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Interactive comment on "Modelling hydrologic impacts of light absorbing aerosol deposition on snow at the catchment scale" *by* Felix N. Matt et al.

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There are four significant issues with the paper that need to be addressed before it is ready for publication:

1) The model independently treats the "wet deposition" of BC (pg 11, lines 12-134) from the deposition of snowfall with which it is nominally associated (pg 11, lines 5-6). This is an issue. Doherty et al. (2014) showed using the CESM/SNICAR model (after which the snow model described herein appears to be very closely modeled) that this results in a factor of 1.5-2.5 high bias in surface snow mixing ratios. The authors should look at this paper, consider the implications for their study, and add analysis/discussion of how this impacts their results (or justify why it doesn't). [Doherty, S. J., C. M. Bitz and M. G. Flanner, 2014: Biases in modeled surface snow BC mixing ratios in prescribed

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aerosol climate model runs, Atmos. Chem. Phys., 14, 11697-11709, doi:10.5194/acp-14-11697-2014.]

RESPONSE:

This is a very important point, and we agree with including a discussion about the implications for our model. We would like to point out, however, that there are some significant differences in the approach of Doherty and the CESM/SNICAR coupling and our own. CESM/SNICAR couples of land surface model with SNICAR whereas we are interested in coupling a hydrologic rainfall-runoff model. There are similarities, of course, in the treatment of the snow, but over all the two approaches prioritize different objectives. As such, there are some significant differences between our approach and the one Doherty et al. (2014) claim to be problematic: The high bias in surface snow BC mixing ratios described by Doherty et al. (2014) refers to global climate model simulations with prescribed aerosol deposition rates (wet and dry), where "the input aerosol fields are often interpolated in time from monthly means. Therefore the episodic nature of aerosol deposition in reality (owing to wet deposition) is generally absent in prescribed-aerosol fields." This then results in the high bias, due to the coupling of the interpolated fields with highly variable meteorology (in particular precipitation). In our case study, however, we use deposition fields originating from the regional aerosol climate model REMO-HAM, forced with ERA-Interim reanalysis data at the boundaries. REMO-HAM output is 3-hourly, which we resample to daily means in order to have consistency between the deposition field and the daily observations used as input data in the hydrological simulations. Due to the use of ERA-Interim at the boundaries (which are not far away from Norway) we argue that the REMO-HAM precipitation is realistic and at least on daily scales should reproduce realistic values in terms of BC deposition. The high bias occurring when using interpolated monthly averages as input should therefore be minimized. We do appreciate this comment, however, and intend to include a more inclusive discussion of these aspects in a revised manuscript.

2) In the sensitivity study of how varying snowpack depth affects the impact of BC on snowpack melt (Section 5.1.4), snowpack SWE is varied while the total *mass* of BC deposited is also kept fixed (see pg. 18, lines 3-5). The justification is that this will "isolate the impact that the snowpack's SWE has on the effect of ARF in snow." However, that isn't quite correct. BC's impact on snow albedo/forcing/melt rate is a function of the mass mixing ratio of BC in snow water (ng BC per gram of SWE) – not of the total mass in the snow. By increasing SWE but not changing the mass of wet-deposited BC two things are being changed simultaneously: total snowpack SWE (definitively increasing) and the BC mass mixing ratio (definitively decreasing). I'd strongly argue that a better approach would have been to increase BC deposition in proportion to the increase in SWE so one can see to what degree having a "equally-polluted" but deeper snowpack changes the effect of the pollution on melt rate, versus a base case with the same pollution "level" (e.g. BC mass mixing ratio) but a shallower snowpack. Either this sensitivity study needs to be re-run or the paper needs to acknowledge that the results reflect these two simultaneous changes, discuss how this impacts their results, and note that this is not likely physically realistic – which makes me question the robustness of conclusion iii (pg 23, line 20).

