

Interactive comment on “A novel integrated method to describe dust and fine supraglacial debris and their effects on ice albedo: the case study of Forni Glacier, Italian Alps” by R. S. Azzoni et al.

Anonymous Referee #1

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General The paper entitled “A novel integrated method to describe dust and fine supraglacial debris and their effects on ice albedo: the case study of Forni Glacier, Italian Alps” by Azzoni et al. is surely interesting and fits well with the aims of the journal. The paper shows an interesting approach to evaluate the effect of the debris coverage on the albedo and therefore on the energy balance of an alpine glacier. Also the monitoring of the debris coverage changes during the ablation season and the effect of the liquid precipitation on the debris coverage and on the albedo are examined. The proposed methods sound good but on the other hand there some major points and

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several minor points that the authors should clarify in order to improve the manuscript. At the actual stage I think that the paper could be accepted on TC only in case that the authors improve the manuscript clarifying the obscure points and make all the proposed changes. Major Points: Title: In my opinion Title is not correct for two reasons: a) Dust and fine debris are not treated separately in the manuscript so also in the title is enough to state “fine debris” b) The main point is that the authors consider the debris coverage only supraglacial when is known that some or even the major part of the debris cover could be englacial and therefore related also to the glacier dynamic. c) How the authors distinguish supraglacial from englacial debris? Abstract: It is not well organized, it can be shortened and more focused on the main aims and main results of the paper. Results: In this sector there are several comments that should be in the Discussion sector. The main point is the validation of the method proposed to estimate the debris coverage and its evolution in the time. Why the authors did not consider to do in the field also another method (i.e. like a visual estimation of the percentage cover) instead to try with some statistical analyses not always clear and probably also auto referential. The other major point is that no statistical significance (p) of the regressions or correlations is presented without this parameter is not possible to verify the statistical meaning of them. Discussion: It is too weak, some of the discussion is now mixed in the results sector and no comparison with the results of other methods of debris coverage estimation or with paper regarding the effects of debris coverage and albedo on other glaciers are carried out Conclusion: It is too long and it seems more a summary of the activities and of the achieved results than focus only on the main results. Minor Points: Abstract Rows 3-6 could be “Despite of the abundant literature related to dust and black carbon deposition on glacier accumulation areas only a few studies on the distribution and properties of fine debris in the ablation areas are available” Rows 7-11 rewrite as “A new protocol to : (i) sample fine debris on melting glacier surface, (ii) quantify their surface coverage and the covering rate, (iii) describe composition and sedimentological properties, (iv) measure ice albedo and (v) identify the relationship between ice albedo and fine debris coverage.

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Introduction Pag.3173 Delete rows 2-10 "In fact, debris can be found. is found on actual debris-covered glaciers (see Kirkbride, 2011)." Pag. 3173 Delete rows 23-2 (3174) Pag. 3174 row 17 Here it is needed to define fine debris and dust because otherwise it is not clear to understand. Pag. 3174 row 19 Organic elements not necessarily by bacterial decomposition because also living organism or pollens can be part of the organic components. Pag. 3174 row 21 the paragraph is debatable because if the fine debris have englacial origin can be originated also by mechanical disintegration of the bedrock below the glacier or by the deformation and weathering processes of the rocks embedded in the ice. In addition, the distance of blowing material can be even much more than hundreds meters depending by wind speed and by the roughness of the area surrounding the glacier. Pag 3176 row 22 the sentence "The AWS1 Forni is already. . . inserted in the SPICE (Solid Precipitation Intercomparison. Should be deleted because it is not relevant for this paper. Methods Pag 3177 row 18 Why 1X1 m The authors should explain why they decide this size and not 0.5X0.5 m or 5x5 m? Pag. 3178 row 7-10 This sentence is quite confusing. If you have 445 images on 51 sites, means that you selected 1 pictures every 9? Is it correct? Pag. 3178 row 26 Which is the accuracy of d with the resolution of the used images? Pag. 3179 rows 1-9 Why the authors did not calibrate the images using others method like point intercepts or similar in the field ? Normally when someone wants to propose a new protocol should use another system to verify the results of the new one. This is one of the main point that the authors should solve. Pag. 3179 row 14 SWin is SWin ? Pag. 3179 rows 13-14 Delete the following :This parameter can range from 0 (all the SWin is absorbed by the surface) to 1 (all the SWin 1 is reflected). Pag. 3180 rows 20-21 why two years with 2 dates and one with three? Pag 3180 rows 26-28 Which was the accuracy of the rain gauge used? If the used rain gauge was not heated why do you use 1.5°C as temperature threshold to consider all the precipitation liquid? In literature also +1 and +2°C were considered so you have to explain and give a citation for justify the limit, but moreover if you have rain gauge measurements why you have to exclude data with temperature below some threshold and not use simply the data

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collected. Why consider only the days with more than 0.2 mm, is it because this the accuracy of the rain gauge? Pag. 3181 row 6 Who decide that 8 samples are enough? Which criteria do you use? Pag. 3181 row 15 From 2 to 5 cm is more than the double! It is not a real accurate sampling! Pag. 3181 rows 7-12 Why sampling was so different every year? C rate what is it, please define? Results Pag. 3183 row 4 Why the statistical significance (p) is not reported Pag. 3183 rows 10-15 Why is necessary to define a completely ice debris-free? It is not really clear and it is also clear that use an indirect measurements of the absence of debris like albedo is not completely correct because albedo of the ice depends also by its structure and roughness. Pag. 3183 row 23 30 events in which period? Why are not reported the rain values of the 30 events? Pag. 3184 rows 16-21 Macrogelivation ? Have the authors some data to say this? Not always macrogelivation is so efficient even in alpine environment. Slope erosion? What does it mean? This material is a product of the rock weathering or of slope erosion ? What does it mean recent? The authors have age of the deposits? All these statements seems just some general assumption not suffragated by any data. Pag. 3185 row 2 Which are the reason to say that it is a particular siderurgic site and not another? Pag. 3185 Row 9 who can say which are the averaged characteristics of the site? Pag. 3185 Row 26 more rounded shapes normally suggest endoglacial transport not supraglacial! Pag. 3186 Where the comparison between measured albedo are compared with the modelled ones? Discussion is partially a repetition of what included in the results section and in general is weaker withouth any comparison with results of relation between glacier and debris cover in other glaciers. Conclusion again are too long and more similar to an abstract than to focus on the main results obtained by the study Reference There are several errors like for example: Aoki et al 2002 is in the text but not in the list and Aoki et al 2006 is in the list but not in the text

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