We are grateful to the evaluations from the reviewers, which have allowed us to clarify and improve the manuscript. Below we addressed the reviewer comments, with the reviewer comments in italic and our response in bold. (The autoconversion and accretion data from HadGEM-UKCA are now available and Figure 6 is updated accordingly in the revised manuscript.)

Anonymous Referee #1

Received and published: 23 October 2015

The article investigates aerosol-cloud interactions and aerosol indirect radiative effects across a range of different climate models. The novel and very interesting aspect of the paper is that the large-scale dynamical settings are taken into account for the evaluation of aerosol-cloud interactions. The authors find a different sensitivity of the liquid water content to cloud condensation nuclei under different large-scale conditions, where regions of subsidence and strong monthly mean updraft are most sensitive. The comparison of different climate models indicates that models particularly strongly diverge in exactly these regimes. A further interesting finding is that the model predictions of the aerosol effects varies much more if different large-scale conditions are taken into account than for global results.

Promoting the idea of binning aerosol-cloud interactions into different dynamical settings is very helpful and an important aspect of advancing our understanding of aerosol-cloud interactions. The impact of large-scale dynamics on cloud-aerosol interactions has received only recently appropriate attention and this paper strongly contributes to highlight the importance of the large-scale dynamics. The large-scale (time and spatial) perspective taken in the paper allows to assess and compare a large number of large-scale dynamical regimes and helps to argue for the importance of large-scale dynamics. However, certainly more detailed work is required to understand the relation between large-scale dynamics, aerosols and cloud processes. The following points should be addressed before final publication.

Specific issues:
1. Please specify how monthly-mean GCM data for PI and PD is extracted from the models, i.e., time-slice experiments or average over specific time period (dates). If averages over a specific time period are used, it should be discussed what other changes in the state of the atmosphere could lead to changes in LWP coinciding with changes in aerosol number density. By just applying the eq. (1) these changes can project on the aerosol susceptibility although they are not physically related to aerosol-cloud interactions.

The monthly-mean GCM data for PI and PD is obtained by averaging over specific time period of 5 years. Natural variability of the simulations might

have some influence on the result. That's why all simulations were nudged toward reanalysis winds from operational forecast centers (some were also nudged toward analyzed temperature). For example, Figure S1 shows the vertical pressure velocity at 500hPa for PD, PI and the difference between them. It can be seen that the difference is quite minor. Nudging can significantly limits natural variability (Kooperman et al., 2012). Meanwhile, eq. (1) also allows some feedbacks, for example cloud feedback on CCN. An explanation about this has been added in the revised manuscript and now it reads (P. 9, l. 182-183): "Note that this metric allows some feedbacks, for example cloud effects on CCN." **and (P. 11, l. 234-238):** "Only ω500 in PD runs is used to derive dynamical regimes and then these dynamical regimes are applied to PI simulations as well, with the assumption that ω500 does not change much from PI to PD. This assumption is reasonable because both PD and PI runs were nudged toward the reanalysis data here, which ensures ω500 is very similar between PD and PI."**.**

2. It would help to clarify the definition of "dynamical regime", if the characteristic spatial and temporal scales of the dynamic processes depicted by the chosen definition would be specified. It should also be discussed how relevant such a coarse definition is for aerosol-cloud interactions, particularly in regions with very transient dynamic systems as for instance in the extra tropics. It would be also helpful to include an additional figure showing the typical distribution of dynamical regimes as used in this study over the globe eventually for different seasons.

Since vertical pressure velocity is used as a criterion here, dynamic regimes generally follow the features of vertical pressure velocity distributions. Figure S1.A shows the distribution of annual mean distribution of ω500. Descending regimes are mostly located at subtropical regions and western coasts of continents, while ascending regimes locate around ITCZ and northern Pacific where storm tracks prevail. As for temporal change, the seasonal evolution of dynamic regimes follows seasonal changes in the major meteorological systems. For example, ascending regimes move north as ITCZ move north and descending regimes move accompanying with the movement of subtropical high. The characteristics of dynamic and thermodynamic regimes were discussed in detail in Bony et al. (2004). For some more specific dynamic regimes, such as stratocumulus, transitional clouds and trade wind cumulus, Figure S2 to S4 show the spatial distribution and temporal features of them. The spatial distribution in different seasons is similar to annual mean result (Fig. 4). As season changes, the spatial patterns do not change much and only the change of cloud fraction is evident. These clarifications of dynamic regimes with spatial and temporal patterns have now been added to the methodology part of revised manuscript and now it reads (P. 10, l.

202-210): "Since vertical pressure velocity is used as a major criterion here, dynamic regimes generally follow the features of vertical pressure velocity distributions. Descending regimes are mostly located at subtropical regions and western coasts of continents, while ascending regimes locates around ITCZ and northern Pacific where storm tracks prevail. The seasonal evolution of dynamic regimes follows seasonal changes in the major meteorological systems. For example, ascending regimes move north/south as ITCZ move north/south and descending regimes move accompanying with subtropical high move. The characteristics of dynamic and thermodynamic regimes were discussed in detail in Bony et al. (2004)."

As is already stated in the text (P. 10. l. 199-P. 201): "Note however that the use of monthly means may obscure some details in the microphysical relationships, especially where the variability of cloud properties is high."**. we acknowledge that the definition could be a little bit coarse. However, it is simple and it provides an effective way to separate different dynamic regimes. Bony and Dufresne (2005) adopted this definition of dynamic regimes and found evident subtropical cloud feedbacks uncertainties among climate models. More importantly, through this definition we do see different features of aerosol-cloud interactions within different dynamic regimes and find strong spread among different models, which could in turn suggest that this definition is effective and useful to understand the uncertainties associated with aerosol-cloud interactions in global climate models.**

3. It should be specified how changes in LWP and CCN are computed: Are the values first binned according to ω500 in PD and PI runs and then subtracted or are the grid point differences binned according to ω500 from either PD or PI runs? If the latter is used some justification is required, as the spatial pattern of ω500 may be different between PD and PI runs.

The latter one is used. Only ω500 in PD runs is used to derive dynamical regimes and then these dynamical regimes are applied to PI simulations as well, with the assumption that ω500 does not change much from PI to PD. This assumption is reasonable because both PD and PI runs were nudged toward the reanalysis data in this study, which ensures ω500 is very similar between PD and PI. Figure S1.C shows the difference of ω500 between PD and PI in CAM5-CLUBB as an example. It can be seen that the difference is indeed very small.

Deriving dynamical regimes from PD and PI runs separately could be another choice as the reviewer has pointed out. Actually, we did test this method at the very beginning of our work. But with this approach, the grid points can be different between PD and PI under each dynamical regime, which might introduce other differences than those from aerosols. In order to avoid this complexity and further considering the fact that the

distribution of ω500 are very similar between PD and PI, we finally decided to choose the latter approach (i.e., gird-point differences are binned according to ω500 from PD runs).

