HESS-2011-103: "Climate change impacts on snow water availability in the Euphrates-Tigris basin"

Response to Anonymous Referee #2 Comments
(please see my responses in blue text below)

Major comment:
I fully appreciate the difficulty of performing in situ validation of modeled SWE estimates in this part of the world. Given this difficulty, however, it would be valuable if the author could provide one or more evaluations of other hydrologic variables against available in situ data, in order to assess the performance of VIC w.r.t. simulation of the overall water balance. A comparison of VIC streamflow with gauged runoff would be one option.

I agree with this reviewer on validating VIC outputs for other variables in the basin. I was able to find monthly discharge data for the Logmar station [38.52N 38.49E] in the upper reaches of the basin for the baseline simulation years and plotted them in a new figure (Figure 7) in the revised manuscript. In general there is good agreement between modeled and observed discharge, especially the timing of peak discharge that is so important in the context of snow melt and associated runoff.

Additionally, this reviewer would appreciate seeing a table or figure that evaluates the downscaled meteorological fields against available in situ station data. My confidence in the model's results under both present and future climate would be enhanced if we could see that the approach for estimating local meteorology is robust.

Please note that I would not use "estimated meteorology" to describe the meteorological data used as forcing in this work as no meteorology was estimated but used as direct inputs. Nevertheless, I agree with the reviewer that comparing NCEP reanalysis and station level data is important to see if there are major discrepancies. So, there is a new table (Table 3) in the revised manuscript that shows this comparison for one station in the basin in Turkey (Diyarbakir station). Please note that station level data was extracted from the National Climatic Data Center's summary of the day records which themselves are extracted from WMO stations.

Minor comments:
Abstract, L12: suggest "high-impact A2 climate change scenario" rather than "aggressive A2 climate change scenario."

I changed the wording according to the suggested form.

p. 3634, L2-3: A reduction in snow pack doesn't necessarily mean a significant reduction in water downstream. The largest impact is a change in seasonality, which would have implications for storage requirements and dam operation. The issue is addressed nicely in the discussion, so the author might rephrase here accordingly.

I agree with the reviewer here. I changed the wording and discussion here in the revised

manuscript to incorporate effects on storage and seasonality.

p. 3638-9: Please clarify how VIC elevation bands were applied. It seems that NCEP fields were downscaled to 1/8 degree using elevation correction and a bilinear interpolation. Does VIC introduce further subgrid variability, or was this the extent of downscaling?

This was the extent of downscaling of the forcing data and VIC does not introduce additional variability. However, what it does is to distribute snow and cold land processes according to the characteristics of the elevation bands. Please note that this distribution is non spatial – i.e. we don't know where in the elevation band the snow accumulation/ablation occurs – we just know the average distribution by elevation band according to the characteristics of the elevation bands supplied by the user. I described this in detail in the later paragraphs in the revised document.

p. 3640, L16: Do I understand correctly that this approach assumes stationarity in sub-monthly variability? If so, please note the assumption. If not, please indicate how non-stationarity is taken into account.

Yes this assumption is correct and is indicated for the reader in the revised manuscript.

p. 3644, L5: why are only 12 out of 13 shown? If this is simply a space constraint, then you might remove the statement from the text and simply note it in the legend of the figure (perhaps explaining that you dropped one model b/c it's so similar to another model). If you've removed an outlier, then please explain the rationale.

Space is exactly the constraint for choice of showing 12 out of 13 models in this figure. I removed the statement from the text and moved it to the legend accordingly. An outlier was not removed but a model result similar to another one (MRI_CDCM2.3.2) so removing one figure did not make much difference.

p. 3645 L12: I find this sentence confusing. Is the point that accumulation, which occurs primarily in Dec-Jan, is more impacted by climate change than residual melt, which has a larger impact on April SWE? Please clarify.

What I describe here is that SWE – which is defined as the amount of water in the snow if it were melted – decreases more in the winter period than in the spring period under the climate change scenarios (as measured my the ensemble model outcome here). So perhaps, the way it is written in that sentence in the original manuscript where the word accumulation is used is confusing. I changed this sentence to read more clearly and removed the accumulation/melting references.

p. 3645 L 24: Please explain the absence of model consensus in April. Is this a temperature effect? A precipitation effect? Is it simply b/c there's very little snow in April?

This is an important point – this is mostly due to lack of snow and thus a clear picture of how much reduction occurs is lost between model variability in the spring. In the winter period however, all models do predict snow availability but simply less of it under a changing climate. I included additional text to clarify this point.

Figure 5: Please clarify what we're seeing in the top panel. If it's a monthly MODIS product, then why does snow cover vary with each 8 day interval?

The MODIS data at the top panel is not monthly but weekly (8-days) composite (MOD10C2). The title in the caption of this figure is wrong and is now been corrected in the revised manuscript. The dates at the top refer to the beginning date of each 8-day composite.

Figures: It would be helpful to have at least one change figure that shows the absolute changes in SWE, not just percentages, in order for the reader to evaluate the actual impact of model uncertainty w.r.t. water resources. Figure 4 provides some guide in this respect, but it is difficult to read monthly values off of that plot with precision.

I agree with the reviewer here and note that there was a similar comment from another reviewer. For this reason, for every scenario SWE change in the new Figures 8 and 9, I also included text number at each month that shows the maximum change for that month among the 4 scenarios considered. The unit of measurement on these figures is mm and must be interpreted in the context of relative change.