Review of the manuscript:
"Cloud Drop Number Concentrations over the Western North Atlantic Ocean: Seasonal Cycle, Aerosol Interrelationships, and Other Influential Factors"
by Dadashazar et al.

General Comment

In the paper "Cloud Drop Number Concentrations over the Western North Atlantic Ocean: Seasonal Cycle, Aerosol Interrelationships, and Other Influential Factors" by Dadashazar et al., the authors investigate cloud droplet number concentrations (Nd) and its influential factors on multiple scales on the basis of diverse observational data sets. The analysis first describes high-resolution aircraft measurements of a flight from the ACTIVATE campaign, and puts these high-resolution measurements into a wider context with the description of the general seasonal cycles of cloud/aerosol/meteorology of the region. The authors go into the details of aerosol size and vertical distributions and calculate aerosol-cloud-interaction (ACI) statistics in different seasons. Potential influential meteorological factors are described in two analyses, using a composite approach by contrasting high and low Nd days, and by the application of a machine learning algorithm. Some of the main findings of the paper are that ACI is generally strongest during DJF, when Nd values tend to be highest, and that high Nd days are shown to feature systematically different meteorology (e.g. stronger continental outflow) when compared to low Nd days.

The topic of the paper is highly relevant to the aerosol/cloud/climate community and of high interest to the readership of ACP. The paper presents a thorough analysis of comprehensive observational data sets that advances the scientific understanding of the observed Nd patterns of the region. The paper is well written and structured and displays high-quality figures. I have only some minor points the authors need to address and some specific remarks that the authors may want to consider. I therefore recommend the manuscript for publication in ACP after minor revisions.

1 Minor Points

1. I think the authors should discuss vertical velocity as one of the main drivers of Nd variability in some more detail. The authors do this to some extent already in the manuscript, but I think it is necessary to point out that this is likely an important factor which is e.g. not provided as an input to the GBRT models. I would suggest that the authors include near-surface wind speed as a proxy for boundary layer

turbulence/updrafts in the GBRT, especially since winds seem to be an important factor in the composite analysis.

2. In section 2.3 I am missing information on the hyperparameters of the GBRT models and how these are tuned during the training/validation phase. This is critical to be able to reproduce the results and informative for readers interested in the technical details of the model setup. In my opinion, this information could be provided for in a table and may be best suited in the supplement, though.

3. What is the temporal relationship between the Nd and precipitation data? I believe it would be good to a) describe the time of satellite observations in subsection 2.2.1 and the precipitation data in 2.2.3 and b) discuss the potential influence of temporal offsets for the purpose of analyzing wet scavenging effects. I am also wondering why the authors did not chose to use information on precipitation from Cloudsat given they already use A-train data.

4. I think it would beneficial to briefly comment if the vertical distribution of aerosols at nighttime (used here) is excepted to be significantly different from the daytime (rest of the data sets analyzed here).

5. This is just to initiate discussion: In the air-quality community, it is well known that the seasonal cycle of satellite-AOD and near-surface particle concentrations over continental regions frequently show contrasting seasonal cycles (e.g. Koelemeijer et al. 2006, 10.1016/j.atmosenv.2006.04.044). This is caused by effects of PBLH and BL humidity (Stirnberg et al. 2018, 10.3390/rs10091353), and can be corrected for to some degree (Arvani et al. 2016, 10.1016/j.atmosenv.2016.06.037). I believe that at least qualitatively there is something to learn from these findings that have implications for the ACI community (and this paper specifically) as well, especially in studies covering continental regions or regions of strong continental outflow. I don't think the authors have to discuss this in their paper, but it may be a useful discussion to have in the ACI community and in my opinion links well to the findings presented here. The authors may chose ignore, comment or discuss this point as they find most useful.

2 Specific Remarks

l. 58 In my opinion, the word "potentially" does not apply to "enhanced cloud albedo" in the case of increased Nd and constant LWP, but only applies to the latter two points.

l. 206 There is a typo: "supermicromemter"

l. 209 Please use SI units (760 torrs)

l. 385 I think Stier (2016, 10.5194/acp-16-6595-2016) is a relevant source that should be cited here.

l. 482 "usually always" - please remove one of them