Review of the manuscript "The effect of input data complexity on the uncertainty in simulated streamflow in a humid, mountainous watershed" by Hoang et al.

I thank the authors for the careful answers and explanations to my comments regarding the first version of their manuscript. I am satisfied with the answers and the adaptations made in the manuscript. I have mainly three concerns left, which I think would be worth to address in the manuscript:

1) Both reviewers asked for the reason for choosing an NSE value of 0.65 as a threshold for good simulations. I assume that many reader will later on have the same question. From this perspective, I would highly recommend that the authors add a short statement on the reason for choosing NSE 0.65 in their manuscript.

2) I like that the authors provided a more detailed explanation of the two-stage calibration process. However this raises the question (as also pointed out be reviewer 1) why a two-stage calibration is needed if there is only streamflow data, but no snow data available. I think it would be worth to address this question when explaining model calibration. Furthermore, I would also mention how you selected the values of the 9 flow parameters during the first calibration round. Did you use default values?

3) As mentioned in the first review round, I recommend to consistently use (HESS guidelines for) units and references. E.g. DEM resolution of 30 m vs. 30m DEM, mm/°C vs. days-1 (in Table 1), group(i) vs. group(2) at P16 L6, some references with doi while others without, etc.