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Interactive comment on “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 H. Castebrunet et al.

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Thanks very much for your interest and comments. Concerning technical corrections, we took them into account (see revised text). We'll just detail here the discussion points. Please find our answers for each of your points.

(a) 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, how-

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ever, whether spontaneous avalanches actually occur.” Indeed, to my knowledge, MI is mainly based on 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?

==> The idea of the CI is to use both information (MI and avalanche counts, taking into account their uncertainties), using them as complementary indicators of avalanche activity. Then, acknowledging the limitations of those indices, we used them. The goal of our study is not to model the response of avalanche activity to short term meteorological situations (e.g. multi day intense snowfall). We clarified and added p. 7 l. 13-21: “It appeared that the explanation may be that averaging over large areas and relatively long periods smoothes the signal, switching from meteorological and snowpack control at the daily scale to seasonal characteristics of the latter, making it possible to capture the predominant factors for the long-term interannual evolution in a more climatological sense with simple statistical regression models. On the other hand, the approach loses the information related to the succession of short term meteorological situations (e.g. multi day intense snowfall) interacting with a few massifs, except from the perspective of their contribution to the annual/seasonal mean. Hence, the approach is adapted to investigate seasons of high/low avalanche activity over large areas, but not for more localized 1-7 day episodes of highest activity.” The “surprising result” that you mentioned p. 590 l. 7 is surprising because the avalanche activity is known to have a non-linear response to meteorological conditions. We clarified p.7 l. 11-22 : “This was a rather surprising result given that the avalanche release process is a strongly discontinuous response to meteorological patterns and changes in snowpack charac-

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teristics, so that a weaker and/or non linear relation was expected for sub-seasonal and seasonal scales. It appeared that the explanation may be that averaging over large areas and relatively long periods smoothes the signal, switching from meteorological and snowpack control at the daily scale to seasonal characteristics of the latter, making it possible to capture the predominant factors for the long-term interannual evolution in a more climatological sense with simple statistical regression models. On the other hand, the approach loses the information related to the succession of short term meteorological situations (e.g. multi day intense snowfall) interacting with a few massifs, except from the perspective of their contribution to the annual/seasonal mean. Hence, the approach is adapted to investigate seasons of high/low avalanche activity over large areas, but not for more localized 1-7 day episodes of highest activity. See Sect.4 for further discussion about spatio-temporal scales.”

(b) “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.”

==> Clearly, our approach does not provide final conclusions about future avalanche activity evolution in all mountain ranges! More modestly, it is a case study whose conclusions may well be somewhat site-specific, even if the physics which is in the snow cover modelling should be the same in other areas. This should be clarified with other studies in other areas, for which the methodological framework we propose in this study could be beneficial.

(c) 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)” ??

==> Indeed, this was unclear. We now write p.4 l. 2-6: “They both suggested an

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ongoing increase in the proportion of wet snow avalanches with regards to dry snow avalanches, and a shift in their timing. This is consistent with already existing field observation of snow cover wetting at small scale and its link with wet snow release susceptibility (Mitterer et al., 2011), but without a clear quantification of how this correlates to the amplitude of change in total avalanche activity.”

(d) p. 587, line 10: “no error-free modelled series” What are those? Can you elaborate on this?

==> “Error-free” means without all the errors detailed before: missing events, wrong zeros, ...). We reformulated p. 5 l. 6-8: “For instance, there does not exist any local series which can be considered fully error-free with certainty. As a consequence, homogenization methods (e.g. Caussinus and Mestre, 2004) are difficult to implement and were not used in this study.”

(e) 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?

==> Yes, only MI is aggregated at the massif scale. We clarified p. 6 l.5-10: “The MEPRA natural snowpack instability index which is a proxy for avalanche hazard (Giraud, 1993, Durand et al., 1999). MEPRA is a diagnostic tools assessing snowpack stability based on Crocus simulated snow stratigraphy. MEPRA outputs, which are computed within each massif for each slope, altitude and aspect classes, are aggregated at the massif scale thereby providing a single scalar value for a given date. This aggregated MEPRA index, called hereafter MI, varies between 0 and 8 (8 being the higher instability level) dependent on both the SAFRAN-Crocus inputs and the characteristics of each massif.”

(f) p. 591, line 15 to end of section 2.2: Does this not rather belong to the discussion?

==> As regression models and CI were already calculated by Castebrunet et al. (2012)

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even if snow and weather data have changed, we did not consider those data as new results rather than existing data used and modified here.

(g) 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?

==> You are right, interpreting which variables are picked up by an automatic selection procedure among a large set of potential, highly correlated, covariates is quite difficult and can even be dangerous. This is largely discussed in the Castebrunet et al. 2012 paper where the method is introduced. Here we chose not to propose this discussion again with details since it is not by far the main point of the paper. We just remembered in Sect 4 “Beyond the questions of the choice of the GCM-RCM chain and of the SRES scenario, numerous uncertainty sources must be kept in mind while considering our results. Those related to snow and meteorological simulations in mountainous environment are detailed in Rousselot et al. (2012), while those specifically linked to the composite index and the linear regression approach are discussed in Castebrunet et al. (2012) Åž. By the way, note that here, regarding variable selection for the models in the past, we go quite fast, also to avoid over-interpretation.

(h) p. 593, line11:“12 km resolution” That is, about 25 % in area of a typical ‘massif’!

