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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 #2. We thank him/her for raising several questions suitable for open discussions. In addition, the review comments are only for essential scientific points; frankly, we are pleased to see this nature of the review. We found that responding to the reviewer's questions strengthen science of the present manuscript. In particular, questions related to very recently published paper Hörhold et al. (2012, EPSL) privide necessary points for open discussion. We

herewith provide replies point by point. The reviewer #2's comments are given using italic font. Our comments are given using plain text. Each comment and answer was numbered in order to be cited easily in future discussions. As for citations, we basically use reference paper list of our TCD paper. Only when we mention some paper(s) not listed in the TCD paper, we provide information of it in this document.

[R2: Reviewer 2 #1]
Fujita et al report results from snow pit studies from one of the most remote regions in East Antarctica. The data are new, interesting, however they are presented in a context much to much focussed on the insolation (Milancovitch) signal observed in O2/N2 and the total air content in ice cores trying to explain the insolation signal's origin.

[A: Authors #1]
Our motivation and purpose is expressed in the first five lines of the abstract. Background was carefully explained in the introduction and in our previous paper (Fujita et al. (2009) JGR). If the reviewer #2 meant "too much focused", it seems to us the criticism is not understandable or something that the author cannot accept. It is just one of most essential scientific focuses of this paper, which we and related scientists are keen to understand.

[R2 #2]
Studies of the formation and metamorphism of firn in the central parts of East Antactic plateaus are important because it is the area where the longest ice core records come from. What controls densification of firn and air enclosure? Which processes determine the age difference between ice and air, thus the gas age, any temperature-CO2 lead-lag analysis but also the Milancovitch signals in ice core used to date ice core records? Is it the depositional history (precipitation, redistribution by winds and metamorphism) at the surface or the impurities in the ice as proposed recently by

Hörhold et al. (2012) but published after this manuscript was submitted? What is the relevance of the manuscript?

[A #2]
A paper Hörhold et al. (2012, EPSL) appeared on line of EPSL after our TCD paper was submitted. We will comment on the data handling and claims of the Hörhold et al. (2012, EPSL) paper later in this document. Also, we will add our comment on them in the revised version of our paper. Please see [A #13].

[R2 #3]
Is it possible to make the case that in some way thermal processes as proposed by Fujita et al. imprint an insolation signature at or close to the surface in the firn matrix which is preserved through the entire firnification when the density increases by a factor of almost three from 300 kg/m3 to more than 820 kg/m3?

[A #3]
It seems to us that no scientific logic is given by the reviewer as to why the range of density variations matter. We have discussed the initial imprint of the physical/mechanical properties of firn which is retained throughout the densification process in our TCD paper and in Fujita et al (2009, JGR) paper. Raising a question mentioning the range of density variations seems to us unreasonable.

[R2 #4]
I am not convinced that the top-down approach will be successful. We may understand the nature of the insolation signal in O2/N2 or total air content (TAC) much earlier by a bottom up approach after we have understood how the air enclosure works. Of course, I may not be right. However, right now we have no clue how the insolation signal becomes imprinted in the air components.

[A #4]
We feel that the reviewer's comment is vague and lack of clear scientific logic. Investigation of surface nature of firn is to better understand the initial imprint of the physical/mechanical properties of firn which is retained throughout the densification process. It is very healthy and necessary approach. Physics related to bubble close-off has been investigated (e.g., Fujita et al. 2009, JGR) as well. To investigate totally from the surface to the close off is quite healthy approach. There seems no scientific benefit to say and to separate/distinguish between "top-down approach" or "bottom-up approach". Each part of the entire causal chain should be understood by our community. Our focus was uppermost several meters of the ice sheet which is still poorly understood. Further investigations are very necessary. Criticism seems to us unreasonable.

[R2 #5]
What I mean is that the model Fujita et al. develop misses an important part, the energy balance of the surface. Without a thorough energy balance of the surface and the firn close to the surface it is hard to understand which components of the energy balance contribute to the formation of density signal and so on.

