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Interactive comment on "Regional scenarios of change over Canada: future climate projections" *by* Zilefac Elvis Asong et al.

Anonymous Referee #2

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General comments:

In this paper, the authors are trying to develop climate change projections based on a subset of CanESM2 simulations, downscaled with the CanRCM4. The authors decided to use 15 simulations out of the total 50 available. The rational for using a 15-member sub-ensemble has not been discussed in the paper. When it is possible to consider all 50 members and when other researchers are evaluating all of them, it does not make any sense to consider just 15 members. It was a different case if the authors were performing an innovative study to solve an important scientific problem and they needed only a short ensemble. No discussion is available on the considered ensemble. Some information on the type of the ensemble considered and how this ensemble was created and what uncertainty aspects the ensemble was expected to address

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were absolutely required in the paper. In addition, some general information on the CanRCM4 was also required. Certain readers like to know such information about the regional model used.

Any new paper is expected to make some scientific contributions in terms of innovative methodologies, new data analysis techniques, solving an unsolved problem, advancing knowledge on specific topics, improving existing understanding on eminent issues, etc. This paper does not make any contribution on any of these aspects. A few selected climate indices related to some aspects of precipitation and temperature fields have been used to derive projected changes for two 30-year future time periods (2030s and 2080s) based on the 15-member ensemble. All results are presented in the form of percentage changes with respect to the 1979-2008, reference period. The benefits of considering an ensemble of simulations have not been explored at all. Furthermore, no attempt has been made to evaluate future changes in the light of the spread provided by the 15-member ensemble.

The title of the paper is misleading. Some analyses are performed and shown for the entire country, while some others are shown for two large Canadian river basins (Mackenzie and Saskatchewan), located in western Canada. The results of the analyses are not discussed for any other region of Canada. Thus, it is not appropriate to misguide readers of the HESS journal by providing an attractive title and knowing that the paper does not provide information on regional changes for the entire country.

None of the changes in selected climatic fields reported in the paper have been statistically validated. There is not even a single mention of a statistical test in the paper. In fact, the paper was dealing with a sensitive and serious issue so it was important to evaluate if the projected changes in the selected climatic indices, presented in the form of percentage changes, fall outside or inside the range of natural variability.

It is stated in the paper that the analyses are targeted for water resources applications and to support CCRN hydrological models. Contrary to this target, selected tempera-

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ture related indices bear little or no significance for water resources and hydrological modelling.

The authors have made several general statements in the paper, without paying attention to their implications. There are several lapses in the analyses and how the changes were produced and interpreted. The usage of readymade software programs has also led to many misconceptions. Several similar studies that have been performed in the past for North America have gone much beyond what is presented in this paper even using a small number of climate model simulations. In addition, the authors did not make an attempt to compare and evaluate their results in the light of previously published papers, reports and technical documents. All of this and several other points of serious concern that have resulted in a sub-standard paper are elaborated below under specific comments.

Specific comments:

[Page Number(s), Line Number(s)]

[2, 43-58] The abstract is based on what is presented in the paper. Therefore, it is more appropriate to comment on the paper as the comments can be related back to the abstract.

[2, 59] Keywords are not so persuasive.

[3, 77] What do you mean by scale-relevant? This sentence must be accompanied by an explanatory sentence. The concept of scale is not the same across different disciplines and scientific problems. Here, this is in the context of climate change and water resources so the concept of scale must be explained.

[3, 83] Do you think, water resources are driven by glaciers in all cold regions? Numerous watersheds in cold regions do not contain glaciers.

[3, 84-86] This sentence needs to be justified in the context of global hydrologic cycle with appropriate references. How can headwater supplies from the Rocky Mountains

impact global hydrological cycle? Some discussion is needed. Provided references appear to be random and have no relation with the global hydrologic cycle.

[3, 86-90] This long sentence can be rephrased. The statement "negative consequences" requires additional elaboration. Across the globe, there are also some regions that have been found sensitive to rising temperatures but those regions are not snow dominated (have a closer look at the IPCC report that has been referred to in the manuscript).

[3, 91-93] The studies referred to did not prove that the hydrologic cycle has become intense because of past warming trends. "Ongoing science. . ." is a big statement, but without sound references. Do you think there are signs to support the notion that the hydrologic cycle has already been intensified in North America? If yes, please provide appropriate references.

[3, 93-94] Burn (1994) did not discuss that shifts in stream hydrographs have occurred. How this information was obtained from the cited paper? Is it your own interpretation?

