We thank the reviewer for considering our manuscript and our response (in blue) to their comments (in black) are provided below. We propose to implement most of the major changes suggested by the reviewers. In the few cases where we do not agree we explain our reasoning.

Responses to major comments of Reviewer #2

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 interpretation. 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:

Noted – we respond to these concerns below.

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.

Partially agree. The unique contribution of this manuscript is that we examine the relative effects of direct climate forcing (rainfall, atmospheric ET drivers) and direct climate forcing combined with climate induced LAI change on runoff under changed climate scenarios. Comparing these enables the LAI effect to be separated out. Most studies to date have looked at either only the direct climate forcing effects or only the combination of climate forcing change coupled with vegetation change. Specifically, our study was done by coupling the LAI-Climate model developed in Tesemma et al. (2014a) into the VIC hydrologic model and assess the impact on catchment runoff of how LAI is modelled (constant seasonal LAI or LAI varying in response to climate) under changing climatic conditions. We investigate two sets of changing climatic conditions: (1) the observed Millennium Drought, which is a persistent (>10 year) large change; and (2) projected climate change for both wet and dry sub-catchments. Our results suggest that modelling LAI in a way that responds to changing climatic conditions is important for modelling runoff during drought and projected climate change. We believe this paper makes a significance contribution to the existing body of knowledge and is a stand-alone paper.

Nevertheless, we do agree that we need to make this clearer in the introduction. We also agree that we need to provide more details about the model description, downscaling methodology, results, and their interpretation so that the significance of this work is more apparent. See our response to Review #1 who also made a similar request for more detail.

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 CO_2 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 CO_2 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.

Partially agree. We agree with the reviewers concern about changes in plant functional types (PFTs) and we discussed our assumption that PFTs did not change in the manuscript. We make this limitation clearer in the paper and will add a comment about timescale of adjustment. Notwithstanding this, we note that our LAI-climate relationships were developed in a region that experienced a ten year drought (2000-2009, called the “Millennium drought”), which is comparable to projected climate conditions under the highest CO_2 emission scenario. The observed Millennium drought makes this study very interesting because we have a chance to see how vegetation responded to such severe water stresses under a prolonged (ten years) climate change. We believe our LAI-climate relationships developed under extreme drought conditions could reasonably represent how LAI may change under comparable anticipated changes in future climate. Furthermore, most over-story trees in our study area are Eucalypts and while some movement of boundaries between dominant species may be expected, water use characteristics are likely to be relatively similar and there is not sufficient information to represent species specific details of either migration or water use.

In addition, it is know that in Australia vegetation growth is highly controlled by precipitation (water supply), and is less controlled by temperature and radiation (Nemani et al. 2003). Hence, most vegetation dynamics can be explained by variation in climate, which formed the basis of the LAI-climate model developed in Tesemma et al. (2014a). We acknowledge changing CO_2 levels could influence vegetation growth, but to a smaller extent than climate does. Finally, while the reviewer has mentioned possible changes in PFTs under climate change, in our study area PFTs are strongly influenced by land use (human activities) such as forest clearing for agriculture, which are difficult to project into the future. It is likely that issues such as fire regime changes (Heath et al., 2014) and changes to forest age (Cornish and Vertessy, 2001) which affects water use would dominate over differences between species. We will acknowledge these limitations in the revised manuscript.

We will revise the manuscript to emphasise the unique opportunity that the Millennium drought has provided to investigate this issue. We will include a discussion of these issues in the introduction section to help readers to be aware of the assumptions made in this analysis at the beginning of the paper, and deepen our discussion section as well.

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. Rodriguezâ˘AR˘ Iturbe, 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 environmental 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.

Agreed. We will replace vegetation productivity with LAI throughout in the revised manuscript.

Responses to specific comments (S.C) of Reviewer #2

P10596 L9-12: This sentence is not clear to me.

Agreed. We will revise the whole sentence for clarity.

P10596 L11: Please do more literature reviews. There are tons of papers that examine 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.

Agreed. We will expand the literature reviews and include those papers mentioned-above in the revised manuscript.

Equations 6 and 7; Qclim, Qnet, and Qlai are confusing because they look like the water yields, but actually percent terms. Change those.

In case where some catchments are wet and some catchments are dry, the percentage is preferable to use which allows comparison across catchments.

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.

Agreed. We will move the detailed results provided in those tables to Supplementary Material and convert the results in the tables into figures that are easier to follow.

References:

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Nemani, R. R., Keeling C. D., Hashimoto H., Jolly, W. M., Piper, S. C., Tucker, C. J., Myneni, R. B., Running, S. W.: Climate-driven increases in global terrestrial net primary production from 1982 to 1999, Science, 300, 1560-1563, 2003.

Tesemma, Z. K., Wei, Y., Western, A. W., and Peel, M. C.: Leaf area index variation for cropland, pasture and tree in response to climatic variation in the Goulburn-Broken catchment, Australia, Journal of Hydrometeorology, 10.1175/JHM-D-13-0108.1, 2014a.

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