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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aircraft millimeter-wave retrievals of cloud liquid water path during VOCALS-REx" *by* P. Zuidema et al.

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Response to reviewers:

All three reviewers note what is essentially a confusion of two goals for the current manuscript: 1) describe the retrieval as well as its errors, and 2) show some basic statistics that could also be useful as a first-order cloud processes assessment (ie above-cloud WVP determination, and cloud adiabaticity).

Both these objectives are important for this study, as the utility of the millimeter-wave measurements for cloud process studies is an essential motivator for pursuing this research - we recognize that measurements in the 170-194 GHz range alone are not ideal for this application to subtropical stratocumulus regime. Thus, we have held on to these two objectives rather than simplifying the paper into an either/or study. However, as correctly noted by the reviewers, the error analysis is currently scattered around the document, and it is unclear what sources of error are and are not being considered.

In this revision we focused on clarifying and centralizing the error analysis. We have also rewritten the introduction to focus more on the role of the GVR within the overall VOCALS-REx scientific sampling strategy.

Below we address the individual comments of each reviewer:

Reviewer 1, specific comments:

pg 19583, line 11-12: This sentence was removed when the introduction was rewritten. We note, for clarification, that the intent of the sentence was to mention that LWPs quantify cloud-aerosol interactions in the sense of constraining aerosol indirect effects, e.g., Feingold et al., GRL, 2003.

As part of VOCALS, GOES-10 satellite data were analyzed for multi-layers, using the technique of Pavolonis and Heidinger (2004), with the data/images available from http://www-angler.larc.nasa/gov/cgi-bin/site/showdoc?mnemonic=VOCALS. These reveal thin upper-level cirrus at times but do not indicate multi-layer liquid water clouds. Liquid water clouds above the boundary layer were also not evident visually during the individual research flights nor through any instrumentation indicators, including the above-cloud GVR measurements.

pg 19590, ln 20-21: done

pg 19591, ln 9-17: For optically thin clouds, the recorded infrared cloud base temperature will indeed be too cold, but will remain constrained by that calculated from a dry adiabatic lapse rate. Over, for example, a 1 km distance between the aircraft and cloud base (large but conceivable), an emissivity of, for example, 0.6, would generate an underestimate in the cloud base temperature by a factor of 0.88 (0.6ˆ(0.25)). This considerable underestimate of the cloud base temperature would then be discarded

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in favor of that calculated using the dry adiabatic lapse rate and decoupling would not be considered - this is estimated to generate an error in the cloud base of $\sim$ 1 K or 100 m, and some underestimate in the actual boundary-layer WVP. If we assume a moist boundary-layer WVP of 20 mm over a depth of 1.5 km, this would correspond to an uncertainty of 0.133 mm - less than what we ascribe to the free-tropospheric WVP error.

pg 19591, ln 14-15: the cloud radar-lidar specifications are now mentioned in the introduction. We note that an error in the removed liquid water of 50 g/mˆ2 corresponds to an underestimate in the boundary layer water vapor path of a mere 0.05 mm.

pg 19591, ln 15-17: We used composite temperature soundings to estimate the cloud temperature at different offshore distances, not the moisture soundings - thanks for noticing this. Given that the cloud top height could occasionally vary significantly from the composite values, we also constrained the cloud temperature to occur below the inversion. The temperature was used to calculate the microwave emission, which was not hugely sensitive to differences of just a couple of degrees.

pg 19591, ln 18-end of paragraph: liquid clouds above the boundary-layer inversion were not observed during VOCALS-REx either visually or with the 94 GHz cloud radar, nor with the GVR. Had they existed, the GVR brightness temperatures would have indicated the presence of the liquid water.

pg 19592, ln 5-6: done ("well-mixed" = cloud base-LCL < 125 m, following Jones et al 2011). The word 'thick' was removed.

pg 19592, ln 7: done

pg 19592, ln 23-24: information on lidar vertical resolution was on pg 19593 ln5, I moved it up a page.

pg 19594, ln 16-18: As written these sentences weren't very clear. Only samples with radar-detected cloud tops contributed here, and mixing at cloud top, when it occurs,

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appears more likely to resemble inhomogeneous mixing, with the cloud droplet sizes little affected (e.g., Painemal and Zuidema, in press, JGR).

