REVIEWER #1

This is a review of the manuscript entitled, "Wildfire impact on Boreal hydrology: empirical study of the Västmanland fire 2014 (Sweden)" by R. Pimentel and B. Arheimer. This is the first version of the manuscript I have evaluated. In their manuscript the authors describe an observational study which uses remote sensing and hydrologic gage information to evaluate the hydrology impacts of forest fire in the boreal forests of Sweden. The manuscript has strong points. The topic of forest fire effects to watershed hydrology is important to science and society, although has not been explicitly evaluated at C1 the watershed scale. The authors use pre-fire and post-fire remote sensing and hydrologic data to interpret the broad scale impacts of forest fire to hydrological parameters using two experimental and two "control" watersheds. Although the study is generally sound, there are a few issues which need to be addressed in order to ensure the study can be compared to other studies and that confounding factors not be confused in the interpretation of results. I have listed the broad issues below and the compact listing of technical corrections (of which there are many) in the comments of the PDF version of the manuscript. Please refer to the commented PDF manuscript for detailed comments.

We would like to start by thanking Reviewer #1 very much for this thorough review as we think the suggested changes improved the manuscript considerable. It was very kind and generous to help us to this extent.

The following issues need to be addressed:

1. There are many English language grammatical errors throughout the manuscript. Please use first person and active voice throughout the manuscript. I tried to make suggestions throughout the manuscript to improve the grammar, but my comments are by no means exhaustive.

We have revised the English language throughout the manuscript. We really appreciate Reviewer #1's comment regarding this issue. Thanks.

2. Throughout the manuscript the authors imply that the forest fire removed 100% of the forest canopy within the burned area, however this is very rarely the case. It is important the authors more accurately describe the severity of the fire and define the % loss of the forest canopy within the research watersheds or some equivalent information which directly addresses the proportion of forest canopy burned and density removed by forest fire. For example, the two burned experimental watersheds are supposedly different severities (intensity was used in text but I think you mean severity). Although this is never explicitly defined. How do you define the parameters which distinguish the "High Affected Catchment" from the "Mid Affected Catchment"?

We have now explained this better in the manuscript (see Page 5, lines 5-9 in the revised manuscript). The severity of the whole burnt area and specifically in each of the two selected catchments have been assessed based on the comparison of Fractional Vegetation Cover (FVeg) before and after the wildfire. FVeg has been derived from NDVI (Normalized Difference Vegetation Index) following (Gutman and Ignatov, 1998). Two MODIS scenes, one before (28/07/2018) and one after (21/08/2018) the wildfire are used for this purpose. The results are shown in the table below, which is now included in the paper as Table 3.

	Before Wildfire	*After Wildfire*	*Change (%)*	*Severity*
Burnt area	*0.712*	*0.353*	*50*	*-*
Catchment A	*0.776*	*0.123*	*84*	*HIGH*
Catchment B	*0.678*	*0.284*	*58*	*MID*

3. The influence of burned forest to earlier snowmelt is attributed primarily to the opening of the canopy (interception and evapotranspiration), which is probably the first order influence. However, when discussing influences of burned forests on snow hydrology parameters, it is important the authors discuss the "albedo effect" of burned forests darkening the snow pack surface which in concert with increased solar radiation drives a doubling of net snowpack shortwave radiation (Gleason et al., 2013).

We have rewritten the full discussion part regarding the changes in the snow dynamics after the wildfire following Reviewer #1's comments. We have tried to be clearer with the role of the albedo and the charred forest remains and its influence in snow melting (see Page 14, lines 15-19 and Page 15, lines 1-12 in the revised manuscript) and also included the suggested reference.

4. In the discussion, the authors lump all forest fire effects together as though it is the same as forest harvest/clear Authors lump all forest fire effects together as though it is the same as forest harvest. There are many things going on here which should be separated 1. loss of forest canopy = less interception, 2. loss of forest canopy = increased solar radiation, 3. burned forest remains = darker snow albedo. 4. Also, there is no evidence that dust has anything to do with forest fire effects on snow hydrology, therefore omit discussion of dust and Painter et al 2007 citation. Although the darkening of snowpacks by dust is a different darkening phenomenon that may be worth discussing in a different sentence, it has nothing to do with forests or forest fires in snow dominated watersheds. Be clear about what you are attributing for the changes you observe.

