynamical processes. The 1.25 km configuration, unsurprisingly, does not perform very well especially in thin clouds.

The manuscript is sound: all steps along the way are described in detail and appear technically correct, and the comparison between observations and model results is careful. The manuscript would be most improved by providing more context, motivation, and narrative structure, so that the inferential path readers are asked to follow becomes more clear. This might be accomplished in extensive if minor revision; the authors might also choose to undertake a more thorough re-thinking.

Readers will be especially grateful for a scientific motivation as to why it's important to look at the distributions of lidar- and radar-derived cloud top and cloud base heights. What model deficiencies does such a comparison highlight? What hypotheses might be tested by examining these statistics? Why have the authors chosen to strike this particular balance of details (e.g. with forward simulation of lidar and radar reflectivities) and abstraction (identifying cloud top and cloud base)? Additional explanation of the motivating ideas would be very welcome. The authors might also use a hypothesis to help prune away a little extraneous material.

As a related point the final section is more speculative than is satisfying. Readers will recognize that the scale of these simulations makes it difficult or impossible to produce variants. They might nonetheless expect the answer to a question or at least evidence for or against a hypothesis. The authors suggest, for example, that the inability of the storm-resolving configurations to reproduce observations in Figures 5 and 6 is due to microphysical choices or possibly vertical resolution. They neglect the possibility that the horizontal resolution is key, which one might argue based on ideas about the scale of buoyancy production. Is it possible to distinguish between these explanations on the basis of the observations, or might the observations be interrogated differently to provide this insight?

The authors combine a sophisticated and careful calculation of synthetic radar and lidar signals, for which differences in the microphysics schemes used SRM and LEM simulations are likely germane, with a very simple analysis based on masking and stratification by liquid water path. How did the authors decide on this approach? Similarly, what motivates the assumption to ignore rain in computing lidar returns (line 200)? It's true that rain is quite unlikely to affect the lidar returns but such an assumption seems inconsistent with the level of detail used in other parts of the calculation.

The substance of the comparison lies in figures 5 and 6, which show the distribution of cloud tops detected by lidar and the tops and bases detected by radar from the observations and the two simulations. Figure 6 elaborates on Figure 5 in sorting by liquid water path. One wonders if other analyses might also be informative, especially a look at the joint distribution of lidar and radar cloud top or the joint distribution of radar top and base, depending on the questions motivating the comparison.

Section 5.3, which primarily describes Figure 6, would be enhanced by focusing on interpretation of the figure in lieu of description.

General minor points:

The referencing is quite heavily biased towards contributions from Germany. The use of sorting analyses in terms of column water vapor is due to Bretherton et al. 2005 (doi:10.1175/JAS3614.1); the sensitivity of cloud fraction to observing system has been discussed since the 1980s and covered exhaustively by Stubenrauch et al. 2013 (doi10.1175/BAMS-D-12-00117.1), etc. A more balanced and complete view would benefit readers and encourage the first author to read widely.

The manuscript's use of language would benefit from polishing by one of the senior

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authors. There are more than a few typographic mistakes and a relatively large number of not-quite-standard English constructions which distract unnecessarily.

The American Meteorological Society, at least, prefers “liftING condensation level” to “liftED”.

Specific minor points The sentence spanning lines 49-50 is vague, provocative, and probably unnecessary.

Section 2, describing the observations, is roughly 55 lines long. Almost half of these and one of two figures is devoted to a discussion of radar sensitivity and the definition of a threshold. Is this point important enough to warrant this level of attention?

Line 153: The line linking the parameterization suite to NWP is perhaps distracting. The authors are careful to describe differences in the microphysical approach in the SRM and LEM later, and to motivate why these might be relevant to the evaluation. This text might raise more questions than it answers.

Line 175: “forward simulations” of what?

Line 182: That forward operators need relevant state information is well-understood. Is there another point here?

Line 193: It does seem a bit odd that the synthetic lidar observations are created with a Radar Simulator while the synthetic radar observations are created with yet another package, but maybe there’s nothing to be done about that.

Line 199: The relevance of water on a telescope to observations from a platform high above the clouds is not obvious.

Line 237: Is the finding that precipitation drops do not extend to the very highest reaches of shallow cumulus novel?

Line 259: A better motivation would be useful here. Readers will not expect a case study to be representative of the entire data set.

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