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

Response to Anonymous Referee #1 Comments
(please see my responses in blue text below)

Major comments:
1. The evaluation of the baseline conditions model is based on 5 years of SSM/I SWE depths and visual inspection of snow cover extents for 4 days. A more comprehensive presentation of the model evaluation including additional performance statistics associated with the two evaluation forms are suggested.

To provide a better assessment of model performance I included a new data set comparison of observed vs. modeled SWE data (Figure 4 in the revised manuscript). The new SWE data were obtained from the Turkish Electrical Works Directorate (EIEI) snow cover statistics yearbook.

I also provided a new table (Table 2) that shows a set of statistics that describe the strength of the relationship between the modeled and observed quantities.

Based on suggestions from other reviewers, I also included a new comparison figure for river discharge between gauge observed and model generated quantities (Figure 7 in the revised manuscript). While the statistics for this comparison are not included in a separate table, I provide a Nash-Sutcliffe model-observations comparison metric on the figure and in the text.

[Page, 3641, line 18] Is there no opportunity to evaluate the model SWE depths with insitu data? Remote sensing data should be "ground-truthed" to some degree wherever possible. The estimated errors for the SSM/I and MOD10C2 satellite datasets should be presented.

To assess the quality of VIC-derived SWE predictions, I made a new comparison to ground-observed SWE data for one location in the northern portion of the basin (Table 2 and Figure 4). In general, this comparison points to a strong correlation between modeled and observed quantities. Having the confidence that VIC is able to produce reasonable estimates of SWE, I then went ahead and kept the remote sensing comparison as is.

Note that while I agree with the reviewer that caution must be exercised when model results are "validated" with remotely sensed data that was put through a model, the comparison still provides a reasonable assessment of model performance. Two reasons support this. First, at the resolution of this work (1/8 degrees), which is considered coarse, the remote sensing estimates are very reasonable. Second, the strong correlation of the magnitude and the timing of SWE estimated by two completely different methods (one with VIC and the other semi-observed with remote sensing) suggests that the model is doing a pretty good job explaining the variability in SWE especially at these coarse grid scales.

[Page 3644, line 1] How do simulated and observed snow cover areas compare? What is the mean bias and is it consistent across evaluation dates? If the VIC model is consistently overestimating for baseline conditions, then there is likely to be a positive bias at lower elevations. Therefore, there is more snow cover to lose during future conditions, thus the magnitude of the declines may be exacerbated. One of the major conclusions of the paper is the snow decline at lower elevations.

I did not compare modeled vs. observed snow covered areas as these were in image format only. However, I compared SWE quantities with two different independent datasets (please see above) and both show strong correlations between modeled and observed SWE. Note that accurate SWE predictions by VIC are only possible by getting the elevation factor correct. In other words, VIC uses a elevation band concept that divides the mountainous areas into bands of elevation and computes snow-related variables separately for each band. I am confident that band-specific SWE and other snow variables are computed accurately because the overall value for a grid cell (which is obtained by area weighted sum of SWE over all elevation bands) which was used in the comparisons in Figures 4 and 5 seem to be correct.

[Page 3660, Fig 4] Does an XY plot of satellite vs simulated SWE depths show any general biases? Is the model performing well over all snow depths? The period of comparison is short however there seems to be a slight negative SWE depth bias.

While not shown in the revised manuscript there are no important positive or negative biases between modeled and observed SWE quantities at a single site. I chose not to include this figure in the revised manuscript for consideration for space but included a section that discusses the results of this comparison as additional point of information. Please see Table 2 for additional statistics.

Minor Comments:
1. [Page 3632, line 16] Inconsistent hyphenation of Euphrates-Tigris basin

I fixed this problem in the text so it is now consistent throughout the text.

2. [Page 3633, line 16] The idea that runoff to rivers will reduce if snow storage declines is misleading. The decline of snow will reduce snowmelt contributions to runoff and timing of runoff events but not necessarily overall runoff volumes.

I agree with the reviewer. Another reviewer also had a similar comment. I revised the section here to correct statement about the timing and volumes.

3. [Page 3634, line 28] For clarity and readability this could read “1st April”

Yes and I made the change to reflect this.

4. [Page 3635, line 13] For readability this could read “Using a super-high-resolution”

Yes and I made the suggested change.

5. [Page, 3640, line 7] The spatial distribution of future climate temperature perturbations are dependent on terrain. What is the grid resolution of the terrain data used? Is it fine enough to reasonably represent the terrain?

All simulations were done at 1/8 degree resolution regardless of the original resolution of the input data (which were different for baseline and climate change scenarios). The only topographic correction to baseline data was the PRISM approach mentioned in the paper. Other than this adjustment, the effect of terrain on temperature and precipitation perturbations were not taken into account. For this reason, entire basin SWE comparisons rather than individual grids comparison are shown and discussed in the paper.

6. [Page 3645, line 13] Consider rewording sentence

This change was made.

7. [Page 3645, line 15] There are no absolute SWE estimates presented in Figure 7, only percentage change from the baseline, so it is difficult to confirm that "little or no SWE will occur in the Spring". Figures 6 and 6a show more disagreements in the sign of the change for the 12 individual models in April than for other months. Combining these for the multi-model mean induces a canceling out and results in much smaller changes in SWE from the baseline conditions in April as shown in Figure 7.

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 Figure 8, 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/month and must be interpreted in the context of relative change.

8. [Page 3646, line 2] Commentary on Figure 5 suggests that the VIC model has a positive spatial snow bias indicating that the VIC model errors are largest at lower elevations. Is there a correlation between model errors (deviations from the observations) and the resulting future SWE response when considered for each separate elevation band? Can the positive bias of 100mm in NCEP reanalysis precipitation data, be attributed as the source of snow accumulation errors.

This is an important comment. I did some preliminary analysis and determined that there is indeed a slight positive bias in SWE predictions in lower elevation zones. For this reason, I included some text in the revised document to caution the reader in the section immediately before the discussion.

9. [Page 3661, Figure 5] A scale would be useful in this figure to convey the size of the snow areas for those not familiar with the region.

I added a scale bar to help reader judge the scale and the size of the area.