We understand that the first version of the discussion in this manuscript was not systematic enough and, as mentioned in the previous comment, we have changed all the first part of the discussion section following your suggestions (see Page 14, lines 15-19 and Page 15, lines 1-12 in the revised manuscript). We have also removed all the comments related to the dust effect throughout the manuscript, as suggested by the reviewer.

5. I am concerned about glossing over any statistics or examination of the inter-annual variability within the pre-fire vs. post-fire periods and how this may be due to the discrepancy in duration of data availability and of data sources. Please include more explicit information about the climate variables and their impacts in the experimental vs. control watersheds.

We have included more information in the manuscript about the methods and data sources used and we have added a comment in the discussion section that the results may be affected by discrepancy in duration of data availability and of data sources (see, page 15, lines 24-30 in the revised manuscript.)

First, regarding statistics and interannual variability, all analysis has been done using the non-parametric Mann-Withney U test, which can be applied to samples with different sizes (in our case 14 and 3 years for periods before and after the fire, respectively) for detecting significant changes between them (see, page 8, lines 8-10 in the revised manuscript.)

Second, data availability is only limited for streamflow and the only full period without observations is the pre-fired period in catchment B. For this case, and for the few gaps in the other 3 gauges stations, we have filled the timeseries with the results from S-HYPE, the national multi-basin model system for Sweden (Lindström et al., 2010; Strömqvist et al., 2012). We have assessed the model performance for the same flow signatures we are using in wildfire evaluation between model and observation. The

assessment shows that only significant changes were observed for the signature annual number of reversal (Rev) (see, page 5, lines 22-27 in the revised manuscript.)

Finally, regarding climate variability, our main goal was to guarantee that the natural variability of the climate did not affect our analysis. The same patterns in weather-related hydrological drivers, is observed between the study periods in both burnt and reference sites (see Page 6, Lines 15-17 in the revised manuscript.)

Please also note the supplement to this comment: https://www.hydrol-earth-syst-sci-discuss.net/hess-2018-387/hess-2018-387-RC1- supplement.pdf Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2018- 387, 2018.

We have listed below all comments in the supplementary that need an answer. Those related to grammatical mistakes were corrected in the new version of the manuscript but not specified below:

Page 1, line 9: Throughout the manuscript the burned area is assumed to have had complete 100% removal of the forest canopy which is very rarely the case. Please more accurately describe the severity of the fire and % loss of canopy density or equivalent information.

In this specific sentence, we are referring to the total burned area by the wildfire. We are not mentioning the severity of the wildfire. Regarding the severity, we have introduced a new paragraph explaining this issue and how we have calculated it (see, page 5, lines 5-11 in the revised manuscript). When we apply the same methodology to the whole burned area, the change in the canopy fraction varied from 0.72 to 0.35. The severity of the wildfire was not the same in the whole area, the differences are shown in the new Figure 1 II.

Page 1, line 16: Be more specific about the exact parameters beyond "snow dynamics"

We have rewritten the sentence.

Page 1, line 17-18: Awkward and vague sentence. What about albedo effect of burned forests? Didn't you quantify earlier snowmelt, so why is this "probably"? What about the post-fire albedo effect?

We have rewritten the sentence mentioned the change in snow albedo after the wildfire. Yes, we did quantify earlier snowmelt, so we change the location of probably in the sentence.

Page 1, line 21: degrees C

We have changed degrees for ºC and checked this error throughout the manuscript.

Page 1, line 24: Again, please explicitly describe how the fire "affected" the total area. There are major assumptions that the entire area was burned equivalently. Use metrics such as area burned with high severity to quantify effect.

We have now quantified the severity of the affected area (see page 5, lines 5-11, in the revised manuscript, and Figure 1 II). In any case, we have mentioned here that the wildfire severity wasn't homogeneous over the whole area.

Page 1, line 28-29: What type of alterations? What type of different components? This is important to the overall point of the manuscript yet address in vague terms here. Described better below but tie together. Also components should be plural.

In this paragraph we are referring to general changes that may occur in land cover (i.e. agricultural changes, the building of a dam or a wildfire). This sentence is just an introduction to set the scene with a broad approach, and the reason we have not been more specific in types of changes neither components of the energy and water budget here is that they will be linked with different changes in each case. In the following paragraphs of the manuscripts, we focus on forest wildfires and are thus more specific.

