Reviewer's comments are in italic style.

General comment

In this paper, the authors applied the Budyko based method to analyze the effect of climatic variability and forest disturbance on the annual runoff, and then separate the change of the baseflow from the total change of the annual runoff by using conductivity mass balance method. In general, it's hard to say that this paper has any strong innovation point, since it is just a case study based on two common methods. The authors just consider the influence of climatic variability and forest disturbance on the rainfall-runoff relationship, but ignore the possible variation in the relationship between baseflow and total runoff caused by the forest disturbance. The results of paper are questionable. Leaving aside the lack in the methodological contribution, this paper also has a very bad presentation. I think this paper cannot be accepted by this journal.

Response:

We thank Reviewer 2 for the comments on our manuscript. We agree that our manuscript can be further improved with a better presentation and more clarifications on the methods. However, after carefully reviewing the comments from this reviewer, who recommended "rejection", we think that the recommendation is not justified. The followings are our responses.

Firstly, Reviewer 2 did not fully capture the research approach of our manuscript. The main objective of our manuscript is to distinguish the cumulative effects of forest disturbance (e.g. logging, fire, and insect infestation) and climate variability on baseflow. To accomplish this objective, the modified double mass curve (MDMC) was applied instead of using the Budyko based method. In our manuscript, the Budyko based method was only used to calculate the actual evapotranspiration. As far as we know, it is impossible to apply the Budyko based method directly to quantify the effects of forest disturbance and climate change on either baseflow or streamflow. In short, we did not apply the Budyko based method for studying our key objective as stated in the reviewer's comment.

Secondly, the reviewer indicated that the manuscript may not have any strong innovation point on methods. We agree that our study did not develop any new methods which was not our key objective. Instead, we focused on the application side of two existing methods. More importantly, separating the effects of forest disturbance and climatic variability to hydrology are normally aimed at annual streamflow. As far as we know, our study is the first one to separate the hydrological effects of forest disturbance and climate on baseflow in a large watershed. In addition, we further modified MDMC for its application on the baseflow. Thus, our study made the first attempt to quantify the effects of forest disturbance on baseflow in a large watershed.

Thirdly, Reviewer 2 also indicated that the relationship between baseflow and total runoff were not considered. In fact, the relationship was fully considered in our study. The conductivity mass balance method (CMB) is an objective and physical-based baseflow method (see section 3.2 of the manuscript). To implement this method, the regression model (Equation 2) was used to estimate the long-term continuous daily conductivity data (see section 3.2.2). Then, estimated daily conductivity data were adopted for baseflow separation. In Equation 2, the simple Fourier series and flow anomalies (i.e. 1 year, 30 days, and 1 day) were added to address the temporal variations of forest disturbance on conductivity data. Additionally, the paired parameters for the CMB method (i.e. conductivities of surface runoff and baseflow) were also selected for each year rather than using constant values for the whole study period to overcome the temporal variations of conductivity. Thus, we believe that the relationship between baseflow and streamflow was fully considered in our study.

Finally, we feel that Reviewer 2 may not find the correct materials of the manuscript. For example, as indicated from comments 10 and 11, Reviewer 2 did not find any tables. In our manuscript, the captions of figures and tables are assorted in an ascendant order. All tables are listed at the end of the manuscript. Please see the original PDF file in the following link to download manuscript (http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-291/hess-2016-291.pdf). Additionally, the reviewer questioned several terms (e.g., forest disturbance) and methods, which are widely used in ecohydrology (please see comments 1, 2, and 8).

Specific comments:

(1) Line 31: "85.2_21.5", "0.22_0.05". I wonder how the terms 21.5 and 0.05 were calculated. The authors presented neither the calculation method nor the specific meaning of these terms in the paper. The similar issues in other parts of this paper are also obvious.

Response: The term of 85.2 ± 21.5 mm and 0.22 ± 0.05 are respective annual average baseflow and baseflow index (BFI), and their standard deviations for the period of 1954-2013. The calculations were done with Microsoft Excel. They are the common statistics describing the average values and deviations.

(2) Line 35: What is the specific meaning of forest disturbance? Increase or decrease in forest cover?

Response: Forest disturbance is the common term in ecology, and refers to the decrease in forest cover. Forest disturbances are the events (e.g., wildfire, harvesting, insect infestation) that cause the changes in forest growth, structures, and ecosystem functions.

(3) Section 2: The study watershed in this paper spans USA and Canada, but the authors just consider the watershed part in Canada. The part in USA accounts for a quite large percentage of the total area (more than a quarter). It's unreasonable to ignore this part.

Response: Thank the reviewer for pointing this out. Our watershed is an international watershed that spans from the Canada to the USA with the flow in the US portion eventually draining into our hydrometric station in Canada. Unfortunately, data on forest disturbance from the US portion are not available. To consider this, we have checked available historic documents and data, and noticed that there are no major disturbance events occurred in the US part. The US part of watershed is located in the national parks, where forest logging is prohibited. Thus, we believe that our forest disturbance data on the Canadian part is representative for the whole watershed. To address this concern, we have added some discussions on this uncertainty.

(4) Line 128: "630 to 2400". The unit of the elevation should be added.

Response: Elevation is described in meters above sea level.

(5) Section 2.2.1: Are the climatic data from the observed data or generated from climate models?