[A #5]
It is clear that insolation is the strongest energy input at the surface in inland of Antarctica. If the reviewer means that he/she hopes to see entire processes of forming each layer's metamorphism due to insolation, albedo and all other factors controlling the heat budget, our community needs to build models covering all meteorological conditions, insolation and metamorphism. Such attempts were preliminarily done by several researchers as we cited in the TCD paper. Perhaps snow metamorphism models such as "Crocus" or "Snowpack" may reasonably work in future for Antarctic firn. However, to our knowledge, present stage is just a stage to check availability of such snow condition models in Antarctic firn (e.g., Brun 2011). Such studies will be

more developed in future. However, a criticism of "no energy balance model in this paper" seems unreasonable request to the TCD paper. It is beyond the scope of the TCD paper.

Reference
Brun, E., Six, D., Picard, G., Vionnet, V., Arnaud, L., Bazile, E., Boone, A., Bouchard, A., Genthon, C., Guidard, V., Le Moigne, P., Rabier, F., and Seity, Y.: Snow/atmosphere coupled simulation at Dome C, Antarctica, JOURNAL OF GLACIOLOGY, 57, 721-736, 2011

[R2 #6]
Is this study relevant to understand the insolation signal in O2/N2 and TAC?

[A #6]
Our answer is Yes. We have discussed how stronger contrasts of IHDF/ILDF cause more amount (and duration) of gas transport during close-off in Fujita et al. (2009, JGR).

[R2 #7]
I am also not convinced by the data presented. They present many aspects and sometimes it hard to follow how all these aspects are interrelated. Are three pits representative enough?

[A #7]
As for the question for the representativeness of the data, we have discussed it in the paper. We have demonstrated that spatial distribution in the depositional environment (Figure 4 (d, e and f)) is visible in the three pits data (Figure 5). In addition, each pit contains data covering period of time more than 20 years. As for the readability, it is something that any authors must always try to improve; in revision of the paper, we will

try to make better readability, responding each of review comments.

[R2 #8]
A whole series of pits would be needed similar to the work the same authors presented in their paper by Sugiyama et al. (2012) or fig. 4f.

[A #8]
It is a very good point. Our Figure 4 is consistent with data in Sugiyama et al. (2012). For example, density variability between DF and MP in their Figure 5b (between $\sim$1010 km and $\sim$1450 km in abscissa) are very similar to our Figure 4. In addition, spatial distribution of grain size in their Figure 5c is also consistent with our pits. Thus, data in (i) our 18-cm pit works (Figure 4 (d, e and f), (ii) three 2.0-4.0 m pit works (at DF, DK190 and MP) and (iii) 1-m-deep pit works by Sugiyama et al (2012) are consistent with each other. It is indeed double cross-checks. This situation convinced authors that the three pits are representative enough for the depositional environment at each location of the pit. Responding to the review comment, we will add a comment on this in the manuscript.

[R2 #9]
The insolation aspect in the introduction is much to long. As one motivation it can be condensed to a single paragraph. There are other interesting aspects than the insolation signal.

[A #9]
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. As we commented to the reviewer #1 (at Page C395, Replies to criticisms for the introduction), we are ready to make this section shorter more or less. However, without introducing thorough background, it is very difficult to develop

dynamic presentation and discussions for the topic of this paper. Background of this work is quite wide and situation of this work is not simple. We hope the reviewers to see scientific benefit of the present paper and benefit of introduction of thorough background to wide readers.

[R2 #10]
Is there a change in the predominant type of precipitation? Is it diamond dust / clear sky precipitation as it is so common at Dome Fuji?

[A #10]
We have discussed it in the paper Fujita et al. (2011, TC). Please see a section "1.3 Processes related to SMB".

[R2 #11]
The mean surface density seems to be pretty stable despite the change in elevation, accumulation rate and wind in the paper by Sugiyama et al (2012). Are the same data shown also in Fig. 4 which indicate somewhat higher density around site DK190 or is it a different data set?

