

Interactive comment on “Validation of MODIS cloud microphysical properties with in situ measurements over the Southeast Pacific” by Q. Min et al.

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We thank the reviewer for very thorough and constructive comments. These comments and suggestions have helped to greatly improve the quality of the paper. Below are our responses to the comments. The response (in blue) follows each comment.

Review: Validation of MODIS cloud microphysical properties with in situ measurements over the Southeast Pacific. Q. Min et al. ACPD

Min et al. compare MODIS cloud microphysics against aircraft observations sampled during VOCALS-REx. MODIS variables are relatively well correlated with in situ observation, although the cloud effective radius (r_e) is overestimated for almost 1.8 microns.

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The authors argue that the adiabaticity and the algorithm assumptions about the cloud vertical structure are main aspects to be taken into account when searching for plausible explanations for the MODIS bias.

While validation analyses like Min et al. are valuable, especially considering the scarcity of in-situ observations in marine stratocumulus, I have major concerns about this manuscript. A recent paper by Painemal and Zuidema (2011, JGR) carried out a similar analysis using basically the same C130 measurements as in Min et al., but including a more comprehensive description of the cloud vertical structure and a better analysis and discussion of the possible errors sources that might explain the MODIS biases including: water vapor path effect, breadth of the droplet size distribution, thermal emissions, and 3D radiative effects. Painemal and Zuidema (PZ11) also investigated MODIS r_e based on 3.7 μm , and 1.6 μm radiances. In addition, they also assessed the significance of their results considering errors in the in-situ probes. I believe that the current manuscript does not show meaningful results that justify publication.

Min et al.: Most error sources of satellite cloud retrievals are known and have been extensively discussed in the literature [Nakajima and King (1990); Li et al. (1994); Platnick and Valero (1995); King et al. 1997; Platnick, 2000, and many others]. Scientists try to figure out specific error sources under certain circumstances from various validation and evaluation studies. Painemal and Zuidema (2011, PZ11) did an excellent job to investigate a set of possible errors sources, including water vapor path effect, breadth of the droplet size distribution, thermal emissions, 3D radiative effects, and retrievals from three different wavelength channels. As they concluded in their paper “Although we cannot highlight any one particular cause for the positive MODIS–in situ re bias, the combined effect of the cloud droplet size distribution width, the precipitation mode for heavily drizzling cases (only), the incorrect specification of the water vapor, and the sensor viewing geometry all conspire to produce effective radii larger than in situ values, most noticeable for the larger effective radii.”

Our study (M12) differs in important ways from Painemal and Zuidema (2011, PZ11).

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In terms of in situ observation it not only includes measurements from the C-130 (85 profiles), but also analyzes G-1 in-situ dataset (26 profiles) – which was actually the original focus of the study. Most importantly, M12 compared the satellite retrievals of cloud microphysical and macrophysical properties with the in-situ measurements, within in the context of aerosol-cloud interactions. M12 surveyed the in-situ measurements of cloud geometric thickness, cloud microphysical properties, and cloud adiabaticity over SEP and analyzed interrelationships between them. M12 particularly paid attention to a critical issue of spatial-temporal variability/mismatch from multiple instruments, not considered in PZ11). M12 limited the temporal difference within one hour, and used both projected and un-projected positions to collocate samples and two averaging domains (5 and 25 km) for our comparison study. M12 pointed out a most likely error source for the observed differences between the MODIS and in-situ measurements, i.e., the cloud vertical assumption associated with cloud thickness, cloud adiabaticity, and cloud drop number concentration. The error source of cloud vertical assumption, like many other error sources, is not a new one. However, M12 used observation data and simulations to illustrate its possibility which is unique.

Major comments: 1. The authors partially repeat the analysis performed by PZ11. I suspect that most of the analyzed samples come from the C-130 aircraft. Hence, it is not surprising that Min et al. find similar results as PZ11. If the authors want to further confirm the findings in PZ11, then they should use an independent dataset, excluding C-130 observations and including additional VOCALS-REx aircraft observations.

Min et al.: As discussed above, our study (M12) differs from Painemal and Zuidema (2011, PZ11) in important ways. In particular, more in situ data is applied (G1 and C130), the focus and the analysis differs, and a different possible source of error is proposed. .

