

## ***Interactive comment on “Uncertainty analysis for estimates of the first indirect aerosol effect” by Y. Chen and J. E. Penner***

**Anonymous Referee #1**

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### General Comments

This is a good paper, making a useful contribution to the study of the important area of indirect aerosol effects on climate. My only general comment is that I think the authors need to emphasise more that this study is assessing the uncertainty of the first indirect effect as parameterized in this model, and how this uncertainty is affected by the selected criteria which are themselves model-derived or -dependent. If other models were used, or other criteria chosen (e.g. model resolution, cloud overlap assumptions, internal aerosol mixing), then the uncertainty estimate would probably be different.

### Specific Comments

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(1) Page 4511, Section 2.1, 1st paragraph. The conversion of aerosol mass to number concentration described here implies that the aerosols are treated as an external mixture. If this is the case, it should be stated clearly.

(2) Pages 4511-4512, Section 2.1. This section doesn't explain where the distributions of temperature, water vapour, cloud liquid water content etc. come from. If they come from a GCM, which GCM? What is its resolution? How long were the runs? What sort of boundary-conditions (e.g. pre- industrial or present- day) were used?

(3) Page 4513, line 8. What is the justification for the assumption that the difference caused by the low/high emissions values represent a one-sigma range? Why not some other range?

(4) Page 4513, last line. Why just the maximum value?

(5) Page 4514, 1st paragraph. Does the first sentence refer to aerosol or cloud droplet size distributions? If the former, then it should be made somewhat clearer that, for the purposes of cloud nucleation, all aerosols are assumed to have the same size distribution (land/sea contrasts aside).

(6) Page 4514, 2nd paragraph. Is this saying that the PI aerosol has the same size distribution over land and sea, whereas the anthropogenic aerosols differ? This needs justification (or at least to be stated clearly).

(7) Page 4516, last paragraph. How is cloud liquid water content, a 3-D quantity, derived from liquid water path, a 2-D quantity?

(8) Page 4518, lines 15-16. Does this explain why the model  $r_{eff}$  is generally less than, and often significantly less than, the retrieved values?

(9) Sentence spanning pages 4519 and 4520. This sentence seems redundant, and is somewhat contradicted by the ones following it: it's only over ocean that there's a clear NH/SH difference.

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(10) Page 4521, end of first paragraph. The explanation of experiment MIN\_R given here in fact explains what would probably happen in the real world, with evolving aerosol spectra, but not why the forcing is so low in the model, which is what is required. Figure B1 shows that, beyond a certain limit, the nucleation parameterization fails, which is the explanation for the small forcing. This should be pointed out more clearly.

(11) Page 4524, line 15. See comment (3).

(12) Page 4525, second paragraph. Surely this is well known, and has been so at least since the work of Platnick & Twomey (1994) on cloud susceptibility, which should at least be referenced.

#### Technical Corrections

(a) Page 4510, line 16. Insert "the" after the first word.

(b) Page 4514, line 22. For consistency, aerosol size should be quoted as radius, not diameter (D).

(c) Page 4516, line 24. Insert "humidity" after "relative".

(d) Page 4516, line 29. LWP is, by definition, a 2-D quantity.

(e) Page 4519, line 19. Droplet concentrations at what level?

(f) Page 4519, line 28. It appears to me that "more" would be the correct word in this context, not "larger".

(g) Page 4521, line 2. The experiment name ("MAX\_R") should be included here, and indeed these names should be used throughout to aid the reader.

(h) Page 4524, line 4, and page 4525, line 5. Which is it, 3-D or 2-D?

(i) Page 4525, line 2. Insert something like "via the first indirect effect" after the word "forcing". This is for those who only read the Conclusions....

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(j) Figure 3. Including global means would be useful. Also (Fig. 3d), it would be worth reiterating that indirect forcing is here defined in terms of the change in top-of-atmosphere net radiation.

(k) Figure 4. See correction (e) above.

(l) Figure 5. This figure would benefit from thicker lines being used for the various curves.

(m) Figure 6. Do the different colours mean anything?

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