We would like to much thank Prof. Peng Gao (Referee #1) for his detailed comments and suggestions on the manuscript. These comments and suggestions will be used to greatly improve the manuscript during the following revision. A point-by-point response to your comments is addressed below.

In this study, the authors attempted to explain the different channel planforms of four reaches in the source area of Yellow River, China using (partially) measured water discharge, stage, and cross section data, as well as qualitative description. First, I think this area is unique regarding to the world large rivers and thus is worth studying. Second, the river dynamics in this area is very complex and hence is very hard to capture. Therefore, I think this study is significant and potentially very useful for understanding the river environment in source areas for large rivers in general. However, I think the authors need to fix a series of problems in the current manuscript before it reaches the level for publication.
Response: Many thanks for your positive comments and pointing out the weakness about the manuscript. We will strive to update the manuscript according to your suggestions.

I describe them in details: Introduction: the authors spend too many sentences describe the progress in channel planforms, in particular braided and anabranching rivers (pages 2 to 5). Instead, I think they should reduce these part.
Response: OK, we will shorten the introduction and focus on the knowledge gap from the existing studies. We feel it is important to contextualize this study, highlighting similarities and differences with conventional literatures. We will give very careful attention to this issue.

In the meantime, they should expand the studies on river diversity in the source area of Yellow River (lines 17-31, page 5) and explain what we need to explore further in details, which could lead to the objectives of this study. There are quite a few English problems in this section.
Response: Good suggestions. We will enhance this part so as to make our studies more sense, emphasizing how and why this intriguing yet understudied part of the work relates to other areas. Meanwhile, the English will be polished and double-checked again.

Sections 2 and 3: these two should be combined into one section. Also, Figs 1 and 2 should be combined.
Response: OK, I can do it.

Section 4: it is very important that the authors specify what value of Manning's n are used for each of the four reaches when they introduce their model because comparison of their difference would provide a quantitative means of showing the impact of vegetation on river morphology. The sentence in lines 4-5 on page 9 does not make sense.
Response: We will specify Manning's *n*, and discuss variability in roughness. We will reconsider the sentences indicate.

Section 5.1.1: Page 10, Lines 14-15: what is the difference between middle and high flood stages? It might be more informative if this description is tied to Figure 8. For example, can one say that middle flood stage may be represented by the high discharges in September and high flood stage may be reflected by the high discharges in July?
Response: Yes, the difference between middle and high flood stages is not very distinct. Here the middle stage means that the channel flow partly submerges the bar surface, but the stage does not completely inundate the vegetation. Therefore, we need to add more explanation on the middle and high flood stages in the section.

Page 10, Line 18: if a water depth of 2.0 m represents the bankfull discharge, then what does the water depth of 3.0 m represent? Can I say that h = 2.0 m is the height at the top of the stable bars in middle channels?
Response: Sure. If a water depth of 2.0 m represents the bankfull discharge, the water depth of 3.0m represent that 1m is inundation water depth. We can think *h*=2.0m is the height at the top of the stable bars surface, but does not submerge the vegetation (i.e., trees)

Page 10, lines 23-32: the message delivered by this paragraph is very vague. It seems to me that the data in October in both 1968 and 1984 follow the curve formed by the data in June and July. If the authors believe there are significant difference between June and July, and August and September, why not use the data in the two periods to run non-linear regression (power function) and see if the exponents of the two are significantly different?
Response: Using non-linear regression for the data in different months in both 1968 and 1984 is very good choice. We will do this job to obtain the exponents so as to quantitatively explain the difference.

The authors should explain quantitatively the geomorphological significance of the two different trends in Fig. 8a and 8b (i.e., the trend formed by the data in March and April against the trend formed by the remaining data). Also, the difference between the high scatter trend for low discharges (probably low flood stages) and regular trends for high discharges (probably high flood stages) should be elaborated. The key is to explain why channels in this reach is semi-braided and semi-anabranching. My guess is vegetation on bars assures that during low and middle flood stages, bars and islands are relatively stable, while during high flood stage, they are unstable. Figure 8 should be used to make this point clear.
Response: We agree with this suggestion regarding analysis on Fig.8a and 8b and their underlying meaning. Accordingly, we will explain quantitatively the geomorphological significance of the two different trends and why channels in this reach are semi-braided and semi-anabranching, further, emphasizing the role of vegetation.

