tektonika

Review Report

Klingelhoefer et al., Lateral evolution of the deep crustal structure of the Lesser Antilles subduction zone from wide-angle seismic modeling. TEKTONIKA, 2025.

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1st Round of Revisions

Decision Letter

Frauke Klingelhoefer, Boris marcaillou, Muriel Laurencin, Mireille Laigle, Jean-Frederic Lebrun, Laure Schenini, David Graindorge, Mikael Evain, Heidrun Kopp:

We have reached a decision regarding your submission to τεκτonικa, "The Deep Crustal Structure of the Lesser Antilles Subduction Zone.".

Our decision is to: Resubmit for Review

Dear authors,

Thank you for the submission of your article to Tektonia, for which we now have received two reviews. It is clear from these reviews, and my own assessment of this manuscript, that this study has the potential to provide novel insights into the northern Lesser Antilles subducting slab and forearc.

Nevertheless, substantial revisions are required. In particular, both reviewers highlight that the evidence for serpentinised mantle at the base of the subducting slab on line AN06 (i.e. the main finding of this study) is equivocal once the uncertainty in the velocity modelling is considered. I also agree with Reviewer 2 that some restructuring is needed at the start of the manuscript to more clearly outline the tectonic setting, previous work, and methods in this paper.

Please note, that you will also need to address the comments by both reviewers in annotated versions of the manuscript (attached to this review), and Reviewer #1 had some concerns about the applicability of the links included in the Data Availability section. Given the nature of these reviews, I am suggesting you have two months to complete these reviews.

I look forward to receiving the revised manuscript

Best,
Jack

Comments by Reviewer 1

For author and editor

This manuscript uses a 2-D wide-angle marine seismic dataset to model the structure of the subducting crust and fore-arc of the northern Lesser Antilles. The resulting velocity model, together with gravity/density modeling, is compared to similar previously published models from other sections of the Lesser Antilles fore-arc to investigate the properties of the down-going oceanic crust and the nature of the fore-arc crust.

The near-trench part of the model and its comparison with global compilations were previously published in Marcaillou et al. (2021). The data quality is difficult to assess since only one figure with four low-resolution record sections is presented. The methodology used to analyze and model the data (forward raytracing) is standard but not the most modern. Travel-time tomography methods are more widely used nowadays in studies using similar datasets, but their choice of forward raytracing modeling could be justified in this case because it was the method used to produce the existing models to which the authors compare their results.

The main conclusion of the manuscript is that amagmatic lithosphere (thin crust and/or serpentinized mantle) characterizes the down-going slab, contributing a large amount of fluids to the system that could impact the mechanical and rheological properties of the plate interface. I find this conclusion to be unsupported by the data and models, and other claims made by the authors lack evidence to support them (more details in the comments below). Although model uncertainties are evaluated, they were ignored when interpreting results. Main conclusions are based on velocity differences that are below the estimated uncertainties, casting doubt in their robustness. Because of all this, together with confusing and unclear explanations throughout the manuscript, I recommend rejection of the manuscript in its current form.

Comments in order of appearance in the manuscript:

“Lateral evolution…” seems to imply temporal changes. Perhaps better to use “Along-arc variability…”?

Line 28. “Oceanic crust… comprises one single layer”. It is not demonstrated that this is required by the data; seems more like a consequence of simplification in the modeling procedure.

Line 30. “high percentage of…” The authors have not quantified the amount of serpentinized mantle or degree of serpentinization.

Line 31. “fluids leaving this highly hydrated subducting slab at different depths…” No evidence is presented to support the claim of fluid expulsion from the slab.

Line 53. Aren’t “subducting topography asperities” already implicitly included in the “barriers and asperities” mentioned before?

Line 69. “seismicity is moderate…” Moderate in terms of rate, or magnitude?

Line 78-79. “15-20° fracture zone” should be “15°20’N fracture zone”. Also, elsewhere in the manuscript it is referred to as “Fifteen-Twenty FZ”. Please be consistent with nomenclature; choose one or the other.

Lines 117-118. Were gravity data acquired during the ANTITHESIS cruises? Text in Lines 168-169 seems to imply yes.

Line 141 (and others). Numbering of section headings is out of order and not sequential.

Figure 1A legend. What do you mean by “Global” volcanoes?

Figure 1B and Line 147. Nomenclature of the difference seismic profiles is inconsistent across the text and figures. For example, profile TRAIL is mentioned many times in the text, but shown in figures as THALES P1. Also, text refers to AN06 while figure labels it as ANT P6. Please be consistent and use the same nomenclature everywhere to facilitate readers’ understanding of the comparisons and locations of the profiles.

Lines 144-145. Add characteristics of the OBSs (e.g., sensors, sampling rate, etc.), and indicate instrument spacing along the profile.

Lines 160-164. In addition to Fig. 5, the MCS profile should be shown without interpretation or velocity model on top.

Figure 3. This is the only figure where the wide-angle data is shown. Quality of figure is poor. Authors should include a supplement with records sections at larger scale.

Line 197-200. The feature described in this text is never mentioned again in the Discussion section. What is its relevance?

Figure 4 and Line 208. In horizontal axis, please use distance from deformation front to be consistent with all the other figures and the distances mentioned in the text.

Line 201. Point to subduction channel in Fig. 5. How thin is it?

Table 1. Many of the colors in 3rd column are indistinguishable from each other. I suggest authors include a supplement with large record sections and interpreted/predicted phases labeled.

Lines 235-236. Please briefly explain what the spread-point function is. Do blue regions have better resolution than red regions in Fig. 6b?

Figure 6. Please consider using color maps suitable for color-vision deficient readers (e.g., Crameri et al., 2020 doi:1038/s41467-020-19160-7).

