Reviewer #3 (Manel Prada)

1 General comments

• The manuscript "Oligocene-Miocene extension led to mantle exhumation in the central Ligurian Basin, Western Alpine Domain" by A. Dannowski and co-authors present new constraints on the petrological nature of the basement in the Ligurian Basin from new wide-angle seismic data and travel-time tomography. The authors show that rather than oceanic crust, as previously interpreted in the area, the northwestern region of the basin experienced crustal thinning and later mantle exhumation. However, I found the occurrence of mantle exhumation difficult to reconcile with the velocity structure of the uppermost mantle presented here. Considering that the mantle is fully exposed to the seawater during the opening of the basins, I found strange that the top of the mantle does not show the typical velocity gradient of exhumed mantle regions, in which Vp increases progressively from ~4.6 km/s (100 % alteration) to 7.8-8.0 km/s (no alteration) (Minshull 2009; https://doi.org/10.1016/j.crte.2008.09.003; Prada et al., 2015 doi: 10.1093/gji/ggv271). While I agree that there is no oceanic crust, the lack of an exhumed mantle-like Vp vertical gradient implies that the mantle was not fully hydrated and thus, exhumed, possibly because of the presence of syn-rift sediments or the existence of hyperextended continental crust. In fact, this interpretation fits nicely with the model. Lower crustal velocities are > 5.5 km/s, which may well be indicative of tilted fault blocks and rotated syn-rift sediments (e.g. Bayracki et al., 2016, Nature Geoscience, DOI: 10.1038/NGEO2671). The top of the continental basement in these settings can be really rough, and thus difficult to identify in OBS data. The fact that you don't see it, doesn't mean it's not there. In addition, mantle Vp is close to 8 km/s in some regions (e.g. beneath OBS205), while it decreases in others to < 7.5 km/s. This pattern resembles the mantle structure underlying continental tilted blocks reported in other rifted margins such as Galicia (Bayracki et al., 2016) and the Porcupine Basin (Prada et al., 2017 EPSL; http://dx.doi.org/10.1016/j.epsl.2017.06.040). Such pattern is attributed to the fault-controlled water influx to the mantle during rifting (Bayracki et al., 2016). In light of these observations, I advise the authors to reconsider their interpretation. Apart from this aspect, I also found some issues during the modelling and in section 5.4 that, if tackled, can help to improve the robustness of the final model, and thus, strengthen the paper. I discuss them the bellow.

We agree that the concept of mantle exhumation poses some interesting aspects and thank the reviewer for pointing these out. Indeed our data may not provide the information if sediments are underlain by thinned continental crust or exhumed mantle. We now discuss both scenarios. Exhumed mantle does not necessarily imply to be exposed directly on the seafloor as we mentioned in lines 270-275. We now additionally clarify this in the discussion by adding extra paragraphs in sections 5.1 and 5.2. We re-phrased the manuscript at several places to open up the discussion about the continental material (thinned continental crust or exhumed subcontinental mantle) underlying Ligurian Basin.

In contrast to the Tyrrhenian Sea we observe a strong in amplitude PmP reflection, which indicates a high velocity contrast at the crust-mantle boundary. The nature of the velocities >5.5 km/s can be debated and we cannot distinguish between fast sediments and left over rotated continental crust blocks (rotated the orthogonal direction to the profile). Sediments have to play a role during rifting and mantle serpentinisation (Ruepke et al., 2013), else we would observe a much higher rate of mantle serpentinisation, we agree, also in areas with remnant blocks of continental crust.

We changed the manuscript title to: *"Seismic evidence for failed rifting in the Ligurian Basin, Western Alpine Domain"*

Regardless of these issues, the paper unequivocally demonstrates that there is no oceanic crust in this region of the Ligurian Basin, and that is of great relevance for the community working on the

Mediterranean region. This study fits nicely with the goals of Solid Earth, and thus, I strongly encourage the authors to tackle all these aspects and resubmit the manuscript for its publication.
Best regards, Manel Prada
Major issues:

• The authors use forward modelling, I presume, to explore the lateral consistency of the seismic phases observed in each receiver. Then, they use this preliminary model as input for the tomography. However, it is confusing the way the authors describe and apply the layer stripping strategy. The authors say, "In a first step only near offset picks with distances smaller than 15 km were inverted." This is rather confusing. It seems that the authors have inverted the travel times within 15 km of offset from each receiver, independently of the seismic phase they correspond to. It would be better to explicitly mention the type of seismic phases that the authors have included in the first step, which I guess by Figure 4, are all sedimentary and crustal phases, plus PmP.

We removed the sentence since it is rather confusing and not necessary to explain the modelling strategy for the final average velocity model. This actually was a process that allowed us to get better acquainted with the data and the model behaviour and which will not be visible in the final results. But indeed, in first steps we only selected picks up to 15 km offset to image the uppermost sediments.

• One would also appreciate more details on the layer stripping strategy. Did the authors overdamped the result of the crustal layer when inverting for mantle phases?

