

We thank the two anonymous referees for their valuable comments and constructive suggestions on the manuscript. Below, we explain how the comments and suggestions are addressed and make note of the revisions we made in the manuscript.

Anonymous Referee #1

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 D_{cs} 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.

We thank the reviewer for a detailed review. We have added the discussion of other similar studies as we replied to the specific comments below. Both text and figures are revised as the reviewer suggested.

Specific comments:

- *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.*

More background and reference about the OAT approach are provided now in the text as “The most widely used SA approach is to conduct “one-at-a-time” (OAT) sensitivity tests that systematically investigate departures of model behavior from the baseline simulation by varying one parameter at a time (e.g., Gao et al., 1996; Lohmann and Ferrachat, 2010; Li et al., 2011). Lohmann and Ferrachat (2010) used the OTA SA method to investigate the impact of important tunable parameters associated with ice cloud optical properties and convective and stratiform clouds on the present-day climate and aerosol

effect in a global climate model. They concluded that tuning of these parameters has a negligible influence on the anthropogenic aerosol effect. However, OAT tests can only test limited number of parameters at the same time and consider only a small fraction of the total parameter uncertainty space. They also cannot take parameter interactions into account (Saltelli and Annonia, 2010). The OAT approach has been criticized for its failure to explore the full parameter space for highly nonlinear models (e.g., Bastidas et al., 2006; Rosero et al., 2010; Lee et al., 2012).”

- 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.*

Some discussion about previous SA studies is moved to the introduction now as “Lee et al. (2012) used the Latin hypercube sampling (LHS) method and applied a Gaussian process emulation technique in a global chemical transport model to explore the parameter space and quantify uncertainty in simulating CCN concentrations. They concluded that CCN concentrations in model simulations are sensitive only to emission parameters in polluted regions but to uncertainties in parameters associated with model processes in all other regions. In a follow-on study, Lee et al. (2013) extended the previous work to examine 28 uncertain model parameters to more fully assess the magnitude and causes of uncertainty in modeling CCN.”

More discussion and comparison between previous SA studies and this study are added in conclusion as “This is generally consistent with the findings by Lee et al. (2012 and 2013). They used a different SA approach (i.e., Gaussian emulation technique and Monte

Carlo sampling approach) and found that the effects of emission parameters (such as sea salt) are normally localized to the place where that parameter is acting while the effects of model parameters (such as the wet scavenging parameter) have non-local impacts.”, “The SA framework used in this study is designed with the capability to quantify the interactions among the parameters. However, the analysis indicates a relatively small contribution of interaction effects among the selected 16 parameters to the overall variation of global mean FNET (~3%) and to the FNET variation over most regions of globe (<15%). This relatively small interaction effect may be partially due to the relatively large group of selected parameters. Lee et al. (2012) found the interaction effect among the 8 selected parameters contributes to 30-50% of the overall uncertainty in modeling CCN. However, Lee et al. (2013) extended the 8 parameters to 28 and found the contribution of interaction effects significantly reduces to <20%. This may indicate that the relative contribution of the parameter interaction to the overall uncertainty will be generally small when a large group of parameters are taken into SA.” and “This study efficiently investigates 16 uncertainty parameters simultaneously compared to the OTA SA approach which generally examines a limited number of parameters (e.g., 4 parameters in Lohmann and Ferrachat (2010)). The relative contribution of parameter interaction to the overall uncertainty is also quantified. Compared to the OTA SA approach, the more comprehensive approach used in this study not only estimates the contribution of each parameter to model sensitivity but also provides its statistical significance, which is rarely obtained by the OTA SA approach due to the limited sampled space of parameter uncertainty. Therefore, this study highlights the benefits of using a more comprehensive SA approach to understand the parametric uncertainties in climate models. Lee et al. (2012 and 2013) focused primarily on the contribution of the aerosol-CCN related parameters to uncertainty in modeling CCN. Lee et al. (2013) admitted that cloud-related parameters might also play an important role in determining aerosol indirect effects. The current study found that emission and aerosol related parameters generally have smaller impacts on the global mean FNET than cloud microphysics related parameters. Although this finding may be model-dependent, it highlights the importance of including cloud related parameters in understanding uncertainties in modeling aerosol radiative forcing (direct and indirect).”