RESPONSE:

We acknowledge the suggestions and will evaluate the results with constant mixing ratio to also include this case. We also plan to replace the "meltout days" as metric for impact of SWE with a relative change in meltout days (compare to comment in the "specific comment" section, "pg 18, line 16 and pg 23, lines 20"). However, we feel the comment: "BC's impact on snow albedo/forcing/melt rate is a function of the mass mixing ratio of BC in snow water (ng BC per gram of SWE) – not of the total mass in the snow." does not take into account that over the course of a melt season, BC can accumulate in the top layer and thus the total BC mass in the snowpack can have a large impact on the snow melt. Constant mixing ratio at different SWE would therefore be a different experiment leading to different conclusions, but not necessarily oppose

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our results. But as we mentioned, we acknowledge that this experiment should be evaluated also in a revised manuscript.

3) pg 21, lines 17-19: "At the same time, tiles bearing large quantities of snow tend to also bear large quantities of BC (in terms of total BC mass) due to the dominantly wet-depositioned BC, which we chose in the model to follow the same redistribution as snow. Only dry deposition is assumed to deposit spatially homogeneous over the sub grid tiles." If I am reading this correctly, the wet-deposited BC is effectively concentrated only onto snow-covered areas. Thus, as the snow becomes increasingly patchy the remaining snow gets more and more BC mass wet-deposited to it. This is completely unphysical: wet-deposited BC falls to the ground, whether it's covered with snow or not. Perhaps I am misunderstanding and this is just an issue of needing better clarity in the writing: The first sentence indicates the wet-deposited BC "follows the same distribution of the snow": Is this of the snow on the ground, or of the snowfall? If the latter, okay; if the former, as the second sentence seems to imply, I don't understand why this choice would be made since it's not physically reasonable. I'd expect this would significantly affect your results.

RESPONSE:

Yes, this is just a misunderstanding. We will clarify our explanation taking into consideration this comment. When writing we chose wet deposition in the model to follow the same redistribution as snow, we do in fact mean snow fall. We will accordingly rewrite this paragraph to avoid confusion about our methods.

4) The paper is difficult to read. Much of it has run-on sentences that are overly convoluted. There are a few specific cases I note below ("Technical corrections") where outright corrections are needed but much more work is needed beyond this. I would strongly suggest that the co-authors work with the lead author to improve the clarity and conciseness of the writing.

RESPONSE:

This comment appears in all three reviews given – we take it very serious and will accordingly work together to improve the structure and the conciseness of the writing.

Specific comments:

References need to be added to support statements made in a number of places: a) pg 2, lines 5 through 14 to support a string of assertions b) pg 7, line 10 re: radiative exchanges dominating snow melt in most snow melt scenarios (and perhaps qualify it, too; I'm assuming temperature is the dominant factor for many conditions) c) pg. 15, line 6 re: LAISI absorbing more efficiently in snow with larger grain size d) pg. 17, lines 16-17: "Hydrophilic BC absorbs stronger than hydrophobic BC under the same conditions due to an increased MAC compared to hydrophobic BC caused by the ageing of BC during atmospheric transport." (In reality, the degree to which this is the case is not very well-established so references supporting this assertion really are needed.)

RESPONSE:

The required references will be added.

pg 2, lines 13-14: organics from combustion and organics in soil also are LAISI and should be added to this list

RESPONSE:

We will add missing LAISI species to the list.

pg 2, lines 17-19: "Current theory indicates the absorbing effect of LAISI is most efficient when the LAISI reside at or close to the snow surface, and that subsequent snow fall burying the LAISI leads to a decline in or complete loss of the effect." The latter half of this statement is not accurate (and no reference is given to support this assertion). This statement assumes that new snowfall is essentially clean (BC-free), then BC is subsequently deposited on top of the snow. In reality BC is deposited *with* the snow, in wet deposition – at least in the real world. So I don't think "current theory" indicates that it works as stated.