This is now clarified in the revised manuscript:(P. 11, l. 234-238): "Only ω 500 in PD runs is used to derive dynamical regimes and then these dynamical regimes are applied to PI simulations as well, with the assumption that ω 500 does not change much from PI to PD. This assumption is reasonable because both PD and PI runs were nudged toward the reanalysis data here, which ensures ω 500 is very similar between PD and PI.".

4. Are the LWP and CCN values for different ω500 arithmetic means for the values in each bin?

Yes. We first sort these 12-month global grid values into 20 dynamical regimes according to their ω500 values, keeping the number of grids in each bin equal. Mean values of LWP, CCN and other fields for each bin are calculated from averaging the values of all grids belonging to that particular bin. Now this explanation has been added to the first paragraph of Section 3.2a (P. 16, l. 328-333): "Figure 1 shows LWP and CCN as a function of vertical pressure velocity at 500 hPa (ω 500) derived from PD simulations. To derive Figure 1, the 12-month monthly global grid values are first sorted into 20 dynamical regimes according to their ω 500 values, keeping the number of samples in each bin equal. LWP, CCN and values of other fields for each bin are then calculated from averaging the values of all samples in that particular bin.".

5. The summary is a bit fuzzy and hard to read, particularly the 3rd to 5th paragraph. Please try to reformulate these. The comparison to findings from previous studies should be more clearly described and potential reasons for discrepancies summarized. Furthermore a short statement on the impact of neglecting mixed phase and ice-phase processes on the results should be included.

The summary is reformulated now. Generally it has been shortened to present the most important results more clearly and concisely. The second paragraph and the 3rd paragraph are now combined into one paragraph. Now the text of 2nd to 4th paragraph in the summary reads:

"The response of liquid water path (LWP) to aerosol perturbations, λ=dlnLWP/dlnCCN, a metric to quantify cloud lifetime effect of aerosols (Wang et al., 2012), shows a large spread within dynamical regimes among GCMs, although the global means are close. This diversity indicates that the aerosol cloud lifetime effect is regime-dependent. It is in strong ascending regimes and subsidence regimes that λ differs most between GCMs (Fig. 2a). Stratocumulus regimes have traditionally been the focus for studying aerosol indirect effects because of their significant cooling effect in climate system (e.g., Ackerman et al., 2004; Bretherton et al., 2007;

Gettelman et al., 2013). However, our results highlight that regimes with strong large-scale ascent should be another important regime to focus on in the future. Our results indicate that aerosol indirect forcing in regimes of vertical ascent is close to, or even larger than that in low cloud regimes (Fig. 7). Note however that these GCMs do not treat aerosol effects in their representations of deep convection that dominates clouds and LWP in regimes with strong ascent, while new versions of CAM exist where a version of the MG microphysics has been embedded in the deep convective parameterization (Song and Zhang, 2011).

By adding LTS as another criterion, we further separated different low cloud types under large-scale subsidence and revealed some further differences in cloud lifetime effect of aerosols on different types of low clouds. For example, the large λ in subsidence regimes in CAM5-CLUBB and ECHAM6-HAM2 comes from both stratocumulus and trade wind cumulus, while in CAM5-CLUBB-MG2 it mostly comes from trade wind cumulus (Fig. 5). It is also interesting to note that the distribution of λ in SPRINTARS and SPRINTATSKK is more likely to depend on LTS rather than vertical pressure velocity."

A discussion about mixed phase and ice phase process has been added to the end the summary (P. 32, l. 687-693):" It is our future plan to carry in-depth analysis to further understand some of the findings documented here, such as the large spread in λ in regimes of vertical ascent in different models. For example, LWP response to aerosol perturbation documented in this study may include contributions from mixed-phase and ice clouds. In- depth analysis of cloud macrophysics and microphysics processes will help to improve the understanding of the model uncertainty."

Minor issues Introduction

1. p. 23686, l. 15ff: Add a sentence with some references on the influence of aerosols on clouds by their potential to modify latent heating and cooling profiles.

Done. The sentence "It is worth noting that delaying the onset of precipitation may further modify latent heating profiles, which could lead to the invigoration of convective clouds (Andreae et al., 2004; Rosenfeld et al., 2008)." **has been added to the second paragraph of the introduction now (P. 4, l. 71-73).**

2. p. 23686, l. 17: Give references to articles considering mixed-phase and ice phase clouds.

Done. Now the text reads (P. 4, l. 74-77): "There are also adjustments on mixed-phase and ice clouds (e.g., Storelvmo et al., 2008; Lohmann and Hoose, 2009; Liu et al. 2012; Storelvmo et al., 2008; Wang et al., 2014). The focus of this study is on liquid cloud response to aerosol perturbation, primarily from large-scale clouds."

3. p. 23687, l. 8ff: Repeating the information from two sentences earlier. Also the *next sentence is very long, please reformulate.*

The sentence "Ackerman et al. (2004) also demonstrated that the reduced cloud droplet size due to increases in aerosol reduces cloud droplet sedimentation." **is deleted now. The following sentence is reformulated into two sentences, which are** "They showed that the entrainment rate was reduced by decreasing available boundary-layer turbulence kinetic energy (TKE). However, Bretherton et al. (2007) found that TKE remained unchanged and changes in entrainment rate is mainly caused by reduced evaporative cooling from removing out liquid water" **(P5, l 98-101).**

4. p. 23692, l.9: "that the frequency of the following sorted dynamic regimes": unclear please reformulated.

We have changed this. Now the text reads (P. 13, l 263-265): "The similar patterns of ω 500 (due to nudging) in these simulations ensure that dynamic regimes defined by ω 500 do not vary much between models."

5. p. 23695, l. 18: replace "largest λ" with "largest global λ"

Done.

6. p. 23696, l. 14ff: sentence starting with "A major improvement ..." is unclear. Please reformulated.

We apology for this mistake made in the typesetting of the paper. It is an incomplete sentence. The full sentence should be "A major improvement of CAM-CLUBB is the better simulation of the transition of stratocumulus to trade wind cumulus over subtropical oceans (Bogenschutz et al., 2013). Fig. 2a shows that … ". **It has now been fixed in the revised manuscript (P. 19, l. 404-407).**

7. p. 23697, l. 2: remove "where storm tracks prevail". This is not really required here and makes sentence hard to read.

This is now removed.

8. p. 23697, l. 7: add "spatial" before "pattern"

Done.

9. p. 23700, l. 19: sentence starting with "By sorting into ..." is unclear. Please reformulate.

We have changed this. Now it is reformulated to "It is in moderate regimes (-20 hPa/d < ω500 < 10 hPa/d) where the result is consistent with Gettelman et al. (2015), which shows larger AUTO/PRECL in CAM5 than CAM5-MG2.*"* **(P. 25, l. 531-533).**

10. p. 23703, l. 18: "Despite the closer global means ..." unclear, please reformulate.