Page 2, line 14: "that changes in forest structure due to forest fire alter snow accumulation and melt" (cite Gleason et al 2013 and Gleason and Nolin 2016)

We have rewritten the sentence, following Reviewer 1's suggestions and included the references (see Page 2, Lines 28-29, in the revised manuscript.)

Page 2, line 14: "In burned forests, snowpack is more exposed to solar radiation and snow albedo is lower due to darkening of the surface from burned forest debris." There is no evidence that dust has anything to do with forest fire effects on snow hydrology, therefore omit discussion of dust and Painter et al 2007 citation. Although the darkening of snowpacks by dust is a different darkening phenomenon that may be worth discussing in a different sentence but has nothing to do with forest fires in snow dominated watersheds.

We have rewritten the sentence, following Reviewer 1's suggestions (see Page 2, Lines 28-29, in the revised manuscript.)

Page 2, line 20: Citation?

This one and the next sentences are connected, hence, the references are placed at the end. They are already there.

Page 2, line 24: Need to quantify what you mean here, or at least include a citation.

We wanted to express that remote sensing is a powerful technique to monitor land cover changes. We have rephrased the sentence (see Page 3, Lines 1-4, in the revised manuscript.)

Page 2, line 28: Is this true? Citation? Less recurrent than what?

Moritz et al. (2014) and Andela et al. (2017) show the world-wide representation of fired area in the period 1996-2012, with a clear smaller number of burned area in northern latitudes. We have rewritten the sentence and included the new references (see Page 3, Lines 7-9, in the revised manuscript.)

Page 2, lines 33-34: Seems like quite an assumption. Was fire never part of the system until recently? Please include citation for grand statements.

Yes, it was a speculative discussion. During this summer specific more extreme weather conditions took place. If we are in a climate change situation, this kind of circumstances could happen more frequently and for this reason this study could be an example of a direct effect of this possible future scenario. We agree, the sentence was not correctly written. We have rewritten it (see Page 3, Lines 11-14, in the revised manuscript.)

Page 3, line 6: Unless you measured the heat generated from the fire itself, I think you mean "severities", although it is a problem that this severity is never quantified explicitly.

Yes, we were referring to severity. We have changed the word. As we mentioned before an explanation about how we defined severity is now added to the manuscript. (see Page 5, Lines 5-11, in the revised manuscript, and Figure 1 II).

Page 3: line 19: What about the influence of aspect? Looks like both reference watersheds are north facing with no south facing control catchments.

Aspect can influence the solar radiation reaching each catchment, and Reviewer #1 is right when affirms that reference site has a more south facing aspect that burnt sites. However, this is not a mountainous area and the relief is rather smooth, so we assume that the aspect effects are negligible in our case.

Page 4, Table 1: Looks like most real data was taken after fire occurred. Do you trust the modeled data?

Yes, we trust them. As it was written in Page 4, lines 2-7, we have assessed the model performance using the same flow signature we are evaluating wildfire effects for the catchment with full time streamflow series. The evaluation shows that only significant changes were observed for the signature annual number of reversal (Rev).

"Gaps in flow discharge time series, (periods 2002-2014 and 2000-2014 for gauging station A and B, respectively) were filled with flow discharges calculated by S-HYPE, national multi-basin model system for Sweden (Lindström et al., 2010; Strömqvist et al., 2012). The model performance for the study sites was evaluated for each of the defined flow signatures (see Appendix material Table A1); concluding that only significant changes between model and observations appear for the signature the annual number of reversal (Rev)."

Page 4, line 6: Do you mean NASA Earth Observations (NEO) products? This acronym already exists no need to make up another one.

No, in this case the observations came from the NASA, but there are other space agencies and companies producing the actual Earth Observation Products. It is a common and extended acronym used in the remote sensing scientific community. Some examples:

- *https://www.journals.elsevier.com/international-journal-of-applied-earth-observation-and-geoinformation/*
- *https://ec.europa.eu/jrc/en/research-topic/earth-observation*

Page 4, line 8: Again, explicitly describe forest fire severities in the treatment watersheds. *An explanation about how we defined severity is now added to the manuscript (see Page 5, Lines 5-11, in the revised manuscript, and Figure 1 II.)*

Page 4, line 22: Seems a little misleading based on Table 1 information.

Thank you for pointing this out - there was in fact a mistake in the letters- A, C and D are the streamflow station running form SMHI from before the fire. It is B that is the new station installed after the fire by SLU. We have corrected this error (see Page 5, Lines 17 and 20, in the revised manuscript.)

