Dear Editor,

We are pleased to submit a revised version of our manuscript. We provide below a detailed point-by-point response (in blue) to all referee comments (recalled in black) and specify all changes in the revised manuscript. We also add a manuscript version showing the changes made within this review process.
We hope that our manuscript is now suitable for publication in Solid Earth.

Sincerely,
Anne Socquet, on behalf of all co-authors

Jeff Freymueller, 19 May 2026

This paper presents a suite of 2D viscoelastic models for the postseismic deformation following the 2014 Iquique earthquake. The key finding is for a low viscosity weak zone (mantle wedge), which is required to explain the postseismic vertical displacements in particular. I think the paper is very well done, and the model they present represents a substantial improvement over past studies. They find that the vertical displacements are especially important in constraining elements of the model, because there are some much stronger tradeoffs within the model parameters when only the horizontal displacements are considered. (I think this is generally true for subduction zone postseismic studies).

The models are 2D rather than 3D. The advantage is that this makes the computational time much shorter, allowing a fuller exploration of the (multi-parameter) parameter space. But the cost is that the model becomes a bit inaccurate far from the trench, because in a 3D model the far field locations will “see” the edges of the rupture but in the 2D case there is no edge to the rupture as it is infinitely long. So the 3D model deformation would likely go to zero a bit closer to the trench than in the 2D model used here. I think in this case the use of a 2D model is OK because the main features that are being modeled and interpreted are close enough to the trench. It is possible that some optimal parameters values might change a bit in the case of a 3D model, but the basic structure they find should not change.

The authors consider a variety of geometric parameters for the viscoelastic structure, and systematically vary these values to find the optimal values. Their final model seems very sensible to me, and I think the basic geometry of an elastic cold nose and a low viscosity mantle wedge is a substantial improvement over previous studies. The “weak zone” that the authors identify really is the mantle wedge. I think it will be more clear to simply refer to it as the mantle wedge.

There are a lot of potential tradeoffs to explore in the model, starting with the contributions of afterslip vs viscoelastic relaxation, and also including potential

variations of the viscoelastic geometry. It is a bit hard to keep track of all of the tests shown in Figures 7-15; this is a challenge for all such papers because there simply are so many things to test. I think the authors could help the reader a bit by providing a paragraph or two at the start of section 5 that would serve as a kind of “road map” to the rest of the section.

We would like to thank the reviewer for his positive feedbacks and suggestions, and for highlighting the added value of this study, notably the importance of the vertical deformation by combining GNSS and InSAR.

We agree with the reviewer that 3D models would be more correct, but that the choice of sticking to 2D models allows us to explore the parameters’ space and different trade-offs, which would not be possible in such an exhaustive manner with a 3D model. We also agree that the main outcome of the paper -- the existence of a weak zone below the volcanic arc -- would hold with a 3D model. A 3D model would likely improve the fit to the far field deformation, that is anyway very poorly constrained with sparse and noisy GNSS observations (This has been added in section 6.1 ‘limitations of the model’). We are therefore grateful that the reviewer supports our positioning to go for a 2D model instead of a 3D one.

The reviewer suggests to call ‘mantle wedge’ what we call the ‘weak zone’. We however disagree here because an important part of this weak zone is composed by the thick (70-80 km) and hot continental crust. This structure of the weak zone is described in detail in the introduction lines 31 to 34 and followed by a series of questions about its exact extent and rheology, motivating the study.

We agree that we explore a large range of parameters. This has the advantage of exploring the trade-offs within the parameters’ space, but makes the paper a bit ‘indigest’. We have done our best to improve the readability of the paper in the revised version, by following the reviewer’s suggestion of adding a road map at the beginning of section 4 “Modelling Strategy” and section 5 “Results”. We have also moved the less meaningful tests in electronic supplement (location of the craton’s border and viscosity of the oceanic mantle).

