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Interactive comment on "Surface energy budget on Larsen and Wilkins ice shelves in the Antarctic Peninsula: results based on reanalyses in 1989–2010" *by* I. Välisuo et al.

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We would like to thank the anonymous referee for spending time to engross, evaluate and comment our manuscript. We were happy to note the constructiveness of the comments and the clear statement of the points that would benefit of specification, reassessment or corrections. The main comments of the referee concerned the nature of the reanalysed products: their suitability for other than synoptic-scale studies and the fact that the flux products result actually from parametrised short-term forecasts. The referee addresses also the melt calculation, criticised the suitability of the 12h forecast-products in melt calculations and seized on the spin-up error. The referee

suggested more validation and more detailed description of the reanalysis models. We got down to these issues by describing more in detail the analyse and forecast products in use, extending the validation to cover the radiative fluxes and recalculating the melt using 6h mean values in which the spin-up error had been removed. In this response we reply to the comments of the referee paragraph by paragraph. Our answers start with 'Authors' answer:' throughout the document. We state which of the suggested improvements we have done to the manuscript. In case we have not performed the changes suggested by the referee, we provide a justified explanation for that. All in all, the referee's comments generated interesting reflection on our study, brought up new perspective to the topic and allowed us to further improve the manuscript.

Anonymous Referee #1 Received and published: 3 April 2013

This paper uses re-analyzes and associated short-term forecasts to evaluate the variability, trends, surface energy budget and surface melting over 2 ice shelves in the Antarctic peninsula. The paper is well constructed and written, and of interest to the community.

My main concern with this paper is that major results and conclusions heavily rely on (re)analyzes products, which are good for synoptic features related to the general circulation, but not so good or at least need to be carefully verified for variables that result from parametrizations. This includes the surface radiation and turbulent fluxes, which are not really analyzes. Unlike analyzes, no observation directly enters the production of such data. They are actually model / forecast products initialized with analysis. In addition, surface parametrizations are notoriously prone to errors in the polar regions (the last IPCC repors explicitly lists boundary layer parametrizations as one major limitation for climate change predictions in the polar regions). The fact that differences between the various "analyzes" of surface fluxes are quantitatively so large (figure 2) supports that the realism of such product must not be taken for granted.

Authors' answer: We have now made it clearer that turbulent and radiative surface fluxes in reanalyses are based on short-term forecasts without any data assimilation (manuscript pp.7 lines 3-5). We have also added in the manuscript more validation of the fluxes. We note that the major results and conclusions on the manuscript are not only based on surface fluxes, but also on analyses of synoptic-scale circulation, for which reanalyses are considered to perform better.

That the authors use forecasts rather than analyzes (of flux) is only mentioned in section 3.4, to report that 3-hour forecasts are available from ERA-I but not used because deemed unrealistic. However, longer-term forecasts are continuations of the 3-hour forecasts: how come they are considered wrong for the 3-hour step but correct for the further steps? If the authors think there is a problem with the 3-hour step, shouldn't this step be substracted from the longer term forecasts before use? Also, ERA-I, and presumably the other analysis products, provide 6-hour forecasts: why do the authors use the 12-hour, clearly much less appropriate to study strongly diurnally variable phenomena (melting, section 3.4).

Authors' answer: As the short and the long forecasts have disadvantages in the melt studies, the short ones due to the likely spin-up error and the long ones due to the smoothening the diurnal variations, we performed new melt calculations using six hour mean values that we calculated from the ERA-Interim products. As the starting time steps for the ERA-Interim forecasts are 00 and 12 UTC, we were able to calculate the six hour values for 24 hour periods by using the 6, 12 and 18 hour forecasts. We calculated the six hour values as follows:

-to obtain hours 06-12, we calculated forecast(00+12) – forecast(00+06)

-to obtain hours 12-18, we calculated forecast(00+18) – forecast(00+12)

-to obtain hours 18-00, we calculated forecast(12+12) – forecast(12+06)

-to obtain hours 00-06, we calculated forecast(12+18) – forecast(12+12)

