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.

REVIEWER #2

The paper *Biases in modeled surface snow BC mixing ratios in prescribed aerosol climate model runs* discusses potential biases in the mixing ratio of BC in snow and its potential impacts on snow albedo due to the methodology used in several recent multi-model studies when prescribed averaged BC deposition rates from the models combined time-varying snowfall rates in the CESM model. Since these papers (e.g. Lee et al. 2013; Jiao et al., 2014) have formed an important basis for current understanding of the BC snow-albedo effect, I find that the objective of the study is important that it should be published, however after some major revisions.

The focus of the study is the identification and quantification of a bias. However, the authors should make it clearer exactly what kind of bias is the study about.
1. The bias between the real MR of BC in surface snow and the modeled MR using the prescribed deposition rates (i.e. problems when comparing with measured BC concentrations)
2. Bias versus the on-line simulations with CESM.
3. As a bias in the climate impact of BC-snow-albedo effect.
I find that 1 and 2 is really mixed in the papers and the discussion should be clarified. Alternative 3 above is not spelled out in the paper, but since the climate impact is the main reason for studying BC in snow at all, many readers would interpret the results (the factor 1.5-2.5 bias in the conclusion) as a measure of a bias in the climate impact. Since a very significant part of the climate impact will be caused by the absorption during the melting season it seems much less clear if this interpretation is valid.

The bias being investigated is 2. above, but it has direct implications for 3., since several prominent estimates of albedo forcing by BC in snow have been conducted using prescribed-aerosol runs of CESM. The last sentence of the introduction we hope now makes this clear: "Here we test whether the use of prescribed BC mass deposition rates in CESM, as was done in the Goldenson et al. (2012), Lee et al. (2013) and Jiao et al. (2014) studies, produces a bias in surface snow BC mixing ratios. The bias being investigated would result from the fact that BC deposition fluxes in CESM prescribed-aerosol runs are decoupled from snow deposition rates. Note that the bias being tested for here is independent of any biases due to errors in input emissions or in modeled transport and scavenging rates; it is purely a result of the mathematical approach taken in the model to estimating surface snow BC mixing ratios."

Major comments:
My main concern with the presentation of the analysis in the paper is that it in several places lacks the detail of information about the methods used that is necessary to evaluate the conclusions drawn. This relates to exactly how the

coupling between prescribed deposition fields and modeled snowfall rates are done (here, and in the previous papers). This is crucial since it relates to key questions such as conservation of mass in the different procedures.

1. Page 13172 Line 13. The main objective of the paper is to show and quantify biases in the methods applied by e.g. Lee et al., and Jiao et al. To really be able to understand the origin of a potential bias we need to know in more detail how the "coupling" between (monthly mean) deposition fields and the daily resolved meteorology (i.e. snowfall rates) are done by Lee et al. and Jiao et al. I have not been able to understand in detail how this was done by reading the Lee et al., and Jiao et al. papers, so some further details should be given here. If the deposition is treated as given by eq. 1 in your paper, then what happens on days without snowfall (SWEnsnowfall=0). Since BCdepnwet is non-zero (a monthly mean) that means that fn=0 while MRnBC, snowfall would be infinite. I assume that the product fnx MRnBC, snowfall in eq. 1 is then set to zero.

The paper now shows explicitly (new Eqn. 1) how this calculation is done in the Lee et al. and Jiao et al. studies. In fact BC deposition and snow deposition are completely independent, so BC "wet deposition" is >0 even in the absence of any snowfall (fn-0), effectively leading to infinite-mixing-ratio "snowfall". This is now discussed clearly in the text.

2. I agree that there will be a positive bias in the MR of BC in the snowfall in the offline simulations on days with very low precipitation rates. However, the paper focuses only on this and I believe that there could also be other (and probably negative) biases using this method that is not discussed at all.

