The manuscript “A Review and Synthesis of Future Earth System Change in the Interior of Western Canada: Part I - Climate and Meteorology” by Stewart et al. provides a review and shows novel research focused on climate change impacts in the Interior of Western Canada. The authors cover a large number of topics, which is necessary in a review article, but try to do this largely through their own research. This results in a fairly long article, which does not provide enough information to fully understand or correctly interpret the presented results. Also, the authors aim to connect results from single sections (e.g., changes in large-scale forcing and extreme events) without showing any physical evidence for such connection and often overinterpret results by making strong assumptions. I provide several suggestions on how to improve the manuscript in my points below. However, substantial effort is needed to improve the manuscript to a publishable level.

Major Comments:

1. The authors did not decide if they want to write a review or if they want to publish novel results. I am generally not against adding novel results to a review article but it makes a review paper much longer and reduces the accessibility of the presented information. The problem with adding your own results is that you have to clearly explain what you have done and you have to provide details on the experimental design. A good example is the use of the WRF-HRCONUS simulation. You are using this simulation in various places but the assumptions of the PGW method are only poorly described. A better strategy would be to refer to the existing publication that evaluated these simulations such as Prein et al. 2017, Dai et al. 2017, or Musselman et al. 2018.
   In general, making better use of existing literature and shortening the text to concisely review our state of knowledge, would make the article more easily accessible.
2. Related to the first point, the authors lose the reader due to the partly lengthy and unfocused assessments. You try to cover a wide range of topics with your own analysis and you are using a wide range of model experiments that have different strengths and weaknesses without addressing them adequately. Each of these topics could easily be its own manuscript. Relying more on existing analyses from e.g., the NARCCAM simulations instead of performing your own analysis would be beneficial. You barely mention any of the existing results from published NARCCAM papers in your review (e.g., Mearns et al. 2013, Gutowski et al. 2010, and others).
3. You mainly review changes in mean patterns and draw conclusions about their impacts on extremes. Extremes (e.g., flooding) are typically caused by weather patterns that are anomalous and changes in mean weather patterns might not be representative to derive estimates for extreme conditions. Please be careful with your interpretation of the effects of mean circulation pattern changes on extremes.
4. Similar to point 3, it seems like that many of your conclusions are speculative and are not based on tested causal relationships. I see no problem in discussing e.g., possible linkages between large-scale circulation changes on extreme phenomena but it should be clear that this is an untested hypothesis since you do not test these relationships in your manuscripts. Overall, I urge the authors to be more careful with the interpretation of their results.
5. You are using a large variety of modeling results with very different assumptions (e.g., CMIP5 vs. WRF-HRCONUS) and do not provide a critical review based on these assumptions.

E.g., In Section 3 you show significant changes in the large-scale circulation patterns and later you use the WRF-HRCONUS simulations, which assume no changes in circulation.

6. Why are you only using the RCP8.5 scenario? A review about climate change in a region should discuss the impacts of emission scenarios and internal climate variability on the presented results (e.g., Desser et al. 2012). Reading your article raises the impression that the presented changes are unavoidable and that emission scenario uncertainties are not important.
7. You misinterpret the rate of precipitation changes according to the Clausius-Clapeyron relationship. ~7 % precipitation increases per degree warming should be only realized for extreme precipitation whereas mean precipitation should theoretically increase by ~2 % per degree warming (e.g., Trenberth et al. 2003). The latter, however, is strongly regionally varying.
8. I encourage the authors to add a discussion on the state-of-the-art of climate change research in the CCRN region and to provide an assessment of future research needs. Highlighting future research needs could help to focus community research on the most important and least understood climate change processes in your region, which could be one of the main outcomes of this article.

Minor Comments:

P1-L26: "…projected to become stronger in each season…" what does that mean? In your assessments you show anomalies and for the reader it is hard to judge if the net effects increase or decrease synoptic scale forcing.

P1L-32: You have 7 references in your entire section 1. Many of the claims that you make here are without any reference.

P1L-32: I suggest to add a table that shows all of the here used model experiments.

P2-L3: "…will become wetter…" does this mean that the south is drying or that the north is becoming wetter regarding absolute precipitation?

P2-L5: "…likely have a huge impact" unless you cite references that show this I would suggest to use likely.

P2-L22-23: The temperature increase is likely a result of increased greenhouse gas forcing in your region and might be accelerated due the mentioned factors.

P3-L30: why did you use a 20/19 yearlong period instead of the standard 30-year long period? It seems like this makes the results harder to compare to the published literature.

P4-19-24: This simulation clearly needs more explanation about the PGW method and the included assumptions.

P4-L19: Why is there a C in Liu et al. (2016C)?

P5L13: Please explain the PNA index in the methods.

P5L18: "cooling is limited" where can I see this?

P5L22-23: 'and offsets its negative impacts' this needs some rewording.

P5L30: 'of historical molded? precipitation'

P6L1: and changes in the synoptic scale forcing

P6L6-7: I do not see any different trend in mid-century in Fig. 3b. The time series looks fairly linear to me.

