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Interactive comment on "Evaluation of preindustrial to present-day black carbon and its albedo forcing from ACCMIP (Atmospheric Chemistry and Climate Model Intercomparison Project)" *by* Y. H. Lee et al.

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We thank the reviewer for their constructive comments and suggestions and have made several changes to the paper to address the issues raised. Reviewers' comments are shown in italics with our response shown after each.

General comments

Overall, this work is an important first step to understand how state-of-the-art global models treat black carbon and it's effect on snow albedo forcing. After addressing the

questions herein, it should be published. There was a great deal of effort made to intercompare models to observations of BC in snow, though as the first reviewer points out more description of the observational techniques is required. One concern is the design of the experiments and some of the jargon you use. In line 19 of page 21720 you state that "Each timeslice ran for 4 to 10 years to obtain statistically significant results." First, what do you mean by "statistically significant"? Nowhere throughout the paper do you define this term. Secondly, if something is "statistically significant" it is relative to something else – and this something else is completely unclear. You do not present any hypothesis testing in the paper (e.g. t-test results). The only statistics presented are the multi-model mean and relative standard deviation. At any rate, your integrations are far too short to do any meaningful hypothesis testing.

Response: We agree that the term of "statistically significant" can be misleading, so the sentence is rewritten in the following.
OLD: "Each timeslice ran for 4 to 10 years to obtain statistically significant results."

NEW: "Each timeslice ran for 4 to 10 years to reduce the amount of interannual variability; this reduces the noise in the computed changes (between simulations) and therefore increases the likelihood of relating them to the associated forcings."

Regarding the experimental design, you state that some models are driven by offline meteorology, implying that others are GCMs that evolve their own meteorology. This presents an issue – if the evolved meteorology is very different between the models the differences in transport and precipitation may overshadow any differences due to the treatment of, for example, BC scavenging parameteriztions.

Response: We certainly agree that the spread of BC predictions among GCMs is affected by the differences in parameterizations used for BC modeling as well as in their simulated meteorology. This is unavoidable in GCM free-running (i.e. not constrained by observations) simulations. As an attempt to reduce the impact on BC by the different GCM evolved meteorology, a set of simulations requested under the ACCMIP was to run with MERRA reanalysis meteorology from year 2000 to 2010. Unfortunately, only two models completed these simulations. Based on our new analysis on precipitation evaluation (see our response on the Dr. Hagler's comment on precipitation for the details), at least for Arctic regions, the differences in precipitation among ACCMIP models are quite small and the ACCMIP models' simulated precipitation agree reasonably with the observation.

Secondly, in the GCMs, does aerosol forcing feedback onto the model radiation fields? In this case, the aerosols may influence the meteorology of the model – something that does not happen in the offline-meteorology driven models. Thus, it seems less of an "apples to apples" comparison when the meteorology is offline or online.

Response: Among 8 models used in the study, only one model (i.e. CICERO-OsloCTM2) is a CTM using offline meteorology and others are GCMs that have the meteorology responding to the aerosol-perturbed radiation fields. Also we did not notice any exceptional behavior in their BC predictions compared to other GCMs.

Minor suggestions: None

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21713, 2012.