2. Min et al. ignore the fact that the vertical penetration of photons depends on the wavelengths used to retrieve r_e (Platnick, 2000). Platnick (2000, JGR) explicitly shows that the statement in p1425, line 19-20 is misleading. Hence, the three different MODIS

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r_e (2.1, 1.6, and 3.7 μm) have to be studied if one wants to determine the consistency among photon penetration, vertical structure, and r_e . It is also mentioned in p1425 that the mean r_e is 5/6 of the cloud top adiabatic r_e . This statement is incorrect, and it is obtained from the following assumption: The liquid water path (LWP) for a vertically homogeneous cloud is: $LWPH = 2/3 \rho_w r_{eh} \tau$ (1) and for an adiabatically stratified cloud (or r_e and water content linearly increasing with height) as: with w the water density, τ the cloud optical thickness, r_{eH} the homogeneous r_e , and r_{es} is at the cloud top. If LWP's and τ 's are equal with (1)=(2), then: $LWPs = 2/3 \rho_w r_{es} \tau$ (2) The question is why the stratified and homogeneous LWP should be equal? The “unphysical” adjustment would come from r_e then. Therefore, I think the factor 5/6 is incorrect from a microphysical and remote sensing point of view. This factor is constantly used to represent some sort of equivalent r_e but, again, I believe this is wrong; in fact it makes much more difficult interpreting the figures.

Min et al: Photon path length distribution is controlled by spatial distributions of scattering and absorption in a multiple scattering media. The vertical penetration length of photons depends on the cloud water absorption partition (single scattering albedo, which is wavelength dependent) and cloud optical property (LWC and microphysical property) profiles. Different clouds (adiabatic or sub-adiabatic) have different vertical distribution of microphysical/optical properties, thus possibly different vertical penetration lengths even for a given wavelength. For a given vertical distribution of cloud optical property, photons with different wavelengths have different penetration length. However, PZ11 found that $Re(2.1\mu\text{m}) > Re(3.7\mu\text{m})$ or $Re(1.6\mu\text{m})$, which differs from expectation: $Re(3.7\mu\text{m}) > Re(2.1\mu\text{m}) > Re(1.6\mu\text{m})$. We added some of above discussion into the revision.

Yes, the effective radius in a vertically uniform cloud would be 5/6 of the effective radius at the top of an adiabatically stratified cloud, for the same optical depth and LWP. For cloud radiation transfer, the cloud optical depth is the most important factor, and Re plays a secondary role. For models, LWP is directly linked to cloud dynami-

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cal/thermodynamical processes. The cloud optical depth and LWP are two key parameters for cloud-radiation interaction. For the same cloud, both of them should be the same for cloud-radiation interaction from either remote sensing or modeling point view.

We used the mean values of the upper 30% of clouds for all comparison in the paper. We clarified this in the current manuscript, and removed the 6/5 lines in Figure 5.

3. The sub-adiabaticity analysis is not statistically significant. A close look at the figures does not reveal any significant change between adiabatic and subadiabatic samples. Why the sub-adiabaticity should affect the retrievals? In terms of the photons vertical penetration, how changes in the vertical structure (induced by cloud top mixing) can affect the retrievals? A more careful investigation requires analyzing the cloud vertical profiles in terms of sub-adiabaticity. This should be easy to do since the number of profiles used is only fifteen.

Min et al: Radiation transfer calculation in a multiple scattering media depends only the distribution of optical properties. Cloud adiabaticity, in a sense, determines the vertical distribution of LWP, and consequently the vertical distribution of optical properties. As shown in Figure 11b, there are some differences in R_e , albeit small, for different adiabaticity, as the reviewer pointed out. However, the cloud adiabaticity has big effects on CDNC and cloud geometric thickness retrievals. Yes, we did visually inspect all profiles of LWC, CDNC, and the ratio of LWC/LWC_{Adia}.

4. Section 4 is not novel. The authors carried out RT simulations for vertically stratified clouds (either adiabatic or sub-adiabatic) and then retrieved the cloud properties using a vertically homogeneous model. Nakajima and King (1990, J. Atmos. Sc.) already documented in detail the main findings in Section 4. Nakajima and King. also retrieved r_e for adiabatic clouds, using a vertically homogeneous model (constant r_e), finding that the retrieved r_e was “smaller” than the cloud top r_e . I believe this is exactly what Min et al. found in their analysis (if one gets rid of the 5/6 factor). In other words, the MODIS positive bias “cannot” be accounted for the use of a vertically uniform model.

