We thank the reviewers for their valuable and useful comments on this manuscript. We believe that their suggestions will further improve our manuscript and we can address these comments in the revised manuscript. These comments are in line with the complexity of the problem this paper seeks to discuss, and we feel highlights the importance of the paper as a means of adding clarity on how hydrologic models change in the changing world we live in. Please see below our response to each of the reviewers' comment.

Anonymous Referee #1

This is an interesting study on the ongoing problem of understanding hydrological nonstationarity. I like the work, but I am unclear regarding the robustness of the results as discussed below.

1. The introduction is well written. I wonder whether there are two other relevant links to be made here. (a) To work on streamflow elasticity (e.g. http://engineering.tufts.edu/cee/people/vogel/documents/climate-elasticty.pdf), and (b) on classification approaches trying to assess nonstationarity (e.g. http://www.hydrolearth-syst-sci.net/18/273/2014/). I think these two previous approaches might be interesting to connect with here since they both found that a lot of the variability in runoff ratio was difficult to explain and predict.

We agree with the reviewer comment to provide a link between the streamflow elasticity approach and the methodology presented here in the revised manuscript. Indeed, normalized sensitivities of runoff ratio to precipitation and fractional vegetation cover in Figure 3a is indicative of elasticity of runoff ratio to changes in precipitation and fractional cover respectively, and this approach is similar to Zheng et al. (2009) for computing climate elasticity of streamflow.

The methodology of Sawicz et al. (2014) to characterize changes in streamflow through catchment classification is interesting. However, the approach requires long term streamflow and climate data records to characterize hydrologic change. While these datasets are available for the Hydrologic Reference Stations in Australia, our methodology is limited by the availability of remotely sensed vegetation products. In the revised Introduction, we will incorporate Sawicz at al. (2014) approach to detect hydrologic change.

2. Similarly, there has been a lot of work on trying to disaggregate the roles of vegetation, storage, energy and moisture on predicting runoff ratio using Budyko type frameworks, which I think also show that it is difficult to come up with simple explanations for reasons for nonstationarity - which I think is line with the results shown here.

We agree with the reviewer comment that it is difficult to disaggregate the role of vegetation, climate and soil moisture on streamflow using the empirical methods such as the Budyko framework or the streamflow elasticity approach. Due to the two-way interactions between catchment water balance and vegetation dynamics, implementation of catchment scale ecohydrologic models is the next logical step to disaggregate the roles of various factors. Nevertheless, previous investigations on assessing climate elasticity of streamflow have shown that the degree of sensitivity of streamflow to various factors depends on the model structure and calibration approach (Sankarasubramanian et al., 2001). Therefore, further research on both data-based and modeling approaches are required.

3. In the results section (3.1) the authors state that variables increase, or decrease, or show trends. It would be good if they could quantify these a bit more, rather than just stating that the trends are statistically significant. Especially since the value of such significance tests is regularly questioned (e.g. http://onlinelibrary.wiley.com/doi/10.1002/esp.3618/abstract).

We will provide additional information about changes in water balance variables and the rate of trends in the revised manuscript. We agree with the reviewer that the results of the trend analysis are impacted by defining the significance level. While we removed the impact of the start and end year on trend analysis and reduced the impact of autocorrelation on trend analysis, we will present the results of a bootstrap procedure introduced by Douglas et al. (2000) to compute the field significance of regional trend tests in the revised manuscript. In this approach, time series of runoff ratio for every catchment will be resampled 10,000 times using the bootstrap approach. In the next step, the Kendall's S is calculated for each bootstrap sample and regional test statistics is calculated for each iteration. Finally, the CDF of regional test statistics is compared with the historical mean. Our preliminary analysis using the bootstrap approach provided similar results to that presented in the manuscript.

4. The main question I have relates to the fact that the authors largely focus on analysing the 20 out of 166 catchments for which they saw nonstationarity in the response. While the subsequent analysis of those 20 is fine, I wonder what can be said about the 146 catchment where runoff ratio is not changing? For example, how many of the stationary catchments have experienced precipitation or vegetation or ET changes similar to the ones where runoff ratio changed? That would be a baseline analysis to see whether an interpretation of the causes of runoff ratio nonstationarity are robust. So my main question to the authors is whether they can demonstrate that the catchments not showing runoff ratio change have experienced changes that are smaller regarding the potential driving variables?

We agree with the reviewer comment to provide a baseline analysis to show whether stationary catchments experienced similar changes in precipitation, runoff and vegetation compared to the catchments with non-stationary hydrologic response. To show these differences, we will implement the approach of Coopersmith et al. (2014) by developing regime curves based on daily runoff, precipitation and monthly fractional vegetation cover for each catchment using pre-drought and drought period data. Our preliminary analysis shows that in some cases, large changes in the regime curves have been observed particularly in catchments with non-stationary response.

Anonymous Referee #2

I am very interested in the analysis and discussions about the different influences of vegetation cover and climate changes on runoff in the manuscript. But in my opinion, some analysis is unconvincing and some conclusion is arbitrary. So, I suggest the authors conduct further improvement on the manuscript. Major comments are given below.

1. I suggest that the authors change the usage of "non-stationary catchment". Significant increasing or decreasing doesn't mean that the catchment is not stable. On the contrary, non-significant trend also does not mean stationary.

