Reply to reviews, Doherty et al. 2014
All replies are given in italicized text below the comment.

REVIEWER #1

Review of "Biases in modeled surface snow BC mixing ratios in prescribed aerosol climate model runs" by Doherty et al. 2014 ACPD.

This paper investigates a bias in model BC concentrations in snow, when the BC concentration in snow is computed in the CESM1 model with prescribed BC deposition flux. The paper demonstrates that the BC mixing ratio (in snow) could be overestimated by using temporally smoothed prescribed BC deposition fluxes with model precipitation that varies on meteorological timescale, especially when small amount of snowfall in CESM concurs with high prescribed BC deposition flux (resulting in unrealistically high BC mixing ratios in snow). The authors suggest an alternative approach to limit the bias in prescribed run, which can be easily applied in future modeling study. I recommend this manuscript be accepted with major revisions. Please see my comments below.

Since this paper deals with a technical approach about offline BC-albedo modeling, I'd like to suggest this paper to be published as a technical note.

A technical note would likely only be read by CESM model developers. Given that the bias we've identified affects the results of studies already published in ACP we feel it is important that this study be published as a regular paper. In addition, other modeling groups are in the process of updating their models to more comprehensively account for forcing by BC and other particles in snow, and the issues raised here apply to any land surface scheme that 1) is driven with temporally inconsistent aerosol and precipitation fluxes, and 2) uses multiple snow layers. This paper serves as a caution to these groups about how to approach modeling this effect.

Major comments

1) Although I agree with the authors on the potential bias in the offline BC-snow modeling, I think the authors should use prognostic model results as a benchmarking rather than another offline method with prescribed BC deposition fluxes (i.e. [MRBC]y or [MRBC]m in Page 13175) – "prognostic model results" mean prognostic BC mixing ratio predictions that are computed with prognostic BC deposition flux. (Reading the title of the paper, I expected this paper to explore the biases in BC mixing ratios in prescribed runs, compared to prognostic runs.)

This is a good suggestion, and a comparison between prognostic-aerosol and prescribed-aerosol runs of CESM1 have been added to the paper. Unfortunately we did not have the resources to do a set of prognostic runs that could be directly compared with our prescribed runs, so we have used a 30-member ensemble of runs that is publicly available for download. These runs are based on the same emissions

year/fluxes as our prescribed runs, though they use CAM5 rather than CAM4, so they are very similar but not identical. We discuss this in the revised text.

The authors should demonstrate that the mean and variability shown in the prognostic BC mixing ratio results are indeed similar to [MRBC]m or [MRBC]y.

This comparison really can't be made, because the prognostic runs have different BC wet deposition fluxes and snowfall rates than the prescribed-aerosol runs, and therefore also than in our offline calculations. As noted in the text, BC wet deposition fluxes in snow were not provided in the diagnostics from the prognostic-run data set we used. In addition, in the prognostic runs MR_BC is influenced by in-snow processes (melting, sublimation).
The offline calculations are intended to isolate the impact of how BC deposition is scaled with snowfall and do not include all factors that influence the mean/median/range in surface snow BC mixing ratios. We hope the reviewer finds it sufficient that we have 1) provided a physical argument for why we expect a high bias, 2) shown that the ratio of MR_BC from the prescribed vs. prognostic runs is very similar to [MR_BC]d:[MR_BC]y in our offline calculations, after accounting for differences in total snowfall and BC mass deposition in the prescribed vs. prognostic runs.

Related to prognostic BC mixing ratio, the authors mentioned some results in page 13178; line3-15 (in the discussion and conclusions section), but it's too brief. Please include them in the result section and explain in more details.

These results are now included in the results section, in addition to the comparison of the ensembles of prescribed-aerosol vs. prognostic-aerosol model runs.

2) The authors claim that [MRBC]y is a more realistic representation of surface snow BC mixing ratios than [MRBC]d because their variability and mean are well compared to the observation (in Page 13180). It is very nice that the authors attempted to compare the offline model results to the observed mean and variability, and, I think, this comparison should be presented in Section 3 (results). However, I have a couple of concerns on this.
a) It wasn't clear what type of BC mixing ratio is used (daily or monthly or seasonal BC mixing ratios?).
b) I thought that this finding is rather predictable, as the measurements tend to represent the seasonal BC mixing ratio (e.g., sampling one time in a season) and thus to have much less variability.
c) I expect that variability in [MRBC]d may be mainly driven by temporal variability, while the observed variability might represent spatial variability more. If so, this comparison is not reasonable.

