Dear Prof. Ursino,

First of all, thank you for your time and effort to review the manuscript entitled "The role of forest maturity on catchment hydrologic stability" for HESS. Please find below the comments of the reviewers followed by our answers in **bold** type. We have also prepared a revised version of the manuscript where changes have been implemented as a response to such comments, together with additional modifications to improve clarity. Before, we would like to stress some important remarks that we believe that should be considered before a decision is taken.

First, our contribution is focused on producing a method that could be applied anywhere else in the world (through LANDSAT images) to improve the land cover descriptors that could be used in hydrological modelling. The use of forest maturity (associated with landscape dynamics) deserves special attention in this context. Second, the study uses two different and independent analyses (without circularity) to relate hydrological characteristics with land cover types. One is based on the calculation of hydrological indices from hydrological data series obtained in 10 flow gauges (empirical data), while the other one is based on the development of 10 independent hydrological models (using IHACRES) to estimate "quick" and "slow" flows (model parameters) for each of those 10 catchments (process-based data). Third, we do not compare just two modelled catchments, which is typical in 'paired-catchment' studies (as the introduction details). On the contrary, the study area contemplates up to 10 catchments representing an irreplaceable land cover gradient created by massive deforestation in some of the catchments almost 400 years ago. These catchments are in the same climatic zone (Atlantic Oceanic) and do have simultaneous meteorological and flow records for more than 15 years (1995 - 2010). We believe that such a spatial design and data availability is exceptional and, thus, ideal to study land cover effects on hydrological extremes.

After reading the comments provided by the reviewers, we came to the conclusion that there was a necessity to emphasize and stress some aspects in order to improve clarity for a wider audience not familiar with the approaches used (which is why we have prepared the revised manuscript). In this sense, we have focused on some misunderstandings that we believe have taken place during the revision process. For example, precipitation is not the only climatic data used in the model IHACRES, and the use of this model does not constitute a circular reasoning (further details are provided in the revision). Another remarkable example is the suggestion by one of the reviewers of using the Corine Land Cover (CLC) dataset. The CLC dataset has a much coarser spatial resolution and it presents vectorial GIS data unsuitable to produce the results that we have been able to obtain with LANDSAT images and land cover classifications. From our point of view, this type of misunderstandings indicate that the study has been undervalued in this first revision.

We believe that through the comments received and the changes implemented, the text is now much clearer and more focused (we have also changed the title). We have provided additional background and text in order to better explain the study and put more emphasis on its important and novel parts. We believe that this manuscript will be of interest to a wide HESS audience.

Should further clarifications or modifications were necessary, we would be happy to provide them.

Thank you again for your attention.

Yours sincerely, Oscar Belmar, on behalf of co-authors

Anonymous Referee #1
Received and published: 3 November 2016

General comments: I reviewed the paper "The role of forest maturity on catchment hydrologic stability" by Oscar Belmar and co-workers. In this paper, the authors attempted to improve the understanding of the impact of land cover on flow extremes (flood and drought) at the catchment scale through an experimental design. The objectives and relevant scientific questions addressed in this paper are within the scope of HESS. However, I have to say that the experimental design of using associated with correlations and regression, and speculating that forest maturity can serve as a better hydrological indicator is a little weak. In addition, the authors stated that the first objective of this paper is to isolate the relative contribution of precipitation and land cover to hydrological extreme events. There is model development. If there was an analytical model developed, this would be an adequate contribution. However, this has not been performed in this manuscript.

The Introduction has been modified to emphasize that the main objective of this study is to improve the understanding of how forests and other predominant land cover types influence catchment hydrology, particularly recurrent floods and droughts, through a land cover gradient design in which climate is homogenous. The contribution of this study resides in two main points: 1) We use several large catchments in the Cantabrian Mountains (NW Spain) with a gradient of forest cover due to human management since the 15th century. 2) We defined forest cover not only through forest surface but also using forest maturity. This distinguishes our study from previous literature. First, previous studies have been based on comparisons between only two catchments with different land cover surface (see state-of-art description in Introduction). Second, our contribution is focused on the improvement of land cover descriptors. This makes our study a potential reference document within hydrologic research, as we conclude that (1) forest maturity results more effective than forest surface and (2) the understanding of forest effect on flow regimes described in previous literature may be complemented with landscape dynamics.

