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Discussions

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

Anonymous Referee #1

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General comments:

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-

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

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).

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.

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.

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.)

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

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.

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

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

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.

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.

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.

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.

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.

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?

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

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.

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.

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

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.

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.)

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)

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.

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?

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?

pg. 17, lines 13-14: "showing that small amounts of BC in snow can impact the snowpack 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).

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

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.

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

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?

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.

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.

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, $\sim$ lines 15-18).

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).

b) The discussion totally ignores the fact that real snowpacks have particulate absorbers other than BC. In this regard the impact of BC (pollution) on snow albedo, radiative forcing and melt rates in this study represent an upper limit. If other absorbers – i.e. naturally-occurring dust and dirt – were also included in the model study the impact of adding BC would be less. This needs to be noted and acknowledged.

Technical corrections:

pg 2, line 20: the wording that LAISI "can reappear and retain near to the surface" is both awkward and not accurate. It doesn't "reappear" – it just becomes more concentrated at the surface as the snow water runs out through the snowpack at a higher rate than the BC.

pg 4, lines 26: P & E need to be defined when they are first used (even though it's pretty obvious what is meant here. . .)

pg 5, lines 14-15: "Furthermore, the presence of a permanent snow layer and snow melt leads to a more challenging identification of periods when the change in liquid water storage is governed by discharge only." I can't figure out what it is you're trying to say here.

pg 18, line 25: "simulated over observed" should be "simulated versus observed"

pg. 20, lines 10-11: "We see the albedo of the max scenario having the largest drop and the one of the no ARF scenario being the lowest." Needs rewording. The *decline* in albedo is smallest; this reads as if the *albedo* is the lowest.

pg. 23, line 22: "To prove the significance. . ." I think you mean to *test* the significance.

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