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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.

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General response for reviewers 1 and 2:

We appreciate the thoughtful comments by the reviewers. All of them are taken into consideration in our responses. The reviewers raised two major concerns, namely: i) lack of model performance evaluation and ii) the choice of spatial resolution for VIC model simulations.

We agree with both reviewers in that the model setup and simulation protocol adopted in this study may not be suitable for a comprehensive characterization of the hydrological processes within the four basin evaluated. However, we would like first to recall

that the main goal here is to make use of the VIC capabilities to get future basin-wide projections in key hydroclimatic variables (e.g. ET, SWE) that are consistent with the imposed changes in precipitation and temperature. Yet, given the general lack of this type of assessment in central Chile, we considered as a reasonable starting point to extend the work done by Demaria and colleagues over the Mataquito basin to three other basins of the same region (Rapel, Maule and Itata).

For instance, the spatial resolution of 0.25 degree follows the forcing available at the time of performing our simulations (we adopted the one of Demaria et al. 2013). This grid is indeed too coarse to evaluate processes at the scale of secondary and tertiary watersheds in Chile (∼100 to 1000 km2). Aware of this, our group is working on a higher resolution (5km) meteorological forcing for regional hydroclimate research in Chile. This dataset is still in evaluation and will not be used in the present study. The 25km-resolution of the Demaria et al. forcing, although at the limits regarding the main basins assessed in this study (∼10000 km2), is however useful for the general purposes above mentioned.

Understanding the limitations of our study, notably the resolution and the use of the VIC-parameters calibrated for Mataquito for the whole domain, we have considered the reviewer suggestions and improved the model evaluation by adding more statistics such as NSE, KGE, PBIAS. We have found that the VIC model adequately captures relevant information contained in these evaluation metrics, which gives further confidence in the use of this model for hydroclimate change projections in central Chile. Results of those evaluations are given in a separate file as a table.

We think that with these changes, and clearer statement of the goals of the paper will improve the manuscript. Please, find below our point-by-point response to the reviewers' comments.

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 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 oC; 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?

Response: The changes in vegetation and land use are not accounted for in these simulations. We agree that the land surface forcing may also modulate the future hydroclimatic regime in this highly anthropized region. However, to face such pertinent question we would need to implement a land-cover/land-use change module in the VIC model and perform new sensitivity runs, which is outside the scope of this study. Yet, this point merits a comment in the revised manuscript, so we will add a discussion on this.

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.

Response: To avoid misinterpretations of change due to annual and decadal-scale variability, we use the standard time frame of 30 years (not decadal) to get climatological regimes across different periods of the 21st century. A trend analysis would be an equally useful choice to assess changes, but we prefer the climatology comparison option to highlight and compare in a simple way near-future, mid-term and long-term projections. We will add a short explanation on this in the revised manuscript.

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.

Response: This main point is partially addressed in our general response above. We have expanded the model evaluation section and added more evaluation statistics such as NSE, KGE, PBIAS (results included in table in separate file). Furthermore, we have included two more stream gauges of Itata and Rapel basins for the evaluation statistics so that we could provide a detailed validation statistic for each basin. As noted by Demaria et al. (2013a), while these evaluation statistics do not demonstrate that the best hydrologic model was developed for each basin, we think that the VIC model generally meets the criteria for satisfactory performance and can be used for climate change impact assessment.

4. In this context, I also miss information on the parameterizations for the VIC model runs in all four catchments (p. 5, lines 20: "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.

Response: We acknowledge your comments in this crucial point. In the study of Demaria et al. (2013a), the VIC model was calibrated to monthly stream flows for the Mataquito basin, then they applied the same VIC-calibrated parameters from the Mataquito basin to the entire domain. In our study, we follow the same approach of Demaria et al. (2013a) and used the same calibrated parameters from the Mataquito basin to the entire domain, and validated for each basin. Granted, the same VIC-calibrated parameters from the Mataquito basin do not resolve explicitly the strong gradients in topography and climate, but detailed calibration and parameterization analysis is not an objective of our present work. Our major goal is rather to analyze the projected changes in hydroclimate in central Chile using the basin averages of the grid-based runoff using the same validated VIC model from Demaria et al. (2013a) with exactly the same model setup and parameterization. Demaría et al (2013a) stated that the the rationale for using the same calibrated parameters for the entire domain is to avoid the possibility of allowing extensive calibration to hide the deficiencies of meteorological forcing fields. Up to date, there is no higher resolution of gridded observation dataset than the dataset of Demaria et al. (2013a). Understanding the limitations of our study, we have expanded the model evaluation part and added more evaluation statistics such as NSE, KGE, PBIAS and validated the VIC model for each basin (results included in table in separate file).

