Anonymous Referee #2

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In this paper, AMIP experiments with changes in all aerosol, sulphate aerosol, and non-sulfate aerosol are used to determine the effects of different aerosol types on the albedo of marine stratocumulus clouds. The main conclusion seems to be that the present day vs. pre-industrial changes in AOD due to anthropogenic aerosol, and hence any changes they induce in in cloud albedo, are small compared to AOD variability. The authors also find that, in their set of models, changes in cloud water content are more important to changes in cloud albedo than changes in AOD.

I found that the methodology was not always well explained, and that the approach was not always well justified. The authors discuss the parameterisation of cloud albedo effect in their models, but then make a strange choice to use AOD as their aerosol metric, despite the fact that this does not directly relate to cloud albedo in the models they use. This makes interpretation of their results very difficult. Aspects of the analysis presented in the paper suggests to me that the authors have the data to revise the study so that their analysis approach is better suited to the questions they are trying to answer, and I will discuss this in more detail below. As such, this paper may be suitable for publication following major revisions.

While the manuscript is reasonably written, it lacks a clear narrative. It will benefit from a clearer statement of the objectives, methodology, and conclusions. However, most of all, it will benefit from an approach that better relates aerosol changes to cloud albedo. I suspect that the issues with the writing will resolve themselves with improvements to the analysis, but the authors should bear this in mind when revising the paper.

We would like to thank the reviewer for his/her comments. We have revised the manuscript in Accordance with the reviewer's suggestions, and address each of the comments in the following.

Major comments This study adopts the method of Bender et al. (2016) for relating AOD to cloud albedo. However, there is no summary of this approach provided here, so that I had to refer to Bender et al. (2016) to know what the method was. I would like to see a revised version of the manuscript that is more self-contained in this respect.

We have made changes in the Introduction to explain the methodology clearer.

The authors have clearly taken the time to look at the parameterisation of the cloud albedo effect in their set of models, commenting on it on page 4, line 8, and again later in the paper. They correctly state that the parameterisation schemes differ in complexity, but don't comment on the fact that they also relate different aerosol species to cloud albedo. Most of my major issues with the manuscript relate to the authors approach to these parameterisation schemes. Firstly, the schemes in all the models used relate cloud droplet number concentration, not AOD, to cloud albedo. I am not convinced that AOD is trivially related to cloud droplet number concentration, and the authors make no real attempt to demonstrate this. Secondly, and most importantly, not all aerosol species affect cloud albedo in the models. Crucially for this work, dust does not directly interact with cloud albedo in any of the models considered here, despite accounting for most of the AOD, and its variability. A third point relates to this: cloud droplet number concentrations are related to different species in the different models. For example, HadGEM2-ES uses only sulphate and sea salt, while CSIRO-Mk3-6-0 also includes carbonaceous aerosol, and sea salt does not contribute to cloud droplet number concentration in IPSL (e.g. Wilcox et al., 2015; references in Table 1 of the submitted manuscript). These differences will have big influence on the regional responses in these models.

I think it is important that the authors repeat their analysis with an aerosol metric that is more closely related to the cloud albedo in the models: cloud droplet number concentration, or species-specific vertically integrated load. The authors already present some analysis of sulphate load, so I hope that this is not too onerous for them, and that it improves their results as I expect it will.

We would like to thank the reviewer for his/her comments. We realize that the way the manuscript was written, our intent with using AOD was not clear, and we have made changes to improve this, and included analysis of cloud droplet number, as suggested. As CDNC at cloud top or vertically resolved is not available for most models, we have used the column integrated cloud droplet number (CDN) in unit $1/m^2$, that all models except one provide.

As the reviewer points out, AOD is not trivially related to CDNC and not all aerosols that contribute to AOD contribute to CDNC. Accordingly, cloud albedo and scene albedo do not necessarily increase with increasing AOD. The AOD may be dominated by aerosols that are not active as CCN, and may also have contributions from absorbing aerosols that actually decrease the scene albedo if they are above the clouds. This is what satellite observations have indicated in previous studies, whereas models seem to have more trouble making these distinctions (Bender et al. 2016). In this study we separate aerosol types to look closer at these relations in the models.

We have made changes to the text according to the points made by the reviewer making clarifications regarding aerosol species contributing to AOD vs. contributing to CDNC, and the relation between AOD and CDNC. (see Section 2, Section 3.2)

We have also added analysis of CDN, parallel to the AOD-analysis, see Figures 3 and 7. As expected, the CDN consistently shows a positive gradient in albedo-cloud faction space (see new Figure 3).

As the issues raised by the reviewer here are quite similar to the major comment made by Reviewer 1, we also refer to the reply to that comment, for further explanation.

