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 #1

The manuscript presents some interesting results on the timing and recurrence intervals of the past earthquakes from Bhutan, Central Himalaya. The main contribution and originality of the study lie in the detailed paleoseismic investigation of a river cut section along the Main Frontal Thrust (MFT) to reconstruct the past earthquake chronology and the average slip rate. The authors have suggested five events and a slip rate of 25.3±4 mm/yr along the Piping Site. The MFT is one of the major plate convergence thrust fault accommodating substantial slip, the estimation of earthquake size, time, and recurrence interval is valuable towards the seismic hazard assessment. Thus I, think the manuscript is indeed important and would draw interest from a wide range of earth scientists and fits the scope of Solid Earth. The manuscript is well written and organized. However, there are some inaccuracies with the interpretation which is discussed in the following section. I encourage the editor to request for a thoughtful revision to address these issues which will refine the manuscript before it is published.

Specific Comments:
Line 45: Comparing Strike-slip Fault and Thrust Fault systems does not seem probable. Kindly quote examples from thrust fault setting.
We disagree. We are not comparing kinematics, only the length of paleoseismic records with respect to the length of the structure for regional-scale faults capable of producing major to great earthquakes.

Line 102: Kindly mention the classification of terraces (youngest-oldest) for easy understanding.
We agree.
Action: added “(younger)” and “(older)” after height.

Line 103: The author keeps switching between Siwalik and Siwaliks. In general, Siwaliks are used when you want to include Upper and Lower Siwalik, else use Siwalik.
We agree.
Action: We checked and corrected throughout the text.

Line 105: Remove ‘s’ from ‘covers’.
We agree.
Action: we implemented proposed corrections.

Line 107: Instead of ‘low relative elevation’ you can use ‘relatively lower elevations’.
We agree.
Action: for clarity, we removed “relative” and added “above present stream”.

Line 259-261: Not clear. Please break it or re-write it.
We agree.
Action: we re-wrote the sentence for easier reading.

Line 264: Because of the low resolution of the image file and non-uniform cleaning of the wall it is difficult to judge the deformation caused by F1. I recommend the author to add a blow-up image of the fault structure from grid 40-45.
We agree.
Action: we added full-resolution orthomosaics as Supplementary Material.

Line 266: Along F6 and F5, no strong deformation is visible, instead U6 is rather horizontal with minor undulation which is quite common with fluvial deposits. Kindly check the validity of both the faults.
We disagree. F5 is not mentioned in that line, but F6 and F7. Localized displacements reach several 10s of cm, which is significant but may appear weak compared to the scale of the exposure. The full-resolution orthomosaic (previous comment) and added blow up in Fig. 7d should clarify this point.

Line 269: In Fig. 6 what does the white dashed lines represent? Kindly mark and classify the faults in the figure. Also extend and mark F2 in Fig. 7b.
We agree. White lines in Figure 6 are confusing.
Action: we modified Figure 6, better defined the various lines in the caption and extended F2 in Figure 7b and 7c.

Line 281: With the present resolution of Fig. 5 it is difficult to see F5. Kindly mark F5 in Fig. 7a.
We agree.
Action: we modified Figure 7a to better show F5.

Line 313: I don't understand how retro-deformation of U0 and U2 is carried out and compared with U1 because these two units are absent in the hanging-wall and the thickness may have varied. Thus, distinguishing E1 and E2 becomes difficult.
We agree.
Action: In agreement with suggestions from both Reviewers, we combined E1 and E2 into a single event with the possibility for a supplementary one.

Line 325: Kindly clarify if 1.5 m is vertical offset or coseismic slip.
We agree.
Action: we replaced "co-seismic slip of ~1.5 m" with "vertical component of co-seismic slip of ~1.5 m".

Line 335-336: How the slip along F4 is calculated using the dip of F3? The F3 has not deformed or cross-cut U3 and U4 which means they were deposited after the event on F3. The inference that the slip propagated along F3 seems improbable instead it looks it occurred along F4 and F5.

We agree. This is a typo; at line 336 “slip for E3 along F4” should read “slip for E3 along F3”. Action: we corrected the typo.

Line 347: It is not fully clear why U5 is recording 13.5 m of dip-slip whereas U6 is recording only 9 m of dip-slip. In fact, it should be the other way around as U6 is an older unit and should record more slip if there were two events.
We disagree. These values are estimates of co-seismic slip, not cumulative.

Line 352: The author gives a detailed description of retro-deformation i.e., extracting elevation from the highest observable point of each unit to their corresponding point in the undeformed section in the footwall. However, my major concern lies in how the retro-deformation of U5 is carried out to evaluate two events. Either by restoring dip slip of U5 along F2 or by restoring its highest point which is around the grid (38,12). With the current explanation and no visible cross-cutting structure in the section, it is difficult to distinguish E4 & E5.
We disagree. Grid point (38,12) corresponds to the highest point of U5 and has recorded cumulative deformation, whereas our retro-deformation consists in sliding along F2 to determine the co-seismic slip associated with event E4. Furthermore, the characterization of E5 is solely based on the geometry of U6; at this point U5 does not exist yet.

