# Review of “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, Y. Wei, M. C. Peel, and A. W. Western

**Summary**

The manuscript from Tesemma et al. presents an analysis of the influence of vegetation productivity on streamflow prediction under climate change. The study combines the LAI (leaf area index) with the variable infiltration capacity (VIC) model to predict the streamflow in thirteen gauged catchments within the Gaulburn-Broken catchment, Australia. The model was applied during the “Millenium Drought” (2000-2009), and two future periods (2021-2050 and 2071-2100) for two different emission scenarios. The future climatic and streamflow outcomes are compared with the historical period 1981-2010. This period includes the “Millenium Drought” which Tesemma et al. also used as a model run. For determining the change in climatic conditions, 38 simulations from 15 Global Climate Models are used. The region of the study is projected to be warmer and drier in the future. The subject is of relevance to the journal of Hydrology and Earth System Sciences given the implications for streamflow predictions globally. Previous studies in this region have focused on the link between climate change (warming, decreasing precipitation) and streamflow, and concluded that runoff would decrease with the ongoing change. Other studies, like Ellis and Hatten (2008), focused on the relationship of vegetation productivity and climate change. This study from Tesemma et al. is pioneering in the relation between vegetation productivity and changing climate and the influence on runoff. This extra step was not taken into account in previous studies and makes this manuscript of relevance.

The results of the study provide evidence that vegetation has a significant effect on the prediction of streamflow with changing climatic conditions. The study helps in optimizing the runoff predictions and gives insight in the complex processes that influence catchment streamflow. The manuscript is easy to read and written in clear language. The presentation of the results, with the large amount of tables, is less clear and makes it somewhat difficult to easily find the most important outcomes. I have some comments and suggestions to improve the study and paper, with the main focus on the possible influence of nutrients and vegetation dynamics on the LAI model and the abundant information in the tables. Also I feel that the inclusion of the Millennium drought in the baseline period, as well as not using a low emission scenario should be defended better. Overall, I recommend to accept the manuscript for publication with minor revisions.

**General comments**

1. Influence of nutrients and vegetation dynamics on the LAI prediction

The process of creating the model to calculate the LAI related to climate variables is described in Tesemma et al. (2014a). This model contains only the precipitation and evaporation as forcing variables. In the discussion of the manuscript, nutrient limitation is mentioned as a factor that can

stop the increase in LAI because of rising $CO_2$ concentrations. This statement is not further used in the manuscript, and not taken into account when modelling the LAI. Net ecosystem production (NEP) is a complex measure, and the main drivers are still partly unclear. There is a relation between nutrient availability and NEP (Fernandez-Martinez et al., 2014) and also direct impacts on ecology (forests) and agriculture or croplands (Fisher et al., 2012). The relation between LAI and forcing is determined spatially, but subsequently used within a temporal framework. Nutrient availability will vary spatially, but it is very difficult to assess the influence of nutrients in that area in about 100 years. The question that emerges is if the relation found to calculate the LAI is still reliable in the selected future periods with nutrient limitation as a possible influencing factor (Fisher et al., 2012). Something to think about besides the nutrients, is the vegetation dynamics. The vegetation dynamics are important to take into account when working with hydrological modelling, because it has a direct influence on latent and sensible heat fluxes and changes in climate (e.g. Montaldo et al., 2005). The dynamics in time are not discussed or included.

The nutrient limitation and vegetation dynamics are difficult to take into account for the model because it is unknown how the area will develop in the future. However, these factors can influence the effect of vegetation productivity in estimating streamflow. When these factors are not modelled, a more elaborated discussion is necessary. I would recommend a discussion part about why these factors are not modelled and how the factors can influence the outcomes and conclusions.

2. Abundant information in tables

There are many tables shown in the manuscript. These tables contain a lot of information, with outcomes for every sub catchment and month. All this information is confusing and not useful in understanding the manuscript and outcomes, and most of the results are not explained or referred to in the text at all. Also, no table is presented with the mean values, these numbers are only mentioned in the text and shown in the figures. I would suggest not to show all those tables, but just make tables with the mean outcomes, as a support to figures 5 and 6. The values of months with “special” outcomes, like January and February, are also worth showing.

