Reviewer #1

General Comments:

1. The manuscript "Modeling the sensitivity of snowmelt, soil moisture and streamflow generation to climate over the Canadian Prairies using a basin classification approach" analyzed the impact of future changes in temperature and precipitation on the hydrology of the Canadian Prairies. The manuscript is well written and discusses the implications of climate changes in different areas of the prairies based on 'local' climatic and land cover features. The novel content is a good fit to HESS and merits publication. That said, a few issues still need to be addressed. General comments are provided below, while specific comments are provided in the annotated PDF file.

Reply: Thanks for your constructive and helpful comments on our work. All your concerns have be appropriately addressed in the revised version. We have provided point-by-point responses to your comments, which are listed as follows.

2. The limitations of the study have to be further emphasized: while the authors do include a description of the limitation of the analysis in the last paragraph of the discussion (L449), that part of the manuscript should be presented in a separate section to draw the attention of the reader. The title of the manuscript should also emphasize the regional nature of the analysis.

Reply: Agreed. We have inserted a new subsection 5.3 to address the limitations of the current study in the discussion. We have emphasized that this study is for regional analysis.

3. Organization: the text would benefit of new subsections in the Material and Methods section and after the Discussion (L181; L449). Moving some narrative around would also improve the flow of the manuscript (please see specific comments in the annotated PDF).

Reply: We have inserted a new subsection 3.4 titled "climate perturbation scenarios", as well as a limitations subsection 5.3. Some narrative have been moved accordingly as suggested in your annotated PDF file.

4. Material and Methods: a more complete description of the model assessment should be provided. The reader can infer that only graphical assessment was used but the description is missing in the methods.

Reply: We have clarified in the section 3.3 that the model performance primarily relied on graphical assessment for the agreement of broad range of values at gauge sites. Additionally, we have added a metric of mass bias to compare the bias between simulated and observed annual streamflow in the revised manuscript.

5. Figure and Table captions: description of acronyms used in tables and figures should be provided to make those items stand-alone (e.g., Fig. 1 – Fallow_wd). The headings of the tables should also be improved (please see example on Table 5 in the annotated PDF file).

Reply: We have added descriptions for many of the acronyms in the tables and figures. However, we believe it would be best not to redefine basin class names in the tables and figures, as doing so might make the captions unnecessarily lengthy. Furthermore, we have improved the rows in Tables 2-7, as suggested in your annotated PDF file.

6. Analysis of extreme events: the authors state in L504 (Conclusions) that precipitation events were not considered in the analysis. However, they provide an analysis of "T and P changes showed different impacts on extreme annual streamflow" (L387-388). While I acknowledge that stream discharge extremes in cold regions are not necessarily linked to specific (extreme) precipitation events due to the cumulative nature of SWE during the winter, it is uncertain how extreme events will impact the timing and magnitude of streamflow, especially if these events take place during the spring snowmelt. I understand that the authors are assessing the impact of the delta method on extreme annual stream discharge, but precipitation events could have an overriding effect in this case. Thus, a discussion about the role of extreme precipitation events on extreme annual streamflow should be included.

Reply: Extreme annual streamflow referred to statistically high or low flows during the 42-year modeling period, but not the extreme streamflow events forced by extreme precipitation events. We have clarified this in the revision. We have corrected some expressions related to the descriptions of Figure 10. To avoid potential confusion, we used statistically peak and low flow instead of extreme flow in the revised manuscript. We acknowledge that our delta method didn't consider disproportionate changes in extreme precipitation events within the scenarios. This is one of the limitations of this study, and we have discussed it in the new subsection 5.3 in the revision. It is important to note that the focus of our study is a sensitivity analysis of the snowmelt, soil moisture and streamflow in response to potential future climate perturbations, rather than a modeling projection of future hydrology in the Prairies. The delta methods we employed, characterized by increment changes in temperature (i.e., per degree) and precipitation (i.e., per 10%), are suitable for the assessment of hydrological sensitivity (i.e., changes in hydrological variables caused by per degree warming or per 10% increases in precipitation). The delta methods provided reasonable temporal distributions of extreme climate like dry, wet, hot and cold as from the long-term historical observations. This allowed model calculation of shifts from spring snowfall to spring rainfall with increasing temperature, which is associated with the generation of extreme flow.

