General Comments:

I have completed my review on the manuscript "Impacts of rainfall features and antecedent soil moisture on occurrence of preferential flow: A study at hillslopes using high frequency monitoring" by Z. Peng, H. Hu, F. Tian, Q. Tie, and S. Zhao. The paper tries to answer the question: how do rainfall features and antecedent soil moisture affect the occurrence of preferential flow on different hillslopes? Generally, the paper uses a quite new technique to evaluate the occurrence of preferential flow. Although the overall results of the paper are interesting, the presentation of the paper (English, structure of written text) is currently still lacking.

Title: second part of the title "A study at hillslopes ..." makes the overall title long and does not provide that much additional information about the content of the paper. My suggestion is to stick to a shorter version of the title.

Reply: Thanks for your suggestion. Given that this study is kind of a case study, we think it would be necessary to present some key information of the study area in the title. We would change the tile into "*impact of rainfall features and antecedent soil moisture on occurrence of preferential flow in a sub-humid catchment*".

Abstract: Some sentences are very long and make it hard to get the main message/ideas of the paper. The paper would improve a lot if the text in the abstract is improved (some specific comments and technical comments regarding this section are given below). Additionally, the abstract should strive to more clearly summarize what the impact of the rainfall features + antecedent moisture conditions are on preferential flow – which factors affect the occurrence and how they affect the frequency/ occurrence of preferential flow.

Reply: Thanks a lot for your suggestions. We will write the sentences into shorter ones, and with the help a native English speaker, we will re-organize the presentation, to make it more readable and more professional. As you suggest, we will illustrate more on the influences of rainfall feature and antecedent soil moisture on preferential flow occurrence, as well as on relevant mechanisms.

Introduction: To create a stronger paper that more clearly demonstrates its "innovation" in the field, I would strongly suggest the authors to more rigidly convey the current limitations of previous research and the added role this paper plays to the discussion.

Additionally it would help to clearly state what hypothesis you have – what you expected as an outcome of your study - and how your findings aid the field. At the moment it is not clearly stated what new concepts/ideas etc. are used (although the method used is e.g. not yet a standard method).

Introduction (2): Related to a comparison of results found in the literature: it makes it easier for the reader to generally know the methods used to analyze preferential flow in all examples. Sometimes the authors do this (e.g. mentioned that a column experiment was used), but critical information about the measurement setup fails (sensors used, dye tracers used, other tracers used?). Allaire et al (2009) - Quantifying preferential flow in soils: A review of different techniques – wrote a whole review on all techniques that could be used compare the results found, which can be used as a reference.

Reply: Thanks a lot for your suggestions on organizing the introduction and presenting the literatures. We will illustrate more to fill the gap between previous studies and our objectives, and state more clearly about the necessity of this study.

Basically, this study was initiated from two considerations. (1) It would be helpful in understanding the processes of subsurface hydrology, if we get the key factors that control the occurrence of preferential flow. Lots of studies have been carried out on this topic. However, contradictory results were obtained in different cases, e.g., the cases of Wu et al. (2014) and Hardie et al. (2013). And to our knowledge, no study on this topic has been carried out in northern China with sub-humid climate and poorly developed underlying soil. Hence, we think this study could be a complementary to the understanding of controlling factors of preferential flow; meanwhile, it would be helpful in understanding hydrological processes of the study area. (2) By far, there are many methods for the detection of preferential flow, but in-suit method is rather limited. The method using wetting front as an indicator, which was proposed by Lin and Zhou (2008)

and later improved by Hardie et al. (2013), could be an alternative option. Since this method has been on applied in only two or three cases to our knowledge, it would be of interest to apply it in our study area, where climate and surface condition are different from previous cases.

English language: currently, there are still a lot of grammar errors and strangely formulated sentences in the manuscript that make the manuscript less easy to read. Authors are advised to ask help from one or more native speakers to improve the level of English of the overall manuscript.

