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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

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First, the authors greatly appreciate the solid review. We try to revise the paper mostly as suggested by the reviewers. We understand the main concern "the discussion of the wind influence on surface mass balance" and for presentations. Though revision work is from now, we describe our tentative reply here using this 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. As

for English expressions, the manuscript was proof-read by a professional proof reader who speaks English as his native language before initial submission. Before revised manuscript is submitted to TC, it will be reviewed again both by author members and the professional person. To make the paper more concise as suggested, we plan to remove two major items from the manuscript. One is comparison of SMB values with those of Huybrechts et al. (2009) because priority of the comparison is lower than the other scientific items. Another item is radio wave scattering from within ice as suggested by the reviewer 2. It will be developed somewhere in future paper. Figures will be modified accordingly. Figure 3 will be removed to make more concise paper as suggested by reviewer #2. Below, our present plans and/or views are described. Each item is numbered (from #1 to #29) to make interactive discussions smoother.

I hope you to advise us about one of your comments. You gave us a comment on spatial distribution of SMB. You wrote (please see #20 below): "Again this is more complex than you describe it. The leeward-upslope effects of accumulation can be very different depending on the wind speed. For high wind speeds you can get more accumulation on the leeward side of the ridge, at least in the vicinity of the ridge." A question: could you please advise us a reference paper about your comment on the leeward-upslope effects? Do the wind effects apply for relatively wide ridges like the ice sheet?

Please note that <# reviewer> and <# authors> below are comments from the reviewer #1 and authors, respectively.

<#1 reviewer> Title: I would stress the traverse character of the data rather than the deep drilling sites, since that implies coverage of a large area, which is of more importance than that two ice core sites are included.

<#1 authors> Title: We hope to choose a revised title "Spatial and temporal variability of snow accumulation on East Antarctic ice divide between Dome Fuji and EPICA DML".

Background is as follows. Our study covers widely DML. But focus of study is the main

TCD
5, C1050–C1059, 2011

ridge between EDML core site and Dome Fuji. One of main purposes of the inland traverses was to better understand the spatial gradients of glaciological conditions between the EDML core site and Dome Fuji core site. We intend that the title reflects this traverse character. The new title also reflects an advice from the reviewer #2.

Abstract: “Glaciological data”: see below. -> Please see author comment below. Counterclockwise windfield: see below. -> Please see author comment below.

<#2 reviewer> Intro: 2064/12-13: why these time periods? Please explain.

<#2 authors> In terms of climatic changes, the time periods cover late-Holocene, later than 7.9 ka. The detailed numbers of the periods (7.9 ka, 722a and 44a) were determined by dating of isochrones. Practical meaning of these different time scales is that they are useful to understand the environmental variability in Antarctica over these periods within the late-Holocene. We plan to comment on this in the revised manuscript.

<#3 reviewer> 25: surface elevation, slope, and prevailing wind field are not glaciological conditions.

<#3 authors> An expression “glaciological conditions” will be replaced with an expression “environment of the ice sheet”.

<#4 reviewer> 2065/9-11: this is a result and should not be part of the introduction, better move it to the results section.

<#4 authors> The sentence will be removed from the introduction in the revision.

<#5 reviewer> 2066/1-2: which reasons?

<#5 authors> The logistic reason was that the Swedish team needed to temporarily be away from the ridge to access to their fuel depot. The scientific reason was to investigate regions not only along the exact ridge but also regions away from the ridge. Also, we were interested in subglacial conditions at locations away from the ridge. These background conditions will be mentioned in the revised version.

TCD

5, C1050–C1059, 2011

<#6 reviewer> 2066/13: this is not true, they not ALWAYS represent upwind and leeward sides.

<#6 authors> The word “always” will be replaced with “often”.

<#7 reviewer> 17: in contrast to: find better expression.

<#7 authors> We suggest an expression “different from”.

<#8 reviewer> 2067: 25: why two years lag for Pinatubo? That’s not “similarly” to Agung. Please explain.

<#8 authors> A paper below shows that the Pinatubo volcanic signal deposition in inland of Antarctica is in 1992-1994 with a peak center in 1993. The deposition of the Pinatubo sulphate aerosol was delayed due to the long transport to the high southern latitudes and its initial existence at high altitudes in the Antarctic atmosphere.

Reference: Cole-Dai, J., E. Mosley-Thompson, and L. G. Thompson (1997), Quantifying the Pinatubo volcanic signal in south polar snow, Geophys. Res. Lett., 24 (21), 2679-2682. We will cite this reference paper and add short explanation for readers.

<#9 reviewer> 2068/2: divided by the age difference of the peaks.

<#9 authors> Because we examined age difference between the peaks and the ice sheet surface at the time of the field work (2007/2008), we think that present expression “divided by the age of the peaks to derive . . .” is still ok. But to avoid causing any misunderstanding by readers, we will choose an expression “divided by the age difference between the peaks and the surface to derive. . .”.

<#10 reviewer> 27: you did not describe the manner of error estimates for the pit studies .?

