We thank Reviewer #2 for the time and the suggestions to improve our manuscript. We provide detailed responses below in black, with quotation marks showing the changes made in the manuscript and associated line numbers on the final (un-tracked) manuscript.

Reviewer #2

This paper presents the results of a high number of model simulations performed to derive runoff from Greenland Ice Sheet as a function of black carbon deposition. The model is using state-of-the art snow model, which includes interactions of Light Absorbing Impurities (LAI) with solar radiation, as well as scavenging by meltwater. They also provide linear equations that relate the increase of total runoff to black carbon deposition. Their results could be used by climate models, which don't have a sophisticated snow model but would like to estimate total runoff from Greenland for a given black carbon deposition rate.

The paper is well written, their approach valid, and their results interesting. I have only few minor comments.

Minor comments: All the figures showing time series with one line per month would gain in clarity by reducing to one month per season.

As the reviewer suggested, we tried to make figures with seasonal curves. However, because the seasonal cycle differs among the quantities shown in the different figures, the seasonal plots become confusing. We provide here two seasonal plots for Figure 5 and 6 as examples, which we cannot see clear seasonal patterns for both wet and dry BC deposition fluxes. Thus, we decide not to change the figures in the manuscript.

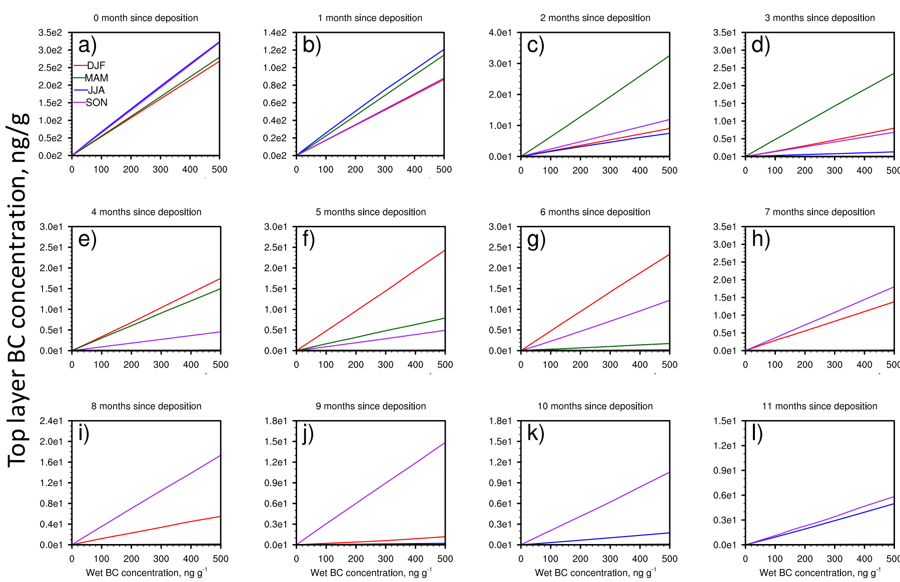

Figure R1. Hydrophilic BC concentration in the top snow layer vs. concentration of BC in precipitation shown at different times (0-11 months, panels a-l) since deposition. The top-snow layer concentrations are averaged over the Greenland region and over ten one-year simulations beginning in years 2006-2015. Different line colors represent different deposition seasons.

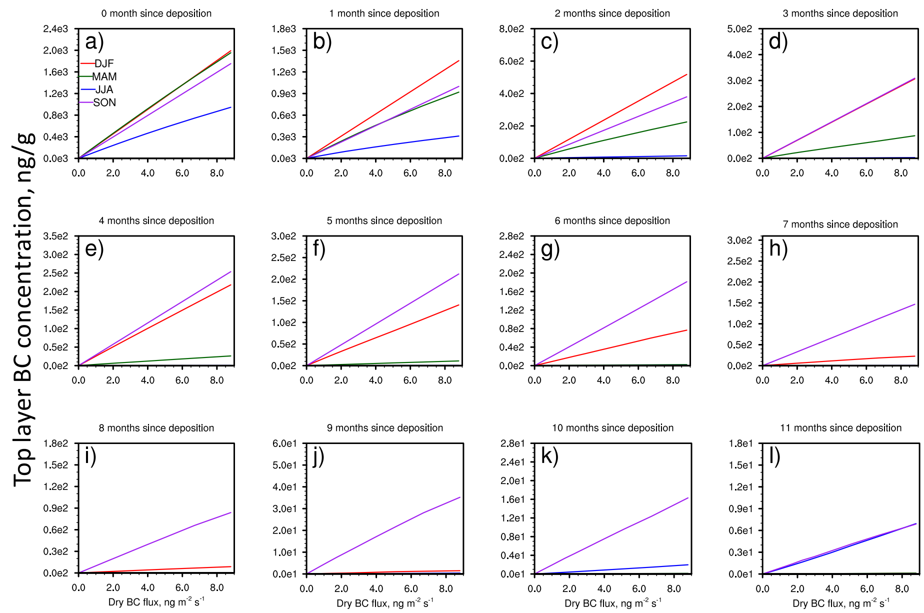

Figure R2. Same as Figure R1, but showing hydrophobic BC concentration in the top snow layer vs. BC dry deposition flux.

