In the following lines we recapitulate our answer to the reviewers and the changes to the manuscript. Please note that the text, figures and tables have been modified in response to the received comments, providing a manuscript with improved structure and coherence.

Anonymous Referee #1

General comments

This manuscript summarizes research to assess the major sources of uncertainty in projections of future streamflow conditions in the au Saumon River in southern Quebec, Canada. The authors try to determine the uncertainty associated with model structure relative to that associated with variability in climate change model forcing data. It is an interesting question, one that has not necessarily been explicitly answered for many watersheds, particularly in Canada. The authors conclude that much of the uncertainty could be attributed to selection of some aspects of hydrological model structure, and not variability in climate model output. This is a notable result that would be a useful contribution to the literature. However, I found the manuscript lacking in several aspects that need to be addressed. These specifically include how the authors define, describe and discuss the categories of uncertainty. Furthermore, the manuscript has contradictory explanations of how the uncertainty associated with climate change was assessed, so the reader is left without a clear answer of the relative uncertainty from different sources. The introduction, in particular, does not include explanations of the sources of uncertainty as strong as those in Section 3.2.1, much to the detriment of the manuscript.

We thank Anonymous Referee #1 for his constructive comments on our manuscript and work.

We provide in the following detailed answers to his comments, taking into account their added value to the manuscript.

Efforts were made to better define the categories of uncertainties and how we evaluate them in current and climate change contexts. Modifications mostly concern the introduction, uncertainty assessment, as well as discussion and conclusion.

Specific comments

Page 14191 Line 20: The choice of words throughout the manuscript meant I remained unsure of what the authors meant by “natural variability” and how its impact on uncertainty was assessed. Did the authors consider natural variability to be the variability among the output from the several GCM members under different runs with different initial conditions? I don’t consider that “natural variability”, I consider that bias or inaccuracy associated with boundary conditions placed upon the model. I suppose that means I disagree with Kay et al (2006) in how they address natural variability and assess change. Either way, the manuscript would benefit greatly from clearer definitions and more in-depth discussion of the levels and types of uncertainty.

The ‘natural climate variability’ referred to “unforced variability internal to the simulated climate system”, as for example in Deser et al., 2012. We changed several elements of the manuscript to better define and exploit this concept.

Main changes page 5 lines 13 to 21:

> “In this work, PET formulations, snow modules, and lumped hydrological structures are compared under climate change, along with the natural variability of the simulated climate system. This later concept is illustrated here with a climatic ensemble based on five members with slightly different initial conditions, such as in Deser et al. (2012), where the natural climate variability refers to the “unforced variability internal to the real or simulated climate system” as evaluated with 40 members. Climate simulation ensembles allow the analysis of their internal variability (which is mainly a demonstration of natural variability) and can be seen as the irreducible fraction of climate simulations uncertainty (Kay et al., 2009, Velázquez et al., 2013), a part of the “unknowable” knowledge stated above.”

In several parts of the manuscript, “natural variability” has been modified to “natural internal variability of simulated climate system” to specify the analysis and refer to its definition.

Added reference:

> Deser, C., Knutti, R., Solomon, S. and Phillips, A. S.: Communication of the role of natural variability in future North American climate, Nat. Clim. Chang., 2(October), 775–780, doi:10.1038/NCLIMATE1562, 2012.

Page 14192 Line 18: Is “global” uncertainty meant to mean overall/total, or global vs.local?

In this manuscript, global uncertainty always refers to “overall/total uncertainty”. Changes have been made accordingly.

Page 14193 Line 8: It is unclear what is meant by “under climate change”. This, along with the phrase “natural variability” makes it difficult to assess exactly what sources of uncertainty are being evaluated. Maybe be very explicit with the definitions of the uncertainty categories.

We clarified this expression all over the manuscript.

Changes page 5 lines 13 to 14:

> “In this work, PET formulations, snow modules, and lumped hydrological structures are compared, along with the natural variability of the simulated climate system.”

I’ve never felt that summary paragraphs like that at the end of the introduction are necessary, but that is just me.

Removed paragraph:

> "Section 2 outlines the methodology, the *au Saumon* catchment, the data, as well as the modeling tools. Section 3 presents and details the results, followed by conclusions and discussion in the section 4."

