Reviewer comments: Mason et al.

I am mostly satisfied with the author's response to my previous comments. I have a few outstanding questions/concerns that should be addressed before publication.

- L178-189: I appreciate the authors plotting the background cloud/aerosol conditions from the AMIP simulations and adding a bit more text to this section. However, I think this section as it stands is still lacking some explanation as to *why* certain regions have different CRF and DRF decompositions. Given that this article is making a case for why certain regions are more optimal than others for MCB, I think it is important to engage more with the susceptibility of different regions to the background conditions within UKESM1.
  For example, you mention that the SP and SA regions have the largest CRF but SEP and SEA have the largest ERF because of the strong DRF. Based on those plots of AOD, CDNC, LWP, and Reff, I speculate this is because the SP and SA have low CDNC but high LWP relative to the SEP and SEA. This means the addition of aerosols to the SP and SA seeds clouds whereas the SEP and SEA are already cloudy and saturated with aerosols, so the additional aerosols are directly scattering sunlight rather than forming clouds (i.e., it is easier to form clouds in less cloudy areas). This is just a plausible guess for the "diminishing returns" you mention, but I would double check this with your AMIP simulations.
- L228-230: You cite Haywood et al. 1997 as an example of successful linear additivity but it would also be good to put your results in the context of Hill & Ming (2010) who argue that at least the subtropical MCB regions have nonlinear additivity. They find a 19% greater cooling from the sum of regions compared to ALL which seems comparable to your results. To my knowledge, this is the original MCB paper that made claims about nonlinear additivity between regions, so it would be good to compare with their findings to support why the paradigm has shifted. I don't feel this comment was fully addressed in my previous review (e.g. differences between Fig 3o and 3p and Fig 4o and 4p).
- ENSO/SOI. I understand that 10 years is short to fully examine the ENSO response and its mechanisms. However, I do feel that if you are going to include it as a target for your optimization strategy, then you should show and discuss those results in Section 5. You should add a figure of the SOI time series, which would show changes in magnitude and frequency compared to a reference. Even if you aren't trying to optimize for the exact oscillations of the reference period, you could qualitatively compare the MCB SOI to the reference. Given that SOI and Niño3.4 have ~years periodicity, you may still be able to see a few cycles which would be informative as to how MCB changes ENSO.

- Ensemble size. I understand that it would be prohibitive to repeat the entire analysis with more ensemble members given the number of combinations for the optimization. However, I think it would be reasonable to run a couple more members for the 10-year Optim-14 , Optim-6, and Midlat experiments given that these are the “flagship” results of this proof-of-concept method. Even though the optimization is designed on one ensemble member, whether the targets are still met across different members would be a more powerful result than what is currently shown. This would also allow you to put some significance shading on Fig 13 and 14 in a more meaningful way than internal variability from annual mean responses. It would also potentially allow you to say something more about the ENSO responses/variability that you can’t comment on with one member. I maintain that one ensemble member isn’t enough to make robust claims about the MCB responses due to the potential of state-dependence in the initial conditions.