Done. Now it is changed to "The response of liquid water path (LWP) to aerosol perturbations, λ=dlnLWP/dlnCCN, a metric to quantify cloud lifetime effect of aerosols (Wang et al., 2012), shows a large spread within dynamical regimes among GCMs, although the global means are close.*"***(P. 30, line 632-635)**

11. p. 23704, l. 24: "Results derived from large eddy ..." unclear, please reformulate.
Done. The sentence is reformulated to "Results derived from large eddy simulation (LES) and single column model (SCM) (e.g., Ackerman et al., 2004; Guo et al., 2011) have shown that λ could be negative under low precipitation situations, which indicates that λ is expected to be smaller under low precipitation situations.*"* **(P. 31, l. 661-664).**

12. p. 23705, l. 7: replace "can reduce" by "reduces", remove "only"

Done.

13. p. 23705, l. 9: replace by "total SCRE decreases in models with prognostic rain scheme compared to those with a diagnostic rain scheme"

Done.

14. p. 23705, l. 20: Monthly mean ω500 also does not necessarily represent the same conditions as also dynamical conditions may vary quite significantly on sub-monthly timescales.

Yes, we agree that monthly mean ω500 may not represent the variation in dynamical regimes on sub-monthly timescales. However, as we stated in our response to the reviewer's specific comment #2, sorting model data according to ω500 is an effective way to reveal uncertainties in aerosol indirect effects among different models. As is already stated in the text (P. 10. l. 199-201):"Note however that the use of monthly means may obscure some details in the microphysical relationships, especially where the variability of cloud properties is high.*"*, **we acknowledge the limitation of using monthly data, and we do plan to carry further analysis with hourly data in the future (see our discussion in the last paragraph .**

15. p. 23706, l. 1: remove "A" from "Appendix A" since there is only one appendix.

Done.

16. caption of Tab. 4: replace "global regimes" by "all dynamical regimes".

Done.

Anonymous Referee #2

Received and published: 5 November 2015

This paper presents an analysis of the regime dependence of the susceptibility of LWP to changes in CCN, from 10 GCMs. The main goal of this analysis is show the importance of examing aerosol-cloud interactions different cloud and dynamical regimes, focusing only on warm clouds. The paper shows that lambda differs most between GCMs in regions of strong ascending regimes and subsidence regimes. Interestingly, the analysis shows that the sensitivity of LWP to changes in aerosol in regions of vertical ascent are equal to or even larger than that in low cloud regions. To the best of my knowledge this is the first paper that assesses aerosol-cloud interactions by dynamic regime, using GCMs. This is an important step to understanding aerosol-cloud interactions, so it is good to see this. In general, I think the paper and the overall results will be of interest to a broad community, but I think there needs to be some more detail about the method and some more analysis to understand the significance of the results. For this reason, I am recommending the paper should be accepted for publication once the following changes have been undertaken.

General comments:
1. It is not completely clear from the paper how the presented LWP and CCN are calculated. From the description in Table 2, I have to assume that the presented is averaged LWP and CCN are spatial averages for the present day, where the space can be the globe or the dynamic regime. The relative change in LWP and CCN from the GCM is the relative change in the spatial average over time (PI to PD). Is this correct? If so, it would be very useful if this could be explicitly stated in the text. At present, I feel that I am having to piece together the method from snippets throughout the entire text (including figure and table captions).

Yes, as described in the caption of Table 2, LWP and CCN are annual spatial averages over ocean from PD simulations, for the purpose of showing annual mean state of each model. As for LWP and CCN of each dynamical regime, they are both spatial and temporal averages from 12-month monthly data. As for how to get LWP and CCN for each bin, please see also our answer to specific comment #4 from reviewer #1. This is now added in the methodology section to further clarify how these fields are calculated (P. 8, l. 171-175): " It is directly calculated as the relative change of monthly mean LWP from pre-industrial (PI) to present day (PD) divided by the relative change of

CCN. Here $dlnLWP=(LWP_{PD}-LWP_{PI})/LWP_{PI}$ and $dlnCCN=(CCN_{PD}-CCN_{PI})/CCN_{PI}$, where LWP_{PD} and LWP_{PI} are LWP in PD and PI, respectively, while CCN_{PD} and CCN_{PI} are CCN in PD and PI, respectively.*"*.

2. Equation 1 defines the susceptibility of LWP to changes in CCN, but it is not clear in this paper how this is calculated. Given past work on precipitation susceptibility, I assume that LWP susceptibility is calculated by binning LWP and the associated in CCN from PI to PD into dynamic regime bins. Then, within a bin, a linear regression is applied to the lnLWP and lnCCN, to obtain lambda. Is this correct? If so this should be stated, so that others can perform the same analysis. Further, this work, particularly figure 2 and table 1 only present a single value for each dynamic bin. It would be very useful and would add to the paper if the authors could present error bars on this figure, or state the correlation for each regression, so that the reader can understand the significance of the trend in lambda with dynamic regime. Past work, e.g. Jiang et al, Terai et al, Hill et al, all presented error bars or correlations coefficients with their work, which helps the reader to understand significance. Is the correlation of LWP to CCN good in the GCMs tested?

The susceptibility of LWP to changes in CCN is not calculated from the linear regression between lnLWP and lnCCN. This is directly calculated as the relative change of LWP from PI to PD divided by the relative change of CCN,i.e., $\lambda=dlnLWP/dlnCCN=[(LWP_{PD}-LWP_{PI})/LWP_{PI}]/[(CCN_{PD}-CCN_{PI})/CCN_{PI}]$. For this reason, we do not provide the error bars or correaltions in Figure 2 and Table 1. The same approach was also used by Wang et al. (2012) to constrain the cloud lifetime of aerosols. The detailed formula is added to methodology part now. We found that using the term 'susceptibility' might be somehow misleading, so now it has been changed to 'the response of LWP to changes in CCN' in the revised manuscript and the text reads (P. 8, l. 171-175):*"*.It is directly calculated as the relative change of monthly mean LWP from pre-industrial (PI) to present day (PD) divided by the relative change of CCN. Here $dlnLWP=(LWP_{PD}-LWP_{PI})/LWP_{PI}$ and $dlnCCN=(CCN_{PD}-CCN_{PI})/CCN_{PI}$, where LWP_{PD} and LWP_{PI} are LWP in PD and PI, respectively, while CCN_{PD} and CCN_{PI} are CCN in PD and PI, respectively.*"*.

3. The paper very clearly states that the focus of this work is warm phase clouds, so it focuses on LWP alone. This is fine, but given that all the GCMs include ice phase processes, it would be useful if the authors would discuss whether the GCMs are producing changes in the ice phase and mixed phase processes and whether these changes are influencing their results. For example, is the sensitivity of the LWP to aerosol in ascending regimes only the result of changes in warm phase rain processes or is there an impact resulting from change in the ice phase and mixed phase processes. I think this type of discussion would give some more insight into the results presented.