Thanks a lot for the list of typo. All suggested corrections have been included in the revised version, details below:

Minor corrections

Line 2. Change earthquake to earthquakes OK
Lines 8. I don’t know what you mean to say by “claims for a weak zone”. The verb is wrong. changed into ‘pleads’
Lines 20-21. Errors in references. fixed
Line 30. Add “is” before “substantially” OK
Line 67. Change “is” to “was” at the start of the line OK
Line 97. Add SAR after interferometric OK

Line 124. “Therefore, we fixed tau to 45 days”. Based on what? The GNSS? You later get a different value for the GNSS. I suspect that it does not make much difference because you are modeling only the total deformation in the InSAR, but why the difference in time constant? We had to pick a value and this is the default value we usually use. We could have used a shorter relaxation time, but it would not have changed the results much as it affects mostly the curvature of the time series during the months following the earthquake, where we have no InSAR data.
Line 129. Change cumulated to cumulative. Change from to for OK
Line 131. Change miss to lack OK
Line 134. Change cumulated to cumulative OK
Line 147. Change Bi-frequency to Dual-frequency. OK
Line 148. Change inverted to estimated OK
Line 149. Add the before VMF1. Add “mapping functions” after VMF1 OK
Line 150. Change keep to estimate OK
Line 156. Add the before eight OK
Line 164. Change developped to developed. OK
Line 206. Change “As for” to “As with” OK
Line 251. Change is to are. Change parameter to parameters OK
Line 270. Add a comma after rheology OK
Line 271. Change transposed to translated OK
Line 353. Change “The afterslip” to “Afterslip” OK
Line 364. Change “add up” to “add constructively” OK
Line 370. Change “show that the” to “showed that” OK
Line 389. Change constraints to constrained OK
Line 393. Add “any” before “cold nose” OK
Line 399. Delete the comma after parameters OK
Figure 4, inset. I suggest using black rather than green. The green could set up an issue for colorblind readers, and black will be at least as clearly visible. We are unfortunately unable to modify the figure as the file is corrupted. However we believe that the figure allows illustrating the model structure well enough.

———————-

Sabrina Metzger, 28 May 2026

Review on the manuscript “Constraints on the Lithospheric Structure and Rheology of Northern Chile from 8-year Post-Seismic Deformation following the Mw 8.1 Iquique Earthquake”, submitted to EGU Solid Earth by Cresseaux et al.

Dear authors, associate editors and editors,

The manuscript of Cresseaux et al. explores a multitude of crustal and mantle rheologic parameters to best predict the surface displacement after the Iquique earthquake observed by GNSS and InSAR time-series.

This is not an easy tasks as the observations have spatial and temporal gaps that need to be filled by using some assumptions, and the two main processes the authors study – afterslip and visco-elastic relaxation – are overlapping but occur on different time-scales.

The synthesized geodetic observations provide (to my knowledge) a new level of completeness of this event and the visco-elastic model is very advanced with many model parameters to be tested. Next to some best-fitting rheologic constraints the main finding (as I understood it) is that a cold nose is needed to reproduce the observed displacements.

I have nothing to criticize on the data analysis, modeling and interpretation, but was overwhelmed by the abundance of parameter tests presented in the manuscript. While this might be very enlightening for modeler experts, the amount of parameter tests might be a bit overwhelming for the general audience that wants to learn more about this particular event. Maybe a few more figures could move to the supporting material and the Results/Discussion/Conclusion could be focusing on a few less points? I will trust the authors on whatever they prefer. But in any case the text should be revised for clarity and English grammar and wording. I will list my main questions or comments below, followed by many smaller comments on the text and figures that can be dealt with rather easily.

I hope the authors find those comments useful. Sabrina Metzger, Potsdam, Mai 27, 2026

Key issues/questions

• Potentially reduce the abundant model tests in the main text I was overwhelmed by the many tests and technicalities which made it hard to filter out my learnings. Do you see a possibility to reduce the number of parameters presented in the main text and refer to some of them in the supporting material? Can you somehow better highlight the geologic importance of this heavy- geophysical method approach for non-experts? Would it be interesting for example, to elaborate on the afterslip patch depth and extent, whether location and amplitude agrees with previous findings, for instance?

• Chi-Square tests It remains unclear to me how you weight the InSAR against GNSS ChiSquare-fits against each other. In other words: How do you derive the red vertical line in Figures 7/8G? Is an objective (1:1) comparison of all four input data sets possible (horizontal vs. vertical vs. asc./dsc. LOS) or should there be a relative weight between them? Does the InSAR fit depend on the number of InSAR samples you use? Is there an inter-parameter comparison even, so the one model that suffices all parameters best?