7. Expanding the Discussion: this section is relatively short and overlooks important results. Besides the extreme events discussed above, one aspect the deserves further discussion is the justification of the ice lens formation to explain reduced infiltration. Ice lens formation in CRHM is strongly influenced by the previous fall moisture status. Thus, enhanced melting caused by warming may not be influential if infiltration was already severely limited or restricted due to antecedent conditions (Gray et al. 2001. Hydrol. Process., 15: 3095-3111). This process should be discussed in more detailed.

Reply: We have added more discussions in the revision, not only for the modelling limitations but also for the enhanced snowmelt surface runoff resulting from a 2℃ warming. It is important to clarify that our intention was not to imply that warming increased overall snowmelt, as both SWE and snowfall decreased with warming. Instead, we intended to convey that a 2℃ warming increased the fractional surface runoff generated by snowmelt. We had a re-investigation on the results and acknowledge that the ice lens should have small impacts on snowmelt infiltration over the basin, considering their areal fraction was less than 0.05. Therefore, we have replaced Figure 7 in the original manuscript with the below Figure R1, and added the following explanations in the revision.

With a warming of 2℃, we observed a reduction in annual peak SWE and snowfall by 14.3 mm (14%) and 20 mm (12%), respectively. On the other hand, total rainfall increased by 20 mm (5.5%). However, snowmelt surface runoff increased by 3.9 mm (3.9%), accompanied by a decrease of 20 mm (40%) in snowmelt infiltration (refer to Figure R1 below). Snowmelt infiltration was strongly constrained by antecedent conditions before May 1st (Figures R1c and R1f), due to the influences of moisture status in the previous fall season.

In the warmer scenarios, snowmelt initiated and concluded earlier, with a majority of the snowpack melting away before May 1st (Figure R1a) at which date the soil layer started to thaw and soil moisture started to decrease (Figure R1c). The thawed soil layer after May 1st greatly facilitated snowmelt infiltration in the baseline scenario, during which considerable snowmelt was still occurring after May 1st. The longer melting period extending to the thawing season in the baseline scenario resulted in a larger fraction of infiltration, whereas the shorter melting period mainly falling within the soil freeze season generated a larger fraction of surface runoff for snowmelt.

