The Cryosphere Discuss., 5, C1060–C1066, 2011
www.the-cryosphere-discuss.net/5/C1060/2011/


The Cryosphere
Discussions



# *Interactive comment on* "Spatial and temporal variability of snow accumulation in Dronning Maud Land, East Antarctica, including two deep ice coring sites at Dome Fuji and EPICA DML" *by* S. Fujita et al.

**S. Fujita et al.**

sfujita@nipr.ac.jp

Received and published: 2 October 2011

First, the authors greatly appreciate the solid review. We understand the main concerns. Though real revision work is from now, we describe our tentative reply here using an advantage of unique system of the Interactive Discussions of the journal TC. Our present plan for revision responding the review is described one by one below. To make the paper more concise as suggested by reviewers, we plan to remove two major items from the manuscript. One is comparison of SMB values with those of Huybrechts et al. (2009). Another item is radio wave scattering from within ice as suggested. It can be developed somewhere in future paper. Figures will be modified accordingly. Figure 3 will be removed to make more concise paper as suggested. Below, our present plans and/or views are described. Each item is numbered to make interactive discussions smoother.

Please note that <# reviewer2> and <# authors> below are comments from the reviewer #2 and authors, respectively.

Could you please reply us for our questions to you in <#13, authors> and <authors #19>?

<#1, reviewer2> My main concern is interpretation of wind impact on SMB. Previous authors point out the correlation of slope along wind direction with SMB, but slope along wind direction is very different with surface topographical slope along the traverse route used by Authors. Moreover the traverse is at ice divide or closer and generally is not subject to katabatic wind. As point out by authors, the orientations of surface snow relief agree with orientation and rotation of storm track. On my opinion is more correctly to speak leeward and downward (shadow effect) respect to storm track or maritime moisture source as point out by authors Par 4.3.1. Relationship between SMB and wind is quite different at ice divide and along the ice sheet slope, where katabatic wind has very strong impact on SMB due to erosion/ablation process. Analogous condition of shadow effect occurs at Dome C (Urbini et al., 2008). Authors should take in account the difference between wind erosion due to katabatic wind and source of moisture/shadow effect and modify the text and figures appropriately.

<#1, authors> We agree that it would be confusing for readers if phenomena near the ice divide and phenomena on the ice sheet surface is not clearly distinguished. We will be careful on that point

<#2, reviewer2> In addition, Authors should improve the readability of manuscript with merge and condense generally the manuscript and in particular Paragraphs 2.2.3, 2.2.4, 2.2.5, postpone to a future manuscript the analysis of backscatter VHF radio

waves and relation with SMB, withdraw fig 3 and backscatter from others figures.

<#2, authors> We will respond as suggested.

<#3, reviewer2> I would suggest to use everywhere ice divide instead of ridge, also in the title I would suggest to use as location "East Antarctic ice divide between EPICA EDML and Dome Fuji."

<#3, authors> We will respond as suggested. The revised title will be "Spatial and temporal variability of snow accumulation on East Antarctic ice divide between Dome Fuji and EPICA DML".

<#4, reviewer2> Authors should homogenize or specify the difference between sub-surface radar sig- nals, snow radar, GPR. Snow radar and GPR are normally used as synonymous in SMB study.

<#4, authors> A solution seems to us that we just call these radars as the Swedish GPR and Japanese GPR, to make it simpler.

<#5, reviewer2> 2067/25 Why do you use time lag difference between Agung and Pinatubo eruption?

<#5, authors> (3) Please see our reply #8 to the anonymous reviewer 1. Two year lag was reported between the eruption timing and peak timing of deposition.

<#6, reviewer2> Paragraph 2.2.3-2.2.4-2.2.5, should be merge and provide information on resolution, uncertainty, investigation depth, scan rate, samples per trace.

<#6, authors> The manuscript will be revised as suggested.

<#7, reviewer2>

2069/10 uncertainty of horizon dated, and impact on SMB.

<#7, authors> The uncertainty is within a few years. And no practical impact on average SMB. We will add comment on this in the revised version.

<#8, reviewer2>

Paragraph 2.2.3-2.2.4-2.2.5, previous authors use to merge density profile along the traverse (Eisen et al., 2008) to convert TWT in depth, why do you use only Dome Fuji? Density profile is correlated to snow accumulation, temperature and wind condition, these climatological conditions are quite different along the traverse.

