



EGUsphere, community comment CC1
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Comment on egusphere-2022-657

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Community comment on "Hydrologic implications of projected changes in rain-on-snow melt for Great Lakes Basin watersheds" by Daniel T. Myers et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-657-CC1>, 2022

This review was prepared as part of graduate program Earth & Environment (course Integrated Topics in Earth & Environment) at Wageningen University, and has been produced under supervision of dr Ryan Teuling. The review has been posted because of its potential usefulness to the authors and editor. Although it has the format of a regular review as was requested by the course, this review was not solicited by the journal, and should be seen as a regular comment. We leave it up to the author's and editor which points will be addressed.

Title paper: "Hydrologic implications of projected changes in rain-on-snow melt for Great Lakes Basin watersheds"

Overall impression

Rain on snow (ROS) melt events can have a big influence on their surroundings and can be either big or small. This manuscript investigates the impact of the climate change on the ROS events in the Great Lakes Basin in northern United States and few states of Canada for the period of 1960 to 2099, focussing on the period of 2040-2069 by the use of the model Soil and Water Assessment Tool (SWAT) with the addition of an energy budget equation to project the ROS events. With the results they looked at relationships and correlations between the different obtained variables. In general, the ROS events tend to happen earlier in the year by mid-21st century compared to the historical 1960-1999 values. The rain to snow ratio changes from around 1.5 historically to 2.0 at the end of the 21st century. This all also has influence on the water yield of the basins which also shift to earlier in the year.

The paper is nicely written and is important in these times of changing climate. The text has a good structure as the results are divided in understandable blocks. The variables are also captured in some good figures, although some changes should be made. Based on these comments, I would recommend publication with minor revisions.

General comments,

Firstly, as definition of a rain-on-snow event in the analysis section is stated that an event occurs on days with >1 mm rainfall on >1 mm snowpack SWE. By reading the Jeong and Sushama, 2018 paper this definition is not complete. It should be days with >1 mm

rainfall on >1 mm snowpack SWE and decreasing SWE. By using the wrong definition rain-on-snow events could be wrongfully depicted in the results, and thus possible differences in conclusions. This should be solved by correctly using the definition and changing this in the paper.

Secondly, in the research question is stated that the change of rain-on-snow melt and hydrology due to climate change will be assessed for the 21st century. In the method an argument is made about that for informing water resources management and because of better agreement with the models the primary focus will be on the mid-21st century (2040-2069). In the rest of the paper late-21st century is only mentioned for the change in ratio of area-weighted winter+spring rain-to-snow. Which has only a 0.1 change to the ratio change of the mid-21st century where the mid-21st century has a better agreement to the models and thus will probably be more accurate. Also in the conclusion at the end is stated "could help prepare for the climatic changes of the 21st century and beyond" where nothing is known for this latter time period with results from this paper. Thus, either the research question has to be rewritten to only the mid-21st century together with the conclusion or the late-21st century should be included in the rest of the analysis.

Lastly, again a comment on the research question but now because change in hydrology due to climate change is asked in the question. When stating hydrology I expect more variables to be analysed than only water yield, the rest of the analysed variables either belong to the change in climatic values such as rain and temperature or change in rain-on-snow events. Also, in the methods groundwater is mentioned to be modelled (line 65) but later not analysed in the results. So, either the phrasing of the research question should be altered or more hydrological variables should be assessed in the paper. This is important as the goal of this paper is to inform water managements to prepare for the changes due to climate change. For them the groundwater or runoff variables are also very important, and as they change with changing ROS melt (as said in line 39-41) this should be addressed.

Comments,

- In lines 312 -320, Myers mentions the difference in findings with Surianon mentioning that the studied times differ. But as the simulation used in this study is done for 1960-2099, the same time periods could be compared as the data will be present after simulations. Why not analyse the same time period (1960-2009) as Suriano to make this comparison possible, to diminish this suggestive difference.
- Lines 338-340, the speculation of influence of rain-to-snow ratio on size and timing of spring snowmelt and summer baseflow is made. This could be analysed by simply calculating the correlation between the COV (center of volume) and the rain-to-snow ratio which are variables present in the results.
- Line 99, "thus. Nineteen climate models were used". There is a missing argument about why you use 19 models instead of more or less. Please add an argument.
- Line 165, the word "drastic" is used. Drastic is not a quantitative value as it is more an subjective use when not supported with arguments. Either rewrite the sentence or add an argument.
- Lines 103-105, this information is a good/ better argument for the statement in line 118-119. As it now seems as random added information but potentially better used in the latter argument.
- Line 167, it is as a reader unclear whether the value mentioned for March is also the maximum or the value at the time of the maximum of April. This should be clarified.
- Line 172, it is unclear if "proportion of melt" means temperature based melt or ROS based melt. This should be clarified.
- Comment on figures in general: in the captions some abbreviations are written out where others are only posted as abbreviation. For the consistency of the paper this should be the same in all figures.

- Line 212, why is 2050s mentioned here instead of the same formulation used in the rest of the paper: "mid-21st century"? This could confuse the reader.
- Figure 6, Instead of "high flows" state "high water yields" in figure title of 6 e and f as stated in the caption for clarification.
- Figure 7, could be better depicted when the figures with frequencies (b, d and f) are on the right and with melt (a, c and e) on the left.

Specific comments,

- Line 168, "a 54% April decrease" should be rephrased, for example "54% decrease in April".
- Line 197, figure 5c should be changed in 5d.
- Figure 7, d is never mentioned in text.
- In figure 3 add in title of figure 3 b, d and f that it is for the period of the mid-century for clarity.
- In figure 4 some alterations on the axis titles can clarify the graphs. In 4b "snowmelt" can be clarified by writing "total snowmelt" and for c "proportion ROS" can be "proportion melt by ROS".

References

Jeong, D. Il and Sushama, L.: Rain-on-snow events over North America based on two Canadian regional climate models, *Clim. Dyn.*, 50, 303–316, <https://doi.org/10.1007/s00382-017-3609-x>, 2018