Reviewer #2 (Jean-Xavier Dessa)

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

The line numbering in the word template is different to the created PDF, however, we think that we easily could find the sections pointed out.

• 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.

Indeed, we changed the reference to Contrucci et al., 2001 and Rollet et al., 2002.

• 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.

We included more details on the airgun system itself:

"A total of 1079 shots were fired by an ~89-liter (5420 inch^3) G-gun array, consisting of 2 sub-arrays. Each sub-array with a cluster of 2x8.5 litres (520 inch^3), followed by a cluster in the middle of 2x6.2 litres (2x380 inch^3, port) and 2x4.1 litres (2x250 inch^3, starboard), and the third cluster again of 2x8.5 litres for both sub-arrays. The array with a string distance of 12 m was towed at 8 m below the sea-surface and 40 m behind the vessel. A shot interval of 60 s resulted in a shot distance of ~123 m. The guns were shot at ~190 bar providing a dominant frequency band of approximately 5-70 Hz."

• 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.

We included more details (Lines 110-113):

"For this purpose, the direct arrival was picked and the deviation between computed and real travel times was minimised by adjusting the OBS's position along the profile. Dislocation off-line cannot be corrected with this method. For 2D traveltime modelling, the stations were projected on to the profile."

• 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?

Indeed, the internal clock used in some of the recorders is a clock, which is controlled by the oscillation frequency of atoms (i.e. caesium), giving very high accuracy, compared to quartz oscillators. On the other hand atomic clocks consume much more power (probably double the rate) than a usual simple quartz oscillator, which is a disadvantage for long term deployments.

• 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).

The labels “a)” and “b)” have been too small. We enlarged it for better visibility and included a panel c).

• 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.

Re-phrased: *“Simultaneously, when phase Ps3 disappears (from OBS208 towards the north (compare to OBS209 (Fig. 2c), where Ps3 only occurs on the southern branch), an additional refracted phase (Pg) (green picks in Fig. 2c-2d) occurs with an increasing range of offsets observed on the stations and becomes longer northwards.”*

• 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 …

We re-phrased this section in order to make it clearer to the reader:

1) Re-phrased the explanation of Ps3 (yellow phase, Figure 2) and then explain the slight changes from south to north.
2) We increased the axis labels on Figure 2 and adjusted a little bit the position of the phase labels. The difference in critical distance between the northern and the southern stations is very small (~5-10 km) and is maybe better to see in Fig. 2a.
3) OBS208 was wrong! Changed to OBS209. To follow the order of Figures we removed the link to Fig. 5. It was simply to give the reader a view to the second dataset to directly follow the changes, since they are very small. We do not want to change the order of figures, since the gravity modelling is based on the seismic results.

Yes, 60 km along the profile and onward towards the north, and it should be seen from OBS209. Indeed a profile KM will make the description much clearer. Added.

• 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.

Figure 5 does not really have a panel (a) and (b) since for example the colour scale of the gravity model is at the top of the figure. We modified the text so that we now call the Figures in the correct order.

• 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?

The data quality of the geophones is commonly controlled by the instrument coupling to the seafloor and thus varies largely between different study areas. Sometimes the geophone shows a similar or better S/N ratio than the hydrophone on our data set, also dependent on the offset range. While single instruments show a similar quality than the hydrophone, overall the hydrophone data was more robust for all stations along the profile. We included the following sentence on the data quality:

"The overall quality of the hydrophone data was slightly better compared to the vertical geophone channel, however, the vertical component was used for picking to confirm…"

• 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?

We inserted and explained in the text and gave a citation (Meléndez et al., 2014):

"The vertical seismometer component was used for picking to confirm and to complement the picks observed on the hydrophone channel. In addition, multiples were picked when above the noise level (because of constructive interference) and where primary waves are below the noise level (Meléndez et al., 2014)."

• 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.

We added some text to clarify:

"To test the model space and its limits, starting models, ranging from velocities between 1.8 km/s and 2.5 km/s at the seafloor with different velocity gradients, and ranging from 4.5 km/s to 7.5 km/s at 12-13.5 km depth to mimic the different types of crust, were manually created using RAYINVR (Zelt, 1999)."

• Line 167: The use of 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 2 criterion of 1 only bears relevance if there is a rigorous and objective way to estimate uncertainties.

We included a sentence on the size of pick uncertainties that were assigned to the different picked phases (Lines 178-180):

"The picks were assigned pick uncertainties ranging from 20 ms for clear near offset phases (Ps1), 30 ms for intermediate offsets (Ps2 and Pg), and up to 50-70 ms for picks at larger offset (Pn and PmP) taking into account the decreased resolution due to the increased wave length of the seismic signal and the decreased signal-noise-ratio."

• 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, as one would expect for a velocity model.

Corrected.

• 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).

We inserted a third panel (Fig. 3b) which compares the MCS data with the OBS data within the range of 300 traces and linked this figure to the text.

• Line 199: Error of reference: the histogram is not in Figure 5a (which does not exist), but in Figure 4e.

Changed.

• 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...

Corrected - wrong copy paste into the template.

• Line 224: Same figure, same problem as in Line 199. Figure 5e instead of 4e.

Corrected.

• 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.

Changed "less evolved" to "less thick". Added that these observations indicate thickening continental crust towards the North.

• 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.

We changed the title in order to focus on our main finding. We changed the abstract and the style of the conclusion to bullet points to improve the visibility of our findings and highlight them.

• 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.

Changed to atypical oceanic domain (AOD) in the figure. Reduced the contrast of the map and enlarged the contrast and thickness of the lines and objects of importance.

• Fig. 2: As mentioned above, labels along axes should be made readable for all data plots.

Axis labels enlarged and phase labels slightly adjusted.

• 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.

We inserted a third part into the figure (Fig 3, panel b) that compares the undulations of the first arrival phase in the OBS with the MCS data. We like to keep this figure, since it provides a good impression on the complexity of the shallow portion of the subsurface and shows the entire data range acquired along this profile.