<#8, authors> We understand that the reviewer mentioned section [92] of the Eisen et al. (2008) paper. We think that we explained at 2069/26-2070/6. In addition, according to our observations along the traverse route, the density of snow averaged over top 1 m had little variability along sites between Dome Fuji and EDML. The result was recently submitted to Journal of Glaciology. Based on the observations including this, we approximated density profile using Dome Fuji data mentioning possible errors.

Submitted paper is as follows. Shin Sugiyama, Hiroyuki Enomoto, Shuji Fujita, Kotaro Fukui, Fumio Nakazawa, Per Holmlund, Sylviane Surdyk, Snow density along the route traversed in the Japanese-Swedish Antarctic Expedition 2007/08, J. Glaciol. submitted

<#9, reviewer2> Ii is not clear between 2070/25 and 2071/6, has previous firn core data been used? or data from compilation of Rotschky et al., 2007? Interpolation data are not reliable reference.

<#9, authors> Previous core data has been used. We will mention it in the revised manuscript. But there are limitations of reliability as we mentioned in the text.

<#10, reviewer2> Par. 2.3 Comparison between prevalent wind direction >10 m/s from model (e.g. ERA 40, NCEP-2, JRA-25, ERA-Interim reanalysis) and surface relief observation could be useful in the discussion.

<#10, authors> It seems to us that comparisons with models are topics to be discussed in future. We hope to make present paper discussions for real observational data. In addition, as you can see comments from the anonymous reviewer #1, there are limitations for interpretation of the snow surface reliefs.



TCD
5, C1060–C1066, 2011

<#11, reviewer2> 2075/1-5 and 2080/10-13 Ice divide and dome are singular features on ice sheet glacio- logical condition (SMB, ice flow, surface topography etc.), so we must aspect smoother condition respect to ice sheet escarpment (south traverse).

<#11, authors> The views will be considered in revision.

<#12, reviewer2> 2075/19-20 should be taken in account also the difference due to ice flow and upstream condition of SMB.

<#12, authors> We have discussed limitation of the thinning estimation in 2.2.5. We will try to mention some more detail of the ice flow and upstream condition in the revised manuscript.

<#13, reviewer2> 2077/4-5 surface relief directions are different between spring and summer. Could be the difference observed from A28 and A23 due to summer survey?

<#13, authors> Could you please advise us basis or reference paper about this comment? Discussion on the snow surface features will be revised based on comments from the reviewer 1.

<#14, reviewer2> 2078 Analysis of wind system should be taken in account the difference between ice sheet slope (Watanabe, 1978) and ice divide (Birnbaum et al., 2010; van As et al.,ãĂĂ2008).

<#14, authors> We will consider it in the revision.

<#15, reviewer2> 2079/12-15 It is obvious also if density profile is different, withdrawn

<#16, authors> We will revise as suggested.

<#16, reviewer2> Paragraph 4.3.1 Compare the gradient along ice divide from Dome C and Vostok (urbini et al., 2008), and shadow effect at Dome C and wind impact at Talos Dome.

<#16, authors> At the moment, we are not sure yet that we can naturally mention it in

a contest of this paper. We will examine the suggested possibility when we revise the manuscript.

<#17, reviewer2> 2081/16 shadow effect of DML-B?

<#17, authors> ice divides including DML-B

<#18, reviewer2>

2083/17-21 PR6.9 map does not agree with SMB between M and EDML, this point should be stressed.

<#18, authors> It will be stressed as suggested.

<#19, reviewer2> Par. 4.3.3 and figure related. Several papers point out that wind ablation is determined by surface slope along the wind direction, I would suggest the Authors to provide also this information along the traverse using digital elevation model and sastrugi direction or atmospheric model.

<#19, authors> Could you please advise us about the papers that are appropriate to be cited?

<#20, reviewer2> Par 4.4 Data from US-Norway traverse must be analysed (Anschutz et al., 2009) and compared for temporal variability.

<#20, authors> Data discussed in Anschutz et al. (2009) are not available to resolve changes in SMB in the 20th century.

<#21, reviewer2> Par 5 modify the conclusion taking in account difference between wind erosion and shadow effect.

<#21, authors> Conclusion will be modified based on the context of the main text.

<#22, reviewer2> Par 4.3.3 Fig. 9 Could be the difference between av. of 44 yr and GPR data at point M due to change ice divide position and related shadow effect position? See also Conway and Rasmussen (2009) and Urbini et al. (2008).

<#22, authors> We will examine this possibility.

Interactive comment on The Cryosphere Discuss., 5, 2061, 2011.