Review for *The Cryosphere Discussion*, 8, 581–640, 2014, doi:10.5194/tcd-8-581-2014
'*Projected changes of snow conditions and avalanche activity in a warming climate: a case study in the French Alps over the 2020–2050 and 2070–2100 periods*'
by Castebrunet and others.

General comments

This contribution is an interesting attempt at investigating the evolution of natural avalanche release in the future considering the impact of projected climate scenarios. The authors themselves are fully aware of the difficulty of such a study (p. 585, lines 3ff, p. 584, lines 25ff). Indeed, most catastrophic avalanche cycles are related to extreme meteorological events but it is still a matter of debate whether those can be reliably captured in complex mountainous environments with the limited topography rendering of today's Regional Climate Models (RCM). Knowing the background and knowledge of some of the authors, I do not question the rigor of the meteorological and statistical methods presented and used in this contribution, but I also admit that this is not my field of expertise. In addition, I acknowledge that they put great effort in giving a detailed description of the methods used in this context.
The work presented here is also based on numerous previous studies in which the authors were involved. Therein they considered single aspects of the present work in details. Thus this contribution builds on a solid basis to take the last step, that is, evolution of avalanche activity under a changing climate.

One of the most difficult tasks, however, is to find appropriate metrics for the intensity of avalanche cycles and their underlying origin. The authors propose two indices, the MEPRA Index (MI) and the Composite Index (CI), which also takes account of observed avalanche activity. Under climate projections, MI can be retrieved from model simulation of the snow cover driven by Regional Climate Models while the use of CI also required robust regressions over the reference period 1961-1990.
In my view, however, both MI and the avalanche activity are points of matter here. On p. 588, lines 14-15, the authors note [adapted text, see technical corrections], "The MEPRA natural stability index is a proxy for avalanche danger. It does not tell, however, whether spontaneous avalanches actually occur." Indeed, to my knowledge, MI is mainly based on a static stability approach that will not reveal all subtleties of natural avalanche formation and may not be well suited to capture wet snow avalanche episodes (p. 590, lines 15-17). The observed avalanche activity in turn may be a good indicator of natural avalanche release during single catastrophic or extreme events where observations can be made. But what about scarcely observed regions and less extreme events with only near misses? While the authors acknowledge the limitations of those indices, they nevertheless use them, averaging the results over large areas (for example, p. 589, lines 22-23). This leads to some surprising results, even to the authors (for example, p. 590, lines 7ff). Is this not worth discussing further? Indeed, I could imagine that natural avalanche release periods in the future may be more related to situations favouring numerous releases of middle size but still threatening avalanches that are less well captured by observations. Thus, one may ask whether the partly good results at these larger scales is somehow simply related to the climatic-geographical situation of the French Alps that will not apply to other regions.

In their conclusions (pp. 610 and 611), the authors put some shy question marks on the usefulness of their results, which I very much appreciate. While I agree that the methods presented are robust, I'm less optimistic they really capture the processes at work, for the reasons mentioned above and because these processes are really not that simply related to air temperature and new snow depth, for example. That brings up some doubt about the chosen indices and the real problem to address, doesn't it? Some feeling of not resolved yet.

In summary, this contribution addresses a yet to be resolved issue but presenting robust methods to be used once better metrics of natural avalanche release are found. In addition, some results may well apply to the French Alps as such. I therefore recommend accepting the paper after the authors addressed the issues raised above and below.

Specific comments

- p. 585, lines 14ff: *and a shift in their timing, in good correlation with field observation of snow cover wetting at small scale and its link with wet snow release susceptibility (Mitterer et al., 2011), ??*
- p. 585, line 25: "*refined snowpack characteristics"*
- p. 608, line 2: "*different complex snow variables*"
  I'd propose to use "elaborate" instead of "*refined*" and "*complex*" in the text.

