

Interactive comment on “Geoengineering by stratospheric SO₂ injection: results from the Met Office HadGEM2 climate model and comparison with the Goddard Institute for Space Studies ModelE” by A. Jones et al.

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Received and published: 8 June 2010

Firstly, we would like to thank the referee for their thoughtful comments.

In response to the referee's general comment about the lack of information about the aerosol schemes used, we have now included descriptions of the schemes used in both models (lines 38-47 and 51-58). As for the Particular Remarks:

“1) page 7424, line 1-2: ‘The experimental designs (...) are sufficiently similar for a comparison to be useful’. I agree with that. The results are comparable in that they

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cover similar periods and identical IPCC A1B scenario. But from an aerosol dynamical point of view couldn't the set up be much more dissimilar with one grid box injection versus global injection, and with a modal aerosol model with detailed aerosol dynamics versus a passive tracer experiment?”

We do not understand why the referee assumes that one model uses a detailed aerosol scheme while the other is a “passive tracer experiment”, as nowhere is this stated. As mentioned above, we have now included more information on the aerosols schemes, which explains that both models use modal schemes with treatments of aerosol chemistry and inter-mode transfer etc. However, this is to a degree irrelevant, as our main point is that, given the same amount of SO₂ injection, two different models produce climate changes which are very similar.

“2) page 7425: Why not start result comparison with an overview on the stratospheric aerosol that is obtained? Making inferences on the global stratospheric aerosol from solar radiation incidence is very cryptic for the reader.”

We have now included a comparison of the geoengineering sulphate aerosol distributions in both models (new Section 3.1 and new Fig. 1), showing very similar distributions (although with different magnitudes) in both, despite the different approaches to SO₂ injection.

“3) page 7425, line 10 ff.: Although I see that model runs done under different meteorological conditions may produce complex radiation patterns as to the geoengineered response, I do not really understand what is meant with the ‘double-call to the radiation scheme’. This may require some clarification.”

This section has been corrected and revised to improve clarity, and we have added more text to explain the technique of using a double call to the radiation scheme (lines 116-120).

“4) page 7426, line 7 ff.: The authors note that the model relapse response is both

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quantitatively and qualitatively different. I would be interested in knowing if the authors have an idea as to what the reasons to this differential behaviour might be. If there is a hint in the text it seems unclear to me.

We do not know of a simple explanation for this, and indeed one may not exist - the formulations of the two models are quite different, and they each have their own characteristics. We therefore prefer not to speculate on this matter, but just to draw attention to this behaviour.

"5) page 7426, line 21 ff.: If not compared with ModelE results, it would be interesting to see the HadGEM2 arctic sea ice response."

We have introduced a new section (3.5 and new Fig. 5) which shows the sea-ice changes in HadGEM2.

"6) page 7428, line 3 ff: This appears to be the most important result to the reviewer. Although homogeneous aerosol injection may cancel out on global average temperature increase due to GHG relative to the 1990's, precipitation does vary both on global average and very significantly on a local base. Why not discuss it in more detail?"

We agree that the general similarity in the precipitation responses is an important result, which is why we already devote a complete sub-section to discussing it (Section 3.4).

"7) page 7429, line 20 ff.: Stratospheric aerosol geoengineering may have a sufficiently large potential to defer global warming for a certain period but dissimilarities on the regional scale and among different climate factors are likely. To assess the model inherent uncertainties comparisons between models with standardised experimental set ups are required. Isn't it remarkable then that with a dissimilar model set up one may reach very similar results? Does this point into the direction that further progress also requires the consideration of certain key mechanisms that may be equally unconsidered in these models? Or does it mean that the climate system, and stratospheric

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geoengineering in particular is insensitive relative to model assumptions?"

We agree with the Referee that intercomparisons with a standardised experimental set up would be very useful, to try to answer the questions that the Referee poses at the end of this point. This is the reason we encourage such an intercomparison in the final paragraph of the paper, and we now include a reference to the paper by Kravitz et al. (2010) which proposes such an intercomparison (GeoMIP).

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 7421, 2010.

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