Page 14195 Line 18: Should read "This procedure assumes that these corrections..."

"hypothesizes" changed to "assumes".

Section 2.5: The content here needs to be elaborated upon as this seems to describe how the relative degrees of uncertainty were evaluated among all the different sources. The manuscript should include much more detail of this key methodological information. For instance, it is confusing that the difference in the REF and FUT time series is used to highlight the uncertainty associated with climate change, but this is different than evaluating the effects of initial conditions on GCM output, which is how descriptions of the evaluation of the uncertainty associated with climate change are explained earlier in the manuscript.

We have modified the manuscript according to this comment.

Page 11 lines 23 to 24 and page 12 lines 1 to 25:

> "After the appraisal of the calibration performance on the Nash-Sutcliffe efficiency, to illustrate the effects of modeling tools selection on the calibration process, an uncertainty assessment is performed mainly based on these simulated and projected hydrographs and resulting hydrological indicators (overall mean flow, OMF).
>
> Cumulative streamflow uncertainty is evaluated first, representing the total uncertainty including hydrological models, PET formulations, snow modules, and climatic members. This step is performed on the CAL period where the measured discharges are available and then on REF and FUT periods to illustrate if this uncertainty varies with the simulated or projected period with climatic inputs.
>
> More, on the CAL period, it may be helpful to explore the reliability of the quantiles' envelopes, empirically drawn from the 3360 simulations, to comment if the latter can be directly interpreted as confidence intervals. The concept of a confidence interval reliability diagram consists in verifying if the observed relative frequency correspond to the simulated one – perfect reliability would result in a 1:1 slope on the diagram (Wilks, 1995). Several confidence intervals are thus plotted (from 0.1 to 0.9) with, for example, 0.5 corresponding to the quartiles spread (25 % to 75 %) and 0.9 corresponding to the spread of the 5 % to 95 % quantiles. Thus, for each of the 3360 simulations and each confidence interval, statement if observed discharge is included or not is verified, resulting in a reliability graph (Boucher et al., 2009; Velázquez et al., 2010).
>
> Streamflow uncertainty is then evaluated for each modeling process (i.e. hydrological, PET, snow, natural climatic variability) based on hydrological indicators, namely the

> overall mean flow (OMF), corresponding to averaged daily flow for the entire simulation period. A process-based streamflow uncertainty is then available, allowing comments about its extent on the observation period and about its change from REF to FUT periods.
>
> All these steps highlight the influences of climate change on water resources, but mostly evaluate the uncertainty in our diagnosis, related to hydrological modeling and natural internal variability of simulated climate system."

Section 3.2.1 Paragraph 3: Some of this should be in the methods section. The manuscript would benefit from an explanation of how the authors used the confidence intervals to definitively determine if there has been a change in streamflow.

We have changed manuscript to include this comment.

Page 12 lines 8 to 17:

> "More, on the CAL period, it may be helpful to explore the reliability of the quantiles' envelopes, empirically drawn from the 3360 simulations, to comment if the latter can be directly interpreted as confidence intervals. The concept of a confidence interval reliability diagram consists in verifying if the observed relative frequency correspond to the simulated one – perfect reliability would result in a 1:1 slope on the diagram (Wilks, 1995). Several confidence intervals are thus plotted (from 0.1 to 0.9) with, for example, 0.5 corresponding to the quartiles spread (25 % to 75 %) and 0.9 corresponding to the spread of the 5 % to 95 % quantiles. Thus, for each of the 3360 simulations and each confidence interval, statement if observed discharge is included or not is verified, resulting in a reliability graph (Boucher et al., 2009; Velázquez et al., 2010)."

Page 14 lines 8 to 13:

> "As mentioned in the material and methods section, exploration of the reliability of the quantiles' envelopes, empirically drawn from the 3360 simulations, aims at commenting if the latter can be directly interpreted as confidence intervals. For this purpose, a confidence interval reliability diagram is computed for the au Saumon catchment. Results in Figure 5 reveal a slight under-dispersion, confirming a possible link between the envelopes drawn in Figure 4 and confidence intervals."

Figures 7 and 8: Only a suggestion, would overlapping these hydrographs within one figure better illustrate the changes?

Figure 8 (now figure 7) draws the REF simulation in the background. However, we opt keeping Figure 7 (now Figure 6) as it is, for REF.

