Response to reviewer's comments

Reviewer A:

This is a fascinating paper and my general opinion is that it should be published with very minimal changes. Among other things, the authors provide an important service to the community in summarizing the relative advantages (e.g., computation time and opportunities to make important corrects to data) of travel time tomography with this approach and in comparing results to FWI (sensitivity to discontinuity depths, etc.). I do, however, have a few clarifying questions—mostly about the methodology and choices made during the process.

[1] I find the sampling surrogate shown in Fig. 3 to be interesting, but I don't understand exactly how it's used in the inversion. Is there an effort made to reduce the effects of high column density so that relatively poorly sampled regions will not be damped to zero perturbation? This seems like it might be happening... given the washed out colors in areas that should be covered by the MERMAID and OBS instruments.

We did describe the weighting of the damping part of regularization in the paragraph that contains equation (3). To clarify our method further we add : "We only scale the damping parameter and apply no explicit column scaling".

[2] One question I had while reading the manuscript is how critical each of the new and somewhat exotic datasets (e.g., OBS, MERMAID) is to the results. The upshot of Fig. 7 (and one takeaway from Fig. 3) is that including those smaller datasets does not produce significant differences compared to previous models that use datasets like EHB. Oceanic regions and the southern hemisphere in general are poorly recovered in the upper mantle, for example. If there is no effort to minimize the effects of repeated paths (using column density or anything else), could the inversion procedure be modified to allow the smaller datasets to contribute more to the final model?

The resolution test show a good upper mantle resolution east of Tonga-Fiji, obviously caused by MERMAIDs filling the typical ocean data gap. We inserted a sentence stating this. We modified the discussion at the end of section 3.3 to explain that we gave the MERMAID data an extra weight in the inversion to compensate for their scarcity (4954 picks compete with 451,484 picks from EHB in the same area!), cite Takeuchi and Kobayashi (2004), and refer to the section with resolution tests to show that this has no dramatic effects of errors being blown up. As stated in response to [1], we apply a scaling to the damping to reduce the effects of variable ray coverage, though of course there is no magic panacea for lack of data and the effects of such scaling are minor.

In one of the follow-up papers where we compare the South Pacific images with modelling of the superplume, we do present more detailed resolution tests for that area with and without the contribution of the MERMAID data. Our m/s already referred to this ("...near Polynesia, where the MERMAID data provide much needed constraints, improving resolving power certainly in the upper half of the mantle (Chen et al., in prep)."). We do not wish do duplicate results among various papers and prefer to leave localized tests to the papers dedicated to that region.

[3] It would be helpful to have a summary of errors in each of the data summary sections 2.x. Statements such as the one on line 114, i.e., that you assigned errors of 0.7 s to JAMSTEC OBS data, would be much more meaningful if the other sections also summarized the errors that were ultimately assigned to the other data. This is especially important given the comparison of standard errors to the chi-square estimates that emerge from the inversion, which is discussed later in the manuscript.

The reviewer perhaps missed the fact that average standard errors were summarized in the last column of Table 1. For the Jamstec OBS data, no information on standard errors was available. We added a sentence saying the 0.7s error is based on our own experience in picking OBS and island station data.

[4] Line 242: a) Why set the ratio of damping/smooth coefficients to 2 and b) why keep it fixed?

As we mentioned in the paper, higher $\epsilon_{smooth}/\epsilon_{damp}$ ratios lead to too much smoothing of well resolved subduction zones. We added a sentence that lower ratios reduce the smoothing such that artefacts become dominant. The optimal damping was found by keeping the ratio fixed and doing a line search.

[5] I can't tell if rays are retraced between iterations of the inversion. If so, please make this more explicit. If not, why not? Perhaps include a discussion of the advantages and drawbacks of repeated raytracing.

No we did not. Ray-tracing through a 3D model is a whole different, and computationally very expensive, ballgame (I am not sure there is even a reliable tracer for global models) and not really necessary. We already cited Bijwaard and Spakman to justify our linearization, but now mention explicitly that we are not redoing the raytracing at the start of section 3 because it would add a very large, and unwarranted, computational burden.

[6] The approach is, of course, beyond reproach and the writing is clear and logical. I appreciate the effort on the part of the authors to assess the utility and accuracy of this technique, which has its roots in early developments of seismic tomography...many of them due to Nolet himself. All the figures are essential and well-made, although I would love to see a few more cross-sections, which I find to be more helpful in addressing questions related to dynamics.

We are currently preparing two papers, one on the Pacific Superswell, one on the Tonga-Fiji subduction, that will show more cross sections. We added that readers can easily create their own cross-sections using Submachine (Hosseini et al., 2018) because the model has now been implemented on that site.

