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Discussions

Interactive comment on "Attribution of detected changes in streamflow using multiple working hypotheses" *by* S. Harrigan et al.

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We thank Anonymous referee #1 for his/her insightful comments and suggestions. Below is a point-by-point response to all referee comments (indicated by RC).

RC: The paper investigates the evidence of changes in streamflow in an Irish catchment and the possible drivers of such changes. Arterial drainage, rather than the NAO Index oscillation, is indicated as the most likely driver of change. The paper elaborates on Merz et al. (2012)'s idea that the attribution of trend is a fundamental step in order to develop correct management responses and adaptation strategies. Attribution of trend is indeed a key question, and the authors present an interesting case study, for which they identify a new main driver of change, compared to previous studies. The paper

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Interactive Discussion

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is well written and structured: the work-flow is understandable and sufficiently easy to follow.

RESPONSE: We agree that this is indeed an important question and thank the referee for his/her supportive remarks.

RC: I have some doubts on the high hopes the authors seem to have on the Multiple working hypothesis framework: I see that the framework helps in identifying the potential drivers of change, but my main concern is that we might not be able to think about all possible drivers of change beforehand (or we might not have measurements on potential drivers). The framework works fine if one can really identify all possible drivers of change and then make a judgement on their effects. If the real driver is out of the framework, we would still be imagining effects of some variables based on probably vague results (exactly what Merz et al. (2012) would want to avoid). Although I appreciate the idea of the MWH framework, there might still be a certain level of subjectivity from the scientist in the choice of the WH which should be taken into consideration.

RESPONSE: The referee has highlighted one of the key challenges with attribution. Indeed for many variables there is simply not enough/no data available to assess quantitatively. However, in essence of Merz et al. (2012) we need to move away from leaving attribution of detected changes to a few paragraphs in the discussion section, usually based on a single driver of change (i.e. soft attribution), and begin to include it more formally in our study framework (i.e. towards hard attribution). We also recognise that knowledge of other potentially important drivers may be missing and the method of Multiple Working Hypotheses (MWH) allows for unknown drivers (p. 12376 L10-12).Therefore, table 1 in our manuscript also includes WH 11 which accommodates the possibility of other (as yet), unknown drivers of hydrological change.

The initial pool of candidate drivers should reflect the full range of knowledge at the time of the analysis. The detected change in Boyne streamflow is very well defined and constrained in time (i.e. large upward step change in the mid-1970s) and thus limits the possibilities when establishing hypotheses of change. We acknowledge that there is a level of subjectivity introduced by the selection of the initial working hypotheses. The selection is limited (and influenced) by current knowledge of the researchers undertaking the study hence the automatic introduction of some subjectivity. MWH adds value by forcing the researcher(s) to consider many working hypotheses of change and not the single (or few) that are conventionally applied. This is arguably a more systematic and holistic approach than is typically found in attribution literature and so helps to reduce conformation bias.

The MWH framework also considers the potential for multiple drivers having important influences and for the possibility of interaction among drivers (Table 1, WH 10 in original manuscript) while accepting that other hitherto unknown drivers (Table 1, WH 11) may have important influences. Hence this study should not be seen as a standalone piece of work but an advance on previous attribution research into changing streamflow in the Boyne catchment (due to the NAO). Moving along the spectrum from soft to hard attribution is likely to be an iterative process in situations where finding weaknesses in established hypotheses of change leads to improved understanding of dominant drivers of change.

Moreover, we would argue that whilst the three ingredients for hard attribution proposed by Merz et al. (2012) (i.e. proof of consistency, proof of inconsistency and confidence statement) are the gold standard it must be acknowledged that this is not always possible. Hard attribution was not achieved within the Boyne case study because the proof of consistency of drainage could not be quantitatively proved. This would require complex physically based modelling of drainage to simulate the observed hydrological response. Future work may examine the possibilities of providing proof of consistency however, descriptors of field drainage are, by nature, largely qualitative and this will be true for other historic drivers of change.

We will provide a more critical reflection on the MWH framework and a stronger admission of currently unknown drivers of change within the discussion (as also suggested by Vorogusyn in his comments).

RC: Moreover at page 12378 the authors discard the WH2 and WH9 based on the fact that the effect of these drivers would be against the direction of the detected trend. What if these drivers would still have played a role, but due to the interaction with other drivers their effect is not visible? In this particular case substantial Water abstraction/diversions are likely to be noted in the history of the gauging station, and it should be possible to detect substantial changes of PET at the Dublin airport, but if one is to take the idea of hard proof for any WH, more evidence would be needed to prove that WH2 and WH9 cannot be accepted. Also, each driver could have an effect at different time scales.