Lines 308, 326. Why use WGM2012 or satellite gravity data? Didn’t the experiment acquire shipborne gravity data? Lines 168-169 indicate the processing of the ship’s gravity data, so the choice of satellite data for the gravity modeling seems strange.

Figure 8. Please explain where in the model the symbols labeled “This study” are chosen from. I do not understand why inverted triangles for “unserpentinized mantle” could have velocities as low as ~2.2 km/s; these are not unserpentinized mantle velocities. Or why the “unserpentinized mantle” symbols plot closer to the Christensen’s serpentinized data points than the stars for “serpentinized mantle”. Something is not well explained or labeled in this figure. Also, given the uncertainties shown in Fig. 7, “unserpentinized mantle” and “serpentinized mantle” symbols are indistinguishable from each other; they are within the

estimated error bars.

Lines 326-327. “To increase the upper mantle of the oceanic domain…” Awkward wording. To increase what?

Line 330. Use of qualitative adjective “highly serpentinized” is not justified. What is the degree of serpentinization and amount of serpentine in the crust needed to reduce densities from 2.75 to 2.7 g/cm3? Could authors be more quantitative? (Same comment is applicable to Line 364 “high degree of serpentinized…”)

Line 332. “gabbro samples”. Cited reference Christensen (2004) does not report information about gabbro samples.

Line 349 and forward. TRAIL profile not labeled in Fig. 1. Where is it located?

Line 352. Here it is claimed that lower crustal velocities at AN06 are ~0.2 km/s higher than for TRAIL. But 0.2 km/s is well below the uncertainty estimates show in Fig. 7, which are 0.6-0.9 km/s. Also, authors need to explain how are the locations of the velocity-depth profiles chosen. Since the velocity in the oceanic crust increases down-dip, how would the interpretation be different if the profiles are taken closer to the trench where velocities are lower? And in the same paragraph it is indicated that a constant velocity gradient for the oceanic crust in AN06 provided the best fit to the data. But this was a modeling choice; nowhere in the manuscript the authors show evidence that such single-gradient crust is required by the data or discuss other possible structures consistent with the data. For example, none of the record sections and raypaths shown in Fig. 3 include oceanic crustal refractions. This phase is the only one that can accurately constrain velocities in the oceanic crust. If the crustal velocity structure is primarily constrained by Moho reflections, as Fig. 3 seems to indicate, then there are huge trade-offs between average crustal velocity and thickness. These aspects are important because they are the basis for the main conclusion that the down-going slab is primarily serpentinized peridotite. I do not think such conclusion if supported by the evidence shown in the manuscript.

Line 356. There is no Fig. 12 in the manuscript.

Lines 357-372. Comparison of V(z) profiles with existing compilations could be misleading because having parameterized the oceanic crust with a single velocity gradient, comparison with other models that include more layers could lead to biases.

Lines 357-358. Which ages of Atlantic and Pacific crust where chosen for the comparison?

Line 359. The choice of using velocities from exhumed mantle sections in rifted margins as a comparative reference seems odd given that there are seismic studies on oceanic crust in locations of reduced magmatism or mantle exposures. For example, since the Davy et al. (2020) paper is cited as a nearby example of heterogeneous crust entering the Lesser Antilles SZ, authors could compare AN06 and TRAIL oceanic crustal structure with the tectonic and magmatic V(z) profiles of Davy et al. Based on the discussion provided in this paragraph, it would be expected that TRAIL crust is similar to Segments 4-5 of Davy et al., while AN06 crust should be similar to Segments 1-3 and NTOs/FZs of Davy et al.

Line 360. I find the statement that “Thicknesses and velocities in the AN06 model fit better to the outline of an upper mantle layer exhumed at the ocean floor” unconvincing. Red profile in

Fig. 10C could be interpreted as well as falling within the light blue region, particularly given the 0.6-0.9 km/s estimated uncertainties.

Lines 368, 462. Reference Sauter et al. (2013) is not about Mid-Atlantic Ridge.

Line 374-375. Are the strong ridgeward dipping reflectors present in the MCS Ant06 profile? If they are, please show them in Fig. 5. If the Moho reflection is absent in the MCS data, how do authors reconcile this with the large velocity contrast at the oceanic Moho in the AN06 velocity model?

Lines 386-387. The statement that the AN6 results show that the Jacksonville patch extends down to mantle wedge depths is not supported by the data shown in the manuscript. As mentioned in my earlier comment, without showing that crustal refractions constrain well the oceanic crustal velocity structure at those depths, the statement lacks evidence.

Line 391. “observed mud volcanism.” Include data or citation to support such observation.

Lines 391-393. Is there any evidence in the model for fluid migration from the slab, like low velocities in the mantle wedge that could be attributed to serpentinization, or velocity anomalies in the fore-arc crust?

Figure 10. It is not clear where the velocity-depth profiles shown in sets C and D were extracted from. Are the red and blue profiles in C the same as in D? Labeling in A seems to indicate that profiles in C correspond to AN06 model and labeling in B that profiles in D correspond TRAIL model, but legend and manuscript text say a different thing. Please clarify. Also, indicate which of the light and dark blue polygons in C correspond to Atlantic and which to Pacific crust.

Lines 422-438. The arguments in this paragraph are being used to explain “thickening of NLA crust”. But there has not been a description of such thickening anywhere in the prior text. Evidence for such thickening should be presented in the Results section. Also, the statement in Lines 429-430 seems to indicate that NLA arc crust has been thickened by volcanism. Is the NLA arc crust abnormally thicker than at other arcs? Hard to tell from Fig. 11.

Figure 11. Indicate the locations where the velocity-depth profiles were extracted from.

Lines 458-460. As indicated in my previous comments, the manuscript lacks convincing evidence for a high velocity structure in the downgoing crust. Also, interpretating such potential high velocity anomaly as resulting from serpentinization requires more explanations because serpentinization decreases, not increases, seismic velocities.