RESPONSE:

Current theory indicates that due to a limited penetration of light in the snow, only LAISI relatively close to the surface is acting decreasing on the albedo. Furthermore, BC can accumulate close to the surface due to sublimation or inefficient melt scavenging, or by dry deposition, an thus exceed LAISI mixing ratios given by that of falling snow. Subsequent snowfall with a lower LAISI mixing ratio in the falling snow than in the surface snow can lead to a burying of layers with higher LAISI mixing ratios (e.g. observed in dust on snow events by Painter et al., 2012). And even if the subsequent snow has the same mixing ration in BC, the optical grain size is typically smaller in fresh snow, so the effect of LAISI will be less in fresh snow then in the previous older snow – which lead to a decline of the effect after snow fall events. However, we agree that we should clarify this section to separate what theory actually says (BC closer to the surface absorbs more efficient than BC further down in the snow pack) and what is process related.

pg. 5, lines 29-30: Here and earlier in the test dust, black carbon, volcanic ash and other light-absorbing aerosols are mentioned, but only BC is included in the model. It would be good to be clear that the only LAISI you are currently accounting for in the model is BC.

RESPONSE:

We will do this. We will also add a discussion how the presence of other LAISI would impact our results.

pg. 6, lines 1-2: You need to be specific about what you mean by a gamma distribution.

RESPONSE:

We will clarify this.

pg. 8, line 14: What is the basis for using this specific formulation?

RESPONSE:

We accidentally stated the wrong equation in the paper. In an older version of the current snow routine, we were using this equation, which we developed by ourself. However, we then changed to a formulation by Taillandier et al. (2007) for dry snow and Brun (1989) for wet snow, on which our here presented model results are based on. This formulation has been used in other studies, e.g. Gabbi et al. (2015). We will change the paper accordingly and add the correct equation and references.

pg 8, lines 20-25. I found this discussion of what is “appropriate” for a surface layer thickness to be confusing. Snow doesn’t have a defined “surface layer” so it’s not as if this is fixed quantity that has some “true” value in the real world. What is appropriate to use for the model surface layer thickness would be a function of what metric you are interested in. Here it could be, for example, the e-folding depth of sunlight penetration, or it could be the depth over which most melt amplification of BC mass is concentrated. Or any number of other things, depending on what you’re interested in.

RESPONSE:

We will clarify this in the text and agree the specifics regarding snow stratigraphy and specifying a surface layer (let alone sampling one!) are difficult.

pg 8, line 24-25: “Since we expect surface concentrations of LAISI in snow to be quite sensitive to the surface layer thickness in our model . . . ” In reality this should only be the case for dry-deposited BC. Wet-deposited BC should be deposited with snow; if the mixing ratio of BC in snowfall were unchanged throughout a new snowfall event the mixing ratio of BC in the surface layer could be completely insensitive to the depth you select for the “surface layer”. It should be made clear that surface concentrations of LAISI in the model might be sensitive to the selected surface layer thickness because you are decoupling BC mass deposition and SWE deposition, and not state this as if this were an inherent property of real ambient snowpacks.

RESPONSE:

“In reality this should only be the case for dry-deposited BC”: This is only correct during snow accumulation. However, during melt (as later shown in the sensitivity study), the surface layer thickness strongly defines the effect of melt amplification on the surface layer mixing ratio. However, we should clarify this in the text, and in fact need an improved definition of surface layer as discussed in the prior comment.

pg. 9, lines 10-11: “We allow for melt from the bottom layer only when the potential melt per time step is exceeding the maximum depth of the surface layer (both in mm SWE).” It’s unclear if you mean that no melt is allowed to occur in the bottom layer until the surface layer is saturated, or if you mean that no melt water is allowed to exit the bottom of the surface layer until the surface layer is saturated.