3. The “Millennium drought”

”The “Millennium drought” is used for one of the two model runs (page 10601), because it is expected that this may be representative for the future conditions (page 10595). This period is also part of the used “historical data” from 1981-2010, as comparison with the future periods (2021-2050 and 2071-2100). The drought period from 1997 until 2009 is more than one third of the historical data, and this can eventually influence the outcomes. A specific outcome was for example that all the seasonal precipitation simulations showed a shift to a drier climate, except for the summer months January and February. The monthly precipitation increased with 1 to 5%. It is very well possible that not including the millennium drought in the comparison leads to less increase or even decrease of the precipitation in these months. The results of the study may become more significant when the drought is not part of the historical comparison data. It is a discussion point if this time period must be used for comparison and I would suggest an extensive discussion about the influence on the results. Another comment on the “Millennium drought” is that two different periods are mentioned in the manuscript. The abstract mentions the years 2000-2009 and the introduction and approach mention the years 1997-2009. What is the correct timespan?

4. Low emission scenario

In the study, two Representative Concentration Pathways (RCP's) are used for future modelling, RCP4.5 and RCP8.5. There are four RCP's (2.6, 4.5, 6.0 and 8.5), four pathways for future climate modelling. The RCP's are a product of collaboration between many parties and consist of comprehensive datasets (Vuuren et al., 2011). RCP8.5 is the highest emission scenario, and 4.5 is a medium emission scenario. In the manuscript is not clearly mentioned why these two scenarios are chosen for the study. I understand the decision, and RCP6.0 is not useful, but I would suggest also the use of RCP2.6 to create a comparison between high and low scenarios. RCP2.6 contains intervention of humans and mitigation decisions (Vuuren et al., 2011a). Although this scenario is not the most likely to occur, it brings new insights for further research. It is important to know what can happen with mitigation and adaptation with respect to future climate, and this study could have been a perfect way to test the influence on streamflow. Adding this to the study is not necessary, but this is something to consider in further research. I also recommend a more extensive explanation why RCP4.5 and 8.5 are chosen.

5. Qclim, Qnet, Qlai

The derivations of the formulas 6, 7 and 8, for calculating Qclim, Qnet and Qlai, are nowhere mentioned in the manuscript or the references. The equations are popping up in part 2.2.3, without any announcement or reference. Also, the equation for Qlai is different from the equations of Qclim and Qnet, because you divide by the first term and not the second term of the numerator. There is no explanation for this. In addition, the outcomes of these three terms are shown in the tables, but not mentioned in the results or conclusion. This is a serious ommission because these are the values that the conclusions are based on. These results should be discussed in the text.

6. Variable monthly LAI

A useful decision was to consider variable monthly LAI instead of mean monthly LAI. Tang et al. (2011) found already that climatic vegetation conditions are not enough to capture temporal variations in LAI. In that paper, interannually variations are described. This is of course a different time scale, but shows the importance of considering variable LAI.

7. Land use change

The land use change is not used in the study, even though the VIC model can take it into account (page 10601). I understand the decision because it is a difficult extra step requiring inclusion of land use scenarios. Besides that, you already have outcomes based on trees, crop and pastures. It is shown that land use change has a big influence on streamflow, but that is directly linked to vegetation dynamics and LAI. For future predictions, land use change is very difficult to include, because temporal variations or trends are very uncertain (Gerten et al., 2008). While I understand the decision of not including it, the possible effects should at least be discussed.

**Specific comments**

Figure 5; two different variables are shown in this figure, but the y-axis looks like one continuous axis. Besides that are the bars of the two variables only separated with colour. To improve this figure,

I would suggest two different y-axis, so separated, and the bars plotted next to each other so it does not look like one continuous bar.

Figure 6; This figure has the same errors as figure 5, and I suggest the same improvements.

Page 10601, line 24/25; The equations 6, 7 and 8 are just popping up in the manuscript, without any announcement and introduction sentence. Announce the equations for example with: the equations for Qclim, Qnet and Qlai are shown below. Or do it the same way as with equations 1, 2, 3 and 4.

Page 10600, line 5; Equation 5 is just popping up without any announcement. Same holds here, as for equation 6, 7 and 8. Announce the equation in a general way, like equation 1, 2, 3 and 4.

Page 10603, line 3-7; This sentence is very long, with a lot of comma's. I would suggest to break this sentence in two different sentences, one for the precipitation and one for the temperature. This makes it easier to read and understand.

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