

Interactive comment on “Crustal structures beneath the Eastern and Southern Alps from ambient noise tomography” by Ehsan Qorbani et al.

Andreas Fichtner (Referee)

andreas.fichtner@erdw.ethz.ch

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Dear editor, Dear authors,

First of all, I need to apologise for this very late review! The past few weeks have been rather chaotic, and the transition to online teaching has consumed a lot of time.

This manuscript presents an ambient-noise tomography study of the Eastern and Southern Alps, largely using data that were collected specifically to study Alpine structure. Following a summary of Alpine geology and previous tomographic studies in that region, the authors provide details on data processing, dispersion measurements,

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and the two-stage inversion procedure (via group-velocity maps to a 3-D model). The manuscript ends with a detailed discussion of the results in the context of the regional geology.

This contribution is really a pleasure to read because the topic is interesting, and because the work is described carefully without being overly verbose. Not being an expert in geology, I will mostly comment on technical issues. In addition to the points raised below, you can find more suggestions and questions in the annotated manuscript.

Major comments

1) Resolution: My major concern is the resolution analysis. In section 4.1, the authors claim, for instance, that resolution length at 16 s is as low as 8 km. Obviously, this is physically impossible. At 16 s, surface wave velocity is around 3 km/s. Therefore, the wavelength is certainly larger than 50 km. It follows that resolution in this transmission tomography can be at most 50 km at 16 s period.

The problem here seems to be that the authors forget the limitations of ray theory. By virtue of the central-slice theorem, ray theory can give infinite resolution, regardless of the frequency content of the waves. In other words, this apparently good resolution is really just an artefact of the ray approximation.

Another problem is that resolution length is a quantity that has a direction. Resolution in one direction is generally different from resolution in another one. So, which direction do you consider here?

2) Details of the inversion: Some technical details of the inversion procedure could be described better. Especially in the first paragraph of section 4, the authors introduce various parameters that seemly control the regularisation of the inverse problem. Without showing an equation, it is difficult to understand what exactly these parameters are, and how their specific values have been determined.

Minor comments

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3) English: The English of the manuscript is good, but can still be improved. For instance, many plural s's are missing. So, I would suggest that a native speaker carefully reads the text.

4) Figures: Some of the figures could be improved. Often, the labels are too small and resolution is a bit low.

5) Others: Please find more smaller comments and questions in the annotated manuscript.

In summary, I think that the resolution analysis needs a little bit more attention. In case the numbers change, the interpretation may need to be adjusted. All in all, this should not require more than a minor revision.

With kind regards

Andreas Fichtner

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2019-177/se-2019-177-RC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-177>, 2020.

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