Discussion of “Operational regional slushflow early warning”

AUTHOR RESPONSE TO REFEREE COMMENT 2

Sund et al.

20 January 2024

Responses to Referee #2 (Dieter Issler)

General comment

Referee comment: *This manuscript reports on the regional forecasting method for slushflows that has been developed and operated by NVE in Norway for about a decade. This slushflow forecasting service is one-of-a-kind worldwide, and the methodology has not been presented in a peer-reviewed scientific journal before to my knowledge. As such, the topic clearly falls into the scope of NHESS, is novel and will be of interest to the journal’s readers. Moreover, I applaud the authors for publishing an account of their methodology in a scientific journal—it is important that a state-wide service like this be properly documented not only in reports written in Norwegian but also in a form that is accessible to scientists worldwide.*

In general, I found the manuscript interesting to read and informative. The comments by Harpa Grímsdóttir are, in my opinion, well taken and should be incorporated in the revised version. In addition, there are three broader aspects—to be explained below—in which I consider improvements highly desirable.

Author comment: We are grateful for your encouraging comments and for your comprehensive review of our manuscript. The comments by Harpa Grímsdóttir are already considered and incorporated in the revised version.

We have taken several steps throughout the manuscript to clarify which part is automatically delivered from Xgeo (simulations) and what is manual work of the forecasters.

Presentation

Author comment: General overview

We have reorganised some of the text and sections (mainly according to RC2’s suggestions):

- Sect. 1 L66 – L84 and L94 – L101 are deleted.
- Last part of Sect. 2 is moved to first part of Sect. 3.
- Fig. 2 is modified, and Table 1 is moved forward in the manuscript for a better connection and reading.
- Sect.3.5 and 3.6 are switched to ease the flowchart reading.
- Previous Sect. 4 Operational assessment is now a part of Sect.3 where we have reorganised the last parts into 3.7 Operationalization and 3.8 Assessment where Fig. 6 constitutes an example.
- Application to other areas is now Sect. 4
- Parts of previous 6.1 are now moved to Sect. 5 Summary and outlook 5.2 Future perspectives.
- Sect. 6.2 Climate change is deleted. One sentence is kept in 5.2 Future perspectives.

Referee comment -2: *Similarly, Sec. 6 contains two disconnected topics. Future developments could be in a final section with conclusions and an outlook, whereas the topic of climate change feels artificially propped onto the paper. The options are to (i) leave it out, (ii) move it into an appendix, or (iii) make it the sole topic of the next-to-last section. The introduction should then, however, clearly motivate why this topic is included in the paper.*

Author comment: We agree. We have decided to leave out the climate change topic. Although there are often questions about how the number of slushflow event is expected to develop with future climate change. Our intention was to show that this is not a straightforward task. Possibly it is a topic for a discussion in a separate paper.

Referee comment -3: *There is a slight incongruence between the steps indicated in Fig. 2 and the sectioning of the text explaining these steps. Evaluation of the WSR could be shown as its own step in Fig. 2, followed by a "synthesis" step. Section 3.5 (observations) does not have a clear counterpart in Fig. 2. The first part of Sec. 4 could become 3.6 or 3.7.*

Author comment -3: We have now added a distinction between observations and simulations in Fig. 2 and added an element under hazard level showing possible re-evaluation of level based on observations during an ongoing situation. We have also highlighted the "ratio-step" (WSR) more clearly under assessment.

Referee comment -4: *Even though the introduction and Sec. 2 are not excessively long, they feel lengthy because they contain many details that are not relevant to the main topic (e.g., the matter of run-out modeling, which is largely irrelevant for a regional forecast). Moreover, the text does not get to the heart of the matter before Sec. 3.*

Author comment -4: We agree and have removed the paragraphs from the manuscript as well as the paragraph on the characteristics of various other Natural hazards forecasting approaches.

However, RC2 also correctly states that the audience "may be assumed knowledgeable about natural hazards in general but not about slushflows in particular." Therefore, we find it useful to provide some background, for readers less familiar with slushflow. After paper submission, we have got inputs in both directions. We are therefore rather confident that the current content is quite balanced and where it should be.

Referee comment -5: *There is no clear rationale stated for why this procedure is the best way of forecasting slushflows. It would also help if the authors stated even more explicitly where subjective assessments enter in the process.*

Author comment -5: We have featured more specifically and comprehensively that the SEW is developed within the given framework and resources that have been available for this task (Sect. 2.2, 3.8 and 5).

We have also included more text on the forecaster's manual assessments of the simulated Xgeo maps (Sect. 3.).