Line: 414: It seems unlikely for an earthquake reaching magnitude 8.5 to have a slip of only 1.5 m. There is some evidence of earthquakes from Himalaya recording lesser slip for example Mw 7.8 1905 Kangra earthquake but there was slip partitioning reported between thrust and strike-slip faults (Szeliga and Bilham, 2017).
We disagree. Hétényi et al. (2016) conclude the AD 1714 earthquake may have reached Mw 7.5-8.5, not 8.5 and we accurately quoted their results. We do not propose a magnitude ourselves. Furthermore, there are very few observed ruptures to compare to (slip partitioning certainly rules out the Kangra earthquake as a proper comparison as pointed out by the Reviewer).

Figure 6

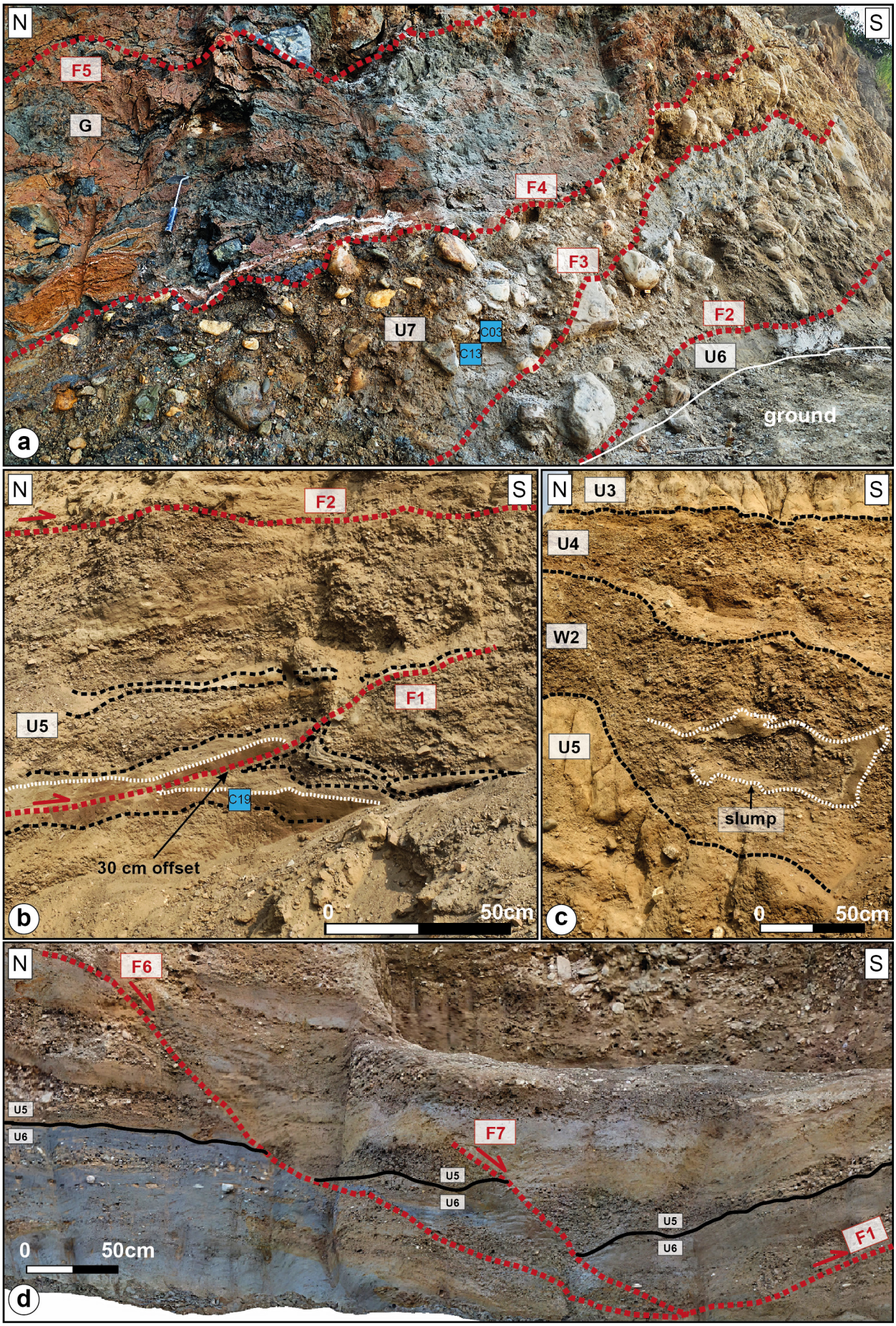
N
S
F5
G
F4
F3
F2
U7
C03
C13
U6
ground
a
N
S
F2
F1
U5
C19
30 cm offset
0
50cm
b
N
U3
S
U4
W2
U5
slump
0
50cm
c
N
F6
S
U5
U6
F7
U5
U6
U5
U6
F1
0
50cm
d

Figure 7