- *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.*

Lee et al. (2012) used Latin hypercube sampling (LHS) to explore the parameter space. In our study, we adopt the quasi Monte Carlo sampling (QMC), which is superior to the Latin hypercube sampling (LHS) method for high-dimensionality problems as it does not have gaps and clumping issues (see Hou et al. 2012). The discussion is now added in the text “Lee et al. (2012) used the Latin hypercube sampling (LHS) method and applied a Gaussian process emulation technique in a global chemical transport model to explore the parameter space and quantify uncertainty in simulating CCN concentrations.” and “A quasi-Monte Carlo (QMC) sampling approach guarantees good dispersion between samples (Caflisch, 1998) and therefore is adopted in this study. QMC sampling can achieve good uniformity even in higher-dimensional projections by filling gaps and avoiding clumps in the sampling points, achieving better performance than MC and LHS in general (Wang and Sloan, 2008; Hou et al., 2012).”

In addition to the different choices of sampling approaches, people may adopt various sensitivity evaluation techniques, for example, multivariate adaptive regression splines (MARS), GLM, Morris method, Sobol’ method, and so on to separate the main, interaction, and high-order effects of input parameters (Friedman 1991; Morris 1991; Sobol’ 1993). Among these methods, both Sobol’ method and GLM can provide quantitative measures of parameter sensitivity in terms of output variances that can be explained/fitted by the linear, interaction, and high-order terms of input variables and integrated with parameter reduction/selection techniques if necessary. Polynomial chaos (PC) expansion approximates the output variable as a function of polynomials of input variables, when used for sensitivity analysis. It is similar to Sobol/GLM method and can

look at linear, quadratic, and higher-order effects, although the collocation sampling points are designed a little differently compared to LHS/QMC. Gaussian process emulation and PC are more appropriate to be used for model optimization and parameter estimation when observations of output variables are available. Some discussion is added now in the text “*There are various sensitivity evaluation techniques, for example, multivariate adaptive regression splines (MARS), generalized linear model (GLM), Morris method, and Sobol’ method. They can be used to separate the main, interaction, and high-order effects of input parameters (Friedman 1991; Morris 1991; Sobol’ 1993). Among these methods, GLM method and Sobol’ method can provide quantitative measures of parameter sensitivity in terms of output variances that can be explained/fitted by the linear, interaction, and high-order terms of input variables and integrated with parameter reduction/selection techniques if necessary. The GLM method is used for sensitivity analysis in this study.*”

- ***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., $\Delta FNET$, either from aerosols or greenhouse gases, to parameters.***

Yes, we agree. We clarify it in the conclusion and discussion “*The analysis indicates a change in FNET between -11.7 and 1.6 $W m^{-2}$ (compared to FNET from the standard CAM5 simulation). This is much larger than the range of -1.8 to -0.1 $W m^{-2}$ given for uncertainty in aerosol radiative forcing in the IPCC AR4 report. However, it should be noted that this study does not show the sensitivity of radiative forcing (i.e., $\Delta FNET$ from pre-industrial to present-day) to the selected parameters, which is worthy of further investigation*”

- ***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.

It is true that the change in ice clouds due to *dcs* can impact the mid- and upper-tropospheric water vapor budget through the water vapor deposition on ice crystals, affecting ice particle size and thus ice sedimentation, or by modifying the vertical transport of water vapor due to the ice cloud feedbacks on the dynamics. The net effect through these interactions on water vapor is shown in Fig. 4. Our results showed that water vapor content increases with *dcs*. The increase of water vapor will reduce outgoing LW and thus increase LWFNETC (less negative), while our results (Fig. 3) showed LWFNETC decreases (more negative) with *dcs*. Therefore, the decrease of LWFNETC (increase outgoing LW) must result from the increase of surface temperature. Although the SST is prescribed in our simulations, temperature feedback over the land is still important. Our results (Fig. 6 and 9) indeed show that the main variation of FNETC due to *dcs* is over the land (particularly over the Arctic). Now it is clarified in the section 3.1 “*The change in TSK mainly occurs over land areas, since the sea surface temperature (SST) is prescribed in this study (will be discussed in section 3.2). The change in ice clouds due to dcs can impact the mid- and upper-tropospheric water vapor budget through various ways such as affecting the water vapor deposition on ice crystals, affecting ice particle size and thus ice sedimentation, or by modifying the vertical transport of water vapor due to ice cloud feedbacks on dynamics. The net effect of these interactions leads to increasing WVP with increasing dcs.*”, and “*Although the increase of WVP with increasing dcs will reduce the outgoing LW and thus increase LWFNETC (less negative), the increase of TSK with dcs reduces LWFNETC (more negative) through increasing outgoing LW radiation under clear-sky (Figure 4). The overall LWFNETC*

change is dominated by the TSK change over the land (will be discussed in section 3.2)."