<#10 authors> We think that we already described it in 2068/4-8. If there is some misunderstanding or improper use of expression by us, please advise us.

TCD

5, C1050–C1059, 2011

<reviewer> Radar: Since my expertise in radar measurements is restricted I won't go into detail here, but leave that to the open discussion.

<#11 reviewer> Wind field: 2073: orientation of surface relief does not necessarily represent the average wind direction, but might just stem from the last storm.

<#11 authors> We agree that orientation of surface relief does not necessarily represent the average wind direction. But it does not simply stem from the last storm. The latter idea is justified only if each storm event erases, bury or reset snow surface relief that occurred in earlier timings. A clear fact is that our observation is done only in quite limited timing; we still need more observational data including satellite remote sensing data. It seems natural to think that storms that occurred at timings closer to timings of observation are more visible as snow surface features at the time of observation. But orientations remain persistently on the snow surface features if we think that sharp peaks in wind direction for the strong wind events in the wind direction-speed plot, as we observed in AWS data sets. The sharp peaks of wind direction for the strong wind events should have the primary effect. Less strong winds with directions deviated from the peak prevailing orientation can also re-distribute snow. But wind speed is relatively small: ability for engraving snow surface is lower. We will comment on these in the revised version.

<#12 reviewer> 2074: why are you so interested in the relationship between wind direction and wind speed? This does not yield any information about the corresponding precipitation/accumulation/SMB.

<#12 authors> We agree that precipitation events occur in various conditions. Temporal changes and spatial distributions are complex phenomena. There are always time-series evolutions. But at the same time, we are interested in a fact that precipitation events very often occur with events of increased wind speed and increase of temperature. This comment is based on some papers listed below. In particular, Figure 6 in Birnbaum et al. (2010) shows that almost every precipitation event is accompanied

TCD

5, C1050–C1059, 2011

by strong events though they may sometimes have lead or lag. Fujita and Abe (2006) showed time-series of precipitation events in their Figure 2. Majority of the precipitation events were with strong wind events if we compare their data with meteorological data of 2003. These data suggest that many of precipitation events and strong wind events are highly associated. In some cases, strong wind can directly affect distribution of the deposited snow. In another cases, strong wind re-distribute snow that has deposited just before or long time before. And there would be cases that deposition of diamond dust is re-distributed by strong winds. Considering the agreement between orientations of snow surface features and peak orientation of the strong wind in AWS data, it seems natural to study link between SMB and the strong wind events. The papers listed below will be mentioned in the revised manuscript.

References

Birnbaum, G., J. Freitag, R. Brauner, G. König-Langlo, E. Schulz, S. Kipfstuhl, H. Oerter, C.H. Reijmer, E. Schlosser, S.H. Faria, H. Ries, B. Loose, A. Herber, M.G. Duda, J.G. Powers, K.W. Manning, and M.R. van den Broeke, Strong-wind events and their influence on the formation of snow dunes: observations from Kohnen station, Dronning Maud Land, Antarctica J. Glaciol., 56 (199), 891-902, 2010.)

Fujita, K., and O. Abe, Stable isotopes in daily precipitation at Dome Fuji, East Antarctica, Geophys. Res. Lett., 33 (L18503), 2006.

Reijmer, C., and M.R. Van den Broeke, Temporal and spatial variability of the surface mass balance in Dronning Maud Land, Antarctica, as derived from automatic weather stations, J. Glaciol., 49 (167), 512-520, 2003.

Schlosser, E., M.G. Duda, J.G. Powers, and K.W. Manning, Precipitation regime of Dronning Maud Land, Antarctica, derived from Antarctic Mesoscale Prediction System (AMPS) archive data, J. Geophys. Res., 113 (D24108), 2008.

Schlosser, E., K.W. Manning, J.G. Powers , M.G. Duda, G. Birnbaum, and K. Fujita,

Characteristics of high precipitation events in Dronning Maud Land, Antarctica, J. Geophys. Res, 115 (D14107), 2010.

<#13 reviewer> 20: somewhat lower: give numbers, please.

<#13 authors> In the revised manuscript, the description will be something like “The point at FB0603 shows a lower value than surrounding data points from the firn core studies by ∼10 kg m-2 a-1 or ∼20 %.

<#14 reviewer> 2076: fig 7: why do you discuss the relationship wind direction-speed? This is not clear. There is no simple relationship between this and SMB. You might have a strong wind-event that erodes the surface to produce the observed sastrugi orientation with net ablation, as well as events with moderate winds that bring relatively high accumulation, but cannot be seen in your investigation of surface structure.

<#14 authors> First, please see our comment #12. To see stairs-like increase of accumulations reported in reference papers (Figure 4 in Reijmer, C. and Van den Broeke, M. R. (2003, J. Glaciol.) and Figure 4 in Kameda et al. (2008)), simple ablation events due to strong wind events seems rare.