==> The downscaling of ALADIN RCM meteorological variables to the SAFRAN geometry is not carried out using ALADIN surface fields; in such a case, the spatial resolution of the RCM would be critical in the downscaling procedure employed given the complex topography of the French Alps which cannot easily be resolved in a RCM. Instead, as explained in the original and revised manuscripts, higher atmosphere synoptic-scale pressure fields from ALADIN are primarily used to determine for each date in the model run an analogue date from the SCM-ERA40 reanalysis, the meteorological conditions are which are considered the downscaled meteorological conditions (same date for the entire French Alps). This method thus employs only synoptic scale fields and relaxes

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the need to explicitly bridge the horizontal resolution of ALADIN with the spatial scale of the SAFRAN massifs.

(i) 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?

==> We are sorry but after verification, we used here a method slightly different than Giraud et al. (2013). All the part describing the method has been rewritten. See modifications p. 10 l. 10 to p. 11 l. 25.

(j) p. 598, lines 24-25:“snow conditions on slopes“ Do we know which conditions you considered on what slopes? I may have missed it.

==> Yes, it is explained p. 5 l. 28 (Sect. 2.1). We consider here a 40° slope.

(k) p. 599, line 20: Your “annual season” is not even a full year. Misleading?

==> Our “annual season” is in fact an “annual avalanche season”. July and August are not considered but we consider that a really very weak avalanche activity occurs during those months at our latitudes. See p.7 l. 4-7.

(l) p. 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.

==> Of course, the Crocus model integrates components of the full energy balance to calculate the composition of the snowpack. We discuss here only the features of the selected meteorological and snow variables used for the regression models.

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(m) 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.

==> Our experience is that the spatial smoothing is well when it allows highlighting the coherence between the different measures of avalanche activity. When MEPRA instability indexes evaluated locally are compared to very local avalanche observations, results are often deceptive, because of the limits of the two types of data/approaches, but also because even in case of high instability, a non-avalanche event remains much more frequent than an avalanche event!

(n) p. 608, line 13:“temperature increase interacting with topography” Can you precise what topographic features you are thinking of?

==> Mostly the altitudinal distribution within the massifs. This is now much clearly stated in Sect. 3.2.1 p. 17 l. 15-25 : “Since the snow and meteorological variable analysis has shown that, at constant altitude, latitudinal gradients (north-south location within the Alps) have little effects on projected changes, these distinguished north/south pictures may be attributable to altitudinal effects. Indeed, several massifs in the Northern sub-region (“Pre-alps”) have a lower altitudinal distribution, with their highest summits in the 2000-2300 altitude range only (Figure 1). In these Pre-alps massifs, avalanche activity is strongly reduced under climate warming by less abundant snow precipitation and the subsequent snowpack decrease, during the full year and even in winter. This induces a weaker but apparently still significant reduction for the whole Northern French Alps. On the contrary, in the southern massifs of higher homogeneity in terms of elevation, wetting induced by warmer conditions of the still important high altitude snowpack in winter leads to more wet snow (Figure 7) and therefore more wet snow avalanches in addition to the always possible dry snow releases (at high altitude, dry snow depths remain significant, Figure 8).”

(o) p. 618, Table 1: French Alps, spring Can you explain the N & E wet snow vs dry

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snow contrast? It seems counterintuitive!

==> No, we do not have any obvious explanation, except that this may be related to the limit of the automatic variable selection procedure, as detailed in (g).

(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. 588, line 12: “0.01 %” Are you sure it is not 1 %? Is it by volume or by mass? 588, line 12: “The thickness of the surface recent dry snow” Let’s all it “The depth of recent dry snow” (see above)?

==> We clarified and completed the paragraph p. 6 l. 28 to p. 7 l. 4: “For the four main aspects (northern, eastern, southern, and western) and 40° slope, the snow depth, the thickness of surface wet snow and the thickness of surface recent dry snow. These variables are derived from outputs of the detailed snowpack model Crocus fed by SAFRAN meteorological conditions (Brun et al., 1992). The thickness of surface wet snow is defined as the sum, starting from the top of the snowpack downwards, of the vertical component of the thickness of the contiguous wet snow layers characterized by a liquid water content greater than 0.5% by volume. The thickness of the surface recent dry snow is defined as the vertical distance between the snowpack surface and the deepest snow layer characterized by a dendricity greater than 0.25. The threshold expressed in terms of dendricity (Brun et al., 1992) ensures that the considered snow layer still features characteristics of precipitation particles or decomposed fragments (Fierz et al., 2009), and accounts for the impact of snow metamorphism on snow layers in a more consistent way than relying only on snow age, because the rate of transformation of snow properties strongly depends on temperature, temperature gradient and the occurrence of wet snow conditions, which is explicitly considered in Crocus and thus captured in our definition of surface recent dry snow.”

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(q) 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?

==> ARPEGE provides limits conditions for ALADIN. Each yearly run of ALADIN corresponds to ALADIN downscaling of a yearly run of ARPEGE. We tried to simplify the sentence p. 10 l. 13-15: “The reference period (called EM6) is a continuous ALADIN simulation between 1961 and 1990, whereas both future climatic periods 2021-2050 (called EM7) and 2071-2100 (called EM9) are simulations consisting of 30 independent yearly simulations.”

Interactive comment on The Cryosphere Discuss., 8, 581, 2014.

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