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- p. 587, line 10: "*no error-free modelled series*"
  What are those? Can you elaborate on this?
- p. 588, lines 19-20: "*aggregated at the massif scale thereby providing a single scalar value for a given date*"
  Is only MI aggregated at the massif scale? I guess you retain the four aspects and the three elevation bands?
- p. 590, Eq. 2: Would it be meaningful to discuss simple examples here? See Fig. 3, for example
- p. 591, line 15 to end of section 2.2: Does this not rather belong to the discussion?
- p. 592, lines 10-12: "*Notably, this is not the case for the Southern French Alps, but the total snow depth for a south facing slope which is included in the model may play a similar role.*"
  Or does this reflect the limitations of such exercises?
- p. 593, line 11: "*12 km resolution*"
  That is, about 25 % in area of a typical 'massif'!
- p. 596, lines 12ff: "*Obtained future samples of annual/seasonal means of the MEPRA index at the annual time scale are close to the ones briefly presented in Giraud et al. (2013), but evaluated with the additional CENT correction."*
  Thus where is the added value of the CENT correction?
- p. 598, lines 24-25: "*snow conditions on slopes*"
  Do we know which conditions you considered on what slopes? I may have missed it.
- p. 599, line 20: Your "*annual season*" is not even a full year. Misleading?
- pp 602-606: What are "*sufficient cold temperatures*" (p. 602, line 3)? Air temperature alone is not sufficient to describe the evolution of the snowpack. It is the full energy balance that matters. This is also of importance for a thinner snowpack (p. 603, line1; p. 606, lines 1 & 3): even though air and snow temperatures may be higher, radiative cooling may lead to weak basis layers that could be triggered by less overload. Thus it seems to me that the problem is oversimplified in this approach.
- p. 606, lines 23ff: Does your approach not lead to over-smoothing, particularly at the larger scales considered? Indeed, I would expect the contrary result: large scale: no correlation, small scale: correlation.
- p. 607, line 24: "*fundamental results*"
  I don't think the results are that fundamental.
- p. 608, line 13: "*temperature increase interacting with topography*"
  Can you precise what topographic features you are thinking of?
- p. 618, Table 1: *French Alps, spring*
  Can you explain the N & E wet snow vs S & W dry snow contrast? It seems counterintuitive!

Technical corrections (by page/line numbers)

- p. 582, line 17: "*total and dry snow depths*"
  Snow depth is snow depth (see International Classification of Seasonal Snow on the Ground [ICSSG]). I doubt the reader may make easily the difference between total and dry snow depth.
- p. 584, line 1: "*snow precipitation phase*" ?
- p. 584, line 8: "*snow variables*"
  Which one?
- p. 584, lines 14 & 22: "*snow evolution*"
  Evolution of what? Area? Height? Microstructure?
- p. 584, line 16: replace "*snow simulations*" by "simulations of the snow cover"

