Solid Earth Discuss.,
https://doi.org/10.5194/se-2019-187-RC2, 2020
© Author(s) 2020. This work is distributed under
the Creative Commons Attribution 4.0 License.

Solid Earth
Discussions
Open Access
EGU

Interactive comment on "Oligocene-Miocene extension led to mantle exhumation in the central Ligurian Basin, Western Alpine Domain" *by* Anke Dannowski et al.

Manel Prada (Referee)

mprada@icm.csic.es

Received and published: 14 February 2020

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

Printer-friendly version

Discussion paper

CC BY

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.

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

Printer-friendly version

Discussion paper

CC BY

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. 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? 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 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)? 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. Line 350-351 and all section

CC BY

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.

Minor changes: Line 18: augmented -> complemented

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

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.

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

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

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2019-187, 2019.

Printer-friendly version

Discussion paper

CC BY