Lines 470-473. Relating seismicity to fluids is only discussed in the text in Line 394, while all the other factors mentioned here were not discussed previously. I would expect that the Conclusions provide a summary of aspects previously discussed throughout the manuscript.

Line 494. Data availability. The link provided correctly points to a data repository for the Antithesis experiment. However, I downloaded the dataset available from that page and it turns out it does not correspond to the data presented in this manuscript; it corresponds to the Garanti experiment (Padron et al., 2021). This should be corrected.

Comments by Reviewer 2

Section A: Overview of manuscript

A1) Overall evaluation, general comments & summary

A1.1) Reviewer's comments

A1.1.1) General evaluation and publication suggestion – Required:

Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.

The manuscript details a 2D valocity model across the Northern Lesser Antilles obtained from forward modeling of refracted arrival times and joint analysis of multichannel seismic reflection (MCS), in an area where the subducting oceanic crust was formed at an amagmatic spreading center. The profile is compared to existing profiles south and north-west of the area and shows a similar arc crust. An oceanic crustal structure compatible with a high serpentinized peridotite content is in agreement with the low seisimicity of this subduction zone segment, which is explained as the result of fluid dehydration flowing upward through the plate interface. This manuscript is suitable for publication in Tektonika but more work is necessary to better demonstrate how the new data exactly confirms or improves the final interpretation that was already derived in other papers. This suggestion would greatly increase the impact of the paper

A1.1.2) What does the submission need to be publishable? (select as needed; comment for all cases)

☐ No changes required

x Rewriting

x Reorganising

x More data/figures

☐ Condensing

☐ Reinterpretation

x Other:

Comments:

Sections numbering are almost impossible to understand. There is even a numbering reset (Section 3 is the first section, then 4, then 1) and the reader is unsure whether some parts are sections or subsections. The text therefore needs to be reorganized. Why not including a "methods" section?

More details are needed on the a-priori structures of the velocity modeling and fit to the data.

In particular, the paper should show very clearly how the data is better modeled by a unique oceanic crustal gradient, and how the slab dip “kink” at km 90 is constrained.

A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)

- ☐ Text
- ☐ Table
- x Figures
- x Supplementary material

Comments:

Additional ***Figures are needed to see details of the oceanic crustal structure fit to the data. In the main paper or in supp. Mat.The text needs to be reorganized***

A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)

- ☐ Quality is poor
- ☐ Research is not reproducible
- ☐ Other

Comments:

[Free form box]

A2) Summary of main merits and main points of improvement

A2.1) Reviewer’s comments

Please describe below in a few sentences (100 to 300 words) the main merits of the submission and suggestions for improvements.

The main merits I have found are...

Section B: Detailed evaluation of manuscript

B1) Title and abstract

B1.1) Reviewer’s comments

*These statements are a **guide** to what good* Titles *and* Abstracts include*. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Title* describes the main topic of the manuscript **accurately** — [Not really]

Although I am not sure if the focus on the amagmatic oceanic crust really is included in "Island arc". If we strictly consider the island arc structure, there is barely any lateral variation in all the profiles. The difference is in the slab properties

The *Title* describes the main topic of the manuscript **succinctly** — [yes]

The *Title* includes **appropriate key terms** — [No]

Please see first remark

The *Abstract* includes a **clear aim and rationale** — [NO]

The abstract states that previous studies show properties that this study also show, but it does not explain why it has an added value.

The *Abstract* supports the rationale with **sufficient background information** — [YES]

The *Abstract* includes a **well-balanced description of the methods** — YES

The *Abstract* describes the **main results sufficiently and adequately** — YES

The *Abstract* clearly describes the **importance/impact of the study** — [NO]

The *Abstract* clearly states the **conclusions of the study** — [YES]

The *Abstract* is **clear** and **well structured** — [YES]

Yes, but there the issues of the introduction are also in the abstract

Comments:

Comments are included after each box above.

The manuscript figures are clear and the amount of work to build the model is very significant.

The main points of improvement I have found are...

The introduction is followed by two sections numbered 3 and 4 (while Introduction has no number) and these sections also look like introduction, with sometimes an added discussion "flavor". I think it indicates the difficulty for the authors to explain what their profile exactly brings when compared to previous studies like, for instance, Marcaillou et al. (2021). I think that the authors should emphasize the role of refraction studies in constraining, beyond structural analysis, the nature of rocks and hydration through velocity analysis. The introduction should much further highlight the importance of the specific location of the profile and how it will complement the other existing profiles. The information is mostly there, but currently the introduction and additional sections (objectives and previous studies) are disorganized and look like too much like lists of informations with a lack of a central thread.

Regarding the results and methods, although it is useful, the Monte-Carlo uncertainty estimations should be more carefully introduced. The method does not totally solve the issue of a-priori input and non-uniqueness in forward models (neither in forward modeling nor in tomography studies, to be fair). For instance, the number of layers is already an a-priori that is not varied in Vmontecarlo (to my knowledge). Monte-Carlo approaches often perturb the models only around the preferred model, in both forward and inverse methods. What I think is lacking in the manuscript is to show the data in order to convince the reader that the more important features of the model can be supported by the data: what exactly in the data constrains the single oceanic crustal gradient? Which layers are constrained with more than wide angle reflections? What data exactly constrain the sudden slab dip change in the model?

B2) Introduction

B2.1) Reviewer's comments

*These statements are a **guide** to what good* Introductions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [YES]

The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [NO]

The *objective/hypothesis/rationale* **flows logically from the background** information — [NO]

The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES] and [NO]

The *Introduction* contains **relevant, suitable citations** — [YES]

The *Introduction* is **organized effectively** — [NO]

Comments:

As already stated in the general comments, the strange "sections" 3 and 4 (objectives and previous work) are expected in the introduction itself, and the rationale should flow from it. In its current form, the "composite" introduction is somewhat repetitive, include elements that could be discussion and lacks rationale.

