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Interactive comment on "A statistical approach to refining snow water equivalent climatologies in Alpine terrain" *by* S. Jörg-Hess et al.

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General comments:

The paper describes a statistical method to model snow water equivalent from a dense network of snow depth measurements using station networks in Switzerland from a climatology perspective. This topic is highly relevant for a series of applications and research fields in climatology, hydrology and snow research (which is also shown with some examples in the paper). Methodical the work is based on very useful previous work on snow density characteristics in Switzerland. The benefit of the new work now is from the extensive analysis of spatialisation methods in order to explore the potential of information from existing measurements of snow height HS and using the snow-density model to compute fields of snow water equivalent (SWE) from HS. Thus the

paper clearly addresses relevant scientific questions within the scope of TC based on novel concepts, ideas and tools. The methodical approaches used are well described and conclusions are well stated and relevant and based on novel results. The paper is set into the context of existing work done in the field of snow hydrology/climatology showing the added value of the authors work. Though the title of the paper is clear with respect to the aim of the paper I would suggest considering improvement (e.g. "Exploring the potential for SWE climatology in Alpine terrain using empirical approaches"). The structuring of the paper is well done and the text is generally easy to read and understand.

Detailed comments:

Under "2.2. Snow observations" a short statement on data quality of snow data has to be given.

Figure 2 illustrates the quantile mapping method. However, the method is well explained in the text and several references are given. Thus I suggest to skip Figure 2.

The reading of the paper could be improved by clearly keep apart the chapters on methods and results. In particular the validation method is explained under methods but again methodical concepts of validation are shown again in the results chapter.

The authors split the sample of SWE in snow-rich and snow-poor days. Is there only statistical reason (variance) for this or is there some more motivation from physical based mechanisms?

Chapter 4 is related to validation of quantile mapping. 4.4.1. the term "Cross-calibration" is introduced. What is meant with validation of cross-calibration during calibration period? Is it not simple validation of calibration period?

In 4.4.2 the spatial and temporal consistence is tested using 4 stations left out from the data set for calibration. However three out of the four stations appear to be spatially

quite close to stations kept for calibration. Thus the validation appears not really robust to me.

Figure 14 is not clear with respect to the explaining text in the manuscript. In the text it is written that linear regression is computed for logarithmic axes of SWE and runoff. The scatter plot in Figure 14 is for the non-logarithmic data but the regression shown is for logarithmic data. This needs some clarification in the caption for Figure 14 (and would be useful to make it more clearly in the text, too). Additionally, I feel not confident with the example of Figure 14 showing the potential of SWE maps for low flow estimation. In fact the prediction band of the regression model is quite large. Is this really useful for practitioners?

The large number of abbreviations used in the study is a bit confusing and reduces the readability e.g. the different grid data sets used. A table on the grids on their role in the study would be helpful.

Minor comments:

P4245/7: delete "cosiderably"

P4251/11: Ad km to 1x1 grid.

P4257/5: SWE climatology is not a tool but a data set for hydrological model calibration/validation.

Figure15: units for SWE are missing.

For several Figures the axes labels are rather small.

Interactive comment on The Cryosphere Discuss., 7, 4241, 2013.