Figure and table changes

- **Fig. 1** Figure text is changed, now including information about the release area.
- **Fig. 2** The figure is modified to ease the reading. The figure text now also has information on the automatic and manual parts of the process.
- **Fig. 6** Seven observations representing other (OTH) snow types are included and WSR-lines (1.0 to 4.0) are visualised.
- **Table 1** Step 1 to 4 according to the steps in the slushflow hazard assessment presented in Fig.2 is now inserted under the four main parameters/sources.

Justification of the methodology

Referee comment -1: *What is the difference between slushflow danger levels 2, 3 and 4? I suspect they differ with regard to the forecasted number or size of events, but I could not verify this from the text.*

Author comment– 1: In the revised manuscript we have included a paragraph describing the different hazard levels used in the NLFWS bulletin (Sect.2.2). However, the levels are not defined by exact number nor sizes of expected slushflows. The hazard levels definitions referred to here, are according to organisational decisions. Within the framework of flood and landslide definitions of hazard levels, this is not (yet) a set up.

We agree it would have been advantageous if the levels were linked to at least an approximate number of slushflows per km^2.

Nevertheless, for internal purposes approximate numbers have been used. However, even then it is challenging to be sure the immediate evaluation is correct as many slushflow may be reported at a much later time than during quality control as discussed in Sect. 3.6, if reported at all. We have included reference to Devoli et al. 2021 where the joint evaluation with landslides is discussed.

Referee comment -2: *How do the values stipulated in Table 2 compare to the observations in the slushflow events from which the table is derived?*

Author comment -2: We have now visualised WSR-lines (1.0, 2.0, 3.0, 4.0) in figure 6. This makes it possible to compare the observations in the slushflow events to the values stipulated in Table 2. We note that it would be expected that WSR values of observations of the same snow type will spread over a range of values, since particularly snow depth will vary al lot on a regional scale. On the other hand, like in this case, if most of the observations of the same snow type falls around the ratio level for the danger level issued, this indicates that the danger level was correct.

Referee comment -3: *How exactly are observations (or non-observations) of slushflows incorporated into the decision process? The statement at lines 489–490 is fairly vague.*

Author comment -3: Thank you for pointing out this shortcoming. Since this is forecasting prior to a situation, preferably there will be no observations of slushflows at this stage. However, during an ongoing situation, and typically a multiple-day situation, observations are used to evaluate if the forecasted hazard level is correct, or if it needs to be changed. We have tried to incorporate this aspect in our revision throughout the manuscript.

Referee comment -4: *Is the regional topography not taken into account at all? At a grid size of 1 km^2, the gridded hydro-meteorological data do not take into account terrain features at the scales relevant for slushflows. Soil frost, soil saturation and ground water level seem to have the same spatial resolution of 1 km^2 and would not be sensitive to temperature differences within a grid cell, which can be substantial in high-relief terrain. It is unclear at which resolution liquid water content in the snow is estimated and in which way exactly it affects the estimated slushflow danger. Similar questions concern the water supply, both from rain on snow and snowmelt. One would expect that the type of terrain determines how frequent terrain features are that favor slushflow formation. The latter presumably has an effect on the hazard level that is assigned under given hydro-meteorological conditions. If this factor is not included in the assessment, it is OK, but it would be helpful for the readers if there was an explicit statement of this.*

Author comment -4: To better highlight our methodological approach to regional SEW we have moved the following text "Our methodological approach to regional SEW is based on the combination of factors that have previously been found to be decisive in the triggering process. However, in the regional approach, the geomorphic terrain features will be of less importance in the assessment, as the size of the area ensures their presence. Nevertheless, they are to some extent reflected in the yellow danger level. The variables for regional assessment of slushflow hazard can then be narrowed down to ground conditions, snow properties, and water supply". It now appears at the beginning of Sect. 3.

In the last part of the current Sect 2.2 we have now added the following text

"Note that the nature of slushflows makes it possible for them to occur both in flat and steep terrain. The current regional early warning is thus not to the same degree as snow avalanche early warning a mean of pinpointing safe terrain types in detail. Still, the different danger levels to some extent give a clue to where the hazard mainly could be expected.

At yellow (2) level the expected appearance of slushflows is mainly restricted to terrain formations particularly prone to slushflows, such as narrow outlets (cirques, funnel shapes) that enhance accumulation of water."

