Dr. Yunfa Miao
Donggang West Road 320
Lanzhou, Gansu 730000
China

August 8, 2017

Dear editor and reviewers,

Thank you very much for your valuable and encouraging comments to revise and resubmit our paper [Vegetation and fire anomalies during the last ~70 ka in the Ili Basin, Central Asia]. We have completely revised the paper and hope that it meets with your and reviewers' approval. We followed most of the reviewers' suggestions, especially two sections ('4.2.2 Taphonomic effect' and '4.2.3 Sedimentation process effect') and two new figures (Figures 8 and 9) are added in order to make the discussion clear. Our responses to the reviewers' comments are underlined. The language of our manuscript was helped to be improved by native English-speaker GeoEditing Company again. Here we show every correction after our revision as "marked manuscript" in order to easily identify the places of changes. Line numbers as remarked by the reviewers in the original manuscript are named firstly. Line numbers named in our comments to the reviewers' remarks refer to the marked manuscript. Then the revised manuscript is also submitted.

Thank you again for re-considering this manuscript.

Sincerely yours,

Yunfa Miao

Yougui Song

Yue Li

Shengli Yang

Yun Li

Reviewer #1:

Major points

1) Anthropogenic drivers of ecosystem changes after ca 36k. The authors conclude that the lack of a marked climatic signal (as inferred from NGRIP, and other local-to-regional archives,) implies that the major driver of ecosystem change should be attributed to humans But as "absence of evidence is not evidence of absence", this argument appears rather weak, and not supported by data. The same vegetation/fire change after 36k could be explained by a series of other factors (which the authors seem not to fully account for) and in particular: 1) ecosystem response to a local climatic event, which is not evident at regional to continental scale. In this respect, the authors do not have any climatic proxy in their data, and thus their discussion of climate variability is quite limited. 2) a taphonomic effect due to loess deposition/changing sedimentological conditions, particularly for charcoal deposition (see also point 2). In addition, more direct evidence of humans in the area (e.g. from archeological data, or from direct anthropogenic pollen indicators) are lacking. Therefore, conclusions such as "it is not difficult to link the local fire anomalies during 47.5-36 ka in the Ili Basin to human activities: the increased occurrence of local fires (for cooking, or burning the uncultivated land) quickly destroyed the vegetation, causing the observed vegetation degeneration." (Line 322-325) are not fully supported by the data. Similarly, a statement such as "the coeval local fire intensification supports human activity as a factor causing fire anomalies after around 6 ka. This relationship can be similarly extended to observed fire anomalies at 47.5-36 ka" (Line315-418) seems very controversial considering that population size was remarkably different, and thus not directly comparable.

Response: Many thanks for your crucial suggestions. After careful consideration, the conclusion of "the major driver of ecosystem change should be attributed to humans" has been rephrased after discussing the particular effects of taphonomic and sedimentary processes. We believe that the ecosystem anomaly was a response to a local climatic/ecologic event. Here we try to respond to each question in turn.

1) The pollen and microcharcoal records are amongst the most important data for

paleoenvironment reconstruction using sediments. Here, the grain-size of dust particles and mineralogical assemblages are also added into the text as a new figure (Figure 8) to compare their paleoenvironmental implications and support our hypothesis. So we think the pollen/microcharcoal data together with grain size and mineralogical data create a stronger story. Please see Lines 260, 314-320 (Figure 8), and 352-377.

2) Yes, the discussion of the 'taphonomic effects' is necessary due to its importance in understanding the deposition/changing sedimentological conditions (Please see **4.2.2 Taphonomic effect**', Lines 336-352). Here, another potential factor, 'sedimentary processes' is also added into the 'Discussion' as a new section (**4.2.3 Sedimentary process effect**, Lines 352-377). After considering and excluding both taphonomic and sedimentary process effects, the possibility of human influence is considered to be stronger than in our original manuscript, although there is still lack of direct archeological data/anthropogenic pollen or other proofs.

3) The sentence in Lines 322-325 was deleted, the statement in Lines 315-418 was kept because, although the population size was remarkably different between during the late Holocene and period of 47.5-36 ka, the similarly high values of ratios of $MC_{>50\mu m}/MC_{>50\mu m}$ during these two periods are absolutely unrelated to the population. This mainly describes the distance between combustion site and studied section.

In summary, after substantially reorganizing the discussion, we believe the renewed manuscript is now stronger.

2) The authors analyze microscopic charcoal of various size classes, account for morphological structure, and link to specific C2 fire properties (e.g. frequency and severity/intensity). Unfortunately, such properties of the fire regime are very difficult to be discerned from microscopic charcoal alone, and lacking a calibration study specific for this archive (loess) and the location. Other factors (change in fuel type, depositional processes, what else?) may be also responsible for some of the observed patterns. Most importantly, charcoal is usually reported as influx, rather than concentrations (as it seems to be the case here), thus does not account for the changes

in sediment rates evident from your depth/age relationship (Fig. 2).

