Answer to a reviewer 1:

The author is grateful to the reviewer for the detailed comments. The answer to the comments are given below.

(1) As pointed out by the Editor in his initial review, the manuscript contains a lot of equations. Such a paper may interest a wide range of scientists, among whom a lot are not familiar with radiative transfer questions and definitions. For this reason, I'd recommend the author to state clearly at the beginning of Sect. 2 what expressions he aims at deriving, and what are the snow physical characteristics that are relevant (snow grain size, shape and impurity content). In particular, the initial decomposition of the albedo in terms of scattering events, and the discussion on random walk, though interesting, seem unappropriate and unnecessary in such a paper. Eventually, only Eqs. (12) and (13) are used. These formulae along with their derivations appear in several papers from the author (Kokhanovsky, 2004; Kokhanovsky and Zege, 2004; Zege et al., 2008). The main point here is not only how to get the equations, but to show that they are efficient and to highlight how to use them.

I have added additional references to the Section 2. Eqs. (12)-(15) are not new.

(2) The application of the analytical formulae to experimental studies is very interesting and essential to highlight the interest and validity of these formulae. Although explicit reference is made to the studies, it would be valuable to describe in more details the experiments, in particular detail which quantities were measured and in which conditions. This description should be systematic each time a new experiment is presented. This is particularly true for the data displayed in Figure 3 for which no information is given on the experimental method, nor on the formula that is used.

The experimental details are given in the referenced papers. I see no point in repetition.

(3) The application of the albedo formula to experimental data seems straightforward, but few details are given on the applicability of the formula to these very experiments. As calculations provide very satisfying results, it seems that the formula is very efficient. In fact, a crucial question when impurities are at stake is their localization within snow. The analytical derivations assume an external mixture, meaning that the impurities are out of the snow grains. In Hadley and Kirchstetter (2012), it is precised that the soot particles were hydrophilic, and thus were likely to be found within snow grains. In Brandt et al. (2011), the authors explain that the soot particles are also likely to be within snow grains, and they also discuss this point in the discussion. Thus it seems that there is some contradiction between the model assumptions and the physical reality. For these reasons, the localization of the impurities should be questionned, along with the fact that the theoretical formula seems in very good agreement with the experimental data, while the representation of impurities is likely to be inadequate.

It was assumed in the derivation that impurities are externally mixed. Such an assumption is consistent with the observed spectral reflectance of a snow layer.

(4) No reference is made to Zege et al. (2008) or Zege et al. (2011) whereas Eq. (30), which is the core of the present paper, is very similar to Eqs. (10) of Zege et al. (2008) or (11) of Zege et al. (2011). Hence the real novelty of the formula should be advanced in comparison with those existing formulae.

Additional references have been added as suggested by a reviewer. Eq. (30) is similar to the equations presented by other authors in earlier publications. The difference is that we propose to consider B_i and B_s as free parameters.

(5) Consistency of notations from previous papers of the authors is sometimes in default, which can be disturbing for the reader. Maybe this ultimate notation is the definitive one, otherwise it could be kept more consistent with previous studies.

I prefer to keep the current notation.

(6) The section titles are very formal. Clarity would probably be increased if these titles were more descriptive, such as "2. Derivation of an analytical formula for the albedo of polluted snow" and "3. Validation from laboratory and field measurements"

I see no point in the change of titles.

(7) I found that the end of the manuscript, when dust is at stake, is not very clear. It is puzzling to see that the dust concentration retrieved from the analytical model is almost twice inferior to the measured value of Painter et al. (2007).

The symbols are clearly defined in the new version of the paper. One possibility to explain the discrepancy of theory and experiment for snow covered by dust is the fact that the vertical distribution of dust concentration is not accounted in the model presented here.

Specific comments

I have accounted for all specific comments clarifying the text except those given below.

P. 535, L11: To what extent is it NEW compared to Zege et al. (2008, 2011)? What part of the paper/theory is NEW?
See above.

P. 536, L11: The derivation of Eq. (4) from Eqs. (3) and (1) is not straightforward. I'd recommend the following derivation:
I see no point in the change of derivation.

P. 536, L6: It's hard to see where Eq. (7) comes from, physically. Why talking in this paper of the random walk theory? Moreover, is it 2D or 3D?
Photons undergo random walk in a snow sample. 1D theory can be used.

P. 538, L18: Following Kokhanovsky (2004), the dense media effects are not really ignored, it just happens that they partly compensate in the case of weakly absorbing media(?). Or it depends what effects you are talking about.
Dense media effects are ignored in this paper. I do not like to discuss the compensation effects here.

p. 541, L3: I have the feeling aopt does not depend AT ALL on the shape. What is the use or the physical meaning of aopt? Is it an important quantity? Is it necessary to talk about it to understand the following text?
a_opt is a proxy for spectral snow albedo.

p. 541, L4: You say "also" but I don't see the difference between Eq. (25) and the sentence "can be directly derived from the reflectance measurements". If you mean that knowing aopt allows the prediction of the albedo, then how can you know aopt? I have the feeling that Eqs. (24) and (25) are the same, with just a new quantity defined.
a_opt can be derived from albedo at one wavelength and used to predict spectral albedo. Eqs. (24) and (25) are the same, with just a new quantity defined.

p. 542, L7: What is the use of Eq. (32)? If you do not use it further, then it is probably C1 not necessary.
Eq. (32) can be used by others in their investigations.

p. 544, L17: if you now write Bd then what is the difference between Bd and Bs?
Bd is the constant for dust. Bs is the constant for soot.