Response to Reviewer comments

We thank the reviewers again for their further feedback.

We have made edits to the manuscript following the 2nd round of comments. We present the reviewers' comments in black and our responses in blue. Changes made to form the revised manuscript are referred to by line number of the new manuscript (e.g. L50 refers to line 50 of the new document). The edits were made using tracked changes to help the reviewers see what changes have been implemented since the last round of revisions.

RC1: Anonymous Referee #1, 04 Jan 2026

General Comments

Hill and Ming 2012 'Nonlinear climate response to regional brightening of tropical marine stratocumulus'

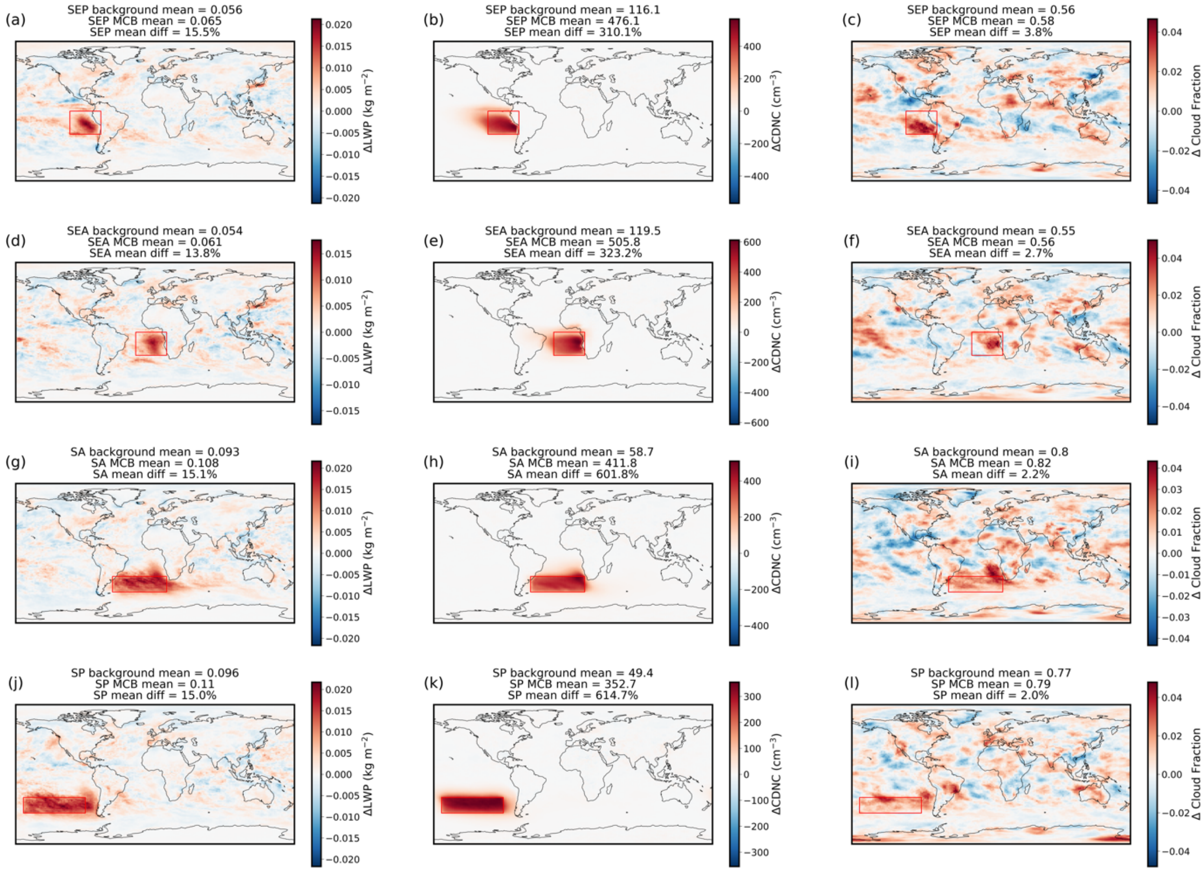
https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2012GL052064

Report #2 (Submitted on 01 May 2026) Anonymous referee #2

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.



We thank the reviewer for their comment. In response we have added the above plot (Supplementary Figure S1). We have plotted the LWP, CDNC and cloud fraction responses for the 50 Tg/yr MCB simulations for the SEP, SEA, SA and SP regions to further explore why these 4 regions with the highest ERF have such differences in their DRF and CRF components. The data confirms the reviewer's expectation that the relative CRF/DRF contributions to the ERF are explained by the relative background conditions of the different regions, with low background CDNC and high LWP for SP/SA with respect to SEP and SEA.

We have added the following text (L164):

'The SP and SA regions have lower background CDNC and higher liquid water path (LWP) compared to SEA and SEP as shown in Figure S1. While MCB in these regions results in a similar absolute change in CDNC in these regions, the higher background CDNC in the SEA and SEP show these regions are initially more saturated with aerosol particles. Therefore, 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.'

