Anonymous Referee #1 **(Review comments in bold;** response in regular type.)

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The authors present a study of the impact of using different meteorological fields, here from the CAM5 and AM5 models, on the output of a global aerosol model (IMPACT). This is a highly relevant topic, as a good fraction of the remaining uncertainty in modeling of aerosol radiative forcing may well be linked to descriptions of water content, clouds, precipitation, vertical fluxes etc. The study is thorougly performed and documented, and the paper is well written. I recommend that it proceed to be published in ACP, however I do have a small number of comments and requests for clarification.

General comments

As a general comment, while I enjoy the details presented in the study I find I miss a closer connection to the ongoing process of understanding the difference between aerosol models. As stated in the introduction, comparison of coupled aerosol/climate models typically combine differences because of diverse treatments of atmospheric aerosol processes and because of varying meteorological fields. Given the difference seen here between two different meteorologies, how much of the differences seen e.g. in the list of references given (Penner et al., 2002, 2006; Kinne et al., 2006; Schulz et al., 2006; Textor et al., 2006, 2007; Shindell et al., 2008, ...) can be explained? This can of course not be thorougly quantified from two fields alone, but a closer link would strengthen the impact of the paper.

While we appreciate the desire to explain differences that appear in the literature, this is extremely difficult given that we do not have access to the meteorological fields nor most of the aerosol models used in the studies quoted by the reviewer. In addition, many of the GCM fields used in those studies as well as the aerosol models have evolved since these studies were published. We find it of interest, however, that the range of direct forcing estimates that we report here, from -0.25 Wm^{-2} to -0.48 Wm^{-2} for the A1 and C1 models and up to – 0.64 Wm^{-2} for C2 is almost as large as the range reported in Schulz et al. (2006), which was 0.04 Wm^{-2} to -0.41 Wm^{-2} even though we use the same aerosol model in all our simulations. In contrast, the range in indirect forcing calculated here is only -1.26 to -1.74 Wm^{-2} which is significantly smaller than the range reported in Penner et al. (2006). However, without a better understanding of what causes these different results, we do not feel it is appropriate to comment on this in the paper.

Currently, the authors state in the conclusion (p10701) that "Today, there is a large emphasize on determining aerosol affects and their interactions with clouds and precipitation. However, our study highlights that as long as the hydrological cycles simulated by different GCMs do not converge, the aerosol fields, direct effects, and aerosol indirect effects will differ due to this factor alone. Thus, at least as much effort should be put into examining this aspect of GCMs as on examining aerosol indirect effects." I agree, and believe the present analysis could be further exploited to quantify the difference. Regarding the simulation setup: The authors have used 2-year consequtive meteorological fields (p10684, l21) from both input models. The emphasis of the analysis is on comparisons of the models, but not so much on the internal variability of each model. Have the authors looked at the effects of using different input years from a single model? Are the differences

seen, e.g. in cloud mass flux and subsequent aerosol loading, robust? I would assume so, but cannot tell to what degree this is from the present analysis.
Due to the limited computer time, for each case we only ran for 2 years. However, we compared five consecutive years meteorological fields from CAM5 and AM3. We found that: 1) The difference of simulated aerosol burden/lifetime caused by the interannual variability (based on the 2 years of simulation) is much smaller than that caused by the differing meteorological fields from the two models; 2) The comparison of the identified meteorological variables, like convective mass flux, large-scale precipitation, in-cloud water content, cloud fields and relative humidity which determine the differences in the simulated aerosol burden/lifetime, direct forcing and indirect forcing still hold for the 5-year data. So we believe that the simulated aerosol differences between the two models are robust though the interannual variability may diminish such differences to a slight extent. We will add a comment to the revised text.

I find I also miss comparisons of the radiative forcing of individual species, especially BC, OA and SO4, but I realize that this requires additional simulations and may be beyond the scope of the present analysis.
Due to the limited computer time we had, we did not calculate the radiative forcing of individual species.

<u>**Technical notes and questions**</u>

p10682,l7: "3-mode version of IMPACT" used twice in two sentences

The second occurrence of “3-mode version of IMPACT” has been replaced by “it”.

p10685,l3: Regarding Figure 1, would propose showing (in addition) the vector difference fields between CAM5 and AM3 results - at least for one or two combinations. It’s a bit hard to spot the difference between two vector fields visually.
The vector difference between CAM5 and AM3 will be provided as supplementary material.

p10688,l6: The connection between condensed water content and precipitation amount is crucial for the later discussion, but I do not find the explanation of why increasing it decreases precipitation quite clear. Clarify?
The relationship between condensed water content and wet deposition strength has been explained in the introduction of the IMPACT model (section 2.1, p10683, line 9- line 15). We added “(see section 2.1) ” after line 6.

p10696,l25: "...two major factors..." Could the difference in BC vertical profiles in the Arctic, as shown in figure 9, also have an impact here? The A1 field is more markedly peaked at high altitudes than C1 and C1 concentration is generally higher. E.g. Shindell and Faluvegi, 2009, show a difference in forcing sign from BC in the Arctic when the forcing is applied low (local emissions) or high (transported), and in a region where the sensitivity of BC forcing to altitude is very strong (e.g. Samset and Myhre, 2011).
We could not exclude this possibility. We added following in the revision.

"The third possible reason is that the BC from A1 is more markedly peaked at higher altitudes than that from C1 (see Figure 9). The direct forcing of BC is sensitive to the altitude of BC (Samset and Myhre, 2011) and can change from cooling to warming when BC is placed above clouds (Shindell and Faluvegi, 2009)."

p10701,l12: emphasize -> emphasis

Done