Reply: Thanks a lot for your comments. We will solve the Language problems with help from a Native English speaker.

Authors differ between a rate based and a sequence based classification of preferential flow. I think it is important to realize that the extra use of a wetting velocity provides only an additional method to detect preferential flow. The method itself is not suitable to identify differences in preferential flow processes. The separation between the results and the discussion is not clear. It seems like the results section still partly continues in the discussion part of the paper. Additionally, data is partly already discussed in the results section. When writing a separate discussion, this should only focus on the discussion of the results, not on the disruption of the results. A solution would be to (1) write a combined results and discussion section or (2) to better separate both sections and the aims of both separate sections of the paper.

Reply: We agree that the rate based method is an additional method to the sequence based method, since preferential flow detected by the two methods may occurs at the same time and cannot be absolutely separated. In this study, we classify the preferential as PF-ns as long as non-sequential responses occur. In this way, we think it would be of interest to compare between the preferential flow that shows sequential responses and non-sequential responses.

Thanks for your suggestions on re-organizing the sections. We will combine Section 3 and Section 4 together as a section of "Results and Discussion". We will make more comparisons between our results with those of the previous studies, so as to make our results sounder and more meaningful.

Specific Comments:

Introduction:

Page 1, line 14: Please specify which frequency. I assume you relate to the frequency of preferential flow occurrence. Be more specific, otherwise this is unclear to the reader.

Reply: In fact, all the words of "frequency" in the abstract refer to "frequency of preferential flow occurrence". It will be clarified in the revised abstract.

Page 1, line 15-16: Again, please specify that you refer to preferential flow frequency.

Reply: Thanks for your suggestions. It will be clarified in revision.

Page 1, line 16-17: "Antecedent soil moisture was also significantly correlated with the frequency. However, this should largely be attributed to the differences of soil moisture among sites, since varying range of soil moisture at a specific site was not wide enough to influence the frequency significantly". This is very unclear as the authors talk both about spatial (site-to site) and temporal (site specific range) soil moisture variability. I do not see how the spatial and temporal occurrence link. . .

Reply: We are sorry for the unclear statement. We will improve the presentation in revision. Generally, points of this sentence is as follow:

If the data of all sites were taken as a whole, a significantly correlation between frequency of PF and antecedent soil moisture would be obtained (see Figure 10).

However, soil moisture was continuously lower at some sites than at other sites. Meanwhile, PF occurred more frequently at sites with higher soil moisture (See Figure 11).

There is a chance that an unknown factor, which induces occurrence of PF, is happened to be more significant at sites with higher soil moisture.

In order to exclude this possibility, we need to analysis the correlation between soil moisture and frequency of PF at a specific site. However unfortunately, the varying range of soil moisture at a specific is not wide enough, and no significant correlation could be obtained.

Page 1, line 26 – 28: I do not see how preferential flow can be densely distributed in soils. It is rather a process that is occurring, which either occurs or does not at a certain moment in time.

Reply: Thanks for your comment. We were meant to state that "macropores are densely distributed…" initially. We will delete the inappropriate statement in revision.

Page 2, line 4: The authors write: "Among the many, rainfall features and antecedent soil moisture are two essential control factors . . ." I noticed these ideas are used throughout the papers, forming the general framework of this paper. I think this is nice and agree with this. Nonetheless, I think it is important to define and accurately separate spatial and temporal components. Preferential flow can occur at a specific location, related to local soil moisture (and even rainfall- in case of vegetation - interception) conditions. As an example we might look at specific locations where preferential flow occurred more frequently and relate this to local conditions. At the same time, we can also look in time and specify temporal differences in precipitation and antecedent moisture, which might be related to seasonal/ climatic conditions at a specific point in time. In this case, we might look at the amount of sensor locations that responded under certain rainfall conditions.