Page 14203 Line 15: What is meant by overall mean flow? Is this mean annual flow or the average flow for the entire simulation period.

Overall mean flow (OMF), corresponds to the averaged daily flow for the entire simulation period.

Page 12 lines 18 to 21:

> "Streamflow uncertainty is then evaluated for each modeling process (i.e. hydrological, PET, snow, natural climatic variability) based on hydrological indicators, namely the overall mean flow (OMF), corresponding to averaged daily flow for the entire simulation period."

Page 14205 Line 28: How could the methods as described definitively determine how various hydrological processes are responsible for the observed uncertainty? The authors have not provided any data or information to support this statement.

We think that the slightly modified section 3.3.2. and modifications all along the manuscript, about categories of uncertainties and how they are evaluated, clearly support this statement, as well as Fig.10 (now Fig.9) and Table 4 (now Table 5).

<u>Technical corrections</u>

Page 14190 Line 10: I do not understand what the authors mean by "Uncertainties are commented on the observation period and on simulated and projected climates." Maybe say "Uncertainty in simulated streamflow under current and projected climates is assessed."?

We have rephrased the sentence.

Page 1 lines 21 to 22:

> "Uncertainty in simulated streamflow under current and projected climates is assessed."

The English grammar and language often seems out of sorts. Another example from the abstract is the second last sentence. It ends with "... propagating this uncertainty on reference and future projection(s), while climatic members add over it." Perhaps the authors could say the "with wide variability in projected future climates further increasing uncertainty". This is an example of a pervasive problem throughout the manuscript that could be addressed with a thorough proof read.

We have rephrased the sentence.

Page 1 lines 26 to 27 and page 2 lines 1 to 2:

> "The analysis also illustrates that the streamflow simulation over the current climate period is already conditioned by tools' selection. This uncertainty is propagated to reference simulations and future projections, amplified by climatic members."

We have also modified other elements of the manuscript to address this comment further.

Page 14191 Line 3: Should read “Quantifying the uncertainties associated with the modelling...”

We have rephrased the sentence.

Page 3 lines 9 to 11:

> “Quantifying the uncertainties associated with the modeling of climate change impacts asks for a consistent and documented approach, reflecting the state of the scientific knowledge (Kiparsky and Gleick, 2004; Dettinger, 2005; Maurer, 2007).”

Page 14192 Line 3: Should read “...offer a simple means for unravelling...”

We have rephrased the sentence.

Page 4 lines 7 to 9:

> “Intercomparison studies offer a simple way of unravelling uncertainties associated to the many hydrological structures and concepts.”

Page 14192 Line 18: Should read “However, scant research addresses...” Similarly, at the beginning of Section 2.3.1.

We have rephrased the sentence.

Page 4 lines 24 to 25:

> “However, scant research addresses this question even if the diversity of PET formulations and concepts is compatible for intercomparison.”

Page 14193 Line 4: Could read “... but the literature targeting snow melt estimates in climate change model projections...”

Here we pointed out that no references are available for uncertainties assessment associated to lumped snow modules applied for future hydrological projections. We only know about few papers targeting lumped snow melt modeling on current period with hydrological modeling purpose. Because these works show large uncertainties on hydrological simulation, we expect that the variability remains at least identical for future projections.

Page 5 lines 8 to 12:

> “The authors are aware of no work addressing the hydrological projections uncertainty emanating from lumped snow modules, but the literature targeting snow melt modeling (e.g. WMO, 1986; Valéry, 2010, Franz et al., 2010) reported large uncertainties on the simulated discharge. It is thus expected that this variability remains at least as important under changing climate.”

Page 14196 Line 19: Watch the verb tenses. “were tested”

We have rephrased the sentence.

Page 8 line 19:

"Twenty conceptual lumped hydrological models (M01 to M20) were tested (see Table 1)."

Page 14202 Line 27: Do the authors mean to say the spring flood is arriving fifteen days earlier?

We have rephrased the sentence.

Page 15 lines 5 to 8:

"Evolution from REF to FUT reveals a spring flood arriving fifteen days earlier, with a slight decrease in the spring high flows."

Page 14203 Line 20: Much of this detail belongs in the figure caption and not the text of the paper.

