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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.

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The authors thank the handling editor, reviewers for their efforts for the manuscript. We carefully read the comments by the Reviewer #1. We thank the reviewer for his/her careful, critical and constructive review. We herewith provide replies point by point. Our order of reply is different from original order of comments by the Reviewer #1. The order was rearranged in a sense to make discussions less complex. We itemized points.

Outline

1. Scientific terms
1-1 "Firn"
1-2 "postdepositional metamorphism"
2. Criticisms for not citing a few of snow optical studies and snow metamorphism
2-1 The method section
2-2 The discussion section
3. Criticisms for the concluding remarks
4. Criticisms for the introduction
5. Criticisms for the method section
6. Criticisms for the result section
7. Criticisms in general
8. More citations for snow/ice microwave dielectric property papers
9. Other changes planned

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1. Scientific terms
1-1 "Firn"

[R1: Reviewer #1]
From a physical point of view, the term "firn" used here is not very helpful, because it is "low density" firn, so normal snow.

[A: Authors]
In the manuscript, we used terms both "snow" and "firn" depending on context.
We cite here an explanation of "firn" in a textbook of Glaciology (Cuffey and Paterson,

2010), which is useful as a basis of our discussion.
Sentences below are cited.
“The original strict meaning of firn is wetted snow that has survived one summer without being transformed to ice. This narrow meaning is no longer accepted, as firn also refers to altered snow on polar glaciers where no melting occurs. The broad definition, however suffers from the lack of a clear division between snow and firn, an ambiguity that we accept. We may sometimes use “snow” when “firn” would be more appropriate. The absence of a clear division reflects the continuous nature of snow transformation; there are no abrupt changes in physical properties, common to all glacial environments, which could as a basis for demarcation.”

Our samples are firn (and at the same time, snow) in the 2.5-m pit that covers a period of time up to 40 years deposition that altered from the original state of the timing of deposition, exposed to diurnal, seasonal and more changes of the physical conditions. In the light of the explanation of firn in the textbook, it can be naturally termed either as firn or snow, considering the continuous nature of snow transformation into ice. As the title of this paper, we are interested in firn nature to depth of bubble close-off. Then, we hope to insist on the term “firn”. To unify the term as “snow” seems to give an impression to readers that we discuss some material distinctly different from firn. We do not hope this situation. It seems to us that we do not have a unique solution. We hope either to keep the terms of the present manuscript as they are, or to unify as “firn” mentioning the background stated by Cuffey and Paterson (2010)

Reference
Cuffey, K. M., and Paterson, W. S. B.: The Physics of Glaciers, Fourth ed., Academic Press; 4 edition (May 17, 2010), 704 pp., 2010.

1-2 “postdepositional metamorphism”

[R1]
What is "postdepositional metamorphism"? All metamorphism in firn is postdepositional, so it is just a pleonasm.

[A]
We cited this term from some geology papers. The reviewer pointed out that “postdepositional” is wordy adjective; we accept to drop of this adjective.

2. Criticisms for not citing a few of snow optical studies and snow metamorphism

2-1 The method section
The reviewer #1 criticized this point as a major problem of the present manuscript. The first criticism is for the method section.

[R1]
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 grain size INDEX. The method as presented here needs substantially deeper evaluation and quantification before it can be used for the interpretation of snow metamorphism.

[A]
We agree with citing suggested “more refined methods” papers in the method section. As for the spectral reflectance of snow, in the manuscript we already cited some of important papers, Wiscombe and Warren (1980), Aoki et al. (2000) and Matzl and Schneebeli (2006). However, our citations did not cover a few papers that were

published within recent several years. We cited Matzl and Schneebeli (2006) because it is directly related to the present manuscript. It showed the field examples of the NIR reflectance of snow at wavelength of 840 to 940 nm, which is close to ours (940nm with half power width of about 50 nm).