B3) Data and methods

B3.1) Reviewer's comments

*These statements are a **guide** to what good* Method *sections include and good practices for* Dataset *accessibility. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Methods* are described **concisely and with enough detail** for reproducibility — [YES]

Necessary information about **data sources/acquisition/processing** is included — [YES]

Data used are accessible via either supplementary files or links in the data availability statement — [YES]

The *Dataset and/or Methods* are **organized effectively** — [NO]

Comments:

The Methods are not sufficiently detailed before the results section. I think the methods should be in the Dataset and methods section and should first describe the refraction and Wide Angle reflection phases and how they constrain the most important layers. In particular, the dataset section should show how the oceanic crustal structure is constrained and how the deep a priori structure is built (the shallow constraints from MCS data are well described but what are the a priori at depth? Is the slab kink an apriori? If not how is it constrained?). Then the results sections can focus more exclusively on velocity gradients and error analysis (more or less as it is)

B4) Results

B4.1) Reviewer's comments

*These statements are a **guide** to what good* Result sections *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Results* findings are **supported by data** — [NO]

The *Results* findings are presented **clearly and succinctly** — [YES]

The text in the *Result* section **cites tables and figures appropriately** — [YES]

The *Results* directly **relate to the study objectives** — [YES]

The *Results* present **data for all the approaches** described in the *Methods* section — [YES] / [NO]

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion.* — [YES]

The *Results* section is **organised effectively** — [YES]

Comments:

This section is well done, but the authors could show much more the key features of the data supporting the main modeling results in a method section before this results section.

B5) Discussion and conclusions

B5.1) Reviewer’s comments

These statements are a ***guide*** *to what good* Discussions *and* Conclusions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Discussion* is **focused on the objectives** of the study — [YES]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [YES]

The *Discussion* section properly identifies all **speculative statements** — [YES] / [NO]

The *Discussion* section presents the **implications of the study** persuasively — [NO]

The *Discussion* section **highlights novel contributions** appropriately — [NO]

The *Discussion* section **addresses the limitations** of the study appropriately — [YES] / [NO]

The *Discussion* section is **organised effectively** — [YES]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES]

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES]

The *Conclusions* are **clear and concise** — [YES]

Comments:

The discussion is easy to read and informative. However, it is difficult to extract the novelty of the results and how these improve previous interpretations: Why was it important to have a new refraction velocity model at this particular location (in intro, but it should be then discussed again here)? How exactly does this new dataset answers limitations of previous interpretations? In its current form, the discussion shows how the new results are in agreement with previous results for the incoming plate and for the arc (with no link between them), with no further implications. The slab geometry dip change in the model is cited but not really discussed, and I think it really should.

B6) Figures, tables and citations

B6.1) Reviewer’s comments

These statements are a ***guide*** *to what good* Figures *and* Tables *include and how they are* *presented. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

Tables and *Figures* are **ordered logically** and **numbered sequentially** — [YES]

Tables and *Figures* have **captions that explain** all their major features — [YES]

Tables and *Figures* have **captions that complement** the information in the main text — [YES]

Tables and *Figures* present data that **relate** to the study objective — [YES]

Tables and *Figures* present data that are **consistent** with and support the description of results — [YES]

Tables and *Figures* have **succinct and informative titles** — [YES]

Figures are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc.…) — [YES]
Please, check our [Figure guidelines]

Figures with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — [YES]

Figures with **maps** have **sufficient location information** (in the map or caption) — [NO]

Cross-sections have clear labels for **scale and coordinates** at ends and within-section kinks — [YES]

All georeferenced elements are provided in common format (.shp, .geotiff, .kml) [in an open-access repository] — [YES] / [NO]

Citations throughout are relevant, suitable, and comprehensive — [YES]

Comments:

All locations discussed in the introduction are not found in the maps. Also, a map would be necessary in the discussion, at least as insets of Figures 10/11 because the reader is lost and it is essential to the clarity of the comparison. For instance, ANTITHESIS 5 is not shown in the Figure 1 map, and even it was, a reminder would be necessary to locate all the discussed profiles

Authors’ Reply to Reviewer 1

This manuscript uses a 2-D wide-angle marine seismic dataset to model the structure of the subducting crust and fore-arc of the northern Lesser Antilles. The resulting velocity model, together with gravity/density modeling, is compared to similar previously published models from other sections of the Lesser Antilles fore-arc to investigate the properties of the down-going oceanic crust and the nature of the fore-arc crust.

The near-trench part of the model and its comparison with global compilations were previously published in Marcaillou et al. (2021). The data quality is difficult to assess since only one figure with four low-resolution record sections is presented. The methodology used to analyze and model the data (forward raytracing) is standard but not the most modern. Travel-time tomography methods are more widely used nowadays in studies using similar datasets, but their choice of forward raytracing modeling could be justified in this case because it was the method used to produce the existing models to which the authors compare their results.

We are not sure that tomographic models which include a large set of parameters set by the user (horizontal and vertical smoothing, number of iterations etc.) are as independent as they are said to be. Additionally most tomographic models include one reflection only (the Moho) and does not take into account information from seismic reflection data. An interesting discussion of forward versus inverse modeling can be found in Zelt, 1999.

Reference :

Zelt, C. A. (1999). Modelling strategies and model assessment for wide-angle seismic traveltime data. Geophysical Journal International, 139(1), 183-204.

The main conclusion of the manuscript is that amagmatic lithosphere (thin crust and/or serpentinized mantle) characterizes the down-going slab, contributing a large amount of fluids to the system that could impact the mechanical and rheological properties of the plate interface. I find this conclusion to be unsupported by the data and models, and other claims made by the authors lack evidence to support them (more details in the comments below). Although model uncertainties are evaluated, they were ignored when interpreting results. Main conclusions are based on velocity differences that are below the estimated uncertainties, casting doubt in their robustness. Because of all this, together with confusing and unclear explanations throughout the manuscript, I recommend rejection of the manuscript in its current form.