Page 11, Lines 1-11: this paragraph is about Fig. 9. I think the figure shows a completely different aspect of stream channels in this reach: channel morphology before 1976 is different from that after 1976. This difference is represented by the two different trends of the data. The authors should run non-linear regression to establish power functions for the two different trends in each listed month and then link this difference to the possible difference of vegetation cover in the two different time periods. This would strengthen the analysis a lot. Fig. 10 is not well tied to the data shown in Figs.8 and 9. It is nice, but there lacks evidence to support it.
Response: OK, it is very good suggestion. We will adopt non-linear regression to build power functions in each listed month and link these differences to the possible difference of vegetation cover. Moreover, we will rethink Fig.10 and augment the analysis on Fig.9.

Section 5.1.2: First, the authors should mentioned Fig. 11 first and then Fig. 12.
Response: OK, we will correct this.

Second, the big problem here is that the postulation raised here (lines 15-19 on page 11) is not fully supported by the only data shown in Fig. 11. The stage data in Fig. 11 are not sufficient to argue the change of flow regime exactly because the channels here are anabranching channels. This means that the same flow stage in different seasons might be associated with different water discharges. Maybe there are no water discharge data available in this reach. If this is the case, the authors should re-think their arguments: the fact that these channels are stable means that sediment (bedload) supplied from upstream (i.e., the Dari reach) is balanced by the sediment transport

capacity in this reach. One way might be useful is to compare the supplied bed load based on the prediction made for the Dari reach with the transport capacity predicted in this reach. The authors should expect that they are similar or very close to each other. Then, the impact of vegetation on the hydraulics might be reflected in Manning's n used in the bedload model. Comparing this value with the one used in the Dari reach may show the impact of vegetation on the stable status of this reach.
Response: We agree with the detailed analysis above. Since Fig.11 did not fully support our analysis, we continue to collect the data of monthly-channel discharge and monthly-sediment transport rate in four hydrological stations (Huangheyan, Dari, Maqu, Tangnaihai). New data and analyses will strengthen this section, in particular, the impact of vegetation more distinct.

Section 5.1.3: This reach is a tributary. If the authors have no water discharge data in this tributary, I suggest to delete this part completely from the current manuscript. This is because only showing a postulated diagram (i.e. Fig. 13) is insufficient to convince the readers about the status of this reach.
Response: OK. In section 5.1.3, the Lanmucuo River is a small meandering river which has no hydrological data, but we conducted field investigations during 2011-2015. Especially, in 2015 we measured the cross-section and mean velocity in the middle reach. Perhaps we delete the Fig. 13, but add other data or figure so as to keep the integrity of this study.

Section 5.1.4: This reach is unstable. Again, just using the temporal changes of channel sections between three years (i.e., Fig. 15) is not enough to explain how vegetation affects them. Again, I think it is very important for the authors to predict bedload transport rates and then use them to calculate the mean sediment load in this reach. By comparing this (or these) mean value(s), the authors may argue that why the reach is not stable. In the meantime, comparing the value of manning's n used in this reach with those used in the first and second reaches along the main river would provide evidence of the impact of vegetation on river morphology.
Response: Yes, the braided reach of Daheba River is quite unstable and the vegetation effect can be ignored here. Actually, the authors have predicted bedload transport rates in this reach. Unfortunately, there are no measured data of bedload transport rates for comparison.

Minor points:
Lines 5-8 on page 6: this description is very confusing;
Response: OK. We will revise it later.

Lines 17-18 on page 7: why should the stable reach have high bedload transport capacity?
Response: The reach is very stable because the dense trees develop on bars/islands as well as river banks. When over-capacity bed load is incoming, the reach is very stable because trees densely develop on bars/islands. When over-capacity bed load is incoming, the stable anabranching channel can not be widened and keep high velocity within the channel so as to efficiently transport bedload relative to unstable braided channel.

Lines 4-5 on page 9: what does the rivers in an arid area have anything to do with rivers in the study area?
Response: Here we cited the references for arid area to justify the correctness of using the Manning formula as flow resistance.

Figure 1: Please mark R1, R2, R3, and R4. Also, only use the arrow to show the direction of flow.
Response: OK, we will add R1,R2, R3, R4 in Fig.1 and correctly use the arrow.

In the legend, ‘Tributary’ and ‘Trunk stream’ should be reversed. Please use ‘Main stream’ rather than ‘Trunk stream’;
Response: Yes, I will change it immediately.

Figures 3-6: these figures should be combined into one figure;
Response: No problems. We are able to combine them into one figure.

Figure8: please use the same legend for the two figures;
Response: OK, I will revise it quickly.

Figure 14: it does not help much in understanding the difference between the regular and flood conditions;
Response: We will reconsider this figure and choose better images.