This study aims to provide empirical evidence, NOT modelling the underlying biophysical processes. Model development is a different field where previous research has already been performed. Previous authors have used the Soil and Water Assessment Tool (SWAT; Arnold et al. 1998), cited in the discussion, and other modelling techniques to determine hydrological impacts of climate and land-use changes in head waters (e.g. Zhang et al., 2016), concluding that there is uncertainty associated with the choice of the model (Morán-Tejeda et al., 2014). Therefore, the use of own hydrologic models does not guarantee that the underlying processes are true and accurate. In any case, we provide suggestions in this sense for future research and studies.

This study uses two different and independent analyses (without circularity) to relate hydrological characteristics to land cover types. One is based on the calculation of hydrological indices from hydrological data series (15 years) obtained in 10 flow gauges (empirical data), while the other one is based on the development of 10 independent hydrological models (using IHACRES) to estimate “quick” and “slow” flows (model parameters) for each of those 10 catchments (process-based data). Such model parameters were computed using flow, precipitation and temperature data.

The specific objective of ‘isolating the relative contribution of precipitation regimes and land cover to hydrological extreme events’ has been clarified in the revised manuscript. This specific objective is accomplished through Partial Correlation Analyses, already used in other published studies on catchment land cover (e.g. King et al., 2005). Similarly, the Ordinary Least Square (OLS) Regression herein used is a widely employed technique also in the hydro-climatic domain (e.g. Hornbeck et al. 1993; Gyawali et al 2015). This information has also been incorporated into the text.

1. Comment: In the revised version of the paper, the authors should clearly state which is the novelty of the paper for which the paper deserves publication.

Please see previous answer (first paragraph).

2. Comment: It seems that the authors try to describe some original interpretations for the phenomenon. I would recommend the authors to show the possible mechanisms a little more specifically. That would help particularly the abstract to be more understandable and attractive.

We include in the revised version of the manuscript changes in the introduction and discussion in order to underline the importance of some possible mechanisms explaining the observed pattern. However, we have tried to be cautious and avoid being speculative.

3. Comment: P1, L26, what do you mean by average conditions?

The text has been clarified to avoid ambiguity.

4. Comment: P1, L29, I suggest the authors add the specific forest area.

The text has been changed, as it is rather a trend observed among different studies cited in the reference provided.

5. Comment: P2, L20 and L31, what is the difference between watershed and catchment? If this two terms have the same meaning, please used one of them consistently.

We have replaced ‘watershed’ by ‘catchment’ all throughout the manuscript.

6. Comment: In the Introduction section, I could not find detailed research progress of the effect of forest or other land cover on hydrological processes.

The text has been changed to include more references in relation to research progress. Please note that the introduction includes now a summary of reviews of catchment experimental designs that addresses this issue, after providing some context and before stating the contributions of this study. Our study presents a unique land cover gradient design based on gauged flows over 15 years in 10 different catchments. This provides more empirical evidence than the obtained through classic ‘paired-catchment’ studies. This message has been reinforced in the text.

7. Comment: The Result section is too short. This again illustrates that the evidence in support of your conclusion is weak and I suggest authors provide more evidence.

Our study provides results obtained from two sources: empirical data and modelled process-based parameters. The study is aimed to report an empirical pattern not to produce a physical model to reproduce it. However, the parameters obtained through the 10 IHACRES models developed for the 10 catchments provide additional information in order to support our conclusions. With all this information, the result section now integrates the text plus three figures and three tables (a third table has been added), increasing considerably the number of pages necessary for publication. These results provide evidence for the land cover classification results in each catchment, for the relationship between rainfall and hydrology and for the relationships between land cover and hydrology.

8. Comment: The comparison and discussion with the similar studies on the impact of land cover on flow extremes is lack in the manuscript.