RESPONSE: There is the possibility that the effect of drivers such as WH 2 and WH 9 could be masked within the streamflow (e.g. if an increase in one variable had a similar magnitude to a decrease in another then their combined effect would be no observable change in streamflow). In spite of this, it is important to consider the timing and absolute contribution of potential drivers to the change being studied, in this case a large step increase in annual mean flow. For example, the degree of water abstractions is so low in the Boyne that it is unlikely to have an important influence. However, we agree that further quantitative information could be employed to consolidate the hard proof needed to discard WH 2 and 9. It is unlikely that we can obtain abstraction totals for the mid-1970s – the period of interest for the change point. However, in the revised manuscript we will look to quantify the volume of abstractions relative to the Q95 flow for contemporary abstractions. Regarding WH9 we have assessed the PET series at Dublin airport for evidence of change using the techniques employed in the paper. No evidence of statistically significant change is apparent in this PET series. We will note that this analysis has been performed in Table 1 and provide a line better summarising results in the text, thereby providing more rigorous evidence against PET as a driver of change. We will also add a comment further emphasising the potential interaction of drivers and the importance of time-scales relative to the change being assessed.

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RC: Unluckily, as the authors point out, to have better understanding of other potential drivers a large amount of data would be needed, so I see that it would be hard to improve on what the authors present here. This is even more true when looking at the large amount of data the authors have available for this study: very few catchments will have such rich series.

RESPONSE: This is very true and is a fundamental challenge for catchment scale river management generally. However, the approach taken here highlights that within disturbed catchments like the Boyne more research needs to be undertaken to quantify the impact of urbanisation and forest cover on streamflow. As noted at the end of Sect. 5.2 the reason for limited current understanding is due to a lack of controlled conditions (i.e. small experimental catchments versus medium and large disturbed catchments). The use of the "virtual control catchment" concept as used here has been useful for eliminating the influence (i.e. to prove inconsistency of climate). However, more physically based modelling is needed to examine the effect of urbanisation and forest cover taking account both extent and location but, again, use of more complex models will introduce more parameter and structural uncertainty to the analysis.

We agree with the reviewer that the demands on data for hard attribution are high and that such data and hence hard attribution may not be possible in all cases. We stop short of hard attribution in our study. However, we see the MWH approach as offering benefits and direction for further research here, as was the case for the potential role of changes in forestry and urbanisation. The fundamental contribution of the MWH approach is to compel researchers to consider other potential drivers of change and to consider, based on available data whether specific hypotheses of change can be ruled out. Even if only soft attribution is possible at least MWH forces this soft attribution to be recognised in context. This is an important cultural change in attribution studies and helps to avoid confirmation bias as noted by the second referee.

RC: At page 12387 the authors mention that trends were applied to the TFPW series if serial correlation was found. It seems from what the authors say in 4.2.1 that this

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mostly affects the tests for the March and August mean flow. In Figure 8 it seems that the TFPW has a larger effect for the test of the observed monthly August flow. Any idea of why the difference is much bigger for August than for March? It also seems that the test statistic for the modelled data has a higher variability in August than in March. In general, I find the large scatter of the test statistic values interesting.

RESPONSE: The strength of trend has an important bearing on the magnitude of differences between observed and TFPW test statistics for the Mann-Kendall test in observed March and August mean flow time-series. Mann-Kendall tests only for evidence of increasing or decreasing trend but does not calculate the actual magnitude of trends. In Fig. 8, the MKZs for March (for both original and TFPW data) is very discernible from random variability (i.e. MKZs values are well above the 5% significance level). In contrast, the trend in observed August mean flow is much harder to discern (both original and TFPW MKZs are less than 5% significance level). To quantify the effect of TFPW on the trend magnitude we applied the Theil–Sen approach (TSA) to both original and TFPW data from observed March and August mean flow. The magnitude of TSA slope (m3/s per year) for observed March mean flow is 0.327 compared to 0.285 for TFPW data. For August, the observed mean flow has a TSA slope of 0.104 compared to 0.093 for TFPW data. Therefore, the influence of TFPW on trend magnitude is greater in March (∼12.8 %) than in August (∼10 .6 %). As the trend in August mean flow is much weaker than in March (by a factor of ∼ 3), there is a greater difference between original and TFPW MKZs values.

In general, there is more scatter in modelled flow MKZs test statistics in the dryer summer months (especially May, July and August) due to the non-linearity of the rainfall-runoff response resulting in greater variability of simulated flow output generated from the three model structures employed, compared to wetter months. The attached Fig. 1 shows this clearly, there is both more spread in MKZs values for each model structure and between model structures in August compared to March reconstructed mean flow time-series.

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RC: Page 12391, Line 2-3. Calling a trend significant because the MKZ=1.97 heavily relies on the Normal approximation of the test statistic. If a MKZ=1.93 deserves to be called “near significant”, a MK=1.97 deserves to be called “just significant”, I believe. This doesn’t change the final discussion, and shows how showing the value of the test statistic rather than an acceptance/rejection value is indeed more informative.