In addition to obtaining the 6 hour cumulative values, which were further transformed

to 6 hour mean values, we removed also the possible spin-up errors by subtracting the first six or twelve hours from the twelve or eighteen hour forecasts. We recalculated the melt using these 6 hour values for surface fluxes. → manuscript pp. 14

Obviously, the reanalyzes are not evaluated with respect to their flux products since there is no available observation in the area of interest. However, observations of both turbulent and radiation fluxes are available at other Antarctic sites in relatively similar conditions. One of the authors was associated with detailed meteorological observations on the Brunt ice shelf, which could be used for a minimal comparative evaluation of the 3 analysis products with respect to radiation and turbulent fluxes.

Authors' answer: To improve the understanding on the reliability of the reanalysed flux products, we validated the surface solar and thermal radiation against flux measurement performed by the British Antarctic Survey on Larsen C ice shelf. The validation period covered a bit more than a year from 22nd of January 2009 to 27th of April 2010. The results (Table 3) suggested that ERA-Interim and NCEP-CFSR performed well especially for the net solar radiation, which is the key factor in the surface energy balance. This bias was only 0.2 Wm-^{2} in ERAI. → manuscript pp. 8 lines 16 onwards

Authors' answer: We did not utilize the observations at the Brunt Ice Shelf for validation of radiative and turbulent surface fluxes because we pondered that, from the point of view of reanalyses, Brunt Ice Shelf is very different from Larsen C: Brunt ice shelf is very narrow (some 50 km) at the location where the meteorological observations were done, and thus entirely missing or poorly represented by the three reanalyses applied in our study. There is no sense in validation if the surface type in a reanalysis is ocean or sloping continent instead of an ice shelf. Instead of using observations at Brunt Ice Shelf, we have added a reference to a recent study by Tastula et al. (2013), who validated several reanalyses over the western Weddell Sea. They showed that the ERA-Interim sensible heat flux had a positive bias of 6 W/m2, which was partly balanced by a negative bias of -3 W/m2 in the latent heat flux. → manuscript pp. 8 line 25–27

The authors mention that the 3 analysis products differ a lot with respect to solar radiation but cannot conclude on which is more realistic. This is a crucial point as solar is a major component of the surface energy balance. Cloudiness, which directly affect solar and certainly accounts for a lot of the differences, is available from satellites.

Authors' answer: According to the additional validation of the surface radiative fluxes, we note that the performance of the reanalysis flux products is in line with the overall performance of the reanalyses. ERA-Interim and NCEP-CFSR obtained the best agreement with the observations. Figure 2 shows that the differences between ERAI and CFSR are rather small and JRA notably differs from them. We conclude that in our study region both ERAI and CFSR produce the surface solar radiation realistically. Hence, we made the selection to use ERAI on the basis of its better performance for inter-annual variations, which is now more clearly stated in Section 2.3.

Also, rather than using the turbulent fluxes straight from the analyzes products, one could consider running a validated atmosphere – surface model forced by the really analyzed variables, wind, temperature and moisture.

Authors' answer: In our opinion, running a validated atmosphere-surface model would not add value to this study. To produce the surface fluxes ECMWF also uses an atmosphere-surface model, which is validated in numerous studies (see Dee et al., 2011).

Incidentally, the authors report the horizontal resolution of the various analyzes and the number of vertical levels, not the height of the levels near the surface. The 2-m and 10-m (standard meteorological) levels are actually extrapolations, not real model levels. It would be good to know about the height of the real model levels.

Authors' answer: The approximate height (slightly varies depending on air density) of the lowest atmospheric model level is 10 m in ERAI, 20m in CFSR, and 80 m in JRA. Hence, the diagnostically calculated 10m wind is in practice the same as the lowest model level wind in ERAI. The height of the lowest model levels have been added

to the manuscript in section 2.1. Instead of extrapolation, the 10 m wind and 2 air temperature and humidity in reanalyses are based on the application of the Monin-Obukhov similarity theory using as boundary conditions the values at the surface and the lowest atmospheric model level.