This information has been extended in this revision. Such comparison, in terms of previous studies at catchment scale, is further developed in the Introduction (where we present the state-of-art, see Comment 6) and in the Discussion (where conclusions of previous studies are presented, including an example also developed using IHACRES). In this sense, we try to explain that our experimental design provides higher empirical evidence than classic 'paired-catchment' studies.

9. Comment: P4, L31, how water interception and retention were estimated to determine the proportions of slow and quick flows? Meanwhile, the authors should define what are slow flows and quick flows, respectively.

This is now better explained in the Methods section. Please note that 'Water interception and retention caused by ground vegetation and soil development were estimated determining proportions of slow and quick flows through a physical model that uses precipitation, temperature and flow data: IHACRES'. 'Slow flows' and 'quick flows' constitute two components in which flow regimes may be decomposed. The former refers to the volumes with highest time of concentration, defined as the time needed for water to flow from the most remote point in a catchment to the catchment outlet. The latter refers to volumes with lowest time of concentration. Therefore, they are employed on the basis that those catchments with higher proportion of slow flows present greater water interception.

'Quick' and 'Slow' flows are model parameters within IHACRES that tend to be used without previous definition. However, we agree that a short definition helps the reader and it has been included in the revised manuscript.

10. Comment: P5, L9, here the authors only take precipitation into consideration as a climatic factor, how about the effect of evapotranspiration?

Precipitation is not the only climatic variable that this study has taken into consideration. IHACRES is able to work either with temperature or with evapotranspiration. In our study, we selected temperature due to data availability.

11. Comment: P5, L22, what does western extreme mean?

The text has been modified to explain that it refers to the western part of the study area.

12. Comment: P7, L20-21, I have no idea what you mean here; please improve.

The text has been modified.

13. Comment: P7, L26, what does the low representation mean?

The text has been modified.

14. Comment: In Table 1, the authors should provide the mean annual cumulative precipitation and mean annual air temperature, and add what period for hydrological variables (i.e., mean runoff and mean flow), climate variables (i.e., precipitation and temperature) and land cover, though this information have been present in the text body. Furthermore, I suggest that the codes and names of river in eastern and western part of the study area should be distinguished. I wonder that what forest surface is. It refers to vegetation coverage, or something else. What is the relationship between forest surface and forest maturity?

Table 1 has been modified to provide the information requested, despite it is also in the text body (as the Referee says), with the exception of climatic data. In this sense, precipitations and temperatures for each catchment are represented in boxplots in greater detail (Table 1 already presents a lot of variables).

Finally, we have clarified the definition of forest surface and forest maturity. Forest surface refers to the area occupied by the Forest vegetation (trees and undergrowth). Forest maturity refers to the degree of development of such formation and must be estimated using an indirect measure (probability of forest class membership), as the highest probabilities are obtained in pixels where forests are not degraded and that do not present a mixture of other land cover types (i.e. degradation or fragmentation at the pixel level).

15. Comment: In table 2, partial correlation analysis have been performed between hydrological index (i.e., 3DMF, FRE9, and BFI) and forest probability. Yet, in Fig. 3 and Fig. 4, the similar analysis were conducted between hydrological index and forest maturity. Can you please explain this?

The text has been corrected. Forest maturity is estimated through forest probability (see Comment 14). We have now used the term 'maturity' consistently.

References (Anonymous Referee #1):

Gyawali, R., Griffis, V.W., Watkins, D.W. & Fennessey, N.M. (2015). Regional regression models for hydro-climate change impact assessment. Hydrological processes, 29, 1972-1985

Hornbeck, J.W., Adams, M.B., Corbett, E.S., Verry, E.S., Lynch, J.A. (1993). Long-term impacts of forest treatments on water yield: a summary for northeastern USA. Journal of Hydrology, 150 (2/4), 323–344

King, R.S., Baker, M.E., Whigham, D.F., Weller, D.E., Jordan, T.E., Kazyak, P.F., & Hurd, M.K. (2005). Spatial considerations for linking watershed land cover to ecological indicators in streams. Ecological Applications, 15, 137-153

