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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deconvolution of Boundary Layer Depth and Aerosol Constraints on Cloud Water Path in Subtropical Stratocumuli" *by* Anna Possner et al.

Anonymous Referee #2

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Review of the manuscript numbered ACP-2019-833

Title: "Deconvolution of Boundary Layer Depth and Aerosol Constraints on Cloud Water Path in Subtropical Stratocumuli" written by Anna Possner et al. Manuscript number: "acp-2019-833". Decision: "Major revision"

In this study, the authors investigated the dependency of the Liquid Water Path (LWP) susceptibility of stratocumulus deck upon the boundary layer (BL) depth using 10 years (from 2007 to 2016) data. From their analyses, the authors elucidated that the susceptibility increases with deepening BL, and magnitude of susceptibility triples with deepening BL. LWP adjustment is one of the important topics in the climate science. And I

agree authors' suggestion that "the discussion based on the knowledge obtained from limited area of stratocumulus below shallow BL" can mislead the scientific community. So, I think this is an important study in the scientific community of the climate science. Most of the discussions in this manuscript is clear, and I agree most of the authors' suggestions. However, some discussions based on the previous process modeling study (slow- and fast- manifold mechanism) need to be modified. In addition, there are some technical problems. Based on the descriptions shown above, my decision is "not-so-major revision", and I encourage the authors to modify the manuscript. Detail comments are shown below.

General Comment:

1: The authors discuss the relationship between LWP-Hc and LWP-HBL in section 3, and try to interpret the difference between LWP-Hc and LWP-HBL relationship, and effects of aerosols on LWP-HBL relationship based on the slow and fast manifold mechanism. I agree that the discussions about slow and fast manifold mechanism are important for reducing the uncertainties of the cloud adjustment process. However, it is difficult for me to connect the results of this study to slow/fast manifold mechanism based on the analyses shown in the body of the manuscript. So, the discussion about the fast/slow manifold mechanism should be modified or removed from the manuscript.

2: The authors indicate the negative and positive LWP susceptibility in non-precipitating clouds and precipitating clouds, respectively (Table 1). This result supports the results of Gryspeerdt et al. (2019). In contrast, Chen et al. (2014), Michibata et al. (2016), Sato et al. (2018) indicated that the susceptibility is negative and positive or zero over precipitating and non-precipitating cloud areas. The authors should add some discussions about the reasons the inconsistency between the results of this study and the previous studies.

Specific Comment:

Title: This study targets on the stratocumulus "decks", and open cellar stratocumuli are excluded from the analyses (CF > 80 %). So, I think the title with the word "deck" is better. For example, "Deconvolution of Boundary Layer Depth and Aerosol Constraints on Cloud Water Path in Subtropical Stratocumulus decks". This is just an example.

Line 63- 64: "In Fig. 1, we show that ..." should be "Figure 1 shows that...". The Figure 1 is originated from Fig. 10 of Muhlbauer et al. (2014), not the authors' work.

Figure 1: LES intercomparison studies targeting on stratocumulus like Stevens et al. (2005); Ackerman et al. (2009), which are representative LES studies for DYCOMS and LES studies on VOLCALS case (Berner et al. 2013) should be added in the figures.

Line 68-69: "merely two campaigns and even fewer LES studies": Some concrete descriptions about the campaigns and LES studies targeting on the deep BL are helpful for readers to identify the previous studies targeting on deep BL.

Line 86: Fig. S2: The authors discuss Reff through the Fig. S2, but no data of Reff in Fig S2. Fig. S2 is same as Fig. S3, so, I think this is just a mistake. The authors should exchange the figure to correct one.

Figure 3: The data for non-precipitating case like Fig. S3 is useful for the reader, because the authors also discuss non-precipitating case in the body of the manuscript.

Line 140-157: As I mentioned in the general comment, it is difficult for me to connect the discussion of the LWP-Hc and LWP-HBL relationship to slow and fast manifold mechanism, through the results of this manuscript. I agree that the slow and fast manifold mechanism need to be considered when we discuss about the LWP adjustment. However, it is no evidence in this manuscript to justify that LWP-Hc and LWP-HBL relationship is regard as slow and fast manifold mechanism. The authors tried to justify through Hc-HBL relationship and Clausius Clapeyron, but I think these discussions could not convince readers that the LWP-Hc and LWP-HBL is regarded as the slow and fast manifold mechanism. In my understanding, the discussions about slow and fast manifold mechanism are not the main topic of the manuscript. So, the elimina-

tion of this part is one of the options. If the authors want to remain this part, I require the authors to add evidences to justify that LWP-Hc and LWP-HBL relationship can be regarded as slow and fast manifold mechanism.