- **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?***

Now Figures S1 and S5 are put into the paper as Figure 9 and Figure 12, respectively. The text is modified accordingly. The other three figures are only mentioned briefly in the text, so they remain in supplementary materials.

Minor/technical comments:

- **1. *p. 12137, line 26. Suggest rewording "vary extremely" to "have extreme heterogeneity".***

Revised as suggested.

- **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.***

It is clarified now in the text as *"To date, global climate models cannot fully treat details of the physical processes governing cloud and aerosol formation, lifetime, and radiative effects due to insufficient physical understanding or relatively coarse spatial resolution (due to computational limitation) that cannot resolve 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.***

It means hundreds or thousands of values for a single parameter. For example, in this study, 256 values are sampled for each parameter. It is clarified now in the text as *"A more comprehensive approach is to populate the statistical distribution of model outputs by sampling hundreds or thousands of possible values for each parameter."*

- **4. *p. 12139, line 23. What particular version of CAM5 was used for this study?***

It is CAM5.1.02. It is clarified now in the text.

- **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.**

It is clarified now in the text as “*the version of CAM5 used in this study*”

- **6. p. 12143. The variables n and N are not defined in this equation.**

We defined them in the text now “ *n is the number of input parameters/factors, and N denotes the normal/Gaussian distribution with mean and variance in the parenthesis.*”

- **7. p. 12145, line 1-3. I think this sentence is confusing – could the authors please clarify?**

It is clarified now in the text as “*These 256 simulations are equally grouped into 8 sub-ranges (from small to large values) for each input parameter (i.e., 32 values are averaged in each sub-range) to rule out effects from the perturbation of other parameters.*”

- **8. p. 12146, line 17. Does “increases with dcs ” mean that $FNET$ increases with increasing dcs ?**

Yes. It is clarified now in the text as “ *$FNET$ increases with increasing dcs and sol_factic* ”.

- **9. p. 12150, line 12. Suggest adding “the” before “following”.**

It is added.

- **10. p. 12150, and elsewhere. Suggesting changing “R square” to “R²”.**

Corrected.

- **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”?**

It is clarified now the text “*Therefore, these regions are less interesting for the characterization of variance sources.*”

- **12. p. 12151, lines 22-26. This sentence is very long and could be shortened.**

The sentence is now separated into two sentences in the text as “*The w_{submin} impact is mainly through its impact on LWP and thus CF by changing cloud droplet number concentrations (i.e., aerosol indirect effects). The impact is mainly over the regions where there are frequent occurrences of liquid-containing clouds (e.g., along the North Pacific and Atlantic storm tracks) and sufficient amount of CCN (see Figure S3 in the supplementary materials).*”

- **13. p. 12151, line 28. Change “the convections are” to “convection is”.**

Corrected.

- **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?**

Here, we were discussing about the effect of parameter *refindex_dust_sw* (determining dust absorptivity) instead of dust emission. Therefore, varying this parameter does not significantly change the ice nuclei. This has been clarified.

- **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?**

BC doesn't serve as IN but does serve as CCN in CAM5. It is corrected now in the text “*The $emis_{BC}$ also affects CF through semi-direct and indirect effect of BC.*”

- **16. p. 12154, line 24. Change “falling speed” to “fallspeed”.**

Corrected.

- **17. p. 12164, Table 1. There are some corrections needed in the table. First, the range given for cd_{nl} 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 $\text{m}^{-1} \text{ s}^{-1}$.

Thanks for checking units. The units are corrected now.

- *I would also suggest drawing a line in the table separating internal from external parameters, to help distinguish them in the table.*

Now, the caption of Table 1 is added with “*The top 8 parameters are defined as internal parameters and the rest are defined as external parameters.*”.

- *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.*

The second footnote of Table 2 is moved into the caption.

Anonymous Referee #2

General comments:

- *This manuscript uses an objective method to analyze a suite of common adjustable parameters in a community General Circulation Model. The analysis provides some useful information on what parameters define sensitivity of the top of the atmosphere radiation. The method is interesting, though could be a bit better explained, and the study and conclusions should be publishable in Atmospheric Chemistry and Physics with some important clarifications and corrections.*

We thank the reviewer for a detailed review. Both text and figures are revised as the reviewer suggested.

