Response to Referee #2:

We would like to thank the referee for her/his helpful remarks. Please find below point-by-point reply to your comments.

Q.1) I wonder why the authors have used the model in a nudged configuration while they use a climate model that can generates its own climate. In other words, it seems to me that if one wants to quantify the feedbacks between DMS, aerosol, and climate in a comprehensive manner, maybe one should consider how the changing DMS/aerosols/clouds may actually affect the climate and (possibly) further influence the DMS fluxes. This is my main concern regarding the paper and the Authors should discuss this issue in details. For example, could the Authors provide a possible estimate of how their results would be changed if they were to use the model in a free climate configuration?

The nudged configuration of the model was used mainly for the following two reasons. a) In order to compute radiative forcing purely due to the DMS emissions under different scenarios, it was necessary to nudge the boundary conditions. b) Also, in our simulations the DMS sea water concentrations are prescribed from the Kettle and Andreae (2000) climatology. So, even in a changing climate the feedback on DMS emissions could not be investigated.

Kloster et al., (2007) investigated the change in DMS sea water concentrations and hence, the emissions to a changing climate using the biogeochemistry model (HAMMOC) coupled to the ocean model (MPI-OM). However, this model was not coupled to a detailed cloud microphysics and the radiation routine to give us the complete feedback loop. The current study was meant to evaluate the aerosol indirect forcing due to increased DMS emissions, mainly driven by the specific geoengineering proposals put forward to counteract global warming by stimulating the aerosol-cloud interactions by iron fertilization (Boyd (2008), Jones et al., (2009), Korhonen et al., (2010), Wingenter et al., (2007)).

Q.2) There is no indication in the paper about the skills of the model regarding the representation of DMS and the related species SO2, sulfate etc. in the Southern Ocean in particular. This may be discussed in Thomas et al. (2010) but as the current paper should be standalone, it would be good to provide some indications in particular for the species relevant for the current study

Following the referee's suggestion, we have added the following information in Section-2 in the revised manuscript.

The performance of the different model components of ECHAM5-HAMMOZ model was evaluated in several studies. The aerosol module, ECHAM5-HAM was evaluated extensively by Stier et al., (2005) and the chemistry component, ECHAM5-MOZ by Pozzoli (2007) and Auvray et al., (2007). The modeled aerosol optical depth in Southern Hemisphere was in good agreement when compared with satellite observations (Stier et al., 2005; Pozzoli et al., 2008b). The size distribution, number concentration and optical properties are reproduced well by the coupled model, though the agreement is better near the surface than in the upper troposphere. The annual mean burdens of the aerosol species simulated by ECHAM5-HAMMOZ was more or less similar compared to those simulated by ECHAM5-HAMMOZ model (Pozzoli et al., 2008b). Regional improvements in the sulfate composition over Europe and US was noted with ECHAM5-HAMMOZ, primarily, due to the interactive calculation of OH concentrations in ECHAM5-HAMMOZ compared to the climatological values used in ECHAM5-HAM. Lohman et al., (1999, 2007) evaluated the cloud microphysical variables and a realistic agreement was found between modelled and observed mean liquid water path, CDNC and effective radius.

In Thomas et al., (2010), the relevant parameters such as the DMS flux to the atmosphere, sulfate distribution and cloud microphysical variables were evaluated. The simulated global annual DMS flux to the atmosphere was estimated to be 23.3 Tg(S)/yr and agrees well with the estimates of Boucher et al., (2003) that used the same gas exchange parameterization of Nightingale et al. (2000) and Kettle and Andreae (2000) DMS climatology. The seasonal variation in modelled

$nssSO4^{2-}$ in our baseline simulation (CTRL) over the southern oceans is comparable to those of Gondwe et al. (2003) who estimated a 7-8 times increase in summer DMS emissions compared to winter. The simulated CD effective radii and cloud liquid water path agree closely with satellite data, but, the model seems to overestimate the CDNC over the 30S-60S latitude belt in summer.

Q.3) In section 3.2.1, the Authors state that "The vertically integrated atmospheric liquid water remains constant in all simulations". What are the implications of such hypothesis for the results ?

The constant vertically integrated atmospheric liquid water in our simulations have implications mainly for the cloud microphysical processes. We found that by doubling the DMS emissions we increase, for example, the CDNC's by only 25% and here the main constraining factor is the availability of atmospheric liquid water. Furthermore, the droplet radii and albedos of clouds formed under constant atmospheric liquid water but under different DMS-derived aerosol loadings are expected to follow Twomey effect. We would, of course, get different results when an increase in atmospheric water in a double CO2 scenario is considered in the analysis. But this was not the main motivation of the study.

Q.4) The Authors mention that "The present study would provide useful insights in evaluating [the effect of iron fertilization]". Could they further discuss this and, for example, place their results in the context of proposed experiments in terms of iron fertilization?

Modulating cloud properties via first and second indirect aerosol effects by the introduction of additional aerosols that act as CCN is one of the many methodologies suggested to counteract warming. It is discussed that this can be done either by artificially and mechanically spraying seasalt aerosols in the atmosphere or by stimulating DMS aerosol-cloud feedback by iron fertilization (for example, recent studies by Boyd (2008), Jones et al., (2009), Korhonen et al., (2010), Wingenter et al., (2007), Woodhouse et al., (2008)). However, these studies focus only on a particular part of the DMS aerosol-cloud-climate feedback loop often ignoring the underlying non-linearities in aerosol-cloud interactions. Our study provides estimates of the degree of non-linearity expected in aerosol-cloud interactions. This is discussed in the second paragraph of Section-1 of the revised manuscript-

Minor comments.
Page 15236 l3: diminsh -> diminish ?
Page 15237 l1: what is meant by "cloud top cloud droplet effective radii"?
The reference (Rast et al., JGR, submitted 2008) probably needs to be updated.

The minor comments have been considered and necessary changes are made. Satellite retrievals can only provide information on cloud top droplet effective radius. So, for realistic comparisons with satellite measurements, the model simulated cloud top droplet radius is used.

The reference is updated as follows:

Rast, J., Schultz, M., Aghedo, A., Bey, I., Brasseur, G., Diehl, T.,Esch, M., Ganzeveld, L., Kirchner, I., Kornblueh, L., Rhodin, A., Roeckner, E., Schmidt, H., Schroede, S., Schulzweida, U., Stier, P. and van Noije, T.: Evaluation of the tropospheric chemistry general circulation model ECHAM5--MOZ and its application to the analysis of the interannual variability in tropospheric ozone from 1960--2000 chemical composition of the troposphere for the period 1960--2000 (RETRO), MPI-Report (Reports on Earth System Science), to appear.

References:

Boyd, P. W.: Ranking geo-engineering schemes, Nature Geosciences, 1, 722–724, 2008.

Jones, A., Haywood, J., and Boucher, O.: Climate impacts of geoengineering marine stratocumulus clouds, J. Geophys. Res., 114,2009.

Korhonen, H., Carslaw, K. S., and Romakkaniemi, S.: Enhancement of marine cloud albedo via controlled sea spray injections: a global model study of the influence of emission rates, microphysics and transport, Atmos. Chem. Phys. Discuss., 10, 735–761, 2010.

Wingenter, O. W., Elliot, S. M., and Blake, D. R.: New directions: Enhancing the natural sulphur cycle to slow global warming, Atmos. Environ., 41, 7373–7375, 2007.

Woodhouse, M. T., Mann, G. W., and Carslaw, K. S.: New Directions: The impact of oceanic iron fertilisation on cloud condensation nuclei, Atmos. Env., 42, 5728–5730, 2008.