We first would like to clarify that there was a mistake in the last sentence in second paragraph of introduction in the original manuscript. Actually all liquid clouds were sampled in this study, not only warm clouds, so cloud water melt from mixed and ice phase processes is also included. We have corrected this in the revised manuscript and it reads (P. 4, l. 74-77): "There are also adjustments on mixed-phase and ice clouds (e.g., Storelvmo et al., 2008; Lohmann and Hoose, 2009; Liu et al. 2012; Storelvmo et al., 2008; Wang et al., 2014). The focus of this study is on liquid cloud response to aerosol perturbation, primarily from large-scale clouds.**".**

It is possible that some of the changes in liquid water path and aerosol indirect forcing may come from changes in ice phase and mixed phase processes (e.g., Gettelman, 2015). Detailed and specific discussions on the role of mixed-phase and ice phase clouds required in-depth analysis and is beyond the scope of the current manuscript and we intend to leave this for a separate study in the future. This has been now stated in the end of summary (P. 32, l. 687-693): " It is our future plan to carry in-depth analysis to further understand some of the findings documented here, such as the large spread in λ in regimes of vertical ascent in different models. For example, LWP response to aerosol perturbation documented in this study may include contributions from mixed-phase and ice clouds. In-depth analysis of cloud macrophysics and microphysics processes will help to improve the understanding of the model uncertainty.*"*

Specific Comments:

1. Page 23685, abstract - "with strong large scale ascend" should be changed to "strong large scale ascent"

Corrected.

2. Page 23688, paragraph 2 – I feel that the authors are inferring that autoconversion is a natural process in the warm rain formation. I would argue that autoconversion is modelling necessity only related to bulk microphysics schemes. For example, bin microphysics and superdroplet schemes do not include a specific parametrisation for autoconversion because it is dealt with the collection equations. The second sentence on paragraph 2 needs to be modified so it is explicitly stated that this relates to only bulk microphysics schemes.

Thanks and we now clarified this in the revised manuscript. Now the text reads (P. 6, l. 128-129): "In warm clouds, cloud microphysical processes are dominated by autoconversion and accretion in bulk microphysics schemes (Gettelman et al., 2013).*"*

3. Page 23688, paragraph 2, last sentence – The last sentence is correct, i.e, using a prognostic rain scheme enhances the dominance of accretion. However, it may be *useful to state that this alone might not be a panacea. For example, Hill et al 2015 showed that for an all-else-equal test, there is still significant differences in the precipitation susceptibility from single moment prognostic rain schemes.*

Yes, thanks for the suggestion. We realized the last sentence might be kind of misleading, which could underline too much the effect of adding prognostic rain scheme. As being pointed out here, we agree that prognostic rain scheme might not be a panacea. This is also the motivation of comparing results derived from models with (e.g. CAM5-MG2) and without (e.g., CAM5-NCAR) prognostic rain-scheme in our work. It has been modified now. Now the statement (P. 7, l. 142-147): "However, Hill et al. (2015) shows that adding prognostic rain scheme alone still cannot reduce the spread of susceptibility of precipitation among different cloud microphysics parameterizations and further shows that increasing the complexity of the rain representation to double-moment significantly reduces the spread of precipitation sensitivity and improves overall consistency between bulk and bin schemes." **has been added.**

4. Page 23692, second paragraph, last 2 sentences – I found this a bit confusing. I think this is saying that the same LWP are not being presented because the models report different LWP, with some including LWP from mixed phase clouds, while others do not. Is this important? Does the impact of changing aerosol on mixed phase clouds impact the results and conclusions from the regime analysis? This point relates back the general comment (3).

Different models treat LWP differently. As summarized in Table 1, most versions of CAM5 sample LWP from stratiform clouds. CAM5-CLUBB and CAM5-CLUBB-MG2 also include shallow convective clouds because higher-order turbulence closure (CLUBB) unifies the treatment of boundary layer turbulence, stratiform clouds and shallow convection (Bogenschutz et al., 2013). SPRINTARS and HadGEM3-UKCA sample LWP from both stratiform clouds and convective clouds. This difference can contribute to the spread of λ in our study, and that's why we show Table 1 and also give some descriptions in the manuscript. This is now noted in the manuscript and it reads: (P. 16, l. 342-345): " The model spread of LWP response is larger in the ascending regimes than in the subsiding regimes. This may be partly related to the fact that the types of clouds included in LWP are not the same in different models (Table 1)."

However, all models sample LWP from liquid clouds, which also include cloud water melt from mixed phase clouds and ice phase clouds. So changes in LWP in the paper may include aerosol impact in mixed-phase or ice clouds. See our answer to general comment (3).

5. Page 23693, second paragraph, last sentence – I like that the authors have stated *that large differences CCN may not correspond to large differences in the Nd because treatment of cloud base updraft. However, it raises the question whether lambda should be defined as the change in LWP vs the change in Nd, not CCN? I am aware that this definition would be difficult to compare with observations, but given that LWP is dependent on Nd, not necessarily CCN, it would be useful to know whether the results presented are sensitive to this definition. Could the authors add some discussion to address this?*

As the focus of this study is about aerosol indirect effects, the current definition of λ provides the direct measure of cloud response to anthropogenic aerosol perturbation and therefore serves our purpose well. As the reviewer noted, the alternative definition of λ as dlnLWP/dlnNd would be difficult to compare with observations, and this new definition also can not directly measure clouds response to anthropogenic aerosols. However, we agreed that the reviewer raised an important point. The interaction between clouds and anthropogenic aerosols arises through a chain of processes, from effects of the CCN on Nd to effects of Nd on cloud water. This chain of processes can be expressed as dlnLWP/dlnCCN=(dlnLWP/dlnNd)*(dlnNd/dlnCCN). It is highly interesting to examine this chain of processes to improve our understanding of aerosol-cloud interactions. In a separate study using the same set of model simulations, we did examine this chain of processes (Ghan et al., 2016). Further discussion about this has been added to Section 3.1 and the text reads (P. 15, l. 312-325): "We also should note that large differences in CCN shown in Table 2 do not necessarily correspond to equally large differences in droplet concentration (N_d), since N_d is primarily dependent on cloud base updraft that is an extremely uncertain parameter and may vary significantly between the GCMs. It therefore seems reasonable to define λ as the change in LWP vs. the change in cloud droplet number concentration (N_d), which would provide a direct insight into how clouds response to N_d change since LWP directly depends on N_d, not necessarily on CCN. However, this alternative definition of λ as dlnLWP/dlnN_d would be difficult to compare with observations, and this also does not directly measure cloud response to anthropogenic aerosols. The interactions between clouds and anthropogenic aerosols arise through a chain of processes, from effects of the CCN on N_d to effects of N_d on cloud water, which can be expressed as dlnLWP/dlnCCN=(dlnLWP/dlnN_d)*(dlnN_d/dlnCCN). This chain of processes has now been examined in Ghan et al., (2016) based on the same set of model simulations documented in this study. "

6. Page 23696, second paragraph, sentence beginning "A major improvement of CAMCLUBB...", this sentence does not make sense. I think some words are missing

Sorry for the mistake here. This is now fixed and please see our answer to specific comment #6 from reviewer #1.

