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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Investigation of the adiabatic assumption for estimating cloud micro- and macrophysical properties from satellite and ground" *by* D. Merk et al.

Anonymous Referee #1

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Review of: Investigation of the adiabatic assumption for estimating cloud micro- and microphysical properties from satellite and ground

Authors: D. Merk, H. Keneke, B. Poppichal and P. Seifert

Recommendation: Accept with major revisions.

This study produces and compares ground- and satellite-based retrievals of H and Nd for 4 days selected from a CloudNet site in Germany. The study focuses on the contribution of uncertainty in f_ad to the retrievals, ignoring contributions from radiometric noise or other factors. The study has been performed in service of the HOPE project.

Studies based on the datasets used by the authors tend to produce results that are regionally dependent, and as far as I know an overview of the regionally-varying findings is still lacking. This study adds to that literature but its connection to previous studies is not well stated. The presentation is unclear and the originality of this contribution needs more clarification. The figures are difficult to read.

I would encourage the authors to include a more explicit overview of the f_ad values reported in the literature, of how it is represented in retrievals of N_d and H, and to provide a more direct comparison to the other studies that have focused on H and N_d retrievals. I will use the example of the southeast Pacific because I am most familiar with that literature, but I would encourage the authors to be as fully comprehensive as possible.

For example, Painemal, D., and Zuidema, 2010, ACP, found an overestimate in H_sat when compared to ship-board measurements, that they attributed to an overestimate in the satellite re (see their appendix). This is similar to the current study's findings, in that this study's H_sat is lower than that measured from the ground - which they attribute to an underestimate in satellite re. So both the Painemal&Zuidema and the current student highlight the importance of the satellite-derived re, with the solar zenith angle differences (I think) resulting in the opposite sense of the bias.

Regarding N_d, there is some disagreement in the literature on how best to calculate Nd from satellite that is related to f_ad. The authors cite Bennartz, 2007 -its Nd calculation assumed an f_ad of 0.8. Painemal and Zuidema 2011 (JGR) p. 8 discuss how f_ad is represented in their Nd calculation vs. that by George and Wood (2011), and Painemal and Zuidema 2013 (ACP) eqn 9 provide another approach for calculating N_d that allows for a fluctuating f_ad. I would encourage the authors to be more explicit on how their study fits in with these and other similar studies, and then use the opportunity to opine on what they think is the best approach for satellite retrievals over Germany.

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more specific comments follow.

abstract: the optimal estimation technique only considers variations in f_ad. please clarify. also mention location, and the 4 dates (these provide some information on the synoptics). mention that the current SEVIRI retrieval underestimates re relative to ground and MODIS measurements (rather than “sensitive to satellite re retrieval”).

introduction: many previous studies are cited. at this point the reader is not yet clear on what the authors are doing. please group the studies that have similar goals but use different instruments (eg lidar, solar radiometers) separately, then discuss the papers that have applied similar instrumental datasets to this study. Briefly but more explicitly summarize previous findings relevant to the current study on f_ad and major uncertainties. Mentioning location of the previous findings and contrasting to the cloudnet site used here can be one way to establish originality. which previous studies are most similar to what the authors pursue here? mention the cloudiness site location explicitly and the 4 dates. mention the OE approach constrains itself to the f_ad model only, and justify why, including why radiometric noise is not being considered. also, how does this study differentiate itself from the cloudnet products? a table might be a nice way to present the results from previous studies (and this one).

2.1 first paragraph could well go in the introduction. be more specific about the instruments and dates.

2.1 bottom of p. 5134. why were no soundings used? the simulated cloud top heights do not match those observed by the radar well according to fig. 1 but I see little discussion of this anywhere.

