Atmos. Chem. Phys. Discuss., 11, C11664–C11667, 2011
www.atmos-chem-phys-discuss.net/11/C11664/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity to deliberate sea salt seeding of marine clouds – observations and model simulations" *by* K. Alterskjær et al.

Anonymous Referee #2

Received and published: 9 November 2011

Review of "Sensitivity to deliberate sea salt seeding of marine clouds - observations and model simulation"

by K. Alterskjaer, J. E. Kristjansson, and O. Seland.

General Comments

This is a nice analysis of one aspect the sea-spray form of geoengineering. In general the method and results are presented clearly, excepting the aspects raised below. Once these questions are addressed I think the paper should be published.

Specific Comments

1. Page 2, Section 2.1. It would be useful to include a few words to indicate to non-

specialists what the phrase "collection five" means. (I happen to know, but I could imagine other readers being confused by a phrase like "collection five optical depth").

2. Page 2, Section 2.2, paragraph 2. The authors state that the aerosol indirect effect is "evaluated through the AeroCom.....project along with the direct aerosol radiative effect and the simulated aerosol fields" - but then no more, leaving the reader thinking, "Well, so how well did it perform?" (It's like saying your model was "compared with observations" but then not saying how well it compared.) More information as to how the model performed in AeroCom is required here.

3. Page 2, Section 2.2, paragraph 3. It says here that the model uses "year 2000 CMIP5 aerosol fields" - does this mean prescribed aerosol distributions ("fields"), or do you in fact mean aerosol *emissions* ? If they are prescribed distributions, then are they daily/monthly/ annual?

4. Page 3, Section 2.2, final paragraph. I suggest the text beginning "Mainly motivated by..." up to the end of Section 2.2 should be deleted - it's a detailed explanation of the ocean model used, which I don't think is relevant in the context of this paper.

5. Page 3, Section 2.3, paragraph after Eq.(5). It says here "The N_min chosen in equations (4) and (5) is of crucial importance." Is this really true? N_min does of course control the *absolute* magnitude of the susceptibility, which is relevant to the absolute forcing (Wm-2) produced. However, the purpose of the paper is to compare the *relative* susceptibility of clouds in different parts of the ocean. Consequently any value of N_min could be used, and as long as it was used consistently, then the same cloud regions would be shown as being the most or least susceptible. A few words of explanation should be included.

6. Page 3, Section 2.3, Eq.(7). Why is this equation included in the paper - how is it used? Is it how optical depth in the model is calculated for comparison with MODIS optical depth? If so, this should be stated. I didn't spot any reference to Eq.(7) elsewhere in the paper, so why is it there?

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7. Page 4, Section 3.1, paragraph 1. Figure 1(a) shows the MODIS-derived susceptibility function as a global field. However, Section 2.1 states that the CDNC values come from the Quaas et al. data set which is for clouds over ocean. So where to the values over land in Fig. 1(a) come from? The origin of CDNC values over land should be explained in Section 2.1 (Satellite Data).

8. Page 4, Section 3.1, paragraph 1. Towards the end of the paragraph it says "the susceptible areas are closely co-located with areas of large cloud droplet effective radii." How are these effective radii determined? From the model? - then this should be stated here. From a satellite retrieval? - then this should be included in the "Satellite Data" section (2.1). Or is this what Eq.(7) is for? - in which case, explain that, and where the values of LWP in Eq.(7) come from.

9. Page 4, Section 3.1, paragraph 2. Text here says: "...the cloud-weighted susceptibility function (eq.5) is dominated by the cloud fraction rather than by the susceptibility," - that's not obvious to me at all. To me, Fig.2(b) looks a lot more like Fig.1(a) (susceptibility - lots of high values in the tropics) than Fig.2(c) (cloud fraction - high values here are in the mid-to-high latitudes).

10. Page 4, Section 3.1, paragraph 2. The text then goes on to say "..the most susceptible areas in unpolluted regions have a small cloud fraction." This raises two points: (a) How is the reader to judge this statement when there's no map of anthropogenic AOD or equivalent? (b) The most susceptible (red) areas in Fig.2(b) seem to have quite high cloud fractions (white areas in Fig.2(c)), so this statement need clarifying.

11. Page 5, Section 3.3, first full paragraph. I don't understand why the model doesn't "reproduce the signals found off the west coast of Canada from MODIS retrievals". Examination of Figs. 1 & 2 indicates similar values for cloud fraction off the west coast of Canada (north of about 30N) in both model (Fig.2c) and MODIS (Fig.2a) of approximately 0.4-0.6, perhaps higher in NorESM. As for the susceptibility function, in this region it's about 0.15-0.25 in MODIS (Fig.1a) but higher in NorESM at 0.25-0.35 (Fig.1b).

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Interactive Comment

So why is the cloud-weighted susceptibility *lower* in NorESM than in MODIS?

12. Page 6, Section 4.2, second full paragraph. The explanations for why some regions show positive forcing are plausible in their own terms, but I see no evidence to support these ideas in the data presented in the paper. Following sea-salt injection, in the regions of positive forcing near Japan we see an increase in LWP (Fig.7b), a decrease in effective radii (Fig.7d), and increases in column-integrated CDNC (Fig.7c) and a general increase in CDNC in a cross-section (Fig.9c). All of these would be expected to lead to increases in cloud albedo and hence a negative forcing. To support their explanation of positive forcing a figure is required showing the change in CDNC at a particular level which actually shows decreases in CDNC co-located with regions of positive forcing.

13. Page 6, Section 4.2, final paragraph. Figure 8 is introduced here, but then nothing about it is discussed and it is not referred to again. It seems redundant and I suggest it is removed (which will make space for the extra figure suggested in point 13 above).

Technical Corrections

1. Page 2, Section 2.2, paragraph 2. The reference to Abdul-Razzak & Ghan needs reformatting.

2. Figure 3. In this figure, white is used for two purposes: to indicate regions of susceptibility between 0.08 and 0.09, and also to indicate regions of missing data. The figure should be plotted differently to remove this ambiguity.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29527, 2011.