Referee comment -5: *Can parts of the analysis process be formulated in mathematically to make their description more precise? I also miss a more general discussion of the pros and cons of different approaches to forecasting slushflows, or at least of the reasons why the ratio of water supply to snow depth should be the most relevant indicator and by itself be sufficient. This question might be discussed on the basis of the hydrological and mechanical mechanisms responsible for slushflow release.*

Author comment -5: We are aware that for snow avalanche hazard assessment a more mathematical approach is used, where the avalanche hazard level is a function of snowpack stability, the frequency distribution of snowpack stability, and avalanche size for a given unit (area and time). Snow avalanche early warning has been conducted and developed for decades in various countries. However, the SEW is at a far earlier stage of development, and thus there is currently not sufficient data for a mathematically precise description of the various types of slushflow release.

In addition, the SEW is developed within and constrained by the existing framework already developed for NVE's other natural hazard EWSs, in particular that of landslide. The organisational decision influences both the danger scale definition and the evaluation process used for slushflows, i.e. there are no separate statistics for issued slushflow bulletins.

This has been better emphasized in the revised manuscript. Among other we have added more details on this aspect, in Sect. 2.2

We also believe that it would be beneficial in a future development to include more of the variety of slushflow releases in the assessment process when the necessary data is available.

Validation of the methodology

Referee comment: *Validation of a regional forecasting method is a crucial yet difficult step because the true hazard level is never known exactly. The manuscript does not carry out a comprehensive validation or at least propose a strategy for doing so. Instead, three slushflow episodes in Norway in 2018, 2021 and 2023 are used to show that slushflow events were expected to occur in all three cases on the basis of the estimated water supply–snow depth ratios. This is a plausibility check but cannot be considered a validation.*

Author comment: We agree that it would have been beneficial to have an individual validation of the slushflow early warning.

Performing a proper validation is currently challenging for two reasons: One is that according to the overall decision landslides and slushflows have been evaluated and validated collectively. The second is, that the hazard levels defined for these natural hazards (flood, shallow landslides and slushflows) is strongly based on *observed* occurrence.

We have included at a paragraph the end of Sect. 3 a reference to and explanation of the evaluation of the Norwegian Landslide Forecast and Warning service (NLFWS) in general as slushflows are subject to the same evaluation system. One of the conclusions in Devoli et al. (2021) - Seven Years of Landslide Forecasting in Norway—Strengths and Limitations is "It is too early to evaluate red levels after only 7 years of operation, because in general red level should occur very rarely (50 years return period in analogy to the national flood warning system)."

By this the difference between the danger levels defined for snow avalanches which are related to the actual condition and that of flood and landslides (including slushflows) where it is decided that the danger levels should reflect statistical occurrence of the events. Strictly speaking this means that in a changing climate what is defined as red level today, may only be an orange level in the future.

Furthermore, it could also be discussed whether it is reasonable to restrain a natural hazard related to snow to a very rare frequency as snow, unlike soil, is a matter that not only varies from year to year but may also change characteristics/properties from day to day.

Nevertheless, these questions are outside our mandate within the organizational decisions.

Bullet point3: One version of this suggestion is already included in the routine and assessment as two persons (independently) perform the hazard assessment daily.

Bullet point4: This has been and still is occasionally done.

Other comments

Referee comment: *Numerous minor comments and language suggestions are collected in the annotated manuscript.*

Author comment: We appreciate being guided towards more precise language. Most of the language suggestions are followed, and grammatical errors are now corrected in the revised manuscript.

Referee comment -Table 1: *Do regional offices, responsible for areas of 30,000--100,000 km^2, have sufficient information on ground conditions?*

Author comment: Yes, to some extent they can provide useful information about ground conditions although not always for all areas.

Referee comment: *Could you characterize the HBV model and the Motovilov approach in two sentences for the sake of non-hydrologists?*

Author comment: We have added "The soil frost depth model is based on the physics of heat conduction in the soil matrix and in the snow cover above the ground. It is used with air temperature data and precipitation data as input."

Referee comment: *Suggestion of "outside Norway" in title Sect. 5 rather than "other areas".*

Author comment: We have kept "other areas" because Longyearbyen, Svalbard is also a part of Norway, but not a part of the SEW that is limited to the Norwegian mainland only.

Referee comment -Fig. 6: *Are these all the data that are available? Should there not be several events of the class OTH?*

Author comment: Fig. 6 was only an example, but we have added examples of OTH snow type as well in addition to a paragraph discussing the event.

Referee comment: *Why no mention the student names.*

Author comment: We principally agree. However, many people have been involved and our main intention has been to acknowledge this. The reason why they are not mentioned is partly that since controls of slushflow partly has been combined with landslides the overview of people is not flawless. Thus. we would not risk leaving a name out. (Another reason is that many other people within the organization have contributed in different ways, among other developing and maintaining the database).

Author comment: We have replaced hazard levels with danger level in the new version.