[4, 95] None of the referred to studies was able to prove that shifts in temperature and precipitation regimes were due to climate change.

[4, 95-98] From the three studies cited here, second and third study did not prove that earlier breakup of seasonal freshwater ice cover is associated with the warming trend. Cause-and-effect type studies or attribution type studies were needed to investigate this phenomenon. Latter type of studies did not exist at the time when the studies that have been referred to were conducted.

[4, 100-101] This sentence is applicable only when summer flows are drawn from glacier melt and snow storages. The conceptual understanding for reduced summer flows due to indicated mechanisms driven by enhanced warming cannot be generalized.

[4, 101-104] Which part of the globe will suffer from more frequent and severe

droughts? Are you referring to just cold regions or the whole world? Please avoid making general statements.

[4, 105-107] Not all of the studies referred to in the above paragraphs were related to cold regions. What about the studies by Arnell (1999) and Bates et al. (2008)? Do you think cold region processes and mechanisms were evaluated in these studies in the context of climatic change? Can the authors further elaborate on the usage of "uncertain climate futures" in this sentence? In the presence of mere natural variability, many natural systems are flexible enough to adjust by themselves. However, one has to be cautious when a persistent change is taking place gradually or by means of abrupt mechanisms.

[4, 107] This sentence is technically not correct. Climate projections are created with ESMs when these models are run under prescribed emissions scenarios.

[4, 111] This is a wrong statement. Climate change projections do not provide a glimpse into possible future water resources impacts and challenges. Climate change projections are translated into water availability indices and then those indices are used to study climate change impacts on global and regional water resources.

[4, 111-114] This statement is not totally correct. It depends on the type of adopted methodology. For example, if one is satisfied with the simple delta method, which is quite straightforward to apply, then there is no need to perform bias correction. As historical observations are not available at every point of interest, how will you bias correct your model outputs at points where observations are not available? Bias correction is a debatable point and also the methodologies are not free of disadvantages.

[4, 115-117] In the first paragraph on page 3, authors promised to study cold regions water resources. Here, it is announced that future changes in precipitation and temperature characteristics will be analyzed, without even mentioning those characteristics. It is important to be consistent throughout the paper.

[5, 123-124] WFDEI is a data product and it is not an observational data set. However, when discussing bias correction on page 4, lines 112 to 113, historical observations are indicated. How a data product can be considered as historical observations data set? There is a clear contradiction in the statements.

[5, 133-138] You need to rephrase some parts of these sentences. According to these sentences, only SRB and MRB are natural systems. What about Great Lakes and Saint Lawrence River system? Do you think this is not a natural system? Overall, this text understates the presence of many other important large river systems, located south of latitude 72 degrees north.

[5, 138] How come the Prairies of Canada came up here suddenly? There should be a systematic transition in the text.

[5, 133-141] Introduction to the study area is surprisingly small. Was it kept small deliberately? Were there any specific reasons behind this?

[6, 143] Are you correcting all climate model outputs? On page 4, last paragraph, only precipitation and temperature are indicated.

[6, 146] Forecasts of the GEM model are mentioned here. How come forecasts from the GEM model can be used to bias correct climate model outputs? Forecasts are future predictions for a targeted time period and are generated without reference to observations because observations are not available for a future period.

[6, 150-153] There is no terminology like GEM-CaPA in use in Canada. The precipitation product is simply known as CaPA. You are not bias correcting whole ESM climate, i.e. the entire output of the global climate model. You are only correcting CanRCM4 downscaled temperature and precipitation outputs. Looks like the authors are totally confused here.

[7, 167] CanRCM4 is not a new regional model. Please verify from the sources quoted in the manuscript. It was useful to have included some technical information about this

model because some readers are interested in knowing general information on various processes and their respective parameterizations. This could be outside the expertise of the authors of this paper. Nonetheless, it is readily available in many published documents. The authors have already referred to a few of these studies wherein this information is available.

[7, 176-177] It is very hard to believe that the authors had access to only 15 ensemble members out of the 50 available. Some of the authors of this paper had access and privileges to use all 50 members. Using just 15 members does not add any value to the analysis since many similar studies had already been completed and published in the past. The large CanRCM4 ensemble was produced purposefully to address various sources of uncertainties and to derive some useful indices of change. In fact, lack of justification for using a subset of 50-member ensemble was correctly pointed out by the Editor at the time of paper submission. In response to that query, the authors of the paper did not provide a satisfactory answer.

