1 General remarks:

The manuscript *Assessment of SWAT spatial and temporal transferability for high altitude glacierised catchments* has been significantly improved compared to the first version, especially the introduction and the methods section. Concerning the results, I am a bit annoyed because your comparisons with Alpine3D and Prevah became very qualitative (Lines 366-376, line 429-435) compared to the previous manuscript, but, at the same time, not really robust. In addition, your main argument to explain the lower performances of the model is the use of a single melt coefficient for snow and ice (Line 341-349). This choice is very questionable in an alpine basin with such a glacier coverage under current conditions. But with regards to the goal of the paper, this is a huge source of uncertainty that you add to your future runoff simulations. My question would be: how could you trust your predictions knowing that the importance of snow melt (which is not that well simulated by your set of parameters) will become higher while the influence of glacier melt will decrease?

In my opinion, this study is a good qualitative assessment of climate change impacts on an alpine basin but the quantitative aspect is very limited. Therefore I would recommend to the authors to mention it more explicitly in the manuscript.

2 Specific comments:

1. Line 32: what do you mean by "management induced environmental changes"?
2. Line 44: *large* instead of *great*
3. Figure 1: add a and b letters for each sub-part of the figure
4. Line 105: Please clarify what you mean by: *What is important in our study is that melted snow is handled by the model the same way as the water that comes from precipitation regarding the calculation of runoff and percolation*
5. Line 111-112: avoid repetitions (detailed, in detail)
6. Line 124: change the verb define.
7. Line 128-130: I am not convinced that you will *reduce the uncertainty of the calibration* by using *detailed soil and land use maps* even if it is a commendable effort. In the same paragraph, put the website reference in the bibliography.
8. Line 142-145: the meteorological parameters you enumerate are available at the Damma station but not all of them are available in Gütsch right?
9. Line 157-160: check the spelling of this sentence!!

10. Section 3.2.3: try to streamline a bit this section especially the part on climate change scenarios which is hard to read (repetitions, intermittent).

11. Line 192-194: the sentences are not really relevant for the reader.

12. Line 221: How can you have *two different but similar watersheds*? They have maybe similarities but they are not similar!

13. Line 235-250: these two paragraphs are hard to read. Try to stremaline them by putting the parameters name into bracket for example!

14. Line 279-289: If I understand well, the snow melt temperatures SMTMP is the threshold under which you have no melt. How do you justify a optimal value of 2.5C which is very high?

15. Line 296-299: Your statement is a bit confusing: About which "previous model" are you talking about? Morevover, you should remind to the reader that you are working with daily time steps. This strongly influences the NSE coefficient.

16. Line 310-311: I don't understand you argument about wet years. Why would your model be less skilled to simulate a wet year? You also mention this argument on line 325-326. Please clarify!

17. Line 317-318: this is not really new: SWAT has been used in glacierized basin in the past.

18. Figure 3a: you can hardly see anything on such graph.

19. Line 333: This is a good idea to evaluate the runoff timing. But why have you applied a 15-days moving average on you data? This is quite brute force and will necessarily smooth out the differences.

20. Line 351-359: as you have daily discharge observations, I am not convinced about the influence of the basin slope on the discharge response. This could have an impact at hourly time step.