7. Page 23702, second paragraph, sentence beginning "Here we investigate the LWP response to aerosol perturbations under low precipitation...". Are the results sensitive to the precipitation threshold applied? Previous work has shown that precipitation susceptibility is sensitive to this threshold.

This is a good point. The results can be potentially sensitive to precipitation threshold. We now tested how our results are sensitive to different thresholds of precipitation (0.01, 0.05, 0.1 and 0.2 mm d^{-1}). Table S1 shows the fractional occurrences of low surface precipitation over global oceans for different thresholds. As we expect, the fractional occurrence of low precipitation increases as the threshold increases. However, the LWP response to aerosol perturbations under low and high precipitation does not change much as the threshold changes. For example, although the threshold has been changed to four times larger from 0.05 mm d^{-1} to 0.2 mm d^{-1}, λ under high and low precipitation situations barely change. This indicates the results of LWP response to CCN changes under low and high precipitation are insensitive to the threshold we applied.

We also looked at aerosol indirect effects (dSCRE) under low and high precipitation with different thresholds (Figure S5-S7 and Figure 7). The aerosol indirect effects contributed by low and high precipitation do change with different thresholds. This is mainly caused by the changing fractional occurrences. However, the conclusion is still reliable no matter how the threshold changes. High precipitation situations contribute the most part of aerosol indirect effects.

As monthly data is used here, the results may not be as sensitive as it would be if instantaneous data is used. As we noted in the summary part, using instantaneous data may also produce different results and we intend to carry a similar analysis using the instantaneous data in a future study. We now added discussion in the end of Section 3 and it reads (P. 28, l. 608-623):
" Our sensitive tests indicate that results in Table 4 and Figure 7 can be potentially sensitive to the precipitation threshold applied to separate high precipitation and low precipitation situations (not shown). The occurrence frequency of low precipitation situations increases with increasing threshold and the magnitude of increase can be different for different models. For example, when the precipitation threshold increases from 0.01 mm d^{-1} to 0.20 mm d^{-1}, the occurrence frequency of low precipitation situations increases from 2% to 37% in CAM5-PNNL while it increases from near 0% to 5% in CAM5-CLUBB. Increasing the precipitation threshold also increases the contribution of low precipitation situations to the total aerosol indirect forcing as the occurrence frequency of low precipitation situations increase. However, our results indicate that the LWP response to aerosol perturbations under low and high precipitation does not change much as the precipitation threshold changes and that high precipitation situations generally contribute more to the total aerosol indirect

forcing for precipitation threshold in the range of 0.01 to 0.20 mm d^{-1}. More work is needed to explore this further such as how results may be different when instantaneous precipitation data (e.g., 3-houly data) is used.”

References:

Bogenschutz, P. A., Gettelman, A., Morrison, H., Larson, V. E., Craig, C., and Schanen, D. P.: Higher-Order Turbulence Closure and Its Impact on Climate Simulations in the Community Atmosphere Model, J. Clim., 26, 9655-9676, 10.1175/jcli-d-13-00075.1, 2013.

Bony, S., Dufresne, J. L., Le Treut, H., Morcrette, J. J., and Senior, C.: On dynamic and thermodynamic components of cloud changes, Clim. Dyn., 22, 71-86, 10.1007/s00382-003-0369-6, 2004.

Bony, S., and Dufresne, J. L.: Marine boundary layer clouds at the heart of tropical cloud feedback uncertainties in climate models, Geophys. Res. Lett., 32, 10.1029/2005gl023851, 2005.

Gettelman, A.: Putting the clouds back in aerosol-cloud interactions, Atmos. Chem. Phys., 15, 12397-12411, 10.5194/acp-15-12397-2015, 2015.

Ghan, S. J., Wang, M., Zhang, S., Ferrachat, S., Gettelman, A., Griesfeller, J., Kipling, Z., Lohmann, U., Morrison, H, Neubauer, D., Partridge, D., Stier, P., Takemura, T., Wang, H., Zhang, K.: Challenges in Constraining Anthropogenic Aerosol Effects on Cloud Radiative Forcing Using Present-day Spatiotemporal Variability, Proc. Natl. Acad. Sci. U. S. A., Accepted, 2016.

Kooperman, G. J., Pritchard, M. S., Ghan, S. J., Wang, M. H., Somerville, R. C. J., and Russell, L. M.: Constraining the influence of natural variability to improve estimates of global aerosol indirect effects in a nudged version of the Community Atmosphere Model 5, J. Geophys. Res.-Atmos., 117, 10.1029/2012jd018588, 2012.

Hill, A. A., B. J. Shipway, and I. A. Boutle (2015), How sensitive are aerosol-precipitation interactions to the warm rain representation?, J. Adv. Model. Earth Syst., 7, 987–1004, doi:10.1002/2014MS000422.

Jiang, H., Feingold, G., and Sorooshian, A.: Effect of Aerosol on the Susceptibility and Efficiency of Precipitation in Warm Trade Cumulus Clouds, J. Atmos. Sci., 67(11), 3525–3540, doi:10.1175/2010JAS3484.1, 2010.

Penner, J. E., Xu, L., and Wang, M. H.: Satellite methods underestimate indirect climate forcing by aerosols, Proc. Natl. Acad. Sci. U. S. A., 108, 13404-13408, 10.1073/pnas.1018526108, 2011.

Stevens, B., and Feingold, G.: Untangling aerosol effects on clouds and precipitation in a buffered system, Nature, 461, 607-613, 10.1038/nature08281, 2009.

Terai, C. R., R. Wood, D. C. Leon, and P. Zuidema (2012), Does precipitation susceptibility vary with increasing cloud thickness in marine stratocumulus?, Atmos. Chem. Phys., 12, 4567–4583, doi:10.5194/acp-12-4567-2012.

Wang, M. H., Ghan, S., Liu, X. H., L'Ecuyer, T. S., Zhang, K., Morrison, H., Ovchinnikov, M., Easter, R., Marchand, R., Chand, D., Qian, Y., and Penner, J. E.: Constraining cloud lifetime effects of aerosols using A-Train satellite observations, Geophys. Res. Lett., 39, 10.1029/2012gl052204, 2012.

Table S1. The fractional occurrences of low surface precipitation in PD cases over global oceans for different precipitation thresholds. Low precipitation situations refer to monthly surface precipitation rate (PRECL) less than the threshold.

