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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.

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The authors certainly present a new type of analysis of fine debris on the ice facies of a glacier in Italy. The attempt at combining image analysis techniques with chemical / lab techniques is commendable. However, further implications of the method may be significantly limited by the methods chosen. I have some serious concerns about this paper which I believe need to be addressed in order for this paper to be considered for publication in The Cryosphere.

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Major comments:

1. A major issue that is not addressed by the authors is the prevalence of debris not only deposited from the air but also resident within the ice itself as it emerges to the glacier surface. Indeed, this paper distinguished debris accumulation on snow vs. ice, but much of the debris in the ice will have emerged after being compacted from snow into firn and then ice. This could have paleo implications rather than current ones. Understanding this concentration and providence is important to sort out the variables the authors discuss (including Cr, lithology, etc).

2. While the user-defined threshold is shown to correspond with debris concentrations better than an average threshold, the choice of threshold is largely arbitrary as the user defines it and therefore would be difficult for other users to adopt. More specifically, the choice of threshold to include concentrated clumps of fine debris vs. the ability to quantify the role of fine, distribute, and as-yet-not-consolidated debris is lacking. The threshold in the histogram is shown to be on a non-unique part of the curve – some feature in the histogram (whether a shoulder, valley, etc.) would be more convincing for a transferrable/scaleable technique. This is especially true in light of user-acknowledged difficulties with roughness, illumination, water content, etc.

3. The authors include consideration of liquid precipitation in their albedo discussion, but do not address the significant role that melt plays on the albedo of the surface. Quantity of melt and quality of the ice surface drainage are crucial to understanding the albedo and therefore the ability to correlate albedo and debris concentrations. E.g. see Pope & Rees 2014 in the International Journal of Earth Observation and Geoinformation for reflectance spectra of "dry ice", "wet ice" and different debris type surfaces in the ablation zone of glaciers in Iceland and Svalbard. Casey et al. 2012 in The Cryosphere also includes a consideration lithological remote sensing, as well.

4. The writing style of the paper presents the paper as largely anecdotal and exploratory in sampling. A tighter "argument" and presentation of the analysis in the

paper would greatly improve the readability of the manuscript and allow the reader to place it within a larger research context.

5. The authors astutely finish the paper by attempting to link their conclusions to more widely applicable remote sensing techniques. However, the study methods would frankly be very ill-suited to remote sensing application for two main reasons. One, there is no demonstrated link between the point measurements on the authors and actual satellite imagery. The greatly inhomogeneous nature of the glacier surface makes this important to demonstrate. Two, even sub-meter spatial resolution available in some commercial imagery would be an order of magnitude higher than the resolution in the images acquired in this study. The mixing in each pixel would be unable to distinguish the level of debris studied here. As such, a sub-pixel (spectral) mixing approach would be necessary for this study to be considered for upscaling. Although it is important to be able to define meaningful implications (especially for publication in The Cryosphere), in this conclusion, the authors reach too wide.

Directed comments:

p3173, l19-22: Dust and black carbon are also of extensive interest in the Arctic and Greenland. Extensive references by Dumont, Benning, Stibal, Anesio, Lutz, etc. would be appropriate here. Also, it would seem that a consideration of the role of ash as a fine particulate present in the Arctic (Iceland, Alaska, etc.) would be meaningful, too.

P3174, l3: correct to "In this contribution, we show the result from our research devoted to quantifying fine. . ."

P3175, last paragraph: In the major comments, Pope & Rees was referenced. We studied spectral responses of different ash/debris cover types, and so a standardized method to sample debris may in fact be helpful as a complement.

P3175, last line: "research" should be singular ("researches" is a verb conjugation, never a pluralized noun"

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P3176, last paragraph: none of the AWS network information is needed in the text.

P3177, L8: should be ". . .we performed 51 field measurements in total on the. . ."

P3179, L11: Brock likely is not the best citation here. Use something like Schaepman-Strub et al 2006 to be more specific about what "albedo" means in this case.

P3180, last paragraph: When considering the effect of liquid precipitation, it would seem to me that not just the physical action of washing needs to be considered – indeed, if some debris is being removed, where it is then going? Are the authors sure that it is not concentrating on the glacier surface? Also, the role that water plays in internal reflections is important – see Gardner and Sharp for a physical model.

Section 3.3 - Note to editor: I don't have a background in sedimentological analysis and so cannot comment on the specifics of their analysis Although the scraping, etc does make some sense to me. . . Although scraping could slightly change the surface routing of water, which wouldn't be independent

P3182, Line 21: "Image analysis yielded 51 d values ranging. . ."

P3182, Line 22: "radiometer varied from 0. . .."

P3182, Line 24:"are" instead of "result" Also "A" plot, not "The" plot.

—Fig 5 does give *some(comfidence, but also that the user is just doing something. To help "make it match"

P3183, last paragraph regarding precipitation: You take into account "liquid precipitation" but do you take into account liquid on the glacier's surface as a result of melt (e.g. days above vs. below freezing) / surface drainage? What about change in ice water content (even from rain) which would be expected due to the difference in spectral irradiance regarding relative amount of light in different wavelengths.

P3184: regarding local source of rock dust – Presumably the regional geology is all quite similar, and so understanding whether the dust was local to the glacier valley

as opposed to a few valleys over, or even more distal (but still in a similar geological province) is not possible?

P3185, lines 12-13: This sentence makes no grammatical sense and I'm sorry I can't understand what it is trying to say. One questions the rate of debris cover addition, but also how a disturbed area would become homogenized, but the importance of the sentence is unclear.

P3186: While the attempt to include error bars by varying d 10% is appreciated, I don't think this addresses the real issue of error in the method. It is that the selection is restricted only to obvious, concentrated dust. As discussed above, this means that the study is limited to particular kinds of fine debris and also that it not repeatable for another user in the same way as it is for these authors.

P3187: Your discussion does not include the role of melt-albedo feedback processes, whether positive (melt -> concentration -> darkening -> more melt) or negative (melt -> runoff -> lightening -> less melt).

P3187: This discussion also addresses the fact that there are spatial and temporal inhomogeneities in the distribution of debris and in glacier albedo. This is already a well-understood conclusion. For this paper to make a more meaningful contribution to the literature, more insight regarding controls on these distributions (as opposed to their presence) would be necessary. The sampling design will be important in these considerations.

P3187 L 9: Starting here, perhaps this is more conclusion rather than still discussion as it repeats earlier discussed material.

Figure 1: the AWS location (black star) doesn't appear be consistent in the map vs the image relative to the glacier terminus. The shading indicating moraines and nunataks also do not appear to be consistent between the image and the map.

Figure 7: plotting in a bar graph in that way implies some meaning to the x-axis of the

measurements. Perhaps better to plot in a meaningful order or use a different kind of plot (histogram, etc.)

Figure 8: dAVE depends as much on the image collection (exposure, camera model, etc.) as anything else, that its subjective nature precludes its future use as a threshold for others to be able to use. Also, it appears like the correlation may be driven by a small number of almost-outliers (around d > 0.5). These paired considerations need to be addressed to make the inclusion of an average threshold more compelling (or, limit the scope and define that consideration of the average is only to demonstrate the utility of the user-defined threshold).

Interactive comment on The Cryosphere Discuss., 8, 3171, 2014.