2nd review of Goto et al.

Thanks for making many of the changes suggested by the referees. I am mostly happy with the paper now. There are just a few places where some additional figures would be useful to show what is written in the text and where the text could be improved to make things clearer.

Major points

L413 – "Therefore, the overestimation of the NSW6-simulated LWP in the eastern Pacific Ocean and Southern Atlantic Ocean effectively balanced the underestimation of the zonal averages of the simulated LWP and unexpectedly led to zonal LWP values closer to the MAC results."

- I found this difficult to follow. I suggest rewording to :-
- "Therefore, the overestimation of the NSW6-simulated LWP in the eastern Pacific Ocean and Southern Atlantic Ocean effectively balanced the underestimation in the Western Pacific Ocean and Indian Ocean, which led to zonal LWP values that were closer to the MAC results."
- Also, it is still not easy to make this comparison from Fig. 2. Map plots of the bias of the models vs MAC would be helpful. As would averages (or the bias and/or LWP fields) for 0-30S as a function of the longitude.

L614 – "When the simulated LWP is underestimated, the simulated aerosols are also underestimated, because the simulated precipitation is generally comparable to the observations in this study, as shown in Figure 1. When the simulated CDR is overestimated, the simulated CCN must be underestimated. This is consistent with the underestimation of the simulated aerosol. Therefore, if the negative biases in the simulated SWCRF are eliminated, the simulated aerosols will increase."

- This is quite speculative. It's not clear how well linked the LWP, CDR and precipitation in the clouds in question are. For frontal mid-latitude precipitation the relationships may not be as straightforward as for stratocumulus. Precipitation is likely to be determined by large scale meteorology. Plus, it's not clear how close to the observations the model needs to be in order to allow a constraint on LWP or CDR. The comparison between models and observations is likely highly uncertain for precipitation. I'm not sure whether you need the extra text here (and it is quite difficult to explain what you are trying to say in a clear way!).

L663 – "Specifically, above a height of 3 km, where ∂Qc is close to zero and ∂CDNC has a positive value, ∂CDR should be small. The possible overestimation of ∂CDR in NSW6 represents possible overestimation of the Twomey effect in NSW6."

- You would need to add dCDR to Fig. 13 to show this.
- Generally it seems strange that such large changes in CDNC in NSW6 (assumed to be the same as dCCN) produce fairly small changes in CDR. E.g., Fig. 12 states that for the US there is around a 60 per cm3 increase in CDNC for NSW6. If we assume a baseline CDNC of 200 per cm3 (which is quite high) then the equates to a 260/200 = 1.3x increase. Since CDR is approximately proportional to CDNC^(-1/3) (assuming equal LWP) then this would be expected to lead to a 1.3^(-1/3) = 0.916x change in CDR. For a PI CDR value of 10um this would lead to a new CDR of 9.16um, or a 0.84um decrease. This is similar to what is

seen in Fig. 12 for NSW6. However, for NDW6 Fig. 12 says that there was only a 4 per cm3 increase in CDNC. Using the same numbers, this would lead to only a 0.07um decrease in CDR; but the quoted change is around 0.6um. So, there seems to be an inconsistency there. Using lower PI values for CDNC could allow the NDW6 numbers to work out, but then the NSW6 value would be out. So, something is not quite right here I think – could it be that the radiation scheme (presumably where CDR is calculated?) of NSW6 doesn't actually assume that the change in CCN is equal to the change in CDNC?

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L680 - "By decreasing the simulated ðCDR, increasing the simulated ðLWP from PI to PD, and increasing the simulated ðCA and ðCF at 1-km height, the negative values of the simulated ERFaci in the industrial regions, such as the United States, Europe, and East Asia, increase in magnitude."

- Presumably you are referring to NSW6 vs NDW6 here? You need to make that clear. However, dCDR is larger (in magnitude) for NSW6, and dLWP, dCA and dCF are smaller. Maybe you meant CDR is more negative, but better to talk in terms of magnitude I think.

L685 – "by considering the uncertainty caused by the assumption in the PI conditions."

- It's not clear what you mean here?

L691 – "Therefore, it was suggested that the ERFaci due to the cloud lifetime effect in NDW6 was larger than that in NSW6 due to the Twomey effect,"

- This needs a bit of explanation. Perhaps something like "This, combined with the smaller magnitude of decrease in CDR for NDW6 vs NSW6 and the larger magnitude increase in LWP and CF, suggests that the ERFaci due to the cloud lifetime effect in NDW6 is larger than that in NSW6 due to the Twomey effect,…"

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Typos/grammar

L18 – "but the differences between the results of NDW6 and NSW6 experiments were larger for some aerosol species, especially dust and sulfate, compared to those between the experiments with different horizontal resolutions, i.e., 14 km and 56 km grid spacing, as shown in a previous study."

- I suggest that you start a new sentence and slightly rewrite this to make this sentence clearer : "However, for some aerosol species, especially dust and sulfate, the differences between the NDW6 and NSW6 experiments were larger than those between experiments with different horizontal resolutions (14 km and 56 km grid spacing), as shown in a previous study."

L34 – "caused by ignorance of sink process in the cloud droplet number concentrations." -> "caused by the ignorance of sink processes for cloud droplet number concentrations."

L218 - "Hoesly et al. (2018) estimated global averages of the differences in the emission amounts of anthropogenic sources between 1850 and 2010 to be 2.1% (sulfate), 12.0% (BC), and 22.7% (OC)."

- I think this should be “Hoesly et al. (2018) estimated that the globally averaged emissions in 1850 were 2.1% of the 2010 emissions for sulfate, 12.0% for BC and 22.7% for OC.”