We assume that the reviewer #1 suggested us addition of some of papers described below. Domine et al. (2006) investigated a correlation between reflectance and the SSA in a wavelength range covering 1310, 1629, 1740 and 2260 nm, using coastal snowpack in the Arctic. After this work, Domine et al. (2007) published a relation between SSA values of snow samples from snowpacks of the Alpine, maritime, tundra and taiga types. They proposed parameterizations of snow SSA with increasing sophistication, by correlating SSA to snow type, density, and snowpack type. More recently, Gallet, Domine et al (2011) investigated vertical profile of the specific surface area and density of the snow at Dome C and on a transect to Dumont D′Urville, Antarctica. Their analyses are mostly firn to depth of 0.7 m. Painter et al. (2007) presented a technique for in situ measurement of the vertical and spatial stratigraphic distribution of snow optical grain size with a coupled contact illumination probe and field spectroradiometer.

We will cite and we should cite some (perhaps not all) of them. One point that requires consideration is whether or not and how much the authors should translate our NIR R values to SSA or optical equivalent diameter citing these very recent snow studies of various types in cold regions. In terms of Physics, discussions with raw R are simpler and in some sense more robust. It can be further developed as future readers want. In contrast, discussions with SSA mean that the authors assume data based on the various snow types above are fully (or basically) applicable to firn that we studied; there is one step here in that sense. One very important point is, however we may call it as a measure of “grain size”, “SSA” or “optical grain size”, they commonly belong to a category of “further interpretation”. For example, SSA is

inversely correlated to grain size. In terms of Physics, R is exactly what we measured. Then it is very healthy to use R for preliminary analysis. Another important point is that introducing “SSA” or “optical equivalent diameter” does not change context of basic discussions and our conclusions at all. In addition, for us it seems healthier that we develop more detailed discussions when we get more data about firn properties here.

Considering all these above, we hope to choose a practical choice. We will attempt to add reference axis of SSA in Figures 12b as top axis and Figure 13 as right axis citing Domine paper(s) with a clear description that we tentatively assume their calibration curve for their snow types is applicable to the Dome Fuji firn. Such giving axes provide just a tentative measure of SSA to readers. At the same time, we add some short comments in terms of SSA in the manuscript. Such additions will not change any of flow of discussions or conclusions. We believe that this is a positive action, responding suggestion of the reviewer #1.

2-2 The discussion section

[R1]
The discussion on grain growth and metamorphism is very weak. No reference is made to snow metamorphism (see papers by Sturm, Dominé, Pinzer and others).

[A]
To answer to this criticism, explanation of background is necessary. It is difficult to reply with simple words. Please accept that we use some detailed explanations below.

Probably, we should have cited a few key papers on snow metamorphism papers at the beginning of metamorphism discussions at Page 1225 Line 4. However, it seems to us that papers by Sturm, Dominé, Pinzer and others are not very good choice.

As for papers related to snow deposition, diagenesis and transformation to ice, in the manuscript, we have cited papers including following; Alley, R. B.(1988), Alley et al.(1982), Arnaud et al. (2000), Birnbaum et al.(2010), Colbeck (1983), Courville et al.(2007), Dang et al. (1997), Freitag et al. papers (2004, 2008) , Gow (1965), Howley and Morris (2006), Hoehold et al. papers (2009, 2011), Hutterli et al. (2009), Iizuka et al. (2004), Koerner (1971), Pfeffer and Mrugala (2002). Among them, Colbeck (1983) is directly related to theory of metamorphism of snow. Pfeffer and Mrugala (2002) showed importance of the temperature gradient and initial snow density. There are very many other studies on both basic principles of the snow metamorphism (both isothermal metamorphism and temperature gradient metamorphism) and examples of snow in the field. We surveyed many of them in writing the manuscript. In writing the paper we simply considered that citing further papers on snow decrease readability of the manuscript, because many are related to detailed behavior of seasonal (winter) snow under temperature condition much higher than Antarctic inland. Focus of earlier paper seems rather different from ours. Seasonal snow is often on the substrate of relatively warm soil that provides heat and moisture. Several papers published by Dr. Strum (e.g., Strum and Benson, 2004; Strum and Benson, 1997; Sturm et al., 1997) are on this type, which cannot be cited in a simple manner. In our manuscript, discussions are on the firn conditions of very low accumulation rate, very low temperature, long years of metamorphism, very special conditions of the heat wave or precipitation regime, conditions without warm soil substrate that provide moisture. Because we cannot cite them without detailed explanations for large and variety of differences of the physical conditions, we did not cite Strum papers. It seems to us that Strum papers can be cited as vague and general mention for earlier works.