P6L33: "could be able" would be more appropriate

P7L1: Please explain why you use a subset of 21 GCMs here.
P7L2: "In particular,…"
P7L14: Is this break in the time series really statistically significant?
P8L24-26: As above, is this change statistically significant. I have a hard time seeing it in Fig. 6b.
P9L14: I would add precipitation runoff to the list.
P9L20-23: Why did you not look at the northward shift in the zero-degree line in the models? Doing it the way you show it here might miss some important feedback mechanisms such as the snow-albedo feedback, which could accelerate the northward movement of the zero-degree isotherm.
P10L8: "in other" what? Areas?
P10L15: You introduced this simulation as WRF-HRCONUS on P4L24. Please be consistent with its naming.
P11L1-5: There is definitively a need for more explanation of these results. You spent page 5-9 of your manuscript talking about the importance of changes in large-scale drivers and then you present the results of the WRF-HRCONUS simulation, which assumes no changes in the large-scale drivers, without any discussion about this assumption.
P11L10: Why do you highlight the chinooks here while other processes might be equally important?
P11L30 to P12L5: I do not see the value of this regime classification compared to what you already said before in this section. This could be removed from the manuscript.
P12L31-32: I would be careful in interpreting these results. The precipitation patterns that you see can be caused by shifts in the most extreme storms, which cause result in areas with large increases and decreases next to each other. Shifts in the location of the precipitation maxima are typically chaotic and ensemble simulation would be necessary to identify the real climate change component of precipitation changes.
P13L10-13: This interpretation of the results is way to overconfident. First, how sure are you that your observed maxima is at exactly this location since you probably have a fairly sparse station network. Second, the effective resolution of climate models is typically >4-8 times its horizontal grid spacing. I would just state the model can capture the location of the precipitation maxima.
P14L12: How can I see that in Fig. 8a?
P14L19: what does 4.6/decade mean? Seasons per decade?
P14L20-21: Why are you comparing your results to the period 2081-2100? A 2.5-degree cooler FM period at the end of the century under RCP8.5 will be much warmer than a 2.5-degree cooler FM period in the past climate.
P14L27: Your summer cannot be typically linked to severe conditions. Severe conditions are per definition rare events and cannot be typical.
P16L7-9: This sounds like you are searching for an excuse to show mid-century results here. If it is that important to show mid- and end-century results you should also do this in the previous sections?
P16L19-30: Why are you discussing changes in mean summer precipitation again here? Some of the results look very different to the once you show in Fig. 10d. This makes it difficult for the reader to know, which interpretation they should belief in. I would suggest to remove this paragraph and maybe add some information to section 3.4.

P17L30-34: could be removed.
P18L1: This entire section has very little relevance to the CCRN region. Mentioning global average results on lightning can be easily misinterpreted as being relevant for Canada, which they most like are not. This section can be easily condensed to a single paragraph with the main message that we simply do not know a lot about future lightning in Canada.
P19L18: How confident are you here based on the large uncertainties in future lighting projections?
P19L20: What about the future availability of fuel?
P20L8: You could mention Fig. 15a here.
P20L12: Fig. 15b
P20L20: I think "link" is too strong here. You did not show any causal relationships in your analysis and your interpretation are mostly based on your interpretation of the results.
P20L23: Understanding the origin of changes would be very important as well.
P21L3: "…natural and anthropogenic factors."
P21L11: please replace "dramatic" with something more quantifiable.
P21L11-12: Which 4 models are you talking about?

Figures:
Fig. 2,4,5,7: The rainbow colortable does not allow to see a lot of details (e.g., Fig. 2 d shows a red area along the west coast but changes in the focus region are shown in a single blue tone. Please select a more appropriate colortable and use fixed color bar ranges in all 4 figures. At the moment it is very hard to compare them. Also, adding a significant layer on top of it (similar to what the IPCC uses in their assessment) would be highly beneficial.

Fig.10: Please adjust the color range. E.g., Fig. 10a has only one red spot in the lower left corner. Gradient would be much easier to see if you would reduce the maximum from 48 to 24 hours.
Fig. 13-14: Please reduce your color range also in these figures.
Fig. 15: This figure could be better imbedded in your manuscript (you only mention it once).

Literature:

Deser, C., Phillips, A., Bourdette, V. and Teng, H., 2012. Uncertainty in climate change projections: the role of internal variability. Climate dynamics, 38(3-4), pp.527-546.

Trenberth, K.E., Dai, A., Rasmussen, R.M. and Parsons, D.B., 2003. The changing character of precipitation. Bulletin of the American Meteorological Society, 84(9), pp.1205-1218.

Prein, A.F., Rasmussen, R.M., Ikeda, K., Liu, C., Clark, M.P. and Holland, G.J., 2017. The future intensification of hourly precipitation extremes. Nature Climate Change, 7(1), p.48.

Musselman, K.N., Lehner, F., Ikeda, K., Clark, M.P., Prein, A.F., Liu, C., Barlage, M. and Rasmussen, R., 2018. Projected increases and shifts in rain-on-snow flood risk over western North America. Nature Climate Change, 8(9), p.808.

Dai, A., Rasmussen, R.M., Liu, C., Ikeda, K. and Prein, A.F., 2017. A new mechanism for warm-season precipitation response to global warming based on convection-permitting simulations. Climate Dynamics, pp.1-26.

Mearns, L.O., Sain, S., Leung, L.R., Bukovsky, M.S., McGinnis, S., Biner, S., Caya, D., Arritt, R.W., Gutowski, W., Takle, E. and Snyder, M., 2013. Climate change projections of the North American regional climate change assessment program (NARCCAP). Climatic Change, 120(4), pp.965-975.

Gutowski Jr, W.J., Arritt, R.W., Kawazoe, S., Flory, D.M., Takle, E.S., Biner, S., Caya, D., Jones, R.G., Laprise, R., Leung, L.R. and Mearns, L.O., 2010. Regional extreme monthly precipitation simulated by NARCCAP RCMs. Journal of Hydrometeorology, 11(6), pp.1373-1379.