As for validating the mean variables, there is more than one AWS operating on the Larsen ice shelf (see http://amrc.ssec.wisc.edu/aws/ for a compilation of existing AWS in Antarctica). The authors mention that measurement errors are not unlikely, comparing with several observations could increase confidence. Incidentally, because of such a relatively high density of surface observations, it would be nice to know how much of these surface data actually go into the production of one or the other re-analysis (through the GTS or other). The authors report that the AWS they used failed in the summer 1992-93, which prevented confirming peaking melt, but data from other AWS could possibly be available.

Authors' answer: We were unable to find any AWS on Larsen C that would have been running already in 1992-1993. Thus confirming the melt peak in 1992-93 remains undone. The surface data collected in the vicinity on the ice shelves goes partly into the production of the reanalyses. All the three reanalyses used the observations of the surface pressure, but only ERA-I assimilates the 2m temperature measurements (Uppala et al. 2005: Table B.1; Onogi et al. 2007: Fig.1; Saha et al). → manuscript pp. 7 lines 6-8

The authors analyze seasonal variability and trends. They also focus on melting, but melting is a threshold product that is often triggered by extremes. The authors should thus also evaluate the summer extremes and their possible interannual variability and trends. A concern about the evaluation of melt from ERA-I surface temperature and fluxes results from the warm bias of this analysis, which should be discussed. The time step for the flux forecasts is also inappropriate, see above. Possible melting trends from energy balance calculations can and should be compared and validated using satellite products. In particular, Barraud et al., Trends in Antarctic Peninsula surface melting conditions from observations and regional climate modeling, (JGR, 118, 1–16, doi:10.1029/2012JF002559, 2013) specifically addresses the issue of melting trends in the Antarctic peninsula. Satellite detection of melt at earlier time can be found in Torinesi et al. 2003 (cited in Barraud et al.).

Authors' answer: To improve the processing of the surface melt, we recalculated the surface melt using six hour values and removing the possible spin-up error (see above) and compared the melt against the results presented by Barrand et al. (2013). Barrand and others present the melt trends on Antarctic Peninsula ice shelves based on satellite observations. They used QuikSCAT (QSCAT) daily enhanced resolution, slice based SIR images with nominal pixel size of 2.225km and estimated effective resolution of ∼5 km. Their study covered the trends in melt onset date, melt season duration as well as the melt strength and extent. Of these, the melt season duration and the amount of surface melt can be directly compared to our study. According to Barrand et al. (2013) the melt season duration (MD) showed large inter-annual variability between 1999 and 2009. They state that negative MD anomalies (shorter melt seasons) occurred on Larsen C and Wilkins ice shelves in 2004 (i.e. summer 2003-2004). Positive anomalies occurred on WIS in 2000, in the vicinity of Larsen B and Larsen C ice shelves during 2002, throughout the southern Larsen C and Larsen D in 2003, and throughout Larsen C in 2006 and 2008. In our melt calculation (during QSCAT era in 1999-2009) based on ERAI, we observed negative anomalies in number of melt days (analogous with melt season duration) on LCIS in summers 2000-2001 and 2003-2004. On WIS short melt seasons were observed in 1999-2000 and 2003-2004. The negative anomalies in summers 2000-2001 (WIS) and 2003-2004 (LCIS and WIS) occurred both in our calculations and in Barrand et al. (2013). Positive anomalies in number of melt days occurred on LCIS in 2001-2002, 2004-2005, 2005-2006 and 2006-2007, and on WIS in 2004-2005 and 2006-2007. Of the positive anomalies summers 1999-2000 (WIS), 2002 (LCIS), and 2006 (LCIS) were identified also by Barrand et al. (2013). A large number of consistent results were identified between our melt studies and the satellite observations presented by Barrand et al. Exceptions were in 2006 and 2008 when

Barrand observed a long MD on southern Larsen C and Larsen D and on all Larsen C, respectively. Our different results in 2006 might be caused by the lower resolution of ERAI and the fact that Larsen D was out of our study area. Also we did not study the spatial difference in melt within the ice shelves. → manuscript pp 15-16

The authors discuss differences in the number of melt days with work by van den Broeke (2005). They report that their melt is calculated using surface temperature while Van den Broeke used 3-m boom temperature, and argue that surface temperature is colder due to surface based inversion, thus their lower number of melt days. However, melting occurs when the surface energy balance is positive. Inversions build when the energy balance is negative, so this cannot be the reason.