We have rewritten this as: "Clouds with retrieved LWPs between 100 and 400 g m$ˆ{-2}$ matched adiabatically-calculated LWP values exceedingly well. This may be a particular feature of the southeast Pacific stratocumulus, as it is consistent with the previous results from a smaller sample size (Zuidema et al. 2005), but not with previous North Atlantic observations (e.g., Pawlowska et al., 2000).

pg 19595, sect. 5.2: done

pg 19582, ln 23-24: done

fig. 8: 'a)' and 'b) labels (need to) be added to the panels.

Reviewer 2, general comments:

We changed the title from "Aircraft millimeter-wave retrievals of cloud liquid water path during VOCALS-REx" to "Aircraft millimeter-wave passive remote sensing of cloud liquid water and water vapor during VOCALS-REx". This acknowledges the additional work done on free-tropospheric WVPs.

It is certainly true that the bandpass response of the frequency channels, and the exact mixture of the sideband response will have an influence on the response to liquid water. Our calculations, monochromatic and assuming an even mixture of the two channels, share these assumptions with those of previous studies (e.g., Cadeddu et al., 2007; Pazmany 2007; Cimini et al., 2009). The assumption of equal weighting of the two sidebands finds support in the actual frequency response of the 8 channels are shown in Fig. 2 of Pazmany (2007). This figure shows that the double sidebands appear to approximately evenly weighted. We have expanded our discussion of the instrument to clarify and justify these assumptions clear.

We only used the 14 GHz channel because its response to LWP is more linear and more sensitive for different WVP values than the combination of the 7 and 14 GHz

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channels would be. The decision to only use the 14 GHz channel was made relatively early and thereby dictated the later calculations (which were time-consuming). That said, a choice to use a combination of the 7 and 14 GHz channels would have been equally valid for the reasons the reviewer points out (particularly decreased sensitivity to uncertainties in the boundary layer water vapor path specification). For further work it would be useful to compare results from a combined 7 and 14 GHz channel calculation to those shown here. We have included mention of this point in the paper.

We respect the reviewer's comments that the retrieval approach and error discussion be presented more coherently and have rewritten the paper with that intent. For example, we now include a table summarizing the different sources of error that we considered.

Reviewer 2, specific comments: these use the original page and line numbers, to ease the correspondence to the review.

We have normalized our presentation of the units of WVP to always be in mm.

page 19583, lines 17-18: done

page 10584, line 14: 1370 m was the average height of the above-cloud aircraft legs

page 19585, end of section 2: we have rewritten and rearranged the section describing the retrieval

page 19585, line 27: A typical standard deviation in the brightness temperature of the 4 channels was on the order of a few degrees, with the statistics from a representative flight now included in the text.

page 19586, line 7: I originally showed the one sonde because it made for the clearest plot. I have modified this to be a 3-panel figure showing all 3 comparison cases.

page 19586, ln 13: We have clarified the paragraph on the instrument calibration. While this is discussed in detail elsewhere, it seemed valuable to include its salient points to

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support the conclusion we draw that the T_b uncertainty be considered about 2K from inaccuracies in the internal reported temperatures.

page 19586, line 14: thanks, done. nice paper.

page 19590, lines 25-28: This sentence wasn't clear, I meant it to say that using all four channels doesn't make sense for the southeast pacific, since two of the channels will be saturated and have no information content.

page 19591, lines 4-7: we have rewritten and expanded this discussion to better address these questions.

page 19591, lines 24-26: The overall variability of the boundary layer WVP can be inferred from Fig. 2 - i.e. a mean value of about 12 mm and a standard deviation of about 3 mm.

section 5 (assessment): okay (we split into 3 subsections, one for each approach).

page 19598, lns 7-10: This sentence never really belonged here, I think I cut and pasted here through an oversight. apologies.

Fig. 6: I was initially just curious to see what the radiosondes would show for the stratosphere - note they do dry out further offshore. I have redone this figure to only extend to 15 km.

I think our assumption of the climatological sounding is as good as the reviewer's suggestion to assume a fixed concentration of 4 ppm. While the weighting functions of the center lines can be sensitive to stratospheric water vapor, most pronounced for WVPs $<$ 1 mm (Racette et al., 2005}, Cadeddu et al. (2007) only found a difference of $\sim$1K in for the center line T_b that could be attributed to uncertainty in the water vapor specification at altitudes above 10 km (for WVP of $\sim$1.6mm), a result supported further in Payne et al. (2008).

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Reviewer 3 raises many of the same concerns as Reviewers 1 and 2; our responses to the previous reviewers will also apply here.

A separate section early on in the manuscript on the airborne instrumentation is now included.

Fig. 7 caption has been fixed, thanks for bringing it to our attention.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 19581, 2011.

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