[7, 178-180] This is an irrelevant sentence. What is the relationship of this study with the CCRN hydrological models? No hydrological model has been used in this study then what is the reason to worry about CCRN models. In addition, international scientific community has nothing to do with CCRN.

[7, 185-187] Is there any specific reason for using the nearest neighbor interpolation scheme for re-gridding CanRCM4 outputs to match the WFDEI specification? Why not other methods, e.g. bilinear interpolation? Literature based support is needed to justify the choice of this method. It was also possible to re-grid WFDEI product. Why was that route not followed?

[7-8, 187-193] Some information on MBCn is needed. Why is this method preferred over many others available? Did you consider long time series of individual variables for each month at each pixel? How did you make transitions from one year to the other? There are no historical data sets available as mentioned here. There are either

processed data products or climate model outputs as described in sections 2.2 and 2.3. Information on model fitting and validation parts is omitted completely. How the changes in quantiles were derived and how they were applied to all simulations including those for the future periods? What sort of dependence structure was found among the realizations of individual variables and across different variables? It is noted that dependence structure among variables was considered, but there is no discussion on the nature of the dependence structure.

[8, 195-203] Description for deriving seasonal changes from mean daily precipitation and temperature is missing. As described in the paper, continuous longer simulations were available then why was the analysis conducted for two different 30 year time periods? Justification for this choice is missing. Were you afraid of non-stationarity related issues that may become evident over longer time periods? Scientific community has invested heavily, both intellectually and economically, to produce longer RCM simulations to support continuous analyses. Why did you choose to use two non-overlapping time windows? This was the case many years ago when continuous simulations were not available due to computational constraints.

[8, 206-209] Why is it necessary to study extreme wet days with a return period of a year? Why is it necessary to use only the extreme value theory to study vulnerability of engineering infrastructure? Some additional information was needed to support these statements.

[8, 209-212] Can you please justify the choice of your method of analysis? How does the first method provide a broader context for evaluating hydrologic and water resources implications of climate change? Only 30 years of data is being used to evaluate various indices (specifically those related to extremal behavior) then how is it possible to derive robust estimates from such short samples?

[9, 218-219] It is not clear if the default thresholds implemented in the package are applicable for the entire country. An extreme value at location x may be a normal value

at another location y. Can you please discuss how the choice of default thresholds maintains spatial consistency and coherency among extremal indices across Canada?

[9, 219-220] Your analysis is targeted at two future periods. Do you think some of the analyses were not performed for these time periods when you mentioned "where applicable" in the sentence? Section 3.2 does not provide any detail contrary to the statement.

[9, 220-225] It is not just enough to say "following water resource-relevant indices" were further examined in the study. You need to comment on the practical utility of these indices. What are the implications of studying 5-day maximum precipitation? For what purpose this variable is used in water resources applications? What aspects of extremely wet days were studied? Nothing is mentioned about this. In what context the indices like TNn and TXx are useful? Why is it necessary to investigate future behavior of these indices? It is very hard to understand implications of changes in TNn and TXx for water resources applications. Certainly, there are implications of changes in these indices in other areas, e.g. public health.

[9, 231] You have listed three points, but your statement indicates two (i.e. the usage of "two dimensions").

[9, 235] There is nothing like unbiased CanRCM4 simulations. WFDEI is a data product that has been used for bias correction. Any bias correction based on a data product cannot generate totally unbiased climate model simulations. It seems like "wrong usage of the technical terminology in this sentence".

[10, 239-240] This statement is partially correct. What about the biases caused by inability of a regional climate model to simulate local land processes and various interactions? ESMs are not the only source of biases.

[10, 242] Abbreviations are not properly inserted.

[10, 243-245] Are you sure Figure 2a is a quantile-quantile plot of R1mm? Something

is seriously wrong with understanding of statistical concepts.

[10, 245-246] It is not clear how CDD and CWD were defined and evaluated in the study. What characteristics of CWD and CDD were evaluated? The CDD and CWD variables were not included in the description provided on page 9, lines 220-225. These variables needed to be listed there.

[10, 246-247] It is difficult to comment on this statement without knowing additional information about CDD and CWD and their respective characteristics.

[10, 247-249] If this statement is true then how is it possible to use WFDEI product for bias correction of model outputs in a satisfactory manner? Indirectly, this statement also tells not to have any confidence in results for areas north of 60 degrees north.

[10, 251] Same comment as for Figure 2a above.

