Dear Prof. Jeannot Trampert:

We thank you and the reviewers for the insightful comments for our manuscript GJI-14-0858 entitled "Remotely triggered earthquakes in South-Central Tibet following the 2004 *Mw* 9.1 Sumatra and 2005 *Mw* 8.6 Nias earthquakes". We are hereby submitting a revised version that accounts for the comments. Below are the detailed responses to all the points. We marked the original comments as **bold**, our reply as regular font, and new text inserted/revised in the paper as *italic*.

We thank you and the reviewers again and hope that the revised version will be accepted for publication.

Best regards,

Dongdong Yao, Zhigang Peng and Xiaofeng Meng

Reviewer: 1
Comments to the Author(s)

1. I suggest changing page 1, lines 44-48, which note that "In most cases, triggered events occur during or immediately following the teleseismic surface waves". One should question the extent to which this perception is a result of observational bias. We identify a causal connection more easily when two events are temporally close together; i.e. if a change in earthquake rate occurs long after some perturbing deformation we are less likely to say they are related. In addition, one also wonders how many studies simply look only within the interval of the teleseismic wavetrain or immediately thereafter? If a significant number of studies, one cannot draw the conclusion noted.

We thank the reviewer's comments. We agree that it is easier to establish a causal connection for two events that are temporally close, but we still cannot explain the mechanisms of delayed triggering. We changed the sentence to "*Triggered events mostly occur during or immediately following the teleseismic surface waves*".

2. How did the authors determine that "While many new local events were detected, 95% are aftershocks of the 2005 Zhongba mainshock"? (pg. 4, lines 30-31).

In total, we detected 6453 events during the study period (03/21/2005-04/14/2005) in the Zhongba region. Among them, 6278 occurred after the 2005 Zhongba event. Because of the close spatio-temporal relationship, we consider them as aftershocks of the 2005 Zhongba event. The two numbers result in the 95% ratio. We changed the sentence to "*Although we detected 6453 events around 2005 Nias mainshock in Zhongba, 6278 of them (95%) occurred shortly after and around the epicenter of the 2005 Zhongba event, which are considered as its aftershocks.*".

3. It would be very interesting to learn whether any of the M>6 earthquakes in Zhongba earthquakes triggered activity in the Gaize region, and how the characteristics of the wavefields from those earthquakes as observed in the Gaize region compared with those of the teleseisms examined.

We agree with the reviewer that it will be a very interesting comparison. Unfortunately, the 2004 and 2008 Zhongba earthquakes are not in the study period, as our initial motivation is to investigate the relationship between the 2005 Nias and the 2005 Zhongba earthquakes. Hence, we can only investigate whether the 2005 Zhongba event triggered earthquakes in the Gaize region. As shown in Figure 5c, the 2005 Zhongba event (2005/04/07, 20:04:41.06) occurred ~244 hours after the 2005 Nias mainshock. No clear seismicity rate changes can be observed. We added the following sentences in the main text "*The M~6 Zhongba sequences are ~300 km away from the Gaize region. Unfortunately, neither the 2004 nor the 2008 event was recorded by the Hi-CLIMB network. The predicted dynamic stress from the 2005 event is ~75 kPa. However, we did not observe clear seismicity rate change near Gaize around the 2005 Zhongba event (Figure 5c). One possible reason could be due to the 'dynamic shadow effect' after the 2005 Nias mainshock. The critically stressed patches near Gaize ruptured when the surface waves of the 2005 Nias earthquake passed by and were not ready when the 2005 Zhongba event occurred. Alternatively, triggering in this region could be frequency dependent (e.g., Brodsky & Prejean 2005), such that only long-period surface waves from very large and distant earthquakes are capable of triggering seismicity.*"

4. Maybe this has been addressed and I've missed it, but to what extent might the apparent difference in triggering in the Zhonga and Gaize regions be due to the difference in detection thresholds? The thresholds for Zhonga are .6 and 1.0, and almost an order of magnitude lower for Gaize, -.8 and -.6. If only events with M>.6 and 1.0 were used in Gaize, how much would the results change?

We agree that the different detection thresholds may cause biased results. To further confirm our observations, we followed the reviewer's suggestion and used the same *Mc* value in Gaize as in Zhongba. The seismicity rate changes in Gaize with different *Mc* values are shown in Figure S7. It is evident that the rate changes patterns are consistent despite the *Mc* value used. We added the following sentences in the main text "*To avoid potential bias from the much lower M_c near Gaize, we also used the same M_c as in Zhongba, and the rate changes remain essentially unchanged (Figure S6).*".

5. Perhaps a test of the reason offered to explain why a dynamic stress shadow would exist (lines 43-45, page 5) would be to look at how many events triggered by the Sumatra earthquake were potentially retriggered by the Nias earthquake. If one assumed that it took longer than 3 months to reload a fault (the time between the Sumatra and Nias earthquakes) and the proposed explanation was true, one would expect that all the faults triggered by the Nias earthquake would be different than those triggered by the Sumatra earthquake. One way to assess this would be to measure which templates triggered which earthquakes, i.e. did templates chosen from post-Sumatra seismicity match only those detected in the pre-Nias interval and templates from the post-Nias period only match post-Nias triggered events? If an event from the post-Nias period matched a template from the post-Sumatra period, indicating it could be a re-rupture of the same fault, this would make questionable the assertion that the post-Sumatra quiescence reflected a using up of finite population of available ripe faults (unless again, they could reload within 3 months, which seems unlikely).