<#15 reviewer> Discussion: I think it would be clearer to show the results from the various SMB data first and then try to explain them using the wind information. The whole paragraph is more confusing than helpful in its present form. Please try to rewrite it a bit more clearly.

<#15 authors> We will try it as suggested.

<#16 reviewer> 2076/23: the wind field has no impact of the flow of maritime air masses over DML, it is a result of the local conditions and general atmospheric flow.

<#16 authors> We agree that the expression was incorrect. In the re-structuring of the paper, this sentence will be removed.

<#17 reviewer> 2077/5: this is not necessarily the case, it just shows the most recent

TCD

5, C1050–C1059, 2011

wind history.

<#17 authors> Please see our comment #11. We agree that it shows the recent wind history. But it also implies that the recent wind history was more variable than the other legs of the traverse.

<#18 reviewer> Last paragraph: these results are not necessarily congruent. Define winter and spring! Investigated time periods are often too short for a general statement like yours. Cyclonic activity is usually largest in spring and fall. However, we don't know enough about their influence on the Antarctic plateau yet.

<#18 authors > In the revised paper, discussions will reflect the suggestions.

<#19 reviewer> 2078 first paragraph: this whole paragraph is not clear. Turned counterclockwise is a strange expression and why does it indicate cyclonic activity? The wind direction connected to a cyclone depends on the location of the cyclone center relative to the discussed site, and the direction of the katabatic winds depends on topography, so it is not possible to make a general statement like yours. Cyclonic activity can lead to wind directions similar to katabatic winds at EPICA DML as well as to an upslope wind. Cyclonic activity is not necessarily the same as strong large-scale forcing. You should differentiate between a cyclone directly affecting the investigation site and a cyclone causing a large-scale flow of moist air towards the interior of DML. The latter can have both NW or NE-direction. Second paragraph: see above

<#19 authors> In the revised version, we will correct these criticized points. In particular, relative changes between prevailing wind direction for katabatic wind and strong wind events will be discussed from one site to another. Reference papers listed in #12 are very informative. Discussions in the revised paper will reflect these papers.

<#20 reviewer> Spatial distribution of SMB: Again this is more complex than you describe it. The leeward-upslope effects of accumulation can be very different depending on the wind speed. For high wind speeds you can get more accumulation on the

TCD
5, C1050–C1059, 2011

leeward side of the ridge, at least in the vicinity of the ridge. Again the question of deposition vs. erosion remains unsolved.

<#20 authors> In the revised version, we will try to reflect this view. A question: could you please advise us a reference paper about your comment on the leeward-upslope effects? Do the wind effects apply for relatively wide ridges like the ice sheet?

<#21 reviewer> Passive microwave data: Should have mean mentioned earlier in the presentation of available data and methods

<#21 authors > It will be revised as suggested.

<#22 reviewer> Increase in accumulation rate during the 20th century Please include references about precipitation studies of Antarctica in your discussion

<#22 authors> It will be revised as suggested. The reference papers listed above and several more papers are candidate for citation.

<#23 reviewer> 2087/88: masks any increase in accumulation rate: the relative change should nevertheless be visible. The difference leeward - windward side is also only relative.

<#23 authors> A fact is that “no increase of SMB” is reported in the leeward sites in many cases. If any increase or decrease of the accumulation rate is homogeneous both in the leeward - windward, the difference leeward - windward side is only relative. But what does the fact mean then? We need to check whether or not assumption for the homogeneity or linearity is really realistic. We will comment on this in the revised manuscript.

<#24 reviewer> Conclusions: 1. . . ..counterclockwise to the katabatic wind. . .., see above, this does not make sense.

<#24 authors> Based on the revision of the main text, these statements will be revised.

<#25 reviewer> 2. See above, lee-effects are more complex than you describe them

TCD

5, C1050–C1059, 2011

<#25 authors> Please see #20.

<#26 reviewer> 3. Counterclockwise prevailing wind field. See above, this does not mean anything. 4. This is not wrong, but I would not say the bedrock topography essentially determines the local-scale variations. It is the surface topography that determines the local-scale variations.

<#26 authors> Based on the revision of the main text, these statements will be revised. As for the statement about the bedrock, we will reconsider.

<#27 reviewer> 6. see above

<#27 authors> Please see #23.

<#28 reviewer> The paper is relatively long. If you want to shorten it, I would suggest skipping the comparison with Huybrecht's modeling. (no must)

<#28 authors> We will revise this point as suggested.

<#29 reviewer> Last remark: some scientists criticize the mixed use of accumulation, net accumulation, and SMB. Everybody knows what is meant, but you might consider to be more precise here.

<#29 authors> Terminology will be checked based on a document below. Cogley, J.G., R. Hock, L.A. Rasmussen, A.A. Arendt, A. Bauder, R.J. Braithwaite, P. Jansson, G. Kaser, M. Möller, L. Nicholson and M. Zemp, 2011, Glossary of Glacier Mass Balance and Related Terms, IHP-VII Technical Documents in Hydrology No. 86, IACS Contribution No. 2, UNESCO-IHP, Paris.

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