[10, 251-253] What parts of the distribution of R1mm were shifted by the MBCn algorithm? Some explanation is required.

[10, 254-256] Useful statement, but how the wet-dry day physics will be impacted by bias correction is not discussed.

[10-11, 256-262] Are you showing results for selected ensemble members or for all in Figure S2? No information is included on the behavior of bias correction for different seasons. There could be seasonal dependencies in the behavior of model outputs. Lots of averaging has been done for the results shown in Figure S2. The differences between corrected and WFDEI values are not called biases. What statistical properties of model outputs were corrected by the MBCn algorithm? From the explanation provided, it appears that MBCn was able to correct the entire statistical distribution and that does not seem to be logical. Additional explanation will be useful.

[11, 267] Time series of “mean daily precipitation” are shown in Figure 3 and not “daily precipitation”. You did not attempt to bias correct entire set of climate variables. Please avoid making general statements.

[11, 275-276] If there is no difference between corrected and raw model outputs then what is the value of bias correction?

[11, 266-276] The results shown in Figure 3 indicate that model simulations (both raw and corrected) are not able to explain high temporal variability of WFDEI data. In that case, what is the advantage of bias correction? Bias correction means that the corrected outputs should closely match with some general features of the data used in bias correction for the same time period. For both river basins and for all seasons, the bias correction algorithm failed to capture high variability of WFDEI data product. This means that it is not possible to derive robust and reliable projections based on the analysis performed in this paper.

[11, 283-284] The numbers shown here are not consistent with those shown in lines 273-279. Please verify.

[11-12, 277-286] Spatial patterns of changes shown in Figure 4 are very similar for the raw and corrected model outputs. Is there any value in showing both results? The similarity issue also raises serious questions about the bias correction methodology.

[12, 288-294] Neither raw nor corrected model outputs are able to reproduce the variability and pattern of low and high temperature values of WFDEI data. Does this indicate total failure of the bias correction algorithm? The pattern of high variability of the WFDEI data clearly stands out for all seasons and for both river basins. Clearly the CanRCM4 is also not able to simulate the behavior of WFDEI data product. This means that it will be dangerous to rely on just one model outputs. Thus, an ensemble of regional and deriving global climate models would be required to derive scientifically meaningful projections.

[12, 300-302] Some eastern parts of the country also show similar patterns as presented here. Why were those parts not discussed?

[13, 309-320] The results shown in Figure 7 seem to be useless. All seasons show

increases in RX5day, with small changes in southern parts of the country, located mainly in Alberta, Saskatchewan and Manitoba. Results at the annual scale show deceases all over the country. This is totally unbelievable. Since the methodology and calculation procedures are not clearly explained in the paper, it is difficult to provide proper guidance. The results for the winter season suggest that there will be more intense snowfall events in the future. Is this consistent with the results shown in Figure 6? Also, is this outcome physically plausible?

[13, 321] Are you showing the results for extremely wet days or extremely wet day precipitation amounts? Please use the right terminology.

[13, 321-322] Can you please discuss implications of almost 200% increases in R99p for engineering infrastructure? You have mentioned this aspect earlier in the paper. In addition, can you also discuss the significance of this almost double increase in the light of results presented in previous similar studies?

[13, 325-328] In Figure 9, probability distributions are shown in the form of smooth curves. Obviously, limited length of simulations and WFDEI data cannot support such smooth curves. How were the authors able to come up with smooth curves? It is indicated that “large tails” are projected for the JJA period compared to other seasons. Large tails is not a correct terminology. “Longer tail” or “thick tail” is generally used in the literature.

[13-14, 328-331] Is there any practical utility of presenting these percentage changes in RX1day mean values? In general, useful return levels from design perspective are used to evaluate expected future changes.

[14, 331-333] For the SRB, you have mentioned mean changes while for the MRB, you have mentioned changes in the mean of RX1day. Which one is correct? Please be consistent. This situation arises when one is unfamiliar with the methodology that is being used and that most likely is the case.

[14, 335-340] Compared to other analyses, here the results are presented only for the 2080s. Was there any specific reason for skipping results for the 2030s?

[14, 340] This is quite a big number and is frightening as well. Can the authors discuss physical plausibility of this large increase in the TNn for the whole country? This may raise alarm bells for many regional temperature-sensitive societal domains.

[15, 363-365] This is not necessarily true. If known, systematic biases can be handled easily. However, it is difficult to preserve physical consistency among and across climate variables and to satisfy requirements of impact models through bias correction.

