Solid Earth
Discussions

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Interactive comment on "An objective rationale for the choice of regularisation parameter with application to global multiple-frequency *S*-wave tomography" *by* C. Zaroli et al.

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This is a very interesting paper that treats one of the fundamental issues in seismic tomography; pursuing an objective way for the determination of reasonable damping parameter (and/or its range). The authors' attempt for finding a reasonable model in the case of multiple-frequency shear wave tomography is intriguing, and the similar idea can be useful for other types of tomographic problems working with multiple frequency models that are expected to have some correlations among them; e.g., surface wave tomography based on multiple-frequency phase/group speed models.

This paper is well written and I am convinced of the way that the authors have proposed

for the determination of the exclusive range of both over-damped and under-damped models (or the range for models with "poor data exploitation" and that "dominated by noise"). The final determination for the best compromised model from the limited range of the trade-off curve seem to be somewhat subjective, as the authors have mentioned, so that the method proposed here may better be represented as "quasi-objective". Though it may not be fully objective since the final model is selected with an arbitrary criterion based on the correlations of models derived from slightly different damping (over 98 % of correlation is achieved within the optimal range of tradeoff curve, in this case), such slight subjectivity is unavoidable in inverse problems and the authors' criteria seems to be practically useful for the automatic determination of the final model. I have only a few minor comments as summarized below, and I recommend the paper be published in Solid Earth with some minor corrections.

1. Page 852-854, Section 3.2:

I understand the reason why the authors have chosen 34 s as the reference period (as explained in P854, L8-16). Still, it will be worth adding statements on how the results could be affected if, for example, they choose 22 s as the reference, or use d_15 rather than d_10 in Fig 3. It should be briefly explained in the main text or in an appendix, or in a form of electronic supplement. (It seems to me that the differences in the wavelength of different period, e.g., 22 and 34 s can affect the scale-length of resolvable heterogeneity, which may have some influences on both axes of the tradeoff curve in Fig 3.)

2. Page 857 line 20-29, explanations for Fig7:

Explanations and interpretation for model differences (Fig.7) in section 4 are too brief. I think PRI-S05 is derived from the same finite-frequency (FF) technique as used in this paper, but the other two (S40RTS and TX2007) are based on ray theory (RT). I believe that this is one of the reasons why FF models (this study and PRI-S05) in Fig 7 tend to show apparently larger velocity perturbations than RT models (S40RTS, TX2007),

since the effects of diffractive healing on travel times (which tend to underestimate the velocity perturbations in the framework of ray theory) are naturally taken into account through the banana-doughnut kernels used in the FF models. Such additional explanations are likely to be useful for readers, since the differences in velocity perturbations among these models can also be caused by the subjective choice of damping.

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