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- p. 584, line 20: "*strong decrease of snow cover*"
  Decrease of what? Area? Height?
- P. 585, line 8: replace "*Past evidences*" by "Evidences of significant changes in real avalanche data series over the last 60 years"
- p. 585, line 12: "**to** *our knowledge*"
- p. 585, line 24 & p. 591, line 3: replace "*Grounding*" by „Based"
- p. 585, line 27: replace "*also*" by "then"
- p. 588, line 7ff: "*the total snow depth, the thickness of surface wet snow and the thickness of surface recent dry snow*"
  Please use a consistent terminology (see ICSSG) => "*Height*" and "*depth*" refer to vertical measurements while "*thickness*" refers to measurements taken perpendicularly to the slope. "*Total"* is not necessary here. Use the same terminology consistently throughout the paper. Personally, I would use "depth" for all three terms.
- p. 588, line 12: "*0.01 %*"
  Are you sure it is not 1 %? Is it by volume or by mass?
- p. 588, line 12: "*The thickness of the surface recent dry snow*"
  Let's call it "The depth of recent dry snow" (see above)?
- p. 588, lines 14-15: "*Natural snowpack instability through the MEPRA index which gives information of the avalanche hazard without being certain that a triggering actually occurred*"
  What about "The MEPRA natural stability index, which is a proxy for avalanche danger. It does not tell, however, whether spontaneous avalanches actually occur."
- p. 588, line 24-25: "*avalanche susceptibility*"
  Define or replace by, for example, "potential avalanche release"
- p. 589, line 1: "*weak instability*": what do you mean? Please explain or reword.
- p. 589, line 12: "P": lower case?
- p. 590, line 25: replace "*similar*" by "equal"
- p. 592, line 15: replace "*successively*" by "in turn"
- p. 592, line 16: replace "*it, and it is*" by "the removed year, which is"
- p. 592, line 20: replace "*during the*" by "with this"
- p. 592, line 26: replace "*correctly able to predict*" by "able to predict correctly"
- p. 592, line 28: replace "*relative roughness*" by "crudeness". Relative to what?
- p. 593, lines 2-3: "*different avalanche triggering contexts*"
  What about "different types of avalanche cycles"
- p. 594, lines 10-11: "*independent year which can exist under considered climatic period*"
  Not clear to me. Do you mean you initialize each yearly run of ALADIN anew with ARPEGE BC data?
- p. 596, lines 5-6: "*the magnitude of the model correction is small*"
  Is it possible to quantify '*small*'?
- pp. 596 & 597: All variables should properly be written as Y_t and not *Yt*
- p. 598, line 26: replace "*regarding*" by „with regards"
- p. 599, line 2: replace "*time scales*" by "time periods"?
- p. 599, lines 21 & 25: "*a ~ +60/75 % mean increase*": does this mean "a mean increase of about 60 to 75 %" ?
- p. 601, line 1: replace "*Total snow depth*" by "Snow depth"
- p. 601, line 23: "*Wet and fresh snow depths*": OK, now we have depths, but ‚*fresh*' could be ‚*wet*' too!
- p. 606, line 1: replace "*melting*" by „wet"

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- p. 607, line 3: "are shown **in** Fig."
- p. 607, line 4: replace "*whole Alps scale*" by "French Alps as a whole"?
- p. 607, line 9: replace "*respectively in green and red*" by "in green and red, respectively".
- p. 608, line 1: "*snow cover decrease*": do you mean "decrease in height of snow / snow depth"?
- p. 609, line 5: replace "*seems*" by "seem"
- p. 611, line 8: replace "*later*" by "delayed"?
- p. 611, line 11: "*clear with*" ?
- p. 611, line 15: replace "*ith*" with "with"
- p. 611, line 16: "*snow extensions*": what do you mean by "*extensions*"?

Tables

- p. 623, Table 5: The proper abbreviation for snow depth is HS (see ICSSG). Use H_{ws}, but not D_{ws} for TWS and H_{ds}, but not D_{ds} for TSRDS.

Figures

- Fig. 1: It's a detail. But it would be appreciated if you could change the Kms (kelvin metre second) at the end of the scale by km.
- Fig. 2: Maybe add that the cross validation is for the reference period.
  The axis titles and labels are hardly readable unless you zoom in at infinitum!
  The same applies to almost all other figures too.
- Figs. 2, 3 & 12: I'd prefer to see (a) at the same scale than (b) & (c), even so the common legend may have to be located in a fourth panel (or integrated in the caption)
- Fig. 4: change "*respectively large and thin bars*" to "large and thin bars, respectively". This will apply to all figures of this type. Watch out for this error in the text too!
- Fig. 12: Unless you zoom-in excessively, the various curves are hardly distinguishable. Did you try with dotted and dashed lines?

References

- p. 612: "*Ammann & Bebi*"
  Please use a correct citation. The text points to SLF Davos, 2000 (p. 585, line 2)
  => SLF (WSL Institute for Snow and Avalanche Research SLF) Davos: Der Lawinenwinter 1999–Ereignisanalyse, Davos, SLF Bibliothek, 588pp., 2000.

Charles Fierz, 2014-05-10