Captions for Figures 9 and 10 (now Figures 8 and 9) are changed.

Page 15 lines 24 to 27:

"Figure 8 illustrates, by type of tools, the OMF uncertainty for simulations on the observation (calibration) period – 168 values per boxplot for the lumped conceptual hydrological models, 140 values per boxplot for the PET formulations, and 480 values per boxplot for the snow modules – while the OMF total uncertainty shows 3360 values."

Section 3.3.2: Much of the content in this section is constructed as a series of short paragraphs, but should be amalgamated into one or two larger paragraphs. A paragraph is meant to contain several thoughts that convey an idea. This section is constructed like a newspaper article.

We have changed this part based on reviewer's advices.

Page 17 lines 6 to 30 and page 18 lines 1 to 2:

"The total OMF relative change fluctuates from -11 % to + 129 %, but its interquartile range is restrained from +4.2 % to +16.2 %, with a median value of +9.3 %. This total uncertainty is distributed between conceptual hydrological modeling tools (namely PET, hydrological models, and snow modules) and climatic members.

The median OMF relative change per lumped conceptual model fluctuates from +6.3 % (M02) to +16.8 % (M08), confirming the sensitivity to the lumped conceptual model selection. The interquartile range is more uniform from one model to the other than in Figure 8, but M08 differs (18.1 %) in that regard – M08 was already identified with poor transposability on the same catchment by Seiller et al. (2012). The lowest inner sensitivity is achieved by M11 (10.9 %). PET OMF relative change is in general slightly higher than for the lumped conceptual models, from +4.1 % (E13) to +17.1 % (E21), stressing also the sensitivity to the selection of a PET formulation. The highest interquartile range is obtained by E21 (14.5 %), and the lowest by E02 (10.6 %). Again, the behaviour of the snow modules is more uniform than for the lumped conceptual models and for the PET formulations. The median OMF relative change of the snow modules are limited from +9.1 % (N2) to +9.9 % (N3), while their interquartile ranges vary from 12.5 % (N3) to 11.9% (N2).

On the other hand, the behaviour of the climatic members is quite distinct. First, the interquartile ranges of their OMF relative change are much reduced when compared to the others: from 4.8 % (C1) to 3.6% (C4), expressing lower inner sensitivity. Second, their median OMF relative changes vary considerably: between +2.7 % (C4) and +19.1 % (C3). This latter characteristic exemplifies the importance of the climatic natural variability. Changes differ greatly from one climatic member to the other. It is thus evident that a single 30-year realisation of the climate is insufficient to depict all the possible variability. Furthermore, it is also striking that an important part of the uncertainty spread revealed by the various hydrological processes actually originates from the climatic natural variability."

Page 14206 Line 3: Could be rephrased from "The importance..." to: "The example of this application to the au Saumon demonstrates the limit of our ability..."

We have rephrased the sentence.

Page 18 lines 3 to 6:

"The example of this application to the *au Saumon* catchment demonstrates the limit of our ability to provide a clear diagnosis of climate change impacts on water resources, especially when looking at the total OMF relative change, combining 16800 simulations and projections."

Page 14207 Line 4: Perhaps the authors should define the acronym behind the QBIC3 project.

This was added.

Page 19 lines 28 to 30:

"The authors acknowledge NSERC, Ouranos, and Hydro-Québec for support, as well as partners in the QBIC3 (Quebec-Bavaria International Collaboration on Climate Change) project."

Table 1: I am not sure "inspiration" or "inspired by" is the appropriate term. I would suggest "reference".

We use instead "Derived from" because some models are modified from their initial version to be adapted to lumped mode.

Figure 4: Again, no need for the word inspired. The terms PG and P2 need to be defined.

We use "Modified from" because this figure is not the initial one.

P_G is now defined.

Anonymous Referee #2

This is an interesting paper looking at the hydrological impacts of climate change for a catchment in Canada with a significant snowmelt contribution to flows. It takes a slightly different angle to that of many impact uncertainty studies, by not including climate modelling uncertainty per se but looking at the effects of natural climate variability via an initial condition ensemble of a climate model. The conclusions on the relative importance of natural climate variability, hydrological model structure, potential evaporation (PE) formulation and snowmelt formulation are very interesting.

