

## Comment on acp-2020-1306

Anonymous Referee #3

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Referee comment on "Sensitivity of low-level clouds and precipitation to anthropogenic aerosol emission in southern West Africa: a DACCIIWA case study" by Adrien Deroubaix et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1306-RC2>, 2021

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I have reviewed "Sensitivity of low-level clouds and precipitation to anthropogenic aerosol emission in southern West Africa: a DACCIIWA case study" by Deroubaix et al. The title succinctly summarizes the study. The study suffers from the problem inherent in case studies, namely generalizability. But it is solid work, and I recommend publication after minor revisions to address my concerns.

My first concern is that the conclusions (precipitation suppression by anthropogenic aerosols delays the breakup of clouds) are mainly a reflection of the cloud physics included in the model. But models are by necessity incomplete. While this model includes a precipitation suppression mechanism via its precipitation microphysics, there are other aerosol effects that could lead to an enhanced loss of cloud cover through evaporation (e.g., Ackerman et al., 2004). These effects are unlikely to be correctly represented in a 5 km resolution model without convection parameterization because the relevant scales are much smaller for shallow clouds. Over all, the enhanced evaporation effect is as strong (Toll et al., 2019) or stronger (Gryspeerdt et al., 2019) than the precipitation suppression effect, but there is likely to be a great amount of diversity depending on cloud regime, aerosol loading, etc.

To gauge how much to trust a model that only parameterizes the precipitation mechanism, it would be extremely helpful to know whether the breakup of the clouds discussed in this case study is mainly evaporation-driven or precipitation-driven to begin with. This is of course easier said than done, because we don't have observations of evaporation flux. But a good starting point would be to ask the model: what fraction of the LWP tendency can be explained by evaporation and what fraction by precipitation? If precipitation plays a sizable role in the cloud dissipation in the model, then the next question is whether the precipitation timeseries shown in Fig. 10 agrees with observations. Therefore, I was disappointed that Fig. 10 does not include any observations at all. It would also be useful to include more description of these clouds; I assume they are fairly deep (but still warm) cumulus clouds for which precipitation dissipation is a reasonable assumption, but the onus is on the authors to make this argument.

My second concern is representativeness. Recognizing that aerosol effects on LWP and cloud cover tend to be subtle and can have either sign, it is hard to draw a general conclusion from this study, even setting the model correctness concern aside for the moment, and I am struggling to identify anything new that I have learned from reading the manuscript. This is of course a general problem of case studies with no easy solution. Ideally, the manuscript would make connections to other work, e.g., longer time period regional modeling, and discuss how this analysis corroborates or modifies conclusions of those longer-term studies. Another approach would be to perform an ensemble of model runs for this case study to explore how robust the conclusions are to meteorological variability or model physics (depending on how the ensemble is designed). Model runs (especially ensembles) are not free, so I do not expect the authors to come up with additional analysis. However, I think it is important for the authors to at least discuss representativeness in the final paper.