- *I have some concerns over the method. The analysis focuses on what increases variance the most, or what parameters explain variance spread, but only for the parameters you have chosen. If something else is more important, you would miss that, correct?*

We agree with the reviewer that there may be other model parameters important to the radiation fluxes at the top of the atmosphere. However, we have stated in the text “*While many parameters likely contribute to uncertainties in CAM5, this study focuses on 16 parameters related to cloud microphysical processes and aerosol physics and chemistry processes including cloud ice microphysics, cloud droplet activation, aerosol wet scavenging, solar radiation absorption by dust, and emission fluxes and size distributions (see the descriptions shown in Table 1). These 16 uncertain parameters were identified by model developers of CAM5 and also agree with previous studies (R. C. Easter, S. J. Ghan, and H. Morrison, personal communication, 2012).*” In addition, “*Other parameters may also be important for radiative fluxes but they are generally less uncertain and so are not examined here.*” and “*The perturbation ranges (from minimum to maximum) of these 16 parameters (shown in Table 1) reflect our best knowledge of parameter uncertainties in aerosol (R. C. Easter and S. J. Ghan, personal communication, 2012) and cloud microphysics parameterizations (H. Morrison, personal communication, 2012) in CAM5.*”

- *Also, I worry about short runs: there is not going to be statistical significance in the Arctic with 4 year runs: the variance of TOA radiative fluxes with variable SSTs is pretty high. I assume your significance tests account for the variance in the fields? This was not clear to me, and not clear if the 4 years are sufficient to tell much.*

We agree that the variance of TOA radiative fluxes with variable SSTs may be high. However, in this study, the 4-year run average is taken as a single sample for statistical analysis. We did not look into the individual year of the 4-year run for statistical analysis. As we described in the text, for each set of parameters, the simulation is run for 5-years and the average of the last 4-years of simulation is taken as one sample experiment. We conducted a total of 256 experiments and thus had 256 samples for the statistical analysis. Therefore, we believe 256 samples should be enough to conduct a statistical analysis. It is further clarified now in the text “*Simulations are run for 2000-2004. The average of the final four years (2001-2004) of the simulation is considered as one sample experiment. A total of 256 experiments are conducted, as described in section 2.3.2, and the statistical analysis is based on these 256 experiments.*”

- *Also: the co-authors who are native English speakers should re-read this for grammar. There are some consistent mistakes in the text. I have noted some of the problems with plural nouns, but there are others.*

The English writing has been improved by native English speakers. Thanks.

Specific comments:

- *Page 12136, Line 24: You should make more use of the LW v.s. SW results, since they help tell about what kind of clouds and sensitivity of them.*

We agree that it is interesting to look at both LW and SW terms. The changes in LW and SW components of FNETC and CF due to parameter perturbations have already been shown in Figure 3, and discussed in section 3.1. In response to the reviewer’s comment, we now bring up the discussion of the LW and SW portion of FNETC and CF to the front with Figure 1. More discussion about the sensitivity of LW and SW portion of CF has been added into section 3.1 and the conclusion section (please also see our response to the comments below).

- ***Page 12137, Line 15: Clouds and aerosols: should be plural***

Corrected.

- ***Page 12138, Line 1: Limitations: plural***

Corrected.

- ***Page 12140, Line 7: treats: plural***

Corrected.

- ***Page 12140, Line 10: Awkward: do not mention 'diagnosed' in the sentence. I would not say diagnosed for convection in cam5: number concentrations are prescribed based on an assumed size***

It is revised now in the text “*In the stratiform cloud microphysics parameterization, mass and number concentrations of cloud droplets and ice crystals are predicted, while those of rain and snow are diagnosed*”

- ***Page 12140, Line 27: Probably better to cite Neale et al 2010, NCAR technical note. R. B. Neale et al. Description of the NCAR Community Atmosphere Model (CAM5.0). Technical Report NCAR/TN-486+STR, National Center for Atmospheric Research, Boulder, CO, USA, 2010.***

Corrected.

- ***Page 12141, Line 8: 4 years with varying SST is going to be a bit hard to get statistical significance for FNET. do you analyze what sigma is? If not, you need to.***

As we clarified above, in this study, the 4-year run average is taken as a single sample for statistical analysis. We did not look into the individual years of the 4-year run for statistical analysis. Please see our response to the comments above.

- ***Page 12141, Line 14: Refer to personal communication below.***

Added.

- **Page 12144, Line 7: are zero (plural)**

Corrected.