• Use of geographic expressions You use many geographic expressions (Altiplano, Cordillera, Volcanic Arc) but do not introduce them in a map, which would be very helpful to non-local-experts.

• Use of precise and complete terms Please carefully review the manuscript for precise naming of terms and correct English (which probably can be done by one of the senior co-authors). For example, avoid the use of the very generic terms “data” and “model” and use “surfacedisplacement”, “geodetic observations”, “predicted displacement” etc. . Avoid sentences like “Figure X shows this and that”, instead say “This and that (Figure X).” I provide many suggestions for the first half of the text, then stopped due to time constraints.

• Order in presenting the best model parameters and parameter boundaries When reading the text, I found it irritating to read of your results (=best-fitting parameters) already in the section where you introduce the model design and refer to a table to read about the model a parameter boundaries.

At the end of section 4.3 you then greatly discuss the parameter boundaries (which people already have explored in the table before). Why not providing numbers at all in Section 4 but refer to the Table, and then later in the results, list and discuss the best-fitting model parameters?

• Role of fluids I did not see any discussion on the role of pore-pressure diffusion following large earthquakes as it has been postulated (e.g. Pena et al., 2025, https://doi.org/10.1038/s41467- 025-65013-6). Are such processes not needed, if a cold nose is included in the model? Can you elaborate on this topic?

We would like to thank very much the reviewer for her very detailed and extensive review, and for the constructive suggestions provided that help improve significantly the revised version of the paper.

Thanks a lot for highlighting the added value of the paper, both on synthesis of geodetic data and on the visco-elastic modelling and parameter's space exploration. Because of this exploration, we understand and acknowledge that the paper might be difficult to read. This has been raised by reviewer #1 as well, and we have done our best to improve the 'flow' of the revised version (including in the wording and descriptions), and better highlight the geologic importance and the key findings for non-experts by adding a road map at the beginning of sections 4 and 5 (see answer to previous reviewer).

The reviewer wonders how the fit to InSAR and GNSS data is weighted relatively in order to choose the best model. The simple answer is that we do not have a 'recipe'. We evaluate separately the chi squared for all different data sets (GNSS horizontal, vertical, InSAR ascending and descending), and we chose the preferred model as being the 'best' compromise, explaining most of the data. This is explained at the beginning of section 4.4.

Finally, we have added in section 6.5 a discussion about the role of pore-pressure diffusion following large earthquakes, as suggested by recent studies. We acknowledge that poroelastic deformation could be an alternative to the transient viscosity (Kelvin solid) modeled through a Burgers rheology.

We are very thankful to the reviewer for providing numerous detailed comments that we have addressed in the revised version. Details are available below:

Minor comments on the text

L20: Check reference-command done

L30: "South American" instead of "Brazilian"? Is there an "is" or "has" missing before "thinning"? OK

L43: Rephrase “of the monitoring of the deformation” – it reads a bit weird. Replace “GPS” by GNSS (method instead of brand) and introduce the abbreviation. Also “InSAR” on the next lie. rephrased

L46: delete “that is” OK

L48: Delete “of the geometry in order” OK

L50: Delete “against the data” and use “The continuous deformation field…” OK

L51: “of the source kinematics” or “of the deformation source”? not found in the text

L55: “Andean Cordillera”? OK

L56: replace “into” with “during” OK

L57: “segment of the subduction zone” OK

L65: “observed at 300 km distance from…” OK

L75: Delete “the” before the date OK

L76: Delete “been” OK

L100: “”that goes from raw data to” reads weird. Can you delete this and rephrase a bit? rephrased

L111: Explain ITRF, respectively, is it relevant or could it be dropped? dropped

L112: “into displacement time-series” (for clarification) OK

L113: what do you mean by misclosure? Phase closure of interferometric loops? Or a comparison of the two time-series results? Please explain. done

L115: Replace “phase delay” with “displacement” this is actually the phase delay, not the displacement (the phase delay includes displacement + atmospheric delay)

L116: “signals” instead of “screens”. “are” instead of “is”. Provide a reference for the trajectory model. Probably Bevis & Brown 2014?L121: “relaxation time tau” (add the tau symbol) OK

L125: The estimation of the secular loading rate v and the signal strength of the post-seismic relaxation A” OK

