Review of

"Evaluation of the CMIP6 marine subtropical stratocumulus cloud albedo and its controlling factors"

submitted to *Atmospheric Chemistry and Physics* by Jian et al.

This study evaluates the simulation of cloud albedo in subtropical stratocumulus regions in CMIP5 and CMIP6 models and investigates its controlling factors using observations. They find that cloud albedo is underestimated in CMIP6 model AMIP simulations but that, on average, CMIP6 models do a better job than CMIP5 models in reproducing the seasonal cycle of cloud albedo in stratocumulus regions. Lastly, they find differing contributions of various aerosol types to changes in cloud albedo, and that these contributions differ between conditions of low and high liquid water path.

Overall, while certainty there are some interesting results contained in this investigation, there are, in my view, substantial issues with the methodology that need to be addressed. Most importantly, there is an insufficient consideration of the meteorological factors impacting stratocumulus clouds.

Below I provide specific comments.

1. **Insufficient consideration of meteorological factors.** Only two meteorological factors are considered in this study: omega900 and RH850. However, subtropical stratocumulus (including cloud optical depth/thickness, LWP, and cloud droplet effective radius) are impacted by several other important meteorological factors, including sea surface temperature, estimated inversion strength, horizontal surface temperature advection, and wind speed (e.g. Fuchs et al. 2018, Scott et al. 2020, and references therein). Therefore, the consideration of only omega900 and RH850 in this study is inadequate. The omission of the other meteorological factors noted above may in fact greatly affect the results of Section 3.2, "The impacts of different aerosol types and meteorological factors on cloud albedo changes", due to confounding effects. The authors state that "If the correlation between the cloud albedo and a [predictor] candidate is significant at a 90% confidence level, the variable was considered as a predictor factor." But which candidates were considered? Inversion strength and advection have been shown to be the dominant meteorological controls on interannual changes in cloud optical depth in eastern ocean stratocumulus regions (Scott. et al. 2020). Therefore, I find it hard to believe that these two cloud-controlling factors are not significantly correlated with cloud albedo.

   Chen et al. (2014) investigated the effects of aerosols on marine warm clouds using observations. They found that the response of LWP to aerosol loading strongly depends on lower tropospheric stability *and* free-tropospheric moisture. This is additional evidence that the omission of several meteorological factors, especially the inversion strength, is a crucial oversight in the present study.

   Finally, the choices of 900 hPa and 850 hPa as vertical levels for omega and RH are not justified. These levels are not external to the boundary layer. Omega700 (or 500) and

RH700 (or 500) should be used instead, as is standard, since they represent *free-tropospheric* vertical velocity and humidity. The authors should use these vertical levels instead, unless they can provide a compelling justification for their unusual choice of vertical levels.

2. **Lack of analysis of satellite simulator output.** Modern analyses of cloud fraction in GCMs should incorporate at least some analysis of satellite simulator output, such as ISCCP simulator cloud fraction. However, the current study compares the raw GCM cloud fraction with satellite cloud fraction, which is a somewhat outdated approach. Some of the differences between GCMs and the observations found in the paper may be due to different definitions of cloud fraction. The authors do note that "this study… employed the total cloud fractions as there are no available MODIS simulator outputs for CMIP6." However, ISCCP simulator output is available for several CMIP5 and CMIP6 models. MODIS cloud fraction is more comparable to ISCCP simulator cloud fraction than it is to the raw GCM cloud fraction.

3. **Lack of analysis of low-level cloud fraction.** The authors should verify that their key observational results are valid for low-level cloud fraction. The regions chosen are dominated by low clouds, but high cloud variability may impact some of the results.

4. **Lack of verification of results with additional observational data.** MODIS is the state-of-the-art passive satellite cloud dataset, but, given that multi-linear regression can be sensitive to the input data, the authors should examine additional satellite data (such as ISCCP cloud fraction and the Multisensor Advanced Climatology of Liquid Water Path [Elsaesser et al. (2017)]) to corroborate their results and establish robustness. Additional reanalyses should be considered as well for the meteorological data. ERA5 is considered to be the most state-of-the-art reanalysis.

5. How is the threshold of 60 g m^{-2} for LWP chosen?

6. **Excessive detail in discussion of results.** I was quite overwhelmed with the amount of detail discussed in the results section of the paper concerning results for individual models and individual regions and for specific performance metrics. Even after reading the paper a few times, I cannot answer the basic question, "Has the simulation of CMIP6 stratocumulus cloud albedo changed in any major way compared to CMIP5, or is it fundamentally unchanged relative CMIP5?" The paper would be improved by identifying the key differences and similarities between CMIP5 and CMIP6, rather than discussing a detailed and hard-to-remember list of very specific results.

References:

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