- **Page 12144, Line 22: Sorry to be slow, but the method is a bit obtuse, and could use some more description. You are building a regression model or series of models to reconstruct FNET? Where does the 'variance' part come in? Please explain in words. Also, what do you use to determine significance? If FNET with a perturbation is significantly different than FNET in the base case? How does variance in the base case factor in? At all? Or are you just looking at contributions to spread in your ensemble? What about parameters that are out of sample?**

In GLM and other similar sensitivity analyses (e.g., Sobol' method), we assume that the output variable (e.g., FNET) is a function of input variables. FNET is calculated for each combination of input parameter sets, therefore the ensemble sample sets are corresponding to an output ensemble. The overall variance of the FNET outputs is called the total variance, due to variability of input parameters. To evaluate how significant a parameter is, we look at how much of the FNET total variance can be explained/fitted using the corresponding parameter. Section 2.3.3 has some detailed mathematical explanation of the GLM model used to link input and output variables. The contribution of each parameter in the overall spread (e.g., FNET total variance) is a reasonable quantitative measure of parameter significance. Note that we cannot measure the significance of a parameter if it is not sampled (e.g, if it is fixed during FNET calculations). We now add more discussion about the sensitivity evaluation methods in section 2.3.3 *“There are various sensitivity evaluation techniques, for example, multivariate adaptive regression splines (MARS), generalized linear model (GLM), Morris method, and Sobol' method. They can be used to separate the main, interaction, and high-order effects of input parameters (Friedman 1991; Morris 1991; Sobol' 1993). Among these methods, GLM method and Sobol' method can provide quantitative measures of parameter sensitivity in terms of output variances that can be explained/fitted by the linear, interaction, and high-order terms of input variables and integrated with*

parameter reduction/selection techniques if necessary. The GLM method is used for sensitivity analysis in this study.”

- ***Page 12145, Line 21: It would also be interesting to look at LW and SW components of each FNET, FNETC AND CF***

We agree that it is interesting to look at both LW and SW terms. The changes in LW and SW components of FNETC and CF due to parameter perturbations have already been shown in Figure 3, and discussed in section 3.1. In response to the reviewer’s comment, we now bring up the discussion of the LW and SW portion of FNETC and CF to the front with Figure 1. More discussion about the sensitivity of LW and SW portion of CF has been added into section 3.1 and the conclusion section (please also see our response to the comments below).

- ***Page 12145, Line 25: Unclear: "relatively insignificant....perturbation."***

It is clarified now in the text as “*When a P-value estimated from the GLM for an input parameter is larger than a significance level of 95%, i.e., 0.05, it indicates that this input parameter is relatively insignificant to the overall variance of response variable. It should be noted that the significance estimation of an input parameter is also dependent on the assigned perturbation range of this input parameter.*”

- ***Page 12145, Line 29: Plural: parameter effects and interactions.***

Corrected.

- ***Page 12146, Line 1: predicts well***

Corrected.

- ***Page 12146, Line 3: So this is variance within the parameter set? You are not trying to explain all the possible variance in FNET in CAM. Clarify,***

The total variance of output (e.g., FNET) is due to variations in input parameters. The relationships between inputs and outputs are naturally nonlinear. Such relationships are assumed polynomial with linear/quadratic/interaction terms in sensitivity analyses to

facilitate testing of the significances of the corresponding linear/quadratic/interaction effects. The polynomial assumption is inappropriate if the fitted variance is too small (i.e., R^2 is close to 0). In this study, the R^2 are very close to 1, which means most of the output total variance can be explained/fitted by the input variables. It is clarified now in the text *“The GLM assumes the polynomial relationships between the output (e.g., FNET) variance and input parameter variations with linear, quadratic, and interaction terms in sensitivity analyses to facilitate testing of the significances of the corresponding linear/quadratic/interaction effects. The polynomial assumption is inappropriate if the fitted variance is too small (i.e., coefficients of determination (R^2) is close to 0).”*, *“The GLM is able to predict the values of FNET, FNETC, and CF simulated by CAM5 with an R^2 of 0.98-0.99, which indicates that 98-99% of the variance of CAM5 simulated FNET, FNETC, and CF is explained by the GLM. This confirms that the assumption of the polynomial relationships between inputs and outputs in the GLM is valid.”*, and *“In general, the GLM models can well predict the FNET, FNETC, and CF variance over most regions at latitudes lower than 70° . High R^2 values of >0.9 indicate that most of the output total variance can be explained by the input parameters over these regions. The regions with relatively lower R^2 values (0.5~0.8), e.g., higher latitudes and Australia, generally also have smaller variance of FNET, FNETC, and CF (Figure 6). Therefore, these regions are less interesting for the characterization of sources of variance”*

- **Page 12146, Line 24: Good. Should mention this earlier: using SW and LW components, and regression v. LWP.**

It is mentioned earlier now with Figure 1.