[15, 366-367] No discussion on this aspect is available in the paper.

[15, 367-369] This is not correct. Your own analysis provided in Figure 9 does not support this statement.

[15, 370-374] The usefulness of the bias correction algorithm has become questionable because it was not able to capture the inherent temporal variability of WFDEI-based precipitation and temperature observations (see Figures 3 and 5).

[16, 391-392] Please avoid making general statements. You have to specifically indicate which wet and dry extremes are expected to increase. Readers of the paper cannot make assumptions.

[16, 393-394] This is another general statement.

[16, 395-396] Can you compare this conclusion with those arrived at in other similar studies for the Canadian Prairies? There are plenty of studies available in the literature.

[17, 406-407] This sentence cannot overcome shortcomings of the analysis presented in this paper.

[17, 408] No serious attempt has been made to compare the results of this study with those available in the literature. For example, the authors did not refer to the Canadian Climate Change Report (https://changingclimate.ca/CCCR2019/) released by Environ-

ment and Climate Change Canada early this year. Surprisingly, some of the authors belong to the same organization and they had access to not only climate model outputs but also to the results obtained.

[17, 414-416] There are many studies that have been performed using data at the same 50-km resolution as used in this study over North America to study climate change over different spatial and temporal scales. A few studies that have been referred to in this paper where, according to the authors, coarse resolution data were used are not the only ones available in the literature.

[17, 419-423] A discussion on the results of this study and those of Wang et al. (2015) for Ontario is missing. The region studied by Wang et al. (2015) is also a part of the current study. This comparison could have shed some light on the ability of the CanRCM4 in simulating precipitation and temperature extremes.

[17-18, 424-428] These results from the literature suggest that it is very dangerous to depend on projections from just one climate model. Thus, the projected changes presented in this paper are subject to large uncertainties since these are derived based on just one model simulations.

[18, 428-431] A direct comparison between these published results and the ones obtained in this study had been interesting. This comparison had given the authors some confidence in developing projections based on one regional model simulations. The same region is also included in the current study.

[18, 432-442] It is surprising to see such studies on the analysis of extremes wherein only 15 years of data had been used. In the context of present study, it is useful to discuss how the level of projected changes in extremal indices compare with those presented in the two referenced studies.

[18, 413-448] It is good to see these comments. However, the authors were also expected to comment on the adequacy of the 15-member ensemble used in their study.

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This commentary is missing.

[19, 450-452] This statement is in contradiction to what was stated before, i.e. the study was conducted in relation to CCRN hydrological models.

[19, 452] Which relevant climate variables were obtained from CCCma?

[19, 452-454] Did you downscale CanESM2 outputs in this study? If not, which probably is the case, then please rephrase this sentence. CanESM2 outputs were downscaled by someone else and, most likely, you only obtained data via CCCma website or data portal.

[19, 454] No observations were used for model evaluation purposes. You only used a processed data product. Hence, please use correct terminology.

[19, 456] Bias correction algorithm was applied to CanRCM4 outputs. Observations are based on a processed climate data product and not on "climate product". Please make correct statements.

[19, 458] What types of extremes were studied? Please state them correctly.

[19, 462] Which mean precipitation will increase?

[19, 467] How come United Sates has been mentioned in this sentence? The authors did not do any analysis for the United Sates. Does this mean that the authors tried to make similar statements as were used in other published similar studies for the United Sates? It is very difficult to imagine such a mistake from some prominent researchers, included in the list of authors.

[20, 473-474] It appears that the authors have made this statement based on the behavior of plotted time series. However, no formal analysis of variability was conducted to support this conclusion.

[20, 478] Please specify the indices that reflect wet and warm extremes. As mentioned before, please avoid making general statements.

[20, 480] Here, there is an incorrect usage of technical terminology.

[20, 479-481] This is an incorrect statement/conclusion as it cannot be verified from Figure 9 (second row).

[20, 483-485] This statement appears to be in contradiction with what is stated in the first sentence of this bulleted paragraph #3.

[20, 485-486] This conclusion implies that the hydrological cycle in the SRB is not being intensified due to warming. Is it correct or not? Please add relevant explanation in the bulleted paragraph #3.

[20, 488] How do you know that CanRCM4 outputs contained systematic biases? No relevant analysis has been reported in the paper.

[20, 489] This sentence needs to be re-written. Biases were removed based on the WFDEI data product.