We followed the reviewer's suggestion and performed a simple test. We first compared the locations of post-Sumatra (i.e. 0-100 hours after the Sumatra event) and post-Nias (i.e. 0-10 hours after the Nias event) templates, which are shown in Figure S8. There is no overlapping among them, which suggests that they occur on different fault segments. We also checked 'which templates triggered [matched] which earthquakes', which is shown in Figure S9. The detected events in post-Sumatra are exclusively matched by templates before the Nias event, while the detected events in post-Nias are matched by templates around the Nias event. Hence, there is no overlapping among detected events in post-Sumatra and post-Nias. We added the following sentences in the main text "*One would argue that the seismic quiescence could be artificial due to the fact that NWMFT can only detect events around those templates. To evaluate this further, we examined the matching pattern to evaluate whether re-rupturing of the same fault patch occurred in the three months after the Sumatra mainshock. As shown in Figure S8, the events triggered by the Sumatra (0-100 hours) and Nias events (0-10 hours) do not overlap and hence may rupture different fault segments. This is consistent with our inference that the fault segments that ruptured immediately after the Sumatra event were not ready to be triggered/ruptured again when the Nias event occurred. In addition, the detected events immediately after the Sumatra event are exclusively matched by templates before the Nias event, while the detected events after the Nias event are matched by templates around it (Figure S9). These observations further corroborate our interpretation of dynamic stress shadow.*".

6. It would also seem that the results don't support the inference sometimes made that the potential to trigger correlates with the background rate, since the Zhongba ambient rate seems higher than that in the Gaize region?

We explained the differences in triggering potential between Zhongba and Gaize by considering different aspects, i.e., geothermal activity, background seismicity rate, seismogenic depth in p. 6, line 38-57 and p.7, line 6-13 of main text. The heat flow map shows Lhasa Terrane has a higher value compared to Qiangtang Terrane (Tao & Shen 2008), which does not support the observations that geothermal/volcanic regions with high heat flows favor triggering. The background rate could contribute to different triggering potential since it would help to estimate whether there are critical faults. We speculated that the 2004 Zhongba earthquake released the accumulated strain and made Zhongba away from the critical state when the 2004 Sumatra event occurred. However, this still could not explain that no triggered earthquakes following the Nias event, but the 2005 Zhongba event occurred ~244 hours later.

We added the following sentences to describe the fact that our observations are inconsistent with the high background rate triggering has higher potential of triggering: "*We note that the Zhongba region has higher background seismicity rate than the Gaize region (i.e., larger a value in Figure S3), but most triggered activity was found in the Gaize region. This observation is also inconsistent with recent* *observations that regions with higher background rate (i.e., geothermal and/or aftershock regions of recent/historic large events) are more susceptible to dynamic triggering (Hough et al. 2003; Jiang et al. 2010; Aiken & Peng 2014).*"

7. The map of Figure 1b is so small it is hard to see the stations. Perhaps it could be enlarged by showing only the region from 75 to 105 degrees E and 0 to 35 degrees N?

Done. We updated the Figure 1b.

Reviewer: 2
Comments to the Author(s)

1. The authors do not state whether they employ their matched filter technique over a network of seismometers (Network Matched Filter technique) or over a single station or channel. From Figure 3, it appears they used a Network Matched filter, but this should be explicitly stated in the text. I make this point because single channel filters are still widely utilized, despite the fact that network filters far outperform single channel filters.

Done. We employed our matched filter technique over a network of seismometers and I changed them to Network Waveform Matched Filter Technique (NWMFT) in the main text.

2. page 3; lines 41-48: You utilized windows of 12 seconds and 5 seconds for the template events, and offset the p-wave window (Z components) and s-wave windows (N,E components). First, this is a very small time window in my opinion. For example, Skoumal et al. (2014, EPSL) performed an analysis on template length for earthquakes in Ohio and found that a template length of 37 seconds produced the smallest number of false detections (though obviously Ohio and interior Asia have vastly different network geometries). Your figure 3 illustrates why longer templates may be beneficial. There is a significant amount of waveform outside of your template window which is obviously related to the earthquake, for example the shear waves arriving on the vertical channel. By not including this signal, the filter is not utilizing all available signal to correlate with. Including extra time, even if there is no signal being recorded, does not

significantly affect the statistics (e.g., MAD) of the resulting filter. For example, including very small amplitude variations on the horizontal components before the shear wave wont affect the filter because zero's correlated with continuous data will not add or subtract to the resulting correlation value of the filter.

