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Solid Earth Discussions

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

Emanuel D. Kästle et al.

emanuel.kaestle@fu-berlin.de

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Dear Editors, Dear Dr. Marco G. Malusà,

thank you for your comments on our manuscript. We have carefully read them and included them in a point-by-point response that you can find below. The original comments are given in **bold** letters. Attached to this response you find the manuscript with all changes highlighted.

Kästle et al. compare their surface-wave tomography model published in 2018 with pre- viously 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

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different way these waves propa- gate. A sensible approach would be integrating the most reliable information provided by surface-wave tomography models with those provided by teleseismic P-wave to- mography 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 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 tele- seismic 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 impor- tant 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 fac- tor 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.

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You are right saying that a critical discussion of the weaknesses of the surface-wave tomographic method is important, especially to readers that are not familiar with this technique. We therefore included a paragraph which shortly explains how the wavelength of surface waves and the station distribution influence the resulting model. We also added a new section which provides some remarks about the general comparability of different tomographic models. Nevertheless, we decided to keep the explanations short, as a more extensive discussion, including resolution tests, is given in the original publication that is quoted in the text (Kästle et al., 2018) and the aim of the present manuscript is more to discuss alternative hypotheses of the upper mantle structures and their geodynamic implications.

We think that the coincidence of the high-velocity anomaly and the Swiss borders is not that curious given that there is also a rather good correlation between the Swiss borders and the central Alpine root and roughly also the expected location of the slab. However, there are two more quantitative reasons derived from our model for which we think it is not just a bias: (a) there is no such effect observed at shorter periods/shallower depth, where the shapes of the anomalies perfectly match the expected pattern of the foreland basins and the deep crustal root under the Alps (Kästle et al., 2018, Fig. 4); (b) a high velocity anomaly that roughly matches the shape of Switzerland is observed at periods >50s (= uppermost mantle depth) in both the earthquake and ambient-noise datasets independently (both datasets are evaluating surface-waves, but from different sources with different methodologies). Nevertheless, we cannot exclude that there may still be a bias, but based on the arguments above, we think it is rather small.

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.

We do not fully understand the second point, stating that there is no evident slab struc-

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ture in the cross sections of Fig. 1. We think that in cross-section B the slabs are very well visible as high-velocity anomalies. But it is indeed true that imaging slabs with surface waves is a rather new approach which means that the comparison with body wave images is not that straightforward and may be something that needs getting used to; other than body wave images of slabs that exist since decades. This is certainly also related to the decreasing resolution of surface waves with depth that we now discuss in the revised manuscript and where we also explain that this causes the apparent slab dip to be less reliable compared to body waves.

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 âĹij200 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 differ- ence. 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.

It is correct that the amount of data has tremendously increased between the earliest and recent models. However, it is the resolution of the models that is the most relevant parameter to judge the model quality and at the same time the most difficult to estimate quantitatively. It is clear that a small dataset of very carefully selected and processed data can result in a more robust model with higher resolution than a very large dataset with only loose quality control. We also want to stress that uncertainties in tomographic models cannot be simply reduced to error bars (see e.g. Boschi and Dzienwonski, 1999, JGR). Nevertheless, we agree that it may be helpful for the reader to have this

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information. Therefore, we added a table that summarizes some of the key model parameters such as number of data, number of stations, grid spacing, etc.

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.

Our intention is to show the spectrum of different models and at the same time not to overload the Figure so it remains easily readable for the reader. We therefore selected three of the seven models that are available to us (following your suggestion we added the one of Piromallo and Morelli (2003)). We agree that there is indeed a lot of potential for discussion about which model might be the most suitable, depending on which aspect one wants to focus. The Hua model is unique in its approach to image both crust, mantle and their anisotropies at the same time collecting a huge dataset. Nevertheless, they produce a rather smooth model which is not easy to interpret and they largely follow the interpretations of Zhao et al. (2016). As pointed out in the comment above, the Zhao model sets itself apart with the novelty of the interpretations and with

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being the first to combine such a dataset for the Alpine region. We thus follow your suggestion and replaced the model in Figure 2.