Are you assuming linearity of surface albedo change by black carbon deposition and metamorphism of snow grains? Should there not be another entry in the kernel for surface temperature?

We agree that surface temperature influences metamorphism of snow grains. However, as the reviewer also pointed out, the main difficulty of climate models is the lack of sophisticated snow model which relates total runoff with black carbon deposition. Therefore, we aimed to find out only the relationship between the amount of black carbon deposition and snow melt in our study. We didn't assume a linear relationship, but based on our simulations and calculations, we found linear regression could appropriately represent this relationship. Thus, we were able to provide linear equations relating the increase of total runoff to black carbon deposition.

It is unclear if the timestep of the snow model is one month, or sub-daily with the kernel data calculated as mean monthly values. If the timestep is monthly then the results for sub-daily timestep may have been quite different. At least an estimated error should be provided in such case.
Another needed precision is about spatial resolution. Line 98 we learned that CRUNCEP input data is 0.5 degree resolution. Is it the same for CLM and the kernel data?

We thank the reviewer for pointing out the confusion of temporal and spatial resolutions. Although the kernel data was calculated as monthly mean values, the timestep of the snow model implemented in our study was actually 0.5 hour. The resolution of CLM is 0.9° x 1.25°. And it is the same for the kernel data.

We add an explanation in Line 159 that "The timestep of CLM/SNICAR for our simulations is 0.5 hour, and the spatial resolution is 0.9° x 1.25°. The kernel product is calculated as monthly mean values with the same spatial resolution."

Line 141 "dry" should be "wet"

This was not a typo. We were trying to use the maximum wet BC deposition flux to calculate the maximum dry deposition flux, and then to implement this value to set up a set of dry BC fluxes with logarithmic spacing.

Line 177: "… as new snowfall dilutes the contaminated snow". Are you mixing layers and not superimposing them? LAI cannot move up in your model, right?

In CLM, snow layers can be combined and divided, as the maximum number of layers is fixed at 5, and both minimum and maximum thicknesses are prescribed for each layer. When layers are combined (e.g., because a layer thickness drops beneath the minimum threshold due to melt), both snow mass and BC mass are added together to establish the properties of the new layer. When the top layer exceeds the maximum thickness (2cm), e.g., due to new snowfall, the excess snow mass and a proportionate amount of BC are 'mixed down' into the layer beneath. We have specified these processes in Line 115, "Deposited particles are assumed to be instantly mixed and homogeneous within the surface snow layer", and "Particle masses are redistributed vertically in each time step proportionately with snow melt through the snow column, scaled by the species-specific melt scavenging efficiency, and snow layer combination and subdivision."

Lines 249-252: Is this due to an amplification effect by metamorphism of snow?

It could be. I added this hypothesis in Line 254 that "This could also be due to an amplification effect by metamorphism of snow in warmer years."

Line 262: "runoff… higher temperature" Is this increase of runoff by higher temperature due to darkening by black carbon or larger grain size?

That's a good point. We add the explanation in Line 265 that "The largest perturbation in summer could be caused by immediate melting due to darkening, as well as larger grain size in warmer environment."

Lines 279-280: "higher light absorptivity of hydrophilic BC" seems to contradict the values of single scattering albedo provided for black carbon in Table 3.5 of Oleson et al. (2013). The single scattering albedo of hydrophobic black carbon is lower than hydrophilic black carbon, indicative of stronger absorption by hydrophobic BC.

We agree that the single scatter albedo (SSA) is indeed lower for hydrophobic BC. However, the mass absorption cross section (MAC; product of the mass extinction cross section and co-SSA) is higher for hydrophilic BC. For absorption perturbations within a highly scattering medium like snow, MAC is the more relevant metric of absorptivity than SSA.

Line 281: "weakly-absorbing sulfate" Sulfate is purely scattering.

The assumed SSA value of sulfate is technically slightly less than 1.0, but we agree with the reviewer's point and have revised this reference to "non-absorbing sulfate" in Line 285.