My main comment would be that, while a relatively large number of PE formulations are compared, there is no consideration/discussion of other possible sources of uncertainty related to PE. For example, page 14197 line 16 mentions the need to set empirical PE coefficients for the catchment, but if different coefficients are needed for different locations under the current climate, then it is conceivable that different coefficients would also be appropriate for possible future climates in a catchment. Similarly, in more process-based PE formulations, crop coefficients like canopy resistance may change in future climates, as plant stomata react to changing levels of carbon dioxide (see e.g. Bell et al. 2011). These and other factors relating to PE and climate change are discussed by Kay et al. (2013). Such factors should at least be acknowledged in this paper.

We thank the Anonymous Referee #2 for his constructive comments. We have addressed his concerns and provide detailed replies in this document.

About the main concern mentioned above, we agree with the reviewer. PET formulations and how they are used, especially in this climate change context and for hydrological projections purpose, are a sensitive part of uncertainty. It is why we have dedicated a scientific paper (under review) specifically to this issue.

However, following the reviewer’s comments, we also added some acknowledgments and references in this present manuscript.

Page 19 Lines 12 to 17:

> “It must be acknowledged that PET equations, especially in this climate change context, also rely on empirical coefficients which add another source of uncertainty. Indeed, if different coefficients are selected for different locations under current climate, it is conceivable that different coefficients would also be appropriate for possible future climates in a catchment. This analysis could be extended on future work on this subject, as for example applied in Kay et al. (2013).”

Added reference:

Kay, A. L., Bell, V. A., Blyth, E. M., Crooks, S. M., Davies, H. N. and Reynard, N. S.: A hydrological perspective on evaporation: historical trends and future projections in Britain, J. Water Clim. Chang., 4(3), 193–208, doi:10.2166/wcc.2013.014, 2013.

Minor comments

1. I believe that the references to Kay et al. 2006 (page 14191) should be Kay et al. 2009.

Yes, it's a mistake, of course the correct reference is Kay et al., 2009.

Changed reference from Kay et al., 2006 to Kay et al., 2009:

"Kay, A. L., Davies, H. N., Bell, V. A., Jones, R. G.: Comparison of uncertainty sources for climate change impacts: flood frequency in England, Clim. Change, 92, 41–63, doi: 10.1007/s10584-008-9471-4, 2009."

Page 3 Lines 17 to 21:

"Several studies addressed all of them (e.g. Vicuna et al., 2007; Minville et al., 2008; Kay et al., 2009; Boyer et al., 2010; Görgen et al., 2010; Teng et al., 2012; Jung et al., 2012) while others focused on specific ones (e.g. Ludwig et al., 2009; Gardner, 2009; Poulin et al., 2011; Bae et al., 2011; Teng et al., 2012; Velázquez et al., 2013)."

Page 3 lines 24 to 29 and page 4 lines 1 to 2:

"For instance, Minville et al. (2008) found that GCMs initiate an important part of the uncertainty but so does, to a lesser extent, climate downscaling and hydrological modeling. Kay et al. (2009) arrived to similar conclusions. They compared six different sources of uncertainty: gas emissions scenarization, global climate modeling (GCM), climate downscaling, natural variability (which is disclosed calculating GCM runs from slightly modified initial conditions), and hydrological model structures and parameters. They found that all contribute to the global uncertainty and that GCMs are the most uncertain."

2. In the Intro discussion on natural climate variability (page 14193) it needs to be made clearer that only the initial conditions are varied between the ensemble members (i.e. make clear the distinction between an initial condition ensemble and either a perturbed parameter ensemble or a multi-model ensemble).

In our manuscript, the term 'natural climate variability' refers to "unforced variability internal to the real or simulated climate system", as mentioned for example by Deser et al., 2012.

We changed corresponding parts of the manuscript following the reviewer's advice.

Main changes page 5 lines 13 to 21 :

"In this work, PET formulations, snow modules, and lumped hydrological structures are compared under climate change, along with the natural variability of the simulated climate system. This later concept is illustrated here with a climatic ensemble based on five members with slightly different initial conditions, such as in Deser et al. (2012), where the natural climate variability refers to the "unforced variability internal to the real or simulated climate system" as evaluated with 40 members. Climate simulation ensembles allow the analysis of their internal variability (which is mainly a demonstration of natural variability) and can be seen as the irreducible fraction of climate simulations uncertainty (Kay et al., 2009, Velázquez et al., 2013), a part of the "unknowable" knowledge stated above."

