We thank the reviewer for the comments on our manuscript. Please find our responses below.

Regarding the reviewer's general input on the design of the Geoengineering Model Intercomparison Project (GeoMIP) G4sea-salt experiment:

The G4sea-salt experiment was designed by Kravitz et al. (2013) to do certain things based on results that previous studies obtained. The main idea of the G4sea-salt experiment is to validate those results in a multi-model context. One of the key questions defined by Kravitz et al. (2013) is: To what extent do the effects of sea spray geoengineering depend upon the location of clouds? In this study we try to address this question. All GeoMIP simulations are idealised to a greater or lesser degree, but the point is that they form a defined, published protocol. The reviewer's stance that publication cannot be recommended because the published protocol doesn't agree with the reviewer's view seems to us miss the point of a MIP using idealised scenarios. It would be like criticizing a study of model responses in the CMIP5 instantaneous 4xCO2 experiment on the basis that such a large and rapid change in CO_2 is unlikely; of course this is true, but it's still an incredibly useful way of learning about climate system behaviour.

The reviewer also makes claims about how this form of geoengineering would be carried out in practice. As actual deployment is currently hypothetical, we are reluctant to make any changes to the manuscript that would alter our protocol in favour of one that is, in theory, more or less realistic.

Specific comments to the authors

In my view, the direct effects produce a large fraction of the ERF for the following reasons:

1. The assumed diameter of the emitted particles is larger than that recommended in specific studies. Connolly et al. (2014), which appeared in a Phil Trans special issue on geoengineering, used detailed parcel modeling to show that median dry particle diameters from 35-100 nm are optimal for brightening marine low clouds. The diameters used in this study are 200 nm (HadGEM), 260 nm (NorESM), and 880 nm (GISS), which require at least an order of magnitude more mass to be sprayed to produce the same brightening effect (see Fig. 1b in Connolly et al. 2014). This would require enormous amounts of energy (the energy required for the production of aerosol particles scales approximately with the overall mass of salt sprayer) compared to the case with smaller particles. Small particles are most effective for brightening clouds, whereas somewhat larger particles (0.2-1 micron) are optimal for the direct effect. Given this information, one could probably have predicted that the direct forcing would be dominant before the model experiments were conducted.

As mentioned above, the main goal of the G4sea-salt experiment is to validate the results that previous ESM studies have provided in a multi-model context. Existing ESM studies that take

into account the sea spray injection process include e.g. Alterskjær et al. (2012, 2013), Jones and Haywood (2012), Korhonen et al. (2010), Partanen et al. (2012), and Wang et al. (2011). In all these studies, the dry diameter of the injected particles is within the interval of 0.20-0.44 µm. Thus, the size of the injected particles in our study is within the size range of the previous ESM studies, which is necessary when validating the results of those studies.

Moreover, we did not choose this range of particle sizes simply because they were what previous studies used. Alterskjær and Kristjánsson (2013) showed using NorESM1-M that injection of accumulation mode sea salt particles resulted in a negative forcing, whereas injection of Aitken mode particles resulted in a positive forcing caused by a strong competition effect combined with high critical supersaturation of Aitken mode sea salt. Although the positive forcing caused by the injection of Aitken mode particles could be due to limitations of the Abdul-Razzak et al. scheme used in NorESM1-M, as suggested by Connolly et al. (2014), representing marine cloud brightening in our simulations requires injections that produce a negative forcing. In using such large-scale models, compromises like this must necessarily be made, but since our focus is on large-scale climate rather than process-level understanding, we are comfortable that this compromise suits the purposes of our study. Because the size distributions of the injected particles are assumed to be equal to those of the natural accumulation mode sea spray aerosol in the three models, the size of the injected particles varies across the models, allowing us to incorporate a study of model spread in our analyses.

Although the main goal of this study is to evaluate previous results, and we certainly do not claim that this study provides the exact details of how this form of geoengineering would be carried out in practice, we would still like to comment on the reviewer's statement "*The assumed diameter of the emitted particles is larger than that recommended in specific studies*". The paper by Connolly et al. (2014) is interesting, but it is only one study. In that study, it is assumed that the injected particles consist of pure sea salt. However, extensive measurements show that organics contribute substantially to the composition of sea spray aerosol, and in many areas is even the dominant constituent (e.g. de Leeuw et al., 2011). As sea spray geoengineering would likely produce particles with a similar composition as natural sea spray, the injected particles would thus need to be larger to activate to cloud droplets compared to when assuming pure sea salt as in Connolly et al. 2014. In particular, the presence of organics strongly suppresses hygroscopic growth compared to pure sea salt. This is relevant since Connolly et al. (2014) conclude that interstitial particles play an important role in controlling the albedo in their study.

Finally, the reviewer writes "*Small particles are most effective for brightening clouds, whereas somewhat larger particles (0.2-1 micron) are optimal for the direct effect. Given this information, one could probably have predicted that the direct forcing would be dominant before the model experiments were conducted*". First of all, we have not predicted that the aerosol direct effect is dominant. The conclusion of this study is that that the effective radiative forcing (ERF) by the injected particles in most regions is as large in clear-sky conditions as in cloudy-sky conditions. The exception is in the subtropical marine stratocumulus regions, in particular for HadGEM2-ES, where the presence of clouds enhances the ERF compared to clear-sky conditions. Jones and Haywood et al. (2012) obtained a much larger radiative impact when maximizing the aerosol indirect effect than when maximizing

the aerosol direct effect. The aerosol indirect effect dominated the radiative impact also in Partanen et al. (2012). Both these sea spray geoengineering studies used injections of accumulation mode particles, similar to our study. Based on previous studies, we are unable to arrive at the reviewer's conclusion.

2. Seeding takes place over the entire tropical ocean. Low cloud cover is limited over much of the warm tropics, so this favors direct forcing to achieve -2 W/m2 ERF. Furthermore, there is cirrus above much of the low cloud in the warm Tropics. This is not how marine cloud brightening would work in practice.

As noted above, we consider statements regarding the practical implementation of marine cloud brightening to be difficult to defend, and we are unwilling to alter our experimental protocol to conform to conjecture. The oceanic regions between 30°S and 30°N have been identified as containing most of the radiatively important stratocumulus cloud decks (Alterskjær et al., 2012; Jones and Haywood, 2012). However, we agree with the reviewer that the presence of cirrus clouds close to the equator is not optimal for sea spray geoengineering. Although high clouds likely reduce the efficiency of sea spray geoengineering, a MIP can provide information on how large this reduction is in different models, as well as how large the horizontal variability in ERF is across the injection area. The results of our study indicate that the horizontal variability may be somewhat lower than seen in, e.g., Partanen et al. (2012).

3. Cloud LWP decreases over much of the region, thus countering some of the Twomey effect

We agree that the LWP response is important. However, our main conclusions given in the title of the paper are based on the simulations with fixed SST. Thus, the response in LWP caused by changes in the atmospheric circulation discussed in Sect. 3.2. does not influence the ERF in Sect. 3.1.

4. No provision for separating direct from indirect effects was built into the experimental design, which is troubling, and should be addressed. APRP is one way to achieve this without re-running simulations.

We have been careful throughout the paper not to claim that the aerosol direct effect dominates the radiative effect, or that the aerosol direct effect would contribute as much as the aerosol indirect effect. This paper focuses (mainly) on whether the presence of clouds increases or decreases the ERF in different areas, and how much the presence of clouds influences the effects of sea spray geoengineering. It would have been troubling if we would have stated that the aerosol direct effect dominates the forcing, or if we would have tried to

estimate the contributions of the aerosol direct and indirect effects. However, that is not the focus of our study.

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