The reviewer #1's criticism includes our not citing Pinzer papers. Pinzer and Schneebeli (2009) published a paper on the snow metamorphism under alternating temperature gradients. They studied morphology and recrystallization in surface snow

under temperature range of -10 °C. This paper discusses faceting and rounding of grains in relation to temperature gradient. However, this paper discusses some specific features of snow; is not easily (or naturally) included in the context of our manuscript.

As for the Domine paper(s), handling is described above in #2-1

Overall, we will cite some from Domine paper(s) and/or Painter paper. As for the Strum paper(s) and Pinzer paper(s), we believe that it is not very suitable or natural to cite them in the present context of our manuscript. Before submitting our paper, we searched for papers that can generally represent snow metamorphism phenomena. We found that it was very difficult to cite earlier metamorphism papers in each portion of our discussions in the manuscript.

References

Domine, F., Salvatori, R., Legagneux, L., Salzano, R., Fily, M., and Casacchia, R.: Correlation between the specific surface area and the short wave infrared (SWIR) reflectance of snow, Cold Regions Science and Technology, 46, 60-68, 10.1016/j.coldregions.2006.06.002, 2006.

Gallet, J. C., Domine, F., Arnaud, L., Picard, G., and Savarino, J.: Vertical profile of the specific surface area and density of the snow at Dome C and on a transect to Dumont D′Urville, Antarctica - albedo calculations and comparison to remote sensing products, Cryosphere, 5, 631-649, 10.5194/tc-5-631-2011, 2011.

Painter, T. H., Molotch, N. P., Cassidy, M., Flanner, M., and Steffen, K.: Instruments and methods - Contact spectroscopy for determination of stratigraphy of snow optical grain size, JOURNAL OF GLACIOLOGY, 53, 121-127, 10.3189/172756507781833947, 2007.

Pinzer, B. R., and Schneebeli, M.: Snow metamorphism under alternating temperature gradients: Morphology and recrystallization in surface snow, Geophysical Research

Letters, 36, 4, L23503 10.1029/2009gl039618, 2009.
Strum, M., and Benson, C. S.: Vapor transport, grain growth and depth-hoar development in the subarctic snow, J. Glaciol., 43, 42-59, 1997.
Strum, M., and Benson, C.: Scales of spatial heterogeneity for perennial and seasonal snow layers, Ann. Glaciol., 38, 253-260, 2004.
Sturm, M., Holmgren, J., Konig, M., and Morris, K.: The thermal conductivity of seasonal snow, JOURNAL OF GLACIOLOGY, 43, 26-41, 1997.

3. Criticisms for the concluding remarks

[R1]
The concluding remarks (section 9) because of this ignorance of a wide body of work not novel."

[A]
If we see each single part of descriptions of our concluding remarks, there may be some phenomenon that was described before for some specific conditions of seasonal snow. However, in terms of polar firn, our concluding remarks should be reasonably described as conclusion of our work, we believe. Again, difficulty appears by difficulty for integrating earlier (seasonal snow in Europe and North America) metamorphism works to explain for the polar firn.
As we stated in #2-1 and #2-2, we never did such "ignorance of a wide body of work". As a context to understand initial stage of firn metamorphism in polar plateau, it is really new, after big efforts of research.

4. Criticisms for the introduction

[R1]
The main motivation of this paper is probably to clarify the role of insolation on metamorphic processes close to the surface. 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 justification. 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 intended 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).

[A]
Introduction part is something one of the most difficult works to write a scientific paper: we are willing to improve. There is no unique solution and in any cases there are rooms to be criticized. In revising the paper, we utilize the reviewer #1's comment very seriously. We provide some comments below.

(i) Main motivation of this paper was described using abstract and introduction. It is to better understand physical process of initial stage of postdepositional metamorphism and its relation to the bubble close-off processes and astronomical dating of the deep ice core. Because this paper covers vary wide range of physical phenomena, we took some length to explain concrete background to readers. This lengthy introduction has a risk to decrease readability. This was at the same time a cause of criticism.