Morán-Tejeda, E., Zabalza, J., Rahman, K., Gago-Silva, A., López-Moreno, J.I., Vicente-Serrano, S., Lehman, A., Tague, C.L. & Beniston, M. (2014). Hydrological impacts of climate and land-use changes in a mountain watershed: uncertainty estimation based on model comparison. Ecohydrology. DOI: 10.1002/eco.1590

Woodward, G., Bonada, N., Brown, L.E., Death, R.G., Durance, I., Gray, C., Hladyz, S., Ledger, M.E., Milner, A.M., Ormerod, S.J., Thompson, R.M., & Pawar, S. (2016). The effects of climatic fluctuations and extreme events on running water ecosystems. Philosophical Transactions of the Royal Society B-Biological Sciences, 371: 20150274

Zhang, L., Nan, Z., Xu, Y., Li, S. (2016). Hydrological impacts of land use change and climate variability in the headwater region of the Heihe River Basin, Northwest China. PLoS ONE 11(6): e0158394. doi:10.1371/journal.pone.0158394

Anonymous Referee #2
Received and published: 7 November 2016

The present manuscript intends to clarify the influence of land-cover on hydrological regime, particularly in extreme events (floods and droughts). The subject fall within the general scope of “Hydrology and Earth System Sciences” journal. The authors suggest the use of Ordinary Least Square (OLS) regression modelling to related explanatory precipitation and land-use variables to three dependent hydrological variables (3 flow indices). My main concern with the manuscript is that the proposed methodology offers several weaknesses, especially regarding the hydrological modelling and the causal relationships between selected explanatory and depending variables, sometimes circular relations. This makes the conclusions, especially the capability to predict extreme hydrological events based on land-cover characteristics, highly questionable. Also, the Introduction does not provide an appropriate “stat-of-the-art”. Authors should deepen the literature review and clearly describe the objectives of the paper, supported by current knowledge about main drivers of change for extreme hydrological events in temperate Atlantic region.

Concerning the methodology, the use of OLS Regression is a widely used technique also in the hydro-climatic domain (e.g. Hornbeck et al. 1993, Gyawali et al 2015). Similarly, Partial Correlation has already been used in other published studies investigating the influence of catchment land cover on river ecosystem components (e.g. King et al., 2005). As we explain below (see answer to the comment about hydrologic analyses), there are no circular relationships. On one hand (empirical), we computed three hydrologic indices using flow and precipitation data over 15 years recorded in flow gauges and meteorological stations. On the other (process-based), we computed two hydrologic parameters (“quick” and “slow” flows) using a model (IHACRES) run with flow, precipitation and temperature data. The use of modelled flows has been avoided given that we have empirical data (‘real’ flows). The hydrological parameters (IHACRES) were used to provide additional context on water interception. Thus, hydrological indices and hydrological parameters constitute two independent lines of computation.

Regarding the objectives and the state-of-art, we have now added new references and improved the manuscript to emphasize that the main objective of this study is to report and observe empirical (based on ‘real’ data) patterns, which might improve the understanding of how forests and other predominant land cover classes influence hydrological extreme events. The contribution of this study resides in two main points: 1) We use 10 catchments in the Cantabrian Mountains (NW Spain) within a gradient of forest cover due to human management since the 15th century but with similar climatic conditions. 2) We defined forest cover not only through forest surface but also using forest maturity. This distinguishes our study from previous literature. First, previous studies at catchment scale have been based on comparisons between only two catchments with different land cover surface. Second, our contribution is focused on the improvement of the land cover descriptors. This makes our study a potential reference document within hydrologic research, as we conclude that (1) forest maturity results more effective than forest surface controlling hydrological extreme events and (2) the understanding of forest effect on flow regimes described in previous literature should be complemented with landscape dynamics.

Specific comments
Introduction: Line 28: "..”land use changes may also affect flood and drought phenology (Scott and Lesch 1997)”. What is flood and drought phenology? Also, Scott and Lesch, 1997 study does not address land-cover effects on extreme hydrological events.