We have substantially fortified our argumentation by additionally showing evidence from reflection seismic profiles and bathymetry data. We also included the heat flow data from Ezenwaka et al., 2022 which indicates the presence of hydrothermal cooling offshore Saint Martin in our study region. To keep the manuscript short we had originally only included references to existing papers, but we agree that the manuscript gains in readability if the original data are also shown.Comments in order of appearance in the manuscript:

"Lateral evolution..." seems to imply temporal changes. Perhaps better to use "Along-arc variability..."?

This has been corrected

Line 28. "Oceanic crust... comprises one single layer". It is not demonstrated that this is required by the data; seems more like a consequence of simplification in the modeling procedure.

Not only would we expect a reflection from the top of a second layer, also the data do not show any change in velocity gradient. We used the minimum-parameter/minimum structure approach, to avoid inclusion of velocity or structural features into the model unconstrained by the data, and this led us to model the crust as one single layer. We added this explanation to the text.

Line 30. "high percentage of..." The authors have not quantified the amount of serpentinized mantle or degree of serpentinization.

This has been corrected, and some more discussion has been added in the section about gravity modelling.

Line 31. "fluids leaving this highly hydrated subducting slab at different depths..." No evidence is presented to support the claim of fluid expulsion from the slab.

We have added a figure showing clearly identifiable mud-volcanoes in the study region close to the wide-angle seismic profile, to better convince the reader.

Line 53. Aren't "subducting topography asperities" already implicitly included in the "barriers and asperities" mentioned before?

Yes, we took this out.

Line 69. "seismicity is moderate..." Moderate in terms of rate, or magnitude?

We propose that Low-to-moderate seismicity is defined as regions with (Mw < 4.5) analogous to Laurendeau et al., 2022. And as the Lesser Antilles have a seismicity of higher Mw, we reworded the text to “modern day seismicity is moderate to slightly elevated (Mw < 6).”

Reference :

Laurendeau, A., Clement, C., & Scotti, O. (2022). A strategy to build a unified data set of moment magnitude estimates for low-to-moderate seismicity regions based on European–Mediterranean data: application to metropolitan France. Geophysical Journal International, 230(3), 1980-2002.

Line 78-79. "15-20° fracture zone" should be "15°20'N fracture zone". Also, elsewhere in the manuscript it is referred to as "Fifteen-Twenty FZ". Please be consistent with nomenclature; choose one or the other.

We made it consistently "Fifteen-Twenty FZ", including Figure 1

Lines 117-118. Were gravity data acquired during the ANTITHESIS cruises? Text in Lines 168-169 seems to imply yes.

Sorry. Yes, data were acquired, but the instruments had technical problems, so in the end we used satellite data. We deleted this misleading sentence.

Line 141 (and others). Numbering of section headings is out of order and not sequential.

This has been corrected.Figure 1A legend. What do you mean by "Global" volcanoes?

We meant “Global volcano compilation” this has been corrected.

Figure 1B and Line 147. Nomenclature of the difference seismic profiles is inconsistent across the text and figures. For example, profile TRAIL is mentioned many times in the text, but shown in figures as THALES P1. Also, text refers to AN06 while figure labels it as ANT P6. Please be consistent and use the same nomenclature everywhere to facilitate readers' understanding of the comparisons and locations of the profiles.

Sorry, we have made the profile names consistent between Figures and text.

Lines 144-145. Add characteristics of the OBSs (e.g., sensors, sampling rate, etc.), and indicate instrument spacing along the profile.

We added the following text :

The OBS recorded on three geophone and a hydrophone channel with a sampling rate of 4 ms (see Auffret et al., 2004 for technical description). Along profile AN06 25 instruments were deployed at a regular spacing of 3 nm (5.5 km).

Lines 160-164. In addition to Fig. 5, the MCS profile should be shown without interpretation or velocity model on top.

We have added this to the figure.

Figure 3. This is the only figure where the wide-angle data is shown. Quality of figure is poor. Authors should include a supplement with records sections at larger scale.

Unfortunately the data figures in most manuscripts are small. However, the data are freely available to the interested reader. (https://www.seanoe.org/data/00455/56629/)

Line 197-200. The feature described in this text is never mentioned again in the Discussion section. What is its relevance?

We deleted this purely descriptive phrase.

Figure 4 and Line 208. In horizontal axis, please use distance from deformation front to be consistent with all the other figures and the distances mentioned in the text.

This has been corrected

Line 201. Point to subduction channel in Fig. 5. How thin is it?

We do not know it is either less then the resolution of our model or absent. We added this to the text. The resolution of the wide-angle seismic data n this depth does not allow to constrain the thickness of the subduction channel. However, on a neighboring seismic reflection profile a thickness ~0.2 to ~1 s twt has been proposed (Boucard et al., 2021).

Reference

Boucard, M., Marcaillou, B., Lebrun, J. F., Laurencin, M., Klingelhoefer, F., Laigle, M., ... & Philippon, M. (2021). Paleogene V‐ shaped basins and Neogene subsidence of the Northern Lesser Antilles Forearc. Tectonics, 40(3), e2020TC006524.

Table 1. Many of the colors in 3rd column are indistinguishable from each other. I suggest authors include a supplement with large record sections and interpreted/predicted phases labeled.

We changed the colors in the figure and the corresponding table, making the figure better readable. All data are available in the Seanoe data repository for the reader who wishes to explore them in more detailLines 235-236. Please briefly explain what the spread-point function is. Do blue regions have better resolution than red regions in Fig. 6b?

