Clim. Past Discuss., 7, C2706–C2710, 2012
www.clim-past-discuss.net/7/C2706/2012/

Climate of the Past Discussions

Interactive comment on "Snow and weather climatic control on snow avalanche occurrence fluctuations over 50 yr in the French Alps" *by* H. Castebrunet et al.

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Received and published: 20 February 2012

Thanks very much for your interest and comments. Please find our answers for each of your points:

(a) Multiple statistical tests are applied but Bonferroni (or related) corrections do not seem to have been applied. This may have led to incorrect inference in places and should be addressed by adjusting the relevant p-values where required.

»> Yes, this is a well know problem of multiple comparisons.

The more tests you make, the higher the likelihood of falsely rejecting the null hypoth-

esis. There are different ways to counter this, the most famous being the Bonferroni correction. However, it is not free of artefacts as well (e.g. it is known to be very conservative), so that we chose not to apply it. A practical justification in our case is that, in all analysed series, the retained models have a (relative) little number of covariates with regards to the full set of possible covariates available for selection, and that all of them are marginally significant.

We have added section 2.5 p.4184 l. 4 "Another problem of stepwise procedures is increasing probabilities with the number of tests done of falsely rejecting the null hypothesis of no effect of the additional variable. There are different ways to counter this, the most famous being the Bonferroni correction, but none of them is free of artefacts, so that we chose not to use them." And section 3.2 p.4186 l. 27. "It must be remembered that no Bonferroni-like correction has been used in the stepwise procedure. A practical justification is that the retained models have all a (relative) little number of covariates with regards to the full set of possible covariates available for selection, and that all selected variables are marginally significant."

(b) I think it would help the reader if the components of the SAFRAN-CROCUS-MEPRA model chain were discussed with respect to alternative models (e.g.CROCUS vs SNOWPACK) so that their merits and demerits as forecasting tools can be assessed.

»> This work has been realized in the frame of a project founded by French ministries, in collaboration between 2 institutes: Météo-France and IRSTEA (ex Cemagref). One of its goals was to combine both institutes' data base (avalanche counts from IRSTEA, and snow and weather parameters from Météo-France snow and weather models). Therefore, discussing different snow and weather models merits and demerits, as forecasting tools, is somewhat beyond the scope of the project and the paper, and some existing studies may better respond to your request. Several snow models exist, the more famous being SNOWPACK (i.e. Bartelt and Lehning, A physical SNOWPACK model for the Swiss avalanche warning: Part I: numerical model, CRST, 35, 3, 123–145, 2002). They have been validated and intecomparated in the framework of the SnowMIP project

(Etchevers and al., Validation of the energy budget of an alpine snowpack simulated by several snow models (SnowMIP project), Ann. Glaciog., 38, 150-158, 2004). Any other snow model could have been used for this study. The advantage of the SAFRAN-CROCUS MEPRA model chain is to give distributed information for each massif and for different levels of elevations and slopes, which allows describing the spatial and temporal variability of the snowpack at large scale, through the integration of numerous snow and weather observations in SAFRAN assimilation.

We added p. 4180 l. 26 “Validation of CROCUS outputs and some comparison with other snow models can be found in Etchevers et al. (2004).”

(c) The conclusion about a peak in activity in _1980 from the 5 year running means is very interesting and important. Does this relate to any meso-scale circulation phenomena (e.g. the NAO etc.?) Some comment on this by the authors would be helpful.

»> Some studies already compared and analyzed correlations between NAO and avalanche events in Iceland (Keylock, GRL, 2003) and the Spanish Pyrenees (Garcia et al., CRST, 2009 ; Gracia-Selles et al., CRST, 2010), with rather conclusive results, presumably because of a relative “clear” link between NAO and winter climate in these regions. In the French Alps, the results of Durand et al. (2009 a, b) show a much less “simple” link between NAO and temperature fluctuations and no signal with variables related to precipitation and snow, so that relating avalanche activity to synoptic patterns was found too ambitious in this first approach. This would require testing different synoptic indexes (not only NAO) to find the most adapted and the paper is already long. . .

We added p. 4203 l. 7 “A possible interesting further work would be to relate these results to synoptic patterns such as annual NAO anomalies, as already pointed out in other regions (Keylock 2003; Garcia et al., 2009).”

(d) Minor comments:

(d1) P.4174 L.25 Either “Mountainous areas and high latitudes are very sensitive: : :”

or “Mountainous areas, as high altitude regions, are very sensitive: : :”

»> ok. We modified: “Mountainous areas and high latitudes are very sensitive”

(d2) P.4175 L.25 “Intuitive” is not clear here.

»> Ok. We modified: Âń Over longer time steps, a strong climatic influence on fluctuations of natural avalanche activity sounds logical, but . . .”

(d3) L26 McCarroll P.4176 L.6 “, which is often”

»> Ok, we modified.

(d4) P. 4178 L6. “in the data series”

»> Ok.

(d5) P.4182 L6. I find it hard to interpret the statement that the data fall within the -2 to +2 interval without accompanying information on the typical autocorrelative structure of the data. It would be helpful if this were provided.

»> The 80 and 90% percentiles considered here have to be interpreted as simple indicators of high/low values when exceeded. We do not evaluate the precise probability of a given value to be exceeded a given year which would indeed require taking into account the autocorrelation structure of the residual. Note anyway that the autocorrelation structure is hard to evaluate with a 50 points time series, especially at long range.

(d6) P.4185 L21. I think the commas are better as “The interannual variability is very high with, for all indicator series, years of : : :”

»> Ok.

(d7) P.4186 L10. If these results are Bonferroni corrected (as they should be) presumably non are significant?

»> See main comment (a).

(d8) P.4187 L27. Is there a non-linear, or threshold relation in evidence here, where, for example, mid-values for depth stabilise the pack, but the largest promote instability? If so, this might also explain the nature of the result found.

»> This is an interesting comment, but the scatter plot of Fig 5 does not show an obvious non-linear relationship between snow depth and the CI.

(d9) P.4202 L10. small increase rather than short?

»> Yes, ok.

(d10) P.4202 L11. “and was more marked” not “mostly marked”

»> Ok.

Interactive comment on Clim. Past Discuss., 7, 4173, 2011.