The paper does not focus only on positive sources of bias. It provides a physical basis for why one would expect to find a bias, then it shows that in fact there is a bias using model comparisons and offline calculations, where the latter in particular isolates the identified effect. As noted in the following text from the paper, the decoupling of BC and snow deposition can lead to either positive or negative biases in the mixing ratio of BC in snowfall on a given day/gridbox, but the former will have a greater influence on surface snow mixing ratios: "Since the sum of a series of ratios (MRBC,snowfall) does not equal the ratio of summed numerators (BCdepwet) and denominators (SWEsnowfall), we expect this decoupling of deposition and snowfall will lead to errors in MRBC. In addition, if there is a large amount of new snowfall, MRBC,snowfall will be anomalously low, but much of this low-mixing-ratio snow will be buried in the snowpack where less (or no) sunlight interacts with it. In contrast, if there is only a small amount of new snowfall, MRBC,snowfall will be anomalously high, and this high-mixing-ratio snow will be near the snow surface and interact with sunlight. In a model with multiple snow layers that are divided with snow accumulation, the mixing ratio in the top-most model snow layer will thus be biased high. The magnitude of the high bias will depend on the model's top snow layer thickness. In this way<u>, low snowfall/high MRBC,snowfall precipitation events will have a greater influence on time-averaged snow albedo than high snowfall/low MRBC,snowfall precipitation</u>

events."

If my assumption above (that the product fnx MRnBC, snowfall in eq. 1 is then set to zero whenever fn=0), this would result in a negative bias (compared to the deposition in the prognostic, e.g. The ACCMIP, model) in total BC deposited during the month (BC mass in the deposition is not conserved). This might be quite important for the albedo during the melting season in the high Arctic and for the whole winter season at lower latitudes where melting episodes occur throughout the winter.

This assumption is incorrect. We apologize that the text was not clear on this point. We have edited the text to show explicitly how the calculation is done in CESM. In prescribed-aerosol runs, the BC wet deposition flux is positive even when new snowfall (fn) is zero. Effectively, yes, this means "infinite mixing ratio snowfall" as fn approaches zero. The total mass of BC deposited and snow water deposited (snowfall) in each gridbox/month is the same in all offline calculations as it is in the prescribed-aerosol mode runs. The only difference is in the relative timing of the deposition of BC vs snowfall.

It seems that the potential negative bias in BC mass in the snow column (and thus in the MR of BC near the surface after partial melting) can be tested based on your simulations described on page 13175. Is there a negative bias in the total BC column mass in the snow using method 1 compared to using method 2 or 3 (on page 13175)? Could this be the cause for the values less than 1.0 in many grid boxes in figures 6 and 7? However, you state (page 13177, line 3) that the off-line calculations do not include sublimation and melting. Does that mean that the potential negative bias would not be accounted for in your calculations?

As we hope is now clear, the same total mass of BC is deposited to snow in all three cases. The effects of sublimation and melting are not included in any of the three sets of offline calculations. In fact if they were included we expect the bias would be even greater, since both melt and sublimation are expected to be greater in snow with higher BC mixing ratios, and increased melt and sublimation both, in turn, lead to higher BC mixing ratios (Flanner et al., 2007; Bond et al., 2013).

3. It is stated several places in the paper (without any reference) that BC deposition rates (in the real world) scales by precipitation rates. I don't agree with this statement. If the BC is very effectively scavenged by the precipitation, then the concentration in the air will be depleted and the MR of BC in the snowfall will go down. Further increase in the precipitation should not lead to increase in the deposition rate. In reality I would expect that the deposition rate is a non-linear function of precipitation rate. This is a key point in the analysis. In light of this I would like to see a short discussion of the physical processes leading to wet deposition of BC and how these are treated in models in general, in and CESM In particular.

A discussion of wet deposition processes is beyond the scope of this paper, which focuses on a bias caused by the method of calculating surface snow mixing ratios, though we do now point out that BC deposition fluxes depend on BC ambient concentrations, scavenging efficiency and snowfall rates. The reviewer's point that BC deposition rates won't scale in a fixed way with precipitation is correct, but BC wet deposition rates certainly are a function of precipitation rates – and certainly go to zero as precipitation goes to zero. We present a physical explanation for why removing this dependence altogether will lead to a high bias, and now support this with a comparisons of prognostic-aerosol vs. prescribed-aeorosol model run ensembles and estimate the magnitude of the effect of decoupling the two with offline calculations that isolate this effect. In the discussion, we have reworded our conclusion so that the results of the offline calculations are presented as an "estimate" of the magnitude of the bias.

Minor comments:
Figure 2. I would have lied to see a panel with the distribution of MR(BC, snowfall) also for the prognostic run with CESM.

