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Atmospheric Measurement Techniques Discussions

Interactive comment on "Quantifying and correcting the effect of vertical penetration assumptions on droplet concentration retrievals from passive satellite instruments" *by* Daniel P. Grosvenor et al.

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Grosvenor et al. (2018) AMTD, REVIEW:

The manuscript describes a method that, in principle, corrects errors in adiabatic satellite cloud droplet number concentration (Nd) due to the inconsistency of utilizing satellite cloud effective radius (r_e) that represents values slightly below the cloud top, whereas satellite cloud optical depth (tau) fully captures the optical thickness of the clouds. To achieve this goal, the authors simulate a number of idealized cloud profiles with a 1D radiative transfer model, and then retrieve r_e and tau from the synthetic reflectances. Next, the authors derive an "effective" tau that corresponds to the optical

thickness where the retrieved r_e and the synthetic r_e match each other (the vertical penetration effect). They use the difference between the retrieved and the effective tau (applying a fit to their theoretical calculations) to quantify the error in MODIS-based Nd that does not account for the fact that the satellite r_e is not exactly that at the cloud top due to the vertical photon penetration, which is in turn dependent on the sensor wavelength and the specific thickness of the cloud (and probably solar zenith angle and viewing geometry).

The manuscript makes an interesting use of the results in Platnick (2000), which shows that the retrieved r_e should differ from the observed r_e by a few um (or less) due to the photon penetration. The manuscript is concise and well-written, however when I first browsed the paper, I got confused about whether the authors wanted to show a real satellite bias in tau (and Nd) or a methodological bias (I realized it was the latter). My fundamental criticism of Grosvenor et al. is that, from a remote sensing point of view, the problem is not that the satellite tau should be reduced because r_e is not at the cloud top. Instead, it is that r_e is smaller than the observed r_e at the top due to vertical stratification, and probably r_e should be somehow increased (i.e. r_e drives the uncertainty in Nd). This is the correct interpretation, as it is well known from the early work by Nakajima (King and co-authors) that satellite tau is almost insensitive to the cloud vertical structure, and only r_e can be greatly affected by the vertical stratification. So, the Nd bias should be expressed in terms of a delta r_e. Another inconsistency (related to my previous comment) is with the use of the (pseudo) adiabatic model, which if I interpret correctly, it implies that the liquid water path (LWP) is proportional to r_e*tau. So, any error calculation applied to Nd has to be also valid for LWP. However, if we apply equation (13) to LWP, i.e.:

LWP_uncorrected/LWP_corrected=(tau/(tau-dtau))

Using a dtau=4.5 for tau=10 (figure 1a), then LWP_uncorrected/LWP_corrected=10/6.5=1.54. A 54% overestimation in LWP is clearly a mathematical contradiction. On the other hand, if, for instance, we utilize the results in Platnick (2000) for a cloud top r_e =12 um, and a retrieved r_e= 10.7 um (2.1 um wavelength), we get:

LWP_uncorrected/LWP_corrected=r_uncorrected/r_corrected=10.7/12=0.89.

That is, the retrieved r_e yields an underestimation of LWP. Again, this result points to a main reasoning problem in the manuscript, which is, the error should not be expressed in terms of tau.

Lastly, the authors say that there are several other errors that can bias r_e and tau. This is a key statement, and a literature review will show that biases in r_e are not dominated by the cloud vertical stratification (I am not aware of any studies that actually show an adiabatic signature in the satellite r_e bias). For instance, If one calculates the difference between MODIS r_e at 2.1 um and 3.7 um, the difference is positive everywhere over the ocean (the difference can be larger than 5 um, see Fig. 10 in Zhang and Platnick, 2011). This result suggests that the error discussed in Grosvenor et al. is negligible. So, I find it surprising that the authors found errors up to 50 % in Nd (Figure 6), which is very large. Since their results are only valid in a plane parallel world (sub-pixel variability is not accounted for) and with the use of idealized profiles, the validity of the correction cannot be demonstrated. The authors do discuss some of these issues but, unfortunately, the main concern remains, that is, it is unclear that the correction will yield an improved estimate of cloud droplet number concentration.

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