Secondly, I don't see a justification for offsetting the vertical and horizontal components. I think this is left over from analysis of low frequency earthquakes in Japan (which brought this technique into the mainstream), where (a) many events are arriving at closer to vertical incidence due to their great station coverage, and (b) where the shear waves are often the only visible phases due to the small nature of the events. Why not just include all of the available signal? The resultant signal to noise ratio should increase.

While I would like to see this reprocessed with longer template times, I do not feel it is strictly necessary to the quality of the results. I fully believe the analysis and interpretation is satisfactory in current form, but the authors might realize slight gains in catalog robustness by increasing the template durations. I do not think that re-processing with longer template times will change any of their key observations or interpretations, and so it might not be worth the computational time.

We agree that longer time window have some benefits, including suppressing background noise and enhance the detection quality. Indeed, we miss some signals from Figure 3, including P wave signals on horizontal channels and S wave on vertical channel. We offset the vertical and horizontal components mainly because P and S waves should have more clear arrivals on vertical and horizontal channels, respectively. We also want to enforce the P and S wave travel time difference to better detect relative further events. Finally, a longer time window will require longer computing time, as mentioned by the reviewer.

Following the reviewer's suggestion, we conducted a test by using different time windows. Specifically, we selected 10 templates in Gaize region and cross correlated with continuous waveform from 2005/03/26 to 2005/03/30. Two different time windows are: 5s (1s before and 4s after P and S waves for vertical and horizontal channels, respectively); 20s (2s before and 18s after P waves for all channels). Figure S2 shows the detection results for different time windows. Clearly, the 20s window has an overall lower CCC while the detection results are similar. From this test, we concluded that the length of detecting time window does not affect our results in this study. We added the following sentence in the main text "*Longer window with both P and S wave information helps suppress the background noise and enhance detection* *quality, but significantly increase computational cost. We conducted a simple test by utilizing windows with different lengths to test how the window length affects the detection result. As shown in Figure S2, different detecting window length results in slightly different detection results, but the overall temporal evolution of seismicity rate remains essentially the same.*”.

3. p 4;l 1-10: How did you combine the resulting catalogs? For example, if multiple of your template events are located very closely to each other and thus have similar waveforms, a new match could be detected from each of these templates. What steps did you take to ensure that your new total catalogs do not have multiple listings for the same earthquake?

The way we remove duplicate detections is that we do not allow overlapping between any two detections following that of Meng et al. [2013]. Specifically, we first found the matched time window for each detection, which is from the earliest start time of the detecting time windows to the latest end time of the detecting time windows among all channels. We then examined if the matched time windows between two consecutive detections overlapped. If overlapping occurs, the detection with higher CCC was kept. This strong constraint may remove a few real and overlapping events, but ensure no duplicate detections.

4. p4; l 26-29: I think it is important to note that your calculated magnitude of completeness (Mc) is not for the whole region, but only for the regions in the immediate vicinity of your template events. This distinction is also important when interpreting your b-value plots, for example in Figure S3. For example, the top-left, bottom-left, and bottom-right panels in Figure 3 shows a cumulative number of earthquakes that appear to have two shoulders in the distribution (another way to look at it is that your b-value fit line under-estimates the cumulative seismicity for about 1 magnitude level above Mc). Look at the bottom right panel in S3, for example. The first shoulder in cumulative seismicity looks like it's at about M2, with a linear region above this magnitude. This is probably the Mc of your initial hand-picked catalog. Then there is a region from M2 to M0 that is also linear, but at a shallower slope. I do not think this is real, but rather has to do with the fact that you're missing events that are far away from any template. In fact, you could calculate the expected number of “missed” events by taking the difference between the higher slope's intercept and the shallower slope's intercept. This curved cumulative seismicity plot could also be skewing

your calculated Mc, as this calculation is based on when the distribution statistically deviates from linearity.

We thank the reviewer for pointing this out. We agree that the NWMFT strongly relies on the distribution of templates, and events far away from templates may be still missing, which makes the calculated *Mc* is only for the regions in vicinity of all template events. This problem is similar with first reviewer's questions #4. In our revised paper, we compared the results with different *Mc* values in Gaize and obtain a very similar pattern of seismicity rate changes. We added the following sentence in the main text "*To avoid potential bias from the much lower M_c near Gaize, we also used the same M_c as in Zhongba, and the rate changes remain essentially unchanged (Figure S6).*".

Minor Points

1. p 1; l 3-4 and p 2; l 28: I prefer to use Network Matched Filter to distinguish this technique from the many single channel techniques currently utilized.

Done. We changed to NWMFT.

2. p 1; l 40: This first sentence is awkward

We changed "*Earthquakes interact with each other within a wide range of space-time windows*" to "*Earthquakes can interact with each other in a wide range of spatial-temporal windows*".

3. p 1; l 47: change "critical" to "critically stressed"

Done.

4. Figure 1 should have a scale bar

Done. We added a distance scale bar in Figure 1c.

Additional changes made by ourselves:

1. We used an updated version of the code (The so-called best-combined method in ZMAP) to compute the magnitude of completeness (*Mc*). The resulting *Mc* value in each region is slightly different, but the overall conclusion remains essentially unchanged.