Unfortunately the diagnostics needed to show this (BC wet deposition mass in snowfall and snowfall mass) are not available from the prognostic-aerosol model runs. We did not have the resources to do these runs ourselves and had to rely on what was available in the prognostic runs we were able to download.

Page 13169,

Line 5. The statement that the BC forcing scales with the column burden is not correct. Samset et al. (Atmos. Chem. Phys., 13, 2423-2434, 2013) shows that the forcing efficiency of BC strongly increases with height, partly because the probability of the BC being located over a (reflecting) cloud increases.

Reworded to say the "vertically-resolved burden".

Line 13. The last part of sentence is unclear. It states "….or the amount of BC (wash-out ….)".

Apologies; we realized on re-reading that the ending of this sentence was ambiguous. It has been reworded to read: "Surface snow BC mixing ratios are determined by the mixing ratio of BC in snowfall (wet deposition), the settling of atmospheric BC onto the snow surface (dry deposition) and in-snow processes that reduce the amount of snow (melting, sublimation) or that reduce the amount of BC (wash-out of BC with snow meltwater)."

At some point in the introduction a comparison between BC albedo forcing estimates from models using prescribed deposition fields and those that treat the BC in snow concentration fully coupled should be added.

Including a review of modeling studies of this forcing is beyond the scope of this paper. It would not be useful to try and make such a comparison across different models in order to show that prescribed-aerosol runs have lower forcing than prognostic-aerosol runs, because any such comparison would be affected by other sources of differences in the models. e.g., See the discussion in Bond et al., (2013) Section 8, pointing out the large range in emissions and physical processes included in different climate model runs estimating this forcing. We would certainly find large differences – possibly in both directions – but would not be able to distinguish how the identified bias contributed to these differences. Therefore we don't agree this should be included in the paper. We do, however, now include comparison to one ensemble of prognostic runs, also done with CESM, based on the same BC emissions and using the same land snow model (CLM's SNICAR).

Line 18. Add reference to Forsstrøm et al., JGR.

DONE.

Line 20. I agree that the CESM-1 model is the only climate model to do this, but there are CTMs that have reasonable snow-modules (i.e. the Oslo-CTM2, Skeie et al. …). This model can calculate the forcing by BC in snow, and from the first sentence in the abstract it seems that it is the potential bias in forcing of BC in snow your paper is focusing on.

True, but the point of this paragraph is to give the reasoning for why CESM was used for the ACCMIP studies. There is no need or reason to list all of the models to date that represent BC-in-snow forcing.

Page 13170.

Line 10. I would suggest elaborating what you mean be "prescribed aerosol runs" for readers not familiar with the Lee at al. paper.

The following text has been added: In the Lee et al. study, each participating ACCMIP model calculated BC atmospheric abundances and deposition rates using a common set of emissions. The resulting deposition fields (e.g. grams BC deposited per m^2 per sec in each gridbox/day) were then used in CESM1 to calculate snowpack BC mixing ratios."

Line 13. In particular it is important to specify if the deposition rates from the different models was included in the offline CESM1 simulations so that the total mass of BC deposited was conserved. The alternative would be to include the deposition rates so that the MR of BC in new fallen snow was conserved.

We have modified the text so it snow states explicitly that all of the offline calculations directly use the BC mass deposition fluxes from the prescribed-aerosol CESM run, so the total monthly BC mass deposition flux in snowfall in each gridbox is the same in the *prescribed-aerosol runs and all offline calculations.*

Line 22. Again, what is "prescribed aerosols" ?

This sentence has been edited to now read: "Goldenson et al. (2012) also used CESM1 with <u>prescribed atmospheric aerosol concentrations and deposition fluxes</u> to compute the climate impacts of BC in snow on both land and sea ice and BC in sea ice."

Page 13171.

Line 17: When I read this sentence the wording "so that the mixing ratios of falling snow are physically unrealistic" seems to point to the AEROCOM models, while here it points to the mixing ratios of falling snow in CESM when the deposition rates from the other models are imposed.

This entire paragraph has been significantly modified, and we hope is now clearer.

Page 13172.

