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

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GENERAL COMMENTS

The manuscript by Kästle et al. has the unquestionable merit of presenting a set of tomographic models from the literature (included a recent one by the main Author) using the same projection and color scale, showing map views at the same depth and cross-sections along the same profiles. This has not been done before at the Alpine scale, as the Authors correctly claim, and it is a first, necessary, step towards a critical review of models and comparison of their results, potentially useful in view of future interpretations/discussions. The following step would be addressing the resolution of any specific tomographic model (be it derived from surface or body waves), which is

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Discussion paper

dependent on the dataset and technique used. This is necessary to assess the robustness of imaged features and avoid over-interpretations. The possibility that smearing and artifacts affect the various models is generally mentioned in the manuscript, but in my view this simple warning is not enough to prevent the many potential readers non-expert in seismic tomography from a subjective judgment in choosing the preferred image among many options, interpreting details that could go beyond the resolution of the model. Not all the high and low velocity anomalies visible in a tomographic image are reliable features, and the amplitude of an anomaly is often less well constrained than its shape. Images may even look different meaning the same structure. In my opinion, having such a large set of images from different models plotted side by side, with no additional information, could easily lead to biased interpretations. More biased than if the reader went through the original papers for each model (provided that detailed information on model errors and resolution are given in those articles). This is my major concern.

The resolution and reliability of any specific tomographic image is strongly dependent on the quality (and not just the quantity) of the dataset. A noisy dataset can hardly be compensated by a simple increase in the data quantity (e.g. Diehl et al., GJI, 2009). Moreover, the starting model and type of parameterization used, the inclusion (or not) of crustal corrections, the station distribution and the ray coverage of the study volume (to what extent are ray-paths crossing), the inversion technique as well as the damping and smoothing used to stabilize the inversion all affect the resulting tomographic models. The resolution issue for each of these models, which is usually comprehensively discussed in the reference papers (and their supplements), is hence crucial for interpretations and should not be disregarded, but used instead to critically assess the geometry and amplitude of structures that can be reliably resolved.

I acknowledge that a comparative assessment of the resolution of the various models considered here is not feasible and that, even if it was possible, it would still remain a qualitative estimate. Nevertheless, my recommendation is adding a section (and perhaps also a table in order to recap all the major info) in the main text of the manuscript, making an effort to summarize the main characteristics of each model considered for comparison and clearly addressing the resolution issue. I would also ask the Authors to consider: 1) plotting on the images the parameterization grid used in each model, at least on the map views (if not possible, then specify in the text and/or table what is the grid spacing adopted by the different models); 2) adding the 100 km and 200 km depth layers in the supplement (for completeness and to better allow the reader following the lateral and vertical continuity of features). In the light of this section similarities or disagreements among the models would be more properly addressed, especially in those cases showing completely anti-correlated images, and the readers would be encouraged towards a critical assessment of tomographic models.

Another suggestion, for the Discussion Section, is devising a table in which summarizing the results from each cited model with respect to the major anomalies discussed in the manuscript. For example: one row for each different model (Kastle, Lippitsch, etc.) and one column for each discussed anomaly (WA slab length, WA break off depth, WA slab origin –European/Adriatic, CA slab length, etc.). I would also mark the cases in which the information is retrieved from the original papers and those in which the Authors of this manuscript possibly propose a different interpretation.

I recommend as well referring more explicitly to the many available figures (of the main text and Supplement) in the Discussion section, when presenting the details of different scenarios. This bunch of figures seems underexploited in this part of the mansucript.

SPECIFIC COMMENTS

P. 2 Line 13: “We apply this approach”. Not clear what kind of approach.

P. 2 Line 29-34: The Authors first admit that the weaker intensity may be either due to lower resolution (artifact) or to actual structure, and then they attribute different anomaly amplitudes to different evolution of the three domains. What is the reason for discarding the hypothesis that it is a matter of lower resolution and retaining the one that it is actual structure? (see also P.4 Line 5, where again amplitude of the high velocity anomaly is addressed). Moreover, the sentence “because the station distribution is not perfect” is not very informative.

Figure 1: Please, indicate: - on map views labels corresponding to mantle anomalies (consider adding the 150 km depth layer if needed), - on cross-sections the interpreted break offs.

P. 4 Line 16: Also the model by Koulakov et al. (2009) includes local-regional data.

Figure S1: - The map view at 150 km of the model by Lippitsch et al. (2003) is different from the one reported in Figure 2. See in particular the different amplitudes of the anomalies north of the Alps in the two figures. - The image of the map view at 150 km by Koulakov et al. (2009) is missing. - Some models show totally anti-correlated images at 150 km depth, like for example the model by Kastle et al. (2018) and the one by Dando et al. (2011). These aspects require a comment in the text.

P. 6 Line 5-6: I would use labels on the figures to indicate anomalies discussed in the text (as done in Fig. 1) because in some cases, as in this sentence for example, it is not clear which anomalies are referred to.

P. 6 Line 10-13: In Figure S10 I see a connection between the high velocity anomaly below the Dinarides and the one below the eastern Alps not only in Lippitsch and Dando models, but also in Koulakov and, possibly, in Zhao models. Why do the Authors exclude such a link in these models? The model by Hua et al. (2017) definitely shows a totally opposed image. The Authors should comment on this.

P. 11 Lines 18-19: “The fast anomaly under the German Molasse, at around 150 km depth in the surface-wave model (Fig. 1C) and the one of Lippitsch et al. (2003) (Fig. 2)” Not clear which anomaly is referred to. Is it EANA in Fig. 1C? I do not see it, though, in the Lippitsch model.

P. 11 Line 33: “In order to understand detachments at lithospheric level, good resolution in the uppermost 200 km is required." I only partially agree with this sentence. In fact, in order to understand detachments the Authors need to refer to models showing structure at larger depths.

Nomenclature: Carpathians, Tauern window and German Molasse are not indicated in the figures.

With best regards

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