Answer RC2

François Doussot, Léo Viallon-Galinier, Nicolas Eckert, Pascal Hagenmuller

---

1 Specific comments

Comment 1

> Fig 2 shows an increase in avalanche activity, including the period 2006-2022. Arguably climate change was already taking effect here, yet rather than decreasing the records increase. This increase is insufficiently explained in the methods, as well as the effect it can have on the results.

We agree that Fig. 2 shows an increase in observed avalanche activity over the 2006–2022 period. However, this period is relatively short for assessing climate trends. In mountain environments, avalanche activity exhibits strong inter-annual variability driven by fluctuations in snowfall, temperature, and snowpack conditions. Consequently, trends computed over a 17-year period may be strongly influenced by internal variability and are not necessarily representative of longer-term climate-driven changes, which are typically assessed over periods of at least 30 years.

The machine-learning model was trained on the observed avalanche record over this period under the assumption that the observation process remained sufficiently homogeneous. This assumption is supported by discussions with local avalanche observers, who did not report major changes in observation practices during the study period. While minor inconsistencies in the observational record cannot be completely excluded, no evidence was identified suggesting a systematic change that could explain the observed increase in avalanche activity.

Importantly, the objective of the model is not to reproduce the temporal trend present in the observations themselves, but rather to learn the statistical relationship between meteorological and snowpack predictors and the observed avalanche activity. The trained model is then applied to meteorological and snow conditions simulated by the reanalysis and climate model chain to reconstruct the expected avalanche activity associated with those conditions. In this framework, potential short-term fluctuations in the observed record primarily affect the calibration dataset but would not impose trends on the prediction.

Nevertheless, we acknowledge that any non-climatic changes in the observational record could influence the learned relationships and therefore represent a source of uncertainty. This limitation is discussed in the manuscript.

Comment 2

> ln 195: Quantile mapping assumes distributions in historical data are preserved in future data. As such, they tend to impose past changes on future data, and are generally not well suited for the analysis of extremes. The authors would improve the paper by mentioning this in the methods, as well as the way the ADAMONT dataset aims to mitigate this. Now the reader has to wait for the discussion of this -in my opinion- important aspect.

> We thank the reviewer for raising this point. Quantile mapping is known to have limitations regarding the preservation of quantile-specific climate change signals and the representation of extremes. Since this aspect is important for interpreting our results, we now introduce it in the Methods section, where the ADAMONT framework is presented, rather than postponing the discussion until later in the manuscript. We also further discuss the potential consequences of this limitation for our analyses in the Discussion section.

Comment 3

In the discussion of these shortcomings due to quantile mapping (sect 4.2), it could be elaborated upon how these shortcomings might affect the results (more than just that they affect the results).

> We agree that it is important to discuss not only the existence of this limitation, but also its potential implications for our results.
>
> As highlighted by Cannon et al. (2015), standard quantile mapping (QM) may modify the climate change signal simulated by the driving GCM, particularly for upper quantiles and extreme events. In their analysis of daily precipitation in Canada, QM was shown to substantially amplify projected changes in extreme precipitation (+500%) relative to the raw GCM output (+120%). More generally, QM does not guarantee preservation of quantile-specific future trends and may therefore alter projected changes in extremes. In this example, extreme precipitations are amplified by the QM method, but it could also be dampened depending on the tail distributions (of the simulated and reference dataset).
>
> In our case, we are unable to quantify the influence of QM on our results, nor even determine the direction of any potential bias. Such an assessment would require a dedicated sensitivity analysis, for example, by comparing multiple bias-correction methods (e.g., QM, QDM, ADAMONT) and/or by analysing the raw climate model outputs. This is beyond the scope of the present study. Nevertheless, we agree that this represents an important source of uncertainty and have strengthened the discussion accordingly. However, we compared the results obtained using the reanalysis (so without QM) and the historical ADAMONT simulations on the same perid. The results in the climate trends are very similar (-4.7% per decade for the reanalysis, -5.9% with ADAMONT in the annual number of avalanches), which shows that the ADAMONT method does not lead to inconsistent climate trends.
>
> We also note that the ADAMONT framework includes a specific treatment for values above the 99.5th percentile. Rather than applying a standard quantile-mapping correction beyond this threshold, a constant adjustment based on the 99.5th percentile is used to allow values outside the historical range (Verfaillie et al., 2017). This approach aims to reduce some of the limitations of classical QM when dealing with unprecedented extremes, although it does not completely remove the uncertainty associated with bias correction of future extremes.
>
> Finally, Cannon et al. (2015) showed that quantile delta mapping (QDM) better preserves the relative changes simulated by the raw GCM. Future work could therefore investigate the sensitivity of our results to the choice of bias-correction and downscaling methodology, here directly imposed by the ADAMONT approach.

Comment 4

fig 5: from the figure it appears if the model underestimates the extremes in 2017 on all metrics. This should be related to the shortcomings due to quantile mapping mentioned above.