Reply: Thanks a lot for your detailed suggestion. It would be a notable improvement to categorize the factors into spatial and temporal components. However, we are afraid that it may not be robust enough to classify meteorology as temporal components, or to classify soil moisture as time-invariant spatial component. Especially in our case, rainfall is spatially heterogeneous, and soil moisture temporally variable, though not very significantly.

Page 2, line 6: "Heppel divided . . . intensity driven and duration driven". I think it only makes sense to make such a statement when explaining afterwards how and why he did this. Otherwise referring to this paper does not really convey a clear message and rather raises questions.

Reply: Thanks a lot for your suggestion. We will illustrate more about Heppel's study, and according to your suggestion above as well, we will re-organize the literature review work of the introduction.

Page 2, line 13: "Wu et al. indicated . . . growth rates along with increasing rainfall intensity". I think it is important to state here how this was measured, as one problem related to the use of soil moisture sensors is that changes in water content are not observed while the soil is saturated (see Graham and Lin, 2011 - Controls and frequency of preferential flow occurrence: A 175-event analysis; Wiekenkamp et al. 2016 – Spatial and Temporal Occurrence of Preferential Flow in a Forested Headwater Catchment; I even believe it is also mentioned in the Hardie (2013) paper). Nonetheless, preferential flow can still occur in reality. This is something to generally note/ keep in mind as a limitation of the method.

Reply: Thanks for your reminding. We will present previous studies in a more detailed and more logical way.

Methods:

Page 3, line 14; Authors obtained information about summer canopy coverage using SPOT, August 2013). This is not sufficient to reconstruct how this information is obtained. Which satellite was used? SPOT 6? Additionally, it would be worthwhile to understand where the 98% comes from – is this the average over all pixels with in the catchment (also: specify resolution).

Reply: We are sorry that we misinformed the source of the canopy coverage information. It should be corrected that the original source is the image from satellite WORLDVIEW-2, and resolution of the image is 1.0m. The image was processed by the Twenty First Century Aerospace Technology Co.,Ltd. (http://www.21at.com.cn/en/), who provided us with the information about canopy coverage.

It should be clarified that 98% is an average coverage of canopy all over the catchment, since some areas in the catchment is free of canopy cover all over the year.

We will add the information above during revision.

Page 3, line 18: ". . .from high to low". Please be more precise. Does this specify the height? If so, please specify that you are talking about altitude (one might confuse it with slope angles). Additionally, I wonder how the effect of canopy can be separated from the geomorphological location on the hillslope, as it seems that all non-vegetated monitoring locations are located on a relatively flat surface.

Reply: "…from high to low" means altitude here. Altitude of each site will be added into Table 1 in revision and the statement will be specified.

Concerning on the canopy coverage, sites S1H1- S1H5 and sites S2H1-S2H3 are located at two typical hillslopes, and both slopes are well covered by canopy, while sites FH1-FH4 are close to 4 meteorological stations, respectively, thus these locations are relatively flat and free of canopy cover.

Page 3, line 21: the authors state that sensors were installed with different maximum depths .e.g. a different number of sensors per location. It would be worthwhile to know why? Was this related to the absolute depth of the soil/ the stone content in a given depth? Additionally, it would be important to know if the number of sensors influences the frequency of preferential flow as detected by the sensor response sequence.

Reply: We are sorry the unclear statement. The probes are installed at different depths for other purposes, while in this study, only the data of the 0-60cm was used, so as to compare between the sites. We will make additional statements to clarify this issue.

Page 4, line 3: Authors state that soils are not that think, but afterwards mention that the soils are 0-2 meters deep. I could imagine that a 2 meter deep soil is not that shallow. To better understand if soils are generally shallow/deep, it would be important to state how deep soils are on average or what the characteristic thickness of soils is (could even be specified for different landscape positions). Is there any information about the soil types that were found at the logger locations (using FAO or American Soil classification)? If available, it would be important to state such information here (and in the related Tables) "Eighty four groups of soil samples. . ." Is this the number of soil samples or is this the number of groups – how many samples per group are there? I guess individual samples were meant here.

