Review of "*Manipulating marine stratocumulus cloud amount and albedo: a process-modeling study of aerosol-cloud-precipitation interactions in response to injection of cloud condensation nuclei*", by Wang, Rasch, and Feingold

Recommendation: Accept subject to some revision
Review by Robert Wood, University of Washington

Overview:

This paper uses large-domain large eddy simulations to explore the sensitivity of marine stratocumulus clouds, under two sets of meteorological conditions and three different background microphysical states, to injections of cloud condensation nuclei (CCN) from ships. The experiments are designed to test a widely-known geoengineering proposal to increase the planetary albedo by increasing the cloud shortwave reflectivity through injections of artificially-generated sea-salt aerosol.

The study is very interesting and finds that only for some of the background cloud states (meteorological/microphysical) do the injections significantly increase the albedo. Clouds with the higher concentrations of background CCN typical of moderately polluted marine stratocumulus are barely susceptible to CCN injection. Clouds with low background CCN are typically more susceptible but only when the background state is precipitating. Indeed precipitation suppression appears to be a necessary condition for the CCN injection to increase cloud albedo, but too much drizzle is found to reduce the susceptibility somewhat. This dependence of albedo increase upon precipitation suppression is not solely because precipitating clouds are also those with low CCN and therefore are more susceptible in the Platnick and Twomey sense, since the dry meteorological conditions case has relatively low CCN (55 $cm^{-3}$ in the unperturbed case) that one would expect to be susceptible.

The results are intriguing and certainly worthy of publication in *Atmospheric Chemistry and Physics*. The manuscript is well-written and the figures very clear and instructive. The study left me with more questions than answers which is not a criticism in this case. I hope the authors can continue their work in this area. I have some specific suggestions and thoughts that the authors might wish to consider, as detailed below.

Main points:

1. I think Table 2 should include a column with the mean cloud cover as well as the other albedo-controlling variables LWP and $N_d$. Given the rather large increases of $N_d$ in the dry case (55 to 85 $cm^{-3}$), and given that there is very little LWP reduction, I am surprised that the albedo change is only 0.01, so can one assume that the cloud cover change is somehow compensating for the $N_d$ change?

It would also be useful to break down the overall albedo increase into a fractional part due to changing $N_d$, a part due to changing cloud LWP and a part due to changes in cloud fractional coverage. Simple formulae could be used to estimate these contributions.

2. Given that Ackerman et al. (2009) found that entrainment (and thus presumably feedbacks on LWP through exchange with the free-troposphere) was more sensitive to cloud droplet sedimentation than to drizzle in the RF02 case, why have the authors not mentioned sedimentation or its potential role?

3. Does exchange with the FT matter for the evolution of the clouds in any of the simulations? That is, is the FT moisture a relevant variable controlling the sensitivity of albedo to CCN injection?

4. What justifies the classification of the two meteorological regimes as DRY and WET? Can the authors state what it is about the simulations that warrants this?

5. Fig 1: The simulation W50P3 has a strange wraparound effect whereby close to the injection location on the left of the panels, the CCN appears to be high in the cloud-free regions. Is this an artifact of having the ship essentially perturb the clouds there twice? Does this affect the results in terms of albedo increases?

6. Are the $N_d$ and LWP values in Table 1 and discussed in the manuscript means for cloudy regions only or over the entire domain? If so, wouldn't it be more physically instructive to discuss LWP/Nd for the cloudy regions only and cloud fraction separately?

7. Does the fact that the unperturbed states are changing so rapidly (i.e. are far from equilibrium) have any impact on the results? This is especially the case in the RF02 set-up where the observed state was rapidly transitioning from closed to open cells. Figure 7 shows this rapid decrease.

8. Can clouds which have precipitation at cloud base but not at the surface be successfully perturbed this way? The simulations performed here seem rather extreme ends of the spectrum (1.27 and 1.87 mm/day **at the surface** for the W50 and W100 cases, and zero for the other two cases). What about for clouds somewhere in the middle?