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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Future shift in winter streamflow modulated by internal variability of climate in southern Ontario" *by* Olivier Champagne et al.

Anonymous Referee #2

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General Comments I found the paper quite interesting and provides some substantial and important conclusions. Having said this, I think it really needs to be much more specific in the methodology, be clear on the assumptions that need to be and acknowledge a few fundamental issues with taking such an approach.

The model appears to have been calibrated for a reasonable period of time against what appears to be streamflow records. It is not clear where the streamflow records were obtained or where the locations of the gauges are. The authors should comment fifth cal/val statistics are sufficient for the analysis on climate change they propose. Also, the choice of Anuspline Homogenized (what they call CanGRD) data over per-

haps other data sets for forcing is not clear.

Consider the focus on snowmelt period and snowmelt simulations, the authors never discuss the appropriateness of the model physics for the snowmelt period. Does the PRMS model use an energy budget or temperature index. Is one method more appropriate for snowmlet, particularly in a climate change context, over another ? This should be at least mentioned.

Data used to derive the physiographic information to develop the model is not described, nor are the basins, except for very cursory comments. For example, there are many small control structures in these systems. The reader needs to know that and be made aware that they have or don't have an influence on the calibration or simulations.

The value of the paper appears to be in the messaging around the ensemble members results. Also, the attribution to synoptic patters provides some very interesting insights and the methodology seems reasonable, but the author would benefit from clearer explanations in sections 4.3. and 4..4. I find this very compelling and interesting, but it seems to get lost because the methodology confounds us in trying to understand what the authors are trying to do. I believe the intent and actual contribution of this work is important and should be published, but substantial clarification and structure to the manuscript is required.

Section 2.2. - comments Authors should state why they used PRMS instead of other models ? What is because it is computationally efficient ? has it been used by operational agencies in the region ? Some clarification is required. This section should include 2 parts. 1. model geo-fabric setup, including details around DEM and landcover (which ones) and how HRUs and routing is derived 2. forcing variables (what is necessary and how they are derived, where they come from) is not clear

Authors should describe better how the HRUs are generated. The reviewer presumes that a single dominant land type and soil type is used for each grid cell (as per the model documentation for PRMS). Authors should define how the grid (which are the same as HRUs ?) are defined in this application of the HRU, and specify that each grid is treated as an HRU. PRMS also requires stream networks, sub-basins, lakes to be defined. a few lines around how this was done or perhaps a schematic on how PRMS was implemented here would be worthwhile. Perhaps a figure similar to Figure 4 in the PRMS user manual but for the author's Big Creek application would be useful. It is difficult to get a sense of how the model was setup for this application.

The last part of section 2.2. describing the meteorological forcing used is also quite confusing. CanGRD (according the Environment Canada) is a monthly, seasonal and annual product. Perhaps the author is referring to the homogenized data used in the development of of the product produced by McKinley, which based on the article cited which I read, does not have a formal name. Also, there are a lot of other products available, so some justification as to what this product, which is quite a bit older thanks some of the more recent published data such as WATCH or CAPA, is being used. Also, can you clarify which streamflow gauges were used ?

Lastly, you mention muskingum routing, but it is not completely clear how this was calibrated. This is likely the most sensitive parameter the the NS criteria. Can you confirm how sensitive the results were to the routing ?

Section 2.3 - comments. A more complete description of the data developed in CanRCM-LE would be useful. I was required to lookup what this data set contained and how the ensembles were generated. I think the authors should actually include some level of detail here.

Section 2.4 - Comments This section is extremely unclear. I would recommend the authors describe what AHC is and at minimum make some reference to how the various ensembles were classified. What is the purpose of doing the ACH analysis, and is there a reference ?

Section 3.1 The methodology becomes clearer after reading this section. I would encourage the authors to maybe re-write some of sections 2.2 to clarify the approach.

It seems that what was done was 1. Calibrate these basins for use with PRMS using historic homogenized and gridded daily (5 years) data. 2. Using the CRCM-LE historic biased corrected forcing for the simulations and run ensembles. The authors should perhaps take a bit of time to describe why this approach was taken e.g. why not calibrate to a 10 year period. Are there any concerns about perhaps parameters values changing under a different climate regime ? Are you concerned about calibrating with Anuspline but driving the model with a different precipitation model, even if it was bias corrected. Some commentary here is necessary. The authors looked at ET, and I assume it was from the PRMS model. Why not use RCM or at least see what the RCM produces ? Since it is based on CLASS, should dit not be a bit more realistic than PRMS ?

The authors show in figure 5 increases in temp and precipitation. Can you clarify if this is the bias corrected values or original CRCM5-LE.

Section 3.2 A paragraph describing what ACH with a reference is required either in the methodology or here. Up to this point in the text, it is unclear why the ACH approach is even necessary. It does get clarified, but should be referenced and explained in section 2. The division between hi-lo and moderate and conglomeration of weather and flow classes seems a bit subjective. The authors should be clearer on how they chose to group these. It is not clear how you have a HiT category since P and T are combined. One assumes that the change in P is simply small. Also the whole section is difficult to follow and essentially describes what is in the table and on the plots, but it doesn't really tell me what I think it is trying to tell me. It seems that this is al about attribution of the change in flows. Is it caused by increases in T, P, or both. Section 3.2 does not really assist me in understanding.

Section 4.1 The authors never mention issues around frozen soils, freeze that cycles or river ice formation. River ice can have a large influence on hydrometric measurements and rating curves. Often it is too dangerous to take flow measurements in the winter so many flow values are estimated that time of year. The authors need to acknowledge something on uncertainty in winter measurements.

Section 4.2 and 4.3 The synoptic discussions are interesting but a bit confusing. This really need to be better explained and expanded.

Specific comments Page 2- Line 25-30 - Did you mean just limited members from CRCM5-LE or a different ensemble from Seiller and Anctil ? Same for Erler ? It would be useful if you clarified if you are using these new ensembles for the first time or you are the first to use all 50 as other authors had only used select ensemble members grin the same set. This is a bit ambiguous. Page 2 Line 30. For readability, it would be useful to add a sentence here as to why using 50 ensemble is important. Page 3: - line 22 should use “computational time” or “model computation time “ instead of model time. Page 3 - Line 25-27 - The authors should expand this to either include the equation or explain this better. The reader who is not completely familiar with PRMS will not understand what the coefficients are used for or what they mean. Page 4 - line 9 - please indicate the time step.

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