In several parts of the manuscript, "natural variability" has been modified to "natural internal variability of simulated climate system" to specify the analysis and refer to its definition.

Added reference:

Deser, C., Knutti, R., Solomon, S. and Phillips, A. S.: Communication of the role of natural variability in future North American climate, Nat. Clim. Chang., 2(October), 775–780, doi:10.1038/NCLIMATE1562, 2012.

3. I don't understand what the penultimate sentence of the Intro (page 14193 lines 20-22) is trying to say - please reword more simply.

We have rephrased the sentence.

Page 5 line 1 and page 6 lines 1 to 2:

"It will illustrate what is our ability to produce a diagnosis of climate change impacts on the water resources of the au Saumon catchment."

4. Figure 3, the list of PE formulations, should be presented as a table, as for the lists of hydrological models (Table 1) and snow modules (Table 2).

Figure 3 become Table 2. All the following Tables and Figures references are changed in respect to this modification.

5. In the text description (page 14201) and caption of Figure 5, the 'pale' and 'dark' blue are transposed - 'pale' describes the 5-95% range and 'dark' the 25-75% range. The same goes for the captions of Figures 7 and 8.

Respective captions are changed.

Page 13 lines 20 to23:

"The cumulative uncertainty on the au Saumon catchment is illustrated in Figure 4: the pale and dark blue envelopes illustrate the distribution of the streamflow ensemble (5 % to 95% and 25 % to 75 %, respectively), the blue line, the median flow, and the black line, the observed flow."

6. Page 14203 line 24 - should be ’25% and 75% quartiles’ (not 2 and 75).

In our submitted manuscript, this problem is not visible. Perhaps the manuscript processing added this error.

7. In Table 3, you could perhaps highlight the best and worse performing options in each column.

Best and worst performing options are now specified by green and red colors in Table 3 (now Table 4).

Terminology

Some of the wording doesn’t seem quite right. For example, ’confronted’ or ’confronts’ when I think you mean ’compared’ or ’compares’ (pages 14192, 14193 and 14199).

“confronted” and “confronts” are changed respectively for “compared” and “compares” in the entire manuscript.

’alternates’ to describe the precipitation partitioning (page 14198), when I think you just mean that there are two formulations, and which one is used depends on altitude (but not in an alternating manner, i.e. one, then the other, then the first again, and so on?).

We have rephrased the sentence.

Page 10 lines 1 to 2:

> “Its precipitation partition, between solid and liquid, can be computed by two different formulations, depending on the layer altitude.”

’synthetizes’ when I think you mean ’summarises’ (page 14200).

“synthetizes” changed for “summarises”.

’propose’ when I think you mean ’present’ (page 14202).

“propose” changed for “present”.

Also be careful with use of the word ’mean’ (e.g. page 14192 line 3 and page 14207 line 21); it should be ’means’, but something like ’way’ would be better, to avoid any confusion with the statistical definition of ’mean’.

We have rephrased taking the advice into consideration.

Page 4 lines 7 to 9:

> “Intercomparison studies offer a simple way for unravelling uncertainties associated to the many hydrological structures and concepts.”

Page 18 lines 22 to 23:

> "Natural climate variability, through climatic members, was also studied for comparison with the diverse hydrological structures."

References

Bell, V.A., Gedney, N., Kay, A.L., Smith, R., Jones, R.G. and Moore, R.J. (2011). Estimating potential evaporation from vegetated surfaces for water management impact assessments using climate model output. Journal of Hydrometeorology, 12, 1127-1136, doi:10.1175/2011JHM1379.1.

Kay, A.L., Bell, V.A., Blyth, E.M., Crooks, S.M., Davies, H.N. and Reynard, N.S.(2013). A hydrological perspective on evaporation: historical trends and future projections in Britain. Journal of Water and Climate Change, 4(3), 193-208, doi:10.2166/wcc.2013.014.

Kay, A.L., Davies, H.N., Bell, V.A. and Jones, R.G. (2009). Comparison of uncertainty sources for climate change impacts: flood frequency in England. Climatic Change, 92(1-2), 41-63, doi: 10.1007/s10584-008-9471-4.