General comments

The paper from Galos et al. presents a valuable contribution in the field of glacier mass balance monitoring, implementing reanalysis procedures optimized in the framework of the World Glacier Monitoring Service (Zemp et al., 2013). In particular, the field measurement efforts, the completeness of the reanalysis and the detailed quantification of uncertainties are appreciable.

Due to logistical issues and/or to the peculiar characteristics of monitored glaciers, it is often impossible to setup point monitoring networks that are evenly distributed and cover the entire surface of the glaciers. Therefore, there is usually the need for interpolating and extrapolating point measurements over unmeasured areas, which often have high lateral gradients of mass balance. In such circumstances, the analyst's knowledge of the monitored glacier becomes decisive, and greatly benefit from repeat observations of snow cover patterns during the ablation season, over several years. The Authors of this paper make extensive use of this information for mapping the mass balance distribution over Langenferner, which in my opinion is another added value of this work.

However, I have one point to highlight, which is the calculation of so-called pseudo-observations in the upper part of the glacier for years without direct measurements in that area. Galos et al. use a physically based energy and mass balance model to do that, starting from meteorological data recorded by weather stations located in the proximity of the glacier. There are some weak points on the way the model has been applied and the transfer function for meteorological variables have been calculated (see detailed comments); apart from this, my principal criticism concerns the use of mass balance models for calculating artificial point measurements, which are required in usampled areas for completing glacier-wide mass balance computations.

Because the high value of mass balance records lies, among others, in their useability as sensitive climatic indicators and for improved understanding of glacier-climate interactions, using pseudo-measurements modelled from meteorological data is a sort of circular procedure.

Previous works by e.g. Haefeli (1962), Jansson (1999), Carturan et al., (2009), and Kuhn et al., (2009) proposed extrapolation procedures that are independent from meteorological observations. In my paper on the mass balance series of the neighbouring La Mare Glacier (Carturan, 2016), I tried implementing these procedures for mass balance extrapolation, combining point measurements and snow cover pattern observations.

Therefore, I recommend at least mentioning these works in the Introduction section, clearly stating why a mass balance model has been preferred for mass balance calculations in unsampled areas, and discussing the limitations of this approach. In particular, I suggest discussing the generalizability of the method (e.g. for glaciers with few or absent meteorological data, indispensable for calculating transfer functions of meteorological variables, glacier cooling effect, etc), and the fields of application of mass balance series that are partly derived from meteorological data (ok e.g. for estimating contribution to sea level rise and management of regional water supplies, but maybe less reliable for early-detection strategies of climate change and process understanding).

References

Carturan L, Cazorzi F and Dalla Fontana G (2009) Enhanced estimation of glacier mass balance in nsampled areas by means of topographic data. Ann. Glaciol., 50, 37–46 (doi: 10.3189/172756409787769519)

Carturan, L. (2016) 'Replacing monitored glaciers undergoing extinction: a new measurement series on La Mare Glacier (Ortles-Cevedale, Italy)', Journal of Glaciology, 62(236), pp. 1093–1103. doi: 10.1017/jog.2016.107.

Haefeli R (1962) The ablation gradient and the retreat of a glacier tongue. In Symposium of Obergurgl, IASH Publication, vol. 58, 49–59

Jansson P (1999) Effect of uncertainties in measured variables on the calculated mass balance of Storglaciären. Geogr. Ann. Ser. A-phys. Geogr., 81(4), 633–642

Kuhn M, Abermann J, Bacher M and Olefs M (2009) The transfer of mass-balance profiles to unmeasured glaciers. Ann. Glaciol., 50 (50), 185–190 (doi: 10.3189/172756409787769618)

Detailed Comments

Page 2, Line 15-25: In the Introduction, I suggest mentioning published works dealing with point mass balance extrapolation methods that are independent from meteorological data (e.g. mass balance vs. altitude). See references in the general comments.

Page 4, line 15: extensive *net* accumulation measurement. Also in the following I suggest specifying net accumulation where required.

Page 4, line 26: I suggest writing here which time system is used (fixed date)

Page 5, line 26-29: did the Authors consider the possibility of using the bedrock topography, as obtained from geophysical measurements, instead of the surface topography, for identifying the divides?

Page 6, line 21: how did the Authors account for this problem? Here and in other parts of the paper the reader is left (temporarily) without explanations

Page 7, line 3: stratigraphic correction only for snow cover (is it fresh snow after measurement?) and not for ablation? How was it done?

Page 7, line 12: raw precipitation or (gauge-undercatch) bias-corrected precipitation? Possible impact on calculations?

Page 7, line 29-31: it is a fact that the spatial variability of the mass balance is large, and largely dependent on the spatial variability of micro-meteorological variables and local topography. Does the model fully account for these factors? Maybe the Authors should briefly recall here the main processes accounted for in the model. Moreover, physically based energy and mass balance models strongly rely on accurate spatially distributed fields (or in situ measurements) of several meteorological variables for their application. Because this is not the case of Langenferner, as stated at line 27 and in Section 3.2.1, there is the need for spatially flexible model tuning for the choice of the optimal parameters at the individual points, based on summer snowline observations. In my opinion, such use of a physically-based energy and mass balance model, instead of simple statistical relationships applied to measured point mass balances and observed snow lines (i.e. independent from meteorological observations, e.g. Carturan, 2016) is questionable and should be better motivated by the Authors.