Page 6, Table 3: Shouldn't this be %?

We are given them as a value between 0 and 1, they are fractions [L^2L^{-2}] not percentages.

Page 7, lines 1-2: Do abbreviations need to be spelled out in text as well as in Table 1?

We have spelled them also in this sentence (see page 8, Lines 1-3, in the revised manuscript.)

Page 7, line 3: What metrics specifically?

We have calculated different values depending on the studied variable (i.e. changes in mean temperature before and after the wildfire, variation in the begging of the snow season, annual mean values of fractional vegetation cover). We have clarified this issue in the new version of the manuscript (see Page 8, Lines 3-4, in the revised manuscript.)

Page 7, Table 4: Can these symbols be bigger? Potentially with a grid separating the rows/columns? This is difficult to read.

We will change later according with the specification of the journal (there is no clear rules for the discussion paper).

Page 7, Table 4: Also define the alpha value used for significance in the text.

We have included also this definition in the text (see Page 8, Lines 12-13, in the revised manuscript.)

Page 8, Figure 2: How do you define the parameters which distinguish the "High Affected Catchment" from the "Mid Affected Catchment"?

An explanation about how we defined severity is now added to the manuscript (see Page 5, Lines 5-11 in the revised manuscript, and Figure 1 II).

Page 9, line 1: What about the variability within the two periods? Especially because the pre-fire data seems shorter in duration and reconstructed using modeling, it is important the pre-fire vs post-fire variability in climate variables be quantified.

The climatic variability within the two periods is analysed and showed at annual and monthly scales in Figure 2 and Figure 3S, respectively. In these figures each boxplot represents the variability of the analysed variable for each catchment and period at both temporal scales. Comments about this issue are written in the paragraph where Reviewer 1 made this comment and in the first paragraph of section 4.1.

Yes, both periods have different duration, 14 and 3 years, respectively. We are aware of that and for this reason we have used the non-parametric Mann-Withney U test, which can be applied to samples with different sizes to detect changes between samples.

Climatic variables are continuous throughout both periods. Hence, there are not any reconstruction on them.

Page 9, line 6: Do you mean interannual variability?

Yes we do, but not only this one, also the variability within the year. We are referring to all water cycle dynamics.

Page 9, line 6: Do you mean "confuse"?

Yes we do. we have changed it.

Page 9, lines 18-23: This is somewhat redundant with first paragraph of this page.

It maybe repeats similar ideas. However, first we want to remark that our results are not affected by natural climate variability and second, the values and information given in this paragraph is more specific and is directly linked with Figure 3. On the contrary,

in the first paragraph of the page, we were commented in a broader way the main findings obtained in Table 2 and Figure 2.

Page 9, line 18: Be consistent in your titles for these treatments throughout the manuscript.

We apologized for the non-consistency here referring to burnt and reference sites. We have corrected the mistake.

Page 9, line 23: Does this have anything to do with the period of record in the pre-fire vs post-fire period? Please state what alpha value is used as significant somewhere in the methods.

This is now done. We have already explained that issue in the comment Page 9, line 1.

Regarding the alpha values is already written in the text (see Page 8, Lines 12-13 in the revised manuscript.)

Page 11, lines 10-11: What about the albedo effect of burned forests on snow hydrology?

We have included a new sentence to refer to possible albedo effects (see Page 12, Line 13 in the revised manuscript.)

Page 11, line 14: Is there a better word for this? Or at least describe the implications you mean.

We have changed the word.

Page 11, line 16: Which direction? How many days? Please summarize key points in text.

The spring peak in the burnt areas occurs about one moth (27 days in average for the two sites) earlier than before the wildfire. We have rewritten the sentence (see Page 12, Lines 17-19 YY in the revised manuscript.)

Page 11, lines 18-20: There are many things going on here which should be separated 1. loss of forest canopy = less interception, 2. loss of forest canopy = increased solar radiation, 3. burned forest remains = darker snow albedo.

Of course, there are many things going on and they should be separated. Nevertheless, in this sentence we are talking about higher summer peak, and therefore no comments are done regarding snow. These peaks are directly related to precipitation/runoff events and thus dealing with evapotranspiration, infiltration and runoff generation.

Page 12, line 12: What about mid-winter melt? Also it seems like quicker melt would cause more peaked discharge. Is this what you mean to say? Please use the actual metrics evaluated.