RESPONSE:

We mean the latter: no melt water is allowed to exit the bottom of the surface layer until the surface layer is saturated. We will clarify this in the text.

pg. 9, line 12: “To date, estimates of the scavenging ratio k are mostly based on experiments conducted by Conway et al. (1996).” Doherty et al. (2013) also estimated the scavenging ratio from ambient snowpacks in two locations. In fact their estimates agreed quite well with that used in Flanner et al.’s SNICAR model – values you seem to adopted here in your model. It would therefore be appropriate to note this, both because there is a study other than Conway et al. (1996) and because their results support the “mid” scavenging values you use.

RESPONSE:

We will adapt the text accordingly.

pg. 9, line 24: The assumption of all wet-deposited BC being hydrophilic and all dry-deposited BC being hydrophobic is not justified, either here or in Section 3.

RESPONSE:

The hygroscopicity of BC particles defines which removal process (wet or dry deposition) will be more effective (e.g. Croft et al., 2005). REMO-HAM accounts for this by applying hygroscopicity depended scavenging parameters to aerosols (e.g. Hienola, 2013). From this, we assume that wet deposited BC has the optical properties of aged, hydrophobic BC.

pg. 10, lines 27-28 and Figure 1: It's not at all clear what is meant by "multiplication factors" or how they are used. Figure 1, left panel: coefficient of variation in what? Specify in the figure caption. Figure 1, right panel: It's not at all clear what is being shown here. What are the "factor numbers"? What is the (unlabeled) vertical axis? Why different "factor numbers" for each CV?

RESPONSE:

* Coefficient of variation:

We assume falling snow in a cell is spatially distributed according to gamma distribution, defined by its coefficient of variation (CV). The Cvs of the gamma-distributed snow are taken from Gisnås (2016), who calculated them on a 1x1 km grid over Norway. They represent the spatial snow distribution in the 1x1km cells at snow maximum. To simulate the gamma-distributed snow in a cell, we divide each cell into 10 tiles.

* Multiplication factors:

During snowfall, each of these tiles then gets snow input from falling snow, multiplied with a factor, according to the gamma distribution. These are then ten multiplication factors. These multiplication factors for each tile are constant over time, but vary from cell to cell, according to the CV. The mean of the multiplication factors is 1, so that the mass balance between falling snow and snow input to a cell is not violated.

* Factor number:

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We should rather call this “tile number”. We will change this in the figure.

* Vertical axis:

The factor, with which falling snow is multiplied. We will add this to the figure.

* “Why different “factor numbers” for each CV?”

One multiplication factor for each subgrid tile. We acknowledge that the explanation of our approach needs to be clarified. We will reword the paragraph accordingly.

pg 12, line 19 / Table 3: Why set radiation to zero during the “accumulation periods”?

RESPOSNE:

The purpose of the accumulation period is only to accumulate a snowpack for the purposes of our sensitivity study. We then slowly melt the snowpack with constant meteorological forcing to explore the parameters specifically in the melt period.

pg. 12, line 21: “The forcing applied during the snow accumulation period of 180 days results in 250 mm of SWE at the end of the accumulation period.” then pg 12, line 25-28: “After the snow accumulation period, we invoked a time invariant forcing to slowly melt the snowpack until meltout. The forcing applied for melt is based on the average forcing during the melt season from mid March until mid July of the Atnsjoen catchment and results in a melt period of ca. 25-35 days, depending on the scenario applied.” I’m quite confused by the use of the term “forcing” here. I would assume you mean radiative forcing, but that would make no sense in the first sentence. Which makes me wonder what you mean by “forcing” in the 2nd and 3rd sentences. Are you calling temperature and precip variations “forcings”? If so, this is quite unconventional, at least for someone from the climate community. Either some explanation or a revision is needed here.