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The reason why LWP VUPPM overestimates LWP ASPPM in Fig. 10c is because Min et al. used equation 1 (above) to calculate it. If they use equation (2) (above), then they would find that $LWP_{ASPPM} > LWP_{VUPPM}$ in Figure 10c. (Note that the CDNC formula is based on LWPS)

Min et al: Yes, as discussed above, many studies discussed this well-known issue. The section 4 shows the simulations with the specific setting of VOCALS observation to further illustrate the relationships between the two assumptions. More importantly, the variation of the observed R_e difference with CDNC and cloud geometric thickness are consistent with that in the VUPPM/ASPPM simulation.

Reviewer’s suggestion, using Equation (2) instead of Equation (1), means that we should treat the retrieved R_e in VUPPM as the R_e at the top of ASPPM. Yes, it will reduce the LWP by a factor of 5/6. We are confused by the assumption consistency.

5. The authors seem to use all the available MODIS retrievals. I suspect that a rigorous screening based on cloud fraction would yield a better agreement. I would imagine that if the analysis includes all the samples, some MODIS scenes would be severely affected by clear sky contamination.

Min et al: Yes, the clear-sky contamination is a big issue, due to the 3D effects. As a result only cases with cloud cover over 0.95 are more used in the 5km domain. The clear-sky contamination is minimized with the application of this criteria.

6. P1432 lines 1-3. I disagree. The figure does not show any dependence on adiabaticity A . The cloud geometrical thickness dependence is actually dependence on the cloud optical thickness in the look-up table. The vertical profile of effective radius and the cloud optical depth within the cloud are the variables that affect the retrievals, rather than the geometrical thickness.

Min et al: As shown in Figure 11b, there are some differences, albeit small, for different adiabaticities. We changed statement in the revision. Radiation transfer calculation in a

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multiple scattering media depends only optical properties, as the reviewer pointed out. However, when converting the cloud structure and microphysical properties into the cloud optical property profile for the RT calculation, particularly for the stratified adiabatic cloud structure, the cloud geometrical thickness matters [Li et al. 1994; Brenguier et al; 2000]

7. Discussions in Platnick and Valero (1995, J. Atmos. Sc.) and PZ11 show that there are many potential sources of uncertainties. Most of them are ignored by Min et al

Min et al: Most error sources of satellite cloud retrievals are known and have been extensively discussed in the literatures [Nakajima and King (1990); Li et al. (1994); Platnick and Valero (1995); King et al. 1997; Platnick, 2000, PZ11; and many others]. Our intention is, as indicated in the general reply, to show the possible error sources over SEP during VOCALS. We did not ignore anything, but we did not want to repeat what have been done, such as in PZ11.

Other Comments: - Cloud top temperature comparison: Zuidema et al. (2009, J. Clim.) reported similar results using MODIS and radiosonde observations with a more comprehensive dataset.

Min et al: Thanks, we cited Zuidema et al. (2009, J. Clim.) in the revision.

- Validation analysis during VOCALS-REx by Zheng et al. (2011, ACP) should also be considered by Min et al. Zheng et al., using Twin Otter aircraft observations, showed that MODIS r_e overestimates the in-situ r_e .

Min et al: Thanks, we cited this in the revision.

- P1423, I am not sure that the copper smelters are the main sources of aerosols (VOCALS-REx ACP publications should help elucidate this point).

Min et al: This topic has been discussed in the VOCALS-REx science plan and in some overview papers [Wood et al, 2010]. The reference in this paper is in accordance with those publications.

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- Why Aqua observations are not included?

Min et al: Most in-situ measurements were taken in the local morning, and Aqua overpasses were in the afternoon. Only a few cases satisfied our matching and collocation criteria between MODIS and in-situ measurements. Since the solar angle and the MODIS instrument on Terra and Aqua are not the same, including Aqua MODIS into our analysis may be problematic.

- P1430, why a 1.65 degrees bias in cloud top temp. is equivalent to 200 m?

Min et al: We used the lapse rate from re-analysis to convert the cloud top temperature bias to the equivalent cloud top vertical height difference.

- King et al. (1997) is not in the references.

Min et al: Thanks, we added it.

- P1434 change "humility", and "metrological".

Min et al: Thanks, we changed them.

- PZ11 show that the bias increases with r_e . This is equivalent to a bias dependence on CDNC and cloud thickness found by Min et al.

Min et al: Re is related to CDNC and LWC.

- I do not understand why the authors exclude the precipitating cases from their analysis. Those cases should also provide interesting results, especially because precipitation is sometimes parameterized as a function of r_e (e.g. Wood et al., 2008, JGR).

Min et al: The precipitating cases are very interesting, as pointed out by the reviewer. However, there were only a few precipitating cases, satisfying our matching and collocation criteria between MODIS and in-situ measurements.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1419, 2012.

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