The Cryosphere
Discussions

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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 #2

Received and published: 20 July 2015

The authors developed an empirical method to estimate the impact of impurities on the ice and snow albedo evolution in an area of western Greenland. The text is so badly written that it is often difficult to read and to extract the meaning of the sentences. In addition to the language problem, also the scientific level is insufficient. The applied method is not rigorous and cannot be accepted in a scientific publication. Model uncertainties are not at all addressed, the model is not validated against independent observations, and the method appears to be completely inadequate for quantitative (and qualitative) estimations of the impact of impurities on snow/ice albedo. I listed

below the main problems in more details.

Main problems:

1) Bad language. Very many sentences are poorly written or grammatically and logically incorrect, showing not only careless in writing, but also lack of clarity of the expressed concepts. Here are few examples:

p. 1348, lines 12-15: "Sophisticated snow albedo model like SNICAR (Flanner and Zender, 2006) have existed for some time. While these models are computationally to demanding to include in large scale models they give important insight to efficient parameterizations (Pirazzini, 2009)". Do the authors mean the model SNICAR efficiently parameterize the quantities that control the snow albedo? In fact, SNICAR is currently applied in several climate and numerical weather prediction models as its computational cost is very limited. However, SNICAN cannot be implemented into those simple models that have a single snow layer.

p. 1354, line 1: "The basic method for snow and ice albedo are the same . . .". Do the authors mean that the method to account of the impact of impurities on albedo is the same for snow and ice?

p. 1355, lines 19-21: "In this study we focus mainly on ice albedo and therefore left the snow albedo is defined by differentiating between wet and dry snow as in Robinson et al. (2010) although the model includes also snow albedo reduction due to BC and dust". This confused sentence leaves totally unclear how snow albedo is treated in the study.

The paper is far from being adequate for scientific publication and needs to be completely rewritten by a scientist who is fluent in English.

2) The method applied in the study is not rigorous and cannot be accepted. First of all, all the equations introduced in Sections 2.2, 2.3, 2.4 and 3.3 are quite rough and consist of simple parameterizations of complex physical processes. They can be applied

only if their uncertainties are properly estimated. As the developed model aims to give quantitative estimations of aerosol impact on albedo reduction, an accurate assessment of the model uncertainties is mandatory. In fact, the large variability of the fitting parameters among different years tells that the model is oversimplified and completely inadequate for quantitative estimations. Secondly, many of the equations (6, 7, 8, 11) are given without referring to the source, and without explaining the reasoning behind their choice or development. Finally, the model is not validated against independent observations, and the fitted parameters vary in different years. This makes the model site and time dependent, which means that it is completely useless.

3) Some of the utilized quantities are not properly defined: for instance, what is M_s obtained via Eq. (9)? At p.1355, line 1 the authors write that "the active fraction $F_{ice,n}$ describes the fact that not all impurities are influencing the albedo". Does it mean that the active fraction is the fraction of aerosol that contributes to the reduction of ice albedo? Also, on p. 1355 lines 14-15 the authors describe the "effective depth" as "related to the absorption length in ice which is depending on wavelength and impurities". What is then the effective depth, the penetration depth of light into the ice at a specific wavelength and impurity content??

4) The authors state that their study focuses mainly on the ice albedo (p. 1355, line 19), but for a large fraction of their considered period (April-September) the ice surface is covered by snow. Thus, from the point of view of surface energy and mass balance, the effect of impurity on snow albedo is equally or more important as the effect of impurities on ice albedo. The authors should study and eventually apply the research done in this field. Some recent publications are:

a. Oaida, C. M., Y. Xue, M. G. Flanner, S. M. Skiles, F. De Sales, and T. H. Painter (2015), Improving snow albedo processes in WRF/SSi regional climate model to assess impact of dust and black carbon in snow on surface energy balance and hydrology over western U.S., J. Geophys. Res. Atmos., 120,doi:10.1002/2014JD022444. b. Kokhanovsky, A. (2013), Spectral reflectance of solar light from dirty snow: a simple

theoretical model and its validation, The Cryosphere, 7, 1325–1331. c. Dang, C., R. E. Brandt, and S. G. Warren (2015), Parameterizations for narrowband and broadband albedo of pure snow and snow containing mineral dust and black carbon, J. Geophys. Res. Atmos., 120, doi:10.1002/2014JD022646.

Some extra comments:

p.1353, lines 20-21: "This parameterization allows the snow albedo to be lower than the ice albedo". In which circumstances snow albedo would be lower than ice albedo? It sounds very unrealistic, unless you consider the case of extremely dirty snow above a completely pure ice (which is also quite unrealistic).

In Eq. (14) and (15) an "effective aerosol concentration ι_{eff}" is introduced. Why it is called "effective"?

p.1356, line 17: does "KAN_U" and "S10" mean "stations U and S10"? Please, remove "KAN_" in all occurrences in the text, or properly name the stations in Fig.4.