Reviewer B:

[7] To first order, the most notable aspect of the model is the unusually high amplitudes (mostly negative) in the mid-south Pacific proximal to where the new OBS data is located relative to the rest of the model (e.g. Figure 5). These high amplitudes persist from the surface down to at least 2033 km depth. While I understand that interpretation of the structure in this region is intentionally reserved for future publications, it is important to describe and explain the source of this artefact in the present paper – particularly since this is the target study area for the interpretive papers. What in your inversion is causing this? This will also presumably bias many of the statistics described elsewhere in the manuscript (ex. the extreme anomaly values in Figure 6c, among others) – a consideration of whether or not statistics throughout are representative of the model as a whole or skewed by this region would be appropriate.

We do not agree that the higher amplitudes are 'an artefact'. As the resolution tests show, the data do a good job of reproducing lower mantle anomalies even in most oceanic areas, whereas the 500 km resolution test shows limited – but not zero – resolution in the Pacific upper mantle where the solution shows anomalies in excess of 1000 km. Though uncertainties in standard errors allow one to deviate from the ideal $\chi^2 = 1$ and modify the amplitudes, that deviation could be positive or negative and our choice ($\chi^2 = 0.98$) seeks middle ground. The differences with the FWI models are less dramatic than with older delay-time models, certainly if one considers that the FWI inversions smooth the anomalies more than we did, so we suspect it is the contribution of the volume-sensitive finite frequency data that allows for a better resolution – and thus less damping – of the oceanic mantle. We have added two figures (S23 and S24) to show the differences with UU-P07, MITP08 and DETOX-P3. We added a full paragraph at the start of section 5 to answer the reviewer's concern.

Minor comments:

[8] Where does the name UNICA25 come from? Is it an acronym?

Université Côte d'Azur 2025 – like many other models a mix of the institution and the year of birth. We added this information to section 3.4.

[9] I would think that a detailed comparison of UNICA25 with the other major P wave models (e.g. UUP07, MITP08, SPiRaL and DETOX) would be more apt than comparison to the FWI models in Section 5, given the distinct methodologies. Not saying this is necessary, but I do think the manuscript would be improved and provide a more useful tool for potential users of the new model if such a description and comparison was applied to other P tomography models (either instead or in addition).

See also our response to [6]. But we disagree with the reviewer that models such as UUP07 are more apt for comparison. The many finite frequency data in our inversion have the same volume sensitivity as those of the full waveform models using cross-correlation delays, whereas the pure travel time inversions only have a line sensivity. However, we added Figures S23 and S24 to compare with UUP-07, MITP08 and DETOX-P3 and mention that UNICA25 is now implemented in Submachine where readers can make their own comparisons with a large variety of models.

[11] Please include a map showing where the new datasets (station networks) are located in context of the broader dataset.

In keeping with our policy not to repeat information across multiple papers unless absolutely necessary, we now explicitly refer to Figs 1 and 3 in Nolet et al. (2025) in section 2.4 but do not reproduce these in the present paper.

[12] Line 114: Where did 0.7 come from?

See response to [3]

[13] Line 128: How do you account for bias due to picking methodology when "adding in" ISC picks to the new data for event corrections?

We are not aware of a known bias, even less able to account for it.

[14] "ERC" acronym is not defined.

Done (European Research Council)

[15] Line 145-148: Can you show an example of this process in the supplement?

Yes, we added Fig. S22; we also corrected a mistake in the description of the Pdiff data set. The linear dispersion fits were sampled at 0 sec period, not 5.

[16] Line 235: Were you able to demonstrate this "overshadowing"? I worry that you overcompensated (see major comment above).

See response to [2].

[17] Line 243: What was the criteria for determining your "preferred" solution?

As we stated in the paper, we wanted to reach a data fit close to 1; and balance smoothing and norm damping such that subduction zones are sharp and artefacts avoided.

[18] Figure 4: Why not include a scale?

The only scale that is important is that of the data fit, which is plotted on the vertical axis. The regularization and the model norm have different scales, (χ^2_{tot} is even a somewhat weird quantity that mixes different physical parameters). All we needed to show here is that they converge to 'some' final numerical value, much later than the convergence of the data fit.

[19] Line 290: This agreement with DETOX does not seem justified to me, especially since DETOX and UUP07 don't agree with each other in the transition zone.

Thanks for spotting this error. It has been replaced with "UNICA25 agrees with DETOX and PRI-P05 that the transition zone has low velocities in the central part of the section."

[20] Typos in lines 66, 217, 274 and 356

Done – in addition to a few other typos and minor language improvements.