We have added to the explanation :

The point-spread function is calculated by perturbing a node and then inverting the perturbed model. If the model is poorly resolved for a given region, then one given nodes's perturbation will be smeared into adjacent nodes, possibly for both the velocities and the boundary depths, if both parameter types are involved. If the model region is well constrained the inversion will be identical or very close to the original model. Thus, the extent of the smearing indicates the spatial resolution of the model (Zelt, 1999). Perturbing single nodes allows defining the spread-point function (SPF) giving the amount of smearing in different model regions (Zelt, 1999).

Figure 6. Please consider using color maps suitable for color-vision deficient readers (e.g., Crameri et al., 2020 doi:1038/s41467-020-19160-7).

This is a good point. However, we would like to keep the colorscales for comparability with the other publications. We will bear this in mind in our future work.

Lines 308, 326. Why use WGM2012 or satellite gravity data? Didn't the experiment acquire shipborne gravity data? Lines 168-169 indicate the processing of the ship's gravity data, so the choice of satellite data for the gravity modeling seems strange.

Yes, we did, but unfortunately the data were not useable. We deleted the text saying that we processed the shipboard data.

Figure 8. Please explain where in the model the symbols labeled "This study" are chosen from. I do not understand why inverted triangles for "unserpentinized mantle" could have velocities as low as ~2.2 km/s; these are not unserpentinized mantle velocities. Or why the "unserpentinized mantle" symbols plot closer to the Christensen's serpentinized data points than the stars for "serpentinized mantle". Something is not well explained or labeled in this figure. Also, given the uncertainties shown in Fig. 7, "unserpentinized mantle" and "serpentinized mantle" symbols are indistinguishable from each other; they are within the estimated error bars.

This was indeed very misleading. We changed the figure and added this in the text :

The triangles and stars correspond to Model 1 and Model 2 in Figure 9. The upper layers which are not changed are marked by blue inverted triangles and lower layers marked by orange triangles correspond to Model 1 without serpentinisation. The orange stars then reflect the decrease of density due to serpentinisation in Model 2.

Lines 326-327. "To increase the upper mantle of the oceanic domain..." Awkward wording. To increase what?

We rephrased to :

To increase the fit of the model to the measured gravity anomaly, we used a slightly lower density simulating a serpentinization of the uppermost mantle of not more than 15%.

Line 330. Use of qualitative adjective "highly serpentinized" is not justified. What is the degree of serpentinization and amount of serpentine in the crust needed to reduce densities from 2.75 to 2.7 g/cm3? Could authors be more quantitative? (Same comment is applicable to Line 364 "high degree of serpentinized…")

This is right the use of the qualitative adjective "highly serpentinized" was not justified. However giving exact examples is difficult as velocities, densities and pressure interplay with each other. Measurements in serpentinized gabbro samples show a density variation between 3.3 g/cm3 at 0% serpentinization to 2.4 g/cm3 at 100 % serpentinization at 1000 Mpa and 400°C (Christensen, 2004,Carlson and Miller, 2003, Carlson, 2006) (Figure 8). Mixing highly serpentinized mantle material with gabbros or basalt might therefore lower the bulk density of the layer. The modelled density change would correspond to an increase of 10-20% of serpentinisation, but will not account for density or temperature effects.

We added this to the text to give the reader some order of magnitude, but fine analysis is clearly not possible.

Added reference :

Carlson, R. L., & Miller, D. J. (2003). Mantle wedge water contents estimated from seismic velocities in partially serpentinized peridotites. Geophysical Research Letters, 30(5).

Carlson, R. L. (2003). Bound water content of the lower oceanic crust estimated from modal analyses and seismic velocities of oceanic diabase and gabbro. Geophysical research letters, 30(22).

Line 332. "gabbro samples". Cited reference Christensen (2004) does not report information about gabbro samples.

This has been corrected

Line 349 and forward. TRAIL profile not labeled in Fig. 1. Where is it located?

This has been corrected, the name has been to TRAIL P2 throughout the text and the figures.

Line 352. Here it is claimed that lower crustal velocities at AN06 are ~0.2 km/s higher than for TRAIL. But 0.2 km/s is well below the uncertainty estimates show in Fig. 7, which are 0.6-0.9 km/s. Also, authors need to explain how are the locations of the velocity-depth profiles chosen. Since the velocity in the oceanic crust increases down-dip, how would the interpretation be different if the profiles are taken closer to the trench where velocities are lower? And in the same paragraph it is indicated that a constant velocity gradient for the oceanic crust in AN06 provided the best fit to the data. But this was a modeling choice; nowhere in the manuscript the authors show evidence that such single-gradient crust is required by the data or discuss other possible structures consistent with the data. For example, none of the record sections and raypaths shown in Fig. 3 include oceanic crustal refractions. This phase is the only one that can accurately constrain velocities in the oceanic crust. If the crustal velocity structure is primarily constrained by Moho reflections, as Fig. 3 seems to indicate, then there are huge trade-offs between average crustal velocity and thickness. These aspects are important because they are the basis for the main conclusion that the down-going slab is primarily serpentinized peridotite. I do not think such conclusion if supported by the evidence shown in the manuscript.

As we explained earlier, the one-layer crust of the oceanic crust is a result from combining forward modeling and minimum-parameter/minimum structure approach. We are showing the main constraint on the model, but this remains a model and can therefore be questioned. So instead of adding more error calculations we have added other data (seismic reflection, bathymetry and heat flow) which confirm our hypothesis, that the downgoing crust is of a different nature along AN06 and THALES P1.

Line 356. There is no Fig. 12 in the manuscript.

Yes, we corrected to Figure 10 and 11.

Lines 357-372. Comparison of V(z) profiles with existing compilations could be misleading because having parameterized the oceanic crust with a single velocity gradient, comparison with other models that include more layers could lead to biases.

