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Interactive comment on "Cloud albedo changes in response to anthropogenic sulfate and non-sulfate aerosol forcings in CMIP5 models" *by* Lena Frey et al.

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

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Discussion paper

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.

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.

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.

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.

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).

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.

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.

Page 5, line 26: Dust and sea salt account for most of the mass, but do they account for most of the change?

Page 6, lines 16 to 26: This section could benefit from a bit more rigour. Are differences significant?

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.

Page 8, line 16: How does this change compare to the preindustrial values?

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).

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

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.

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).

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

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