Page 7, line 31 to Page 8, Line 2: I think that actual measurements, instead of pseudo-observations derived from climatic/meteorological data, should be preferably used in investigations concerning glacier-climate interactions (there is a risk of circular reasoning).

Page 8, line 7-8: fixed lapse rates and fixed offsets of temperature imply fixed glacier cooling/damping effects and simple air temperature distribution, whereas in the recent literature there is evidence of a significant variability of these processes within/among glaciers (e.g. Greuell and Böhm, 1998; Shea and Moore, 2010; Petersen and Pellicciotti, 2011; Petersen et al., 2013; Carturan et al., 2015). The same is valid for the other meteorological variables, which have been calculated using transfer functions derived at one point over the glacier. I suggest at least discussing this issue.

Greuell, W. and Böhm, R.: 2m temperatures along melting midlatitude glaciers, and implications for the sensitivity of the mass balance to variations in temperature, J. Glaciol., 44, 9–20, 1998.

Petersen, L. and Pellicciotti, F.: Spatial and temporal variability of air temperature on a melting glacier: atmospheric controls, extrapolation methods and their effect on melt modeling, Juncal Norte Glacier, Chile, J. Geophys. Res., 116, D23109, doi:10.1029/2011JD015842, 2011.

Petersen, L., Pellicciotti, F., Juszak, I., Carenzo, M., and Brock, B.: Suitability of a constant air temperature lapse rate over an Alpine glacier: testing the Greuell and Böhm model as an alternative, Ann. Glaciol., 54, 120–130, 2013.

Shea, J. M. and Moore, R. D.: Prediction of spatially distributed regional-scale fields of air temperature and vapor pressure over mountain glaciers, J. Geophys. Res., 115, D23107, doi:10.1029/2010JD014351, 2010.

Page 8, line 24: I think the main reason for this improvement lies on a better calculation of the liquid vs. solid precipitation fractions. Do the Authors agree?

Page 8, line 28: Γ_0 therefore mainly accounts for gauge undercatch errors, precipitation vertical and horizontal gradients, and snow redistribution (and possibly ablation?). I suggest commenting on this.

Page 9, line 16-18: this tuning procedure is based on the assumption that the parameters controlling ablation processes, tuned at stake 22, are also valid elsewhere and that there are no significant biases in the calculation of spatial fields of meteorological variables, because otherwise there could be compensating effects from errors in modelling accumulation and ablation processes. My suggestion is to briefly discuss this aspect. In addition, I suggest better clarifying and repeating here for which stakes and in which years the model calculations have been done.

Page 15, line 31: is this assessment bases on the procedure proposed by Rolstad et al., (2009)? If so, I suggest adding this reference here.

Page 16, line 5. Compared to the period from 2005 to 2012, in 2013 the AAR was significantly larger in most glaciers of the Ortles-Cevedale. The effects on the mean glacier density were likely small or negligible, but maybe the Authors could shortly comment on that.

Page 17, line 26: maybe here the Authors could replace objective with automatic extrapolations. Here and elsewhere, specify net (accumulation) where required.

Page 19, line 23-24: in my opinion this is a key point and an essential prerequisite for reliable mass balance mapping and calculations. The Authors should mention here previous works highlighting the importance of observations regarding the snow cover pattern (e.g. Kaser et al., 2003; Østrem and Brugman, 1991)

Kaser, Georg, Andrew Fountain, and Peter Jansson. A manual for monitoring the mass balance of mountain glaciers. Paris: Unesco, 2003.

Østrem, G., and M. Brugman. "Mass balance measurement techniques." A manual for field and office work, Environment Canada, Saskatoon (1991).

Page 21, line 17-18: this good agreement is largely dependent on snow line observations, which have been used as constrains for model calibration at individual points, and on the representative location of stake 22 (where the model has been optimized) compared to the location of the modelled stakes. Given also the simplified transfer functions used for the meteorological variables, I wonder how much generalizable the proposed method is, what is its added value in comparison to statistical procedures based only on observations (point measurements and snow line mapping), and how to consider mass balance pseudo-observations derived from climatic data used alongside actual observations. In my opinion, the implementation of such combined (measured + modelled from meteorological data) mass balance datasets poses some limitations to their use for process-understanding or as key variables in monitoring strategies of the Earth climate (Zemp and others, 2005; WGMS, 2008). This is because the climatic indicator (glacier mass balance) becomes dependent on the same climatic variables it should be a proxy of.

Zemp M, Frauenfelder R, Haeberli W and Hoelzle M (2005) Worldwide glacier mass balance measurements: general trends and first results of the extraordinary year 2003 in Central Europe. In Data of Glaciological Studies [Materialy glyatsiologicheskih issledovanii], Moscow, Russia, vol. 99, 3–12.

WGMS (2008) In Zemp M and 5 others eds. Global glacier changes: facts and figures. UNEP, World Glacier Monitoring Service, Zürich, Switzerland

Figure 2: it is unclear if the snow probings refer to winter mass balance measurements or to annual net accumulation measurements. In the second case, the location of winter balance measurements should be added.

Figure 7: I suggest adding the 1:1 line