L130 “cumulative LOS displacements” OK

L131: “far-field InSAR information unaffected by a post-seismic signal” OK

L132: Delete “choose to”. “cumulative” instead of “cumulated” (also later) OK

L135: “linear function of range and azimuth” = “linear ramp” or “linear plane”. Clarify what you fit to what (InSAR to the GNSS reference frame, I assume?) OK

141: “GNSS time series [analysis]”? OK

143: It would be informative to add one more sentence with infos on “Data processing” : From what to what format using which software. (You explain it below so maybe reshuffle the sentences a bit.) OK

145: “RINEX”? Before and after this sentence you talk about processing so maybe reorder the sentences somewhat. It is unclear to me what is the same as in Bertrand et al. and what is new in the detailed processing explanations. We tried to clarify

147: Explain NNR OK

153: Can you reword the statement “allows us to avoid the contribution“ and instead say something like “we assume that the remaining signals are insignificant for this work” or so. OK

156: “do not have sufficient data” OK

160: Mention that what you provide is an Euler pole rotation with lat/lon/angular rate. OK

163: “trajectory model following Bevis and Brown”. What is the difference between the Bevis/Brown approach and the Marill approach? OK. It is another code. Differences are explained in the referenced papers.

171/172: Is there a proper reference for these influence radius numbers, where one can read how they are derived? No, not to my knowledge…

172: “moment magnitude” OK

185: What is the difference from this equation to the last term in Equation 2? Nothing really, I guess, somaybe you can skip it? OK equation has been removed

190: “time-series contains only data shortly after the earthquake”

197: The detailed explanation of the RMS could be skipped (or simply mention that it is an estimate for the goodness of fit between observation and model). OK

201: “If the time-series only begin several months after…” OK

207: “We consider…” Rearrange the text fragments in this sentence, it reads a bit weird. OK. some unnecessary details have been skipped there.

217: This cyan-sentence is better placed in the figure caption than here. OK

220: bring → provide, deliver, produce OK

228: Delete “The” in “The Figure2” OK

290: “with coherent amplitude despite”? OK

233: trench-perpendicular OK

235: “published under the MIT license”: This reads a bit weird and/or could be a legal issue, as MIT is not affiliated with the authors? Removed, that was indeed useless.

239: “1900 km from the trench towards west and 2100 km towards east” We think that our formulation is more correct.

242: “horizontal direction, the bottom in the vertical direction” OK

249: “Five” instead of “5” OK

251: Delete this sentence and add “The structural parameter…” directly to the before sentence (no new paragraph). OK

257-260: From how you phrase it it is unclear to me whether these statements should be here (model design)or more belong to the discussion. It sounds like you tested various models and show that what you found matches the findings of others. If so, then I would move it to the discussion. We describe and justify the model structure. Values are given for the best model to get an indication of the actual geometry of the model. We have added a road map at the beginning of sections 4 and 5 to explain that we describe the best model and then discuss the effect of moving individual parameters.

261: “low velocity (weak) zone”? This is the weak zone.

Table 2: Delete the first “Elastic”, or replace “for the…” with “used in the model”. No comma after rho. I also suggest to write “vp” and “vs”, or somehow reduce the space between “V s” and “V p”.OK

285: Delete “use a simple approach” and write “kinematics”. OK

286: The second equation looks so complex, I would not put it in line with the text but on an extra line. We tried to simplify.

293: “We explore ten…” “Three” instead of “3”, “four” instead of “4”. OK

299: No new paragraph here, you are still counting your varying parameters. The same in line 306. OK we tried to clarify.

312: Can you be more precise, e.g. saying “between the observed and predicted surface displacements” and explaining HOW you determine the best model? (By visual and quantitative best-fit, for example, RMS etc.) OK we added some details in our summary.

315: “offsets” instead of “data” We changed into ‘observations’.

317: “GNSS and InSAR observations” OK

325: “Chi-square-fits”? Or “ratios”? chi-squared

327: Is dispersion another term for root-mean-square? no

331: “InSAR displacement map” OK

333: Where does the 1.4826 come from? The factor 1.4826 makes the scaled MAD comparable to the standard deviation under a normal distribution. This is now explained in the text.