- **Page 12147, Line 10: Can you explain the "surface temperature feedback" statement?**

This part of the text is clarified based on the other reviewer's comment, which has been revised to *“The change in TSK mainly occurs over land areas, since the SST is prescribed in this study (will be discussed in section 3.2). The change in ice clouds due to dcs can impact the mid- and upper-tropospheric water vapor budget through various ways such as affecting the water vapor deposition on ice crystals, affecting ice particle size and thus*

ice sedimentation, or by modifying the vertical transport of water vapor due to ice cloud feedbacks on dynamics. The net effect of these interactions leads to increasing WVP with increasing dcs. The perturbation of dcs significantly affects LWFNETC variance with contribution of 86%, but has negligible impact on SWFNETC. Although the increase of WVP with increasing dcs will reduce the outgoing LW and thus increase LWFNETC (less negative), the increase of TSK with dcs reduces LWFNETC (more negative) through increasing outgoing LW radiation under clear-sky (Figure 4). The overall LWFNETC change is dominated by the TSK change over the land (this will be discussed in section 3.2)."

- ***Page 12147, Line 13: in CAM5 development***

Corrected.

- ***Page 112148, Line 7: Why do fall speed parameters for ice/ snow affect SWCF so much? What does 'mixed phase processes' mean?***

It is explained in the text now *“Increasing ai and as reduces the IWP (through ice and snow sedimentation) and the LWP (through microphysical processes in mixed-phase clouds, e.g., Bergeron-Findeisen process by falling ice and snow), weakens the LWCF and SWCF at a similar magnitude, and thus results in a relatively small impact on CF and FNET.”* The “mixed phase processes” means mixed-phase cloud microphysical processes. It is clarified now in the text.

- ***Page 12150, Line 12: Do you mean spread among the simulations, or variance of the 4 years? I worry that 4 years is not long enough to generate statistics.***

As we clarified above, in this study, the 4-year run average is taken as a single sample for statistical analysis. We did not look into the individual year of the 4-year run for statistical analysis. Please see our response to the comments above.

- ***Page 12152, Line 22: Continents (plural) in this paragraph.***

Corrected.

- ***Page 12153, Line 14: Maybe all emissions components could be added to one figure, and published in the main text as a new figure.***

Now Figure S5 is put into the paper as Figure 12. The corresponding text is modified.

- ***Page 12154, Line 17: But the effect is combined anyway: the FNET variance is because of the variables, so they better reproduce it.***

Thanks for the comment.

- ***Page 12154, Line 25: Summarize LW v. SW. Page 12156, Line 28: Comment on LW and SW please. Which is more sensitive? Is it LW because of DCS?***

The LW portion of CF is more sensitive to the cloud microphysics related parameters compared to the SW portion. It is mainly due to the impact of *dcs*. More discussion about the sensitivity of LW and SW portion of CF is added into section 3.1 and section of conclusions as “*This may indicate that the changes in dcs mainly affect high clouds, which generally have much larger impact on LWCF than on SWCF.*”, “*The impact of wsubmin on LWCF is negligible. This may indicate that changes in wsubmin mainly affect low clouds that generally have much larger impact on SWCF rather than LWCF.*”, “*In general, the changes of ai and as have comparable impact on CF; however, ai has a larger impact on high clouds and hence on LWCF and a smaller impact on low clouds and hence on SWCF than as.*”, “*The variance of the LW and SW components of FNETC and CF is also analyzed. The results show that the global mean FNET variance is dominated by the CF variance with the assigned parameter ranges. Most selected cloud microphysics and emission related parameters are found to have statistically significant impacts on the global mean FNET. Cloud microphysics related parameters significantly affect both high and low clouds and hence CF. dcs mainly affects high clouds and hence CF through LWCF. ai has a larger impact on high clouds and hence LWCF and a smaller impact on low clouds and hence SWCF than as. cdnl mainly affects low clouds and hence SWCF.*”, and “*Overall, these four cloud microphysics related parameters all have larger impact on high clouds and hence LWCF.*”