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Discussions

Interactive comment on "Albedo reduction caused by black carbon and dust accumulation: a quantitive model applied to the western margin of the Greenland ice sheet" *by* T. Goelles and C. E. Bøggild

Anonymous Referee #1

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Review of

Albedo reduction caused by black carbon and dust accumulation: a quantitative model applied to the western margin of the Greenland ice sheet

by Goessel and Bøggild

General

This paper describes a simple model for dust deposition, outcropping and accumulation on ablating ice surfaces. It is intended for ice sheet models and must therefore

be computationally efficient. The model requires substantive input in the form of background albedo values, englacial dust profiles and requires calibration using the 'active fraction'. The originality for these applications is reasonable, but the scientific rigour is insufficient, as detailed below. It is unclear to me what the added value of this model is to more sophisticated models like SNICAR that are also (reasonably) computationally efficient, and that anyway provide most of the parameterizations that are used in this model and that can be simplified as desired.

Unfortunately, the paper is not very well written, with many typo's (even in the title!) and/or textual ambiguities of which several are listed below as minor comments. The figure quality is good. All in all, before publication in The Cryosphere, substantial revisions would be necessary which I am unsure is possible based on the chosen approach and available data.

Major comments

The main weakness of the paper is that the active fraction (called 'a very powerful parameter' on page 1365) is used as a tuning parameter. This makes it possible to obtain almost any albedo reduction for any given dust concentration. As a result, the model is not universally applicable, as it must be calibrated for each location and year. Another weakness is that concentrations of englacial dust, the main source of dust in the ablation zone, must also be prescribed. These deficiencies must be discussed and possible solutions provided, including a possible physical model for the active fraction. Another serious flaw is the absence of an independent model evaluation using data different from those that were used to calibrate the model.

Minor comments

p. 1361: "The high ice albedos at S5 makes it necessary to set the active fraction of BC and dust to zero." Setting the active fraction to zero means that the simulated concentrations are no longer important. But does this not simply reduce the model to an interpolation between snow and ice albedo?

p. 1349: the distinction between 'atmosphere' and 'tundra' is not mutually exclusive: dust from the tundra also reaches the ice through the atmosphere. Better: atmosphere (from distant sources) and atmosphere (from local sources).

p. 1350: it is assumed that once incorporated in the snowpack, particles will not be suspended again into the atmosphere. But what if drifting snow sublimates into the air, releasing particles that were originally included? Could this be a significant effect?

Section 4.2: the model fails to reproduce the 2009 high mid-summer albedo: "... year 2009 was problematic as the autumn 5 snow cover was building up earlier than normal." But isn't a model specifically meant to reproduce non-normal events? This requires more reflection and discussion.

p. 1365, l. 14: 'validated' is not the right word here for two reasons. First, models can only be evaluated, not validated, because by definition they are an approximation of reality. Secondly, the albedo model was calibrated using the same data, so there is no independent evaluation. Perhaps the phrase 'applied to' or 'tested with' is better to use here.

p. 1348, l. 13: The SNICAR model is already implemented in various large scale models, and is not too computationally demanding for long term applications.

Textual comments

p. 1355, l. 19: This sentence is unclear. Suggest to change into: "In this study we focus manly on ice albedo and therefore for snow albedo we only differentiate between wet and dry snow as in Robinson et al. (2010), even though the model includes snow albedo reduction due to BC and dust."

p. 1355, l. 19: manly -> mainly

p. 1346, l. 22: surface melt -> meltwater runoff

p. 1347, l. 3: model -> models; then treated -> then it is usually treated

p. 1348, l. 13: to -> too

p. 1348, l. 17: remove 'based' or use the phrase 'process model'

p. 1349: replace 'effected' by 'affected' (2x), also page 1362.

p. 1349, l. 15: and -> before

p. 1352, l. 12: typo 'that has to that has to'

p. 1352, l. 15: '...applied TO Greenland...'

p. 1353, typo in header 2.4.1

p. 1354, l. 9: 'At high zenith angles close to solstice at Kangerlussuaq the maximum albedo increase with high impurity loadings is below 0.04' Please provide reference for this statement.

p. 1354, l. 17: replace '1/200' by '200 times'

p. 1358, l. 20: for -> to

p. 1362, l. 10: reasonable -> reasonably

p. 1363, l. 11: as just -> even if just

p. 1363, l. 28: As is BC which is -> BC is

p. 1364, l. 2: where -> were

p. 1365: higher temperatures -> more melt (?)

p. 1365, l. 6: warmer -> higher

p. 1365, l. 21: affect -> effect

Figure 6, caption: " The parameters are optimized for different yeas and then used to simulate the entire period" What is meant here? What parameter values are ultimately chosen?

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