RESPONSE:

In the hydrologic community it is not untypical to simply use the term ’forcing’ to refer to the suite of meteorological forcing data. However, we acknowledge this should be clarified and will do so in revised text to better distinguish between the meteorological forcing (radiation, precipitation, temperature, relative humidity, wind speed, aerosol deposition) and the aerosol radiative forcing (which we refer to as the additional absorption of incoming SW radiation due to BC in snow compared to hypothetical clean snow).

pg 13, Sections 4.1.2 & 4.1.3 and Figure 3c: There aren't different "species" of BC. "Hydrophobic BC" generally refers to fresh – i.e. uncoated – BC, and "hydrophilic BC" is really BC that's been coated. The BC itself in each is essentially the same. I'd suggest a re-wording/re-naming.

RESPONSE:

We were defining "species" of LAISI according to radiative properties – in which the two are different (Flanner, 2012 uses similar wording). However, we can clarify this in the text.

pg 14, lines 8-9: "Bayesian Kriging" This needs a bit of an explanation or at least a reference.

RESPONSE:

We will add the according reference to the paragraph.

pg. 14, lines 9-10: "For precipitation, BC deposition rates, wind speed and relative humidity this implies interpolation to the model cells via inverse distance weighting, with a constant vertical gradient applied for precipitation." Do you mean that precip varies with land altitude (with some constant gradient) or that precip is constant with altitude? If the latter, rewording is needed; if the former, some quantification of this vertical gradient is needed.

RESPONSE:

We refer to Førland (1979), who investigated the elevation dependency of precipitation in Norway, and apply a 5% increase in precipitation for every 100 m increase in altitude. We will add the reference to the text and reword the description of our method.

pg. 14, line 11 and Figure 5: a) It's not clear what is meant by a "split sample calibration". b) What is used to calibrate the model? What parameters are varied to achieve the best "calibration" / tuning? c) In Figure 5, the top panel shows data for 2007-2012 with the first three years as "calibration" data. In the bottom panel, these same three years are shown as "validation". Isn't that a bit circular? Or perhaps I don't understand what the difference is between the two panels. d) In Figure 5, it's not stated whether the model is run assuming a perfectly clean snowpack (BC deposition = 0), or something else. (See comment below re: Conclusions section and Figures 5-7.)

RESPONSE:

a) "The split-sample test is a classical test in hydrological modeling, which can be used when sufficient long time series of control data for both calibration and validation period are available and catchment conditions are stationary, which we assume to be true during the simulation period. If the split sample test gives acceptable results, a final calibration can be conducted, making use of the full control data" (from "Distributed Hydrological Modelling; edited by Michael B. Abbott and Jens Christian Refsgaard"; page 50). The above described procedure is the one we used in our analysis. We will clarify the meaning of the split-sample tests by adding the above reference to the text.

b) We use observed discharge for model calibration. This is mentioned later in the paper (Sect. 5.2.1), but should of course be mentioned here as well. A table with the calibration parameters and the final estimates of the parameters after calibration will be added to the paper.

c) In Fig. 5, both panels show the six years simulation period, form Sept. 2006 – Aug. 2012. Referring to the answer given in a), after receiving acceptable results from the split-sample test (shown in the upper panel of Fig. 5, green curve shows calibration, red curve validation period), we ran a final calibration, making use of the full control data (lower panel of Fig. 5), which results in a similar NSE. By describing the split sample-test in more detail (see a)), we hope that our procedure becomes clear.

d) For model calibration we assume the mid-scenario, since this is what we expect to represent "reality" the best. Max, min and no-ARF scenarios then use different deposition rates and parameters related to the LAISI representation in the snowpack, but otherwise the same settings as used in the model calibration. We will clarify the use of the deposition scenario during calibration in the text.

pg 15, lines 14-18: "The stronger increase in surface BC in model setups with thinner surface layer is due the inversely proportional relationship of the surface layer thickness with the increase in impurity concentration under the same mass flux of LAISI into the surface layer (from deposition or melt amplification): halving the surface layer thickness, leaving the mass flux of LAISI into the surface layer unchanged, leads to a doubling of the increase in the LAISI concentration and thus to differences in the vertical distribution of LAISI . . . " Again, this is only true because you are decoupling BC wet deposition and snowfall deposition. (see comment above re: pg 8, line 24-25)

