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The Cryosphere Discussions

Interactive comment on "Statistical adaptation of ALADIN RCM outputs over the French alpine massifs – application to future climate and snow cover" *by* M. Rousselot et al.

Anonymous Referee #1

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Review

Statistical adaptation of ALADIN RCM outputs over the French alpine massifs – application to future climate and snow cover

by M. Rousselot et al.

GENERAL REMARKS

The paper of Rousselot et al. presents temperature, precipitation and snow cover scenarios for the French Alps as derived from a suite of experiments of the regional climate model ALADIN. A statistical downscaling procedure based on an analogue

method and using a high-resolution analysis of the SAFRAN model as well as the CROCUS snow model is applied to the RCM output.

I consider the study as novel and as valuable to the scientific community, both concerning statistical climate downscaling and climate impact assessment. It is suited for the readership of the journal. The paper is well-structured, the language is rather clear and precise (although the English could still be improved). Most of the conclusions reached are supported by the presented results. However, the current version of the paper suffers from some methodological drawbacks and some deficiencies in the interpretation of the results obtained. The results are not put into the broader context of existing studies dealing with future European and Alpine climate change. Also the graphical presentation of the results in the figures should be improved.

Altogether, I could recommend a publication of the study, but only after a number of substantial revisions. These revisions seem to be feasible within a limited amount of time. Given the relevance of the subject I'd certainly encourage the authors to work on their paper and to present a revised version acceptable for publication. Please find below a listing of major points that should definitely be improved as well as a list of suggested minor revisions.

With kind regards.

MAJOR POINTS

Description of analogue method in Section 2.2: The description of the method applied should be improved and clarified. There are some open questions remaining. For instance, what is the spatial resolution of the grid on which the analogue method is performed? The grid is shown in Figure 1, but the grid spacing is not mentioned. Similarly, please also mention the grid resolution of ERA40 and the ARPEGE analysis. The latter two models probably provide data on a coarser grid than ALADIN, but are interpolated to the same analysis grid. The effect of the differing grid resolutions in ALADIN on one side and in ERA40 / ARPEGE on the other side should be discussed.

The ALADIN output probably contains much more regional detail which makes the search of analogous days in ERA40 / ARPEGE difficult unless the analysis grid is close to the coarser ERA40 / ARPEGE grid and regional details in the ALADIN results are smoothed out by the interpolation procedure. A further open question is whether the analogue method is applied separately for different seasons / different times of the year (which would be advantageous) or for the entire year. I.e.: Is the search for analogue days only carried out in the same season or based on the entire year? On page 177 line 8 "seasonal anomalies" are mentioned which suggests a seasonal analysis, but this point remains unclear.

page 177 lines 18-22: It is obvious that the different topographies of ERA40 and ALADIN have to be considered when comparing 2m temperatures in the analogue method. But this should be done BEFORE the analogue search is applied. The a-posteriori correction of the finally obtained analogue fields, as mentioned by the authors, will correct biases in the final climatology but will not guarantee a consistent search for analogue meteorological conditions. The authors need to comment on this point and need to clarify their methodology.

Search of analogue conditions in the scenario periods: It can be expected that especially in the later ALADIN scenario period (2071-2100), meteorological conditions will appear that are not found in the ERA40 re-analysis due to, for instance, much higher temperatures. This would imply that the minimum distances (Eq. 1) would generally be larger for the scenario periods than for the control period. I'd very much appreciate if the authors could present a figure showing the mean distances for the three periods (1961-1990, 2021-2050, 2071-2100). In case that distances are indeed larger for the scenario periods there's some danger that the climate change signal will be underestimated by the analogue method (for example, the extremely warm conditions in a scenario climate might not be found in the D09a analysis and the analogue day with the minimum distance would still be far too cold). The authors should then comment on this issue. It might help to include a comparison of regional scale temperature

changes simulated by the analogue method (e.g. averaged over the entire French Alps) with temperatures changes actually simulated by the ALADIN experiments. These two should roughly agree with each other.

page 179 lines 13-14: I wouldn't support this statement. I'd rather say that the validation presented evaluates the representation of weather types in the ALADIN control experiment. If the circulation in ALADIN control is strongly biased, also the weather types derived from the SAFRAN fields resulting from the analogue method applied on the ALADIN control output will have important biases when compared against D09a. Please clarify this point.

page 181 lines 13-17: Based on Figure 5, the CT climatology is WETTER than D09a, not drier. Have the axis labels in Figure 5 been mixed up? I guess so, since also Table 2 reveals an underestimation by CT. Please clarify and either correct the figure or the text section and the table.

