Solid Earth Discussions

Solid Earth Discuss., author comment AC2
https://doi.org/10.5194/se-2021-6-AC2, 2021

Reply on RC1

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-AC2, 2021

Dear Bernd Schurr,

We would like to thank you for reviewing our manuscript se-2021-6 submitted to EGU Solid Earth. Below are our responses to the different points 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.

- l.58 “seismic noise” – I think this is not really seismic noise but overwhelming signal, i.e. numerous often simultaneous aftershocks, that is causing problems to most detectors.

We have changed that part to better highlight the issues of detecting aftershocks right after a large mainshock (line 52-54 of the revised manuscript)

- l.28: change “activity” to “deformation”. I think there is a “itself” missing after “express”.

Done

- l.49: change “highly” to “more”

Done

- l.89ff: “The cumulative surface displacements are calculated at every hour since the mainshock origin time by computing the average positions over a 1-hour time window centered on the time of interest.” Does cumulative refer here for cumulative during the one hour processed or cumulative since the mainsock. I assume the earlier, but please clarify.

We agree that the terminology that we use here is rather confusing. A given position in time on the position time series represents how much surface displacement has occurred since the mainshock. This is what we meant by « cumulative ». Therefore, the average position that we compute and use during the inversions represents how much surface displacement has occurred after N hour(s). Consequently, each snapshot on the former Figure 3 and 6 represent the total amount of afterslip that has occur after N hour(s). We have attempted to clarify that point in the text (line 100-104 in the revised manuscript).

- l.126: "yellow circles" should be "purple stars"?

Done

- l.129: There is a word missing after "second". Maybe "patch".

Done

- The general fuzziness of both co-seismic and post-seismic slip models makes this assertion difficult to maintain (and the authors actually relativize it later in the paragraph). In particular, different fault model used (simple plain slab like the authors or varying slab dip based on e.g. slab2.0) in the modeling will shift location of slip. To start interpreting this, at least the modeling set-up of co- and post-seismic slip should be the same.

We do not think that it would be relevant to the study to add our own co-seismic slip model using our own dataset. There are already plenty of co-seismic models from other groups, that we show in Supplementary Materials S6, and which already illustrates the variability of the co-seismic slip area. To the first order, our model will very likely match the ones displayed and thus it will not change the discussion. Also, as we mention in the text, we are not making any assertion regarding the penetration of afterslip inside the co-seismic area by concluding that this is an observation for which we cannot reliably address the veracity (line 180 in the revised manuscript).

- First of all, slip in Fig. 3 and 6 I assume is the slip during the respective hour, not the cumulative slip added up also from the previous hours (must be based on the amplitudes and the fact that some patches vanish).

We think that our answer to comment l.89ff should clarify that point.

- I don't understand why, if only the offsets of the 2 largest aftershocks are corrected (hour 1 and hour 5), all slip vanishes in the southern patch also during the other hours. Please explain.

This relates to one of the point you have raised previously (l.89ff). The surface observations record the total amount of afterslip after n-hours. As we explain in the text, the observed afterslip to the south is for the most part due to the two largest aftershocks. So, by removing the signal from these earthquakes, there is consequently no slip in this

region anymore.

- Fig. S7.1: The second M6.8 aftershock occurs during hour 5 and clearly shows up as a step in the graphed moment. However a step in slip seems to occur mainly in hour 6? Is this an averaging effect?

This is indeed an averaging effect. The second aftershock occurs at the end of the time window used for averaging the position at hour 5. Therefore, within that window, the majority of the data used for the averaging are not affected by the offset from that aftershock. Instead, the windows for hour 6 and after are fully offset by the aftershock. This explains the shift between the seismic moment and the geodetic moment. Also, instead of showing the continuous evolution of seismic moment, we were showing the seismic moment summed over time windows of 1 hour, to match the time window covered by the afterslip. This accentuated the effect even more. Thus, we have changed that figure (see Figure 6 in the revised manuscript).

- l.197ff: Please elaborate in one or two sentences what this predicted accelation phase signifies.

We have added a sentence to clarify what is this acceleration phase (line 256-257 in the revised manuscript).

- l.201ff: Please mention where the cited studies were based (Ecuador and Japan).

Done

- l.209: Change "rate-and-state law" to "rate-and-state friction law" here and everywhere else.

Done

- l.257: I wonder, are aftershocks anywhere actually operationally forecasted based on some models (maybe a citation would be good)? If so, I would assume that models have to be simple and robust. Here e.g. CFS would naturally predict aftershocks around the co-seismic rupture area, where they do occur, for the Illapel eqk and also for many other subduction zone earthquakes. I wonder, how realistic and it is to actually do the hindsight analysis outlined here in near real time and if it really adds value. Of course, this could be tested.

It is indeed, very difficult to predict the added value of including information about very early afterslip for forecasting aftershocks, and the point that we want to make is that we have now the capability to investigate such question. However, it is true that we might have been too ambitious by having the word « operational ». In fact, we are not aware if this is done anywhere. We have rephrased that last sentence to reflect better our point (line 319-321 of the revised manuscript).

- l.271ff: "Our additional finding is that the slip patterns that we observe after 12 hours persists over the first 2 months. When that is the case, information about very-early post-seismic slip can help to characterize longer-lasting post-seismic slip, which can prove to be useful to include for the forecast of aftershocks locations." But can this

really be generalized?

A community reviewer pointed out to us that the fact that afterslip patterns seem rather stationary in time, and that is the case for many examples. Following that question, we have investigated the literature on the question. We have only found one study that suggest a hint of afterslip migration (see line 157-161 of the revised manuscript). Therefore, although we cannot conclude with certainty that this can be generalized, it seems reasonable to make the assumption that afterslip is commonly stationary. We have added a sentence to reflect that (line 340-342 of the revised manuscript).

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2021-6/se-2021-6-AC2-supplement.zip