- 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).

We have compared our results to that of Hill & Ming (2010), and find similar degrees of nonlinear additivity between the regions (16% vs 19%). We do not intend to suggest a paradigm shift so have amended the phrasing in the manuscript.

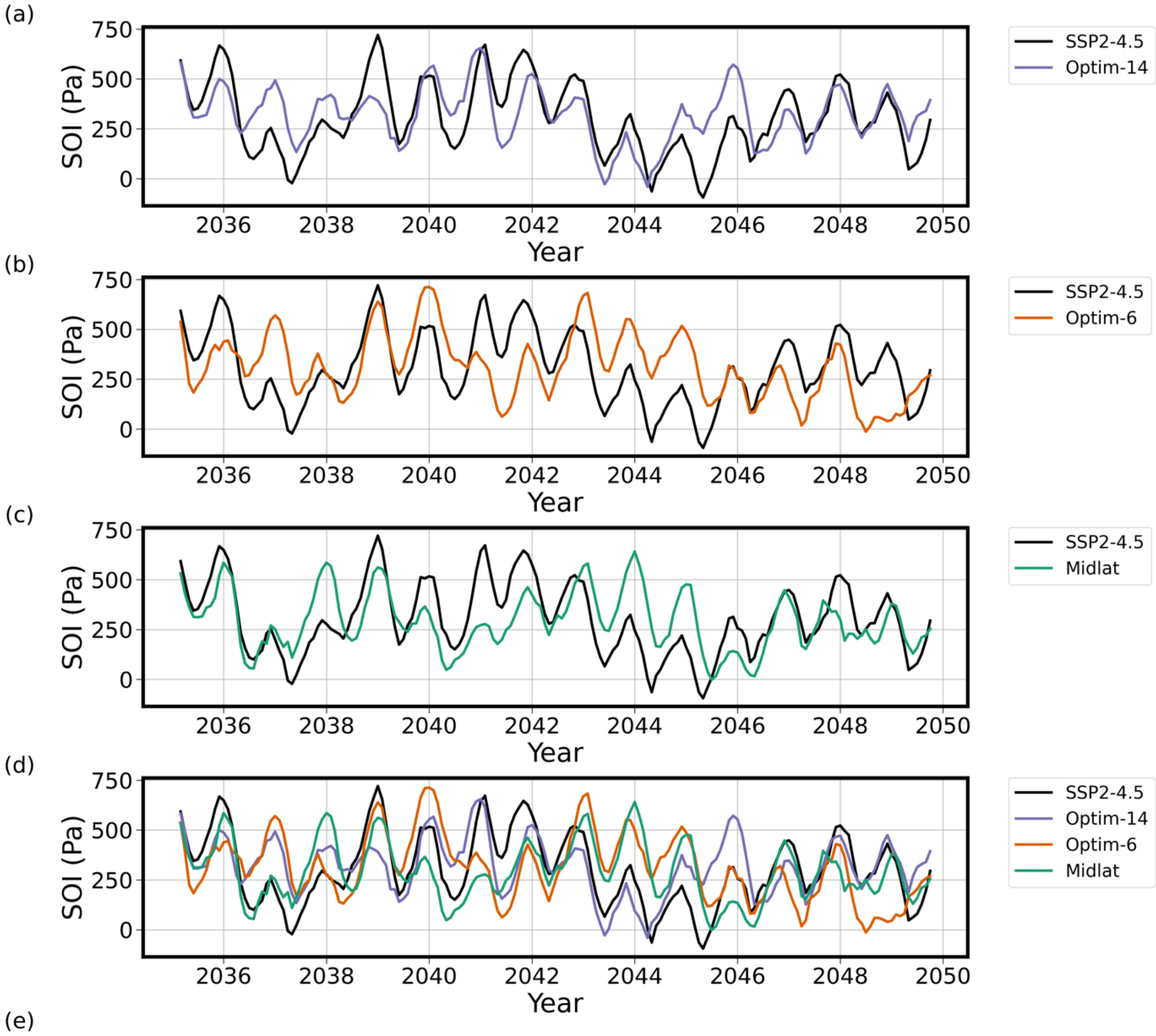
The differences between Fig 3o and 3p and Fig 4o and 4p do show better (zonally averaged) additivity for temperature that is much less the case for precipitation which is much noisier. Fig 4o/p show predicting spatial patterns of hydrological variables (e.g. what happens to precipitation over the Amazon) using the assumption of additivity across patch simulations would produce big differences from the simulated response. The global mean precipitation for the 700 Tg yr^{-1} simulation reaches 80% of the sum of the 14 regions compared to 86% for global mean temperature implying slightly lower additivity for precipitation.

Nevertheless, while the assumption of additivity of climate responses is not perfect, we think it is still useful and interesting for exploring optimisation using this assumption to approximate climate responses, as done in this study. We refer you to Figure 15 where the simulations are run with perturbations to several areas at once – you can see that the zonal mean change in the precipitation does generally follow the target. We have made adjustments to the manuscript to further acknowledge your points in this comment.

