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Hydrology and Earth System Sciences Discussions

Interactive comment on "Regional effects of vegetation restoration on water yield across the Loess Plateau, China" *by* X. M. Feng et al.

Anonymous Referee #1

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This paper attempts to quantify the regional-scale impact on water yield due to vegetation change during the Grain-for-Green (GFG) reforestation campaign, starting in the late 1990s, over the Loess Plateau of China. They separate vegetation effects from climate effects to determine their relative importance to water yield. For the purposes of this study, water yield was defined as precipitation (P) minus evapotranspiration (ET), so the authors also develop an annual and monthly ET model for the region. The question of what role vegetation change plays in changing water yield with and without climate variability and change at a regional scale is relevant to the field o hydrology in general and is within the scope of HESS. The ET model developed here is relatively novel, and the results are quite interesting and provide additional information necessary in understanding hydrologic response to afforestation.

The paper is generally well-written, though there are several minor errors that I assume will be caught in copy-editing (now that copy-editing is standard). The title clearly reflects the contents of the paper. Though clear, the abstract suggests that the authors will compare regional to plot scale results directly, which does not happen in the paper. The structure of the paper makes sense overall; however, I think that the description of the ET model on p. 4171 (lines 12-22) should be moved to the methods section.

I have two basic concerns with this paper.

The first concern is that the authors state: "We assume that the climatic effect on water yield during the post restoration period is the difference between Scheme 2 and Scheme 1 with recognition that climate-land cover feedbacks during the entire study period" (p. 171, lines 6-9). Because of the non-linearity in hydrologic response, it is highly unlikely that the difference between scheme 2 and scheme 1 gives the climatic effect on water yield. It gives the climatic effect on water yield given the observed changes in vegetation. Simply rephrasing this would suffice. I also suspect that the land cover change was impacted in part by climate, i.e., rates of growth respond to climatic conditions, such that even the fixed climate simulation includes some degree of climate response. I do not expect the authors to be able to deconvolve this response; however, I would like for them to address in the paper the degree to which they might expect this to impact their results. One approach that would help to clarify the climate vs. vegetation impacts would be to do a simulation with fixed vegetation and variable climate.

The second concern is that the authors do not perform any significance testing on their trend results. In general, I think that the conclusions presented are justified by their results; however, if the trends in water yield are not statistically significant or if the difference between the fixed and variable climate cases is not statistically significant, the conclusions may not be justified. The paper discusses regional versus point-scale estimates of afforestation impacts on water yield, but the results are all given at individual grid cells. Although the spatial variability is quite interesting, it would be worthwhile to

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also report the basin wide trends.

Specific comments or questions related to the science:

p. 4170, line 4: The break in the data sets for different vegetation sensors coincides with the timing of the GFG. What impact could this have on the results?

p. 4171, lines 8-9: The response is likely non-linear. Why not run a simulation with observed climate and fixed vegetation? Also, what is the anticipated impact of climate on the observed vegetation changes? (as commented above)

p. 4173, lines 7-9: What type of trend test did you perform? And which "regression statistical tests" did you perform?

p. 4176, lines 4-8: Give the % decrease in runoff from this study over the Heihe catchment as a point of comparison.

p. 4177, lines 15-18: What mechanism explained higher baseflow rates despite higher ET in the southeastern U.S.?

Figure 4: In 2006 (average year), modeled ET is almost always higher than precipitation. Please comment on reasons for this in the paper. Also, the font on the axis labels is hard to read.

Figures 5 and 6: Which, if any, of these trends are statistically significant? I'd rather see the regions with insignificant trends masked out. Also, it would make more sense when reporting % of basin area with decreases in water yield to report % of basin area with statistically significant decreases in water yield. Where were the differences between vegetation change only and vegetation change with climate significantly different? A map showing the difference in water yield trend between scheme 1 and scheme 2, masked out in areas where differences are insignificant, would provide a good summary of these results.

More specific comments:

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This section of the review is meant to point out places in the manuscript where methods or interpretations are not entirely clear as written. In these cases, simply re-writing or adding a bit of detail will address the question. p. 4168, lines 14-16: change “Where, Ld is the average daytime length of each month, RHOSAT is the saturated vapor density (g m-3) at the mean air temperature (T); and where T is the monthly mean air temperature (âŮęC);” to: “Where, Ld is the average daytime length of each month, RHOSAT is the saturated vapor density (g m-3) at monthly the mean air temperature (T, âŮęC);”

p. 4169, line 8: It would be much more intuitive to call “Watershed-ET” method something more like “Water-Balance-ET.”

p. 4170, line 4: should be NDVI, not NDIV.

p. 4170, line 23: should be “ET is affected by both land cover and climate change, and, therefore, so is water yield.”

p. 4171, lines 2-4: rework for clarity: “. . .the water yield model was run with a fixed climate of 1999, when the GFG project was first initiated, and land cover in the model was changed gradually during 1999–2007 to mimic the observed changes. . .” Was the vegetation in the fixed climate model the same as in the second model?

p. 4174, lines 3-5: Early in the paper, the authors state that 1999 is the pre-GFG period, and 2000-2007 is the post-GFG period. When calculating relative change, do you use 2000-2007 average minus 1999 water yield? Or is “relative change” the trend in water yield over the period 1999-2007 divided by the baseline?

p. 4174, lines 11-12: “Temporal water yield change for the entire Loess Plateau region was defined as the water yield amount in post-vegetation restoration period minus that under the baseline condition (i.e., in 1999).” Does this mean “Temporal water yield change for the entire Loess Plateau region was defined as the water yield amount in each year of the post-vegetation restoration period minus water yield under the baseline condition (i.e., in 1999).”?

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p. 4176, lines 20-24: Grammar on “Regional veg- etation restoration, especially in water-limited northwest Loess Plateau areas, should consider long term climate trends so the influence of land cover and land use change on water availability by humans and ecosystems (i.e. water use by plant and aquatic biota in streams).” Try something like: “Long-term climate trends [and variability?] should be considered when planning regional vegetation restoration, especially in water-limited northwest Loess Plateau areas, so that the land cover and land use changes do not limit water availability beyond what can sustain human and ecosystem uses.”

p. 4176, line 27: I think “flush stormflow” should be “flash stormflow.”

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 4161, 2012.