Response: Climate data were generated from the ClimateBC, which is a standalone program. It extracts and downscales PRISM monthly climate data to scale-free point locations. We used the resolution of 800 × 800 m climate data for our study. Please see Section 2.2.1 for details.

(6) Lines 145: The multiplication sign cannot be represented by 'x'.

Response: Accepted.

(7) Section 3.1: This sub-section which just gives the data of forest disturbance should not be put in the method section.

Response: Forest disturbance is calculated through original forest inventory data, which is not simple forest cover data. In this study, equivalent-clear-cut area (ECA) was adopted as proxy of forest disturbance. ECA is an integrated indicator that considers the forest disturbance and hydrological recovery at the different biogeoclimatic zones, elevations, species and etc. Thus, we placed the forest disturbance calculation in the *Methods* section. To address this comment, we have shorten our descriptions.

(8) Line 325: Why the parameter w in the Budyko-type equation is set to be 2?

Response: This equation (Equation 8) is developed by Zhang et al. (2001). According to Google Scholar, this publication has been cited more than 1250 times. This equation has been validated through 250 global catchments and widely used to calculate evapotranspiration at the catchment scale. The suggested plant-available water coefficient (*w*) for forest is 2 (Zhang et al., 2001). Given the large proportion of forest cover in our watershed, it is reasonable to set the *w* to be 2.

(9) Line 366: The detected breaking point in baseflow is in year 1972, but the most significant change in forest cover occurred in 1991 (see Line 204 in this paper). It seems that the change in baseflow has nothing to do with the forest disturbance. Why?

Response: Thanks for pointing this out. We made a mistake in our calculations on determining the breakpoint of 1972 in the first version of our manuscript. We then re-calculated our data and determined that the new breaking point on MDMC was in the year of 1991, which coincides with forest disturbance history. The breaking point on MDMC were further tested by two breaking point tests (Table 3). Here are recalculated results.

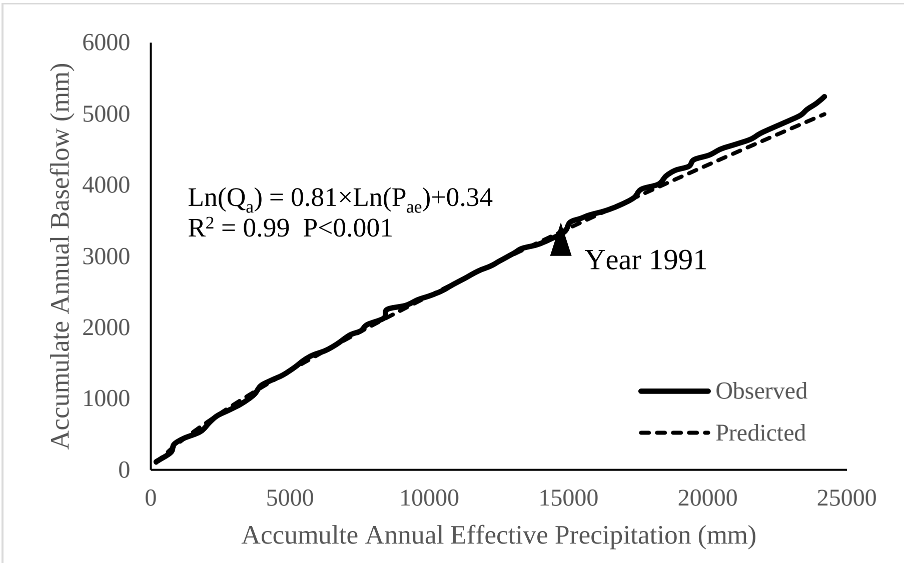

Figure 9. Modified Double Mass Curve of cumulative annual baseflow vs. cumulative effective precipitation.

Table 3. Breaking point tests for the slopes of MDMC for baseflow

Change Point	Pettitt test		Z test	
	K	P	Z	P
Year 1991	389	0.032	-3.39	0.001

Table 4. Relative contributions of forest disturbance and climate variability to annual baseflow in the Upper Similkameen River watershed from 1992 to 2013.

Period	ΔBF	ΔBF_f	$\Delta BF_f/BF$ (%)	ΔBF_c	$\Delta BF_c/BF$ (%)	R_f (%)	R_c (%)	CECA (%)
1992-2003	-5.6	4.6	5.9	-10.2	-13.0	36.5	63.5	14.6
2004-2013	2.7	11.7	14.2	-9.0	-10.9	61.4	38.6	24.6
1992-2013	-1.8	7.8	9.8	-9.7	-12.0	50.3	49.7	18.3

(10) Figures: Why the number of figures in this paper starts from 11? It is inconsistent with the figure number in text.

(11) Tables 1_4 cannot be found in this paper.

Response: The captions of figures in this paper are in an assorted order. Please refer to the manuscript for details. Tables are listed at the end of the manuscript. Please see the original PDF files in the following link for details (http://www.hydrol-earth-syst-sci-discuss.net/hess-2016-291/hess-2016-291.pdf).

References

Wei, X., and M. Zhang (2010), Quantifying streamflow change caused by forest disturbance at a large spatial scale: A single watershed study, Water Resour. Res., 46, W12525, doi: 10.1029/2010WR009250.

Zhang, L., W. R. Dawes, and G. R. Walker (2001), Response of mean annual evapotranspiration to vegetation changes at catchment scale, Water Resour. Res., 37(3), 701–708, doi: 10.1029/2000WR900325.