Anonymous Referee #2

The authors present an interesting analysis into the potential combined effects of climate and land-cover change in a major tributary of the Colorado River. While the paper has sufficient novelty to be of interest to the community, several key oversights need to be addressed. For this reason, I recommend the paper undergo minor revisions prior to publication.

Major points

1. The authors conclude that understory regrowth leads to reduced streamflows. While this is a logical conclusion, additional discussion into other important mechanistic changes is warranted. First, if the model used by the authors doesn't account for lateral flow (i.e. is it a 1-D model?), then the authors need to acknowledge lack of process representation important for vegetation/hydrology interaction. For example, low-lying vegetation can receive water from wetter-headwaters areas of a catchment (e.g. Troch et al., 2009, Hydrologic Processes; Thompson et al., 2011, WRR). If these dynamics are ignored, then at minimum the authors need to acknowledge how the findings of these earlier works may impact the results in their manuscript.

Response: Yes, the model we used in this work, the Variable Infiltration Capacity (VIC) hydrologic model, does not include lateral flow. This is largely because VIC was developed as a land surface scheme for regional studies of climate change impacts and it is not meant for hillslope or plot-scale studies. In this work, we are considering regional scale responses of streamflow to climate change and land cover disturbances thus we feel it is an appropriate model for our study. This study is part of a larger project focused on understanding the kinds of questions regarding lateral flow that the reviewer raises, and we aim to investigate this question specifically in future efforts. We have added a section discussing the lack of lateral flow consideration in our study to the Discussion section and have cited the papers suggested by the reviewer, along with other citations from a recent study by Pribulick et al. 2016 (suggested by Reviewer #1) that does incorporate lateral flow. Please see track changed version of the manuscript.

2. An important missing piece is a justification for the settings used in the 'disturbed' forest scenario. While LAI was changed, the authors need to provide more justification for why the vegetation was modified the way that it was, and how this compares to what previous modeling studies have done. For example, other studies have explored changes in canopy transmissivity associated with forest disturbance (e.g. Bewley et al., 2010, J.Hydrology. The authors cite this paper, but do not reference the important 'calibration' to transmissivity that was done) involving calibration of forest parameters to observations, or remote sensing (Baker et al., 2017, RSE) while others have also modified the stomatal resistance, which is critical for accurately modulating ET, that is to say that modifying LAI alone may result in an inaccurate change to the total ET (e.g. Livneh et al.,; J.Hydrology).

Response: In our study, we did not modify LAI. We modified the forest cover percentages, and kept LAI, albedo and vegetation canopy spacing the same as these will be altered in VIC as the forest cover percentages change in our scenarios. One of the reasons why we believe shrublands respond the way they do, is that shrubland LAI is in fact, similar, to forest LAI in the forested, upland headwater catchment regions of the San Juan River basin. Please see our response to Reviewer #1, question #1/

3. Along the lines of the previous comment, it is unclear whether the authors validated their model beyond historical streamflow comparison. While streamflow comparison is important, a

validation of the impact of imposed vegetation changes is warranted to ensure the settings and modifications are realistic, while challenging, this could be done on historical observations of key model structural components using in situ and remotely sensed observations. Alternatively, if validating the model settings is outside the scope of the present manuscript, then the authors need to clearly state this as a
limitation of their study in the discussion.

Response: We based our study of vegetation changes on two sources of information, dynamic vegetation in CMIP5 and the changes in vegetation disturbances determined by McDowell et al. 2015. One of the challenges that we face is that historical comparisons of vegetation disturbances are not valid for future, no-analogue conditions. We do not know what these future changes are going to look like, hence we examine a scenario-style approach to changing forest to simulate disturbances and then discuss the responses. We do not attempt to say that our methods are correct, and we refer to them only as projections of the future changes. We have added a line into the Discussion on page 15 that clearly states this point.