Reply: Soils are poorly developed in the study area. They are mostly mixed with gravels and bedrock is occasionally exposed at the surface. To our experiences of soil sampling, it was usually hard to drill deeper than 60cm by maual tools, but we digged deeper than 100cm at some locations. So we think soil depth is highly spatially heterogeneous, but generally shallow.

We will extend Table 2 to include soil properties of each layer. Soil types will be classified by the FAO standard, and will be added in to Table 2.

It should be clarified that the "84" refers to the number of group. A group means soil samples collected at a depth of site, and 3 samples were collected in each group.

Page 4, line 8: Authors mention the usage of Rosetta to estimate Ks values. It would be important to state the function(s) used in the hierarchical artificial neural network model of ROSETTA – how are the measured soil physical parameters used to calculate Ks? Ranges in soil properties per site are referred to in the methods section and given in table 2. What do the authors think about the factors that are the most influential for preferential flow occurrence? Is the rather range of hydraulic properties, the hydraulic properties of the most upper layer, or differences in hydraulic properties within the soil profile important for preferential flow occurrence?

Reply: To our knowledge, the ROSETTA software has been trained by a big dataset of soil properties, and its predictions are reasonably reliable. The soil properties we used to calculate Ks include bulk density, percentage of clay, silt and sand particles.

Unfortunately, we did not think about the influence of soil properties on preferential flow occurrence. But thanks to your questions, we agree that it's of importance in studying preferential flow. We will try to have a discussion on this issue base on our data, and will present the result if it is sound.

Page 4, line 15 – 22: The determination of a rainfall event is commonly only defined by precipitation characteristics itself. In this case, the change in soil moisture at all depths is used. Why?

Reply: This is a special requirement of this study. Since preferential flow is indicated by the responses of probes, we will not be able to confirm the responses are caused by rainfall if measurements of the probes are continuously changing before the rain.

Page 4, line 15 – 22 (2): The determination of a rainfall event relies on hourly thresholds. Is the original 10 minute resolution soil moisture and precipitation used for this approach or is the data aggregated to hourly values to determine the event start and end?

Reply: The original data are used to determine start and end. Since methods used to detect preferential flow are based on high frequency monitoring, we cannot afford to lose any temporal resolution of the data.

Page 4, line 24: Authors refer to the "Hardie et al. (2013) method". It is however unclear what type of method (the classification, mentioned later in the section or the wetting front velocities?)– What specific part of the analysis is referred to? Plus, it would be important to specify this here for reader that has not read the Hardie paper.

Reply: Thanks for your suggestion. Both the classification of preferential flow (i.e., PF-ns and PF-rate) and the methods to detect preferential flow are referred to Hardie et al. (2013). We will specify the statement in revision.

Page 5, line 2: Please replace "penetration velocity of the wetting front" by "wetting front velocity". Do this consequently – also for other parts in the manuscript. Additionally, one could question whether Eq. 1 needs to be written out here.

Reply: Thanks a lot. We will use the phrase "wetting front velocity" in the manuscript. Concerning on Eq.(1), we think it's concise way to illustrate how the wetting front velocity was obtained in this study, though equation is quite simple.

Results:

Page 5, line 24: "Differences . . . 46.8 mm". In which time frame? An hour/event/10 minute measurement/cumulative?

Reply: The number "46.8 mm" is a cumulative difference based on 10 minute measurement at the two sites. We will clarify this issue in revision.

Page 6, line 3: "In order to compare . . . selected". I wonder how the similarity of the events was examined. Should start and end date any of the events be the same for all rain gage locations? If not, how were "rainfall events observed by all rain gages" selected?

Reply: Yes, the start and end time of the selected events are the same for the rain gauges. We will improve the presentation to make it clearer.

