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Interactive Comment

Interactive comment on "Evaluating MODIS cloud retrievals with in situ observations from VOCALS-REx" *by* N. J. King et al.

N. J. King et al.

nicholas.king@postgrad.manchester.ac.uk

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The authors would like to thank the reviewers for their thoughtful and helpful comments. Below is set out responses to each of the reviewer's comments where R denotes the reviewer's comment and A denotes the author's response.

Reviewer #1

R: "I enjoyed reading this manuscript. It is well organized and well written. The discussion is thorough and illuminating . . . My overall recommendation is accept with minor revision. "

A: Thank you

R: “Detailed comments: 1. How are in situ cloud optical thickness (Tau) and LWP measurement made? In section 2.1, the in situ measurements of Re and LWC are explained. However, I didn’t find any discussion on Tau and LWP measurements, even though they are compared to MODIS retrieval later in Figure 3 and 4. An important point to clarify is that the Tau and LWP are column integrated variables, whereas in situ measurements are made along aircraft track. This may not be a big issue if Tau and LWP vary slowly in horizontal. However, several papers indicate that Tau and LWP can vary rapidly within small scales (10-100m). Therefore, it is important to clarify how Tau and LWP are derived from in situ measurements in Section 2.1 2”

A: The derivation of vertically integrated LWP and optical depth values is discussed in the last paragraph of the comparison methodology section. The fact that the optical depth and LWP are vertically integrated quantities is discussed throughout the paper. The horizontal variation of LWP and optical depth is explored by studying the horizontal variation of LWC, effective radius and number concentration during straight and level runs by the aircraft as shown in figure 1 (now figure 2). The potential role of horizontal variability of LWC and by extension LWP and optical depth on the paper’s results is discussed throughout the manuscript. Further clarifications of these points have been added to the paper.

R: “Why is comparison made at 5x5km? I understand that the horizontal extent of aircraft profiling is from 2-7km. But why not just choose the MODIS pixels that encompass the aircraft track? ”

A: A 5x5km comparison domain is chosen to provide a consistent methodology between cases and with other studies. As mentioned in the paper matching an individual aircraft profile with specific pixels is difficult given the differences in spatial and temporal sampling from the different platforms. A 5x5km comparison domain is also chosen in order to minimise the influence of any pixel level uncertainties in the cloud retrieval products for example as a result of brightening and shadowing effects. This point is made in the paper.

R: “Why CDP, instead of 2DS, is used to derive the width of droplet size distribution? When discussing the assumption of the width of droplet size distribution as a potential error source in Section 4, the CDP measurement is used. But isn't 2DS able to cover wider droplet size spectral? Why not use 2DS measurements to derive standard deviation of the droplet size distribution? ”

A: The CDP measurements are thought to represent measurements of the cloud droplet size distribution whereas the 2DS measurements are thought to represent measurements of drizzle and precipitation sized droplets. The width of the cloud droplet size distribution and the presence of a drizzle mode in the distribution are treated as separate effects in the analysis. No cloud retrieval scheme includes a drizzle mode in the size distribution used for radiative transfer calculations since it is unknown from remote sensing measurements alone whether a drizzle mode is present. The CDP is therefore used to test the width of the cloud droplet size distribution since this is the parameter that retrieval algorithms assume a priori, not the presence or magnitude of a drizzle mode. This clarification has been added to the paper.

R: “Sub-pixel variability test: It is nice to see that the authors attempt to address the effect of cloud horizontal heterogeneity on MODIS retrieval and in situ sampling. But with the continuous in situ measurements the authors should be able to go further than a simple heterogeneity index test. In fact, the authors can make a high resolution radiative transfer simulation of cloud reflectance and synthetic retrievals based on in situ measurement. Then high-resolution cloud reflectance can be aggregate to MODIS resolution to obtain MODIS-like retrievals. By comparing the statistics of the high-resolution retrievals and the MODIS like retrievals, the effect of cloud horizontal heterogeneity might be better addressed. ”

A: Unfortunately the in situ measurements do not provide a high-resolution three dimensional picture of the microphysical properties of the cloud. During vertical profiles of the cloud the horizontal variability is unknown and during straight and level runs the vertical variability is unknown. In order to build a high-resolution three-dimensional

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picture of the cloud from either straight and level or vertical profile flight sections, the variability in the unknown dimension would have to be assumed and would likely introduce large artefacts into any such analysis. Simulation of high-resolution radiative transfer and aggregation onto a MODIS resolution is a subject on which entire papers have been dedicated (see e.g. Zinner et al 2010) and is therefore clearly beyond the scope of this study.

