Response to Referee 1

We thank the referee for his/her valuable comments and suggestions. The responses to your comments are below each comment.

The paper presents an exploration of the impact of perturbations to 5 parameters within the cloud scheme in ECHAM5 on cloud amount, precipitation and top of atmosphere radiative balance. The analysis is extended to consider the impact of these parameter changes on radiative forcings attributable to anthropogenic aerosol effects. The language and the structure in the paper is excellently written, and the paper flows and is easy to read. The results presented in the paper represent an extensive body of work. The weakness of the current draft however is that the implication of the results is not clearly identified and hence the scientific relevance is not clear to the reader. This needs to be addressed before publication.

Specific points:
The main issue with this paper is that, while the paper is well written and the work done significant, points which the paper aims to make are not clear. Who is the target audience who will make use of the results? What are the wider implications beyond ECHAM5 model tuning? What new questions does this work provoke? I can make guesses but I am not sure they are what the authors intended nor convinced that most people will guess the same.

The paper has 2 purposes. It seeks to evaluate how the tuning of the key cloud parameters affects the present-day climate and if it affects the climate beyond the quantities it immediately controls. Moreover we wanted to investigate if the tuning of the global-mean present-day climate has implications for the anthropogenic aerosol effect. We made this clearer.

(1) The abstract only lists a series of statements about the results (what is the significance?). The introduction is very thorough, well researched, but does not provide a motivation for the work (what are the authors seeking to test/show and why?). The conclusions are perhaps the better of the three, but still fall short of identifying what the implications of the work are. This needs to be addressed before possible publication.

The abstract, introduction and conclusions have been improved.

The other specific points are as follows. These just need to be clarified.
(2) "Is a TOA flux in a fixed SST run expected to be in balance?" The authors make an assumption that the TOA in the prescribed SST ECHAM5 simulations should be in balance (plus or minus 1 Wm-2). I would question whether this should be the case – given contemporary atmospheric composition and SSTs isn't the TOA expected to be out of balance? Comparisons of fixed SST and fully coupled configurations of other climate models suggest that the prescribed SST configurations often have a positive bias of 1 to maybe as much as 2 Wm-2 whilst the net TOA for the equivalent coupled model control simulations are near balance.

You are right, the TOA net radiation flux in a fixed SST simulation does not need to be in balance for the coupled model simulation to be in radiative balance at TOA. However, our goal is not to tune the coupled model. In AGCM simulations alone a net TOA radiation balance is desirable for the comparison with observations. Moreover a balanced present-day AGCM ensures that the heat fluxes over the oceans correspond to the SST and that there is no mismatch between the two.

(3) "Are these the only parameters which are under constrained by observations or unobservable and which play a role in top of atmosphere balance?" For example within the UK Met Office modelling framework there are a large number of parameters which are either unobservable (such as rain auto-conversion, used in this study, and many others such as the critical relative humidity) and parameters which are in principle observable but which in practice are poorly constrained by what obs. we have (e.g. ice fall speed). Taking parameters from these two groups together there are a very large number of parameters which are likely to impact top of the atmosphere radiation, cloud and precipitation diagnostics – something which is likely to be true for all current models including ECHAM5. Given that this paper explores only 5 and that there is likely to be sensitivity to other unexplored parameters, how do the authors interpret the significance of their results?

You are right. The four parameters that were chosen in this paper are only a subset of possible parameters that could have been chosen. We chose those parameters as those are the ones that are modified when the cloud microphysics scheme is improved. Simply speaking if the goal just is to have a TOA net radiation balance between +/- 1 W m^{-2} and to ensure that the longwave and shortwave cloud forcing are within 5 W m^{-2} of the observations, it should be sufficient to tune 2 parameters. Therefore the focus of the re-tuning of the model is placed on re-tuning the autoconversion and aggregation rates. However, we let ourselves also guide from observations of the liquid water path and total cloud cover that are related to the large-scale cloud scheme as well. Guiding meaning that we prefer simulations in which the liquid water path lies between 50 and 84 g m^{-2} over the oceans, as this is what observations suggest and that the total cloud cover lies between 62 and 75%. In order to match these 4 quantities, at least 4 tuning parameters are necessary. More than 4 tuning parameters would mean that the system is over-determined. This would correspond to the situation that you describe. If we can match these 4 observations, which we can with the current simulations, we do not need to explore further tuning quantities. We clarified this.

(4) Do the authors want to comment on the apparent differences in radiative impacts of anthropogenic forcing arising from this study and Haerter et al, 2009? Both use the same underlying atmospheric model – and study some but not all of the same parameters, but the Haerter study produces a wider range of historical aerosol forcing. The Haerter et al, study suggest a much larger parameteric uncertainty for aerosol forcing – is the smaller range in this paper a result or just an artefact of different experimental methodologies? In either case it would be useful if this could be discussed.

The differences to the Haerter et al. study have been added.

General comments:
(1) Page 3 line 7, "If the GCMs are also free to choose their emission data base, then all publications of the total indirect aerosol effect can be compared." – do the authors mean "can not be compared"?

The statement has been clarified.

(2) Page 7, line 13: "However, based on our experience one year is sufficient in order to evaluate the global annual mean radiation balance at the top-of-the atmosphere (TOA), which is the goal of this study." How much year to year variability is there likely to be in TOA? Is this likely to have an impact on the tolerance of allowable TOA?

The year-to-year variability of the net TOA radiation is small. It amounts to a standard deviation of 0.2 W m^{-2} as obtained from two simulations run over 10 years. We added that.

(3) Page 7 Set-up of the Simulations, 2nd paragraph: It is not clear what these simulations are, how they differ from the previous fixed SST experiments and why nudging was introduced. Can the authors expand this discussion?

The discussion has been expanded.