Line 4. Please make it clear that "aerosol fields" refers to "aerosol deposition fields"

Atmospheric concentrations are also specified in prescribed aerosol runs. We have reworded to reflect this. ("When prescribed, atmospheric aerosol concentrations and deposition fluxes are typically independent of the meteorological fields in the model").

Line 19. "… a series of offline calculations". A problem with this paper is that it is difficult to get an overview of the models simulations that have been performed. Could you be mode specific up front about how many experiments are performed.

The text describing the models used in the prognostic-aerosol and prescribed-aerosol model comparison and describing the offline calculations has been significantly modified, in response to both reviewers' comments. Per the request from Reviewer #1, Table 1 has been added, giving an overview of the model runs and offline calculations used herein.

Page 13173

Line 6. The sentence indicates that you not only perform offline simulations, while in fact there are no on-line simulations here. My suggestion is simply to remove the word "offline".

DONE.

Line 14, Eq 1 and 2. The units are not correct. BCdep(n,dry) is the rate per second (from line 19) while it should be the daily rate.

Thanks for catching this error; it has been corrected.

It is not quite obvious to me that this procedure assures conservation of mass (i.e. that the total mass of BC deposited by the prognostic models is equal to the mass in CLM4.

BCdep,dry and BCdep,wet are taken directly from the model, so the deposited mass in our offline calculation equals exactly that in the prescribed-aerosol CLM4 model run. We are not sure what the reviewer means by "BC deposited by the prognostic models" being "equal to the mass in CLM4". The deposited mass in CLM4 prognostic runs may differ from the deposited mass in CLM4 prescribed runs. But what is relevant here is: If the deposited mass were the same, would you get the same surface snow BC mixing ratios? This is the overall question we are trying to answer in this paper. We hope this is now clear with the significant revisions to the text.

Page 13174

Line 9. This is not how it is generally done in CESM1-CAM4.0, but it refers to the offline simulations I suppose. Please specify.

The reviewer is referring to the statement: "In CESM1-CAM4, the BC deposition flux at a given time is interpolated from 10 monthly input fields (Fig. 1)." This is in fact how it is done. In the default version of CAM4, the aerosol fields are prescribed in space and time, rather than simulated. Another commonly used configuration of CAM4, however, prognoses aerosol fields with a bulk aerosol model. The deposition fields shown in Figure 1 <u>are</u> the aerosol deposition fields used in a prescribed-aerosol runs of CESM-CAM4. So this is quite clear, we have revised this sentence to now read: "In CESM1-CAM4, the BC deposition flux at a given time is interpolated from monthly input fields; the deposition fields from the prescribed-aerosol run used here are shown in Figure 1 for two model gridboxes."

Lines 14-18. I think I understand this explanation, but I believe it can be made clearer.

We have modified this text as follows:
"The mixing ratio of BC in surface snow (MR_{BC}) at each timestep n is determined by the addition of BC through dry deposition ($BCdep_{wet}$) and by the addition of snow with a BC mixing ratio ($MR_{BC,snowfall}$) determined by the ratio $BCdep_{wet}$:$SWE_{snowfall}$. When wet-deposited BC and snowfall are decoupled, $BCdep_{wet}$ and $SWE_{snowfall}$ independently are added to the snowpack. Since the sum of a series of ratios ($MR_{BC,snowfall}$) does not equal the ratio of a series of sums (total $BCdep_{wet}$ and total $SWE_{snowfall}$), we expect this decoupling of deposition and snowfall will lead to errors in MR_{BC}."

Figure 2. Units are missing in figure 2!

Thanks for catching this! Units (ng/g) have been added.

Page 13176

Line 9. It is stated in the paper that "BCdep(wet) is , by definition, a function of precipitation rates". It might be correct that it is a function of the precipitation rate, but this function is probably quite non-linear. If the BC is well aged and thus in a hydrophilic form, even very light precipitation could be very efficient in removing BC (causing MRBC, snowfall to be high), and thus additional precipitation would not increase BCdep(wet) significantly. To my knowledge there are very few actual measurements of the MR of BC in snowfall, so I think the statements in this paragraph must be moderated. The fact that [MRBC, snowfall]d from the models are more variable (and much higher) than the measured MR of BC in surface snow is not a proof that events with light precip and high MRBC, snowfall do not happen in nature 8although the models probably tend to overestimate it be distributing precipitation over too large areas in the grid boxes). It could be that pots deposition processes (e.g. wind drift) will remove these very thin, high BC layers created by light snowfall.

