Solid Earth Discussions

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# Reply on CC2

Cedric Twardzik et al.

Author comment on "Very early identification of a bimodal frictional behavior during thepost-seismic phase of the 2015 Mw8.3 Illapel, Chile, earthquake" by Cedric Twardzik et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-6-AC4, 2021

Dear Dietrich Lange,

First of all, we would like to thank you for the community comments that you have made on our study that we have submitted to EGU Solid Earth (manuscript se-2021-6). Below are our responses to the comments that you have made. We have attached as a .zip file an annotated pdf of the manuscript so that changes can be tracked as well as an updated version of the Supplementary Materials.

- In the manuscript, you sometimes refer to "south/north of the rupture area," but it seems that the two patches you classify as south and north of the rupture are still located in the co-seismic region (e.g., between 30-32°S). I suggest clarifying the two patches exact position and how they are related to the co-seismic slip.

We have highlighted the regions that we are referring to on Figure 3 so that the reader can clearly see the location of what we refer to as the southern patch and the northern patch. We have also added that added information in the text (line 143 and line 149 in the revised manuscript).

- In particular, previous authors observed a widening of the aftershock zone of the Illapel earthquake (e.g., Lange et al., 2016, GJI, doi: 10.1093/gji/ggw218). It would be interesting to state if this is observed with the catalog based on template matching and how this relates to the inverted afterslip. I assume that the possible expansion outside the co-seismic region is too small to be robustly inverted by the hourly evolving afterslip model. I suggest adding some words to this.

This is an interesting point that you raise regarding the migration of aftershocks. Huang et al. (2017) as well as Frank et al. (2017), from which the catalogs of aftershocks used in our studies are taken, seem to make a similar observation, even when looking at the very early stage. However, regarding the afterslip, we do not see such migration. Instead, as mentioned in the text, we only observe a growth of the afterslip patches (see line 142 in the revised manuscript). It is likely that our afterslip maps don't allow to investigate such fine details. Because we already mention in the text that we do not observe any afterslip migration, we have decided not to discuss that point in the main text.

- In particular, the authors find that the partitioning in seismic and aseismic slip changes in time (As mentioned in Line 207). I could not feasible see this in Figure 7 and S7.1, and suggest to simply plot the displacement versus the number of events for the patches to show the relationship between both processes. For example, Lange et al. (2014, GJI, doi: 10.1093/gji/ggu292) mapped for the late postseismic (e.g. >1d) of the Maule 2010 earthquake the partitioning of seismic to aseismic slip, which was relatively stable in time.

We would like to clarify that we do not argue that the seismic/aseismic slip partitioning changes over time. Line 207 in the former manuscript only relates to the mechanical link that has been proposed between afterslip and aftershocks. One usual argument for the fact that afterslip is mechanically driving aftershocks is based on the fact that the shape of the time evolution of afterslip closely match the shape of the time evolution of the cumulative number of aftershocks. But, this is not what we observe. Figure 7 (now Figure 8 in the revised manuscript) shows that : the afterslip (blue line) evolves clearly differently than the number of aftershocks (orange dashed line).

- There is a similar observation for strike-slip faulting for early observation of afterslip and aftershocks, and their relation (Savage, 2007, GRL, doi: 10.1029/2010GL042872) shows that aftershock seismicity rate is not proportional to the stress relaxation rate for the San-Andreas fault.

Thank you for pointing that study to our attention. At the time of writing the first version, we have attempted to find other cases exhibiting such behavior, without success. Thus, we have added that to the main text (line 268-270 in the revised manuscript).

- I suggest adding a caption to Fig. S7.1 and S7.2. Figure S7, S7.1, and S7.2 (right panels) might need additional labeling for the number of aftershocks. Currently, only the slip is labeled.

This is done on Figures 6, 8 and S7.2 in the revised manuscript and Supplementary Materials

- Table 1 and 2 do not contribute and might belong to the supplementary.

Done (see Supplementary Material S1)

- I suggest showing Figure S7.1 (temporal development southern patch) in the primary material since parts of the findings are difficult to understand without this figure.

Done (see Figure 6 in the revised manuscript).

- Line 9: …. and thus could contribute to a more data-drivenforecasts of long-term aftershocks. I cannot necessarily follow the argument in Line 9. Since the partitioning of seismic slip in some places changes in time prediction might be very difficult if this process remains enigmatic. Does the suggested forecast suggested here use Omori-laws and constant b-values, such as modelled by (Jonsdottir et al., 2006, Tectonophysics 424, https://doi.org/10.1016/j.tecto.2006.03.036.)?

We would like to clarify again that we do not argue that the seismic/aseismic slip partitioning changes over time. However, we do agree that this last sentence of the

abstract might be too ambitious of an opening with respect to our finding. We have rephrased that last sentence (line 9-11 of the new manuscript).

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2021-6/se-2021-6-AC4-supplement.zip