(ii) In the introduction, we cited some essential conclusions from Fujita et al. (2009) because we believe it is very important as information of how firn properties evolves and influence gas transport. We believe that Fujita et al. (2009)'s discussion is much improved one compared with the other earlier hypotheses. Section 1.2 provides important information on how transport process of gas molecules in firn during bubble close-off occurs. On this point, we are against view of the reviewer who judged as "Section 1.2 is not important at all". These phenomena gave strong motivations to researchers to better understand the firn processes. It should be introduced. However, the authors are ready to attempt to make it short for a better readability.
(iii) The reviewer #1's comment on the subsection 1.3 is incorrect. Hutterli et al. (2009) claimed that accumulation rate plays important roles for the temperature gradient metamorphism to modulate real effect of insolation. We believed that it the effect of the accumulation rate to modulate firn properties was overemphasized. Our conclusion in this paper is that the effect of the accumulation is less than Hutterli et al. (2009) claimed. (iv) Subsection 1.4 was intended to give concrete introduction of work what we did. Responding the comment of the reviewer #1, we agree that we move some part to beginning of the method section, reorganizing the beginning of the method section.
(v) The last sentence of the reviewer #1 was not understandable for us. Very local spatial (cm to 10 m) variability of the deposition is a problem different from average accumulation rate and its seasonal characteristics.

5. Criticisms for the method section

[R1]
Itemization (i)-(iv) below was by the authors.
(i) The methods section is poorly focused. Some data are irrelevant in this context (weather data during the traverse),

(ii) some are overly lengthy (2.2.2), (2.2.3)
(iii) Subsubsection 2.2.4 part i: is this already published elsewhere?
(iv) 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?

[A]
(i) Weather data were presented to show conditions of temperature and wind conditions (speed and directions) typical in the investigated area. The data concretely demonstrate conditions under which metamorphism of snow occur, to readers. Indeed, figure 2 was cited several times in the paper.
(ii) If (2.2.2), (2.2.3) are too long, we are willing to leave only essentials in the main text and move some parts to appendix. However, to make appendix also makes a risk to decrease some readability. We hope to keep present condition.
(iii) As for the subsubsection 2.2.4 part I, the method have been described in (Fujita et al., 2009; Jones, 1976; Matsuoka et al., 1997, 1998; Komiyama et al., 1991).
(iv) LASM data and some other data are under more investigation and processing, which can be more developed in future papers. But it will take some time. Using one of LASM images, we demonstrated how the firn sample looks as geometry of the surface, as a basis of showing D in Figure 7a. We believe that we do not need send them to appendix. In addition, we do not need to show more of such examples in the context of the present manuscript, which will decrease readability.

6. Criticisms for the result section

[R1]
The results section is very poorly organized, it is a mixture of results, discussions and conclusion".

[A]
Some mixtures of discussions were unavoidable because this study is handling very complex system and chain of various phenomena. It is not as simple as ordinary laboratory experiments or relatively simple field observation that can be handled with simpler structure of discussions. Inside of this study, there are variety of measurements and data to better understand the complex system. Otherwise, it seems to us that we need to split this paper into two or more papers.

7. Criticisms in general

[R1]
Fujita et al. investigated with non-standard semi-quantitative 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.

[A]
Like the introduction, this is something that the authors should always improve. We are willing to improve them according to necessity.

8. More citations for snow/ice microwave dielectric property papers

[R1]
"The evaluation does not take into account two really relevant paper by Maetzler and Maetzler and Wegmueller."

[A]

In revising the paper, we will cite Maetzler (1996) at Page 1222 Line 23, possibly with some other studies of snow density and the real part of the dielectric permittivity. Historically, there are several of such papers. We examined a possibility to cite Maetzler and Wegmueller (1987) but we believe that we cannot cite it naturally in the context of the present manuscript.

9. Other changes planned
Responding to a criticism for insufficient citations for metamorphism, we plan to add reference papers on thermal conductivity of firn (Strum et al. 1997) at Page 1228 Lines 12-16.

Reference
Sturm, M., Holmgren, J., Konig, M., and Morris, K.: The thermal conductivity of seasonal snow, JOURNAL OF GLACIOLOGY, 43, 26-41, 1997.

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