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?

Response: As we have used the same VIC model configuration and parameterization from Demaria et al. (2013a, b), we have refrained from calibration and parameterization in this study. Therefore, we did not validate the soil thickness nor checked the impacts of different soil depths on the results.

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.

Response: These sentences are indeed confusing and will be reformulated. Our point here is that there is no gridded ET dataset to be really considered as "observations-driven". Diagnostic, simulated or satellite-derived ET data may be used as a reference but with cautions, as we try to do in our comparison. We are including other reference dataset (e.g. MODIS) in addition to GLEAM to make our analysis more robust, and have more certainty on the described model biases.

7. I am not totally convinced that the spatial resolution suits well the spatial extent of the basins presented here. A grid of 0.25o seems to be a bit rough. Assuming cells of 0.25o*0.25o 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.

Response: As we note in the general response above, our selection for spatial resolution is merely based on the data availability of meteorological forcing fields at the moment of performing these simulations. Although clearly not desirable for sub-basins analyses, we think the grid used is still useful for the purposes of this study.

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.

Response: We expanded the model evaluation part and we created a table of evaluation statistics (results included in table in separate file) including pearson correlation coefficient (r), ratio of RMSE to the standard deviation of the observations (RSR), percent bias (PBIAS), Nash-Sutcliffe efficiency (NSE) and Kling-Gupta efficiency (KGE). Please note that all these statistics are based on monthly and annual time series. Overall, the VIC model performance is very good in Itata basin and adequate for Maule and Mataquito basins. These results for Maule and Mataquito basins are similar to those in Demaria et al. (2013a, b). On the other hand, the model simulation for Rapel basin shows a poorer performance albeit with low PBIAS and NSE value greater than 0. The reason for this is most probably related with the catchment characteristics of the Rapel basin as it has the highest amount of snow water equivalent among the four studied basins and corresponds to a snowmelt-dominated basin. As noted by Demaria et al. (2013a), while these evaluation statistics do not demonstrate that the best hydrologic model was developed for each basin, we think that the VIC model generally meets the criteria for satisfactory performance and can be used for climate change impact assessment.

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.

Response: Thanks for pointing this out. We agree with your comments. In the revised manuscript, we have applied a 3-day moving window to smooth out anomalously large values and recalculated return periods. Furthermore, the figure provided in Supplementary Materials (Fig. S6) includes 95% confidence interval of the return periods.

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.

Response: The discussion section indeed lacks of content and we will improve it in the revised version.

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.

Response: As mentioned by Ohmura and Wild (2002) the direction of evaporation trend is not determined by temperature alone. Particularly, in snowmelt basins a complex set of feedback mechanisms, due to both warming and precipitation related changes, may have a great impact on the water balance components at seasonal scale. Indeed, although mean annual ET change illustrates a weak tendency of decreasing, our analysis on absolute seasonal changes of water balance components (pg. 9, lines 25 to 35, and pg. 10, lines 1 to 17) indicates that there is a slightly increase in ET in winter and late winter months, due to earlier snowmelt. However, we agree that that section 3.2.2 is not very clear and in the context of previous comment (10), we will expand the discussion on the physical processes that control the changes.

Ohmura, A. and Wild, M. (2002): Is the hydrological cycle accelerating? Science 298, 1345–1346.

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.

Response: As noted in the previous comments (10, 11), we agree that section 3.2.2 is not very clear. Therefore, we will clearly separate the results and discussion with more contents on the evaluation of the different processes.

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.

Response: We will put the corresponding information in 3.2 to the method section. All reported changes (%) are respective to the reference period. We will use one way of spelling.

Thank you for your constructive comments.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-690/hess-2016-690-AC3-supplement.pdf

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