Authors' answer: Melting often occurs on the Larsen ice shelf in the presence of a surface inversion. The presence of a surface inversion indicates a downwards sensible heat flux, but, over LIS in summer this can occur during a positive surface energy balance if there is advection of warm air to the area. Warm advection is a common feature over Larsen C ice shelf due to the föhn effect created by the AP mountains and the westerly winds. This phenomenon has been studies for example by King et al. (2008).

The latent heat issue is more realistic, however it remains to demonstrate that the quantities of energy involved (very small compared to other components) can explain a difference.

Authors' answer: We have demonstrated the effect of latent heat flux on melting by doing the melt calculations both with and without the latent heat flux. In our calculations the latent heat flux reduces the summertime melt by over 20% on LCIS and WIS. (See page 14 line 24-25)

At some point, the authors use multiple regression to evaluate the contributions of atmospheric pressure, components of wind and wind strength, and cloudiness to the net surface heat flux. One assumes (but this needs to be clearly stated) that they use multi linear regression. They should mention how they selected to use a given number and choice of variables for the different seasons and regions. Actually, this part is fairly inclusive, and the conclusion could have been reached with mere linear correlations.

Authors' answer: The regression analysis was performed using a stepwise multi-linear regression model. For each season and region the variable to be explained is the surface net energy flux and the possible explanatory variables are mean sea level pressure, 10-m wind speed, 10-m wind components and the cloud fraction. The model calculates which combination of explanatory variables yields the best degree of explanation (measured as R^2 and RMSE) for the surface net energy flux. As possible explanatory variables we use the above-mentioned ones, because we want to understand how the net flux is controlled by synoptic-scale weather. It would be rather trivial to explain the net flux on the basis of its radiative and turbulent components. We do not think that the conclusions would have been reached simply by bilinear correlation analyses.

Interactive comment on The Cryosphere Discuss., 7, 1269, 2013.

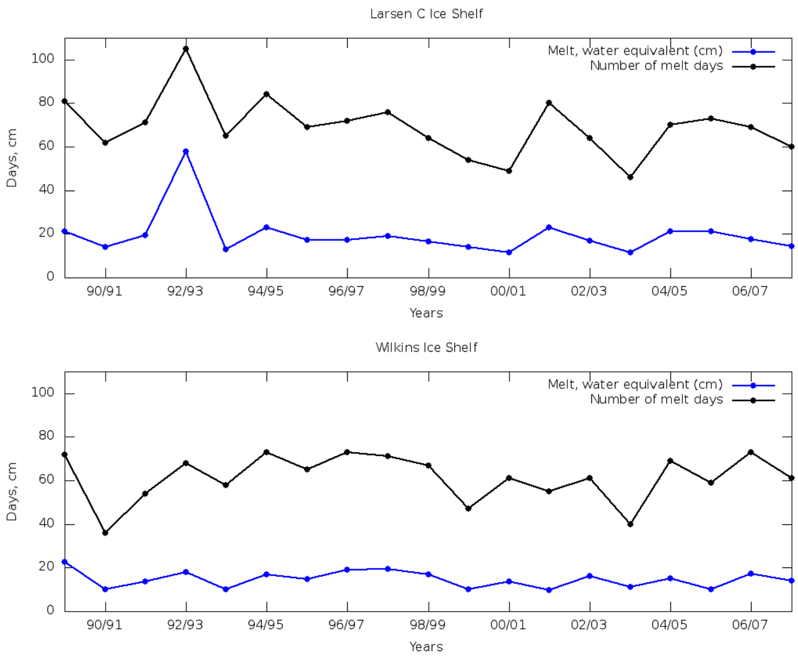

Fig. 1. Figure 12: Mean summertime melting and number of melt days on Larsen C and Wilkins ice shelves calculated on the basis of ERA-Interim reanalysis