Based in particular on Reviewer #2's comments, as well as on the issues raised by this reviewer in points a) to c) above, we have decided to remove the comparison to observed snow mixing ratios. As pointed out by this reviewer, there is the issue of the

comparability of time/spatial-scale averaging in the model versus the observations. In addition, there are other potential sources of error/bias in the models that could affect the comparison, so it does not do much to strengthen the point we are trying to make. We think it is better to let the physical argument, the newly-added prescribed-vs-prognostic model comparison and offline calculations stand on their own.

3) The author raised a point that the BC albedo forcing estimates used in IPCC AR5 is overestimated due to this bias. This is one of the significant results in this paper. However, the authors did not consider and mention that the preindustrial BC mixing ratio is suffering from this bias as well. If the preindustrial BC mixing ratio is also biased high, the overall impact in BC albedo forcing may be insignificant?? Please elaborate how this bias could influence the BC albedo forcing.

We don't ever say that the IPCC AR5 estimate is biased high; we just point out which model studies were affected by this bias.

If the pre-industrial BC mixing ratio is also biased high due to this effect there will still be a high bias in the industrial-era forcing because the forcing is based on the difference (not the ratio) of the radiative effect in both time periods. The difference should be biased by a similar factor as the absolute bias. In addition, because of the positive feedbacks in BC albedo forcing the difference will be amplified. Without conducting full simulations it's difficult to know how big the resulting high bias will be.

4) I felt that readability and clarity are lacking in some parts in this paper. Here, I list some suggestions.
a) Use Table to present the method.

DONE

b) Use a consistent run name (Choose either CESMmet run or CESM run. Similarly, CRUNCEPmet run or CRUNCEP run. CRUNCEP sometimes shows up as CRU/NCEP. I also see "NCARmet". Is that actually "CESMmet"?).

Thanks for catching this; NCARmet has been corrected to CESMmet throughout.

c) if possible and proper, utilize comma to improve the readability. One example is in page 13172;line 19 (.. ratios, we conducted..). I included a few examples in the minor comments, but there are more in the manuscript. So please search and correct them.

We have reviewed and revised the paper for readability and hope the reviewer now finds it improved.

5) I think Figure 5 to 7 do not add much information. Also the authors do not explain the spatial distributions shown in them at all. Since Figure 8 present the same data presented in Figure 5-7 in the histogram, I think Figure 5-7 should be removed.

Figure 8 highlights seasonal variations in the bias, whereas Figures 5-7 showed spatial variations in the bias. While we believe it is beyond the scope of the paper to explore in much detail the spatial distributions in these figures, some readers may be interested in how previous studies using CESM with prescribed aerosols were biased in specific sub-regions of our three regions. We think these figures are therefore useful but are not critical to the analysis presented in the main paper, so we have kept them but moved to a Supplemental section.

In any case, I was wondering why the color-bar scale in Figure 5 doesn't go below 1, while those in Figures 6 and 7 start from zero.

Apologies; that was simply an oversight. The scale for all of these figures snow starts at zero. Thanks for catching this.

Minor comments

1) In Abstract– Right after the first sentence, it would be helpful for readers if you explain that BC mixing ratio in snow is a key variable for BC albedo forcing and describe the bias a little more. The second sentence alone doesn't seems to be enough.

DONE – Added a lead-in sentence to this effect.

2) Page 13170: line 1-2 (In addition, the reduction ... larger-grained snow) – Do you have a reference for this?

Yes, and it has been added.

3) Page 13171: line 13 – Lee et al. (2013) and Shindell et al. (2013) are basically the same ACCMIP study. Providing both papers here gives me an impression that they are two different studies. It is better to cite Lee et al. (2013) as that is the ACCMIP paper covering BC albedo effect in details.

DONE

4) Page 13171; line 12-20 – This paragraph doesn't seem to belong in the introduction.

Reworded so this paragraph states what we do in the study, rather than stating the result.

Also, which of your results support the sentence ("Here we show that the use of prescribed… would be given by runs with prognostic aerosol deposition")? I don't think you present BC mixing ratio computed based prognostic aerosol deposition. (This is related to one of my major comments)

As noted above, we also added a comparison to an ensemble of prognostic model runs, and the comparison support this statement.

5) Page 13171; line 25 ("These prognostic model runs are initialized with emissions") - the word "initialize" is inappropriate. Please rewrite the sentence. You could say, "in these prognostic model runs, aerosols are emitted directly or formed from aerosol precursor...".

DONE

6) Page 13172; line 3 - "tropospheric BC concentration"--> "ambient BC concentration".