RESPONSE: We agree with the referee and have tried where possible to remove such arbitrary interpretations of statistical results by reporting test statistics and p-values. We will change the wording on p. 12391 L1-2 to “. . ., with one reconstructed Q10 series showing a barely significant trend (MKZs 1.97).”

RC: Page 12393, Line 3-4 states “This discrepancy is particularly evident for high flows (Q10) and during winter months.” The winter month discrepancy holds for the modelled monthly flows of Figure 7, but significant change is also found in summer months mean flows of the observed series (Figure 8). This indicates a more complex change.

RESPONSE: This is an interesting point that merits further interpretation and discussion in Sect. 5. The majority of published literature on the influence of arterial drainage schemes on streamflow highlight the wet times of the year (i.e. winter for Ireland) as having greatest influence. There may be a smaller contribution in some summers (such as the summer floods in 2007). However, Fig(s) 6 and 7 show that the magnitude of change is most important (volumetric), rather than if a change is statistically significant or not. This highlights the limitations of basing conclusions on statistical significance alone. A statistically significant trend in summer mean flows (with low discharge values) contributes very little to the large change in annual mean, when compared to the significant change evident in, for example, observed January streamflow. We will provide a short discussion in section 5.1 to clarify this point.

RC: Figure 8: wrong understanding of what the whiskers in a boxplot are (or something is not well explained). If the whiskers are the minimum and the maximum of the observed statistics how can there be extreme outliers outside? If the de-

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fault of most boxplots functions were used the whiskers extend to the most extreme data point which is no more than 1.5 times the interquartile range from the box (as can be seen, for example, in the R help file (http://stat.ethz.ch/R-manual/R-devel/library/graphics/html/boxplot.html). This is based on properties of the normal distribution. If another choice was made on how to draw the whiskers, more details are needed on how outliers are defined.

RESPONSE: The boxplots were produced in R using the default parameters within the 'boxplot' function. The reviewer rightly points out that the Fig. 8 caption description is incorrect. The caption will be changed in the revised manuscript to: '. . ., whiskers 1.5 times the interquartile range (IQR) and grey circles outliers outside the IQR.'

RC: Finally the correct citation for the FEH (cited at page 12378 - line 1) is: Institute of Hydrology. Flood Estimation Handbook (five volumes), Centre for Ecology & Hydrology, Wallingford, UK, 1999.

RESPONSE: The correct citation will be used in the revised manuscript. We thank the referee for bringing this to our attention.

REFERENCES:

Merz, B., Vorogushyn, S., Uhlemann, S., Delgado, J. and Hundecha, Y.: HESS Opinions "More efforts and scientific rigour are needed to attribute trends in flood time series", Hydrol. Earth Syst. Sci., 16, 1379–1387, doi:10.5194/hess-16-1379-2012, 2012.

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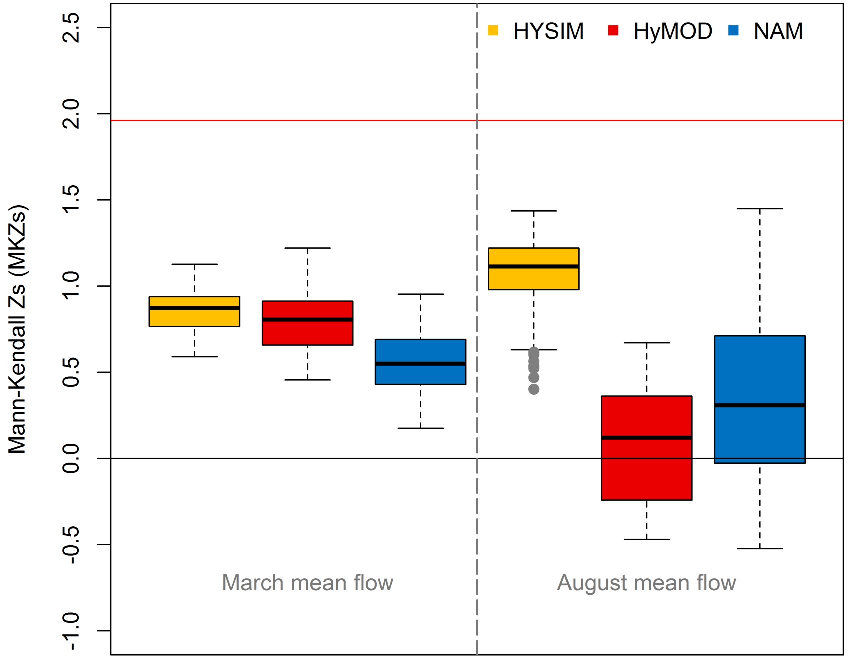

Fig. 1. Mann-Kendall tests for monotonic trend in March (left) and August (right) reconstructed mean flow indicators. Boxplots summarise individual reconstructed time-series MKZs values for each model.