We agree, it is always possible to add more layers into a wide-angle seismic model. However, this is why we always use the minimum-parameter/minimum structure approach to avoid including artefacts into the model. We have added the exact position of the vz to Figure 11.Lines 357-358. Which ages of Atlantic and Pacific crust where chosen for the comparison?

These are 144-170 Ma for the Atlantic and 29-140 Ma for the Pacific crust. We have added the information to the figure caption.

Line 359. The choice of using velocities from exhumed mantle sections in rifted margins as a comparative reference seems odd given that there are seismic studies on oceanic crust in locations of reduced magmatism or mantle exposures. For example, since the Davy et al. (2020) paper is cited as a nearby example of heterogeneous crust entering the Lesser Antilles SZ, authors could compare AN06 and TRAIL oceanic crustal structure with the tectonic and magmatic V(z) profiles of Davy et al. Based on the discussion provided in this paragraph, it would be expected that TRAIL crust is similar to Segments 4-5 of Davy et al., while AN06 crust should be similar to Segments 1-3 and NTOs/FZs of Davy et al.

Yes, we have compared these studies, and included the result into Figure 11. We also added the following text :

Comparing the results from our study to those of Davy et al., 2020 shows that while the velocities and thickness of the crust along the THALES P1 is comparable to segment centers in their study, the characteristics of crust along the AN06 profile better fit the segments end and fracture zone regions. This is in good agreement with our proposition that crust at the AN06 profile originates mainly from tectonic accretion.

Line 360. I find the statement that "Thicknesses and velocities in the AN06 model fit better to the outline of an upper mantle layer exhumed at the ocean floor" unconvincing. Red profile in Fig. 10C could be interpreted as well as falling within the light blue region, particularly given the 0.6-0.9 km/s estimated uncertainties.

We have added more evidence for the existence of fluids in this part of the subductions zone, in form of reflection seismic and bathymetric data showing clear differences of our two study regions.

Lines 368, 462. Reference Sauter et al. (2013) is not about Mid-Atlantic Ridge.

Yes, this it is the Southwest Indian ridge. This was corrected.

Line 374-375. Are the strong ridgeward dipping reflectors present in the MCS Ant06 profile? If they are, please show them in Fig. 5. If the Moho reflection is absent in the MCS data, how do authors reconcile this with the large velocity contrast at the oceanic Moho in the AN06 velocity model?

The reflectors are only weakly discernible in AN06. However we included a Figure from the middle of the Jacksonville Patch were they have a stronger amplitude.

Lines 386-387. The statement that the AN6 results show that the Jacksonville patch extends down to mantle wedge depths is not supported by the data shown in the manuscript. As mentioned in my earlier comment, without showing that crustal refractions constrain well the oceanic crustal velocity structure at those depths, the statement lacks evidence.

We added a figure showing the deep reflectors in the downgoing ocean crust in the Jacksonville Patch to demonstrate its extend (Figure 2B).

Line 391. "observed mud volcanism." Include data or citation to support such observation.

To be most convincing we included an additional Figure showing a bathymetric map of our study region 4 mud-volcanoes in a 15x15km region. (Figure 2A)Lines 391-393. Is there any evidence in the model for fluid migration from the slab, like low velocities
in the mantle wedge that could be attributed to serpentinization, or velocity anomalies in the fore-arc crust?

Yes, there are numerous mud-volcanoes and pockmarks in the forearc domain (New Figure 2A) that have been recently interpreted to correspond to fluid seeps coming from the subduction megathrust through deeply rooted forearc crustal faults. We also interpret the strong amplitude intracrustal reflectors of the lower plate (New Figure 2B) to contain fluid-rich and/or serpentinized rocks

Figure 10. It is not clear where the velocity-depth profiles shown in sets C and D were extracted from. Are the red and blue profiles in C the same as in D? Labeling in A seems to indicate that profiles in C correspond to AN06 model and labeling in B that profiles in D correspond TRAIL model, but legend and manuscript text say a different thing. Please clarify. Also, indicate which of the light and dark blue polygons in C correspond to Atlantic and which to Pacific crust.

We clarified this in the Figure and the caption.

Lines 422-438. The arguments in this paragraph are being used to explain "thickening of NLA crust". But there has not been a description of such thickening anywhere in the prior text. Evidence for such thickening should be presented in the Results section. Also, the statement in Lines 429-430 seems to indicate that NLA arc crust has been thickened by volcanism. Is the NLA arc crust abnormally thicker than at other arcs? Hard to tell from Fig. 11.

We agree that it is not possible to tell if the NLA crust is thickened by volcanism and it is not unusually thick compared to other arcs. We rewrote and shortened this part of the discussion.

Figure 11. Indicate the locations where the velocity-depth profiles were extracted from.

The locations are marked by black line segments on top of the models. The vz profiles are a mean of vz profiles extracted every 10 km. We added this explanation to the figure caption.

Lines 458-460. As indicated in my previous comments, the manuscript lacks convincing evidence for a high velocity structure in the downgoing crust. Also, interpretating such potential high velocity anomaly as resulting from serpentinization requires more explanations because serpentinization decreases, not increases, seismic velocities.

This unusual type of crust in the downgoing slab is taken from the velocity models but confirmed by the unusual reflectors imaged in the crust and the numerous fluid expulsion sites found on the oceanic crust and in the forearc domain. Together we hope to make a convincing case of higher degrees of serpentinite in the downgoing crust than in the comparison region further south. We propose that this higher degree of serpentinisation influences the seismicity of the region.

Lines 470-473. Relating seismicity to fluids is only discussed in the text in Line 394, while all the other factors mentioned here were not discussed previously. I would expect that the Conclusions provide a summary of aspects previously discussed throughout the manuscript.

Yes, we added two paragraphs in the discussion, in addition to showing water expulsion sites in an additional figure, to better introduce our conclusions. Some of the conclusion text has been relocated to the discussion section.