Model	f, low thre=0.01	f, low thre=0.05	f, low thre=0.1	f, low thre=0.2
CAM5	0.02	0.18	0.27	0.37
CAM5-MG2	0.23	0.32	0.39	0.48
CAM5-PNNL	0.02	0.19	0.28	0.37
CAM5-CLUBB	0.00	0.01	0.02	0.05
CAM5-CLUBB-MG2	0.03	0.08	0.16	0.30
ECHAM6-HAM2	0.04	0.12	0.18	0.25
SPARINTARS	0.00	0.02	0.03	0.06
SPARINTARS-KK	0.00	0.02	0.03	0.05
ModelE2-TOMAS	0.00	0.00	0.001	0.01
HadGEM3-UKCA	0.01	0.02	0.06	0.15

Table S2. λ under low and high surface precipitation situations only over downdraft regimes for different thresholds.

	thre=0.01		thre=0.05		thre=0.1		thre=0.2	
Model	λ[a] low, down	λ[b] high, down	λ[a] low, down	λ[b] high, down	λ[a] low, down	λ[b] high, down	λ[a] low, down	λ[b] high, down
CAM5	0.16	0.18	0.20	0.18	0.21	0.19	0.21	0.19
CAM5-MG2	0.16	0.22	0.18	0.23	0.19	0.24	0.21	0.25
CAM5-PNNL	0.12	0.16	0.16	0.17	0.17	0.17	0.18	0.17
CAM5-CLUBB	Nan	0.30	0.34	0.30	0.33	0.30	0.34	0.31
CAM5-CLUBB-MG2	0.18	0.30	0.21	0.33	0.26	0.33	0.31	0.32
ECHAM6-HAM2	0.13	0.24	0.24	0.23	0.25	0.23	0.27	0.22
SPARINTARS	Nan	0.01	0.06	0.01	0.06	0.01	0.04	0.01
SPARINTARS-KK	Nan	0.04	0.25	0.04	0.24	0.04	0.20	0.04
ModelE2-TOMAS	Nan	0.00	Nan	0.00	-0.011	0.001	-0.01	0.00
HadGEM3-UKCA	0.03	0.03	0.04	0.03	0.04	0.03	0.04	0.03

[a] λ under low PRECL for downdraft regimes

[b] λ under high PRECL for downdraft regimes

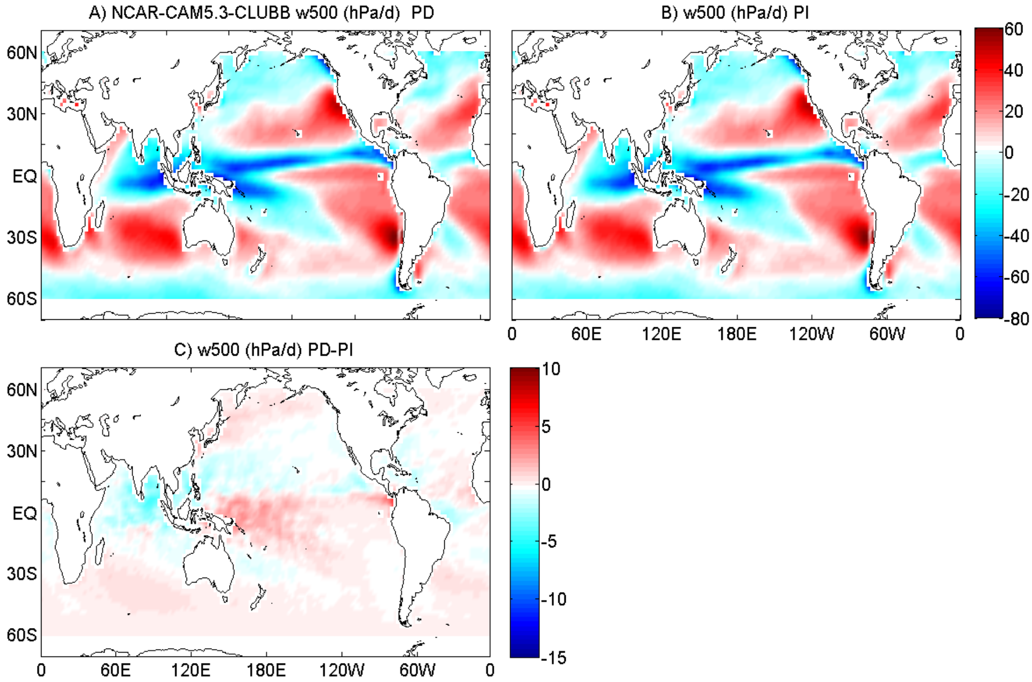

Figure S1. Annual average vertical pressure velocity at 500hPa level derived from A) present day simulation (PD), B) pre-industrial simulation (PI) and C) their difference PD-PI in CAM5-CLUBB.

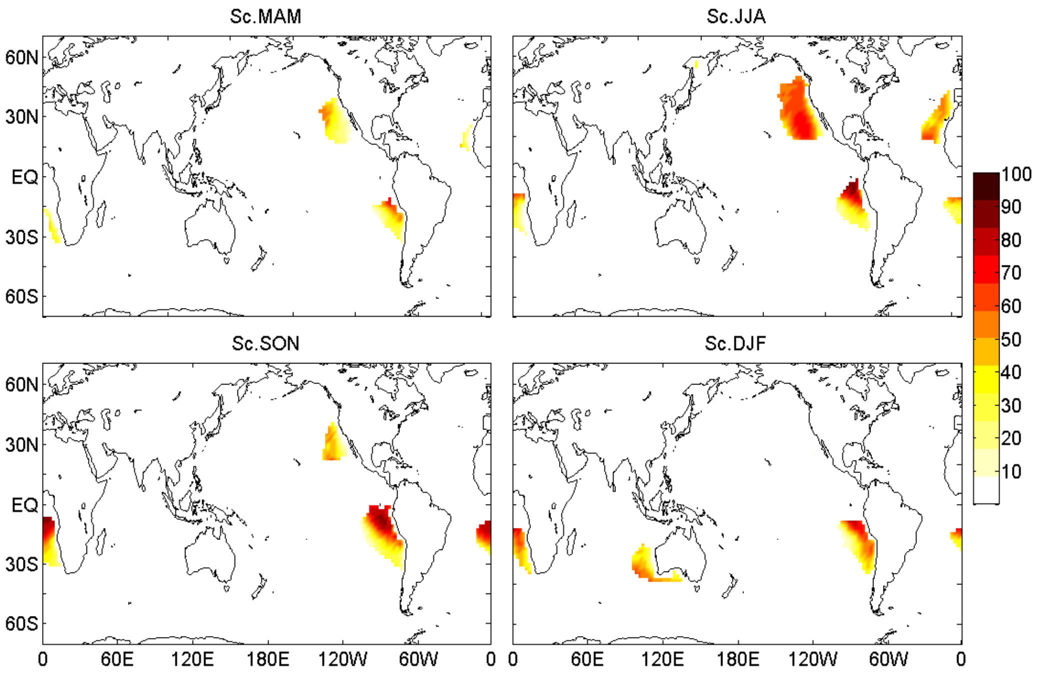

Figure S2. The seasonal mean (MAM, JJA, SON and DJF) cloud fraction of stratocumulus regime derived from PD monthly simulation in CAM5-CLUBB.