The text has been re-written to clarify this issue.

Material and Methods 2.2 Land cover characteristics Why did the authors choose to obtain the land-cover information from a supervised classification of a Landsat image and didn't use Corine Land Cover data? The CLC2012 is a free inventory of land cover in 44 classes available for Europe. Forest classes in CLC usually represent mature stages of development and are classified according to forest types (broad-leaved woodlands, coniferous forests, mixed forests). Also, It is not justified why such particular land-use classes have been selected. For instance, what about impervious surfaces? Impervious areas have also impacts in runoff patterns. In my opinion this is more a land-use land-cover (LULC) classification system than only a restrict biophysical description of land type.

Authors should clarify this aspect and include a measure of the accuracy assessment for this supervised classification.

The text in the methods section has been modified in order to better explain the use of the selected land cover classes in our study area. This study only uses 3 land cover classes (out of the 7 land cover classes obtained in the classification process). This study is part of a wider research project that looks at the effects of land cover changes on river systems. Only the dominant land cover classes in the territory were presented in this manuscript: forests, shrubs and pasture land, as they occupy in average more than 10% of the studied catchment areas. Other land cover classes (forest plantation, agricultural, denuded rock and urban) were excluded because of their low extension (less than 10 %).

We have developed our own classification in order to get a much better spatial resolution than the provided by the Corine Land Cover (CLC). The spatial resolution of the CLC is 1:100 000, which is appropriate for large scales (identifying and locating major problem areas in order to monitor the implementation of Community and national policies). On the contrary, the Landsat images present a scale close to 1:20 000, suitable to monitor regional land cover in sensitive areas for local management (European Environment Agency, 1995). In addition, the CLC provides a 'hard classification' expressed as vectorial data that does not allow estimating probability of class membership, which is precisely one of our main contributions. The pixels selected for the forest training dataset used to develop our classifications were all pixels that did not present any degree of degradation or fragmentation at the pixel level (i.e. 'pure' old native forest pixels). Thus, the higher the probability of a pixel for the forest class membership the closest the pixel is to a mature forest, and the lower the probability of a pixel for the forest class membership the higher the dominance of other land cover classes. Increases in the probability of forest class membership could be thought of as a continuum from degraded to mature forests, due to successional dynamics.

Therefore, we did NOT use 'a restricted biophysical description of land cover types'. We used a more accurate land cover classification in which all land cover types were included (forest, forest plantation, shrub, agricultural, pasture, denuded rock and

urban). However, we only reported relationships with hydrological indices and hydrological parameters for those land cover types that were relevant in the study area (more than 10% in averaged surface within the selected catchments).

This information has now been incorporated into the text, as well as a measure of the accuracy assessment for the supervised classification.

2.4 Hydrological analysis The methodology is not well described: Authors should explain what is considered a "flood" and a "drought" in this study. 10-year flood? 50-year flood? What is an extreme hydrological event in this climate region? And why the three selected flow indices are the most appropriate to characterize "flood" and "droughts" in this climatic region? What are "quick flows" and "slow flows"? Authors should explain the cause-effect with the three flow indices?

We have made changes in the methodological description in order to avoid confusions in this regard. We now emphasize that in our study there are two independent hydrologic analyses:

> **1. Computing hydrologic indices, associated with 'recurrent' floods and droughts (hydrologic extremes), using flow and precipitation series gathered over 15 years in flow gauges and meteorological stations. These indices were selected due to their common use in the scientific literature, as the same or similar indices (with a different number of days or times the mean flow) have been extensively used in the literature (e.g., Richter et al. 1996; Olden and Poff 2003; Snelder et al. 2009; Belmar et al 2011; Peñas et al., 2016). Using different number of days or times the mean flow in additional tests provided analogous results but the relationships were better using lower number of days for flow magnitude and higher number of times the median for flow frequency. Obviously, this is due to a better characterization of flow rises by minimizing the number of days used to compute their magnitude, as these phenomena take place in short periods, and by increasing the magnitude considered to compute their frequency, as this allows characterizing greater flow rises. This has also been included in the manuscript, in order to complete the discussion.**
>
> **2. Running IHACRES using flow, precipitation and temperature series in order to obtain 'quick' and 'slow' flows (model parameters), associated with the degree of water interception.**

