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.

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

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

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.

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.

Few minor inconsistencies:

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