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Discussions

Interactive comment on "Climate change impacts on hydroclimatic regimes and extremes over Andean basins in central Chile" *by* Deniz Bozkurt et al.

Anonymous Referee #2

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Bozkurt et al. provide new insights into climate change impacts on hydroclimatic regimes and extremes in Andean basins of central Chile. The authors used modeled data to obtain their results and draw their conclusions. In general, I found the manuscript by Bozkurt et al. important since Chile is vulnerable to climate-change-driven impacts on the regional water resources. However, while reading the manuscript, major concerns arise. Most, but not all, of my concerns are related to the preferred model and its applicability for the presented case.

1. The authors showed projections of substantially decreasing discharge by the end of the century. The authors did exclusively concentrate on climate change effects on the regional hydrology, which was the main goal of this study of course. However, I

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am convinced that land-use change plays also a major role in this specific area. Did the authors also account for such effects? Land-use change effects often outpace the effect of climate change. Assuming increasing temperature (up to 3.6° C; p. 7 line: 30) and declining precipitation (-30-40%), one might expect changes in the potential vegetation and, thus, the potential land-uses. Against the background of this strong climate shift, I was stunned when reading that ET did not change so much. Can the authors provide any number on the relative contributions of both changes?

2. The authors chose decadal periods to analyze climate change effects, which is absolutely fine. If the aim of the study were to quantify changes in hydroclimatic regimes however, a trend analysis would be probably a better approach. To me, the chosen decadal time scale sounds a bit arbitrary and the authors should – at least – explain better why they chose that explicit temporal step.

3. The authors used the VIC model, though they do not explain why this model suits best for their study. The authors only state that Demaria et al (2013) successfully applied the VIC model to the Mataquito basin. I am not convinced that the VIC model is thus necessarily the best choice for all four studied basins.

4. I this context, I also miss information on the parameterizations for the VIC model runs in all four catchments (p. 5, lines 20 ff: "Parameterization and calibration within VIC is performed primarily through adjustments of infiltration parameters, soil layer thickness and baseflow parameters"... and (lines 25-27) that "vegetation and soil hydraulic properties are based on values from Sheffield et al. (2006). Infiltration parameters, soil layer thickness and baseflow parameters are based on values from Demaria et al. (2013a), which calibrated and validated the model in the study area"). Demaria et al (2013a) only calibrated and validated the model for the Mataquito basin? To me it sounds like that the authors applied the same parameter set to all basins, which is not necessarily given. Regarding the mentioned strong gradients, e.g. in topography and climate, I doubt that such a simplification is justified.

5. The authors assume average soil thicknesses > of 1.7m, as indicated (p 7, line 17). Is this average soil depth realistic? From my own experience I can state that soil thickness varies a lot across Southern and Central Chile. Casanova et al. (2013) (The Soils of Chile) provide a valuable overview of Chilean soils and could be used at least as a reference to compare their soil depth assumptions. Did the authors somehow validate the soil thicknesses used in the model?

6. The authors state that ET is hard to estimate which is definitely true. However, arguing that (p. 7, lines10ff) "diagnostic evapotranspiration products such as GLEAM have important uncertainties" and "large uncertainties in simulated evapotranspiration by land surface models have been reported in several studies" is not a valid argument. If the chosen method has (intrinsic?) problems to realistically simulate ET, the authors should ask if this specific approach is appropriate.

7. I am not totally convinced that the spatial resolution suits well the spatial extent of the basins presented here. A grid of 0.25° seems to be a bit rough. Assuming cells of 0.25°*0.25° yields roughly 750 km2. The authors estimate the basins to 13700, 6300, and 11500 km2. This yields few cells only even for the largest basin. At the same time the authors emphasize strong topographic gradients (e.g., p.3, lines 8-9), which challenges the rough spatial discretization. I suggest considering a finer spatial resolution.

8. The authors state (p. 6, lines 13-14:) that "based on a detailed streamflow validation, Demaria et al. (2013a) illustrated a reasonable agreement between the VIC model simulations and observed fields." The authors point out that the model predictions and the observed values are highly correlated (r=0.8 to 0.9). Based on what correlation is that number derived? The chose of a suitable measure is not trivial in this area that shows strong hydroclimatic seasonality.

9. I am not completely convinced about the outcomes of the return period calculation. Floods are stochastic and, thus, a resampling method (such as bootstrapping) could

be performed to account for uncertainties. The uncertainties are particularly important for the maximum flows. Slight changes here may cause huge differences. I suggest including uncertainty analysis here.

10. The discussion section is rather poor and does more or less concentrate on the consequences of climate change. I am inclined to say that the presented discussion is rather a conclusion section – if condensed. The discussion section should evaluate the different processes that may explain the modeling results, too. The authors mention glacial retreat or loss of snow cover, which in turn may affect soil moisture etc. However, a discussion on these (among others) is missing.

11. The authors present data on decreasing ET for the modeled periods? Is this purely due to the declining precipitation? I (naively?) expect ET to increase since the temperature increase is substantial over the simulated periods. I miss a discussion here.

12. The section 3.2.2 on changes in hydroclimatic changes mixes results and (a relatively superficial) discussion. I suggest separating both, which facilitates the reader in following the arguments provided by the authors. In general, I think the results and discussion sections should be better separated in order to provide a better evaluating of the findings.

Minor:

1. The section 3.2 “Hydroclimatic projections” belongs rather to the method section and not to the results. 2. I am a bit confused about the rates in temperature and precipitation change over the modeled scenario periods. All reported changes (%) are respective to the reference period or do the rates account for the changes from one modeled period to the next one? 3. P. 10, lines 28-29: In one sentence “much” and “mainly”. I suggest rewording this sentence. 4. The authors sometimes flip between using passive and active wording, e.g. p. 11, lin 19 and 24: “We assess. . .” and “. . .is assessed”. I suggest staying consistent and using one way of spelling.

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