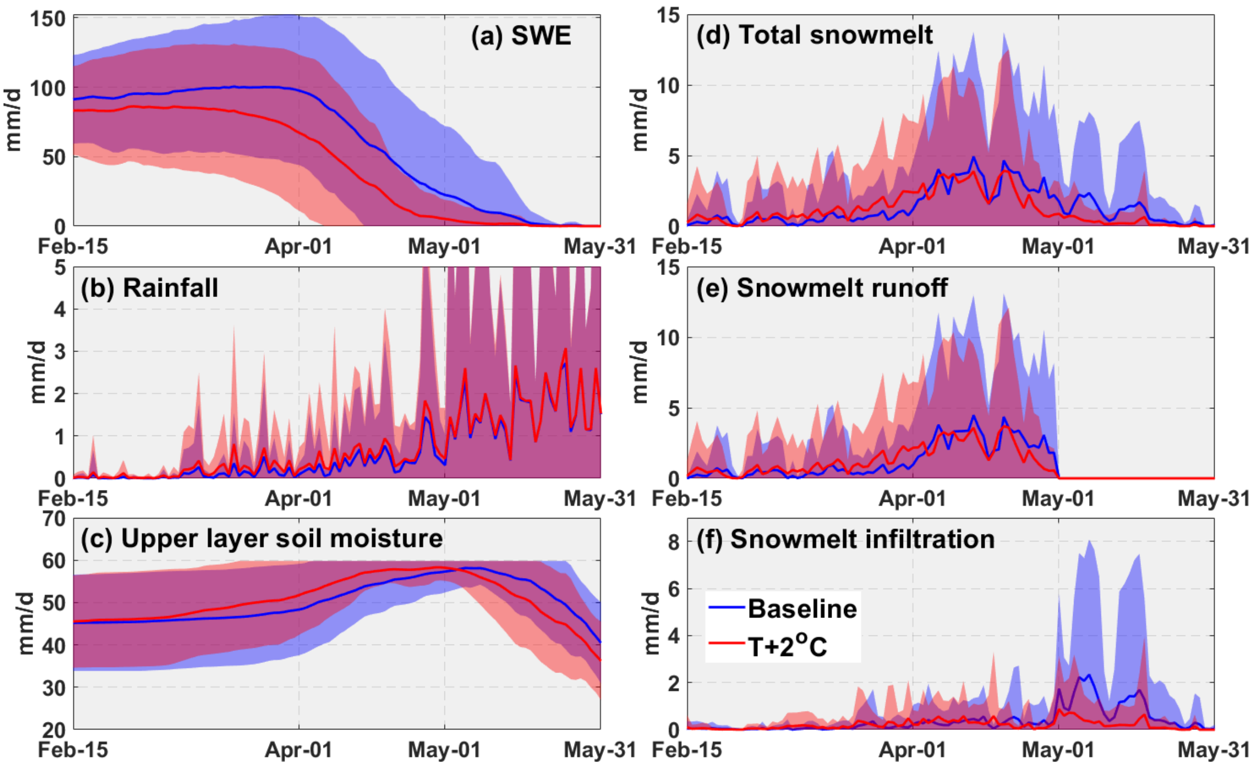

Figure R1. Comparisons of daily snow accumulation and melt, snowmelt runoff and infiltration, rainfall, and soil moisture in two *T* input scenarios. Solid lines represent the mean daily values throughout the modeling period, whilst the bands indicate the range of values within the standard deviation (+/-).

Specific comments in the annotated PDF file:

8. Title: "Modeling the Regional sensitivity.."; Line 17: "warming-induced".

Reply: Corrected.

9. Lines 47-48: How about extreme events?

Reply: The delta method does not explicitly consider disproportionate changes in extreme precipitation events within the scenarios, therefore we focused on assessment of hydrological sensitivity at annual and monthly scales (i.e., changes in hydrological variables caused by a unit warming or increases in precipitation). We have clarified this in the revised discussion section and pointed to this as an area for future investigation, as to include additional analysis of this regard here would be unwieldy. See our responses to your comment #6.

10. Line 81: "Virtual basin (VB) modeling using…"; Line 110: "Virtual basin (VB) modeling"

Reply: Corrected.

11. Line 96: annual cropping

Reply: Yes, this is annual cropping land. For the sake of simplicity and ease of understanding, we called them cropland in the text.

12. Line 112: fallow fields-This is a declining practice in the Canadian Prairies. Although the fraction in Table 1 are within range, it is important to indicate this information in the text.

Reply: We have added that fallow is a declining practice in the Prairies, which is supported by the small fallow areal fraction across all the basin types in Table 1.

13. Line 152: Does it mean not all processes are physically-based?

Reply: All surface hydrological in CRHM are physically based, that's why we called it strongly physically based (See Pomeroy et al. 2013; 2022). Because of this, the parameters (which have physical meaning) are observable, and do not require calibration. Some deeper sub-surface processes are represented conceptually in CRHM and therefore the parameters (which are not normally observed in any case) might require calibration. However, this was not done in the models presented here as the parameters, which were abducted from similar basins, did well enough to yield good agreement in our model assessment stages. In part, this is because sub-surface flows (and therefore parameters) are typically unimportant runoff generation mechanisms in the Canadian Prairies. We have clarified this in the revised discussion section 5.3.

Pomeroy, John W., Xing Fang, Kevin Shook, and Paul H. Whitfield. "Predicting in Ungauged Basins Using Physical Principles Obtained Using the Deductive, Inductive, and Abductive Reasoning Approach." Putting Prediction in Ungauged Basins into Practice, 43–63, 2013.

Pomeroy, J. W., Brown, T., Fang, X., Shook, K. R., Pradhananga, D., Armstrong, R., Harder, P., Marsh, C., Costa, D., Krogh, S. A., Aubry-Wake, C., Annand, H., Lawford, P., He, Z., Kompanizare, M., Lopez-Moreno, J. I..: The Cold Regions Hydrological Modelling Platform for hydrological diagnosis and prediction based on process understanding, J. Hydrol., 615, 128711, https://doi.org/10.1016/j.jhydrol.2022.128711, 2022.

14. Figure 2: Clarify in caption, just like Table 1.

Reply: Done.

15. Line 166: Continuously maintained?

Reply: Yes. Clarified.

16. Line 170: class’s->class; Lines 174-175: one is-> (1); the other is->(2)

Reply: Corrected.