340: “ignoring the component along North-South” OK

342: we compare observed and predicted surface displacements along…” OK

348: “GNSS time series show” OK

348ff: Is it worth discussing the outliers from the general observation in Figure 3 (time constant increases with distance from trench), i.e. BN01 at the coast, and the high variations in the intermediate-field? You have an idea why you see these details? There are some uncertainties in the observations, and some lack of data that make the fit poorly constrained in some cases. We have decided not to discuss these details in the paper in order to keep focused on the main first-order features. The paper is already long enough…

353: “Afterslip is a relaxation phenomenon on the plate interface…” OK but we kept fault plane as all faults are not necessarily plate interfaces

356: “of the observations at the coastal stations” OK

358: “contribution … to our best model” OK

363: “significant. Although the contribution…” OK

367: Why should they be sensitive to different processes? Is one producing more vertical uplift than the other, thus canceling out the signal in one LOS-direction? Yes that’s it. It is explained beforehand

367: “InSAR displacement maps from both, ascending and descending tracks,…” (be more precise with your descriptions and avoid using terms like “data” and “model”, if possible. OK

380: “Altiplano”, maybe also “Brasilian craton”. These geographic term is of high importance for the interpretation of your models. For the benefit of non-local experts I suggest you mark the region somewhere in (or in many of) your figures. We believe that the regional terms are generic enough or well known enough for non local experts (Western/Eastern cordillera, Altiplano, Brazil….)

385: Apologies if I have overread it, but: Do you explain somewhere how you weight GNSS Chi-Square against InSAR Chi-Square? Are they directly comparable or is there a relative weight between them? In Fig 7G one could also put the best thickness more at 27 or 28 km, no? (Ok, maybe because you did not test these numbers, but only 30 km, I see.) The same question is valid for Figure 8G. How to weight InSAR data points against GNSS? As explained earlier, we evaluate separately the chi squared for all different data sets (GNSS horizontal, vertical, InSAR ascending and descending), and we chose the preferred model

as being the ‘best’ compromise, explaining most of the data. Our objective is not to find THE best model, but to get a first order and get a feeling of how varying the parameters affect the deformation field and fit to the data.

439: What is your opinion on the influence of pore-pressure changes to such models? (e.g. Pena et al., 2025, https://doi.org/10.1038/s41467-025-65013-6) Do you think they are not needed if a cold nose is included? We have added a discussion about this in section 6.6.

Should (in the ChiSquare analysis vertical and horizontal fits be treated equally from this perspective or not?) We believe that the vertical and InSAR data are key to constrain the geometry of the weak zone. The effect of each parameter on the modeled deformation and their potential trade-off is now explained in section 6.1.

444: In the bracket provide the full parameter name instead of the variable. Done

454: “long spatial wavelength”OK

456: “GNSS stations” OK

458: Consider using plural for “InSAR time-series” (also further up) as they contain lots of data points and rates and offsets. OK

459: GPS → GNSS OK

465: Cordillera? (mark it on a map if you mention it and refer to the respective figure). This is clearly visible from figure 4.

475: Label the volcanic arc on a map. The volcanic arc is along the Western Cordillera, this is now specified.

479: “seismic” what? tomography. It has been added. Thank you for noticing.

506: “Western Cordillera”? OK. Capital letters have been added at the beginning of the words.

519ff: The links look long and complex and not very stable on a long-term. I see that the first link could also be shortened by using the data DOI (http://doi.org/10.17178/GNSS.products.SouthAmerica_GIPSYX.daily). We have decided to keep the links as they are.

I also could not access the peps.cnes.fr webpage with the Sentinel-1 data (offset maps? Full time-series?) and the Brazilian network RINEX files. The peps website gives access to raw data (SLCs). The access to the Brazilian data should be granted by the Brazilian colleagues (usually a simple request by email is enough).

Comments on the figures

Figure 1: (Caption) Provide the source of the EQ locations (USGS?) yes and the convergence arrow and rate (angermann?) kendrick et al. 2003. Information has been added

Figure 2: (caption) “projected into LOS”. I suggest to zoom in a bit to increase the details. As of now there is nearly 2 degrees eastern longitudes and 1 degree western longitude and 1 degree southern longitude with no information. Could the white profile be shortened as there is no data to compare to in the following figures? I miss a discussion of this figure. Are you happy with the agreement between InSAR and GNSS? Why and where are the biggest discrepancies? This figure is discussed in section 3.3. Because the paper is long already we do not feel that much more discussion about local discrepancies is needed. Both Figures 2 and A28 show that the global consistency between InSAR and GNSS is good in average (We should keep in mind that GNSS data suffer data gaps and interseismic is corrected from a model).

