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Interactive comment

Interactive comment on "The sub-adiabatic model as a concept for evaluating the representation and radiative effects of low-level clouds in a high-resolution atmospheric model" *by* Vasileios Barlakas et al.

Anonymous Referee #2

Received and published: 7 August 2019

Summary Barlakas et al. exhaustively analyze simulated shallow cloud profiles over Germany from a few select days to analyze cloud radiative effects (CREs) and the minimal set of parameters necessary to represent these effects. A number of different complementary and at times overlapping analyses are performed to isolate the optimal CRE parameter set, which are generally in close agreement with past observational and modeling studies.

Due to major concerns regarding the novel contributions of this study, I recommend major revisions before publication. The authors may consider trimming the manuscript

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to be considerably shorter given the redundancy of many of their results.

General comments I have serious concerns about the novel contributions of this study and I found that the results of the work undermined the stated motivation: 1. There is already a significant body of work on the topic of sub-adiabaticity (much of which the authors cite) and the results of this study seem to confirm past findings (in particular, those of Merk et al., 2016) with little added insight into process (radiative, microphysical, etc.) besides pointing out that single-moment microphysics schemes leave much to be desired (which has been explored by e.g., Igel et al., 2015, JAS). Much work has also been done with respect to statistical emulators for understanding cloud radiative effects (e.g. Feingold et al., 2016; Glassmeier et al., 2019) and the aggregation of model data over all shallow cloudy columns severely limited the authors' ability to examine details regarding differences between cumulus and stratus (which likely exhibit very different fad), the diurnal cycle, or radiative effects across spatial scales – an exploration of the latter would be especially useful since the 300 m HOPE dataset is finer in horizontal resolution than the existing remote sensing products this study is designed to improve (typically ~1 km pixel size).

Finally, I get the sense that this paper only deals with sub-adiabaticity in passing – the latter half of the paper is primarily concerned with describing CREs with a minimal set of variables and is almost completely disconnected from the title of the paper. Sub-adiabaticity seems to have only a weak influence on CREs.

2. With respect to motivation, the authors rely heavily and repeatedly on the idea that there are large uncertainties in aircraft measurements of cloud drop number concentration (ND in the authors' notation), which they justify by citing the ND retrieval review paper of Grosvenor et al. (2018) – specifically, I believe they refer to Grosvenor et al.'s Figure 5 (which is in turn based on data used in Siebert et al., 2013) and accompanying discussion. This is an unfortunate figure. The disagreement of two probes (Phase Doppler Interferometer and Particulate Volume Monitor; PDI and PVM, respectively) at concentrations of ND >350 cm-3 is used as evidence that in situ probes have a general,

systematic problem measuring ND.

The issue with this illustration is that one of the two probes used (PVM) is not designed to measure ND and I am aware of no other publication in which this is even attempted. The PVM measures extinction from a population of cloud drops and makes no explicit count of particle density. In fact, I'm not even sure how this quantity was generated since the PVM returns only two data streams: total particle volume and surface area. The PDI, on the other hand, is frequently used by both the airborne cloud physics and industrial spray characterization communities and has been demonstrated to accurately count (and size) particles up to a concentration of O(10ˆ5) cm-3. An intercomparison of PDI with other probes that explicitly count particles (CAS, FSSP, CDP, Holodec. . .etc. – there are a great number and I don't understand why Grosvenor et al. chose such an ill-suited probe for their figure) would likely show a much better overlap in the PDFs of ND from different probes; such an intercomparison of the latest generation of cloud probes is currently underway for the recent NASA ORACLES campaign, which sampled a wide variety of concentration conditions due to the campaign's focus on interaction of clouds with overlying smoke layers during the stratocumulus to cumulus transition.

I am strongly opposed to the use of phrasing such as "large instrumental uncertainties" (e.g. page 23, lines 10-11) as I think this point is vastly overstated by Grosvenor et al. (2018), an assertion backed by their discussion of myriad other issues with retrieval assumptions ahead of any problems with in situ measurements.