17. Line 177: How were the models assessed? Which metrics were used?

Reply: We have clarified in the revision that the model performance was primarily evaluated through graphical assessment, which includes comparing simulated and observed data using various plots and visual representations for the agreement of broad range of values at gauge sites. For qualitative reference, we have added a metric of mass bias between simulated and observed streamflow in the revision. We have also emphasized that due to the virtual basin nature of the models, traditional approaches for model assessment for basin-specific model applications to instrumented basins are not appropriate here.

18. Line 181: Insert new section: Future climate simulations.

Reply: We have inserted a new subsection titled “Climate perturbation scenarios”.

19. Line 187: drop by-> decrease; Lines 201-205: Move here as a new paragraph.

Reply: Corrected.

20. Line 248: Indicate in the m&m section that only graphical assessment was used.

Reply: Done.

21. Line 251: Provide examples for “dry and grassland-characterized basins”; Line 253: Give examples for “wet and cropland-dominated basins”; Lines 258-259: Please provide examples.

Reply: We acknowledge your concern regarding the clarification of basin names in the results section. The basin categories were defined and explained in lines 231-232 of section 3.3. To avoid unnecessary redundancy, we agree that it would be more concise and coherent not to repeat the basin names in the results section.

22. Line 254: this range seems much cooler. Are reasons and implications discussed in the next section?

Reply: The smaller absolute *TES* of peak SWE in the wet and cropland-dominated basins than in the dry and grassland-characterized basin was because the wetter baseline climate in the cropland-dominated basins. The wetter climate resulted in larger baseline SWE and thereby generated a smaller percentage decrease in SWE caused by warming. We have added this in the revision.

23. Line 267: results-> Results.

Reply: Done.

24. Figure 5: Include definitions of acronyms.

Reply: We have provided definitions of *TES* and *PES* in the caption. Acronyms for basin types have been defined when they first appeared in the text. We prefer not to define the basin class names again. However, we ensured that the basin class names are consistently used and referred to in a clear and coherent manner throughout the manuscript.

25. Lines 272-273: This sentence seems ambiguous and needs some clarification. Wouldn't warming cause increased infiltration and runoff? These variables need to be discussed in the content of SWE as well. Am I missing anything here? Please clarify.

Reply: Warming resulted in a decrease in snow water equivalent (SWE), as indicated in Table 2. Consequently, both infiltration and runoff resulting from snowmelt were reduced in warming scenarios (except for the 1-2°C warming scenarios in the PHT class, which has been discussed in our response to your comment #7). Specifically, the decrease in snowmelt infiltration was more pronounced in the cropland-dominated basin types compared to other basin types. This implies that a larger fraction of snowmelt surface runoff occurred in the warming scenarios in these basins, which helped to lessen the reduction in snowmelt surface runoff. We have provided further clarification on this point in the revision.

26. Lines 292-294: This reads like discussion. Please move to respective section.

Reply: We have replaced the original Figure 7 with Figure R1. See our above response to your comment #7

27. Lines 296-297: this seems counter-intuitive, although justifiable by the ice-lense formation. However, the magnitude seems large. Please provide average reduction in infiltration.

Reply: See our above response to your comment #7, and we have provided average changes in SWE, snowmelt runoff and infiltration here.

28. Lines 306-310: The delta approach implies increases in all seasons evenly. Is it in agreement with increases projected by GCMS?

Reply: Yes, the delta approach resulted in consistent changes in precipitation across the seasons. The change ranges of –20% to +30% are in agreement with precipitation changes from the projection of GCMs. It is important to mention that our study focused on the assessment of hydrological sensitivity and did not specifically account for changes in extreme precipitation events. We added this in the revised discussion section 5.3.

29. Table 5: Relocate 'required P to offset warming(%)' and add 'TEMP change'. Similarly for 'required T to offset P rising'. Add definition of acronyms. Similarly in Tables 6 and 7.

Reply: We have fixed this. We agree that it would be more concise and coherent not to repeat the basin full names in caption.

30. Line 351: '-31.2% $℃^{-1}$' seems large.

Reply: Yes, streamflow in the IG basins showed the largest sensitivity to warming because of the strong influences of warming on the basin's connected area fraction.

31. Line 387: In the study, extreme events were only increased by their respective delta values used. Is this reasonable for PPT? Aren't PPT extremes expected to increase disproportionately?

Reply: See our responses to your comments #6, #9 and #28. Inconsideration of the disproportionate changes in extreme precipitation events was one of the limitations of the delta approach. But, this approach was used for the sensitivity assessment, not for projection of future hydrology. We added more discussion in the new section 5.3.

