

## ***Interactive comment on “Late Miocene-Pliocene climate evolution recorded by the red clay covered on the Xiaoshuizi planation surface, NE Tibetan Plateau” by Xiaomiao Li et al.***

### **Anonymous Referee #2**

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Li et al. analyze a red clay sequence from Northeastern Tibet that spans from 6.7 to 3.6 Ma for carbonate content, major and minor element concentration, magnetic susceptibility, and grain size. The authors suggest that the proxies show a locally dry, westerlies-dominated climate from 6.7 to 4.8 Ma followed by a wetter, monsoonal climate from 4.8-3.6 Ma. Li et al. propose that closure of the Isthmus of Panama might be responsible for this climate shift. Although the proxy records are a valuable addition to our understanding of climate change and variability, the manuscript has several major flaws that should prevent publication in its current form.

General Comments:

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Spelling and Grammar:

I have not edited this manuscript for spelling and grammar. I strongly encourage the authors to seek assistance from a very proficient or native English speaker. Also, please review the manuscript for organizational mistakes (e.g. Figures 2 and 3 are not cited in the text; incorrect citations).

Statistics:

The authors need to provide more information about the magnetostratigraphic ages. What is the temporal resolution of the records? What are the temporal uncertainties? Can the records accurately resolve all the cycles you discuss (e.g. precession)? Does variable deposition rate impact the signals?

How did the authors decide that 4.8 Ma was the appropriate transition point? It seems arbitrary to me. I see no clear transition in Figure 3. Are the two periods (6.7-4.8 Ma and 4.8-3.6 Ma) statistically distinct?

I find the signal filtering in Figure 5 questionable. First, the authors filter the data at frequencies with insignificant power (e.g. the 100 kyr filtering of carbonate content). Further, the most significant signals exist at frequencies that are difficult to explain, which the authors dismiss, and many of the discussed signals are barely significant at 90% confidence. The wavelet plots highlight the limited signal strength. Even if the filtered signals are sound, the filtered signals changes do not well align with the benthic  $\delta^{18}O$  record.

Interpretation:

The potential drivers of the climate signals are often overstated. Connections are made with limited support. Many of the mechanisms discussed are still debated, particularly the Isthmus of Panama hypothesis and timing of Tibetan Plateau uplift. At the least, the authors need to do a better job citing recent literature and discussing the remaining uncertainties. Also, many of the citations are not primary sources for the associated

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statements.

Specific Comments:

Line 51: Earth's orbital went through many cycles over this period, so the "orbital configuration" statement does not make much sense.

Lines 51-53: These statements, such as "comparable temperatures in the tropic region", require citations.

Line 65: Please clarify the link between mean tropical Pacific east-west gradient and ENSO.

Line 68: The timing of uplift of the Tibetan Plateau is heavily debated. . .

Line 70: Lunt et al. (2008) is not a direct source for the closure of the Isthmus of Panama. More recent works debate the timing of closure (e.g. Bacon et al., 2015; O'Dea et al., 2016).

Line 72: This statement is also not well supported. For example, Lunt et al. (2008), who are cited earlier, found closure of the Panama seaway to have little influence on NH glaciation. In general, the authors need to update their citations and discuss the literature more thoroughly. These ideas are far from settled, yet they are presented as facts.

Line 80-82: Citation?

Line 85: "Arctic volume" means "Arctic ice volume"?

Lines 105-106: This statement does not make sense.

Lines 136-140: Sources for these data?

Line 154: This requires more detail.

Line 175-182: How did you decide on these intervals? Did you test that they are statistically distinct?

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Line 224-226: How are you sure that it relates to monsoon strength? Could it be seasonal or evaporative changes?

Lines 235-237: Both statements are significant at 99% confidence?

Lines 282-286: Are you sure these signals are real? If so, how might you explain the cycles not related to orbital variability?

Lines 294-295: Doesn't this "incomplete nature of the red climate time series" impact all of the frequency analyses? How can you distinguish real and fake signals?

Line 302: I believe that a 23 kyr filter makes more sense for the climate response to orbital change.

Line 304: What record? Lisiecki and Raymo (2005)?

Line 306: I do not observe this in the filtered record. . . Is this change significant? How much do these filter components contribute to the complete signal?

Line 309-310: Where is this shown? The 41 kyr signal in the benthic records do not well align with the data.

Lines 317-319: I see no clear changes in the records. You need statistical support.

Line 361: ODP source?

Line 368: ". . . roughly parallel. . ." I do not see a correlation. Please quantify.

Line 384: This is possible but not necessarily the case.

Lines 392-393: Cooler air can hold less vapor, but this statement is an extreme simplification.

Lines 402-403: Your record captures seasonal variability?

Lines 469-471: Citation?

Line 480: Why global moisture and not local moisture?

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Lines 484-487: This does not make sense.

Lines 491-492: Are you talking about regional or global albedo?

Lines 492-495: Citation?

Lines 497-498: Could this discrepancy relate to differences between short term variability and the mean climate state?

Line 502: "We noticed"? You mean the authors of these other publications noticed?

Lines 502-506: How close are these events in time?

Figures:

Figure 1a: The winds do not look correct. Also, 850 hPa winds do not exist over the Plateau. . .

Figure 2 and Figure 3 are not cited in the text.

Figure 3: It is difficult to see how the axes align with the lines

Figure 5d: Do the black lines represent significance?

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