In the remainder of the review, specific comments reference "PX, LY" where X is page number and Y is line number. When a direct edit to the text is suggested, it is given in italics. (see attached document)

Specific comments

P2, L7: "taking placed" should be "taking place"

P2, L21-22: “fixed droplet number distribution” – ambiguous terminology; “fixed droplet size distribution” would be clearer.

P2, L23: “Double-moment microphysical schemes. . .are only recently becoming more widespread”: Perhaps in the operational forecasting community this is true, but in research modeling (especially of warm clouds), double-moment schemes have been common for at least a decade.

P5, L16: Why do you use an indirect measure for rain/drizzle instead of directly examining rain water mixing ratio? I understand that it makes for a more straightforward comparison with observations, but it seems like an unnecessary step.

P6, L2: “The model outputs the. . .”

P6, L19: Is the assumption of vertical homogeneity a “scheme?” Seems like an odd word choice.

P7, L15: “Clapeyron relationship”

P7, L16-17: This sentence is difficult to follow. Rephrase and simplify the structure for clarity.

P11, L8: Remove “the” from “the 5 May. . .”

P13, L12-13: “with a 5/6 slope” – possibly remove the word “fit,” doesn’t make sense in context

P13, L18: If fad only accounts for 0.14% of the variance in ïĄť, what’s the point of all this?

P13, Section 4: The step by step narrative of the PC analysis is overwrought. If you primarily intend to use the results of the RC analysis to justify the minimal set of variables needed to represent CREs, skip the PC discussion; the PC and RC results are sufficiently similar that it is redundant. P13, L34: “optimal” instead of “optimized”

P14, Table 3 caption: Remove trailing zero from “moderate [0.40, 0.6)” for consistency

P14, L7-9: Rearrange sentence beginning “However, the PCs. . .” to simplify structure for clarity.

P14, L11: remove “so-called” – this makes it look like other people have a different name for it

P15, L9-10: I am confused by what you’re doing here – are you always running multiple simulations, or for scenarios S2-S4 are you imposing LWC/ND profiles that are not actually from the simulations?

P15-16, L33-3: the assumptions would be more clearly expressed in a table

P16, L1-2: Why are there drops in the free troposphere?

P16, L3: “where the liquid water path is preserved”

P16, L8: “following the climatology of a coarse. . .” – you only use the ECHAM value. Is this representative of what all GCMs do? If not, the generalization doesn’t work.

P17, Section 5.1.2: I found the latter half of this discussion to be very difficult to follow, especially the references to various scenarios by only a letter or number near the end of the section (i.e. last paragraph, P18).

P18, Table 5 caption: Cosine SZA was just given in text (and will hopefully be put in a separate table of assumptions) – remove since redundant.

P18, L1: “and the rest of the simulated. . .”

P19, Table 6: Two things: 1) numbering of scenarios is off by one and 2) since BOA and TOA are almost always within 5% or 1 W/m2 of each other, can you just pick one and reduce the amount of information here? This table would be much more effective/digestible.

P19, L11-14: You can test whether effective radius is outside the range. Is this an issue

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or isn't it?

P20, Section 5.1.3: As with the PCA results, what is the point of showing both correlations? You almost exclusively discuss Spearman, so why not just show that?

P20, L7: Capitalize "Spearman"

P24, L3: "uncover potential shortcomings in. . .models": you only compared the model to itself, so how did you uncover shortcomings? Do you mean LES vs. GCM? Beyond discussing single- vs. double-moment microphysics (an already well-known issue), what shortcomings did you uncover?

P24, L11: quantify contributions of 3rd/4th components to total variance here.

P24, L11: delete "so-called"

P25, L9: again, is the ECHAM climatological ND representative? Is it even backed by observations? You have not made a case for why this is a good number to use, besides the fact that a single GCM uses it.

P25, L10: How do two fixed values constitute a profile?

P27, Eq A13: is exponent in denominator a typo? D^0=1.

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2019-137/acp-2019-137-RC3-supplement.pdf

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