Dear editor and reviewers,

We would like to thank you for providing additional comments about our manuscript se-2021-6 untitled « Very early identification of a bimodal frictional behavior during the post-seismic phase of the 2015 Mw8.3 Illapel, Chile, earthquake ». Below, you will find a point-by-point response to all the comments. The questions/comments are in blue while our responses are written in black. Note that the line numbers are taken from the manuscript with tracked-changes.

Referee #1 : Bernd Schurr

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Referee #3 : Sylvain Barbot

[0] Some important reviewer suggestions, such as constructing their own coseismic slip distribution, were ignored.

Although we argued in our previous revision that we do not see the relevance of producing our own co-seismic model, we have decided to add to the Supplementary Materials our own co-seismic model (see changes in Supplementary Materials S6) since this has been pointed out by several reviewers. We have also added that information in the main text (line 182 in the revised manuscript). We also include the co-seismic static offsets used to obtain the model (see Table S3 in Supplementary Materials S1). The conclusions remain unchanged.

[1] Line 16: A recent contribution to this debate includes

Ozawa, S. and Ando, R., 2021. Mainshock and Aftershock Sequence Simulation in Geometrically Complex Fault Zones. Journal of Geophysical Research: Solid Earth, 126(2), p.e2020JB020865.

but other work can be discussed in more detail.

[2] Line 17: This sentence is vague and the discussion could be more thorough, including more recent contributions. For example, Bath law describes the maximum size of aftershocks, although it does not always work.

[3] Line 19: "All of that" is too colloquial. The few introductory paragraphs could be better phrased to highlight current knowledge on the physics of aftershocks.

Based on these comments, we have attempted to re-write the first two paragraphs of the manuscript so that it reflects better the current knowledge regarding aftershocks behavior (line 13-46).

[4] Line 114: It is still unclear why a MCMC approach is used when the problem is entirely linear and therefore has a closed-form solution. See the work of

Hang, Y., Barbot, S., Dauwels, J., Wang, T., Nanjundiah, P. and Qiu, Q., 2020. Outlier-insensitive Bayesian inference for linear inverse problems (OutIBI) with applications to space geodetic data. Geophysical Journal International, 221(1), pp.334-350.

for linear problems that need nonlinear optimization.

We agree with the comment that when the problem is entirely linear with Gaussian uncertainty on the data and on the prior model, there is a closed-form solution. However, there are two reasons why it is not applicable in our case : (1) we assume a uniform prior on the model and (2) we use a positivity constraint on the slip, which truncate our prior distribution. This is why we believe that a MCMC approach is more suited for our problem. We have added some clarification in the main text (line 130-132). As pointed out, Hang et al. (2020) provides a way to overcome the issues mentioned above, but we were not aware of that study at the time of submission. Regarding this study, we do not think it is worth re-doing all the work using that approach,

especially considering the potential time necessary to get use to the suite of programs. But, we will investigate how it can potentially be used for our future works.

[5] Figure 3: What is the grey area north of 29ºS ?

This is an issue from the plotting routine that we have now fixed on all the Figures in the main text and the Supplementary Materials.

[6] Figure 6: Please indicate the units in the y-axis label (N.m or x 10^{17} N m).

Done on Figure 6 and all other relevant Figures.

[7] Although I cannot make any more informed suggestions on the matter, is there anything in the offshore or ashore geomorphology that indicates structural controls of coseismic and afterslip dynamics? For example, this section of the Chile subduction zone is an erosive margin, implying a rough plate interface.

We agree with the comment that it is difficult the make strong hypothesis regarding what could control the behavior of afterslip and coseismic slip. We already attempted to provide some hypothesis in the main text (line 269-283). For instance, we highlight the study of Poli et al. (2017), that shows evidence for strong fracturation in this region. The way fluids circulate in this region could have played a role at controlling coseismic slip and afterslip. We also point out that coupling is different for each of the afterslip patch (Métois et al., 2014). This could also maybe explain the bimodal behavior that we obtain. Regarding the roughness of the plate interface, it is true that this could also be proposed to explain the behavior of coseismic slip and afterslip (although we can only make hypothesis from our study). We have added that in the main text citing the study of Comte et al. (2019), which provides evidence that materials are entering the subduction interface at the region of Illapel. We also point to the study by Lange et al. (2016) who also suggests that roughness of the plate interface might play a role at controlling the spatial pattern of long-term aftershocks following the Illapel earthquake (line 274-279).