Reviewer #2

R: “This is an insightful evaluation of the MODIS retrievals of cloud microphysical properties for the southeast Pacific stratocumulus region.”

A: Thank you.

R: “The abstract should mention the platform (BAe-146) somewhere. The abstract also currently fails to include any mention of the CDP probe intercomparison, and of its ramifications for the MODIS retrieval evaluation - a major oversight. ”

A: These points have been added to the abstract.

R: “The discussion on the MODIS cloud top determination on p. 23700-23701 didn't make sense to me. The MODIS operational cloud top location algorithm I am aware of uses the CO_2 splitting technique with wavelengths in the 13-14 micron range, producing a cloud top pressure estimate that is only then converted to a temperature or altitude. See e.g. Holz et al. 2008 JGR, Menzel et al. 2008 JAMC for more background. This is the cloud top that is referred to in the Garay et al and Harshvardhan papers. The 3.7 micron cloud top temperature alluded to by the authors needs a reference - I am assuming it is a MODIS variable as otherwise the authors would not be using it. Is it producing a cloud top temperature, from which a cloud top pressure is inferred? Or are these separate retrievals? Fig. 9 shows discrepancies of 250-300 hPa in insitu and modis CTP, which would be consistent with the CO_2 technique, but is inconsistent with the 1-2K insitu-modis cloud top temperature differences shown. What products exactly

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did the authors use? Coincidentally, the MODIS cloud top temperature underestimate is consistent with a comparison to radiosondes done in Zuidema et al., 2009, J Clim, using an 11 micron derived cloud top temperature. ”

A: The CO2 splitting technique is only used when there is enough signal from a cloud in the CO2 bands. This is almost never the case for stratus clouds such as those present during VOCALS (private communication Richard Frey) and instead an infrared cloud top retrieval is made using band 31 (11.1 micron) combined with GDAS data. This is the retrieval referred to in the paper. The paper does not refer to a 3.7 micron cloud top temperature. Instead it is mentioned that the cloud top temperature (derived from band 31) is used to correct the 3.7 micron band for thermal emission. Clarifications have been added to the amended manuscript to make these points clearer.

R: “p. 23701: The authors mention the MODIS algorithm uses a standard deviation of 0.35 for its lognormal size distribution. This is equivalent to an effective variance of 0.13. MODIS has been using an effective variance value of 0.10 since its Collection 4 (Steve Platnick, personal communication). The change isn’t reported in any of the MODIS papers thus difficult for outside researchers to know. It is equivalent to a standard deviation of âĹij0.32. The authors are advised to check with Steve Platnick to confirm. ”

A: Thank you for pointing this out. The authors have checked with Steven Platnick as to the exact value used and the manuscript has been amended accordingly.

R: “The authors conclude MODIS also overestimates cloud optical depth, in contrast to PZ11. It may be worth looking at a couple of those scenes more carefully, in particular those indicated with red points in Fig. 3, to see if an explanation can be found. ”

A: The sources of error in comparing an in situ measured optical depth with a remote sensing retrieval are discussed throughout the paper and therefore no strong conclusions are drawn on under or over estimation of optical depth. It is concluded that there is no strong evidence of systematic overestimation of optical depth. It is mentioned in

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the paper that this is in agreement with PZ11. The authors have investigated all individual cases carefully and have presented all relevant details in the paper. No immediate explanation for the large overestimation of optical depth for the red points is found and this is presented in the paper.

R: “Figures: A figure showing the locations of the BAe profiles used (near-shore? far-off?) would be useful - Table 1 is helpful but a figure is easier to take in. Similarly, another column in Table 1 indicating the Gregorian calendar date of the flights would be useful. ”

A: A figure showing the locations of the profiles has been added to the paper.

R: “Several of the figures are quite difficult to read: Fig 2, 5, 6, 7,8,9,10,11, 12 – basically most of them. Please increase the font size. ”

A: The figures have been made larger

Reviewer #3

R: “I, therefore, recommend publishing the paper after a minor revision. My minor comments follow. ”

A: Thank you

R: “A larger optical depth and larger particle size derived from MODIS compared to in situ observations do not theoretically make sense. If the optical depth derived from a non-absorbing wavelength is too large, a smaller particle size is needed to give the required absorption in near-IR. I mean by required here is that the same amount of absorption computed with particle size derived from in the situ observations. If a plane parallel cloud assumption holds, a thicker optical thickness and a larger particle size give too low near IR reflectance compared with near IR radiance observed by MODIS. Therefore, this paper’s result indicates that MODIS radiances at visible wavelength, and other three near IR wavelengths cannot be simulated (i.e. modeled radiances for all four wavelengths do not agree with MODIS observation) using cloud propertied de-