Figure 3: (Caption) “the vertical grey line separates the two…” OK

Figure 4: The red color for the explored parameters is painful to distinguish (or not distinguishable at all for readers with impairment), so better choose another color scheme, e.g. you can use very weak colors for the background and then black for the parameters (and all other text in gray). We are unfortunately unable to modify the figure as the file is corrupted. However we believe that the figure allows illustrating the model structure well enough.

(Caption:) Technically you do not use the stations, you use the GNSS offsets (or rates) measured at these sites. Is “delimination” an English word? Is there a difference between gray-labeled and black-labeled GNSS stations? Suggestions: “The explored afterslip parameters are patch depth Ad, slip amplitude Aa and patch width Aw.” “Five” instead if “5”. “co- and post-seismic slip patch imposed”. OK. We tried to improve the caption.

Figure 5/13: Mark the stations shown here in a map further up for orientation. I suggest to label the y-axis with “trench-perpendicular (mm)” to be clear on it (also in the following figures). Use a stronger color for the measurements – they are hard to distinguish from the error bars. I suggest to sort horizontal time-series on the left and vertical on the right column and add sub-labels a) to l) so that you can directly refer to them in the text, if needed. Also, to quickly grasp the models, it would be good to provide a legend directly in one of the top sub-figures. Would it be helpful for the interpretation to use equal y-axes limits for horizontal and vertical each? The stations used here are shown along a cross section in figure 4 and in map view in supplementary figure A1.

Figure 6: (Caption) “Trench-perpendicular cross sections…”, then delete the second sentence. No need to explain the color code in the caption if it is already explained in a legend (but add the gray InSAR polygon to the legend, too). “displacement” instead of “data”. The dark blue color is hard to distinguish from the black. C and D do not present InSAR, right? You seem to explain E and F twice. I would delete “, 1.4826 times”, but better explain in the text, where this number comes from. We changed the caption following the reviewer’s suggestions. We prefer however to keep explaining the color code in the caption to make it more clear.

Figure 7/8/9/10: Consider showing only every second h-model, they are very similar and hard to discriminate with this color scheme. “InSAR A149” is incomplete. The legend is

incomplete, provide units to the thicknesses at least. Add “(marked by a bright red vertical line)” to the last sentence. “Figure” instead of “figure”.The legend has been completed and the units of the explored parameters have been specified in the legends.

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Anonymous Referee #3, 01 Jun 2026

This paper combines 8 years of GNSS time series with 5 years of InSAR to characterize post-seismic deformation of a Mw8.1 earthquake Authors jointly inverted afterslip and viscoelastic relaxation using a 2D FEM that explored several parameters: afterslip geometry, structure of a weak upper plate, cold-nose decoupling depth, oceanic mantle viscosities. Authors preferred an elastic cold nose reaching slab-mantle decoupling depth and Burgers rheology for the weak zone.

This study has significant importance in understanding the deformation after a megathrust earthquake. They explored the particular time interval of an earthquake cycle that connects the short-term coseismic and long-term interseismic periods, which is often the most difficult time period to model and characterize. This paper has reasonably dealt with this difficulty. There are a few other strong points of this paper:

- The dataset is valuable; the longer observation window is a genuine advance
- The core conclusions are plausible and reasonably supported, especially the cold-nose result and weak-zone Burgers rheology result

However, several methodological choices warrant stronger justification.

