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Hydrology and Earth System Sciences Discussions

Interactive comment on "Climate change impacts on snow water availability in the Euphrates-Tigris basin" *by* M. Özdoğan

Anonymous Referee #1

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This review has been prepared for the paper entitled 'Climate change impacts on snow water availability in the Euphrates-Tigres basin' by M. Ozdogan. The paper presents modelling results for the potential impacts of future climate change for a range of emission scenarios on snowmelt water resources for the upper Euphrates-Tigres basin for 2050 and 2090. The main findings from the study included a reduction in snowpack during early winter (December-January) with largest reductions occurring at lower elevations (< 500m) and a shift in the timing of snow accumulation to later months.

The manuscript contributes original distributed snow modelling simulations and data for the complex terrain of the Euphrates-Tigres region with a focus on snowmelt impacts from a resource perspective for a range of climate scenarios providing a good level of

scientific significance.

The modelling approach involves the generation of synthetic timeseries by applying perturbations to historic climate timeseries data which are then used to force the hydrological model, this is an appropriate method that is commonly used for assessing the off-line (uncoupled) impacts of climate perturbations on simulated snowpack resources. While the study is of good scientific quality, a more thorough evaluation of the snow model particularly at low elevations would add strength to the conclusions for these areas.

The manuscript reads reasonably well and has a clear logical structure and progression. The figures are easily interpreted. Overall the paper uses a good quality presentation.

I recommend the manuscript e accepted with minor revisions.

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.

[Page, 3641, line 18] Is there no opportunity to evaluate the model SWE depths with in-situ 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.

[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

HESSD

8, C1461–C1464, 2011

conclusions of the paper is the snow decline at lower elevations.

[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.

Minor Comments:

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

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.

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

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

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?

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

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.

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

HESSD

8, C1461–C1464, 2011

band? Can the positive bias of 100mm in NCEP reanalysis precipitation data, be attributed as the source of snow accumulation errors.

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.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 3631, 2011.