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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.

Anonymous Referee #1

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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.

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).

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. 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. 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. 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.

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.

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.).

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. 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.

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.

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