Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-17-RC1, 2019

Solid Earth
Discussions
Open Access

Interactive comment on "Slab Break-offs in the Alpine Subduction Zone" *by* Emanuel D. Kästle et al.

Marco Giovanni Malusa' (Referee)

marco.malusa@unimib.it

Received and published: 8 March 2019

GENERAL COMMENTS

Kästle et al. compare their surface-wave tomography model published in 2018 with previously published teleseismic P-wave tomography models. Different techniques have different pros and cons. For example, body waves tend to smear structure vertically, and surface waves do that horizontally, due to the different way these waves propagate. A sensible approach would be integrating the most reliable information provided by surface-wave tomography models with those provided by teleseismic P-wave tomography models, in order to delineate a common geodynamic scenario that best fits both tomographic images and geologic constraints. Although this is the target stated in

Printer-friendly version

Discussion paper

the abstract, I am afraid it is not attained by the manuscript in its present form.

1) In order to integrate the strengths of the different methods, the authors should first discuss the weakness of each method and model. This is quickly discussed for teleseismic P-wave tomography models, but it is not satisfactorily discussed for surface-wave tomography. Quoted from the manuscript (section 1): "At present, the high-resolution regional tomographic models of the Alpine upper mantle disagree on important structures, such as the suggested present-day detachment of the European slab under the western Alps and the subduction polarity under the eastern Alps (Adriatic vs. European subduction) (Lippitsch et al., 2003; Koulakov et al., 2009; Dando et al., 2011; Mitterbauer et al., 2011; Zhao et al., 2016; Hua et al., 2017). An important limiting factor in the discussion on the structure of the Alpine slabs is the poor resolution in the uppermost mantle layer of teleseismic body-wave models, due to almost vertical ray paths and lower data coverage (e.g., Boschi et al., 2010). In the light of a new model which does not suffer from this limitation (Kästle et al., 2018), we discuss the Alpine slab geometries and their possible detachments. The latter model is reliable down to a depth of approximately 200 km with teleseismic body-wave models for the deeper parts of the sections".

At a first glance, the surface-wave tomography model by Kästle et al. (2018) may suffer of major limitations as well. For example, in the map of Fig. 1 there is a striking correspondence between the highest velocity region (in blue) and the political borders of Switzerland. Is it just by chance or there is some sort of sampling bias? I am not an expert of surface-wave tomography (most of the potential readers of this paper are not), but this is an evident feature that should be discussed carefully in the text. Another evident weakness is that no evident slab structure is shown in the cross sections of Fig. 1. This means that the mantle structure at depth >100 km is necessarily based on teleseismic P-wave tomography only, and that the linkage between surface-wave and body-wave tomography models is not straightforward.

2) Kästle et al. underline in the manuscript that the high-resolution regional tomo-

graphic models of the Alpine upper mantle disagree on important structures. I fully agree. But they do not consider that the quality of the tomography models is improved through time for a number of reasons. For example, the tomography model by Lippitsch et al. (2003) is based on ∼200 stations and 4199 relative P wave travel time readings, whereas the tomography model by Zhao et al. (2016), which first benefited from the opening of the European seismic databases, is based on more than 500 broadband seismic stations and 41,838 relative traveltime residuals. This likely makes a difference. In the revised manuscript, this information should be explicitly included in Fig. 2, and this issue should be discussed in detail in the main text and for the different tomographic models.

3) I have concerns about the selection of tomography models in Fig. 2. Quoted from the manuscript: "The early model of Lippitsch et al. (2003) was selected because it has been and still is the reference (e.g., Handy et al., 2015); the model of Koulakov et al. (2009), because it covers much of Europe, allowing interpretations that go beyond the Alpine region; the model of Hua et al. (2017) is the most recent one which covers the entire Alps and also includes local-earthquake data." I understand that the Lippitsch et al. (2003)'s model is probably the choice for most geologists working north of the Alps (276 quotations in Google Scholar), but it is not the choice for many geologists working south of the Alps, who more often refer to the model by Piromallo and Morelli (2003) (635 quotations in Google Scholar), and now to the model by Zhao et al. (2016) that better fits the geology of the Alpine region (30 quotations in Google Scholar). The Zhao et al. (2016)'s model should be included in Fig. 2 for the following reasons: (1) it is the first model that benefited from the opening of the European seismic databases; (ii) it is the first model explicitly contradicting the view proposed by Lippitsch et alii. I also recommend using the same color scale for the different teleseismic tomography models in Fig. 2 (different color scales may either emphasise or mask the inferred velocity gaps)

4) One of the main conclusions of the paper is a shallow slab breakoff (ca 100 km

Printer-friendly version

Discussion paper

depth) beneath the Western Alps at <10 Ma. From a mechanical point of view, if subduction in the Western Alps ended somewhere between 35 and 30 Ma, why the inferred breakoff took place more than 20 Myr later? And why breakoff occurred in correspondence with normal thickness European crust? This should be discussed carefully in the main text.

5) Again concerning shallow slab breakoff in the Western Alps at <10 Ma. As illustrated by Kästle et al., geophysical evidence of slab breakoff in the Western Alps is highly questionable. However, this issue could be examined from a different perspective, that is: the slab breakoff theory was first proposed by Davies and von Blanckenburg (1995) to explain Periadriatic magmatism and exhumation of high pressure metamorphic rocks in the Alps (see review in Garzanti et al., 2018). Is there any magmatism in the Western Alps at <10 Ma? Is there any exhumation of high pressure metamorphic rocks in the Western Alps at <10 Ma? The answer is: No, there isn't.

In summary, I am not fully convinced that the manuscript may provide, in its present form, a useful contribution to the ongoing debate on the structure of the Alpine upper mantle. However, I would be happy to see a fully revised version of this manuscript including a thorough discussion of the issues listed above, and a better integration between geophysical constraints and available geologic data, especially for the southern side of the Alps.

Best regards,

Marco G. Malusà

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-17, 2019.