

Interactive comment on “A 2600-yr-long paleoseismic record for the Himalayan Main Frontal Thrust (Western Bhutan)” by Romain Le Roux-Mallouf et al.

Anonymous Referee #2

Received and published: 23 June 2020

General comments

I enjoyed reading this manuscript that bring new constraints on the timing of paleo-earthquakes and the return period of surface rupturing events along the Main Frontal Thrust in Piping, western Bhoutan. Indeed, this study documents an exceptional natural rivercut of the frontal thrust at the junction between two river catchments. The active fault affects the alluvial cone of the tributary of the main river while a flight of alluvial terraces were abandoned and preserved along the main stream. The work done at the front of this outcrop is spectacular and well documented. The text reads well and is informative. Most of the conclusions appear supported by the observations

Printer-friendly version

Discussion paper



and are documented in the text. However, some observations appear to me either less convincing given their present documentation or they should be associated with less weight in the conclusion. I am personally not confident with the conclusion regarding the number of earthquakes. I further find that the estimate of the cumulated slip on the fault is not associated with reasonable uncertainties. I finally regret the absence of confrontation between the paleoearthquake ruptures and the alluvial terrace abandonment in the hangingwall of the thrust. My conclusion is therefore that the article's need moderate revisions including additional arguments or that the conclusions need some slight down tuning.

Specific comments: %%%%

I have not been convinced by the documentation of event E1 which affects the same units as E2. I cannot make the difference between one and the other and recommend either to find complementary observations or argument for the existence of two earthquakes at this site. Without any additional arguments on the outcrop I would personally go for documenting a scenario with one earthquake E1 (unless finding two generations of abandoned terraces with ages consistent with the two earthquakes), further mentioning that two earthquakes were described at sites further east within the period post 940AD, suggesting there is possibly more than one. I had a really hard time in understanding how the retro-deformation constrained so precisely the cumulated slip on the fault (~ 40.2 m over 1629 ± 255 yr line 418 !) given the significant uncertainty on the dip of the faults associated to the fact that most of the observations control mainly the vertical offsets which needs to be translated in slip on the fault. An estimate of the uncertainties associated to the estimate of the total amount of slip could be more realist. I regret that the manuscript does not integrate at least a paragraph on the relations between paleo-earthquakes and alluvial terraces that might have been abandoned after tectonic events. Indeed, some of the events described in the manuscript accommodate enough to create more than 10 meters of vertical offsets and might have been followed by episodes of severe incision. I do not understand the reason why these terraces,

[Printer-friendly version](#)[Discussion paper](#)

were not dated in order to facilitate both the determination of the incremental incision. I suggest that section 4 incorporates a discussion on eventual relations between events and terrace abandonment.

Additional remarks on text and figures:

Line 22-25 : Far field convergence is estimated at 17.0 ± 0.5 mm/yr (Marechal et al., 2016). The average slip rate @ “ 25.3 ± 4 mm/yr” is therefore significantly larger than the geodetic and geomorphological results. Line 82-83 “This last event contributes to the debate about the possible deficit of seismic moment”. I do not understand, please, rephrase. Line 107: replace at low relative elevation by “ at low elevation above the present day river course”. Line 223 : Wedge W1 is described as affected by intense internal deformation. Is it true ? If true, it should be documented on Figure 8 Line 390: Need to downtune the paragraph on E1, provided that there are no additional informations than those described here. Indeed, this earthquake is not documented properly at this site and the constraints appear elusive to me.

Figure 1: Damak trench is not on the map (Wesnousky et al., 2016) nor Charnath trench (Rizza et al., 2019). Bagmati, Sir Bardibas, Kayarmara and Mahara are not at the right place.

Figure 2: I suggest reporting a few altitudes along the Wang Chu course (as well as in terrace T2 North and T5 so that the reader can estimate the elevation of the terraces above the river without going back to the text. I am surprised to see that the supposed trace of the Main Boundary Thrust and Main Frontal Thrust are straight through the river, without drawing a small V in the valley toward the North, a likely feature despite the relatively steep fault dips. I recommend to check properly the shape of the trace of the fault provided it dips with the value mentioned in the text.

Figure 8: The wedge W1 is given in the text as intensely deformed (“ . . . exhibit little stratigraphy, intense internal deformation”) Line 223. Would it be possible to see that on this figure ? or with a zoom on W1 in supplementary data material ?

Interactive
comment

Printer-friendly version

Discussion paper



Figure 9: Scale is needed. The amount shortening or coseismic slip should be reported on this figure at every step, with their uncertainty.

Figure 10: Add unit W2

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-59>, 2020.

SED

Interactive
comment

Printer-friendly version

Discussion paper