Line 158-164: The authors discuss about the effect of the decoupling, but as the authors mentions in Line 162-163, no conclusion about the decoupling is obtained. So, I think this part is not necessary, and can be removed from the manuscript.

Table 1: Sample number for each column is helpful for readers. In addition, the regression statistics (e.g., error, residual, and so on) are helpful for readers. The information can be added as a supplemental material.

Line 174-177: In this part, the authors suggest that the anticorrelation between Nd and HBL is attributed to the climatological deepening of BL and increase of the distance to continental sources of anthropogenic pollution. However, there are no results to confirm these two suggestions. The trend of BL height and distance to continental sources are helpful for readers.

Line 178-180: The authors suggested that the anticorrelation between Nd and HBL vanishes in a deregionalised and deseasonalised version as shown in Fig. S3. However, weak anticorrelation, which is shown in black line of Fig. S3, is seen in climatological mean (red) and non-precipitating clouds (green) shown in Fig. S3. Is the word "anticorrelation" is same as "negative correlation"? If so, the authors should add some descriptions about the weak anticorrelation in climatological mean (red) and non-precipitating clouds (green) shown in Fig. S3. The value of slope for each case in Fig. S3 is helpful for readers. If not, please added the definition of anticorrelation more correctly.

Line 182-183: In this part, the authors suggest that the Nd and HBL climatology are not impacted by the precipitation, but the negative correlation in precipitating case is small but non-precipitating case is large. I think this means that the negative correlation is impacted.

Line 220-221: As I mentioned in the comment for Table 1, sample number for each column is helpful for readers.

Line 273-274: As I mentioned in general comment, it is difficult to regard LWP-Hc and LWP- HBL relationship as the fast and flow manifold mechanism from the results shown in the manuscript. Please do not misunderstanding, I agree the importance of slow and fast manifold mechanism.

Minor or technical Comment:

Figure 2: There are many contour lines around tropics, mid-latitude area, and ITCZ zone, and it is difficult to see the value over these areas. Of course, I understand that these areas are out of the scope of this study, but the figure need to be modified.

Line 147: “Hc” should be italic form and "c" should be subscript.

Figure 5: Unit of each variable in logarithmic is helpful for readers.

Line 153: Full spelling of SST (sea surface temperature) and FT (may by free troposphere) is helpful for readers.

Line 228: “(6b)” should be “(Fig. 6b)”.

Line 234: I think the word “LWP adjustment” is used as slwp. Is this right? If so, slwp is easy to be understood.

Figures S1, S2, and S3: The label of Figure 1, 2 and 3 shown in supplemental material should be Figure S1, S2, and S3.

Reference:

Ackerman, A. S., and Coauthors, 2009: Large-Eddy Simulations of a Drizzling, Stratocumulus-Topped Marine Boundary Layer. Mon. Weather Rev., 137, 1083–1110, https://doi.org/10.1175/2008MWR2582.1. Berner, A. H., C. S. Bretherton, R. Wood, and A. Muhlbauer, 2013: Marine boundary layer cloud regimes and POC for-

mation in a CRM coupled to a bulk aerosol scheme. Atmos. Chem. Phys., 13, 12549–12572, https://doi.org/10.5194/acp-13-12549-2013. Chen, Y.-C., M. W. Christensen, G. L. Stephens, and J. H. Seinfeld, 2014: Satellite-based estimate of global aerosol–cloud radiative forcing by marine warm clouds. Nat. Geosci., 7, 643–646, https://doi.org/10.1038/ngeo2214. Matsui, T., H. Masunaga, S. M. Kreidenweis, R. a. Pielke, W.-K. Tao, M. Chin, and Y. J. Kaufman, 2006: Satellite-based assessment of marine low cloud variability associated with aerosol, atmospheric stability, and the diurnal cycle. J. Geophys. Res., 111, D17204, https://doi.org/10.1029/2005JD006097. Michibata, T., K. Suzuki, Y. Sato, and T. Takemura, 2016: The source of discrepancies in aerosol–cloud–precipitation interactions between GCM and A-Train retrievals. Atmos. Chem. Phys., 16, 15413–15424, https://doi.org/10.5194/acp-16-15413-2016. Sato, Y., D. Goto, T. Michibata, K. Suzuki, T. Takemura, H. Tomita, and T. Nakajima, 2018: Aerosol effects on cloud water amounts were successfully simulated by a global cloud-system resolving model. Nat. Commun., 9, 985, https://doi.org/10.1038/s41467-018-03379-6. Stevens, B., and Coauthors, 2005: Evaluation of Large-Eddy Simulations via Observations of Nocturnal Marine Stratocumulus. Mon. Weather Rev., 133, 1443–1462, https://doi.org/10.1175/MWR2930.1.

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