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Discussions

Interactive comment on "Reconciling the dynamic relationship between climate variables and vegetation productivity into a hydrological model to improve streamflow prediction under climate change" *by* Z. K. Tesemma et al.

Anonymous Referee #2

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This manuscript presents the VIC model results under climate change scenarios for 13 different watersheds in southeastern Australia. Based on their simulations, the reduction in water yield was (or will be) mitigated by the vegetation responses to hydroclimate changes (warmer). Although the manuscript presents an interesting point and is worthy of publication (somewhere), I am not sure it rises to the level of a HESS paper. Some of the results are quite obvious to me (the mitigation role of vegetation in climate changes). It could have been greatly improved by incorporating much more details in several sections, particularly the model description, results, and their inter-

pretation. Especially, I cannot find any further or in-depth discussion in the manuscript, which makes me feel more like reading a modeling exercise rather than a paper. My recommendation comes w/ three caveats:

First, I'm a bit concerned about overlaps with two papers listed in references (Tessema et al. 2014a and b). It seems that the LAI models and predictions were already covered by the first paper, and the modeling part (calibration and validation) were presented in the paired paper (Tessema et al. 2004b). In these kinds of scenario-based hydrological simulations, downscaling and bias correction processes would be most interesting to many readers (Hay, L. E., et al. "Use of regional climate model output for hydrologic simulations." Journal of Hydrometeorology 3.5 (2002): 571-590.) . However, I cannot find any merit about those processes. The presented downscaling process seems like a simple data generator based on the baseline climate data rather than actual statistical downscaling. It seems that the study site is located along the strong orographic gradient, however this factor was completely ignored in those processes. Check this paper (Praskievicz, Sarah, and Patrick Bartlein. "Hydrologic modeling using elevationally adjusted NARR and NARCCAP regional climate-model simulations: Tucannon River, Washington." Journal of Hydrology 517 (2014): 803-814.). They used a topographic correction of regional climate-model data for modeling the hydrology of mountainous basins for simulating hydrology under past or future climates. With the current downscaling method (I am not sure I can say 'downscaling'), the predicted scenarios would be too much constrained by the baseline climate data, and will only produce averaged responses from GCM models. 2.2.2 section definitely overlaps with Tessema et al. 2014a. 2.2.3 session is about how to deconvolve the simulation results into CC and vegetation effect. What are the unique methods and equations in this manuscript? I briefly read the first paper in review. I am not sure whether this manuscript can be a stand-alone paper in a current form.

Second, the manuscript starts with the critiques of stationarity assumption in future hydrological simulations (P10595 L24). I totally agree to this point in that the traditional hydrologic modeling has often ignored the importance of vegetation response during hydrologic regime changes. Many papers related to climate changes have mentioned the importance of vegetation in mitigating the effect of anthropogenic CO2 emission and resulting temperature increases. I think that the authors should have written in-depth discussion regarding this point. However, it would be also the same problem to use the equation 5 for the prediction of LAI values in the future. It is naive to predict LAI values in 100 years only with 6-9 months P - PET deficits. Leaving nutrient and CO2 issues aside, the authors assumes the constant PFT (plant functional types) for their simulations. However, tree lines will definitely move upward with warmer climate. I am sure this constant PFT assumption led to the conclusion that ET would decrease and soil remain wetter even with warmer climate (P10608 L5), which I cannot agree to. The constant PFT assumption would decrease LAI values for tree dramatically, which might result in wetter soils with warmer climate. However, you would never get wetter soils under warmer climate. Rather, all trees would die off due to drought stress, and be substituted by other drought tolerant species.

Third, I am not comfortable with the equivalence between LAI and productivity. Throughout the manuscript, those two terms were assumed as the same, but it is definitely not. Hydrologists often made the same mistake (e.g. RodriguezâĂŘIturbe, I., et al. "On the spatial and temporal links between vegetation, climate, and soil moisture." Water Resources Research 35.12 (1999): 3709-3722). Although LAI can be a result of accumulated productivity through allocation of photosynthates, the allocation ratios between above and belowground would be quickly responding to water and nutrient availability (Litton, Creighton M., James W. Raich, and Michael G. Ryan. "Carbon allocation in forest ecosystems." Global Change Biology 13.10 (2007): 2089-2109). This allocation process should be understood under the optimality principle for the compromise between different resources (light and water/nutrient). For example, this would lead to the conclusion that the vegetation with the same LAI values would have the same productivity regardless of their locations and climates, such as semiarid and tropical environment. This is why most remote sensing based models incorporate different en-

vironmental constraints, such as VPD, temperature, ET/PET etc., to convert LAI values to NPP/GPP terms (e.g. MODIS GPP/NPP), rather than using a constant radiation use efficiency value. Please remove the productivity term throughout the manuscript.

Specific comments P10596 L9-12: This sentence is not clear to me. P10596 L11: Please do more literature reviews. There are tons of papers that examines the relationship between vegetation water use and streamflow generation under climate changes especially in Mediterranean climate regions (e.g. Walko, Robert L., et al. "Coupled atmosphere-biophysics-hydrology models for environmental modeling." Journal of applied meteorology 39.6 (2000): 931-944). Check the recent papers from Dr. Christina Tague at UCSB. Equations 6 and 7; Qclim, Qnet, and Qlai are confusing because they look like the water yields, but actually percent terms. Change those. P10608 L3-5: This is the most controversial result from the paper. I cannot agree. Do you need Table 2 to Table 5. Nobody would read those.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 10593, 2014.