Comment on hess-2021-433

Anonymous Referee #2

Referee comment on "Trends and variability in snowmelt in China under climate change"
by Yong Yang et al., Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2021-433-RC2, 2021

As the key source of freshwater, snowmelt water resource in China has never been quantified on a national scale. This study used a simple temperature index model to calculate the snowmelt in China. The model is shown to perform acceptably well in China when the outputs were validated by snowfall, snow depth, snow cover extent and snow water equivalent. The results of this paper have important significance for understanding the distribution and variation of snowmelt in China. The simple model in this paper is interesting and the description of the model is comprehensive, and I think the method provides useful guidance for calculating snowmelt water resources outside China. In general, I think this work is valuable and of interest to the great community, and the manuscript is well written and worthy of publication in HESS. My comments are listed as follows:

- Line 11: the spatial resolution, 0.5 seconds? Shouldn't it be 0.5 minutes? Please check it throughout the manuscript.
- Line 16: change the unit "m^3" to "m^3 year^{-1}", and revise it throughout the manuscript.
- Line 19: Should it be "snowmelt water resource"? or "snowmelt time", "snowmelt rate"? "snowmelt" isn't clear, I think.
- Line 38: "contributes" to "contribute".
- Line 61: "snow meltwater" to "snowmelt".
- Line 64: change "aslo" to "also".
- Line 93: Figure 1a showed the mean snow depth from 557 meteorological stations in China. The mean snow depth is little significant, and it is better expressed by accumulated snow depth or the maximum snow depth. Figure 1c. Please use 3 as superscript in cm^3.
- Line 95: "mean snow density in China" is monthly or yearly? It should be introduced clearly.
- Line 101: The data link "https://doi.org/10.5281/zenodo.3114194forprecipitation" can not be connected.
- Line 115: The threshold temperature in China in this study should be shown in this manuscript by figure or table, or partly shown, I suggest.
- Line 117: What is the interpolated method?

- Line 124: Please delete "by".
- Line 129: The original dataset is snow depth, and the authors used this dataset to verify the snow cover extent. Please explain that.
- Line 135: downscaling was finished by yourself or others? It should be elaborated in detail.
- Line 151: The unit of DDF is mm $°□^{-1}$ day^{-1}, but the unit in equation (4) (Line 164) is cm $°□^{-1}$ day^{-1}. Please check.

16: Line 182: NSE equals one is not understandable, RMSE is not small for the temperature value. And the table does not reflect the monthly difference, but it has said monthly parameter in the title.

- Line 259: 263 (5.7%). 5.7%? please check.
- Lines 268-276: The number of meteorological stations used for snow depth verification was 264, far fewer than the 557 stations in Figure 1a. Why choose so few meteorological stations for verification?
- Line 316: Delete the extra word "with".
- Line 327: It is better to cite Fig.7b before Fig.8 in the manuscript.
- Line 387: "4.2.2" to "4.3.2".
- Line 415: "were shown" to "are shown".
- Line 340: How is the density used when using the model from point to surface in China? Do you use the average value for the five typical regions from different sites or other methods? It should be explained in detail.
- Lines 409-451: 4.4 Future changes of snowmelt under different climate scenarios. The historical period is from 1951 to 2017, while the future periods are different decades, namely the 2030s, 2050s, and 2090s. The comparison periods are inconsistent. I suggest changing "historical period" to "reference period", and setting the period of the reference period to be the same as those of the future comparison periods.
- Why did you select the 2030s, 2050s, and 2090s? It should be introduced clearly.