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 would like to thank the reviewers and editorial board for their constructive feedbacks that helped us to improve our manuscript.
We hope that our manuscript is now suitable for publication in Solid Earth.

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

Jeff Freymueller, 16 Sep 2026
accepted subject to minor revision
I think that the authors have addressed the review comments effectively. I have only a few very minor comments and suggestions. Line numbers refer to the version without changes tracked.
Line 131. Remove the comma after “both” done
Line 133. Add “the” before “coupling model” done
Line 157. Replace “IGS2014” with “IGS14 realization of ITRF2014” done
Line 182. Change “does not” to “reasonable variations in this value do not” done
Line 228. Add “a” before “finite element” done
Line 255. I suggest that here, at the first mention of the weak zone, you should mention that this includes the mantle wedge but it not restricted to the mantle wedge. Something like, “a weak zone, which includes the mantle wedge and part of what would otherwise be lithosphere” done

Finally, below is just a suggestion -- edit or not as you think best.

Lines 502-510 (poroelastic deformation). I think you can also say that the effect of poroelasticity is largest out in the forearc, above the slipping patch. This is logical given where the dilatational strains here, but you can also refer to Hughes et al. (2010, EPSL), which shows in their Figure 7 where the poroelastic deformation is concentrated. Peña et al. (2022) presented a poroelastic plus afterslip model for Maule, but looked at only a short period of data and did not account for viscoelastic relaxation (which may have biased their model; the magnitude could be mis-estimated, but the spatial pattern is likely physical). But you can see in their Figure 5 that the horizontal poroelastic displacements are large offshore, and modest in the coastal area (some uplift is predicted there).

Hughes et al. (2010):
https://www.sciencedirect.com/science/article/pii/S0012821X10001548#fig7

Peña et al. (2022):
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2022GL098144

Thank you for pointing this out. We agree with this comment. We now cite Hughes et al. 2010 and have added the points suggested by the reviewer in the section 6.5 that is now :
Recent studies have suggested that pore-pressure diffusion following large earthquakes may play an important role in post-seismic deformation, particularly during the early post-seismic phase \citep{pena2025pore}. Poroelastic deformation is expected to be largest in the forearc above and around the coseismic slipping region, where the coseismic dilatational strains are concentrated \citep{Hughes2010Poroelastic}. Because our geodetic observations span six years following the earthquake, the long-term deformation cannot be explained by poroelastic processes alone. Poroelastic deformation is expected to decay during the early post-seismic phase, whereas viscoelastic relaxation can persist over several years or decades. Thus, the long-term deformation observed in our data requires a viscoelastic contribution, which provides robust constraints on both the geometry and Maxwell viscosity of the weak zone. Moreover, the weak zone inferred from our modeling is centered around the volcanic arc, substantially farther inland than the forearc region where poroelastic deformation is expected to be concentrated. The spatial separation between the predicted poroelastic deformation and the deformation signal associated with our weak zone suggests that the lack of poroelastic modeling is unlikely to significantly affect our estimates of the geometry and viscosity of the weak zone.

Poroelasticity could nevertheless contribute to part of the transient deformation that we attribute to a low transient viscosity during the first few months after the earthquake. \citet{pena2022poroelasticity} showed that accounting for poroelastic deformation can significantly modify the inferred afterslip distribution during this early phase. Their analysis, however, does not account for viscoelastic relaxation, which may affect the estimated magnitude of the different contributions, although the spatial pattern of poroelastic deformation is likely to remain physically meaningful. A trade-off between poroelastic deformation and afterslip is therefore possible, as both are expected to produce significant deformation in the forearc. In contrast, the transient viscoelastic deformation inferred in our model is primarily associated with the weak zone further inland, and is therefore less likely to be strongly affected by this trade-off. Quantifying the respective contributions of poroelasticity, afterslip, and transient viscoelastic relaxation would require a joint modeling approach, which is beyond the scope of the present study.

Sabrina Metzger, 27 Aug 2026

accepted as is

Anonymous Referee #3, 01 Jun 2026
accepted subject to minor revision
After reading the revised manuscript, I still have concerns regarding the identification of the preferred model, demonstration of parameter exploration and rheological interpretation. However, I don't think adding more works towards these concerns will

change the central conclusion of this paper. Therefore, I am recommending a minor revision.

The remaining points can primarily be addressed by clarifying the methodology, documenting the tests already mentioned in the response, and qualifying statements where the evidence is conditional on the adopted model assumptions.

The authors have addressed my comments reasonably, especially the clarification of the modeling strategy, discussion of parameter trade-offs, and poroelasticity. The two central contributions of this paper, GNSS and InSAR data, and the proposed cold-nose and weak-zone structure, are scientifically useful and a timely contribution to the community who are increasingly looking to incorporate the effects of structural heterogeneity in modeling interseismic and coseismic deformation using geodetic data.

After reading the revised manuscript, I still have concerns regarding the identification of the preferred model, demonstration of parameter exploration and rheological interpretation. However, I don't think adding more works towards these concerns will change the central conclusion of this paper. Therefore, I am recommending a minor revision.