[A #11]
Data sets of Sugiyama et al (2012) are different from ours. As we answered in [A #8] above, three different sets of data have consistent features in terms of density and grain size.

[R2 #12]
Is the D2m a meaningful grain size if site MP is only half as old as Dome F?

[A #12]
There is no problem. Rather, it is one of our scientific points. We discussed enhance-

ment of metamorphism as a function of residence time within the shallow portion of the ice sheet.

[R2 #13]
The cross over of initially high/low density firn has now been questioned by Hörhold et al. (2012). Impurities seem to play an important role for the entire firnification in the deeper firn and modulate the density of deep firn. This probably means that the surface density signature is not important for the densification of the deep firn and the density structure at the firn-ice transition where the air is entrapped in ice. What is the consequence for your interpretation and conclusions?

[A #13]
It was a bit surprising for us to see that Hörhold et al. (2012) seems to give up the idea of the cross over of initially high/low density firn that these authors had in their earlier papers. We have several concerns to both data handling and discussions of the Hörhold et al. (2012) paper. We believe that Hörhold et al. (2012) paper needs be cited only carefully at the moment. Our concerns are as follows.

i. Hörhold et al (2012) seems to emphasize development of correlations between density anomaly and Ca++ content with increasing depth. However, we find essentially no depth-dependent evolution of the correlation at depths below $\sim$15 m depths in their data. This implies that the correlation does not mean any link in terms of physics (e.g., softening of ice due to impurity). Rather, it seems likely that the correlation simply mean seasonal variation of impurity amount at the surface. It seems just a synchronicity between variation of the impurity concentration and IHDF/ILDF variations.

ii. Correlation coefficient is generally low except some limited depths of Greenland sites. It seems that Hörhold et al. (2012) paper gave too much emphasis on some limited depths of Greenland sites in their presentation. This kind of bias may cause a risk of misleading.

iii. Their Figure 1 is presented in a way to emphasize the evolution of the correlation coefficient at depths near the surface. However, it seems to us that their presentation is quite tricky. By using logarithmic scale to Ca++ data, both the authors and readers lost a chance to examine negative correlations between density anomaly and Ca++ near the surface. In Greenland, Ca++ concentration is often high in spring and early summer. Almost at the same timing of season snow density is low in case of Greenland (it is opposite in inland of Antarctica). Then, there should be negative correlations between Ca++ concentration and density. It seems to us that the cross over of initially high/low density firn does occur.

iv. In contrast to the explained causal chain of the cross over of initially high/low density firn, no convincing causal chain of the impurity effect (logarithmic of Ca++ concentration) is given.

In summary, scientific claims of the Hörhold et al. (2012) paper should be tested by our community in the near future using both Greenland firn cores and Antarctic firn cores. In particular, the initial correlations between Ca++ and density should be demonstrated without any bias. We will cite Hörhold et al. (2012) paper near P. 1234 Line 24.

[R2 #14]
The paper deals with to many aspects. For example, Fig. 9 does not give much additional information. Probably also Fig. 14-16 are not very helpful.

[R2 #14]
We disagree with these comments. To see reality of the firn (Figure 9) means much to readers. Why just showing a microscopic image is a problem? The reviewer's

criticism are to presentations for real conditions of the temperature distribution both in firn and air (Figure 14), time series of deposition (Figure 15) and an explanation for factors controlling formation of firn strata (Figure 16). They are all very important for better understanding the scientific topics in this study. It we discount (or disregard) such real data, readability of the papers will be worse. Simple figures often tell to readers much more than words. In addition, we commented in our TCD paper that a recent claim by Laepple et al. (2011a,b) that there is a strong seasonality in the accumulation rate (more than $\pm 30\,\%$) in plateau of East Antarctica at present climate is not the case at DF (Page 1232, Line 23). If these authors show a figure like our Figure 15, real condition would have been apparent in our scientific community.

Once again, we thank the reviewer #2 for providing us the review comments in terms of essential scientific points of the TCD paper.

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