Response: Yes, another good question. In fact, the black carbon, such as charred plant biomass，charcoal，char, soot and ultimately graphite after combustion, are used to reconstruct the paleofires. The climatic conditions，fuel type，fire intensity，combustion temperature，duration of the fire etc., are usually considered as the main factors in the fire description, but fire reconstructions for the past 70 ka are still very rare in contrast to the pollen as well as other environmental proxy records.

Here, we think the most important aspect of this manuscript is that fire in the Ili Basin over the last 70 ka is discussed together with the pollen data for the first time. Without pollen data, we think interpretation of the microcharcoal data would be very limited. Second, although many factors seem to influence the fire process, the basic natural characteristics of the paleoclimate, vegetation types as well as lightning in the Ili Basin remain broadly stable throughout the whole sedimentation period due to its particular location (Central Asia). No other particular factors could have affected the fire patterns except for humans. Thirdly, besides the fire frequency/strength, this is the first time that the distance between the combustion sites and study section in the wind-blown Central Asia has been discussed, but we know there are many difficulties and some unresolved problems because no other established references can be directly cited. So, after carefully considering all questions raised by you and Reviewer #2, we substantially revised the 'Introduction' and 'Discussions'.

Finally, following the reviewers' concerns, we also think that the influx is a good proxy to describe the fire frequency/strength, because it does not account for changes in sediment rates evident from your depth/age relationship. Here, the detailed information of microcharcoal influxes is shown as below (Figure R1). The zone divisions in this figure directly follow Figure 3. For convenient comparison, the MC result as shown in Figure 5 is also shown here. From Figure R1, the anomalies between 1070 cm and 780 cm are still clear, and have been well presented by the results of MC (Figure 5). The most obvious differences occurred at the bottom of the section (1750-2100 cm), characterized by almost the lowest values of influxes relative to typical MC. This can be explained by the lowest sedimentation rate (only ~15

cm/ka) in this part, which was merely a tenth of the younger rate (150 cm/ka; the sedimentation rate changes have been shown in Figure 1 before).

Beside the roughly similar results between the MC and influxes, the pollen concentrations have been widely used to describe the paleoenvironment and paleoecologic changes, and the interesting result of the pollen concentrations is its similarly obvious change which occurred at around 41-36 ka. So, after considering these two points, the microcharcoal influxes will not be presented in the manuscript unless necessary.

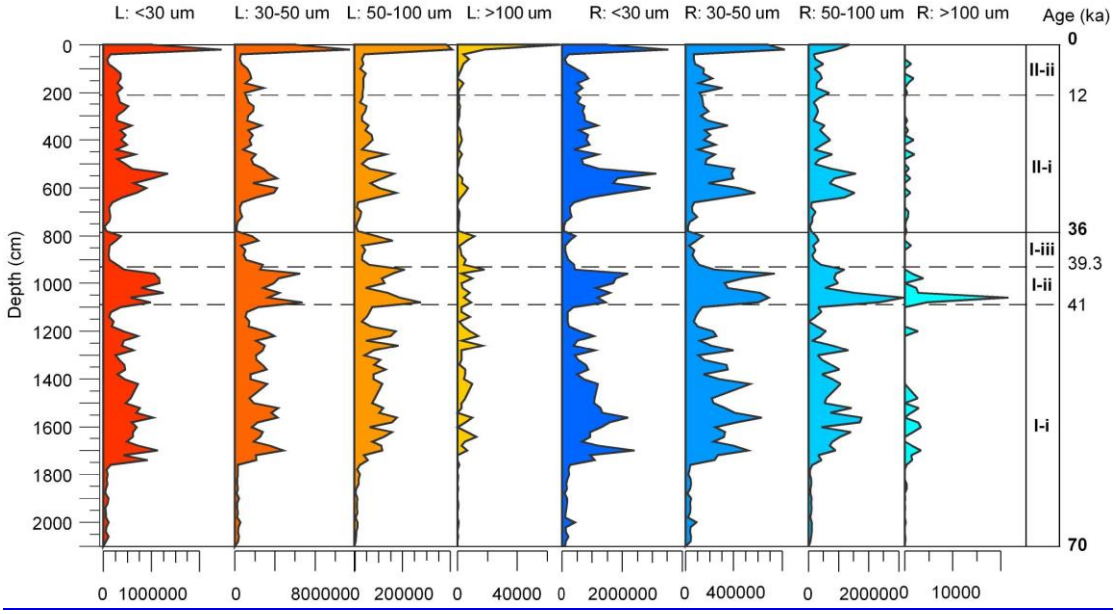

Figure R1. The microcharcoal influx records for different sizes and shapes in the NLK section (unit: grains/cm^2/ka; L: elongated shapes; R: rounder shapes; zone divisions follow Figure 3 in text).

