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Reply on RC1

Vikas Nataraja et al.

Author comment on "Segmentation-Based Multi-Pixel Cloud Optical Thickness Retrieval Using a Convolutional Neural Network" by Vikas Nataraja et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-45-AC1, 2022

Dear Reviewer,

Thank you for your feedback on our manuscript, and we appreciate you taking the time to do so. Our responses to your comments are detailed below:

Line 17-18, a few references regarding the importance of COT might be helpful, with Zhao and Garrett (2015, doi: 10.1002/2014GL062015) suggested.

-> Thank you for pointing that out. We will follow this advice in the revised version. Instead of including the haze-specific paper the reviewer suggested, we chose the more general cloud climatology paper on ISCCP by Rossow and Schiffer (1991, doi: 10.1175/1520-0477(1991)072<0002:ICDP>2.0.CO;2) where COT features prominently in Figure 4. We will change the text as follows:

"*Cloud optical thickness (COT) is important for the shortwave CRE, and it is therefore a key parameter in cloud climatologies (e.g., Rossow and Schiffer, 1991, Figure 4). Deriving the COT accurately from satellite imagery will help to improve our understanding of the energy budget*."

Line 23, the inhomogeneity issue exists in both spatial and temporal.

-> We agree, but since we are proposing a spatial-based solution with the Convolutional Neural Network (CNN), we limited our discussions to spatial-related problems.

Line 46, IP bias is not defined yet in the main text, while defined in the abstract.

-> Thank you for pointing this out, we will make this change in time for the next stage of the review.

Line 51-53, I appreciate the information here. However, I wonder if the satellite spatial resolution is high enough to make us ensure that the optimum occurs at a scale of about 1 km.

> This is a good point. However, the optimum was determined with synthetic data (Figure 13 in Davis et al., 1997, https://doi.org/10.1175/1520-0469(1997)054<0241:TLSBIS>2.0.CO;2, cited in the paper). In this case, artificial clouds representing stratocumulus were generated with a fractal cloud algorithm and fed into 3D RT calculations to generate synthetic radiance data. From these, COT was derived, and the optimum resolution of an imager was determined. These (and related) findings ended up guiding the choice of the spatial resolution of EOS imagers (at least they were one factor).

Line 63, "distinguished"

-> We will address this typo in time for the next stage of the review.

Figure 2, It seems to me that the difference (IPA COT) has a very good linear relationship with true COT, making me think that the IPA COT could be highly improved by simply corrected with this linear relationship. If this is true, why do not we use this simple method?

-> This is a very valid and appropriate observation. In fact, the IPA dependence of (retrieved-true) COT on the COT is even more linear than for the CNN retrieval. We will add this observation to the revised text. It is indeed possible to parameterize this effect as a 3D correction, and this has been done in the past. We described these approaches in Section 1.2. See for example Equations (2), (3), (4), and the related literature citations. Iwabuchi and Hayasaka (2002) introduced a more complex statistical parameterization. The problem is that the parameters in all of these are fixed, and derived for very specific cloud fields in a multivariate fitting manner. What we do instead is use the spatial context to drive the 3D correction in a flexible and generalizable manner. To be more specific, for the simplest parameterization (fitting a linear regression line to the IPA results from Figure 2), the problem is that the slope varies from scene to scene. You can see this in Figure 8, where the slope of the IPA retrieval is plotted as a function of cloud fraction and other scene parameters. In other words, a single slope parameter does not allow the correction of IPA retrievals. The CNN technique can be regarded as a more complex form of "fitting", where a parameterized correction of 3D effects in COT retrievals is done, in part, as a function of the structure of the cloud field. Still, we appreciate this comment as it made us think about the distinction between simple linear parametrizations and more complex CNN approaches. Once we hear back from the complete review committee, we will add a statement to the revised manuscript. Here is a draft of what that might look like:

"It is worth noting that the IPA in Fig. 2 does appear to have a linear relationship with the (retrieved - true) COT which would imply that it is indeed possible to parameterize this effect as a 3D correction. Furthermore, as we discuss in Sect. 1.2, there have been approaches that have attempted to do so, including Iwabuchi and Hayasaka (2002) who introduced a more complex statistical parameterization. However, the underlying problem with such a method is that the parameters are fixed, and derived for very specific cloud fields using multivariate fitting. By contrast, with our proposed CNN (and future iterations of it), the intention is to utilize the existing spatial context in cloud imagery to learn the

underlying features that can then be generalized and applied to correct 3D radiative/net horizontal photon transport effects."

Line 207, what are the three aerosol number concentrations?

-> Per the Yamaguchi et al. (2009) paper that conducted the study over the Sulu Sea (and cited in this paragraph), the three aerosol concentrations were 35, 150 and 230 mg^{-1}. We will add this to the revised manuscript.

Line 214-217, why do the authors only use two daytime periods?

-> The Lagrangian LES conducted by Kazil et al. (2021) focus on a cloud state transition (closed- to open-cell stratocumulus) from the first to the second daytime period. The simulations capture the cloud state transition in its entirety, which is sufficient for the work's objectives. The cloud deck dissipates shortly after the end of the simulations (as seen in satellite imagery) and longer simulations would not provide additional data. Finally, the LES in Kazil et al. (2021) use very large domains and sectional (bin) cloud microphysics, which makes them expensive.

Equation(5), In my understanding, this equation calculates the water vapor amount instead of liquid water content. Could the authors help explain?

-> Thank you very much for pointing out this error, we missed this. The equation is indeed supposed to use mr_{cloud} rather than mr_{water}. We will make this change in the revised manuscript.