Thank you for providing this comment. The term "non-stationary catchment" is used for a matter of brevity in the manuscript. In some cases, we have used the term "catchments with non-stationary hydrologic response" in the manuscript. We will clarify the above usage further in the revised manuscript.

2. What I am most interested are figure 3 and figure 4. For figure 3b, the authors state that "In catchments with positive precipitation fractional vegetation cover relationships, fractional vegetation cover sensitivities decline with increases in annual precipitation across the catchments". But I would argue that, fractional vegetation cover sensitivity increases significantly with increases in annual precipitation across the catchments when precipitation is smaller than 700 mm; Authors also concluded statement "Fractional vegetation cover sensitivity is highest in the xeric (arid) catchments with lower mean annual precipitation compared to the rest of the non-stationary catchments" from figure 3b. But I cannot see any direct index reflecting "arid". I would suggest that authors plot dFtot/dP against with PET/P in figure 3b, as well as in figure 4a.

We will revise this statement in the revised manuscript to state that "across catchments with positive precipitation fractional vegetation cover relationships, fractional vegetation coverage sensitivity approaches zero in catchments with higher mean annual precipitation".

We will incorporate reviewer comment to show aridity-index with dFtot/dP in the revised manuscript and similarly for figure 4a. Catchment 7 with the lowest amount of mean annual precipitation has the largest aridity-index among non-stationary catchments.

3. For figure 3c, the authors should point out: what ranges of HI values mean dry and what HI values mean wet? It is also interesting that in wet regions (low HI), vegetation cover increases when the climate becomes dryer (HI increases)? Authors should give reasonable explanations.

In arid and semiarid catchments, quick flow constitutes most of the total streamflow (S is almost equal to total runoff in equation 3). Therefore, we expect HI to approach 1 in arid catchments. In humid catchments, quick flow runoff is smaller than the total stream flow and HI is less than 1. In catchments with limited storage, HI is undefined (0/0) (Troch et al., 2009). We will clarify these ranges in the revised manuscript. Please see Troch et al. (2009) for additional details.

The second question is a very important point and it is a subject of further investigations to identify the exact cause of vegetation increase under dryer conditions in group B catchments. One plausible mechanism as discussed here and in an earlier paper by Brooks et al. (2011) is nutrient limitation as similar behavior is also observed in some of the MOPEX catchments located in the humid climate. In this paper, we hypothesize that nutrient, light and temperature limitations may contribute to the observed response. With limited data on sunshine hours, we were able to show that in some of these catchments light limitation contributes to the observed pattern. However, no information about nutrient content is available to test this hypothesis. We also used various remote sensing datasets to make sure the observed pattern is not the artifact of remote sensing data. The next step is to use ecohydrologic models that can incorporate nutrient limitation in simulating carbon dynamics and vegetation growth.

4. For figure 3d, because high Ftot always locates in wet regions. So, according to figure 3d, in dryer regions (low Ftot), runoff coefficient always increases as vegetation cover increases?
This is conflict with the conclusion that reforestation and forest growth usually significantly decrease the runoff in dry regions.

To clarify this point, we refer to Figure 5b where interactions between precipitation-fractional vegetation cover and runoff ratio are outlined. As can be seen in Figure 5b, positive correlations between precipitation and fractional vegetation cover exist in water limited catchments (Group A). This means that higher precipitation increases productivity and Q/P. As can be seen in Figure 3a and 3b, sensitivity of runoff ratio to fractional vegetation cover is positive in drier catchments (water limited catchments based on our classification). As period of higher productivity coincides with higher precipitation (positive precipitation-fractional vegetation cover relationship) in these catchments, runoff ratio increases in years with higher precipitation. It should be noted that the percentage of tree cover in these drier catchments are more than 60% with a few exceptions (Table S1, supplementary Information). In Group B catchments percent tree cover is higher than water limited catchments. Overall, mean annual runoff ratio and its variability (standard deviation) are smaller in drier catchments with smaller mean fractional vegetation cover (Figure 2). We will clarify this point in the revised manuscript.

5. For figure 4b, the authors concluded that ": in catchments where groundwater constitutes significant component of stream flow, fractional vegetation cover exhibits smaller variability: : :". I would also suggest that the authors used the ratio of base flow to total runoff to replace the mean based flow as the x axis.

We will use baseflow index instead of mean baseflow in the revised manuscript.

6. The authors only analyze the vegetation cover besides climate factors. Former studies showed that catchment area and slope etc. are also very important factors, which might significant influences the changes of runoff to climate and vegetation cover changes. The areas of selected catchments ranges from 6.6 to 232846 km^2, which might bring unexpected influences on the analysis about figure 3 and 4. That is also probably the reason while only 20/166 catchments showed significant trends in runoff coefficients. So I suggest the authors should consider other catchment factors and explain the underlying reasons.

We will explore the impact of slope and area in the revised manuscript. However, within non-stationary catchments no significant differences in catchment mean slope exist and catchment area ranges from 18.7 to 5158 km^2 (Table 1).

7. Lack specific data and method descriptions. For example, authors didn’t explain how ET and PET were calculated etc.

Thank you for your comment. We will include detailed descriptions of ET and PET computations in the revised manuscript.

References:

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