Snow cover validation in Section 3.3: As the downscaled meteorological fields are corrected by the q-q-method a-posteriori based on the same time period 1961-1990, it is not very astonishing that the derived snow cover simulated by CROCUS is in a rather good agreement with D09b. A proper validation of the entire downscaling method (including the bias correction by the q-q-method) would split the period 1961-1990 into a calibration period in which the parameters for the q-q-method are derived and a validation period in which the calibrated correction functions are simply applied (preferentially in a split-sample cross-validation framework) and snow cover is evaluated only for that validation period. Such an analysis would strongly enhance the confidence into the method. I think it is not absolutely necessary to include such an analysis in the paper, but it would increase the quality of the validation exercise considerably. The authors should think about it.

Analysis of surface temperature changes in Section 4.2: It would be very valuable if the larger-scale (entire French Alps or large sub-domains thereof) 2m temperature changes obtained by applying the downscaling methodology could be compared against the temperature changes that are actually simulated by ALADIN on its 12km grid. On a larger scale these changes should agree with each other. If the authors could show such a comparison the confidence into the downscaling scheme would be raised and concerns regarding the finding of analogue conditions in a future climate (see comment above) would be less severe. In principle, the same also applies for the analysis of precipitation changes in Section 4.3.

Analysis of temperature and precipitation changes in Sections 4.2 and 4.3: The findings of the presented study are not related to previous works on future climate changes in the Alps and in Central Europe. Quite some studies on this topic exist (most of them based on the PRUDENCE and ENSEMBLES experiments), and the results obtained should be put into that broader context (agreement / disagreement with previous works?).

page 185 lines 6-9: I disagree with this statement. What the authors show is a comparison of RELATIVE precipitation changes between the control and the scenario periods. Even if these changes are constant, they could still result in changes of the gradient of ABSOLUTE precipitation in the study area (which is the standard unit for precipitation gradients). Please rethink this paragraph and reformulate.

Analysis of snow changes in Section 4.4: It seems that a discussion of the reason for the strong elevation dependence of snow changes is missing. The temperature change signal is similar at all altitudes. The authors should discuss why, nevertheless, relative snow cover changes or stronger at low altitudes and in the southern parts (lower temperature level, shorter snow season, etc.).

page 187 line 5-6: It is not clear a-priori that the bias correction by the q-q-method conserves inter-parameter consistency. Apparently, the method is applied separately for all driving parameters of the CROCUS snow model. If the authors want to claim that inter-parameter consistency is conserved by their approach, they need to show it.

page 187 lines 20-22: I might have missed it, but right now it is not clear to me where this has been shown. A comparison against the direct ALADIN outputs (as suggested above) has not been carried out. Please better clarify this point.

page 187 lines 24-29: This statement is certainly true. However, the study presented does apparently not make use of the full 12 km resolution of the ALADIN RCM. The common grid on which the analogue method is carried out seems to be much coarser (see Figure 1). Please specify the grid resolution and consider reformulating this paragraph.

Table 1: This table seems incomplete and doesn't correspond to the text section 2.3. Please specify the meaning of rows and columns and check the entries.

Figure 1: This figure definitely needs to be improved. It is not clear if this figure shows the ALADIN RCM domain or only a part thereof. Please specify. Furthermore, a horizontal scale is missing. Also the resolution of the analysis grid (black dots) should be specified either in the text or in the figure caption, ideally in both. The four sub-domains North, Central, Southern and extreme Southern Alps are referred to in the text but cannot be identified from Figure 1. I suggest to colour the individual massifs according to the sub-domain.

Figure 3: A legend (meaning of the dark gray and light gray bars) should be added. In panel d) the light gray bars (CT experiment) do not add up to 100%. What's the reason for this? Are there unclassified days in the weather classification scheme? Please clarify.

Figures 4 and 5: These figures are too small and hardly readable, their size should be increased. In general, I'm wondering how useful the correlation coefficient is as validation measure in a q-q-diagram. As quantiles are shown, the correlation will probably always be strongly positive. Does the literature present any other metric that is more useful? If so, please consider of switching to another metric. In Figure 5, the axis labels seem to be mixed up (see comment above).