Changes to original text (L186):

"A comparison of the sum of these 14 simulations to the explicit 700 Tg yr^{-1} simulation, where 50 Tg yr^{-1} is deployed in each region simultaneously, is also shown in Fig. 3(o-p) and Fig. 4(o-p). A cooling of 4.77 ± 0.61 °C and a drying of 0.46 ± 0.08 mm day^{-1} from summing the individual responses compares to a cooling of 4.10 ± 0.69 °C and a drying of 0.37 ± 0.05 mm day^{-1} responses for the 700 Tg yr^{-1} simulation. ΔT_{glob} and ΔP_{glob} of the 700 Tg yr^{-1} simulation reached 86% and 80% of the magnitude of the sum of all 14 responses in the 50 Tg yr^{-1} patch simulations. The global mean ERF of the simulated 700 Tg yr^{-1} simulation was 90% of the sum of all 14 simulations. This non-additivity is consistent with previous studies which found the sum of responses to regional MCB perturbations tends to overestimate the cooling when MCB is applied simultaneously (Hill & Ming, 2012; Rasch et al., 2024; Hirasawa et al., 2026). Hill & Ming (2012) identified a 19% greater cooling from the sum of the temperature responses of three subtropical MCB regions compared to simulations of MCB in all three regions simultaneously, while Hirasawa et al., (2026) found that the sum of cooling from five midlatitude regions overestimates cooling by 13% to 29% in three models. Thus, models consistently indicate there is some non-linearity in the climate response to MCB. In terms of precipitation, Fig. 4 (o) and (p) show much worse spatial agreement between the sum and explicitly simulated simulation for precipitation, in large part because it is a noisier field. Despite this limited spatial agreement, we assume that the global mean responses ΔP_{glob} are sufficiently additive, since the sum of the fourteen regional ΔP_{glob} anomalies is 80% of the ΔP_{glob} response in the simultaneous 700 Tg yr^{-1} emission simulation. As we shall see, when running strategies that include perturbations from many different areas simultaneously, the resulting zonal precipitation patterns show reasonable agreement with the desired target (Fig. 15) suggesting that the approach is valid."

- 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.



(e)

Simulation	Mean (Pa)	Std (Pa)
SSP2-4.5	308.2	236.5
Optim-14	324.5	200.7
Optim-6	317.3	214.7
Midlat	297.2	205.5

We have added the above plot (Supplementary Figure S2). These are 5 monthly rolling mean SOI timeseries for the three deployments in Section 5, to visualise changes in magnitude and frequency of ENSO due to MCB in the model, with panel (d) overlaying all 3 simulations for their comparison. A summary table showing the mean and standard deviation of the monthly SOI for the 15 year simulations is included in panel (e). It is unclear from these timeseries what the overall impact on ENSO is for each of the deployments but have included them in the supplementary and discussed them in section 5.

A new paper (Wan et al., 2026) has just come out looking at the potential of different MCB interventions in the southeast Pacific to mitigate extremes by weakening El Nino events. The paper encourages the consideration of including targets affecting climate variability and teleconnections in SRM research, so we have referenced it in Sections 5 and 6.

We have added the following text (L431):

'All three deployments have ΔSOI responses within the target range, with smaller changes to the 2040s mean ΔSOI when compared to the responses to 50 Tg yr^{-1} MCB deployments in individual regions, as seen in Figure 6(d). The deployments do not have any deployment in the SEP region which has the largest ΔSOI response, although they all include deployment in the SP region which has the second largest ΔSOI response. However, these ΔSOI responses are highly variable as seen in Table 4. Figure S2 shows the SOI timeseries for the three deployments, which shows a small reduction in the standard deviation of SOI that is not statistically significant.'

(And to L498):

'Alternative targets that consider the magnitude and frequency of El Niño and La Niña events, along with ITCZ targets, would be interesting to incorporate into future optimisation analyses. MCB could potentially play a role in reducing strong ENSO events as suggested by Wan et al. (2026), motivating further research into the potential and risks of MCB used to target specific ENSO events to mitigate extremes.'

- 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.

We thank the reviewer for their comment. While we agree that a number of ensemble members would allow for a more robust analysis in Section 5, there was a change in HPC at the UK's Met Office after running our simulations. The analysis in this paper used output from UKESM1.0 that was run based on the Unified Model vn 11.2. This UM version is not supported on the Met Office's new HPC.

We have added this explanation and further discussion of the limitation of the one ensemble member to section 2 'Model description and outline of simulations' (L137):

'The Unified Model (UM) version used in this study is no longer supported, preventing an expansion in ensemble size for these three simulations with the same configuration. A single ensemble member is, therefore, a limitation of this study. However, an exploration of the climate responses of these simulations in Section 5 acts as a proof of concept for the methodology used in this paper. Future studies should certainly expand on the ensemble size.'