

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

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Received and published: 4 February 2015

We thank Richard Essery for these very helpful and constructive comments, which will help to improve this manuscript. His comments are pasted in quotation marks.

“I think that the abstract should make it clearer, as the main text does, that the example action given is the implementation of an avalanche warning service at large cost. It seems fairly obvious that an action of that sort would be more likely to be based on the climatology of a region rather than cumulated forecasts, and forecasts can still be highly valuable in preparing for individual extreme events”

The conclusion in the abstract is based on a cost-loss ratio, not on the absolute costs. We have removed the brackets in the abstract when referring to costs relatively to an-

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anticipated losses to emphasize on this ratio, not on the costs alone. The cost-loss ratio integrates all potential users. At a low cost-loss ratio both expensive and cheap measures are combined, relative to anticipated losses when the measure is not applied. It seems not that obvious to me that an implementation of an avalanche warning service would be more likely based on climatology of a region. Implementation of such a service is obviously a measure at large costs, but what are the potential losses? Rheinberger et al. (2009) tried to assess the losses in their study, and I cannot read any obvious conclusions in this matter. The optimal option (installing of a warning service or structural measures) seems to be dependent on site-specific avalanche paths characteristics (which is not dependent on climatology) and the economic importance of the road (which is included in the potential losses). Moreover, they presented results were dependent on a social discount rate. Applying the presented range of this discount rate dramatically altered the losses (no effect on the costs). To implement an avalanche warning service, although at large cost, inherited both very large and very low cost-loss ratios, dependent how one interprets the losses in terms of the social discount rate. On a process based view I would also disagree that an implementation of a warning service is likely based on climatology. Also regions with low snow amounts may cause in general a serious threat for roads. Large temperature gradients in a shallow snowpack cause faceting of crystals, and subsequently cause avalanches, triggered for example by small amounts of new snow. Climatology of precipitation is to my opinion not a good argument of implementing an avalanche forecast. But climatology serves well as a reference in this analysis, since the reference is not good.

“page 5728, line 19 “the question of how much snow”.”

Changed as suggested.

“5728, 24 NWP models were not initially developed with adequate spatial resolutions for complex terrain, and there are few such even now.”

This sentence was changed to include the reviewer’s comment.

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“5729 Note that the “double penalty” affects a feature that is correctly forecast in magnitude but spatially offset from observations. The illustration in Ebert et al. (2008) uses a radar-based precipitation product on a 5 km grid; I don’t think that it could be so readily identified for the coarser and irregularly spaced weather stations here.”

The reviewer is correct that this effect will not apply for our coarser spaced stations. We excluded the term “double penalty effect”.

“5729, 11 “which cause regular verification metrics”.”

Excluded due to the changes mentioned above.

“5730, 5 “a snow storm on 12 February 2000”.”

Changed as suggested.

“5732, 24 “the question of how well”.”

Changed as suggested.

“5733, 15 CaPA has been operational since 2011, so why were 2012/13 data not available?”

As written in the original manuscript in line 8 of page 5733, we had to download the data on a daily basis from the mentioned source, since the data is only available for approximately 24 hours until it is deleted again. For a current project of operationally assisting avalanche forecasters in Canada we downloaded continuously relevant data, which initially not included CaPA. At the time of the analysis it was not clear if a request of archived data is possible.

“5733, 25 How large are the differences between model and station elevations?”

An analysis of this topic was also requested by the other reviewer and is now added to the manuscript.

“5734, 11 The term “snow harp” (a device developed by SLF) will not be meaningful to

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most readers.”

We added a description of this measurement device.

“5734 Is it either snow depth or snow water equivalent measurements that are used at each site and never both? How do the numbers of non-precipitation events compare for sites where both measurements are available?”

We added a comment when introducing the stations that at many stations both measurements are available. This is also visible in Fig. 1. We also clarified the sentence in line 9 on page 5739, in which we investigated the non-precipitation events for sites with both measurements. We also added a clearer explanation of why non-precipitation events are different between the both stations.

“5736, 4 What criteria were used to identify observations as outliers?”

We investigated obvious outliers by visual inspection of the data. This is now stated in the manuscript.

“5736, 14 “greater than specific thresholds”.”

Changed as suggested.

“5737, 16 “the decision maker suffers a certain loss”.”

This is the original formulation.

“5737, 18 “based on the empirical frequency”.” Changed as suggested.

5738, 6 “economic loss relative to decisions”

Changed as suggested.

“5739, 5 This point would be a little more clear if the same vertical scales were used in figures 2 a and b.”

Changed as suggested.

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“5740, 6 Yang et al. is missing from the references”

We thank the reviewer for finding this missing reference.

“5740, 23 “The values for CaPA are shown”.”

Changed as suggested.

“5740, 27 “both the NWP models”.”

Changed as suggested.

“5741, 26 The WMO SPICE programme could provide the suggested independent measurements <http://www.wmo.int/pages/prog/www/IMOP/intercomparisons/SPICE/SPICE.I>

The SPICE program was mentioned in the new version of the manuscript.

“5742, 24 “a subset of the same stations”.”

Changed as suggested.

“5742, 28 Should be $(a+c)/n$? I’m not clear what “the baserate of the categories” means.”

Corrected and clarified.

“5743, 6 “, but the parameterization was done”.”

Changed as suggested.

“5743, 21 “The high resolution GEM-LAM in the winter”.”

Changed as suggested.

“5743, 24 “in both the verification data sets”.”

Changed as suggested.

“5744, 23 “these measures should not rely on a precipitation forecast alone”.”

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Changed as suggested, but we still used the word “rely”. We anticipated that this was meant by the reviewer.

“5745, 16 “we want to give an example”.”

Changed as suggested.

“5748, 14 “underestimation by the NWP models”.” Changed as suggested.

Interactive comment on The Cryosphere Discuss., 8, 5727, 2014.

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