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Interactive comment on "Verification of forecasted winter precipitation in complex terrain" *by* M. Schirmer and B. Jamieson

Anonymous Referee #1

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The discussion paper presents an evaluation of winter precipitation forecasted by two NWP models and analysed by a precipitation analysis system in the mountains of western Canada and north-western US. Two winter seasons are considered for the NWP models while only one winter season is available for the precipitation analysis system.

The evaluation is not carried out using data measured at rain gauges because of the undercatch bias for solid precipitation but relies on automatic stations measuring snow water equivalent (SWE) or snow depth. The detailed snowpack model SNOWPACK is used to account for settling processes and to get daily new snow amount based on the precipitation simulated by the 2 NWP systems. The evaluation is based on performance measures that evaluate bias and accuracy of forecasted and analysed precipitation. An analysis of the economic value is also provided.

The topic of this paper is important for many users of weather forecast in mountainous terrain including avalanche and flood forecasters. It as also very interesting for the growing community studying the benefit of using NWP output to drive snowpack simulation in mountainous terrain. Prior to publication in TC, several points need to be clarified and the impact of some assumptions should be discussed in more details. They are listed below (General comments) followed by more specific and technical comments.

General comments

1) In this study, continuous modelled precipitation time series are made of successive 12-h forecasts (+6h to +18h using two initiation times per day). The authors should precise which initiation times are considered (00Z and 12Z?). An interesting aspect would be to consider time series made of successive 24-h forecast from one initiation time per day. This would allow the authors to build two continuous time series. Using 24-h precipitation forecast is more relevant for an avalanche or flood forecaster than using successive 12-h forecasts. Indeed, they generally need to take decision based on the forecast for the next 24 hours (see the example of road closure P 5745). What is the impact on performance measures? Does it change the economic values of a forecast?

2) The elevation difference between actual and model terrain height is a key parameter when evaluating NWP models in complex terrain. It is only mentioned in the text (P5733 l. 23 to P 5734 l. 3). A figure summarizing the differences between actual and model terrain height at different horizontal resolutions would help the reader to quantify the importance of theses differences. To handle these differences, the authors corrected the modelled data (including precipitation) for elevation differences following Liston and Elder (2006). The impact of the correction must be clearly quantified, especially since precipitations are corrected based on a factor that varies seasonally (Eq. 33 and Tab 1 in Liston and Elder (2006)). The text mentions that "these corrections increased the performance of the model" (p 5734, l. 1). To what extent are they improved? Are model

scores similar when considering for evaluation only stations with an absolute value of difference between actual and model terrain height lower than a given value (100 m or 200 m for example)? At 2.5 km grid spacing, the number of stations should be sufficient to compute relevant statistics.

3) The authors use the "daily new snow amount" to evaluate the quality of forecasted precipitation. The term "daily new snow amount (HTN)" should be more precisely quantified. Indeed, it is usually defined as: "Height of new snow is the depth in centimetres of freshly fallen snow that accumulated on a snow board during a standard observing period of 24 hours." (Fierz et al, 2009). In this study, the height of new snow has not the same definition and refers to a difference of snow depth between 24 hours. It includes the settling of new snow under its own weight and the settling of the underlying snow layers. The author uses SNOWPACK to account for the settling processes. A more accurate description of the use of SNOWPACK would be very helpful. For example, through a subsection describing the use of a detailed snowpack model to evaluate daily new snow amount: (i) Which atmospheric forcings are used to drive SNOWPACK? (ii) Is SNOWPACK run continuously from the beginning of the winter? (iii) What are the main limitations of the method: settling, density of fresh snow, melting, wind-induced erosion, . . . (partially discussed P 5743 l. 5-11).

Specific comments

1) Title: The name of the paper is questionable since it also contains an evaluation of output from a precipitation analysis system. Outputs from this system are not "forecasted precipitation". Therefore the name of the paper should be modified. Maybe "Verification of analysed and forecasted winter precipitation in complex terrain".

2) P 5728 l. 19 to 26: This paragraph is rather unclear and should be reformulated to focus more on the importance of a good estimation of winter precipitation in complex terrain and why NWP models are relevant for this estimation. Maybe split this paragraph into two paragraphs.

3) P 5729: Clearly define the terms "high-resolution" versus "low-resolution" since the meaning of these expressions differs from one community to another.

4) P 5733: l. 15 to 22: Mention the horizontal resolution of the CaPa precipitation analysis. Since your domain of interest covers Canada and the US, it would be interesting to know if the density of data considered for the precipitation analysis is similar on both side of the border. This may have an influence on resulting precipitation fields.

5) P 5733 l. 5 to 14: Include a short description of physical parameterizations in the NWP models involve in the generation of precipitation (cloud microphysical scheme, convection scheme . . .). This will help the users from other models to know what is implemented in GEM.

6) P 5735, l.6-17: Precise over which hours are considered to compute observed daily accumulation (HN and HNW)? Same question for simulated daily accumulation (P 5734, l. 7-9) ?

7) P 5737 l. 19: Eq (4) to (8) must be coherent. In Tab. 1, the variables a, b, c and d refer to numbers of events while in Eq (4) a, b and c refer to the relative frequency of the different outcomes contained in the contingency table (a/n, b/n and c/n with n being the total numbers of observations).

8)P 5738, l. 17-19: the authors mention the analysis of model performance as a function of difference between station and model elevation. However, the results of this analysis do not appear in the paper (see General comment 2).

9) P 5739, l. 5-10: A potential explanation could also be the settling of new snow. Steinkolger et al (2009) reports settling rates reaching 10 cm/day for freshly fallen snow.

10) P 5740, l.11-13: The poorer performances of CaPA in mountainous terrain in wintertime is not only associated with the quality of data entering the analysis system. It is also associated with the fact that correlation functions do not account for elevation and the number of stations entering the analysis may not be optimal in mountainous terrain.

See Durand et al. (1993) for an analysis system specifically developed for mountains.

11) P 5743 l 25: Present the analysis of economic value in a separated subsection to clarify the paper and reduce the size of current section 3.1.

12) P 5747 l. 2-3: no dependency was found with elevation at the scale of western Canada and NW US. What about potential elevation dependencies at the scale of a mountain range with a sufficient number of stations? In the US, it appears on Fig. 7 that you may have a sufficient number of stations in some mountain ranges to carry out such analysis.

13) P 5751 l. 14-17: the evaluation of a regional climate model (RCM) in complex terrain is not the main topic of this paper focusing on the evaluation of NWP system to forecast daily winter precipitation in complex terrain. The configuration of the NWP model may have evolve during the evaluation period and this evolution period covers only 2 winters (contrary to Ikeda et al. (2010) who studied for example four winters). I recommend the authors to remove the mention to RCM throughout the paper (at the end of the introduction and in the conclusion).

Technical comments

Text

Abstract: mention that this study is focusing on winter precipitation earlier in the Abstract.

P 5730 l. 1: replace “used in our study” by “evaluated in our study” since no specific GEM simulation has been carried out in this study.

P 5731, l. 29: use “Mahfouf” instead of “Mahfoufh”.

P 5737, l. 18: “. . . based on the empirical . . .”

P 5747, l.23: add “turbulent suspension” as a process not resolved at the scale of current NWP models.

Tables and Figures

Table 1, caption: specify if the variables a, b, c and d refer to number of event or relative frequency.

References

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