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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Dynamics of water fluxes and storages in an Alpine karst catchment under current and potential future climate conditions" *by* Zhao Chen et al.

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Reply to comments of an anonymous referee (Reviewer 2) on the manuscript "Dynamics of water fluxes and storages in an Alpine karst catchment under current and potential future climate conditions" by Zhao Chen et al.

Summary of the reviewer: Chen et al. simulate the water storages in a karst catchment using a distributed numerical model. The authors also predicted the hydrology changes under climate changes and stated the significant impacts on karst hydrogeological responses. Overall, this paper is novel and well written, so I would recommend HESS publish after a major revision.

Reply: We thank the reviewer for her/his positive, very useful and constructive comments that will contribute to improve the manuscript. Most of the referees' remarks will be taken into account. According to her/his comments, we will perform the following changes.

Comments:

P1L12: I suggest the authors to provide a brief introduction of distributed numerical model when mentioned this term, since the readers might need some help to understand this word. If the authors do not want to have a description in the abstract, simply talk about the details of distributed numerical model later.

Reply: We will give more details about the term "distributed numerical model" in the introduction.

P1L19-27: I would expect a few sentences to specifically highlight that why the study of karst catchment is important, and the water resources in karst region are vulnerable under future climate change conditions. What is the difference of hydrological responses between karst and non-karst catchment? What it the scientific merit in this study?

Reply: We will highlight the importance of karst catchment study and mention that karst water resources are especially vulnerable under changing climate conditions. Also we will explain shortly the difference of hydrological responses between karst and non-karst catchment in order to elaborate the significance of this study.

P2L10-11: I would say the lack of input variables and model parameters in hydrology model is not only a challenge in Alpine, but also for the hydrological models in other regions. And, is "spatially-distributed model" equal to "distributed numerical model"? Just try to keep consistent and avoid misunderstanding.

Reply: The reviewer's remarks are reasonable. We will modify the text accordingly.

P3L1-3: I doubt if it is appropriate to say the relationship between subsurface hydrology and climate has not been considered in detailed. Numerous papers have tried to addressed the relationship, if you simply google some keywords. I would recommend the authors take a look at the review paper by Taylor et al., 2013, Groundwater water and climate change, Nature Climate Change, DOI: 10.1038/NCLIMATE1744.

Reply: Of course we know the important and highly-cited paper by Richard Taylor et al., and we will discuss and cite this paper and other relevant papers in the revised version of our manuscript.

P4L29: What is the source of meteorological data did the authors use? What parameters does the model need? It seems that the authors use the in-situ observational data from the meteorology stations. Since the authors mentioned the uncertainty issues of weather forcing at the very end of this paper, I suggest the authors take a look at the climatological/meteorological reanalysis dataset. I'm not familiar with the reanalysis product in Europe, but I'm sure there are some datasets (e.g. ERA-Interim) or global datasets you can use.

Reply: The suggested ERA-Interim dataset has unfortunately too coarse spatial resolution (80 km) for this study area, which only 35 km2 but varies in elevation from 1000 m to 2230 m. The model needs three input variables, i.e. precipitation, temperature and relative humidity at hourly time steps. These variables were measured at nine weather stations across the study area (Figure 1b). Each meteorological data type is interpolated at a 100 m $\times$ 100 m grid using combined inverse distance weighting and linear regression gridding, in order to consider its spatial variability in the study domain.

P5Ln4-12: The authors mentioned that the melt factor and radiation coefficients were estimated by model calibration. What observational data did you use to calibrate the parameters? And also, I'm afraid the snow accumulation and melting equations are too simple, especially considering the importance of snow melt in this study. Could you validate the accuracy of snow accumulation and melting?

Reply: The study area is high-alpine and difficult to access. The observation data is

strongly limited and we do not have direct snow observation to validate the accuracy of simulated snow accumulation and melting. Due to the lack of data, we applied the HBV snow routine and modified the snow melt equation after Hock (1999). His study demonstrated that the modified snow melt equation is able to simulate realistic hourly melt and its spatial pattern in complex topography. In order to achieve an effective calibration of the snow routine used in this study, we have developed the multi-step model calibration procedure described in section 3.4.2. The discharge time series from November 2013 to June 2014 (whereas snow accumulation and melting dominated) measured by the gauging stations at QS and QA were used for the calibration. However, the simulated snow melt and especially the accumulation are associated with uncertainty. We will consider this critical point and discuss the consequences of choosing a conceptual-type snow simulation approach.

P5Ln20: I'm not sure if the calibration strategy is an important part in this paper. I would recommend the authors address the physics of the distributed numerical model rather than the calibration.

