Review of **"A sensitivity study of radiative fluxes at the top of the atmosphere to cloud-microphysics and aerosol parameters in the Community Atmosphere Model CAM5"**, by Zhao et al., submitted to *Atmospheric Chemistry and Physics*

General comments

The manuscript uses Sensitivity Analysis (SA) to characterize the impact of perturbations in internal and external model parameters on top of atmosphere radiative fluxes and other key quantities simulated by CAM5. Key findings are that the cloud ice to snow autoconversion size threshold Dcs contributes most substantially to the variance, and that interactions between parameters is relatively unimportant in explaining variance, globally and over most regions. This is a very well-written paper that addresses a timely and important topic. The methodology and presentation of results seems robust. The main issues are the need to discuss these results in the broader context of other similar studies and in order to make this work accessible to a wide readership. Specific suggestions in this regard are given below. Overall, my recommendation is to accept pending minor revisions.

Specific scientific comments

1. As alluded to above, I think this paper would benefit greatly by discussing this approach and results in the context of other recent studies using sensitivity analysis (or "uncertainty quantification", which is a bit of a misnomer in my opinion) applied to climate modeling. There is very brief mention of one-at-a-time sensitivity tests in the introduction, and a single reference (Saltelli and Annonia, 2010), but nothing else. More background on this problem should be given here. The authors do mention some other SA studies applied to climate modeling in the last section (first paragraph on p. 12156); I suggest moving this to the introduction and expanding upon it.

A related point is that it would be helpful to discuss and compare/contrast results here with these previous SA studies. For example, a key finding of this study is that parameter interaction is relatively unimportant in explaining the variance. I would not have necessarily expected this result a priori. How does this compare with other studies, such as Lee et al. (2012), which is cited by the authors, who used large-dimensional parameter space SA to look at sensitivity to aerosol parameters in a climate model? The current study is actually quite similar to Lee et al. (2012) in many regards, so it is surprising that the authors only mention the paper very briefly in the last section with regard to SA.

2. Similar to comment #1, I would like to see some discussion of the advantages and disadvantages of the GLM method compared with other multi-dimensional SA methods that have been developed and applied to climate modeling, such as the Gaussian emulator (Lee et al. 2012). Other emulator-based approaches include, for example, polynomial chaos expansion (though I'm not aware if this has actually been applied to climate model SA). Overall, at least some general discussion and summary of the various statistical techniques for sensitivity analysis of large-dimensional parameter spaces is warranted.

3. The authors point out that variability of FNET across the ensemble is much larger than uncertainty in FNET in the IPCC AR5 report for aerosol radiative forcing. However, it should be pointed out that large sensitivity of quantities like FNET to internal and external model parameters does not necessarily imply

large sensitivity of the *change* in the quantity, e.g., ΔFNET, either from aerosols or greenhouse gases, to parameters.

4. The sensitivity of clear-sky net radiation (FNETC) to the parameter dcs is rather interesting, given that dcs is an ice cloud microphysical parameter. The authors primarily ascribe this sensitivity to surface temperature feedback, that may in turn also impact atmospheric water vapor content (p. 12146-12147). However, SST is fixed in these simulations, which implies that this feedback is limited (i.e., over land). I'm wondering if a more likely explanation for this sensitivity is the impact of ice clouds on mid- and upper-tropospheric water vapor content and hence outgoing LW (clear-sky), either directly by ice diffusional growth and sedimentation or indirectly by modifying vertical transport of water vapor via resolved dynamics and/or parameterized convection/vertical mixing. Modification of dcs should have a large impact on vapor growth and hence uptake of water vapor, by modifying ice cloud characteristics.

5. Why are several figures included as a supplement? I don't see any clear rationale for doing this – can't these figures simply be included with the rest of the paper?

Minor/technical comments

1. p. 12137, line 26. Suggest rewording "vary extremely" to "have extreme heterogeneity".
2. p. 12138, line 1. Perhaps this is what the authors mean by "computational limitation", but I would argue that a primary limitation in the ability of GCMs to treat clouds (and aerosols) is their inability to resolve the cloud dynamics.
3. p. 12139, line 5. I think the authors mean hundreds or thousands of parameter values for several different parameters, not for a single parameter? This could be clarified.
4. p. 12139, line 23. What particular version of CAM5 was used for this study?
5. p. 12140, line 17. I would suggest not using the term "current version" here, because this could be confusing for future readers. The "current version" of CAM5 is always changing.
6. p. 12143. The variables n and N are not defined in this equation.
7. p. 12145, line 1-3. I think this sentence is confusing – could the authors please clarify?
8. p. 12146, line 17. Does "increases with dcs" mean that FNET increases with increasing dcs?
9. p. 12150, line 12. Suggest adding "the" before "following".
10. p. 12150, and elsewhere. Suggesting changing "R square" to "R^2".
11. p. 12150, line 20. I don't understand this sentence: "Therefore, they have less interest in terms of investigating variance sources." Who is "they"?
12. p. 12151, lines 22-26. This sentence is very long and could be shortened.
13. p. 12151, line 28. Change "the convections are" to "convection is".
14. p. 12152, line 6. The authors ascribe changes on CF from dust emission to semi-direct effect of dust. What about indirect effect via ice nuclei?
15. p. 12152, line 26. Same comment as above in #14 but for BC. Or is ice nucleation on BC turned off? Can BC serve as CCN in CAM5 if internally mixed?
16. p. 12154, line 24. Change "falling speed" to "fallspeed".
17. p. 12164, Table 1. There are some corrections needed in the table. First, the range given for cdnl is not correct, I think the maximum value should be 10 cm^{-3} instead of 10^7 cm^{-3}. Second, units for the

fallspeed parameter for cloud ice should be in s^{-1}, not m^{-1} s^{-1}. I would also suggest drawing a line in the table separating internal from external parameters, to help distinguish them in the table.

18. p. 12165, Table 2. I would suggest putting the information in the second footnote in the table caption, as this is essential information for understanding the table.