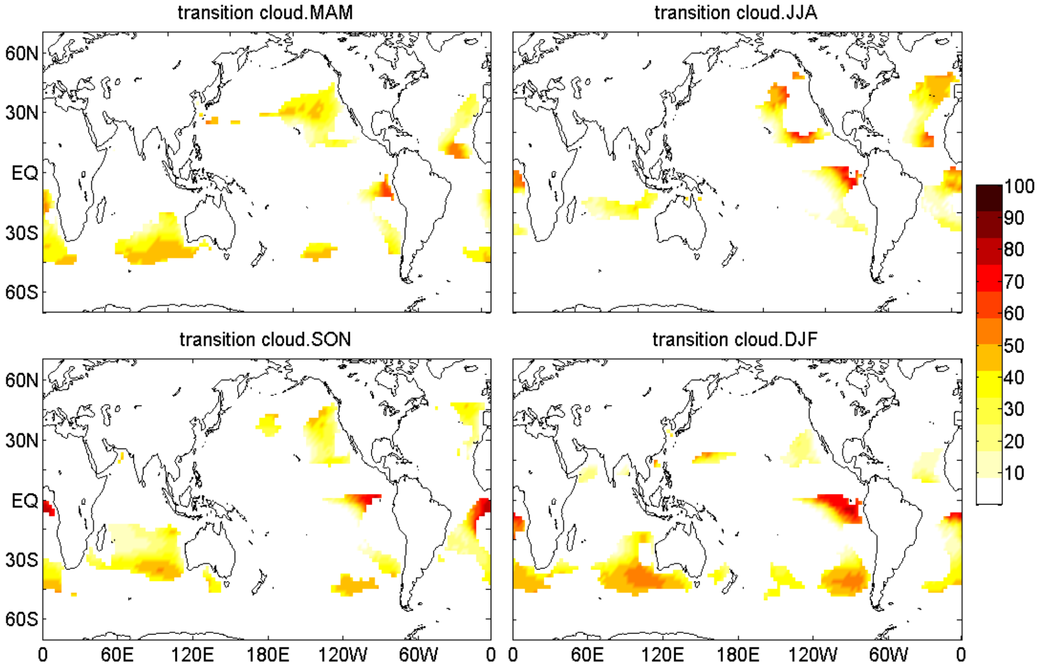

Figure S3. Same as Fig. S2, but for transitional clouds.

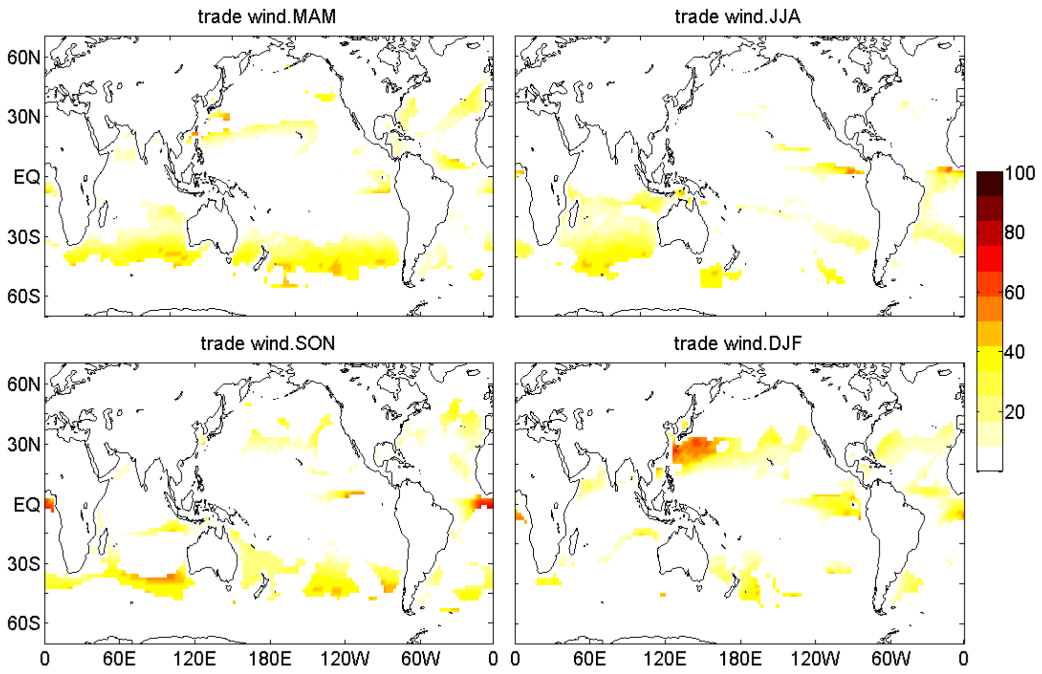

Figure S4. Same as Fig. S2, but for trade wind cumulus.