- There is a 7-month InSAR gap, which means the early uplift signal that the paper argues requires a Kelvin body is dependent only on GNSS. The authors should state this limitation explicitly at the point where the Burgers conclusion is drawn, not only in the data section. We have added in section 5.4 the fact that the Kelvin viscosity is constrained by GNSS data over the first period.
- I got a little confused with referencing InSAR with GNSS mentioned in section 3.1. Then, in section 6.3, authors state that GPS reference issue will propagate into InSAR flattening. Also, in section 5.3, authors note that the ascending InSAR track brings the “strongest constraint” on the optimal location of the cold nose limit. These statements seem contradictory, and I am having a hard time distinguishing an independent InSAR constraint from the one inherited through referencing. Shouldn’t a GNSS-only or InSAR-only fit, shown side by side, strengthen the argument?
- The parameter exploration (section 4.3) in this paper varies individually around a preferred model instead of jointly sampling the parameter space. If these parameters are coupled, few would definitely show trade-offs (such as afterslip amplitude/width vs near-field viscosity, dcn vs h vs Xcraton). Therefore, the sequential search in the paper risks settling in a local minimum and underestimating uncertainties. The authors should at minimum, in section 4.3, (i) state if and where joint exploration was performed, (ii) justify the order of

parameter exploration, (ii) report uncertainties around preferred values rather than listing optimal point values (the reader cannot judge whether dcn = 84 km is meaningfully distinct from 80 or 88 km). The afterslip amplitude-width grid search is the right idea and should be extended to the parameters that most strongly trade off.

- One of the major limitations of the current methodology is how the geodetic signal is partitioned. From my reading (and please correct me if I am wrong), the partitioning is built into the model rather than derived from it. I see that depth, width, amplitude, and time function of afterslip are prescribed. I wonder how this mechanical decoupling between co-seismic viscoelastic run and afterslip would affect the final interpretation. In a self-consistent model, co-seismic and afterslip stresses both should load the viscoelastic medium, and the relaxation should modify the stress that drives the afterslip. Summing two independent runs neglects this coupling. The authors should at least argue why this is acceptable over a decadal window. This seems important since the afterslip is allowed to extend to the very depth range where the weak zone and decoupling begin to matter. The claim "the early uplift requires a transient Burgers rheology" is only as robust as the imposed afterslip vertical signature.

We thank the reviewer very much for their review and relevant and useful comments. Notably, we appreciate that the reviewer acknowledges the added value of the dataset, with a long observation window, as well as the relevance of the core conclusions, notably the cold-nose geometry and the necessity for a weak-zone with a Burgers rheology.

We agree with the reviewer that the need for a Kelvin body essentially relies on the GNSS data. We do not find however that this is a strong limitation as the GNSS data in the early period and in near field are of pretty good quality, including for the vertical, and the fit of the model with a Burgers viscosity is significantly improved compared to the model with a Maxwell viscosity.

We agree that explaining the referencing of InSAR with GNSS is never straightforward and can be confusing for non-experts. Indeed InSAR is a relative measurement that should be offset and tilted onto the GPS. However, with the NSBAS processing that we use, the curvature of the INSAR is NOT inherited from GNSS and is a strong independent constraint for our models. In our case, this peak in the InSAR data is beautifully colocated with the peak of GNSS data, and we find a model that reproduces the location (and the amplitude) of the peak, that depends on the location (and amplitude) of the viscosity contrast at depth

We agree with the reviewer that the parameter exploration section is somewhat difficult to follow, as also pointed out by the two other reviewers. In the revised version, we have clarified our positioning at the beginning of section 4 and explain how the exploration is performed. We do not aim at providing the ultimate best model, in the least square sense,

but we want to reach a ‘prefered’ reasonable model that fits our complete data set, with a reasonable compromise between horizontal and vertical, and that also makes sense from the geological point of view.

We agree that there are some trade-offs between parameters. The important parameters that we found to be affected by some tradeoff are :

- the crustal thickness, that changes slightly the wavelength and the amplitude of the deformation signal
- the maxwell viscosity in the weak zone, that changes the amplitude of long term signal associated with viscous relaxation, notably on the vertical

Although it surely exists, the trade-off between these two parameters is pretty small as the elastic thickness does not affect the deformation signal very much.

Other important parameters without major trade-off are:

- the location of the cold-nose border, that controls the location of the maximum uplift
- the continental viscosity, that does not induce changes at short time scale, but changes the amplitude of the viscoelastic deformation at long times scales, notably in far field
- the kelvin viscosity in the weak zone, that changes the short-term (and the long-term) signal associated with the viscoelastic relaxation. In that case, a small tradeoff with the prescribed afterslip exists, but to obtain the observed vertical deformation in short-term, a significant contribution of the transient Kelvin viscosity in the weak zone is needed.

