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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Time-series analysis of the long-term hydrologic impacts of afforestation in the Águeda watershed of North-Central Portugal" *by* D. Hawtree et al.

Anonymous Referee #4

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GENERAL COMMENTS

1. The manuscript attempts to discern the long-term (maximum 75 years) impacts of afforestation (with pines and eucalyptus) on the climatic (precipitation) and hydrologic (streamflow and baseflow) variables for a watershed in Portugal. In doing so, the authors analyzed the temporal trends with different time-periods of afforestation using the non-parametric method of Mann-Kendall test. Although the presented trend results of the hydroclimatic and flow variables appear to be correct, I don't think that the posed research question (afforestation impacts on hydrology) is answered clearly, let alone directly, by the presented results. Instead, the paper used many lines of arguments

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and hypotheses to indirectly attribute decreasing trends in baseflow variables to that in precipitation, while no-trends in streamflow were explained by a seemingly stretched logic of soil-water repellency. At the end, the stretched deductive reasoning (eliminating possible causes step-by-step) provided conclusions, which may or may not be true. Therefore, the findings presented in this study are largely conjectural, rather than being inferential in nature.

2. The overall trends in the hydrologic variables are likely to be the combined outcomes of the long-term changes in both climatic and land use/cover regimes. The authors acknowledge this for baseflow variables in the discussion. However, although the authors state in the Introduction that the eucalyptus is known to have a higher ET than that of pines, I am really surprised that the long-term trends in temperature (as a surrogate for ET) were not analyzed and synthesized with those of the flow variables. Instead, the authors sort of dismiss the possible trends in ET arguing that the plants' root zones are shallower than the water table depth!

3. The paper justifies the use of trend testing, compared to hydrologic modeling citing the lack of data and knowledge on the complexity in soil geomorphology over the entire study period. I have hard time to accept this argument. I believe a well calibrated and validated model with current data can be used to answer the land use/cover impact question posed here by conducting a proper sensitivity analysis. I don't think that the trend testing approach needs to be justified as done in this paper. Instead, the data-driven method can be justified as a complementary approach to the largely physically-based watershed hydrologic modeling.

SPECIFIC AND TECHNICAL COMMENTS

1. Abstract, line 6: "7 years of data" should be revised to "75 years. . ."

2. Section 2.3: The results of Mann-Kendall test could vary if there were too many missing data-years. Please state the number of missing data that you allowed for the different variables.

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3. Page 12235, lines 21-23: Please include the negative (-) sign before the trend magnitudes.

4. Page 12236, lines 5-6: It is unclear what is meant by the first clause of this last sentence: “These results indicated that the trend in streamflow yield during this period was fairly consistent across the year..”. Most periods didn’t show any significant trends; the one period that showed trend should have a single Theil-Sen slope value by default!

5. Page 12236, line 17: Should be “wet” instead of “west”.

6. Page 12237, lines 9-11: The following sentence does not make sense: “This could have led to longer recovery times for soil moisture during the resumption of the wet season, which could have amplified soil water repellency during this period (both in terms of the duration and severity)”. Shouldn’t the logic be the other way around?

7. Section 4.3: Except up to line 15 (page 12240), the entire section is about conjecture rather than inferences based on the presented results. It must be substantially revised by mainly focusing the inferences.

8. Page 12241, lines 8-10: The following clause does not make sense to me: “. . .leading to an increase in quick flow (particularly via fast sub-surface flow from macropore infiltration) and the rapid conversion of precipitation into runoff”

9. Page 12241, lines 11-12: The following sentence is incorrect, according to Fig. 6: “Notably, the significant reductions in BFI were confined to the wet period, with only one exception”

10. Page 12241, lines 11-23: The presented logic and the entire paragraph, starting with an inaccurate statement (line 11-12), do not make sense. Please substantially revise or remove.

11. Page 12242, lines 23-26: The last sentence of this paragraph is vague. It does not contribute any new information, and should be removed.

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12. Page 12243, lines 1-20: The entire paragraph is full of conjectures with grandiose statements that are not really supported by the presented results. I recommend that the authors rewrite this paragraph, if they really want to include a second paragraph in the Conclusions, by following the second half of their Abstract.

13. Although the Table 2 presents statistics of observations for the dry months (June-Sept), the corresponding hydro-climatic trends were not presented due to unreliable data. This may confuse some readers.

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