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

Black text: reviewer comment
Green text: Author answer

Anonymous Referee #2

I enjoyed reading this manuscript that bring new constraints on the timing of paleoearthquakes 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 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.
We agree.
Action: see next comment.

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.
We agree. We thank the Reviewer for this helpful suggestion and modify the text (sections 3.3 and 4) accordingly.

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.

We agree. Although we do not mention “40.2 m” but “~40.2 m”, we agree a more detailed assessment of uncertainties should be provided.
Action: we added details on uncertainties on slip measurements, dip angles and inferred slip values.

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.
We disagree. The distribution of terrace heights (~1 m, ~11 m, ~33 m, ~43 m, ~80 m, ~90 m, ~100 m and ~170 m) does not suggest clear systematic abandonment associated with repeated co-seismic uplift. Including them in our analysis without evidence for faulting and adequate determination of faulting ages would be model-driven.

I do not understand the reason why these terraces, 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.
We disagree. Most terraces at this site were dated and we have accumulated a significant dataset on incision rates. However, presenting said dataset is a whole different study that will be written up and submitted at a later stage.

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.
After implementation of a detailed analysis of uncertainties, our final slip rate is 24.9 ± 10.4 mm/yr, which largely encompasses geodetic and geomorphological results.
Action: none.

Line 82-83 “This last event contributes to the debate about the possible deficit of seismic moment . . ...”. I do not understand, please, rephrase.
We agree. Our wording was not clear enough.
Action: we rewrote that sentence.

Line 107: replace at low relative elevation by “ at low elevation above the present day river course”.
We agree.
Action: we implemented proposed modification.

Line 223 : Wedge W1 is described as affected by intense internal deformation. Is it true ? If true, it should be documented on Figure 8.
We agree. This is a mistake, only W2 exhibits intense internal deformation.
Action: we corrected the text accordingly.

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.
We agree.
Action: see previous comments that address this point in detail.

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.
We agree.
Action: we corrected Figure 1 following suggestions from R2.

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.
We agree.
Action: we modified Figure 2 following suggestions from R2.

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 mentionned in the text.
We disagree. The MBT is very steep here (as shown in Fig. 2b) and the section of MFT mapped does not affect strong reliefs; alluvial fans exhibit shallow slopes and the floor of the Wang Chu plain is basically flat. There is no geometric reason to invoque a V-shaped trace.

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 ?
We agree. As addressed in this Reviewer's comment on Line 223.
Action: see comment on Line 223.

Figure 9: Scale is needed. The amount shortening or coseismic slip should be reported on this figure at every step, with their uncertainty.
We disagree. Figure 9 is a schematic illustration of the retro-deformation process. Our analysis of co-seismic displacements is based on the direct analysis of the original log and inferences stated in the text. Thus, adding co-seismic values on Figure 9 would suggest said values were measured from the Figure and be misleading to the reader.

Figure 10: Add unit W2
We disagree. This is a chronostratigraphic model and there are no samples collected from W2 to be displayed here. W2 being a scarp-derived slump from unit U5, its age would be that of U5, anyway. The main text as well as Figure 9 do explain the place of W2 within the stratigraphic section and its relationship to event E4.