Finally, the following parameters are less impactful and without much trade-off. We have therefore moved those tests to the electronic supplement in the revised version:

- the craton border, that slightly modifies the amplitude in far field, and that is not critical in this model
- the oceanic viscosity, that does not infer any change at short time scale, but changes amplitude of the vertical in the forearc at long time-scale.

A discussion about these trade-offs has now been added in the section 6.1.

In this paper we do not aim at performing any inversion nor doing a full parameter space exploration. We only want to evaluate the effect of each parameter on the resulting deformation, and compare it with the geodetic data, which is something that is rarely done for viscoelastic models with this level of detail. In several papers, including some of the most impactful, only one parameter is discussed, without a lot of data to constrain it. Here we explore a few parameters associated with the geometry and the rheology of the weak zone. Our ambition is to evidence the first order structure, not to explore a fully complex 3D structure, that our data would not be able to constrain (even though we have the most complete geodetic data set to date for this earthquake).

Finally, the reviewer understood correctly, the partitioning is built into the model and the afterslip is prescribed. We do not model a stress driven afterslip, because we were mostly interested in assessing the rheology of the overriding plate, and the geometry of the weak zone. We agree that it would be more elegant and correct to load the viscoelastic medium with both the co-seismic and the afterslip stresses. However, in our case the slip ratio co-seismic / afterslip is 7m/40cm, so a ratio of about 20. The stress loading by the afterslip is therefore negligible as it represents only 5 to 10% of added stress. This was confirmed by test simulations that we have run, and for simplicity in the model handling, we have decided to neglect the retroaction mechanism.

Minor comments:

1. Line 8 (abstract): “The amplitude of the uplift pattern claims for a weak zone”. Replace the word “claims for” with “requires” or “calls for”. We have changed into ‘pleads’.
2. In the first paragraph of the introduction, fix the citation formats (currently: "?e.g.>[] pollitz2006post, panet2010upper, suito2017importance, Li2017PostseismicRheology, boulze2022post") done
3. Define what you mean by ‘weakened zone’ in the first paragraph of the introduction so that readers will understand the reason behind this characterization. We have added that we mean low viscosity zone.
4. Second from the last sentence in introduction reads,*The use of a simple 2D model, rather than a 3D model, allows us to test 50 various configurations against the data*.’ Authors should add another line after this that justifies their choice of isolated parameter testing in a 2D setting rather than doing co-evolution of parameters in a 3D setting. We have added a sentence following the reviewer’s suggestion.
5. Can you the numbers such as 1.10^(20) to a consistent pattern of 1 * 10^(20). I mistook this for a decimal point first. Done
6. Fix the section 5.1 heading. The wording does not make sense at all. The title has been changed into ‘Evidence Supporting the Need for Both Afterslip and Visco-Elastic Relaxation’
7. In Figure B2, I see the x-axis titled ‘afterslip depth’ goes from 75-100. But Table 1 shows the exploration range of afterslip depth goes from 30-50 km. This is confusing and should be made internally consistent. Thanks for noticing this inconsistency. Indeed this is our mistake: what was plot in figure B2 was the location of the afterslip as a function of the distance to the trench (which can be converted into depth by projecting on the megathrust interface). This is now fixed.
8. A Kelvin rigidity of 136 GPa for the weak zone seems unusually high. Please provide a physical justification or clarify that this is an effective parameter rising from Burgers to Maxwell transposition. The Kelvin rigidity controls the amplitude of the transient (recoverable) postseismic deformation, whereas the Maxwell rigidity controls the long-term viscous relaxation. In Burgers postseismic models, the relative Kelvin and Maxwell rigidities are commonly prescribed rather than independently constrained. Some studies adopt μK ~ μM, whereas others use μK>μM to enhance the transient

response. For example in Klein et al. 2016, their prefered model uses $\mu K = 2.6 * \mu M$, which is even larger than the ratio we use.

Klein, E., Fleitout, L., Vigny, C., & Garaud, J. D. (2016). Afterslip and viscoelastic relaxation model inferred from the large-scale post-seismic deformation following the 2010 M w 8.8 Maule earthquake (Chile). Geophysical Journal International, 205(3), 1455-1472.

9. The Luo and Wang (2021a) and (2021b) appear to be the exact same paper. Indeed. Sorry for the mistake, this has been fixed.