The Cryosphere Discuss., 6, C323–C325, 2012
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The Cryosphere Discussions

Interactive comment on "Formation and metamorphism of stratified firn at sites located under spatial variations of accumulation rate and wind speed on the East Antarctic ice divide near Dome Fuji" *by* S. Fujita et al.

Anonymous Referee #1

Received and published: 17 April 2012

Fujita et. al investigated with non-standard semi-quantitaive high-resolution methods the firn at three antarctic sites. The paper has interesting new data, but I found the logic of the exposition of the paper and the interpretation of the data difficult to follow, and sometimes I could not follow the logic. In the following I try to describe and suggest possible improvements.

The main motivation of this paper is probably to clarify the role of insolation on metamorphic processes close to the surface.

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Section 1 is way to long for the content presented afterwards. In many respects, it is a repetition of Fujita et al (2009). Subsection 1.1 cites many papers irrelevant to the supposed context of this paper. Section 1.2 is not important at all - lines p 1209 l 25 to the end of p 1210 is a lengthy self-citation of Fujita et al (2009). Subsection 1.3 seems to be a justifaction. In this context, it remains unclear to the end what is NOT correct with the paper by Hutterli et al. (2009), because this paper exactly suggests insolation as a major factor. Subsection 1.4 belongs to methods (which kind of continue in subsection 2.1). These two sections have no relevance at all to the probably intendend purpose of this paper. Section 2.1 p 1214, l 8 ends with sentence which belongs either to the introduction or the discussion. Actually, this sentence is key in the sense that the authors do not seem to understand the local spatial variability, as excellently described by Kameda et al. (2008).

The methods section is poorly focused. Some data are irrelvant in this context (weather data during the traverse), some are overly lengthy (2.2.2), (2.2.3) Subsubsection 2.2.4 part i: is this already published elsewhere? The evaluation does not take into account two really relevant paper by Maetzler and Maetzler and Wegmueller. The section iii on near-infrared reflectivity is very crude - there are more refined methods, eg. see Domine et al and Painter et al, which quantitatively measure specific surface area or optically equivalent diameter, and not a grainsize INDEX. The method as presented here needs substantially deeper evaluation and quantification before it can be used for the interpretation of snow metamorphism.

Section iv is more an appendix, as it is used just for one figure, but not really used - probably because samples close to surface were too fragile? It is a pitty that not the Japanese inventend casting techniques was applied for this study!

The results section is very poorly organized, it is a mixture of results, discussions and conclusion".

The discussion on grain growth and metamorphism is very weak. No reference is made

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to snow metamorphism (see papers by Sturm, Dominé, Pinzer and others). From a physical point of view, the term "firn" used here is not very helpful, because it is "low-density" firn, so normal snow.

The concluding remarks (section 9) because of this ignorance of a wide body of work not novel.

The paper in its present form should be rejected. The data should be re-analyzed (eg. reflectivity, permittivity, stratigraphic interpretation) and the paper re-written (the literature of snow metamorphism should be included).

Other comments l 1: What is "postdepositional metamorphism"? All metamorphism in firn is postdepositional, so it is just a pleonasm.

References Zhou, Y., N. Azuma, T. Kameda (2002), A stratification model of surface snow at Dome Fuji Station, Antarctica, Polar Meteorology and Glaciology, 16, 61-73.

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Maetzler, C. (1996), Microwave permittivity of dry snow, IEEE Transactions on Geoscience and Remote Sensing, 34(2), 573-581, doi:10.1109/36.485133.

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Interactive comment on The Cryosphere Discuss., 6, 1205, 2012.