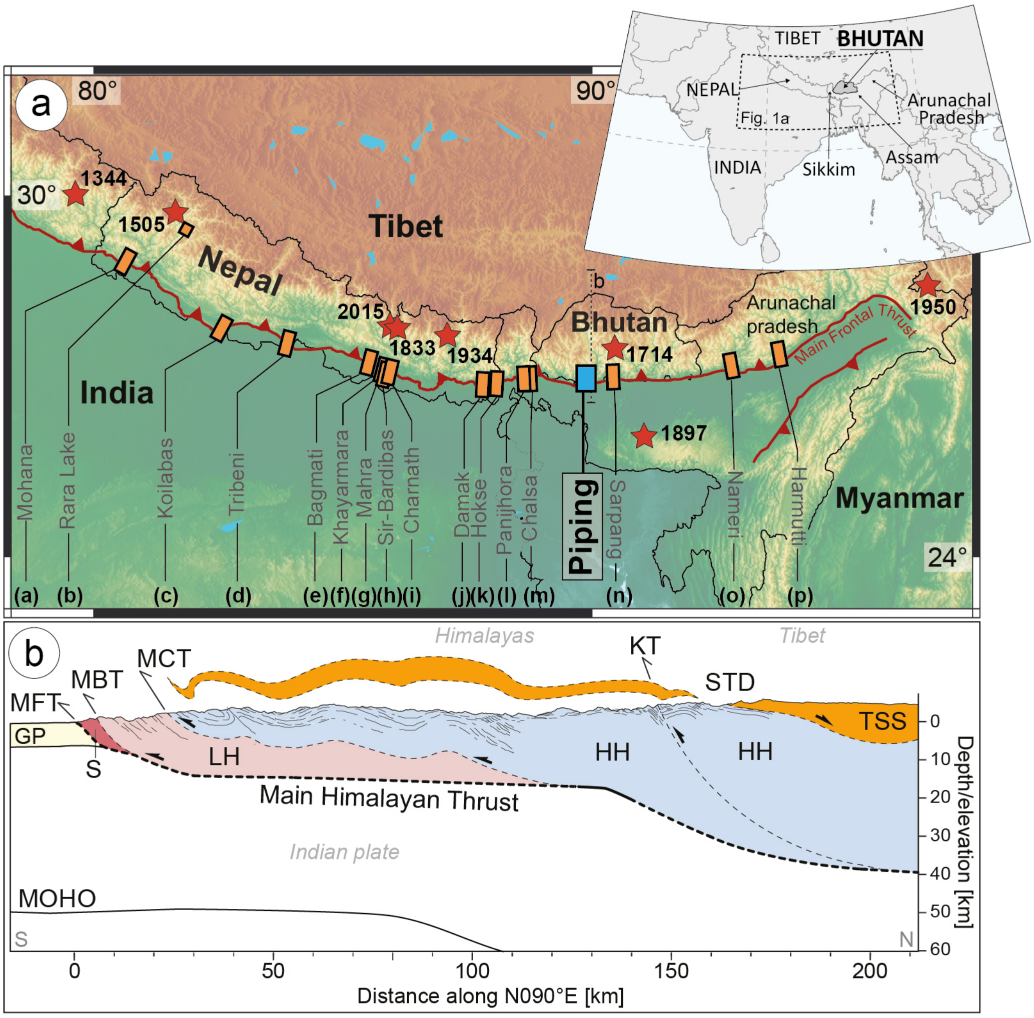
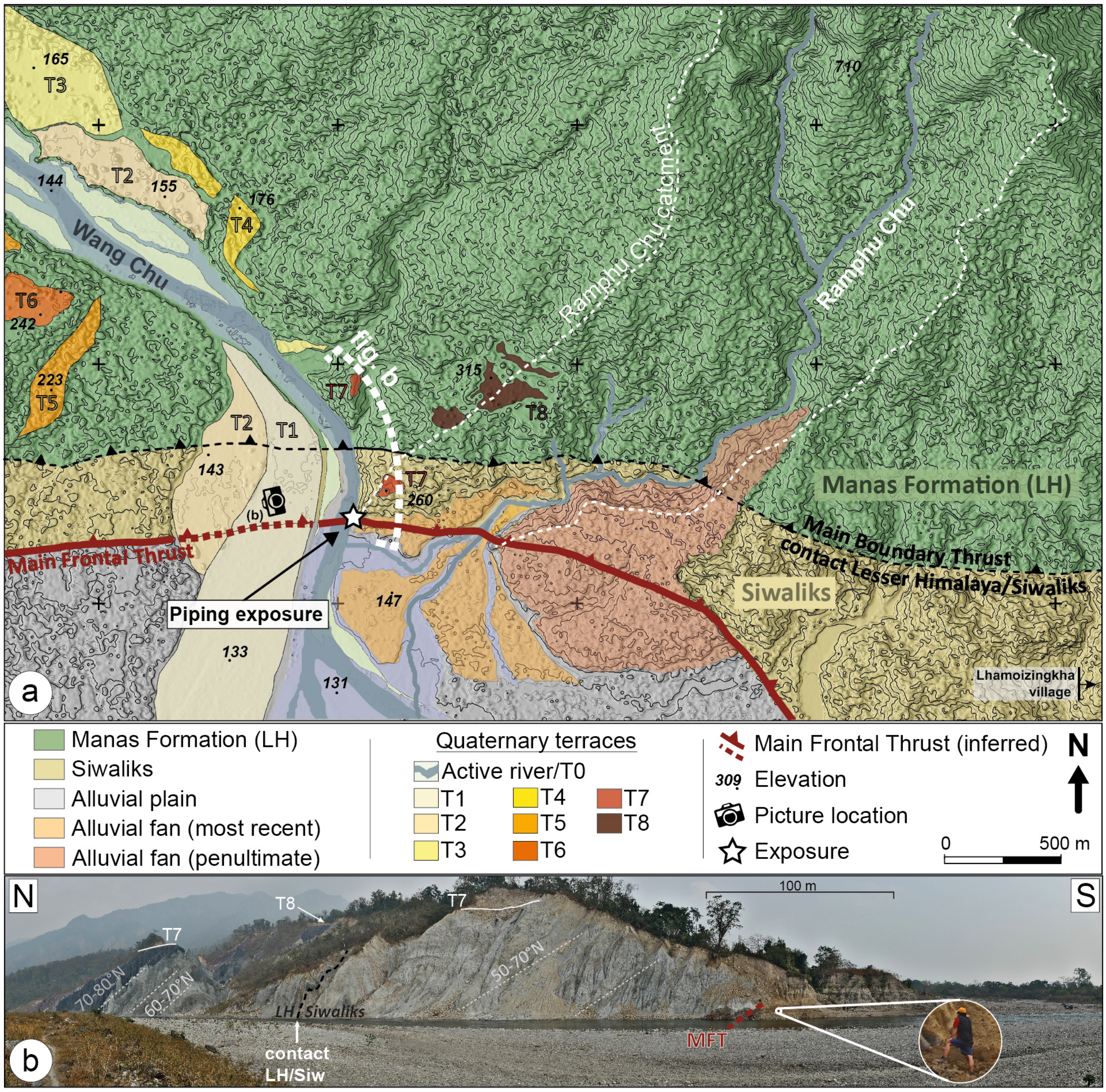