Mid-winter melting maybe is more important after the wildfire, since the snowpack is less protected and has a higher albedo. We do not have clear evidence of that, since we do not have real snowpack depth measurements in the area. However, we have rewritten the sentence to mention this possible effect (see Page 13, Lines 13-15 in the revised manuscript.)

Page 12, line 14. No volume changes were detected?

Changes in volume are represented by the Qsp flow signature. No significant changes for this signature were observed at annual and monthly scale. Only, a statistically significant positive variation was found in September for the high-affected catchment.

Page 13, line 5, Is this defined somewhere? How is this measured? Please do

No, it was not. We have rewritten the sentence to be clearer (see Page 14, Line 7 in the revised manuscript.)

Page 13, line 7, Very vague. Explicity discuss what you mean by "changes". Do you mean changes from before the wildfire?

We have rewritten the sentence trying to be more specific (see Page 14, Line 8 in the revised manuscript.)

Page 13, line 10, These were immediately following fire. How does that compare to the analysis duration of your study? How do you think this might change the result?

We have changed the number showed only to those for the first year to be able to compare with the cited study. We have also specified the tendency showed by the snow remote sensing observations during the other 2 years after the study and compared with some other works (see Page 15, Lines 3-5 in the revised manuscript.)

Page 13, lines 12-17, Why not use data/references from citations which measured these fluxes in burned forests over snow? such as Burles and Boon 2010 and Gleason and Nolin 2016.

We have rewritten the full paragraph following Reviewer #1's recommendations and included the references (see Page 14, Lines 15-20 and Page 15, Lines 1-12 in the revised manuscript)

Page 13, line 15, Discuss how this is different than in a burned forest

We have removed this sentence in the new version.

Page 13, line 15, Badly written needs citation

We have deleted this sentence in the new version.

Page 13, line 15: Redundant with sensible and latent heat fluxes stated above

We have deleted this sentence in the new version.

Page 13, line 15: Use proper citation format, and Gleason and Nolin 2016. Also be clear in your words this is very awkward. Charred soil does not get on top of snowpacks, but charred debris from the forest canopy does.

We have deleted this sentence in the new version and been clear with the correct terminology. Moreover, we have used the suggested citation throughout the manuscript where it was appropriated.

Page 13, lines 22-24: Very awkward final sentence. Please rephrase. Also provide a general closing statement about broader impacts and generally why should we care about these results.

We have rewritten the full paragraph following Reviewer #1's recommendations (see Page 14, Lines 15-20 and Page 15, Lines 1-12 in the revised manuscript)

Page 14, lines 7-9: This is important information in the site description and is a little late to discuss now. Also were these parameters measured or is this simply anecdotal.

We have written a small sentence about this issue in section 1 Introduction (see Page 2, Lines, 7-9 in the revised manuscript)

We do not have any in situ measurement of any soil/rock properties.

Page 14, line 12. Did you measure this? Or is this anecdotal?

We do not have any measurement of the forest interception capacity.

Page 15, line 6: Use increase or decrease to summarize findings, "change" means very little.

We have been more specific following Reviewer #1's comment (see Page 16, Lines 10-11 in the revised manuscript.)

Page 15, line 13: the extent of the burned area. How exactly was this done?

An explanation about how we defined wildfire extension and severity is now added to the manuscript (see Page 5, Lines 5-11, in the revised manuscript, and Figure 1 II).

Page 15, line 15: I think it is important that you end with a general conclusion about what this means on a broader scale and generally why should we care.

We now start the Conclusion section with the following paragraph (before highlighting the specific findings) to give a more general outlook on importance,

"Wildfires do impact Boreal hydrology, which might be of importance in a future climate as an indirect consequence of climate change. Global warming is most significant in the Arctic region and as a consequence, wildfires may become more frequent and sever in boreal forests in the future. However, it should be noted that overall the detected impact on hydrology from wildfires is rather small compared to other on-going environmental changes (for instance direct effects of climate change or river regulations)."

REVIEWER #2

The manuscript investigates the hydrologic effects of the wildfires after the Västmanland fire 2014 in Sweden using a paired-watershed approach. The subject of the manuscript is of interest to a wide range of audience and is well within the scope of HESS. Overall, I think the paper can be published after a thorough technical revision and addressing some minor comments as brought below.

We would like to thank Reviewer#2 for these comments. We think the suggested changes improved the manuscript considerable.

