Solid Earth Discussions

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Reply on CC1

Cedric Twardzik et al.

Author comment on "Very early identification of a bimodal frictional behavior during the post-seismic phase of the 2015 M_w 8.3 Illapel, Chile, earthquake" by Cedric Twardzik et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-6-AC1, 2021

Dear Sylvain Barbot,

We are very grateful for the community comments that you have made regarding our manuscript se-2021-6 submitted to EGU Solid Earth. You will find below our responses to the various points that you have raised.

- Abstract lines 1-3: a lot of "slip" in just a few sentences.

We have rewritten the beginning of the abstract so that we avoid such repetitions of the word slip.

- Line 34: unclear what "potentially reducing the propagation of errors" means.

What we meant to say was that the approach of forecasting aftershocks using Coulomb stress changes relies on the modeling of the slip distribution as well as the modeling of the Coulomb stress changes. Thus, for the modeling part alone, there are potentially two sources of errors. On the contrary, forecasting aftershocks solely based on afterslip implies only one source of error for the modeling part. We have rephrased that sentence to make that point clearer (line 33-35 of the new manuscript).

- Line 46-48: detection of early aftershocks after the Gorkha earthquake was discussed in the study

Thank you for drawing our attention to that study. We have included it in the article to answer that comment, which has also been raised by reviewer Bernd Schurr (line 51-53 of the new manuscript).

- Line 64: 15 GNSS stations within 350 km does not sound like much. Discussion of resolution and sensitivity is in order. & Lines 104-113: Not sure why a Monte Carlo sampling method is used here as the problem is entirely linear and can be solved by least squares with Laplacian regularization. It would be useful to document the resolution of the inverse problem or to characterize it with a checkerboard test.

We have added a resolution analysis to the Supplementary Material S5. More specifically, we show a map of the resolution in Figure S5.1 (i.e., diagonal elements of the resolution matrix -- Tarantola and Valette 1982). We also show on Figure S5.1 a map of the restitution for the two major patches discussed in the main text to show the potential smearing effect when slip is imaged in these areas.

- Line 137: I can't recall an example of the opposite. Do we have examples of afterslip distributions that are firmly not time/space separable?

After doing some literature review, we have to agree with the reviewer that it is difficult to find examples showing afterslip migration. So far, we could only find one study suggesting hints for afterslip migration : Jiang et al. (2021). Even in that study, the migration is not so much on the causative fault plane, but more migration of afterslip to adjacent faults. Therefore, it is true that the most common observation is that afterslip is stable over time. We have modified the text to reflect that (line 152-155 of the new manuscript).

- Line 143-144: Note the work of Salman, R., Hill, E.M., Feng, L., Lindsey, E.O., Mele Veedu, D., Barbot, S., Banerjee, P., Hermawan, I. and Natawidjaja, D.H., 2017. Piecemeal rupture of the Mentawai Patch, Sumatra: the 2008 mw 7.2 North Pagai earthquake sequence. Journal of Geophysical Research: Solid Earth, 122(11), pp.9404-9419.

We have added that reference in the main text as a potential mechanism for explaining an overlap between co-seismic slip and afterslip (line 162-166 of the new manuscript). However, we want to emphasize that, as stated in the text, our conclusion is that the resolution of our afterslip model along with the variability of coseismic models does not provide enough evidences to conclude with certainty if this is the case or not.

- Lines 156-158: Is it possible that this deep slip patch may in fact represent strain on crustal faults above the megathrust ?

This is an interesting point that is raised here. However, we do not think that this is the case. We have looked at the contribution of this patch alone on the surface displacements (Supplementary Materials S5). When looking at that, we find that this patch do not generate a significant signal at the surface, i.e., displacements with amplitudes larger than the noise level of the time series. In addition, we also provide in Supplementary Material S5 two qualitative arguments that in our opinion reflect the fact that this patch is unreliable : (1) its spatio-temporal evolution is not very stable compared to the main areas of afterslip (see Figure 3 in the main text) and (2) it almost completely disappears when we use the position time series corrected from the two large aftershocks (see Figure 7 in the main text). This is why we have reached the conclusion that this patch is rather uncertain and more likely an artefact from the inversion.

- Line 181: Wasn't the geodetic moment 8E19 Nm after the deep patch is removed? So the seismic moment is actually greater than the geodetic moment? Also, shouldn't the comparison be with the geodetic moment at the time of the Mw 7.1 earthquake instead of at the end of the 12 hours?

ndeed, the estimated geodetic moment over the whole fault is 8.3E19 Nm while the estimated seismic moment over the whole fault is 9.5E19 Nm. We have changed the text to provide a better explanation for that discrepancy (line 203-218 of the new manuscript). In particular, we find that most of the seismic moment is released in the southern patch (~9.0E19 Nm). In that same region the geodetic moment is even lower (~4.5E19 Nm). We provide several explanation for that difference. First, seismic moment of the earthquakes in the GCMT catalog are obtained using PREM for the Earth structure, a model that differs from ours and that can over-estimate the rigidity especially at shallow depths (Beck and Lay , 1999). Then, the Kalman filter used to process the GNSS has been tuned to properly recover slow processes such as afterslip. Therefore, it might not be suited to recover static offsets from large earthquakes, which can distort the recovery of the real ground motion in that case (Choi, 2007). Finally, as pointed out by Konca et al.

(2007), there is also a moment-dip trade-off when using near-field geodetic data. Thus, the fact that our fault plane only approximates the real geometry of the Slab1.0 model could also explain the discrepancy.

- Figure 2: It would be useful to show the "time since mainshock" as a second x-axis. Please also indicate the moment magnitude of the two large aftershocks next to their dashed blue lines.

We do not think that it is relevant to show a second x-axis with « time since the mainshock » as the orange dots are by construction n-hours after the mainshock. However, we have added the moment magnitude of the two large aftershocks next to the dashed lines on the plots.

- Figures 3 & 5: the repetitive degrees around every subplots are redundant. Consider showing only the left and bottom ones. Consider better showing the trench with the usual chevrons. Indicate the meaning of the blue area in the legend. Add the moment magnitude of the aftershock next to the respective star.

Done

- Figure 4: Remove the title "postseismic 12 hours" as it shows afterslip distribution for longer periods.

Done

- Figure 7: This should be replaced by a composite with Figures S7.1 and S7.2. The corresponding discussion of the number of aftershocks and the logarithm of the same in the main text is not particularly useful. Instead, focus on the obvious difference between Figures S7.1 and S7.2.

We have taken that into account especially since this was also pointed out by all of the other reviewers. Figure 6 now shows the evolution of the geodetic moment of afterslip along with the seismic moment released by aftershocks. The curves are on the same sub-figures by are displayed using distincts y-axis. That way, they look like normalized so that we can compare the temporal evolution, but the distinct y-scales allow the reader to get the actual value of the moment. We have kept a comparison between the geodetic moment of afterslip and the time evolution of the number of aftershocks (Figure 8) as we still discuss that relationship in the main text.

- Finally, please consider commenting the phenomenology shown in Figure S7.1. Why is the cumulative moment of aftershocks increasing so much in the northern segment around 6-7 hours? How does that translate in terms of fault slip? It does not seem clear from the various figures. It is hard to tell if the moment is significant because the plots use "normalized" time dependence. Since the geodetic and afterslip moment are so similar to the south, why not using moment (Nm) as the y-axis?

We think that this comment comes from the fact that the curves were normalized giving the false impression of a very large increase of seismic moment. We believe that with Figure 6 in the new manuscript, the fact that this increase is in fact very small should be more obvious.