

## ***Interactive comment on “Diagnosing the decline in climatic mass balance of glaciers in Svalbard over 1957–2014” by Torbjørn Ims Østby et al.***

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This paper presents estimation of SMB at 1km of resolution over Svalbard downscaling directly the ERA reanalysis to 1km as in Hanna et al. (2008) over GrIS. This papers is generally well written and deserves to be published in TC although the main conclusions of this paper (lines 7-16, pg 32) just confirm previous results (e.g. Lang et al, 2015) and do not really bring new stuff. However, the methodology used is different than previous studies and gives results at 1km which will be very useful afterwards to force ice sheet models. Nevertheless, before publication, some issues need to be resolved if it is not a too big job for the authors.

Major:

- line 2, pg 32 and abstract: the authors suggests a change of SMB around 1980 which

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corresponds to the switch from ERA40 to ERAint. Discontinuities in SST/SIC are also mentioned around 1980 in the manuscript. How does this discontinuity impact on the results presented here? This change should be more discussed because it could be just an artefact of the use of inhomogeneous dataset. How do the results compare over 1980-2001 when both reanalysis are available? Estimation using ERA-40 over 1979-2001 should be added in this manuscript to check the homogeneity (assumed in the manuscript without proof: pg 31, line 3) of this SMB reconstruction over 1957-2014.

Minor:

- line 26, pg 4: the raw resolution of ERA-40 is 1.125deg and not 0.75deg (as ERA-Interim) suggesting that a first interpolation is made here. This could explain some differences between ERA40 and ERA-Interim over 1979-2001 and the problem of SST/SIC before and after 1979.

- line 11, pg 7: most of the observations used here were assimilated into the ERA-reanalysis which explains the good agreement (as also tells in the manuscript). It will be useful to add a comparison using the raw ERA outputs to check that the downscaling method does not bring additional uncertainties.

- line 12, pg 9: 5 to 25 mm / yr or month ? The units need to be more precise.

- line 10, pg 13: the 6 hourly outputs are linearly interpolated to 3min. A bilinear interpolation (allowing to represent the max/min of temperature) will be more adequate. Tmin, Tmax from ERA can be used to better represent the daily cycle with 6 hourly outputs. When the temperature is near 0deg, this could impact on the melt.

- Fig 6, pg 16 and line 6, pg 30: the SMB is mostly >0 in the North while is <0 in the South (y<8650km) everywhere. This is quite strange. Is it realistic? Do you have an explanation for this? For me, it seems rather to be an artefact from the use of the ERA (not representing these parts in its DEM) as forcing. This difference between south and north is less pronounced in MAR (Lang et al., 2015).

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- Table 7, pg 23: the mean biases listed in Table 3 should be pointed on a map (e.g. on Fig 6).

- line 11, pg 25; line 12, pg 26, ...: the future projections presented in this paper are mostly very hazardous and speculative and are, for me, out of the scope of this paper which should focus only on present climate (as suggested in the title). It will be more robust to apply this methodology to GCM forcings and not ERA + anomalies for future projections.

- Sections 6.2 and 6.3, pg 27 should be put after Section 5.1.

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