We overdamped the model and included this in the text.

• On the other hand, the authors follow some sort of Monte Carlo analysis to assess the space of possible solutions but they only use 17 models for the crustal level and even a lower number for mantle phases, 12. The final standard deviation is low in Fig. 4. My concern is that given the low number of realizations tested the initial standard deviation (which one would appreciate seeing in the supplementary material) might below as well. I suggest testing at least 100 models for each layer, which is what is commonly done in this type of study to assess the uncertainty of model parameters. The outcome of this uncertainty analysis in its present form is not convincing which may lead to skepticism of the final interpretation.

In contrast to 100 or even 1000 different models, to call it Monte Carlo analysis, we preferred to calculate a smaller set of models to test a wider model space. In contrast to the commonly performed automated generated Monte Carlo models with velocity perturbations of a few percent from an already well fitting starting model, we set up the starting models by hand and tested limits of the model space until the model still converged. In the statistics, we included only starting models which converged to a low chi^2. The two outlying models (now shown in Fig. 4c), "fast" and "slow", would lose their weight if we would add even more automated starting models in the centre of the model space. Thus, we think the statistics based on a few manually created starting models are supporting a robust final average model although based on a lower number of starting models.

The starting models used were 1D hanging below the seafloor along the profile now shown in Figure 4 as (c) and we added a sentence on the 1D structure of the starting models in the text.

• In addition, the authors may want to provide more details on this type of statistical test, right now is a bit vague. How are the initial models? Are they randomly created or they are derived from the forward modelling? The authors could add figures of the initial models, initial standard deviation, as well as the results from forward modelling in the supplementary material. Do they add Gaussian random noise to the picks (I would encourage them to add this to the test)?

Added now in 4c and as inlay in 4d to better document what the input for the modelling is. Random Gaussian noise was not added, but during modelling re-picking of phases (fine adjustments to the wavelet) did not lead to major differences in the resulting velocity model. We added a sentence:

"Random Gaussian noise was not added, to the travel time picks, however, during modelling re-picking of phases (mainly fine adjustments to the picks) did not lead to major differences in the resulting velocity model."

• The gravity modelling could be also improved as well. The authors could show how the gravity response derived from a density model with a homogeneous mantle density of 3.3 g/cc compares with the model they have and the observed anomaly. That would help to discern between serpentinized mantle and non-altered mantle rock, which in turn would allow to strengthen the hypothesis of the paper.

The anomalies in the density have been related to the anomalies in seismic velocities. However, the influence of these localised anomalies that are buried by several kilometres of sediments is minor for the general trend of the model fit to the satellite-derived gravity data. Based solely on gravity data, we cannot judge if the mantle is serpentinised in these patches.

• Line 350-351 and all section 5.4: "seafloor spreading and formation of oceanic crust was not initiated during the extension of the Ligurian Basin.". I would be more cautious here, it seems that the authors are saying that there is no oceanic crust in the whole Ligurian basin. Extension in this basin increases from north to south and as in the Tyrrhenian formation processes may significantly change from the north (region imaged in this study) to the south.

The Ligurian Basin is the NE part of the Liguro-Provencial basin. The studied profile covers the SW part of the Ligurian Basin. Along the profile, we do not observe oceanic crust. Of course, oceanic crust may still occur in the larger Liguro-Provencial basin, but we rule out that there is any oceanic crust towards the NE, within the Ligurian Basin.

We carefully discuss this and point out that the COT might be nearby and oceanic crust occurs towards the S and/or SW as observed by Gailler at al. (2009).

Minor changes:

• Line 18: augmented -> complemented

Changed.

• Line 22-23: "exhumation of sub-continental mantle which eventually became serpentinised". According to the models of mantle exhumation crustal faulting initiates the hydration of the mantle during rifting. Thus, serpentinization occurs before the exhumation. The authors should modify this sentence accordingly.

Re-phrased.

• Section 3.2 The GEOLOG recorder. This section is a bit out of place since this is not a technical paper and thus, it distracts the reader from the main point. I suggest moving this section to supplementary material and briefly mentioning the GEOLOG recording system in section 3.1.

We keep this section in its place, since we cannot refer to a technical paper describing the data logger. The title describes clear what the reader can expect by reading this section and can jump to the next section if it becomes too technically. We included some more technical specifications on the airgun system as well (as recommended by reviewer #2) and hence give more background information on the data acquisition parameters.

• Line 154: The gravimetric data (Fig. 5a) show a change approx. 20 km south of OBS208. Please add the numbering of OBS in Figure 5a.

It is 20 km south of OBS209. Additionally, we now also give the corresponding profile kilometre for this change in the text. We added the OBS numbers as shown in Figure 2.

• Figure 1: There is a bracket missing in Rollet et al. (2002), and it would be good to see the numbering of the OBS/H shown in Fig. 2 instead of OBS/H 201-208-215.

We changed the numbering according to figure 2. Corrected the bracket.