a
80°
90°
30°
24°
1344
1505
Nepal
Tibet
India
Bhutan
2015
1833
1934
1714
1897
1950
Arunachal pradesh
Main Frontal Thrust
Myanmar
b
Mohana
Rara Lake
Koilabas
Tribeni
Bagmati
Khayarmara
Mahra
Sir-Bardibas
Charnath
Damak
Hokse
Panijhora
Chalsa
Piping
Sarpang
Nameri
Harmutti
(a) (b) (c) (d) (e)(f)(g)(h)(i) (j)(k)(l) (m) (n) (o) (p)
TIBET
BHUTAN
NEPAL
Fig. 1a
Arunachal Pradesh
INDIA
Sikkim
Assam
b
Himalayas
Tibet
MCT
MBT
MFT
KT
STD
GP
S
LH
HH
HH
TSS
Main Himalayan Thrust
Indian plate
MOHO
S
N
0
10
20
30
40
50
60
Depth/elevation [km]
0
50
100
150
200
Distance along N090°E [km]

Figure 1



165
T3
T2
155
144
176
T4
Wang Chu
T6
242
223
T5
fig. b
T7
315
T8
Ramphu Chu catchment
710
Ramphu Chu
T2
T1
143
(b)
T7
260
Main Frontal Thrust
Piping exposure
147
133
131
Manas Formation (LH)
Main Boundary Thrust
contact Lesser Himalaya/Siwaliks
Siwaliks
Lhamoizingkha village
a
Manas Formation (LH)
Siwaliks
Alluvial plain
Alluvial fan (most recent)
Alluvial fan (penultimate)
Quaternary terraces
Active river/T0
T1
T2
T3
T4
T5
T6
T7
T8
Main Frontal Thrust (inferred)
309 Elevation
Picture location
Exposure
N
0
500 m
N
S
100 m
T8
T7
T7
70-80°N
60-70°N
50-70°N
LH
Siwaliks
contact LH/Siw
MFT
b

Figure 2