Figure 2: please specify the formula used for the curve that was fitted. And what was the RMSE of this fitted curve? In the text, the authors mention that they used a Pearson III curve. Please specify what type of curve is meant (I do not consider this a standard method). –

Reply: We are sorry that we may have miswritten the name of curve. It should be more properly named as Pearson type III distribution. It is a gamma distribution, and the parameters can be obtained from the skewness (Cs) and the coefficient of variation (Cv) of the data series. We will illustrate more about the fitting curve in revision.

Page 6, line 12: Differences between rainfall features were tested against the Gaussian distribution. Why?

Reply: In the beginning, we aimed to confirm that the distribution of rainfall features were significantly different from the Gaussian distribution. But in the second view of the manuscript, this argument appears to be not that necessary in interpretating data. We will delete this comparison in revision.

Figure 4: I would prefer to see the data values in a table as it is difficult to infer the exact significance between sites. A table will additionally provide extra information (exact values). 'Considering the rainfall events: Overall, it is not well specified which rainfall characteristics are used for a specific event. Where the average characteristics for all location used or are the location specific rainfall

characteristics considered? Additionally: which rainfall data was used for the monitoring sites where no rainfall was measured?

Reply: Thanks for your suggestion. We will change Figure 4 to a Table, to have all the values been clearly presented.

In this study, when a monitoring site is no installed with a rain gauge, measurements of the nearest rain gauge were used for this site.

Regarding figure 6: During both monitoring periods, the FH locations had several situations in which they all reacted preferentially. However, there is no situation in which all 12 sensor locations reacted at the same time. This would be an interesting point to bring up and discuss. Additionally, it is not that clear that the top four bars belong to period 1 and the lower 12 to period 2. To improve this, such information could be directly added in the figure. - On the statistics similarity of rainfall: only the similarity of the rainfall characteristics during the 39 simultaneous events was tested. Nonetheless, the events that were not occurring at the same time amongst all sites and that created local differences were not considered. Although these additional events/ variation is number of events do inform us about rainfall heterogeneities, they were "kicked out". Is it fair to afterwards state that precipitation differences did not influence the occurrence of preferential flow, although they might generate local differences amongst locations e.g. antecedent soil moisture conditions, canopy wetness.

Reply: Thanks for your suggestion. We will add temporal information to the two parts of Figure 6.

If we understand correctly, the reviewers means that the frequency of preferential flow is a biased result since we kicked out some rainfall event, unless the rainfall features do not influence the occurrence of preferential flow. We agree on this point. Since the kicked out rainfall events are dominantly light rains, which are less likely to induce preferential flow as presented by the results, the frequency of preferential flow should have been overestimated in our study. However, if we want to compare between different sites, we would hope to check the responses of the sites to the same or at least similar rainfall processes. This is reason why we compared the distribution of rainfall features measured at different locations (see Figure 4), and this is the way to make the results of showed by Figure 11 meaningful.

Discussion:

Figure 7: It is not clear if the rainfall features vs. type of flow included all the specific rainfall amount for all individual events * the individual sites. If this is the case, it is logical that there is an overlap in characteristics, as Figure 6 already shows that individual locations during the same event might cause different responses, which explains why similar rainfall conditions end up in the different classes.

Reply: Figure 7 shows the collective results of the responses of all sites to rainfall. Indeed, Figure 6 shows that responses of different sites to the same rainfall event could be different. But these differences should be ascribed to some other factors, such as antecedent soil moisture, slope gradient or surface cover as we discussed in the later part. Therefore, the relationship between rainfall features and frequency of preferential flow at a specific site may not be the same with that showed in Figure 7. But still, since Figure 7 shows the responses of all sites to the similar rainfall events, the results should be statistically valid.