[20, 489-490] The analysis presented in the paper indicates that the climate change signal has been derived based on raw and corrected CanRCM4 outputs. The analysis does not reflect the impact of climate change signal based on the uncorrected and corrected data as indicated in this sentence.

[21, 498-506] This conclusion does not help in overcoming the inherent shortcomings of the study. Similar but more robust analyses have already been reported in numerous studies over North America. There was no point in repeating previous analyses. It had been wise to use outputs from many regional models driven by many global model outputs. Such outputs from numerous regional models are already available.

Figure 2:

Only the member that was plotted at the end is visible. The authors could have used spatial plots to demonstrate what they are trying to. Systematic biases seem to be disappeared. Since plots reflect the entire country, it was better not to use the word

"domain" because analysis has also been conducted separately for two large river basins. There should be separate systematic evaluation of winter, summer and transitionary periods to uncover underlying biases. This figure indicates that there are no substantial differences between ensemble members. Automatically, one would question the usefulness of the analysis conducted in this paper. Probably, if the authors have used the entire 50-member ensemble of the CanRCM4 then this situation may not have come up.

Figure 3:

Mackenzie is a huge river basin that encompasses many climatic zones. How is it possible to justify basin averaging of various climatic variables and indices by ignoring differences in climatological features? Some supporting discussion is needed with respect to water resources since that was the intended target of the paper. Model simulations, both corrected and uncorrected, are unable to capture very high variability of WFDEI data product and hence reliability of such simulations is questionable. Do the authors have any supporting arguments?

Figure 4:

Figure caption indicates multi-ensemble mean of climatologically averaged changes . . . Only one 15-member ensemble is used in the study. Why did you mention multi-ensemble? What do you mean by "climatologically averaged changes"? Please provide detailed explanation in the related text. You did not study changes in spatial variability of precipitation in a quantitative manner. What was the point in mentioning this in the caption? Patterns of raw and corrected precipitation are very similar then, what is the advantage of bias correction? If you have sufficient confidence in the bias correction methodology then there is no point in showing plots of both corrected and uncorrected data. The range (i.e. -80 to +80%) used for plots is rather large. This may have masked spatial variations in daily mean precipitation. Temporal results are shown for two large river basins while spatial plots are shown for the entire country. This has

led to an inconsistent presentation style.

Figure 5:

Y-axis title should reflect anomalies and not just daily mean temperature. Same issues as discussed for Figure 3, i.e. neither raw nor corrected model outputs are able to reproduce variability and pattern of observed data product. Does this reflect failure of the bias correction algorithm?

Figure 6:

The scale is restricted to 10 degrees C. What is the maximum model simulated change? Ten degrees C warming is projected in daily mean values. What is the level of change in extreme values?

Figure 7:

RX5day is increasing everywhere for all seasons except small decreases for the JJA period, specifically for southern parts of Alberta, Saskatchewan and Manitoba. Changes at the annual scale are all negative. How is this possible? There appears to be a serious mistake in the analysis. Results suggest more snow in winter months. Is this consistent with the results shown in Figure 6? Is this pattern physically plausible?

Figure 8:

How to differentiate between snow-related extremal indices and rain-related extremal indices from the results shown in this figure? Are you showing results for extremely wet days or extremely wet day precipitation amounts?

Figure 9:

There are considerable differences between the corrected and observed (WFDEI-based) distribution functions for the 1990s. In fact, both distribution functions should be very close to each other after bias correction. For the JJA period for the SRB, raw model distribution is much closer to the observed distribution function compared to the

corrected distribution function for the 1990s. For the 2080s, extremes have considerably been reduced in frequency for the SRB (JJA). This is in contradiction with what is presented in Figure 8. For DJF for the SRB, tails of distribution functions for the 2030s and 2080s are not as longer/thick as that of the observed distribution function. This contradicts what is presented in Figure 8. The same is the case for the SON period for the SRB. Distribution comparison appears to be a useless exercise as it is difficult to derive any useful results.

Figure 10:

Spatially the differences between raw and corrected model outputs are not obvious. How the changes were calculated? Did you take the monthly minimum of each month, calculated the change and averaged it over the seasonal window and then over the considered period? Or, did you take the seasonal minimum values, calculated the differences and averaged them over the considered period? May be a different procedure was adopted. Well, the adopted procedure needs to be elaborated clearly in the text part of the paper.

Figure 11:

Legend convention does not match with other figures. Probably you need larger panels for this figure.

References:

Not reviewed

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-249, 2019.

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