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)

An identical color scale would overemphasize or mask some anomalies and whatever choice we make it would always "punish" either the models with a high or the ones with a low range of values. We are illustrating this in the figure attached to this response. The differences in the ranges of values may be due to the authors' choices made during the inversion and not necessarily related to the physical properties of the materials. This would rather distract from our purpose of comparing geometries and structures. This aspect is also mentioned in the revised manuscript.

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

We changed the text, mentioning on the one hand that a delay between the onset of continental collision and break off is to be expected (van Hunen and Allen, 2011, EPSL) and on the other hand the possible occurrence of a weak zone created by the push from asthenospheric flow from underneath Adria towards the west and thermal erosion of the slab.

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

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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.

The western Alps are affected by significant uplift and exhumation. Whether HP rocks are also being exhumed at the moment cannot be assessed! Different researchers propose that a mantle source likely contributes to this process (Fox et al., 2015, Sternai et al., 2019). Geological processes accompanying slab-break off are complex and not sufficiently understood yet in order to prove or disprove slab break-off simply by looking at its surface expression. Despite the ambiguities, we think there are good tomographic indications for slab break-off from different methods. In order to integrate the comments above we modified the MS by discussing the proposed break-off and its timing more openly, and clearly avoiding to state that a <10 Ma break-off is the only solution to the western Alpine situation.

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 be- tween geophysical constraints and available geologic data, especially for the southern side of the Alps.

Best regards, Marco G. Malusà

Best regards,

Emanuel Kästle

Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2019-17/se-2019-17-AC1-supplement.pdf

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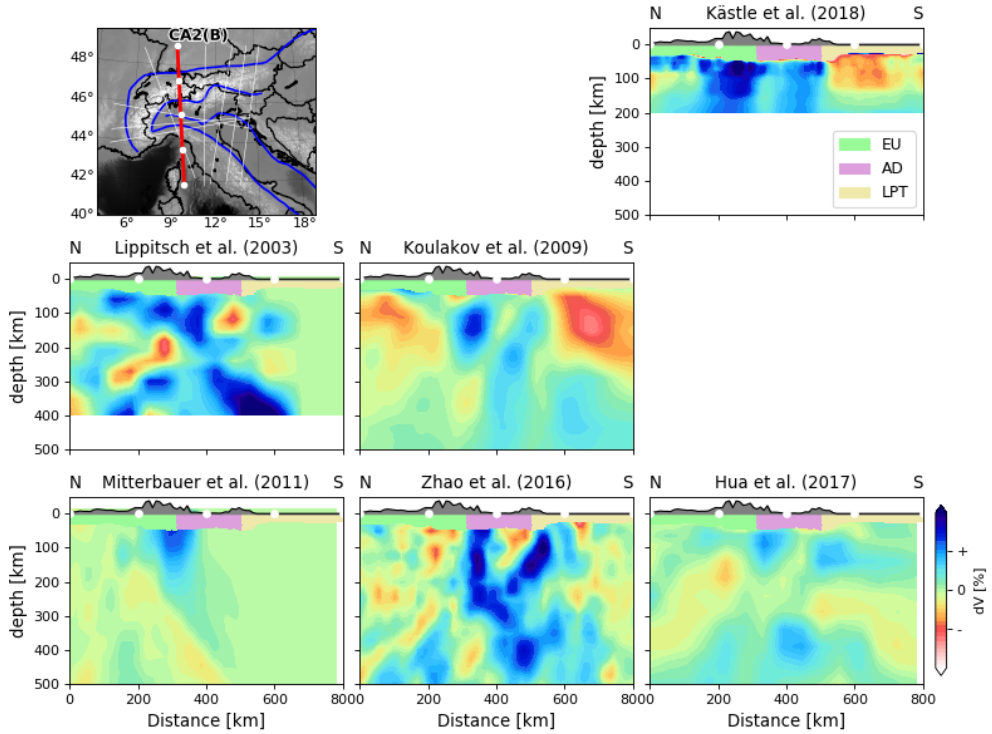

Fig. 1.

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