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

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1 General comments

The manuscript presents us with a study of crustal structures over a profile crossing along strike in the central Ligurian Basin, by means of seismic refraction and seismic reflection. The main finding of the paper is that a transition from thinned continental crust to unroofed mantle covered by thick sediments is observed from northeast to southwest along the profile. In other words, no mantle-derived oceanic crust seems to be observed in the "oceanic" part of the basin. On these grounds alone, the paper

represents a very interesting contribution and is definitely worthy of publication. I have some comments on the manuscript, that can be ranked as minor but that are numerous though. I think addressing them thoroughly would help improve clarity and consistency.

2 Specific comments and suggested corrections

- Line 60: Dessa et al., 2011 is not a relevant citation as far as the Corsican margin is concerned. Rollet et al.'s paper and perhaps a few others should rather be cited here.
- Lines 102-103: Some more details would be welcome on the seismic source and its tuning. How many airguns? What minimum and maximum volumes? What depth of immersion? What frequency range? Considering the level of technical details provided on the GEOLOG data logger for instance, there is room for a bit more information here.
- Line 104: Still on the technical side, we are told that the symmetry of the direct arrival was used as a criterion to refine station locations. It makes perfect sense but it would be interesting to explain how this is done practically (i.e., how the position is updated from an observed asymmetry). Either a few words or a reference could be provided.
- Line 120: Technical again, and I might be unfamiliar with some recent developments, but I fail to see how an atomic clock may control the sampling rate of an autonomous sea-bottom instrument. Should we understand that in other contexts (such as in a lab...), the data logger would have this capability to be fitted with an atomic clock?
- Line 142: Nitpicking a bit, but Figure 3 has no a) and b) panels and yet, a reference is made here to Fig. 3a (although the sense of it is clear).

- Line 149: The meaning of a “longer” Pg phase is unclear to me. Should we understand that it is observed along a larger range of offsets? It might be rephrased.
- Line 153-155: The text is a bit confused here. 1) Fig. 2b is invoked but does not correspond to a northern station, as the sentence seems to imply; 2) It is not clear to me that the critical distance is larger to the north (this may have a lot to do with the fact that labels along axes in Figure 2 are not readable, see my specific comment on this point below); 3) OBS208 is not labelled in any figure and the discussed change in gravimetric data is not even located below, but 20 km south if the text is to be believed. Why then not giving the actual location of said change in km along the profile rather than with respect to a remote OBS? On a sidenote, to the best of my understanding, this location would correspond to a distance of 50 km along the profile, where I was not able to identify any change in the free air anomaly. Are we talking about the decrease observed from 60 km onward? Confusing indeed. . .
- Line 154: Same remark as for Line 142. No a) and b) panels in Figure 5. Note that this figure is referred to in the text before any reference is made to Figure 4. Normally, figures are numbered in the same order as that in which they are called in the text.
- Line 159: We are told that picking was made on hydrophone data rather than geophone. Is there a justification to this? One would arguably expect a better sensitivity of geophones. Is there any issue with data quality on geophones?
- Line 161: I do not find it completely obvious that using multiples yields more information to confirm layer boundaries. Could the authors provide a citation or a bit of explanation to support this claim?
- Line 166: Some more details explaining how the set of additional starting models were generated would be most welcome. On what assumptions were they built?

Do they span a large area in the model space? Etc. This might even warrant a figure if it can be made synthetic enough.

- Line 167: The use of χ^2 criterion begs the question of travel time uncertainties and how they were assessed. No information is given about that and that too would be most welcome. A χ^2 criterion of 1 only bears relevance if there is a rigorous and objective way to estimate uncertainties.
- Line 171 and 175: Standard deviation values are provided in s here, which does not make any sense ito me and is not coherent with Figure 4b an 4c, where they are given in $km.s^{-1}$, as one would expect for a velocity model.
- Line 181-182: To back up the claim that lateral velocity variations are a consequence of the irregularity of the salt layer, it would be interesting to compare the wavelengths of these anomalies with those of the salt unit as imaged in MCS data. This would furthermore provide some added value to the MCS data which are practically of no use in the discussion of structures as it stands (let alone parasound data)—this point is discussed below (comment on Fig. 3).
- Line 199: Error of reference: the histogram is not in Figure 5a (which does not exist), but in Figure 4e.
- Line 217: The title of the section is wrong. “Discussion” rather than “Introduction” should feature here. The section title is likely inherited from the manuscript template...
- Line 224: Same figure, same problem as in Line 199. Figure 5e instead of 4e.
- Line 267-268: It is not clear to me what the authors mean with a less evolved crust. Do they mean the thickness of it? Its nature? As a result, I find the meaning of the last two sentences of this paragraph rather enigmatic.

- Line 309: I find the sentence about the preference of the authors for a magmatic origin to the observed magnetic anomalies rather than a relation with an unmapped spreading axis to be quite an understatement as their own results seem to completely rule out the possibility of a ridge axis (no oceanic crust is an overwhelming argument against the existence of any accretion axis at any time here I believe). More generally, I think this very interesting result and its implications are not highlighted enough in the discussion.
- Fig. 1: A lot of features on the map, some not very visible, some practically invisible due to a poor choice of non-contrasting colors with respect to the bathymetric background or to the use of thin lines. Rollet et al. refer to "atypical oceanic domain" instead of "atypical oceanic crust". Since the main result of this study is to rule out the existence of an oceanic igneous crust, the reference to an "Atypical Oceanic Crust" is a bit confusing here. I would suggest the same appellation as in Rollet et al.
- Fig. 2: As mentioned above, labels along axes should be made readable for all data plots.
- Fig. 3: Features in the MCS profile are barely visible and poorly discussed in the text. This observation holds even more true for parasound results which do not back results at all. I think dropping them could be considered.

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