Therefore, in terms of computation, there is no cause-effect relationship between the 'quick' and 'slow' flows (hydrological parameters) and the three hydrological indices. Hydrological parameters and indices have been calculated independently (we assume that this has been misunderstood and that it is why Referee #2 points out issues about 'circular relations' in the general overview). As we had empirical data, we did not compute modelled flows and we only used the hydrologic parameters of the model to provide additional context on water interception. Base flows tend to show a similar response to 'slow' flows (in our case, they increase with forest maturity and decrease with shrubs). This is because base flows tend to have high time of concentration, as they are associated with water interception. The definition of 'quick' and 'slow' flows may help to understand this. We offer the same answer provided to the Referee #1:

'Slow flows' and 'quick flows' constitute the two components in which flow regimes may be decomposed. The former refers to the volumes with highest time of concentration, defined as the time needed for water to flow from the most remote point in a catchment to the catchment outlet. The latter refers to volumes with lowest time of concentration. Therefore, they are employed on the basis that those catchments with higher proportion of slow flows present greater water interception. 'Quick' and 'Slow' flows are model parameters within IHACRES that tend to be used without previous definition. However, we agree that a short definition helps the reader and it has been included in the revised manuscript.

As stated, all these comments and ideas have been incorporated or emphasized in the manuscript.

Results Authors need to detail the results. What is the percentage of the hydrological variance that is explained by the precipitation component and by the land-use component? Please give more information in the results section to support our arguments. Probably explore the joint impact of climate and land-cover effects on extreme hydrological events. The poor correlations with the forest surface can be related with some misclassifications errors of the supervised classification?

Specific changes have been introduced along the text in order to improve its clarity in this sense. We aimed to isolate the effect of land cover on the selected hydrologic indices and parameters from the one caused by precipitation through Partial Correlation Analyses. Therefore, the isolated effect is quantified by the partial correlations.

Regarding the idea of 'Probably explore the joint impact of climate and land cover effects', previous studies have focused on this domain (e.g. Zhang et al., 2016) using modelling techniques. This is out of the scope. This study aims to provide empirical evidence, not modelling the underlying biophysical processes. However, we provide suggestions in this sense for future research and studies.

Concerning the possibility of misclassification errors, the poor correlations with forest surface cannot be related with a misclassification error. Both the overall classification accuracy and forest accuracy were greater than 80%. The estimation of the accuracy has been included in Methods and Results.

Discussion I would urge the author to not over-conclude the results of this study regarding those 10 small-catchments.

We have contextualized in the manuscript the obtained conclusions for oceanic temperate catchments. Nevertheless, we would like to insist in the importance of the gradient and the study area herein presented. Whereas 'paired catchment' studies use two catchments, we have managed to obtain a set of catchments with a gradient in land cover characteristics and empirical data that allow developing regression modelling techniques in order to find patterns in the relationship between land cover characteristics and hydrology. Moreover, the size of these catchments is by no means small (between 30 and 650 km^2) in comparison with other studies aimed at similar objectives (< 10 km^2, e.g.: Smith 1992; Robinson et al. 1991; Lane and Mackay 2001).

Together with the fact that we have provided an additional descriptor to characterize forests (maturity, obtained through probability using fuzzy logic approaches) and concluded that the effect of forest on hydrology may be influenced by land cover dynamics, we reckon that these results are a valuable advance in ecohydrological research.

References (Anonymous Referee #2):

European Environment Agency (1995). CORINE Land Cover. http://www.eea.europa.eu/publications/COR0-landcover. Last accessed: 17/11/2016

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Hornbeck, J.W., Adams, M.B., Corbett, E.S., Verry, E.S., Lynch, J.A. (1993). Long-term impacts of forest treatments on water yield: a summary for northeastern USA. Journal of Hydrology, 150 (2/4), 323–344

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