The remaining points can primarily be addressed by clarifying the methodology, documenting the tests already mentioned in the response, and qualifying statements where the evidence is conditional on the adopted model assumptions.

1. Selection of preferred model and parameter trade-offs

Authors made clarification that this study primarily evaluates model sensitivity (as opposed to a joint inversion), which is helpful. However, the criterion used in selecting preferred compromise between the GNSS components and InSAR tracks remains a little unclear and needs to be more explicit. This will help a reader who doesn't model geodetic data regularly.

Our approach is described in Section 4.4. There is no simple metric that can identify the "best model". We therefore select the preferred model based on several criteria: visual inspection of the overall pattern of modelled versus observed surface deformation, and the fit, in a chi-squared sense, between the model and the different observations (horizontal and vertical GNSS, ascending and descending InSAR). We could compute a weighted RMS or weighted chi-squared, but this would introduce the difficult issue of how to weight the different datasets relative to one another. Although such an approach might appear more objective, it would in fact conceal strong choices regarding the relative weighting of the datasets. As there is no unambiguous recipe for determining these weights, we prefer to show explicitly where the different datasets agree and disagree with the model.

Also, Table 1 should report the values as "preferred" rather than "optimal", which seems more consistent with the stated modeling approach.

We have changed "optimal" to "preferred" when mentioning the model of parameters according to the reviewer's suggestion.

Please also acknowledge that when testing individual parameters, the other parameters are fixed

We have clarified this in section 4.4, by changing the last sentence of the first paragraph as :

We then investigate the influence of each model parameter by varying the parameter ranges around the best model, ***keeping all other parameters fixed****.*

2. Interpretation of Burgers rheology

The revised manuscript more clearly identifies early GNSS observations as the primary constraints on the transient response. Also, the newly added section 6.5 now acknowledges the potential contribution of poroelastic deformation to early response attributed to transient viscosity. This is a good addition to the discussion. On this note, I would suggest, soften the wording in the abstract and conclusion that Burgers rheology is "required". A potential wording can be, "Results support/indicate Burgers rheology within the tested model family".

We do not consider our wording to be particularly biased. In our discussion of poroelastic relaxation, we acknowledge that it may contribute to early post-seismic deformation. However, as aptly pointed out by Reviewer 1, Jeff Freymueller, the poroelastic effect is expected to be strongest in the forearc, above the slipping patch, and to be significant primarily during the first 2–3 months after the mainshock. Poroelasticity therefore primarily trades off against afterslip. Our results show, however, that a weak zone farther inland, close to the volcanic arc, is required to reproduce the observed deformation pattern, notably the vertical observations. Interestingly, Peña et al. (2022) also require viscoelastic relaxation to reproduce the uplift observed at station COLC, located close to the volcanic arc, during the first few months following the earthquake (Figure 2g).

By investigating post-seismic deformation over several years, rather than just a few months, we demonstrate that a time-dependent viscosity is needed to fit both the short- and long-term deformation. We therefore model this transient rheological behavior using a Burgers body and consider this finding sufficiently robust to retain it in both the abstract and the conclusion.

Please also clarify whether the Maxwell model shown in Figures 10–11 was independently adjusted or obtained by replacing the rheology while retaining the other preferred parameters.

In these figures the Maxwell viscosity is the same. The only difference is the addition or not of a Kelvin body. This has been added in the caption of the figure 10.

3. Kelvin rigidity

The response suggests that prescribing a Kelvin rigidity greater than the Maxwell rigidity enhances the transient response. Please clarify what is meant by "enhances," particularly whether this refers to deformation amplitude or its timescale. For a Kelvin element under constant applied stress, the limiting recoverable strain is inversely proportional to Kelvin rigidity, while the characteristic retardation time is given by $\tau_K = \eta_K/\mu_K$. Increasing Kelvin rigidity at fixed viscosity therefore reduces the recoverable strain and shortens the retardation time; it does not increase the initial deformation rate.

Although the surface response of the complete model also depends on stress redistribution and geometry, a larger Kelvin rigidity cannot generally be justified as producing a larger transient deformation. Please explain the basis for selecting 136 GPa, clarify its role as an effective rheological parameter, and acknowledge that the inferred Kelvin viscosity is conditional on this prescribed value.

We agree that our use of the term "enhance the transient response" was imprecise. For a given Kelvin viscosity, increasing the Kelvin rigidity reduces the amplitude of the recoverable

Kelvin strain while shortening its characteristic retardation time. Our intention was to refer to the timescale of the transient response, rather than to its amplitude (it will be faster/shorter).

Few minor inconsistencies:

- Replace “pleads for a weak zone” in the abstract with “supports/suggests the presence of a weak zone.”

done

- Clarify the normalization of the afterslip time function in Section 4.2 so that it is consistent with the seven-month amplitude defined in Section 4.3.

This has now been modified.

- Reconcile the descriptions of InSAR profile extraction: Section 3.1 refers to a mean over 125 km on each side of the profile, whereas Section 4.4 refers to medians across a 100 km- wide section.

Thanks for finding this inconsistency. We compute the median of the InSAR signal across a 100 km-wide section. This has now been changed in section 3.1