Reply: We thank the reviewer for this recommendation. We will include a more detailed description about the model used in this study (see response letter to reviewer 1) and address in this part the physics represented in the model. We do think that the proposed calibration strategy is an import part for this study though, which helped us to achieve an effective model calibration.

P6Ln25-26: How did you include the infiltration in the storage calculation for the non-karst area? Please explain or consider rewriting this sentence.

Reply: The surface runoff from the non-karst area can infiltrate into the underground karst drainage network due to the conduits C34 – C38 constructed in the upper part of the valley (Figure 1c). The flow through the conduits into the underground karst drainage network is considered as the infiltration (allogenic recharge) into the karst aquifer. We could quantify explicitly the infiltration and use Equation 7 to calculate the storage for the non-karst area.

P7Ln20: It seems the authors use one year (water year 2014) simulation as the base to make future projection with the changing precipitation and ET forcing. Should you consider run the distributed numerical model in multiple years? The climatological average hydrological responses from the model should be used here, if long-term data are available.

Reply: We agree with the reviewer that the distributed numerical model should be run in multiple years to make projections with the changing precipitation and ET forcing. However the study area is high-alpine and difficult to assess. The complete hydrological monitoring system has been operating since 2012 and we were lucky to obtain at least one complete hydrological year data, which can be used for this modeling study. To better assess the model, despite of one hydrological year data, we proposed performing a split sample test by using multiple bootstrapping of subsets of the observation period (section 3.2). This approach is already used by Hartmann et al (2012). We will include and discuss the test result in the revised manuscript.

P7Ln23: The total volume of mass water does not make sense to the readers who are not familiar to the study area. Is it better to use flux unit (m, or m/day, divided by the area of study domain) to represent the water mass? (I would say it's an open question for the authors to think about). And also, I highly recommend the authors plot the mass budget of each component instead of using the time-series plot in Figure 5.

Reply: The reason why we used the total volume of mass water is the flow calculation referred to different areas (karst area and / or non-karst area). To better compare the flow components with each other, we decided to use the total volume of mass water as unit. Additionally, we will adjust Figure 5 for increased simplicity (e.g. plot the mass budget of each component).

P8Ln14: The references of projected precipitation and ET are missing? Are these predictions estimated from an earth system model?

Reply: The "current situation" is the model simulation based on the existing hydrological data from 2013 to 2014. We will clarify this in the revised version of the manuscript.

P8Ln20: How did you estimate the snowmelt and snow storage? Did you simply compute from the snowmelt equations in Sect. 3.3 or from an earth system model? Please explain and provide more information.

Reply: The description about the maximal snow storage is referred to the simulation time step 3109 (Figure 8). There we estimated the snowmelt using the equations described in section 3.3 and assuming that the positive water storage value is equal the snow storage, which is admittedly a quite pragmatic approach. In fact, the maximum snow storage is clearly underestimated at that simulation time step, as we did not consider the temporary water storage volume in the karst aquifer, which is negative. In the revised manuscript, we will show the calculated snow storage obtained from the HBV snow routine, which is already implemented in the current model. Accordingly, we will modify the text and add a new figure regarding the snow storage. Also, we will add some more discussion about the uncertainties that resulted from our approach.

P9Ln19-20: The "spatial-temporal" distribution is one of the major finding and novel point in this study. I recommend the authors address this point more.

Reply: We thank the reviewer for this suggestion and we will follow that recommendation.

P10Ln17-18: What is the statistics of surface runoff responses to heavy rainfall events? Overestimation or underestimation? How did you compare? In general, I don't think you can directly compare the simulated surface runoff with streamflow measurement.

Reply: Here we meant the surface runoff generated from the non-karst area. The comparison between the simulated and observed surface runoff can be done using Figure 4. The model underestimates surface runoff if fluxes are greater than about 2m3/s. We will rewrite this part and avoid the misunderstanding.

P10Ln27: Should be “hydrological process sensitivities”?

Reply: We thank the reviewer for this suggestion. This sounds very elegant. We will use this term.

P11Ln7-9: The different hydrological responses at karst springs are interesting in this study. I recommend the authors highlight the importance of elevation dependency and the permeability of aquifer in water storage capacity and streamflow discharge.

Reply: We thank the reviewer for this recommendation and we will do that.

Additional comments: Looking at the reviewer #1 comment, I agree that a more detailed description of the distributed numerical model should be included, and the difference between this model and the previous paper should be highlighted.

Reply: We will include a more detailed description about the model in this manuscript. Also we will add a brief discussion about the differences between the initial model developed by Chen & Goldscheider 2014 and the extended model used in this study.

References

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