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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the characteristics of aerosol indirect effect based on dynamic regimes in global climate models" *by* S. Zhang et al.

Anonymous Referee #2

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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 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).

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?

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 pro-

cesses, 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.

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

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.

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.

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).

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?

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

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.

References

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