

## ***Interactive comment on “Investigating the liquid water path over the tropical Atlantic with synergistic airborne measurements” by Marek Jacob et al.***

### **Anonymous Referee #2**

Received and published: 8 April 2019

This manuscript provides a nice overview of the LWP retrievals for the 2 NARVAL campaigns. The documentation will be a useful resource for future publications. I think a bit more discussion of the science possible with the integration of the radar and Lidar datasets, and of the radar data itself would enhance the scientific impact of the manuscript, but these constitute minor comments, incorporated into those provided below. Also, while I am not sure of the Copernicus standards, I would recommend that DOIs be generated for the datasets and included within the manuscript.

Minor comments:

p.2 line 14: also mention the clear-sky contribution to the field of view (it is mentioned

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later but the sentence suggest precip is the major error source).

p. 2 Line 27: what do Greenwald et al and other conclude for the tropical Atlantic region you are interested in?

P. 3. some where I think the adiabatic constraint on LWP is worth mentioning. Is it possible to construct an adiabatic estimate from the Lidar cloud top height and dropsonde RH-derived cloud base do you think? This is an earnest question - I am not sure how well this would work. But it would provide an additional constraint on the retrieval that might be more physical than the imposed  $1000 \text{ g/m}^2$  (and its relaxation), and could also provide some additional physical insights. For example, in clean marine stratocumulus regions, the adiabatic constraint on LWP seems to hold well until about  $200 \text{ g/m}^2$ , at which point precipitation begins to deplete LWP (Zuidema et al, 2005, fig. 8 and 9). I think during RICO the adiabaticity deviated more quickly from the theoretical maximum because of mixing with environment air (Raubert et al., 2007). Related to this I do not see any discussion on the radar or cloud top height at which precipitation becomes discernible later on in the manuscript - perhaps I missed it.

P. 3 lines 11-19: what is approximately the spatial footprint of the HAMP instrumentation? It would be nice to see this number in relation to the satellite spatial footprints. On p. 6 you mention that the different footprints and sensitivities of the instruments are covered in Stevens et al 2019, but a brief summary here would be useful.

P. 7 line 23: how is scattering off of the ocean surface dealt with?

p.9 line 24: where is the ocean emissivity represented? It would be nice to see a bit more discussion of the ocean surface microwave radiation characteristics in general. A figure of the emission/scattering as a function of SST and wind speed, for the 2 frequencies would be nice, for example. How much does error in the surface characterization contribute to the overall error?

p.13 line 4: Is there any cloud fraction within a model column? At a grid spacing of 0.5

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degree, clouds will not necessarily fill the full grid box.

p. 17 : it would be nice to see the retrieved LWP/RWP as a function of the vertically-integrated reflectivity from both campaigns as part of fig. 11. They should look the same, if not, that may tell you something about the cloud droplet number concentration variation between the two seasons.

How does RWP spatial heterogeneity affect the retrieval do you think?

P. 18: how does WVP vary in this example?

p. 19, lines 17-18: Some discussion of the sampling of the diurnal cycle (I presume HALO only flew during the day, were cumuli more prevalent in the afternoon?), and how that might alias into the results from the 2 seasons would be nice. I presume the BCO LWP measurements mentioned are diurnal averages

P.11: an easy additional plot would be how LWP and RWP vary with lidar-derived cloud top height. This would be of scientific interest. How would that compare to, e.g., Byers and Hall, 1955?

P. 23, data availability: do the datasets have dois? They should.

The writing overall is fine, but there are small awkward uses of the English language sprinkled throughout that reflect English as a second language. If it is possible to find a native English speaker to read it that would polish the manuscript. In particular the abstract and its first sentence needs a revisit (you could consider just removing the first sentence).

Other comments on the abstract: mention the frequencies you use. You don't mention the linear regression approach, is that intentional? Mention clear-sky frequency and LWP statistics, as opposed to focusing on IWV - the title only mentions LWP after all. Overall the abstract seems to have been written in a hurry.

References:

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Byers, H. R., and R. K. Hall, 1955: A census of cumulus- cloud height versus precipitation in the vicinity of Puerto Rico during the winter and spring of 1953–1954. *J. Meteor.*, 12, 176–178.

Rauber et al., 2007: Rain in Shallow Cumulus over the Ocean, *Bull. Am. Meteor. Soc.*, 88, pp. 1912-1928. doi:10.1175/BAMS-88-12-1912

Zuidema, P., E. Westwater, C. Fairall and D. Hazen, 2005: Ship-based Liquid Water Path Estimates in Marine Stratocumulus. *J. Geophys. Res.*, 110, D20206, doi:10.1029/2005JD005833

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Interactive comment on *Atmos. Meas. Tech. Discuss.*, doi:10.5194/amt-2019-18, 2019.

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