Review for *Spaceborne differential absorption radar water vapor retrieval capabilities in tropical and subtropical boundary layer cloud regimes* by *R. Roy et al*

The study assesses the capabilities of spaceborne Differential Absorption Radar (DAR) for tropospheric humidity profiling in typical planetary boundary layer (PBL) cloud conditions along the transition from sub-tropical to tropical regimes. Five different LES model cases are used to simulate spaceborne radar measurements at 155.5, 168 and 174.8 GHz. DAR capabilities are evaluated based on the simulated measurements for each scene regarding the retrieval of in-cloud water vapor profiles, sub- and above-cloud water vapor amount, as well as IWV in clear-air scenes. Application examples are given for deriving near-surface RH and in-cloud temperature profiles. The authors analyze expected uncertainties and instrument requirements and conclude that spaceborne DAR holds the potential of filling current observational gaps of PBL profiling.

The study presents a suitable and novel framework to address the current observational gap of PBL profiling. It is an important contribution to the field with exciting results, and particularly timely as DAR and G-band radar technique are advancing. The study is clearly written and well presented. It comprises a very detailed presentation of the applied forward simulator and retrieval algorithm method, as well as an extensive results section. The amount of material makes the manuscript quite long. I have some major and minor comments of what is otherwise a convincing manuscript.

General comments:

1. DAR aims at filling observational gaps in PBL profiling. The authors should emphasize which type of variability can be resolved by their approach, and if this meets the needed requirements to characterize the analyzed regimes.
2. The use of a scale height for exponential humidity interpolation requires more justification. In trade wind conditions, the humidity profile strongly deviates from an exponential profile. The limitations should be further discussed and the impact of this assumption on the retrieval results should be quantified in more detail.
3. Intelligent scanning technique is a powerful tool for future satellite-based applications. When applying this sampling strategy, do the authors account for the tilted, off-nadir inclination angle and the resulting change of surface NRCS in their forward simulations? If not, a quantification of the impact should be added.
4. The authors add a third frequency to the standard 2-frequency DAR approach. More information should be provided regarding the choice of the channel. The authors should also highlight in which conditions and at which heights signal saturation occurs in one of the three channels, e.g. as function of hydrometeor and gas loading. Which conditions would be particularly favorable and which are most challenging for spaceborne DAR to measure in and fill existing observational gaps?
5. The manuscript contains a lot of material, at times distracting the reader from the main messages. For example, the authors develop and present a radar forward simulator in detail as tool for their analysis. Why did they not consider an established tool such as CR-Sim (Oue et al., 2020; https://gmd.copernicus.org/articles/13/1975/2020/) or PAMTRA (Mech et al., 2020; https://doi.org/10.5194/gmd-13-4229-2020/)? Changes should be highlighted, also regarding the simulator presented in Millán et al., 2020 (https://doi.org/10.5194/amt-13-5193-2020). Many details on radar simulation are extensively described by literature. Thus, the authors should
   (i) move technical details in Sec. 2.2. to the appendix;
   (ii) reduce their selection of Figures 5-12; e.g. by compressing the presentation of the simulated measurements and/or moving them to Sec. 2.2.3 or the appendix

Specific comments:

1. Two relevant contributions to this field are missing in the introduction: Lamer et al (2021), doi.org/10.5194/amt-14-3615-2021 ; Schnitt et al (2020) , doi.org/10.1175/JTECH-D-19-0110.1
2. p.3, ll. 16 More details should be provided on selection of 155.5GHz channel as third frequency. Why was the channel at 167 GHz (Roy et al, 2020) moved to 168 GHz here?
3. p.4, tab. 1: the following LES parameters should be added: vertical top height of LES simulation; longitude and latitude of domain center; considered time step; IWV variability within the domain. I suggest to indicate mean and standard deviation. IWV (or TCWV?) should be used consistently throughout the manuscript. I also suggest to quantify the variability of ρ_v and IWV in the respective simulations to give the reader a feeling on the expected variations of the respective moisture fields.
4. p.4, ll.5: A clarifying sentence should be added on how the different horizontal and vertical resolutions of LES and MERRA-2 were accounted for when completing the LES columns. A reference and specifications to the used MERRA-2 dataset are missing.
5. p.4, Sec. 2.2: also see major comment 5. The authors should highlight the advantages of their radar simulator compared to existing radar forward models.
6. p.8, l.17: Which 'relevant variables' were chosen?
7. p.9, tab.3: range indication for dBZ_{min} and explanation for * in horizontal footprint line are missing.
8. p.11, Fig.2: I suggest to add the reflectivity profiles of f_0 and f_2 to the right panels of (a) and (b), respectively. Considering a detection threshold of -34dBZ as given in Tab 3, a detection of the profile as shown in (b) below 2km does not seem possible. A clarifying statement should be added on how saturated profiles are dealt with in the retrieval, and how often saturation occurs in the 5 different LES cases. How many profiles were simulated in total? CFAD diagrams could give the reader an additional overview on the simulated radar measurements.
9. p.15, ll.11: the authors should clarify how exactly they chose the profiles that were sampled when applying intelligent scanning sampling strategy.
10. p.15, ll.11: Do the authors think that an intelligent scanning technique could be, in a realistic application, combined with adjusting one of the frequencies to overcome sensor saturation for the expected conditions?
11. p.16, Fig.3: figure caption for panel (d) is missing.
12. p.17, ll.2; Fig. 4(c) (and 6,8,10d): The authors should add a clarifying comment how the ρ_v precision changes at different heights (e.g. lower BL below 3.9km in the GATE case), or why the specific shown height was chosen. I suggest to add a clarifying statement, or to add the precision at different heights to the respective panel.
13. p.24, Fig.13: This figure summarizes the results of the intelligently scanning method nicely. How do these results differ when applying the analysis to the nadir sampling strategy? I suggest to address this in the text to uphold figure clarity. The authors should add to their discussion whether the derived along track averaging distance is sufficient to resolve the expected horizontal water vapor variability.
14. p.25, ll.18: I am wondering if the required fulfillment of the 200m vertical resolution systematically excludes certain conditions – can the authors provide a threshold or typical cases in which this requirement is not fulfilled?
15. p.26, tab. 4: A column giving the surface temperature should be added.
16. p.26 ll.20: A reference for this statement should be added.

17. p.27, l.15: the authors should provide more information about the applied LWC > $0.01gm^{-3}$ threshold and the in-cloud temperature derivation for thinner clouds. How does this threshold impact the presented results?
18. p.27: Fig 14b) is referenced before 14a).
19. p.28, conclusions: based on their experience with the present analysis, I encourage the authors to add a concluding remark how their simulated system would perform in different conditions such as at higher latitudes.