4. Research now exists that suggests that forests disturbance can cause both increases and decreases in streamflow. While the authors cite part of the literature, they ignore important foundational publications on this topic (see references within Adams et al., 2012, Ecohydrology). Newer research also exists that uses large-sample catchments to study the effects of forest disturbance and should be referenced here (Buma et al., 2017; ERL), so as to put the findings in a clearer context relative to the literature.

Response: We do discuss this in our manuscript. We draw the reviewer's attention to page 3,. "To date, predictions of future streamflow in forested river basins under future changes in climate and land cover have exhibited wide disagreement as to the strength and even the sign of change." We cite in this paragraph Adams et al 2012, and Guardiola-Claramonte et al., 2011, for example. In Adams et al. 2012, Table 2 lists studies that included hydrological response to tree die-off. This table includes one study we did not reference that cites mostly decreasing streamflow (1 out of 8 catchments) and no change (7 out of 8 catchments), Somer et al. 2010. This is also discussed in McDowell et al. (In press, 2017), and we have added this citation to the paragraph. We have now added the reference to our paragraph, and specifically noted Adams et al.'s Table 2. We have modified the text in the Introduction and have also added the newly published Buma et al. 2017 to our citations in this paragraph.

5. More details into the climate of the study basin is warranted, e.g. mean annual precipitation, temperature (include summary numbers in addition to figure 4), mean elevation of the basin, etc. Most importantly, the authors need to comment on how representative this semi-arid basin is of the Colorado basin as a whole and whether the results here are indicative of what other parts of the world might see, or whether this climate/landscape is sufficiently unique.

Response: We have added mean annual precipitation and January/July temperature to the Study Site description. Figure 1 provides the elevation across the basin, so we have not added elevation statistics to this paragraph. The San Juan can be thought of as a microcosm of the Colorado, because it contains both high elevation mountains representative of the upper basin, and the flat lowland, semi-arid environment indicative of the lower basin. We have amended our Study Site description to read: "The San Juan basin captures the diversity present across the CRB. For instance, high elevation (> 4000 m) Colorado mountain ranges and large, snowmelt driven rivers comprise the upper San Juan basin. The lower San Juan basin, located in New Mexico and Arizona, is flat, semi-arid and representative of the lower Colorado, with intermittent streams that drain into the main tributary of the San Juan during the summer when they are charged by summer monsoonal rains."

Minor

Figure 3: this is a nice figure, however the axis labels are way too small and need to be increased, as does the font size in the legend. Furthermore, the green shading should be included in the legend.

Response: Thank you. We have adjusted this figure to address your comments and Reviewer #1's comments. The figures axes labels are now larger and the legend font is larger. The green and grey shading are now included in the legend.

Figure 4, the lines are too thin and difficult to see. The authors should increase the line thickness by at least a factor of 2.

Response: Thank you. We have adjusted the line thickness and added axes units to this figure.

Figure 5 is very difficult to follow. First, it seems to me that including multiple colored circles directly on the plot is confusing, since it's unclear whether these are a part of the plot or a legend. To overcome this, the authors should add a color bar and remove these circles. Second, the circles in the upper left of the plot obfuscate the graph, since I cannot find any other circles (other than a small one in the upper right), so instead the authors should state in the caption that the size of the symbols corresponds to the percent forest, there could be a length bar in the legend to show this, but the circles are too distracting/confusing. Lastly, the temperature description in the top center of the plot belongs in a legend.

Response: Thank you. We have amended the figure and we think it is vastly improved.

Figure 6: The lines are very difficult to distinguish, particularly in panel (a). I recommend that the authors avoid using dashed and solid lines of the same color in such close proximity, i.e. use lines of different colors.

Response: Thank you. We have amended the figure panel a to add more colors to differentiate the lines.

Citations

McDowell, N. G., Michaletz, S., Bennett, K. E., Solander, K., Xu, C., Maxwell, R. M., Allen, C. D., and Middleton, R. S.: Predicting Chronic Climate-Driven Disturbances and Their Mitigation, Trends in Ecology & Evolution (TREE), In press, 2017.