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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Droplet number prediction uncertainties from CCN: an integrated assessment using observations and a global adjoint model" *by* R. H. Moore et al.

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Response to Reviewer #1

We thank the reviewer for their detailed comments and contributions to the paper. Responses to the italicized reviewer comments are shown below.

General Comments

Literature: The authors seem not to be aware of the very large body of studies on the topic in the recent literature. They fail to discuss at all the manifold works around ground-based and satellite remote sensing. As a start, I suggest the authors read and *discuss the recent overview studies on the term dln N_d / dln N_a by McComiskey and Feingold (Atmos. Chem. Phys. 2012; doi:10.5194/acp-12-1031-2012) and Nakajima and Schulz (What do we know about large-scale changes of aerosols, clouds, and the radiation budget? in Clouds and the Perturbed Climate System. Ernst Strüngmann Forum, edited by J. Heintzenberg and R. J. Charlson, ISBN 978-0-262-01287-4, MIT press, Cambridge, 2009.). On the albedo susceptibility, I suggest e.g., the studies by Bellouin et al. (Atmos. Chem. Phys. Discuss., 2012; http://www.atmos-chem physdiscuss.net/12/20073/2012/acpd-12-20073-2012-discussion.html), Oreopoulos et al. (J. Geophys. Res. 2008, doi 10.1029/2007JD009655) or Quaas et al. (J. Geophys. Res. 2008; doi 10.1029/2007JD008962).*

We thank the reviewer for directing us to these studies. The manuscript was by no means a thorough review of all the studies that use sensitivity calculations, but we have added most of the requested references.

(2) The model evaluation at the sites (Table 3) is far too superficial. So far, these results are in bulk qualified "reasonably" good. Firstly, the numbers need to be made comparable, and secondly, a thorough quantitative evaluation is necessary. From the numbers provided, the usefulness of the model may be questioned at least at stations 3, 7, 12, 19, 20, 21, 29, 33.

Table 3 is not intended to be a thorough model validation, but rather represents the measurement and model inputs to the propagated uncertainty calculation. In some locations, as the reviewer notes, the observed aerosol concentrations vary significantly from the simulated annual mean values possibly due to statiotemporal variability. To explore how sensitive the derived droplet concentration sensitivities are to the model mean, we have run additional simulations with twice and one-half the aerosol concentrations in each grid cell (which represents the extent of the model-observation difference). This analysis shows that aerosol concentration variability in the model introduces an additional, small (≤5%) into the derived N_d uncertainty, but this does not alter the overall conclusions of the analysis. These simulations are described in the newly-added supplementary material.

(3) The uncertainties in their study needs discussion. So far, just one simulation is conducted with two globally constant values of the updraft for land and ocean, respectively. How sensitive are the result to this oversimplification? The model uses the simulated size distributions and chemical compositions to compute Na at each time step and grid-point. Also for this quantity, large sensitivity of the results is expected.

It is true that these simplifications contribute additional uncertainty in computing cloud droplet number concentrations, and we have now made note of this in the text that the present analysis does not account for these additional sources of uncertainty.

(4) The cloud albedo definition seems wrong, or at least the authors need to justify why they believe that Twomey's formula (their eq. 1) should apply to their unconventional definition.

Equation 1 has been modified as suggested by the reviewer and all albedo sensitivity calculations have been recomputed.

Minor comments

The term "normalised sensitivity" (e.g., dN_d/dN_a / (N_d/N_a)) is unusual. One would rather call this relative or logarithmic sensitivity (dln N_d / dln N_a).

"normalised sensitivity" is standard terminology in other fields (e.g., air quality), and we prefer to keep it.

$p20493$

l13: units?

This has been fixed.

l19: The qualification "reasonably well" needs quantitative corroboration. As it stands, the comparison to the in situ observations appears almost useless.

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Although the reviewer's point is well taken, the comparison is not useless, as it provides an upper/lower bound of discrepancy between the two quantities, and is used for examining the robustness of the droplet sensitivity calculations in each location.

$p20495$

l22: The difference between all-sky and clear-sky albedo is not the cloud albedo. The quantity the authors compute is the cloud radiative effect normalised by the incident radiation. For cloud fraction f, in a grid box, the all-sky albedo is the weighted sum of the cloud- and clear-sky-albedo:

A_all-sky = f A_cloud + (1 – f) A_clear

A_cloud = (A_all-sky – A_clear) / f + A_clear

As correctly stated in l26, nevertheless the quantity is useful, it is just unconventional and thus more difficult to interpret.

This is an excellent point, and we thank the reviewer for bringing this up. We have modified Equation 1 and recomputed the results using the reviewer's suggestions. We include a plot of the product of (dA_cloud/dlnNa)(f) in Figure 3d to present this quantity while reporting f and (dA_cloud/dlnNa) separately in Table 3.

$p20496$

l3: It is important to note that this definition of cloud albedo is different from the one discussed above.

This has been changed.

l9: Nd to be used in Eq. 1 in this approach is from the simulation, I assume?

Correct.

$p20497$

l3: Why "overprediction uncertainty" and not just "overprediction"?

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This has been changed per the reviewer's suggestion.

Tab. 2: How are the uncertainty ranges defined?

Closure overpredictions reported in the literature are not described in the same way; therefore, there is no single definition for the ranges reported in Table 2. Consequently, we used our judgment in interpreting the closure results described in each study, to use error metrics as consistently as possible. In many cases, this means that the numbers represent an average and standard deviation, while in some, the numbers represent the reported mean and total range of variability. This has been clarified in the text.

Tab. 3: This table should show the simulated CCN range for the observed s range. It should also list smax.

We have added the simulated s_{max} to Table 3. We do not feel that running additional simulations to find CCN concentrations over the range of experimental supersaturations informs the analysis of derived droplet and albedo sensitivities.

Caption: Albedo sensitivity should read dA/dNa. Means are provided only for the model, and standard deviations, only for the multi-station results. The data source for the satellite albedo should be stated. Why is this also only a mean value?

The caption has been fixed and a reference to the data sources described in the text has been added. Means and standard deviations are reported across the 35 different study locations and across all model grid cells. Standard deviations are not reported for the individual sites as in many locations, the sample area consists of only one or two model grid cells, while other locations (particular airborne studies) have larger sample areas.

Fig. 1: Which level is shown?

All simulations are at the surface.

Fig. 2: How are the data sampled? Is this one time step globally, or several timesteps

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for a specific region?

The data are the global annual mean values shown spatially in Figure 1, and each data point reflects one grid cell. This has been clarified in the caption.

Fig. 3: It would be useful to show the term dA/dN_d.

Done.

Fig. 4: Also the intermediate term, dln N_d /dln N

dln N_d / dln Na is already included as Figure 1c.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20483, 2012.

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