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Climate of the Past Discussions

Interactive comment on "Vegetation and fire anomalies during the last 70 ka in the Ili Basin, Central Asia, and their implications for the ecology change caused by human activities" *by* Yunfa Miao et al.

Anonymous Referee #1

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This manuscript presents a long-term vegetation and fire history from Central Asia covering the last ca 70k yrs. The authors combine pollen and charcoal data from a loess sequence and discuss the relative roles of climate and human impact for the observed ecosystem changes. On the base of their vegetation and fire data, and of other available paleoclimatic sequences, they conclude that human impact is the most likely factor, supporting the idea of an "early human impact" on ecosystem changes in this region. The question whether humans may have modified vegetation and fire well before the current interglacial is certainly intriguing, and with many implications for our under-

standing of the legacy of human impact on pre-Holocene ecosystems. Unfortunately, neither evidence from their data, nor the argumentations presented in the discussion session, seem to support particularly this idea, which thus emerges as speculative and very elusive from their conclusions (see major points below). The paper should be refocused on a more balanced view of the possible causes of vegetation changes (including all possible factors, see below), and considering that the weight of evidence at the present does not fully support any prevailing cause. 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." (L 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" (L315-418) seem very controversial considering that population size was remarkably different, and thus not directly comparable. 2) The authors analyze microscopic charcoal of various size classes, account for morphological structure, and link to specific

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).

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.

Minor points. - Introduction: first 2 paragraphs are too general and could be better focused, e.g. highlighting the lack of paleorecords in this deposition environments, and how loess can be an alternative archive. - L83. "...prevailing westerly winds, down its axis". Can you clarify? I'm not sure what you mean here. -L102 not clear what you mean for "rubification" in the figure. - L 155, and L213-214. You should better explain what "similar concentration and percentage pollen" do mean for the overall interpretation of the pollen record. - L179 should be Asteraceae - L186. I do see changes in the charcoal, but I don't see a sharp change after 36 k, compared to before. - Fig 2. Not clear what the blue/green and red series do represent in the final depth-age model. - Fig 3. A better age scale would help. Plus, adding charcoal would make the charcoal/pollen comparison easier. - Fig 6. You state that "no (climate) anomalies occurred during 41-36k in the climate proxy presented in the figure. The "arid index" from Central Asia, though, seem 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

uncertainties among the different records, before attempting any regional comparison.

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