DONE

7) Page 13172; line 12 – add comma between "studies" and "these".

DONE

8) Page 13172; line 12-15 – It's better to define what CRU and NCEP standard for and then use the abbreviation.

DONE

9) Equation 1 – Please explain what each term means (i.e., day n dry deposition + day n wet deposition + day n-1 contribution)

DONE – added a sentence to this effect.

10) Page 13173; line 19 ("BC dep,wet") – "deposition rates" should be "deposition fluxes". The unit is for flux, not rate. Also, the unit seems wrong [ng m-2 -sec]. Instead of "-sec", it should be "day-1".

Thanks for the corrections and for noting the error. These have been fixed.

11) Page 13174; line 9 - CAM4.0->CAM4

DONE

12) Page 13175; line 3 – the phrase "three different calculations for MR" was very confusing to me, until I read more below. Either organize the sentences to avoid any confusion or refer to a Table (if you make a Table to summarize the three different MRBC,snowfall calculations).

The entire section describing the model runs and offline calculations has been

significantly modified, and a table giving an overview of both added (Table 1).

13) Page 13175; line 14-24 - Can you present this also in Table?
See my major comment for run name.

DONE

14) Page 13176; line 23 - missing comma between two MR.

This was a transcribing error. The comma is in the original Word .doc.

15) Page 13177; line 3 – don't --> do not

DONE

16) Page 13178; line 9 – please remove Shindell et al. (2013). Shindell et al (2013) uses the results from Lee et al. (2013).

DONE

17) Page 13178; line 26 – ACC-MIP ACCMIP

DONE

18) Page 13179; line 3 – seasonally-averaged seasonally averaged

DONE

19) Page 13179; line 25 – Is "conclude" right choice? Maybe "assume" is better.

This has been changed to "estimate". We think the physical arguments plus our model comparisons and offline calculations allow for a stronger statement than "assume", but agree that "conclude" was to definite.

21) Page 13180; line 14-24 – When comparing to observation, did you use seasonal-mean of BC mixing ratio or daily mixing ratio? Also, when making a seasonal average, it seems more reasonable to use snowfall weighted mean, not just simple arithmetic mean. Looking at Figure 2, I guess you use simple arithmetic mean.

The comparison to observations has been removed.
However: The average given was a simple average. The point about using a snowfall-weighted average is a good one. Snowfall-weighted averages (as well as medians) of the snowfall BC mixing ratio from the offline calculations are included in the new Table 3, and are discussed in the Results.

22) Page 13180; line 30 – "sunlight usually will" --> "sunlight usually can"

DONE

23) Page 13183; line 3-10 – I can't understand this paragraph. Can you please rephrase this?

This has been rephrased as follows: "While this will produce an inconsistency in the mass balance of BC within the prescribed model runs (i.e. the change with time in the mass of BC in the atmosphere will not equal BC minus BC deposited to the surface within the prescribed–aerosol runs), both the atmospheric BC concentrations and surface snow BC mixing ratios in the model calculation will be physically more realistic. This is preferable to maintaining mass balance within the prescribed-aerosol run since both the atmospheric concentrations and deposition rates are anyhow prescribed, and the climatically important variable in studies of albedo forcing is the surface snow BC mixing ratio."

24) Table 1 – NCARmet --> CESMmet

DONE

25) Figure 1 – CAM4.0 -->CAM4

DONE

26) Figure 2 – I am puzzled by the mean values in Figure 2: the mean of [MRBC,snowfall]d is 1e26 times higher than the other values! Isn't this mean supposed to be annual mean? But did you actually use simple arithmetic mean of daily mixing ratio to compute seasonal or annual mean in the paper? If so, I can't understand how [MRBC]d is only a factor of two higher. Please either correct the mean or explain how the huge difference in [MRBC,snowfall]d results in a factor of two difference in [MRBC]d.

Yes, we used a simple arithmetic mean. As we hope is now clear in the revised text, [MRBC]d is not calculated using [MRBC,snowfall]d, since the latter goes to zero when snowfall is zero. Instead, BC that is wet-deposited has no dependence on snowfall rate. This is now spelled out much more clearly.

27) Figure 3 -- Please state clearly that [MRBC]model uses prescribed BC deposition fluxes, which is also affected by the bias.

DONE

28) Figure 4 – I'd like to suggest to merge Figure 3 and 4, by adding the red shaded area into Figure 4. The caption has a typo: [MRBC]d (green x's) --> [MRBC]y (green x's)

Good idea; we have done this. And thanks for catching the error.