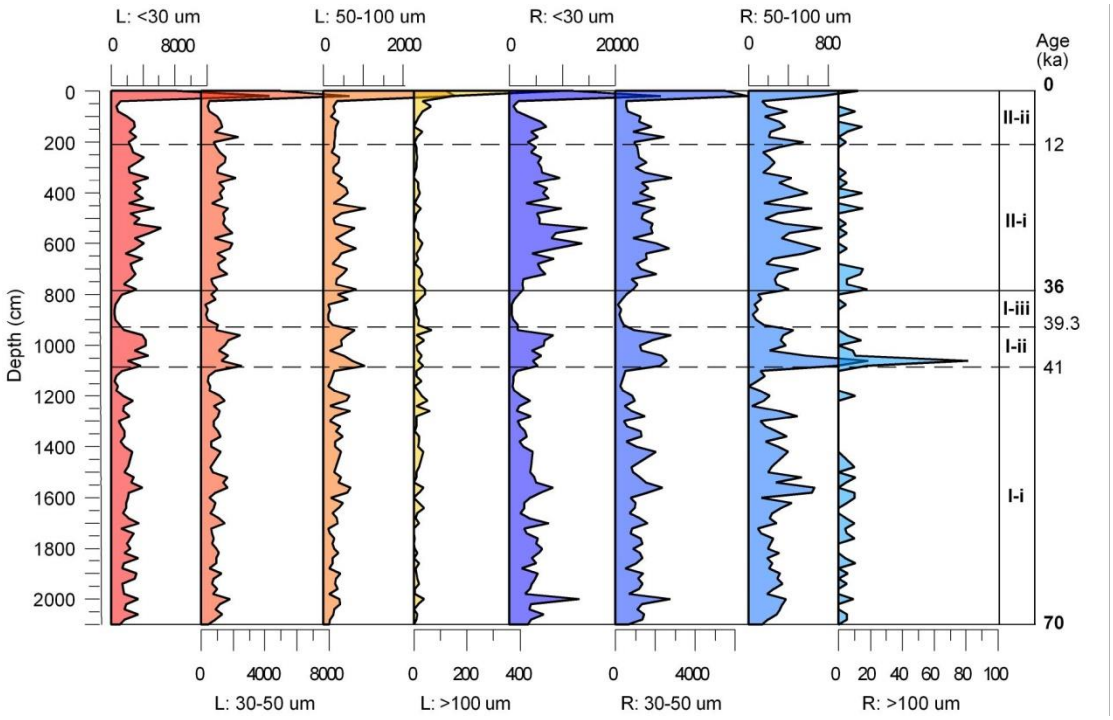

Figure 5. The MC records for different sizes and shapes in the NLK section (unit: grains/g; L: elongated shapes; R: rounder shapes).

3) The main conclusion that" In future, the use of a massive and sustained ecological program of vegetation rehabilitation in the arid and semiarid region should reduce the risk of destructive fire in order to avoid a similar local vegetation disaster to that which occurred at 36 ka" sounds quite anachronistic. I suggest rephrasing, or even remove it.

Response: Yes, removed.

Minor points.

- Introduction: first 2 paragraphs are too general and could be better focused, e.g. highlighting the lack of paleorecords in this deposition environment, and how loess can be an alternative archive.

Response: These paragraphs have been revised accordingly (Please see Lines 59-80).

- L83. "...prevailing westerly winds, down its axis". Can you clarify? I'm not sure what you mean here.

Response: Yes, ‘down its axis’ is deleted now.

-L102 not clear what you mean for “rubification” in the figure.

Response: Yes, this expression of ‘rubification’ is deleted.

- L155, and L213-214. You should better explain what “similar concentration and percentage pollen” do mean for the overall interpretation of the pollen record.

Response: It is changed as followings: ‘The pollen concentrations also follow a similarly stable trend except for the anomalies between 41 and 36 ka’. Please see Line 236.

- L179 should be Asteraceae

Response: Yes, corrected.

- L286. I do see changes in the charcoal, but I don’t seea sharp change after 36k, compared to before.

Response: Yes. It should be ‘a sharp decrease to a normal level’ (Please see Line 396).

- Fig 2. Not clear what the blue/green and red series do represent in the final depth age model.

Response: Yes, you are right. Two sentences have been added into the caption: ‘Radiocarbon ages (Beta and XAAMS) appear to saturate below a depth of 6.5 m at ca. 30 cal ka BP (purple dashed line), while the OSL ages continue to increase with depth. The OSL ages are used as age-depth model’.

- Fig 3. A better age scale would help. Plus, adding charcoal would make the charcoal/pollen comparison easier.

Response: Sorry for missing your first question. For the second, we still want to separate them into two independent plots for clear presentation because if they were

compiled together the process would become unclear.

- Fig 6. You state that “no (climate) anomalies occurred during 41-36 ka in the climate proxy presented in the figure. The “arid index” from Central Asia, though, seems to show a significant increase in aridity, which is almost synchronous with the fire/vegetation change that you discuss. What do you mean, then, by “anomaly”? Also note that you should also account for the age C3 uncertainties among the different records, before attempting any regional comparison.

Response: A very good question. We carefully checked the arid index again in Figure 6 and it seems that the arid anomaly is still obvious, albeit much smaller than our arid index based on pollen assemblages at around 36 ka. So, the sentence of ‘no obvious anomalies occurred during 41-36 ka’ has been deleted. Here, the ages of all proxies cited are together regarded as good age controls, otherwise it will become much more difficult to correlate them together.