Figures 8, 9, 10, 11, 12 and 13: The markers in these figures are too small and hardly recognizable. Their size should be increased. Furthermore, it is not clear how the standard deviations on which the error bars are based have been computed. Is it the standard deviation of the 30 individual annual climate change signals (individual years in the scenario period with respect to the 30-year mean in the control period?). Please specify this either in the method section or in the figure captions. In Figures 9, 11 and 13 a legend (indicating the meaning of the colors) would be helpful. In Figure 9, the panels have been mixed up in the caption (Northern Alps are for instance shown by panels a and e, not by panels a and b). Also in Figure 10, the caption has to be adjusted (the panels of the right row are missing in the description). The legends in Figure 12 are too small as well, and the rows (B1, A1B, A2 ?) and columns (change 2021-2050, SD 2021-2050, change 2071-2100, SD 2071-2100) need to be labeled for clarification. In all Figure captions "A1" needs to be replaced by "A1B".

MINOR POINTS

page 171, title: I'd suggest to replace "French alpine massifs" by "French Alps", which is as informative but shorter and better suited for a paper title.

page 172 line 27 to page 173 line 1: With "local changes in climate" the authors probably mean potential feedbacks of snow cover changes on, for instance, temperature changes. If so, please make this point more clear (e.g. mention the snow-albedo feedback).

page 173 line 3: "Long term climatology" should be replaced by "Long term snow cover climatologies" to make clear which parameter is referred to.

page 173 line 23: "150-300km" instead of "300-150km".

page 174 line 20: "25-50km" instead of "50-25km".

page 174 line 21: "van der Linden and Mitchell" instead of "linden and Mitchell".

page 174 line 24: The study of Haylock et al. is mainly concerned with the setup

of a gridded observational dataset for RCM validation and not with RCM experiments themselves. The citation at this place is somewhat misleading.

page 175 lines 1-10: The objectives of the work should be better clarified and explicitly mentioned.

page 176 line 5: "output of several recent" instead of "output of the recent".

page 176 lines 5-12: It should be clarified on which domain the RCM ALADIN was run. It's probably not the entire European continent, and probably it's also not the domain shown in Figure 1.

page 177, line 9: In case of ALADIN, isn't the method also applied to the scenario periods 2021-2050 and 2071-2100?

page 178 lines 3-4: This statement is not correct as also time series representing the current control climate (1961-1990) are treated.

page 181 lines 10-12: In my opinion, the most likely reason is a bias already in the driving ARPEGE experiment (boundary conditions for ALADIN), which penetrates through the RCM. Could the authors comment on this?

page 183 line 5: I'd suggest to replace "gradient" by "dependence" or "dependency" here since, formally, no gradient has been calculated. The same applies to page 185 line 5.

page 183 lines 15-17: This sentence is not clear to me. Please reformulate.

page 183, line 19: The increasing north-south gradient cannot be derived from Table 2 as suggested here. Also: doesn't this statement contradict the statement in the beginning of the paragraph ("temperature changes . . . seem relatively uniform from the Northern to the Extreme Southern Alps")? Please clarify.

page 184 line 2: I'd suggest to replace "anomalies" by "changes". The same applies to page 184 line 7.

page 184 lines 26-27: The main reason for this unclear picture is the internal variability of precipitation which is probably still larger than any climate change signal by 2021-2050. This issue should be mentioned and briefly discussed.

page 184 line 8: What's the meaning of "significant" here? Apparently no significance test has been carried out. I'd therefore suggest to replace "significant" by "clear" or a similar expression.

page 185 line 25: I'd suggest to replace "in the SWE reduction" by "in the relative SWE reduction" since this is shown in the figure and conclusions for absolute changes could be different.

page 186: What's the meaning of "two distinct ERA40 datasets" here. Please clarify.

page 189 lines 1-3: This conclusion would be stronger of the authors could add an analysis of changes in the TIMING of the snow cover period (in addition to an analysis of SWE changes).

page 190 lines 6-7: I'd suggest to replace "highly sensitive to altitudinal gradient" by "strongly dependent on altitude".

Figure 7: I'd suggest to replace "a" in the figure legend directly by "2021-2050" and "b" by 2071-2100. The current version is rather cryptic.

Interactive comment on The Cryosphere Discuss., 6, 171, 2012.