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rived from the in situ measurements. A similar argument has been used to understand large MODIS derived cloud particles in earlier studies. If a retrieved optical thickness is too small, cloud particle size must be larger to obtain the same amount of absorption in near IR (i.e. to match MODIS near IR radiances), which is pointed out by Coakley et al. (2005, J. Atmos. Oceanic Technol. 22 3-17) and Kato et al (2006, J. Geophys. Res. 111, doi:10.1029/2005JD006668). But this result shows that both optical depth and particle size are larger, which is not explained by 1D radiative transfer theory. The authors mentioned that 3D effects do not explain the observed difference either. If in situ derived cloud properties were used in radiance computations, modeled radiances are not likely to match radiances at 4 MODIS wavelengths. I cannot tell what causes this apparently a fundamental mismatch or whether this affects the analysis shown, for example, Figure 8 from the information given in the paper. The authors need to mention this inconsistency before discussing that no vertical particle size profile information can be derived from 3 near IR MODIS channels. They also need to explain that the discrepancy is not caused by, for example, computation of cloud optical depth or effective radius from in situ conservations."

A: The reviewer is correct in that if the optical depth is incorrectly retrieved then this may (at some optical depths and droplet sizes where the retrieval is not orthogonal) impact on the retrieval of droplet size. The paper has presented as in depth an analysis as possible with the available data to get to the bottom of the problem. It is found (and mentioned throughout the paper) that the comparison of in situ optical depth with MODIS optical depth is troublesome and therefore no strong conclusions are drawn about any systematic over or under-estimation of optical depth by MODIS. Any uncertainties associated with the in situ measurement of optical depth however do not impact on in situ measurements of droplet size. In situ measurements show an increase in droplet size from cloud base to cloud top whereas the signal from MODIS retrievals at different wavelengths often suggests the reverse.

The points that the reviewer appears to be making are already discussed in the paper.

Similarly to previous studies the authors cannot definitively explain (and do not attempt to do so) the discrepancies between in situ and MODIS data. The purpose of the paper is to present the available data and explore all possible avenues of analysis from the data. The reviewer does not suggest any further analysis which is not presented in the paper and it is difficult to tell how the reviewer is proposing that the paper could be improved.

R: “The authors do not describe cloud fields and environmental conditions that these 13 flights were taken place. Are clouds horizontally uniform, overcast and single layer (no cirrus above)? What were solar zenith angles and MODIS viewing angles? Including MODIS viewing geometry provides information that gives a clue to the mystery. Adding above information makes the paper even longer from the current length, which might already be too long. But the authors can shorten current version significantly. Foe example, the sentence appears on page 23698 line 16 is repeated at lest three times in the paper. The current version needs significant editing to avoid going similar discussion over and over. In addition, there are several incomplete sentences and sentences that are too long. ”

A: An in depth analysis of the homogeneity of the individual scenes and the clouds observed throughout the campaign are already presented in the paper. None of the MODIS scenes used were flagged by the MODIS algorithm as being multi-layered and no cases of cirrus above were flagged by MODIS. This information has been added to the paper. The solar zenith angle and MODIS viewing angle for each case have been added to the summary table. No correlations were found between any of the variables studied and either solar or viewing geometry. This information has been added to the summary of the paper.

Important points in the paper have been discussed in more than one section as and when they are relevant to each section. The reviewer has not indicated the locations of any of the incomplete or overly long sentences which the reviewer mentions. The paper was carefully prepared and edited by the authors. This is reflected in the fact

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that other reviewers have praised the composition of the paper.

R: “Page 23692 line 17 Rt is defined as the particle size at 1 km below the cloud top. Please explain why r3.7 is substituted for rt. ”

A: r3.7 is substituted for rt due to the fact that r3.7 should theoretically give the best indication of the droplet size close to the cloud top. This has been explained in numerous places in the paper but clarification has been added to the relevant section.

R: “Page 23702 line 25 Optical absorption. Is what the authors meant simply “absorption”? ”

A: The absorption referred to is in fact optical absorption, however the word optical has been deleted in order to maintain consistency with the language used in the rest of the paper.

R: “Figure 1 The x label needs to be normalized standard deviation. ”

A: The label has been changed.

R: “Figures 2, 6, 10, and 12 are too small and hard to see. ”

A: The size s of the figures have been increased.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23679, 2012.