> We agree that the model underestimates the extreme avalanche activity observed during the 2017–2018 period (Fig. 5), as discussed in the manuscript. In the answer to RC1, we discuss why the model tends to underestimate this type of events. More specifically, to answer this comment, we would like to clarify that Fig. 5 corresponds to simulations driven by the S2M reanalysis, and therefore does not involve any quantile mapping correction. The underestimation of these extreme events is thus not related to the quantile-mapping method.
>
> This discrepancy is most likely explained by the rarity of such extreme avalanche conditions: the 2017/2018 extreme avalanche event has never been observed in the training dataset. The machine-learning model learns the relationship between meteorological/snowpack predictors and observed avalanche activity from historical data; however, very high daily avalanche counts such as those observed in January 2018 are poorly represented (or have never been seen) in the training dataset. As a consequence, the model tends to regress toward more frequently observed conditions and cannot fully reproduce extreme regimes that lie outside or at the edge of the training distribution.
>
> This limitation is inherent in the approach used here and reflects the assumption that the training dataset provides a sufficiently representative sample of avalanche activity, including extreme events. When this assumption is not fully satisfied, extreme values are typically underestimated, particularly at the daily scale. To predict such events, the model would likely benefit from a larger training dataset, for example by extending the training period further back in time. However, as discussed in the manuscript, incorporating older observations introduces additional challenges related to the increased heterogeneity of the observational record, which may negatively affect the consistency and quality of the training dataset.

Comment 5

ln 330; temporal linear trends are calculated (% per decade). Why the assumption that the changes should be linear in time, since warming or CO2 increase are not?

> We thank the reviewer for this relevant comment. The assumption of a linear trend does not imply that the underlying climate forcing (e.g. temperature or CO2 concentration) evolves linearly in time. Rather, it provides a first-order approximation of the average rate of change in avalanche activity over the study period.
>
> Given the relatively short time window considered in this study, a linear model offers a simple and robust way to summarize long-term tendencies while limiting the influence of interannual variability. This approach is widely used in climatological and cryospheric studies to express changes as rates per decade, which facilitates comparison with previous work.
>
> We acknowledge that climate drivers and associated physical processes may evolve nonlinearly. However, capturing such non-linear responses at a local scale, where the interannual variability is very high, would require either longer time series or explicitly time-varying models, which are beyond the scope of the present analysis. Here, the linear trend is intended as a descriptive metric rather than a mechanistic assumption about the system.
>
> Future work could also investigate more flexible formulations, such as piecewise linear trends, to account for potential changes in the rate of evolution over the study period. This extension is beyond the scope of the present analysis but represents a promising direction for further research.

Comment 6

> fig 6: Not clear in black and white.(I don't know if that is still a requirement in this day and age, but:) This could be improved by different linestyles for observed and simulated trendlines (e.g. dashed and dot-dashed).

> This has been corrected in the new version of the manuscript.

Comment 7

> In general, many studies have noted or predicted a decrease in avalanche activity due to climate change. However the question why is not always elaborated on sufficiently. The obvious explanation is that the winters are simply getting shorter, and as a result, less avalanches occur over a season. Section 4.3 (and possibly the introduction) could benefit from mentioning this point explicitly. IMHO the interesting question is whether the decrease in avalanche activity can be solely explained by the decrease in winter duration, or if there are other dynamics at play. I hate to be the reviewer suggesting that his own paper be cited, but I'm going to do it anyway, as I feel that in this case mentioning this work is relevant.

> Thank you for this relevant comment. We agree that the role of the shortening of the avalanche season should be explicitly acknowledged. This point has now been added in the introduction. We analysed the seasonal distribution of the simulated avalanche activity (Fig. 7b), which shows a shortening of the avalanche season in average. It has now been explicitly added to Section 4.3, which mentions the work you suggested. However, we did not directly link the computed trend to the evolution of the physical drivers. While the reduction in winter duration is a key and physically intuitive explanation, since avalanche activity cannot occur in the absence of sufficient snow cover, other processes such as changes in snow stratigraphy may also contribute to snowpack stability. If we want to go further and link the results of the machine-learning model (number of avalanches) to the physical drivers, it would require a dedicated interpretable modelling framework (using shapley values for example), which is beyond the scope of the present machine-learning approach. In purpose, we therefore did not further interpret the physical drivers of the simulated trends.

2 Additional modifications

In the previous version of the manuscript, a single model was trained using the S2M reanalysis dataset over the 2006/2007–2022/2023 winter seasons and then applied to the entire 1958/1959–2022/2023 period to estimate daily avalanche activity. However, this approach introduces a methodological inconsistency because the training period is also included in the reconstruction period. As shown by the leave-one-year-out cross-validation, the model exhibits systematic biases. When the model is evaluated on the data used for training, these biases are artificially reduced, which may, in turn, affect the estimated long-term trend.

To avoid this issue, we revised the methodology. For the 1958/1959–2005/2006 period, avalanche activity is estimated using a model trained on the 2006/2007–2022/2023 dataset, as in the previous version of the manuscript. For the 2006/2007–2022/2023 period, we use the results of the leave-one-year-out method as done in Sec. 3.1, ensuring that each year is predicted by a model that was not trained on that year.

This revised approach provides a clearer separation between model training and application, thereby avoiding potential in-sample bias in the trend analysis. The correction leads to moderate

	Annual	DJF	MAM	Worst week
Previous version	-6.0%	-4.7%	-8.4%	-3.5%
New version	-4.7%	-1.5%	-8.4%	-1.7%

Table 1: Changes in the computed trends from the reanalysis. DJF refers to winter months (December, January, February), and MAM to March, April and May.

changes in the results, with generally weaker estimated trends than those reported in the previous version, as shown in Table 1. The discussion is still consistent with these new computed trends.