32. Line 424: are->were; Line 427: is->was; Line 481: insert space between virtual basin and (VB).

Reply: Corrected.

33. Lines 449-461: This should be an independent section (e.g. limitations of the analysis) to call the attention of the readers.

Reply: We have moved this to a new subsection addressing the limitations of the study in the revision.

34. Lines 504-505: This should be included in the last paragraph of the discussion.

Reply: Fixed.

Reviewer #2

1. The paper evaluated the effects of climate perturbations on snowmelt, soil moisture and streamflow generation in small Canadian Prairie basins using a modeling approach based on classification of basin biophysical and hydraulic parameters. The topic is very interesting and valuable and the manuscript is well organized. However, there are still some problems with the paper and it needs to be revised before it can be considered for acceptance.

Reply: Thank you for your valuable feedback on our work. We appreciate your constructive and helpful comments. All your concerns have been addressed in the revised version. In the following, we provide point-by-point responses to your comments.

2. Lines 139-141. It seems arbitrary to state that the CRHM is strongly physically based and requires no parameter calibration. More details and discussions are needed to explain the reasons for the calibration-free strategy for the CRHM.

Reply: We have provided a more detailed explanations about the physically-based nature of CRHM and need for calibration in the revision (see also response to Reviewer 1 above). The ability of CRHM to give good results without calibration has been well established in the published literature. Pomeroy et al (2022) show many examples of this. The use of CRHM parameters "abducted" from similar basins has also been very established in Pomeroy et al. (2013). In our study, the CRHM-based virtual basin hydrological models were not specifically tailored to site-specific basins. Instead, they were designed to represent the median land cover and hydrological characteristics of each of the seven major basin types spanning the entire Canadian Prairies ecozone. Therefore, the model parameters were not calibrated against observations from specific basins. This approach was taken to avoid using optimized parameters that may yield high performance in the calibrated basins but perform poorly in other ungauged basins of the same class. The parameter estimation process involved using median values derived from field surveys and existing literature knowledge for each of the seven basin types. This approach allowed us to capture the typical hydrological behaviour of these basin types. Additionally, it is important to note that streamflow discharge observations over the Canadian Prairies are very limited, especially for small basins, making it impossible to calibrate the CRHM-based models at each specific location within the Prairie basins. Considering this data limitation, the virtual basin hydrological models were utilized to assess the median sensitivity of hydrological processes to climate perturbations based on representative meteorological data for the seven basin types. This approach enabled us to evaluate the general response of hydrological processes across these basin types. We have revised the text to add clarity on these points in section 5.3.

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3. In the Section 4.1, the performance of the CRHM-based hydrological models should be evaluated quantitatively. Further, the performance of the model seems not good when used to simulate streamflow. It would bring great uncertainties when used to analyze the streamflow sensitivity to P and T perturbations. I suggest put more attention on the uncertainties of the model to make the results more reliable.

Reply: We added a mass bias metric for further quantitative evaluation of the model performance in the revision. We discussed in the text that biases in the simulated monthly and annual streamflow could be caused by two main factors (lines 223-231 in the original manuscript): First, the AHCCD meteorological stations are generally located outside of the basins that were gauged by the selected WSC stations (Figure 1 in the original manuscript). This spatial mismatch between meteorological and streamflow data sources could contribute to differences in the simulated and observed streamflow. Second, the CRHM-based VB hydrological models were structured and parameterized using the median characteristics of each class. The virtual basin models then represent the median characteristics in each of the basin types. Therefore, there may be inherent uncertainties associated with representing the full range of hydrological variability within each basin class. Simulation inevitably showed biases (–0.71 to 0.54) from the gauged streamflow due to the virtual basin nature of the models. Despite these considerations, we can see reasonable agreement between the simulated and observed seasonal pattern of streamflow based on graphical assessment. We have also added discussion of the potential uncertainty and limitations caused by the virtual-basin approach in the revised discussion section.

4. In Fig 3, the simulated and observed values of streamflow are too small to be distinguished for most months. I suggest to modify to make it clear.

Reply: In the Prairies, streamflow is often low or even negligible in most months. This characteristic of the hydrological regime in the region can lead to minimal differences between the model simulations and observations during these months. We tried to refine the Figure to make the comparisons during low flow months more clear, which turned to have very small improvements.