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.

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 ($SWE^{n}_{snowfall}$=0). Since $BCdep^{n}_{wet}$ is non-zero (a monthly mean) that means that f_n=0 while $MR^{n}_{BC, snowfall}$ would be infinite. I assume that the product f_nx $MR^{n}_{BC, snowfall}$ in eq. 1 is then set to zero.

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.

If my assumption above (that the product $f_n x\ MR^n_{BC,\ snowfall}$ in eq. 1 is then set to zero whenever $f_n=0$), 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.

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?

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.

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.

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.

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

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.

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

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.

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.

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.

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

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.

Page 13172.

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

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.

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".

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.

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.

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.

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

Figure 2. Units are missing in figure 2!

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 $MR_{BC,\ snowfall}$ 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 $[MR_{BC,\ snowfall}]_d$ 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 $MR_{BC,\ snowfall}$ 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.

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 $SWE_{snowfall}$> SWE_{surf} 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).

Line 8-13. In Figure 3 there are results for $[MR_{BC}]_{model}$. 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.

Line 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.

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.

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.

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.

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.