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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 #3

Received and published: 1 December 2014

General comments

This paper addresses the question of whether land use changes are detectable in terms of runoff from a 400km^2 catchment in Portugal. A long hydrometeorological record (70 years) is analysed using a series of trend tests on different sub-periods. The paper is well written and the topic is of significant hydrological interest as there are relatively few empirical analyses of hydrologic change in larger catchments in the literature (compared with paired studies of small catchments).

I have three major concerns that need to be addressed before this paper would be

acceptable. This will require some additional analysis and will also require changes to the way significance test decisions are made in the statistical approach.

First, the paper examines trends in precipitation and streamflow variables separately but the key question is really has the catchment response changed. It is well known that there is a strong relationship between rainfall and runoff at the annual timescale, so the question of the paper should really be expressed as "has the rainfall-runoff relationship changed?" I think the authors should analyse this relationship in addition to the analyses they have conducted as this relationship captures the effect of internal catchment dynamics. As an illustration of this issue, there is a positive trend in streamflow from 1946-1970 that corresponds to an almost significant (p value = 0.11) positive trend in rainfall for the same period. The streamflow increase may just reflect a rainfall increase, rather than any internal change in the catchment. Looking at the rainfall-runoff relationship would help by controlling for rainfall changes.

Second, there is little consideration of the overall water balance setting and some contradictions are implied in what is presented. Table 2 and Section 3.2 imply the long-term evapotranspiration is about 1180mm/a. While the potential evapotranspiration (PET) is not provided, this would seem to be a large fraction of the PET, suggesting little constraint on soil water availability. However the soils are said to be very shallow and there is little rain in summer when PET is high, so there should be a substantial effect of soil water stress. These two things seem hard to reconcile and currently detract from the overall confidence in the results and the hydrologic interpretations. I think a more thorough discussion of the hydrological setting and water balance is needed, including (but by no means limited to) presentation of PET information.

Third, the paper has a major problem in the application of the statistical testing. Only 9% of the 240 tests conducted were significant. A 95% confidence level was used, hence you expect at least 5% of the tests to be significant. That is, more than half the positive results might be due to chance alone. The methodology needs to control for multiple testing using Bonforoni corrections or a more sophisticated method such as

the False Discovery Ratio approach of Benjamini and Hochberg (1995).

Given this doubt about the trend testing and doubts about the consistency of the overall water balance (see above), I worry about the authors trying to infer process changes. The lack of consideration of the relationship between rainfall and runoff further adds to this concern. Are the authors looking for explanatory effects where no explanations are needed?

Specific comments

The term "streamflow yield" is used throughout the paper for "runoff coefficient". Generally streamflow yield refers to runoff depth (what the authors refer to as runoff quantity). I would suggest using "runoff coefficient".

In the abstract give some indication of the pattern of landuse change over time.

12226, L 3-8. Very long sentence. Break it up.

12228, L10-15. This gave me the impression little was known of the patterns of change over time but later more detail is given. Also could a better record be constructed from 1974 onwards via Landsat images?

12229, L14. What are the typical depths of the "shallow" soils.

12229, L19. Say what the "natural" vegetation types are here.

12230. There is an extensive literature on fire effects from California, Australia, and Mediterranean countries. It might be worth providing a brief summary in the introduction and drawing on the broader literature.

12231, L10. Has the consistency of the rainfall record been checked, e.g. by double mass curve analysis?

12231, L23. What was done when <5% was missing but the logarithmic decay wasn't used?

12232, L19-20. This is unclear – needs rewording to explain what was done.

12232, L23. What does "non-zero mean" mean? Is it that the 95%CI didn't contain zero? Clarify.

12232, L25. What was the value of the serial correlation?

12233, L3. It is not clear to me why you swap to Mann-Kendall when it seems that Thiel-Sen could be applied to the whitened data as a trend test?

12234, L5. Was the precipitation seasonality consistent between the various periods. Changes in seasonality could impact runoff.

12234, L25. High Pearson correlation is a start but it would also be good to know what the slope of the relationship is and the means from the two methods.

12235, L2-3. This actually only shows the consistency over the period 2001-2009, not necessarily the whole study period.

12236, L26. Please provide the definition of aridity index.

12238. As discussed in the general comments, the discussion on this page regarding shallow soils, low summer rainfall and high summer evaporative demand seems inconsistent with what the overall water balance suggests. This needs some further clarification.

12240, L18-25. I was under the impression that the conversions occurred over an extended period. I would have thought that the effects of soil disturbance etc would be so smeared out as to be undetectable?

12241, L25-28. It is not clear what is meant by positive and negative here. There is an implicit assumption as to what is good.

12242, L4-8. Following on from the previous comment. This is inconsistent with the positive and negative comments on the previous page because here you say the effects

can't be differentiated.

Table 2. I would like to see PET included.

Figure 2. Plot PET.

Technical corrections

12224, L6. 70 years not 7 years

12230, L25. Should be "....in the Águeda...."

12231, L22. Not clear what logarithmic means – do you mean traditional linear reservoir i.e. fitting on a semi-log plot?

12234, L22. Change to "..... results obtained...."

12240, L26. Change "expectedly" to "expected"

Figure 1. The colours can't all be differentiated. This needs improvement.

Figure 3. The y-axis labels are reversed.

References

Benjamini, Y., and Y. Hochberg (1995), Controlling the false discovery rate: a practical and powerful approach to multiple testing., Journal of the Royal Statistical Society. Series B (Methodological), 289-300

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 12223, 2014.