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.

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. $[MR_{BC}]_y$ or $[MR_{BC}]_m$ 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.) The authors should demonstrate that the mean and variability shown in the prognostic BC mixing ratio results are indeed similar to $[MR_{BC}]_m$ or $[MR_{BC}]_y$. 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.

2) The authors claim that $[MR_{BC}]_y$ is a more realistic representation of surface snow BC mixing ratios than $[MR_{BC}]_d$ 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 $[MR_{BC}]_d$ may be mainly driven by temporal variability, while the observed variability might represent spatial variability more. If so, this comparison is not reasonable.

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.

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. 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"?). 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.

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. 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.

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.
2) Page 13170: line 1-2 (In addition, the reduction ... larger-grained snow) – Do you have a reference for this?
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.
4) Page 13171; line 12-20 – This paragraph doesn't seem to belong in the introduction. 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)
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…".
6) Page 13172; line 3 - "tropospheric BC concentration" → "ambient BC concentration"

7) Page 13172; line 12 – add comma between "studies" and "these". I recommend searching the entire text to find the similar problem.

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

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

10) Page 13173; line 19 ("BC dep,wet") – "deposition rates" should be "depositon 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".

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

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 $MR_{BC,snowfall}$ calculations).

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

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

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

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

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

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

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

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.

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

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

24) Table 1 – NCARmet ➜ CESMmet

25) Figure 1 – CAM4.0 ➜CAM4

26) Figure 2 – I am puzzled by the mean values in Figure 2: the mean of $[MR_{BC,snowfall}]_d$ 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 $[MR_{BC}]_d$ is only a factor of two higher. Please either correct the mean or explain how the huge difference in $[MR_{BC,snowfall}]_d$ results in a factor of two difference in $[MR_{BC}]_d$.

27) Figure 3 – Please state clearly that $[MR_{BC}]_{model}$ uses prescribed BC deposition fluxes, which is also affected by the bias.

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: $[MR_{BC}]_d$ (green x's) ➜ $[MR_{BC}]_y$ (green x's)