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The Cryosphere Discussions

Interactive comment on "pSNOWPACK: a forecasting tool for avalanche warning services" *by* S. Bellaire et al.

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This paper presents the first attempt I am aware of to use forecasted data to drive the snow model SNOWPACK. In the past, SNOWPACK has been driven using real-time weather station data in a nowcasting mode. While I do think the topic warrants publication, I feel that some of the findings have been somewhat overstated and I think some changes are necessary to increase the clarity of the article. I would consider most of these comments to constitute minor revisions.

The title does not really convey what the article is about. This article is not about forecasting or avalanche warning services, but rather about a validation test to see how well SNOWPACK works at a single point when the data driving the model come from a gridded weather forecasting model. I think the title should be re-worded to better

reflect the content of the article. Perhaps something about using forecasted weather data to drive a snowpack model? The authors state in the article (line 8 on page 2264), "avalanche warning services are more interested in the snow cover layering and the formation and evolution of critical layers". As you'll see from my next paragraph, I feel this article does not sufficiently focus on snow structure for that to be a major component of the paper or the title.

The authors state in the Abstract (line 7-8) and throughout the paper that they hope to forecast three key factors: new snow amounts, surface hoar formation, and crust formation. However, they only really thoroughly and quantitatively assess the ability of their model chain to assess new snow and total snow depth. To assess surface hoar formation and crust formation they only visually compare a single manual profile in one season against the model output for that day, and then conclude that the two are in "good agreement" (line 15, p. 2266). Given that the model was driven by forecasted data, I agree that the model chain has more-or-less done a reasonable job. However, from the standpoint of an avalanche forecaster, the two profiles contain enough differences that I am not convinced a field person would consider the agreement to be good. More importantly, we are only looking at a single profile. Many more profiles (preferably from a couple different locations) would be needed to make any solid conclusions about how well the model chain is doing at predicting the snow structure. It would have been nice for the authors to include an additional location that may have had more manual profiles available. Without that, I would suggest that the authors focus their efforts on the snow depth and new snow amounts and not on the snow structure. I think the snow structure can be mentioned, and this can be given as an example, but I don't think that much in the way of conclusions can be drawn from this single visual example. As such, I feel like some of the wording in the article should be toned down. For example, I find the sentence at the end of the abstract (lines 15-18) to be a large claim based on the limited snow structure data examined in this work.

The time steps for the forecasted GEMS data are not clear to me. Through the be-

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ginning of the paper I kept wondering what “forecasted” GEMS numbers were being used to drive the model. Reading the methods (p. 2258) it sounds like the forecasted data are three hour time steps that are produced every 12 hours. Is that true? So, the first time step is three hours after the model output, then 6, 9, and 12 hours? If this is the case, it would be helpful to explain it more clearly early in the paper. It seems like an explanation could be something simple like “Every 12 hours, GEMS provides three hour time step data for the upcoming 12 hours. The model was run with these forecasted data.”

One interpretation made by the authors was that “early season over-estimation of snow depth” is due to the model “treating precipitation as snow only instead of rain or a mixture of rain and snow” (lines 18-20, p. 2264). I assume they have data to support this? Further, in the next paragraph the authors state that “negative outliers. . .are related to large storm event with low-density snow”. In this case a single example is given. Would it be possible to better show all of these results in some graphical form? Perhaps a simple scatterplot of forecasted versus observed HN24 with a 1:1 line could be instructive? This might graphically show that the large snow events are underestimated, and perhaps a group of events (like the overestimated early season events) could be circled (or identified in some way).

On line 3 of page 2266 the authors state that “the most important information for avalanche warning services is information about the snow cover stratigraphy”. I would argue that the most important information is about the snow cover stability. Forecasters combine this stability information with information about snow cover layering, so I think you could say that stratigraphy is one important piece of information, but certainly not “the most important”. Further, I feel like this paragraph should be worded carefully (see my other comments above) to reflect the fact that this is only one profile and so only gives us the first snapshot of how we might be able to look at stratigraphy, but that more data are needed to look at this problem carefully or to draw any firm conclusions.

Minor points:

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1. What does the “p” in pSNOWPACK stand for? All I could think is that it might stand for “practical” or “practitioner”, but the current version of SNOWPACK is already used by practitioners. Would “f” be better (fSNOWPACK, as in “forecasted SNOWPACK”)? Or perhaps the authors have a better idea, or could at least tell us what the “p” stands for?

2. On page 2254, line 27 the authors state that there are few snow observations so this “often” results in very little or no information about the state of the snow cover. I would argue that if this is truly the case then the forecasters should not be forecasting for a region that large. Such information is necessary for a reasonable forecast. This might read better if it emphasized the “large area”. Perhaps something like: For very large forecasting regions this might result in little or no information on the state of the snowcover in some areas.

3. Delete the sentence that starts on line 4 of p. 2255. It’s too redundant. Add “data from” between the words “to” and “about” on line 3.

4. On page 2255 it feels like there is a great deal of discussion that essentially says that Canada is big and data sparse. Perhaps this could be condensed a bit?

5. Also on page 2255 the authors state that there are some areas that have no weather station data and no skilled observations, making forecasts in these areas “challenging” (line 17). If there is really no data for these areas, I would say that reasonable forecasts are more than challenging. . .they are likely impossible.

6. Replace “became” with “are becoming” on line 18, p. 2255.

7. Line 1-3 on page 2257 are not really true since I think more than one profile would be needed for a true “assessment” (see discussion in the more major points at the beginning of this review).

8. I would suggest replacing the words “very good” with “reasonable” on line 23 of page 2257 since a number of factors could affect this measurement and this technique.

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9. Replace “was” with “were”, line 10 on p. 2258.

10. How far away is the GEMS data grid point from the Fidelity study plot? The lat/lon for the point and the difference in elevation from the point to Fidelity are given (lines 18-20), but how far away is the point? How will that distance affect the results?

11. Add an “s” to “Beside”, line 22, p. 2262.

12. Top of page 2263: Again, it should be emphasized that a comparison with a single profile is only a very early start at trying to understand things like surface hoar and crust formation, and we cannot draw strong conclusions from this.

13. Add an “s” to “parameter”, line 13, p. 2263.

14. Replace “contained not” with “did not contain”, line 20, p. 2265.

15. Replace “Columbian” with “Columbia”, line 21, p. 2265.

Interactive comment on The Cryosphere Discuss., 5, 2253, 2011.