2.2 line 5: “the most interesting cloud deck” - please make this more specific/objective. 2.2 how was the drizzle/no-drizzle threshold specified? how sensitive are your results to this threshold? at the other end, how sensitive is the radar? 2.2 p. 5140 lines 6-19: why not provide your own estimate of the uncertainty in gamma_ad(T,p)? you can estimate the cloud base temperature for your 4 cases. given the poor NWP estimate of

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cloud top temperature, this would provide a stronger argument for a smaller gamma_ad uncertainty than what you provide here.

p. 5142 line 9: please clarify what the beta index is for the reader rather than referencing other papers.

3.3.2: please explain why we should care about f_ad to the exclusion of other factors. this should go in the introduction. among other factors worth considering I'd also suggest the radar vertical resolution and radar sensitivity, and the beta index, which serves as a measure of the droplet spectral width. how confident are you in the ground-based H retrievals?

3.3.2: doesn't the radar Z profile give you some information about f_ad ? do all the cases show a Z profile that increases with height, as one would expect for a non-drizzling cloud? I cannot tell from the figures.

3.3.2 p. 5144 lines 23-26: only now are the readers told the methodological constraints imposed upon this study. these need to go into the introduction and motivated better.

4.1.1. p. 5145 lines 8-9: please be more specific about the contribution to enhanced Q_l by drizzle and the underestimation of actual H_cloud. perhaps subsample your dataset further to exclude such cases? further on on p 5146 you mention it is primarily the H<400m clouds that are superadiabatic. is this because the radar doesn't see the upper radar range gates? estimate the resulting uncertainty.

4.1.1. p. 5146, line 12-15: finally, a quantitative assessment of Q_l and H uncertainty. I would suggest subsetting your sample to reduce the relative size of these contributions.

p. 5148 line 7-10: I cannot see this feature in fig. 1b.

p. 5150 line 4:I would be surprised if drizzle is strongly contributing to a higher microwave-derived q_l. see Zuidema et al., 2005 (JGR) Appendix A for a quantification, to develop your intuition on this. But if drizzle is apparent in the radar reflectivity profile, that profile doesn't meet the selection criteria and should not be considered,

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no?

4.2.2: do you find modis-seviri differences in re and tau as a function of sza? if not previously reported, it would be useful to do so.

4.2.2. page 5153, end: please, somewhere you need to discuss your drizzle reflectivity threshold and your sensitivity to that threshold.

conclusions: it seems to me that the main contribution of the study could be to suggest a subadiabaticity factor for the satellite retrievals, or a way of incorporating subadiaticity into the satellite retrievals based on the initial retrieval of H and Nd. do the authors have any thoughts on how to do this? it is mentioned at the end but rather vaguely. or is a good take-away point that the SEVIRI re retrievals appear to be too low - is this an original finding? you mention solar radiation observations - are those available at the cloudnet sites?

figures: the figures 1-2 are very difficult to read. perhaps in final form they will be a larger format? I would at least suggest using the plot size better, e.g., selecting y-ranges in fig 1 that show more of the data. could they perhaps be shown as 2x2 panels rather than one row of 4?

fig. 1 a: I don't believe I saw the Seviri CTH overestimate discussed anywhere. . . fig. 6: modis and seviri are difficult to distinguish. fig. 7: extremely difficult to read. please find a way of enlarging.

references: George, R. C., and R. Wood (2010), Subseasonal variability of low cloud radiative properties over the southeast Pacific Ocean, Atmos. Chem. Phys., 10, 4047–4063, doi:10.5194/acp-10-4047-2010 Painemal, D. and P. Zuidema, 2010: Microphysical variability in Southeast Pacific âĂĺstratocumulus clouds: Synoptic conditions and radiative response. Atmos. Chem. Phys., âĂĺ10, pp. 6255-6269. D. Painemal and P. Zuidema, 2011: Assessment of MODIS cloud effective radius and optical thickness retrievals over the Southeast Pacific with VOCALS-REx in-situ measurements. J. Geo-

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Painemal, D. and P. Zuidema, 2013: The first aerosol indirect effect quantified through airborne remote sensing during VOCALS-REx. Atmos. Chem. Phys., 13, pp. 917-931. doi:10.5194/acp-13-917-2013.

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