Minor comments Page 2, Line 5: I would need to read Ackerman et al., but my understanding is that absorbing aerosols reduce cloud cover by causing a local heating, rather than anything to do with their efficiency as CCN. Since the role of absorbing aerosol is key to the discussion later in the paper, it would be nice for the authors to clarify this a bit more.
Thank you, this has been clarified.

Page 3, line 2 (and later): The authors state that absorbing aerosols overlying the cloud is not well represented in models. This is a point that is revisited later. As this is one of the things investigated in the paper, I would like to see a bit more background on this (not necessarily at this point in the paper). Is this poorly represented because of shortcomings in the modelled cloud distribution, aerosol distribution, aerosol properties, etc.? How might these things affect the result? My suspicion is that it is a combination of several factors, including a tendency of models to underestimate the amount of aerosol above the cloud layer (e.g. Peers et al., 2016), and to underestimate atmospheric heating due to carbonaceous aerosols (e.g. Myhre and Samset, 2015).
We have expanded this discussion and added the suggested references. We are currently working on a closer investigation of the reasons for discrepancies, and test the sensitivity of the vertical aerosol profile to various parameters in NorESM, similar to the way Kipling et al. 2016 investigate HadGEM3. This reference is also added.

Page 3, line 13 (and later): The authors mention the large inter-model variations in global-mean AOD. For the interpretation of their results, I think it is important to highlight that properties such as aerosol mass and number, which are more closely related to modelled cloud albedo, are even more diverse, and not so easily tuned to observations.
Model diversity in AOD change due to differences in aerosol mass and number and the parameterization of radiative properties of different aerosol types. Properties like aerosol mass and number as well as cloud droplet number are indeed poorly constrained by observations, allowing for large inter-model variation. We have highlighted this in Section 3.1 and 3.2.

Page 3, line 30: Aerosol particles are not parameterised in models, but their interactions with clouds are. However, modelled aerosols are idealised compared to the real world, so perhaps that is what the authors mean here.
Of course, we have replaced the word “parameterised” with “represented”.

Page 5, line 26: Dust and sea salt account for most of the mass, but do they account for most of the change?
No, the aerosol types that contribute to the changes between experiments are those with anthropogenic emission sources, sulfate in the Sulfate case, and BC and OM in the Non Sulfate case, as mentioned in Section 2. This has been clarified.

Page 6, lines 16 to 26: This section could benefit from a bit more rigour. Are differences significant?
We have updated significant statement in the text. All changes are significant except for two models in two different regions.

Page 7: The section is lacking mentions of dust, which you might expect to be important in these regions. In the models, it won't affect the cloud albedo, but it might affect the clouds by changing the heating profiles.
Dust is important in some of the regions studied, as mentioned in Sections 3.2, 3.3. and 3.4. In Section 3.4, we now point out that the dust may affect clouds by changing the heating profiles.

Page 8, line 16: How does this change compare to the preindustrial values?
Relative differences between sensitivity experiments and preindustrial case have been updated in the text.

Page 9, line 15: The authors suggest that LWP is more important for cloud albedo than anthropogenic aerosol change, and should include some context from previous publications here, e.g. Gettleman (2015).
Thanks for the reference. We added it to Sections 3.3 and 4.

Page 11, line 23: Species dependence is in fact hard-coded in the models.
This has been clarified.

Figure 6/7: One of the main conclusions in the text is that the correlations in Figure 7 are much stronger than those in Figure 6. The authors should quantify this, as it is not obvious by eye.
Figures 6-7 have been replaced with a new figure, showing a correlation matrix between different variables and investigating further their relation to cloud albedo changes

Table 3: The caption says that all of the correlation coefficients in this table are significant at the 95% level (this should say 5% level). I find this surprising given the magnitude of some of the coefficients (0.01, 0.02, 0.03,. . .), and what I think is only a small number of data points (20 years for sstClim experiments? – this should be stated in the text).
This table has been removed. The new figure 7 includes the correlation between AOD and pre-industrial sulfate loading. The focus is on sulfate loading rather than other aeorosl types, since it is expected to be the main driver of AOD changes between the used CMIP5 experiments.

Gettelman (2015) Putting the clouds back in aerosol–cloud interactions doi: 10.5194/acp-15-12397-2015
Myhre and Samset (2015) Standard climate models radiation codes underestimate black carbon radiative forcing doi: 10.5194/acp-15-2883-2015
Peers et al. (2016) Comparison of aerosol optical properties above clouds between POLDER and AeroCom models over the South East Atlantic Ocean during the fire season, doi: 10.1002/2016GL068222
Wilcox et al. (2015) Quantifying sources of inter-model diversity in the cloud albedo effect doi: 10.1002/2015GL063301
Thank you for the references. We added the suggested new references.

Note: A new output version for the model CSIRO was provided on the ESGF data archive and was used for the analysis in the revised manuscript.