Page 8, line 5:" The values ranges . . .calculated". Frequencies here probably refer to the total number of sites that responded preferentially. It is important to mention such information specifically – e.g. if you integrated the data over time (to look spatially) or in space (to look temporally). Examples of papers that apply such approaches can be found in Liu and Lin (2015 - Frequency and control of subsurface preferential flow occurrence in the Shale Hills catchment: from Pedon to catchment scales).

Reply: Thanks a lot for your suggestion. We will improve the presentation in revision, to make the illustration clearer.

Page 8, line 22: "..., n =233". Where does this n value come from? Where location specific rainfall conditions connected? Again, it is not clear how rainfall conditions were used – site specific or only event specific? The way this data is used should be better described throughout the manuscript.

Reply: “n” is the cumulative number of the events that observed by the 9 rain gauges and used for analysis. We will illustrate more clearly about this correlation analysis.

Page 8, section “On the influence of antecedent soil moisture”. Again, here it is important to state when antecedent soil moisture conditions were spatially or temporally used. . .

Reply: Thanks a lot for your suggestion. We will improve the presentation in revision, to make the illustration clearer.

Figure 11, page 20: Why is the antecedent moisture not directly plotted against the frequency of preferential flow? This would better show the relationship between both variables.

Reply: We thought to list the names of the sites in the same order with those of other figures (e.g., Figure 6), which may help one’s reading. Thanks for your suggestion, and we will plot the antecedent soil moisture versus frequency of preferential flow, and will have the names of sites be labeled close to relevant value dot.

Page 16, line 5 and page 20, line5: In these different figures (5 and 12), you visualize the soil moisture response to rainfall at different depths. Be consequent and use the same color scheme for both images. Generally, I think it is more difficult to follow the legend in the black and white images (what is what). Therefore, I would suggest either using different grey-tones or sticking to the colored figures.

Reply: Thanks a lot for your suggestion. We will redraw Figure 5 into a colored image, with the same color scheme with Figure 12.

Technical Corrections:

Introduction: technical comments were detailed described for the introduction (as a start and an example). Authors should however ask aid from a native speaker to check the manuscript more detailed.

Page 1, line 9: rephrase “observation was conducted . . ..” Additionally, you might need to specify what observations; this is not clear in this sentence, and it is part of the abstract, sentences need to be very clear (this is the part that is most read).

Reply: Thanks for your suggestion. Besides the background information of the study area, e.g., topography, soil texture, canopy coverage, soil moisture was observed at 12 sites, 9 of which were equipped with rain gauges.

We will specify the “observation” in revision.

Page 1, line 12 “5-10 times of the saturated . . .” Remove “of”.

Reply: “of” will be dropped in revision.

Page 1, line 9(end) rephrase “Totally . . .”

Reply: This sentence will be rephrased.

Page 1, line 13: change "in average” to “on average”.

Reply: We will change "in average” to “on average”.

Page 1, line 19 – 22: “Further examination suggested that topography and surface cover . . . preferential flow”. This sentence is too long and there are unclear connections – why does the sentence end with soil moisture where it started with the factors surface cover and topography?

Reply: We were about the emphasis that topography and surface covers plays a more essential role than soil moisture in controlling occurrence of preferential flow. We will rewrite this sentence into short one and state more clearly.

Page 2, line 18: “though Hardie et al. (2013) suggested . . .” This sentence build-up creates the idea that although Hardie et al. (2013) suggested it, other researchers do not agree. However, the agreement between authors/researchers is not in any way related to what Hardie et al. (2013) suggested. Please rewrite.

Reply: Thanks for your reminding. Since Hardie's et al. (2013) statement is not necessary in this sentence, we will drop the citation of the Hardie's et al. (2013) study here.

Methods/Results:

The authors use the word "rainfalls" multiple times in the manuscript (page 15, caption Figure 2, 3.1 results). There is no such thing as "rainfalls", as the plural form or "rainfall" does not exist. Please rephrase this throughout the manuscript.

Reply: Thanks for reminding. We will check throughout the manuscript, to make sure that the word is properly used.