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Solid Earth
Discussions

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.

K. Liu (Editor)

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This is a very interesting manuscript by C. Zaroli et al. to discuss the choice of a priori damping parameters in the application of global multiple-frequency S-wave tomography. Both reviewers have provided their comments and suggestions. The authors need to clarify and make better explanation of the optimal range of damping parameters in section 3.1 and 3.2 as concerned by the first reviewer, which would be one of the most important focus in the revision. In addition, the authors need to address those questions and concerned raised by the second reviewer. Overall, it is a very interesting paper recommended to be published in Solid Earth after the authors address the

detailed comments and questions below from these two reviewers .

Reviewer #1: In this paper, the authors present an objective rationale for the choice of a priori damping parameter used in global multiple-frequency tomography. I understand the individual argument to get the optimal range of damping in sections 3.1 and 3.2. However it sounds complicated for me. I do not know why they did not adopt the simple cross validation rather than their criteria. I mean testing how well a dataset sampled randomly from all dataset is ïňĄtted by the model obtained from the remainder of the dataset. If there is any reason to avoid such simple method I would like to hear it.

And I suppose that the damping value close to their preferred one may be given by the misïňĄt function of a single-band data subset with respect to multi-band model that obtained from all dataset except the single-band data subset. For instance, in case of 10 s data misïňĄt, the model is constructed from the data except 10 s data and the damping value will be given by reversal point in the trade-off curve between jjMB15;22;34;51jj22 and 2 red(MB15;22;34;51; d10).(hard to display here, see the reviewer's original comments) If that is the case, I think they do not need the criterion in section 3.3 that I feel subjective as the authors also mention. And if the damping values obtained above strongly depend on what period data are used for misïňĄt calculation, it may reïňĆect the noise level of each period.

I do not have a problem with their statement about their tomographic result, because this paper does not address to interpretation of the obtained detail structure.

Reviewer #2: 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 ïňĄnding 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 ïňĄnal 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 ïňĄnal 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 ïňĄnal 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 brieïňĆy 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 inïňĆuences 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 (FFig.7) in section 4 are too brief. I think PRI-S05 is derived from the same ïňĄnite-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 pertur-

bations 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.

Interactive comment on Solid Earth Discuss., 5, 841, 2013.