My major concern is with regard to the availability of the observed streamflow data for different time periods at different watershed outlets. It would be helpful if the authors add some information with regard to this issue and if it may have caused some bias in the inferences regarding the hydrologic effects.

We have now included more information in the manuscript about the methods and data sources used and we have added a comment in the discussion section that the results may be affected by discrepancy in duration of data availability and of data sources (see, Page 15, Lines 24-30; Page 8, Lines 8-10; Page 5, Lines 22-27; and Page 6, Lines 15-17 in the revised manuscript.)

Data availability is only limited for streamflow and the only full period without observations is the pre-fired period in catchment B. For this case, and for the few gaps in the other 3 gauges stations, we have filled the timeseries with the results from S-HYPE, the national multi-basin model system for Sweden (Lindström et al., 2010; Strömqvist et al., 2012). We have assessed the model performance for the same flow signatures we are using in wildfire evaluation between model and observation. The assessment shows that only significant changes were observed for the signature annual number of reversal (Rev).

NOTE: there was an error when describing the stations A-D, which is now corrected.

We examined the weather interference with hydrology, to ensure that the natural variability in climate did not affect our analysis. The same behaviour in climate-related drivers is observed in both burnt and reference sites.

The other issue is with lack of accounting for burn severity of the subwatersheds. Often the burn severity is in direct relationship with the extent of the hydrologic effects (please see Havel et al., 2018). So, adding that into the analysis would also add to the scientific content of the manuscript.

We have now added a Table with this analysis to the Method section and we have added this information to the analysis throughout the paper. We agree that this adds significantly to the scientific content.

The severity of the whole burnt area and in specifically in each of the two selected catchments have been assessed based on the comparison of Fractional Vegetation Cover (FVeg) before and after the wildfire. FVeg has been derived from NDVI (Normalized Difference Vegetation Index) following (Gutman and Ignatov, 1998). Two MODIS scenes, one before (28/07/2018) and one after (21/08/2018) the wildfire are used for this purpose. The results are show in the table below.

	Before Wildfire	After Wildfire	Change (%)	Severity
Burnt area	0.712	0.353	50	-
Catchment A	0.776	0.123	84	HIGH
Catchment B	0.678	0.284	58	MID

The introduction of the manuscript should be revised such that it accentuates the novelty of the study. There is a huge literature on hydrologic effects of wildfires and application paired-watershed approach for detecting the changes pre/post some natural or anthropogenic perturbation in the watershed. The introduction should explicitly mention how this study stands out in the literature and how it contributes to the current understandings.

We have now better emphasised our contribution to scientific knowledge in the introduction (see Page 1, Lines 22-30 in the revised manuscript) by inserting a new paragraph at the beginning. We think that the Introduction do cite a lot of relevant literature showing the state-of-art and then we end by describing our contribution. But with this new paragraph up-front this becomes even more outspoken.

There is a huge literature on hydrologic effects of wildfires in general and application paired-watershed approach for detecting various the changes pre/post some natural or anthropogenic perturbation. Although, to our knowledge, the topic of forest fire effects to watershed hydrology has not yet been explicitly evaluated at the watershed scale using various sources of empirical data (see comment from Reviewer No 1).

Here, we combine remote sensing and hydrologic data pre-fire and post-fire to interpret the broad scale impacts of forest fire to hydrological parameters, using two experimental and two "control" watersheds.

It would be beneficial if the authors look at the baseflow as another component of the streamflow. Often baseflow is greatly changed as a result of wildfires and assessing that would add to the understanding of how the hydrologic regime has changed during the post-wildfire conditions.

We have specifically looked to the low flow conditions. In Table 4, 23 flow signatures are directly related to different aspects of base flow: a) magnitude – BFI, Q5; b) frequency – LowFr; and c) duration – LowDurVar. Small duration of low flow condition was found at annual scale in the burnt area, the smaller water retention capacity of the soil favours more constant conditions. At monthly scale, only significant changes were found for Q5 in September due to the same reason (see Table 5, and Page 13 Lines 20-21 and Page 14, Line 1 in the revised manuscript.)

In table 1, the column "Forest area" seems to be in ratio no percent.

Yes, this is correct.

Please revise. Reference: Havel, A., Tasdighi, A., and Arabi, M.: Assessing the hydrologic response to wildfires in mountainous regions, Hydrol. Earth Syst. Sci., 22, 2527-2550, https://doi.org/10.5194/hess-22-2527-2018, 2018.

This reference was valuable and has now been added to the manuscript.