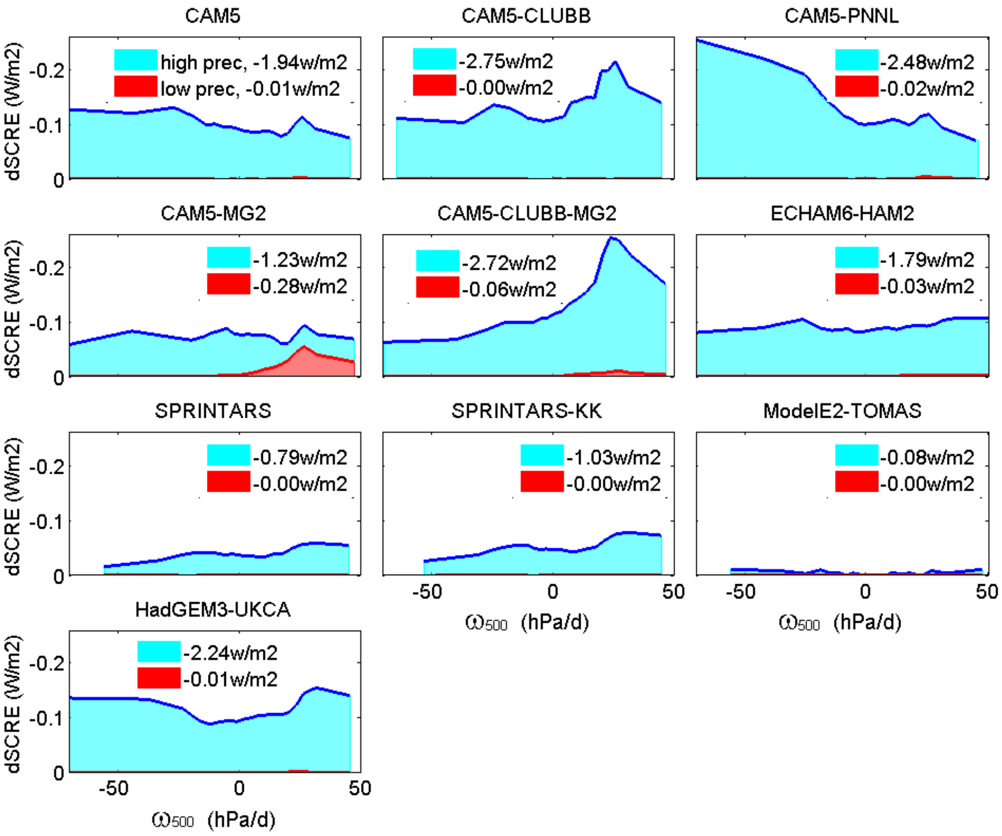

Fig S5. Same as Figure 7, but for threshold=0.01 mm d^{-1}.

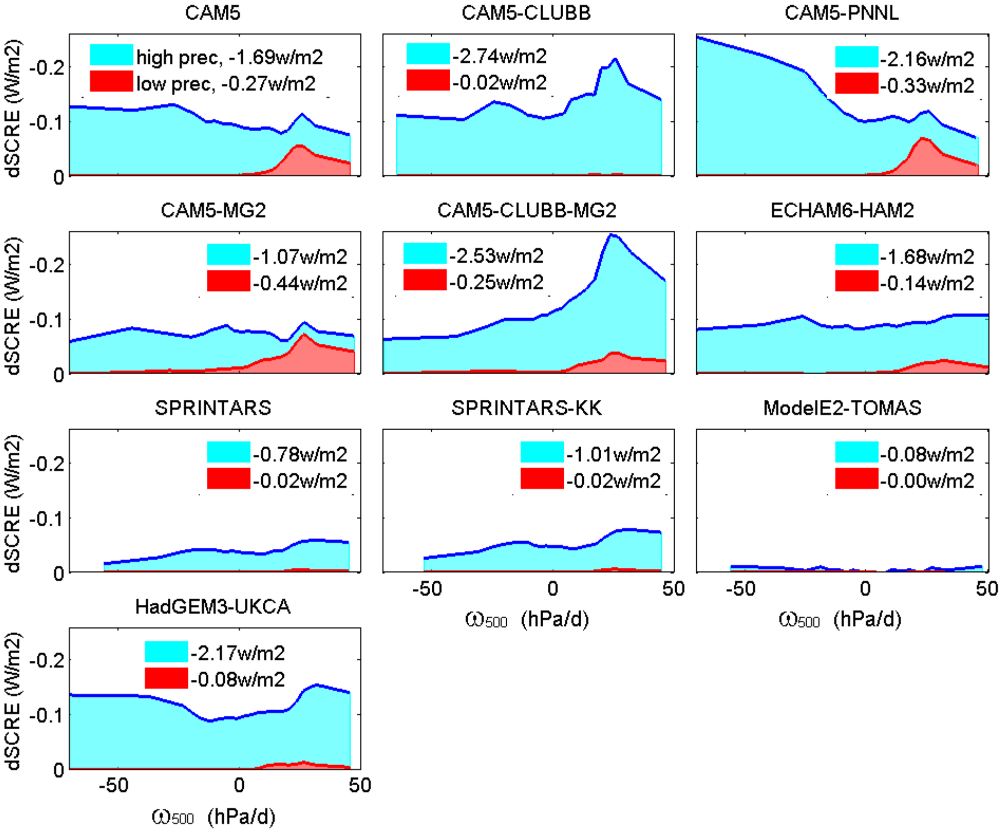

Fig S6. Same as Figure 7, but for threshold=0.05 mm d^{-1}.

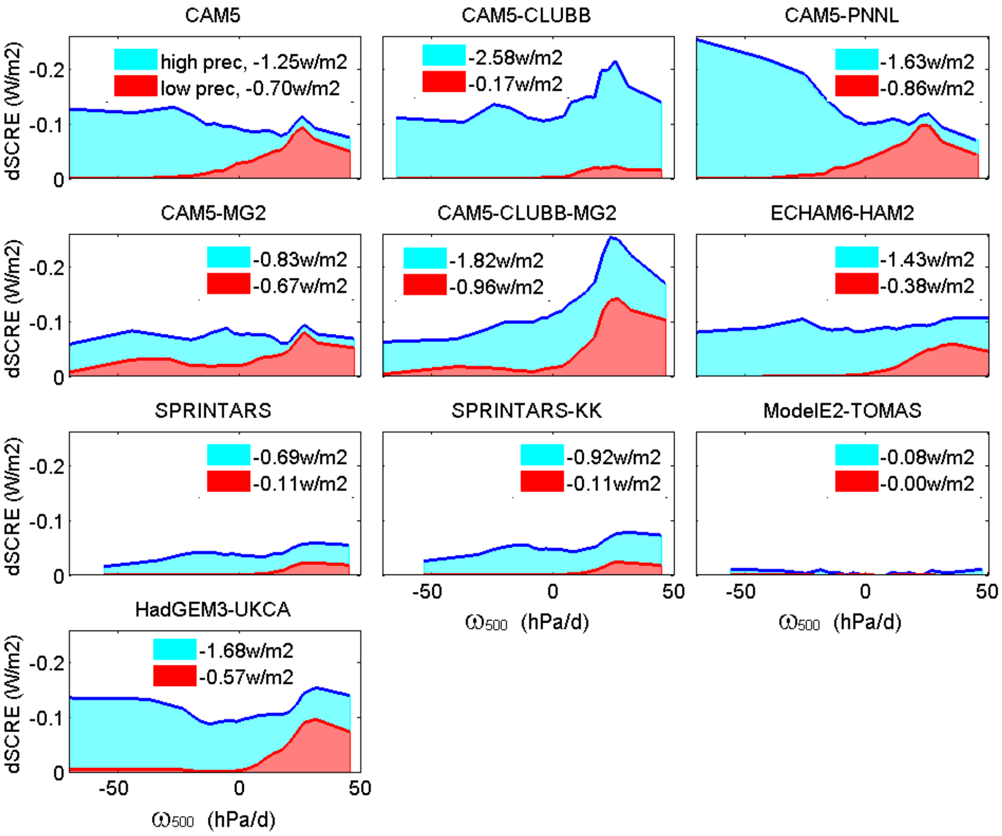

Fig S7. Same as Figure 7, but for threshold=0.2 mm d^{-1}.