RESPONSE:

In this paragraph, we try to investigate how the choice model surface layer impacts the BC concentration in the layer and thus the snow melt. In the experiment, we simulate a snow pack with a certain mixing ratio of BC at melt onset until all snow is melted. During the snow melt, no deposition of BC is applied to the snow pack (see Table 3). Thus, the increase in BC mixing ratio in the surface layer is due to melt amplification solely, and has nothing to do with decoupling BC wet deposition and snowfall deposition (however, we should mention this specifically in the text, since this obviously led to some confusion). When stating "The stronger increase in surface BC in model setups with thinner surface layer is due the inversely proportional relationship of the surface layer thickness with the increase in impurity concentration under the same mass flux of LAISI into the surface layer (from deposition or melt amplification): ...", we generally describe that any mass input of BC in the surface layer without a mass input of snow, will lead to an increase in the BC mixing ratio, and the increase is inversely proportional to the thickness of the surface layer. This mass input can originate from deposition (in particular dry deposition) or during melt from the bottom layer (melt amplification). However, as mentioned above, in our model experiment no deposition of any kind is applied during the melt phase, and the increase in the surface layer BC mixing ratio is due to the BC mass input from the bottom layer sole.

pg. 16, lines 8-9: "a doubling of the surface layer LAISI concentration occurs already when the accumulated melt equals the surface layer thickness": "Equals" in terms of what? SWE? Where is this shown? I don't see this in Figure 3.

RESPONSE:

"Equals" is meant in terms of mm SWE. We will add this in the text. The doubling follows logically from the model representation of the surface layer.

pg. 16, lines 17-18: "the surface concentration of the aerosol simulated strongly depends on the magnitude of the surface layer," Poor wording. What do you mean by the "magnitude" of the surface layer? The surface layer depth?

RESPONSE:

We mean the "surface layer thickness". We will change wording.

pg. 16 Section 5.1.2 and Figure 3a: I'm confused by what is shown in Figure 3a middle panel. The BC concentration appears to start at zero, then increase from there. How can the surface snow concentration start at zero? How can there be a "factor increase" for a parameter that starts at zero?

RESPONSE:

This is misleading. The mixing ratio of BC at the begin of the melt season is set to 11 ng/g (which is equivalent with the min-estimate pre-season BC). The curves shown in Figure 3, middle panel don't start at zero, but at 11 ng/g. The point of choosing this value is to show the potential of impact of relatively small concentrations of BC, and at the same time investigate the impact of the model specific parametrization on the impact. However, we missed to mention the pre-season BC mixing ratio in the text of Sect. 4.1. We will add this to the description of the sensitivity study.

pg. 17, lines 13-14: "showing that small amounts of BC in snow can impact the snow-pack evolution over the whole melt period even if it undergoes an efficient scavenging process." Up to here no results have been presented that indicate what pre-melt surface snow BC mixing ratios are. This isn't given until pg 20 Section 5.2.3. So the reader really can't know whether a) the model is giving reasonable surface snow mixing ratios of BC and b) what you mean by "small amounts of BC in snow". (Nominally Figure 3 would show this, but as noted above these values all start at zero so it's hard to know what point in the evolution of concentrations you're talking about here).

RESPONSE:

It is correct that we need to mention the pre-melt surface snow BC mixing ratio in the sensitivity study description in Sect. 4.1. As described above, the values don't start at 0.

pg. 17, line 26: "which we assume to be the most suited": Based on what?