We have now deleted this sentence, rather than complicating the discussion. We now include a comparison to an ensemble of prognostic model runs, which makes the point for us.

Page 13177.

Line 1. The number 10% indicates that dry deposition is of minor importance. However, if a significant fraction of the snowfalls give precipitation rates with SWEsnowfall> SWEsurf then drydep would contribute a larger fraction to BC in the surface layer. The contribution to BC in the surface layer is a more relevant number and should be given instead (or in addition).

True. So this sentence was deleted, since it didn't provide any critical information.

Line 8-13. In Figure 3 there are results for [MRBC]model. Please elaborate what this is. Is it results from on-line simulations? This discussion is confusing, and I don't see how you draw the conclusion in the last sentence.

We have clarified what $[MR_{BC}]_{model}$ is, and revised the text. We hope the meaning is now clear.

Lines 9-12 I suppose that the larger deviations during the melt season could be quite important for the eventual bias in radiative forcing over the year. Please elaborate on how this deviation during the melt season would affect the overall estimate of the bias.

This point is now made in the discussion of the prognostic-aerosol vs prescribed-aerosol model runs: "Positive feedbacks (e.g. consolidation of BC in surface snow *during snow-melt) are included in both runs, so any resulting differences in surface snow BC mixing ratios will be amplified." This is one reason why the offline calculations are used: to isolate the effect of the bias without feedbacks.*

Page 13178

Line 17-19. It is claimed that "Biases in the prognostic model's precipitation rates will therefore translate directly to biases in the aerosol mass deposition rates". I don't agree with this statement. On a global scale there is no relation at all. Everything that is emitted will eventually be deposited. Also regionally there would tend to be an offset. Slower precipitation would increase the lifetime of BC thus enhancing the atmospheric concentrations leading higher MR of BC in the precipitation when it eventually starts.

On a global scale, we agree; but at a local scale we do not. The text has been edited to read: "Biases in the prognostic model's precipitation rates <u>at a given location</u> will therefore translate directly to biases in the aerosol mass deposition rates."

Page 13179.

Line 10. I would not call CRUNCEP "observed" snowfall. Even the point observations that go into the reanalysis is quite shaky when it comes to snowfall.

True. Reworded to "reanalysis".

Page 13180

Line 18. I am a bit surprised that the uncertainty in the measured MR in newly fallen snow at Dye-2 is symmetric +/- 3.5 ng/g. I would suspect that this rather would be log-normal. Also I comparison between this number (7.5 +/- 3.5) and the distribution in fig. 2a is not straight forward. The extremely high values for the MR of BC in new snow (fig 2a) is obviously from episodes with extremely low precipitation rates which could never be sampled by physically collecting new snow.

The comparison to observed mixing ratios has been removed; see replies to Reviewer #1.
As an aside, this is a standard deviation (not uncertainty) that was given, and the distribution of mixing ratios around 3.5 ng/g was roughly symmetric.

Page 13182:

Line 21: You suggest prescribing mass mixing ratios in snowfall as a solution to the problem. As you indicate that creates another problem of mass conservation (which I suspect is there already, cf. comments above). I am quite skeptical to this conclusion, and I would think that as a first conclusion 8recommendation) would be for the climate models to include a snow module so that the vertical profile of BC (and other impurities) in the snowpack can be kept track of. In terms of additional computation time and storage it should be very limited, and in my mind this would be a much better way forward. This could be done by including SNICAR in other models or develop something simpler, but in my mind still better than the what you suggest or keep using the methods from Lee et al. and Jiao et al.

SNICAR is already included in the prescribed-aerosol model runs, as is now stated explicitly in the revised text. Thus, vertical profiles of BC in the snowpack are already being tracked. In any case, the issue being focused on here is how deposition to the surface snow layer is treated, not what happens to the BC once it is in the snowpack. (While the latter is of course needed for accurate calculations of this forcing, it is not the source of the bias we've identified here). We have retained our idea as one suggestion, but phrase it as one option rather than as <u>the</u> solution. (As an aside, mass conservation is not an issue currently in the prescribed-aerosol runs. This is now pointed out explicitly in the text.)