RESPONSE:

"Most suited" in term of "representing reality the best". The parameters of the mid estimate are based on literature values (for a in depth description see e.g. Sect 2.2.2: Aerosols in the snow pack).

pg 18, line 16 and pg 23, lines 20 (bullet item iii): "are less impacted" (pg 18) and "are more prone to be affected (pg 23): By what metric? The "melt shift days"? Is this the only metric of importance? Figure 4 shows the "meltout shift" vs snowpack SWE as a function of different BC scavenging ratios. The meltout shift of 60 days for the deepest snowpack is indeed impressive, but a) we don't know what the BC mass mixing ratios were in this model run and b) we don't know what the total number of melt days is so it's kind of hard to put these results into context. (It must've been at least a few months for the meltout shift result of 60 days. Is this correct??) Perhaps the relative change in number of meltout days (as a percentage?) would be a better metric.

RESPONSE:

We acknowledge that "meltout shift in days" is an insufficient metric. We will accordingly rerun the test and change the metric to "relative change in meltout days".

pg. 18, line 23: "NSEs" needs to be defined.

RESPONSE:

NSE is already defined pg 14, line 15 as Nash-Sutcliffe model efficiency.

pg. 18, lines 25 "winter discharge": what time period is "winter" here? Also, Figure 6 does not indicate seasonality. How do we know that the low flow cases are in winter?

RESPONSE:

We refer to "winter discharge" as to the period from circa beginning of November until end of March, when discharge slowly drops to a minimum at the end of the winter season. Here, relatively low flows between 0 and 15 m3 s-1 are predominant, which the model underestimates (see Fig. 5). Even though no seasonally is shown in Fig 6, one can clearly see that low flows are underestimated, as shown in Fig. 5, whereas higher flows are better represented. We will rephrase this passage of the text, making clear that no seasonality is mentioned in relation to the scatter plot in Fig.6.

pg. 19, Section 5.2.2.: Again it's difficult to interpret these results since we don't yet know what the model was calculating for surface snow BC mass mixing ratios, and whether they were even vaguely realistic. So I think some re-ordering of the presentation of results is needed.

RESPONSE:

We will change the text accordingly to discussing the BC mixing ratios before the BC impact on discharge and aerosol radiative forcing from BC.

pg. 21, lines 26-29: “Qualitatively, we feel this represents reality well, in that if we think about snow patches in a catchment at the end of the season, they tend to be ’dirty’, as the concentration of impurities increases while the water melts away.” Yes, but in the real world, on which you are basing your observations of reasonableness, this visible darkening of the snow is very likely dirt accumulating, not BC.

RESPONSE:

We acknowledge this reality, and would like to indicate that dirt is a component of LAISI – in the most broad sense. Granted, it was not what this study focused on, but we feel the BC would follow the same general pattern.

pg. 23, lines 24-25: “Even though our model approach is conservative due the lacking implementation of the effect of LAISI on the grain size growth and due to the choice of a remote northern catchment of only medium snow accumulation” It should have been spelled out sooner that grain size growth is not affected by the presence of LAISI (i.e. on pg 8, âĹij lines 15-18).

RESPONSE:

We will add this as suggested to Sect. 2.2.2 (Aerosols in the snowpack).

Conclusions:

a) Figures 5 - 7 and Results Discussion + Conclusions: The study indicates that inclusion of BC in snow has a significant impact on melt timing (Figure 7). Yet it’s not at all clear whether the model calibration and validation (Figure 5) include the effects of BC or are based on using a clean snowpack. I was surprised that there was no testing or discussion of whether including BC in snow improves modeled vs observed catchment outflow volume/timing (Figures 5 & 6).

RESPONSE:

* "Yet it's not at all clear whether the model calibration and validation (Figure 5) include the effects of BC": The calibration include BC – as mentioned before, we missed to mention this in Sect. 4.2 (Case study model setup and calibration). We will add this to the text.

* "no testing or discussion of whether including BC in snow improves modeled vs observed catchment outflow volume/timing": We will add and investigation about model improvement and add a discussion part about potential improvement in simulation in the revised manuscript.

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