Line 494. Data availability. The link provided correctly points to a data repository for the Antithesis experiment. However, I downloaded the dataset available from that page and it turns out it does not correspond to the data presented in this manuscript; it corresponds to the Garanti experiment (Padron et al., 2021). This should be corrected.

The GARANTIE and ANTITHESIS data were switched in the depot, this has been corrected.Reviewer G, edits :

L. 73 : showed to → suggest

L. 78 : was → is

L. 83 : “probably” deleted

L. 83 : “probably” inserted

L. 84 : “probably” deleted

L. 88 : associated → together

L. 90 : the → a

L. 95 : Virgin islands → Virgin Islands

L. 108 : model → models

L. 112 : discuss → discussed

L. 119 : 17°30 → 17°30’

L. 95 : Virgin Island → Virgin islands

L. 124 : Eventually a → A

L. 176 : reflection seismic → seismic reflection

L. 194 : three → four

L. 199 : we left bathymetric as is is identified in bathymetric data.

L. 201 : corner → wedge

L. 222 : minimize … model error → maximize … data fit

L. 270 : who → that

L. 325 : deleted “calculated”

L. 331 : increase → improve

L. 367 : Central Atlantic ridge → mid-Atlantic RidgeReviewer G

L. 16 : "probably" should not be a word used in key points6→ corrected

L. 27: Layer in what sense? We added the explanation “with a constant velocity gradient”

L. 38 : builds → is divided into

L. 64 : has long been recognized → has been identified

Difference to other arcs are discussed later in the manuscript.

L. 70 : crust → lithosphere-asthenosphere

L. 84 : “is it still part of the introduction?” Yes, and we corrected the numbering.

L. 108 : there in → therein

L. 112 : discuss → discussed

L. 122 : shallowing → more shallow location

Fig. 1 : please locate on map : It is now located as Fifteen-Twenty FZ on the Figure 1.

L. 145 : the full 4.5 km or a subset → full, we clarified in the text

L. 165 : Kronsberg → Kongsberg

Fig. 3 : isn’t the first arrival here, in continuity of the blue phase ? Yes.

Are there oceanic crustal arrivals ? Are the turning waves in each layer ? Yes, there are oceanic crustal arrivals, however some layers are mainly constraint by reflection from MCS and wide-angle seismic arrivals (see resolution figure).

L. 190 : Phases first ? What a-priori ? Sorry, we do not understand the question.

L. 198 : again what is constraining this unique gradient ? The unique gradient has been used using the minimum-parameter/minimum structure approach (see Reviewer E).

L. 205 : layers → interfaces

L. 206 : I’d write the opposite: velocity layers correspond to stratified sediments. → rewritten to :

Lower velocities in the layers correspond to well stratified sedimentary units along the margin slope as well as poorly reflective and highly deformed sediments of the narrow accretionary prism (between model distance 0 to 25km)

L. 218 : At the start → To start with

Table 1 : justified → Yes, we think this is justified if the person who is modelling bears in mind to not overfit the deeper layers.

Fig. 6 : What is the white ? < 0.1 ? White corresponds to zones not imaged by seismic rays, but constraint by gravity modeling.

Fig. 6 : the squares mean two different things in each graph and the notion of ray hit count for each node is a bit weird, why no square on some interfaces? If this is linked to MCS constraints, please remind it in the caption

We agree that the squares add additional complexity to the figure, given that some constraints are given from the use of reflection seismic data. Also they do not add highly important information, and uncertainties are easier explained with the Monte Carlo modelling presented also in the manuscript. So we deleted the misleading squared from the figure.

L. 168 : well, not totally. I think this is an excessive statement. Yes, we agree, so we reformulated to : They have the advantage of being less biased from the interpreter’s preferences than other error estimation methods

L. 283 : which can be criticized regarding monte carlo methods always leading to the prefered model! However, I know this is a standard asked by many journals now, so... ok. We agree.

Figure 7 : the number of layers is already an a priori. We agree.

L. 295 : reference ? We added 2 references : Christenson and Mooney, 1995, Ludwig, Nafe and Drake, 1970, and Hamilton, 1978.Christensen, N. I., & Mooney, W. D. (1995). Seismic velocity structure and composition of the continental crust: A global view. Journal of Geophysical Research: Solid Earth, 100(B6), 9761-9788.

Hamilton, E.L., Sound velocity-density in sea-floor sediments and rocks, J. Acoust. Soc. Am., 63, 366-377, 1978.

Ludwig, W. J., Nafe, J. E., & Drake, C. L. (1970). The Sea, Vol. 4, Part 1.

Figure 8: is it ok to underestimate densities compared to experiments? Yes, because we wanted to explore, if serpentinites, which have a lower density than gabbros might better fit the data.

L. 330 : I have a question regarding this: what is the percentage of serpentinization needed to have a lower density of the crust? We have added the following phrase : “The modelled density change would correspond to an increase of 10-20% of serpentinisation, but will not account for density or temperature effects.”

Figure 9 : does it make sense to de-hydrate so soon? We took this estimation from numerical modelling by Lars Rüpke and coauthors and we now added this to reference it :

Rüpke, L. H., Morgan, J. P., Hort, M., & Connolly, J. A. (2004). Serpentine and the subduction zone water cycle. Earth and Planetary Science Letters, 223(1-2), 17-34.

However, starting dehydration at a greater depth will have very small effects on our gravity model.

Figure 10 : how is the kink constrained ? What is the explanation?

yes but why exactly? If the kink in the slab dip is well constrained why not discussing it more? Is it a priori from other studies or not? And L. 472 yes but why exactly? If the kink in the slab dip is well constrained why not discussing it more? Is it a priori from other studies or not?

We are not sure we understand the question. No a-priori information was added to the model. To the geometry of the slab and its relation to seismicity we addred the reference to Laurencin et al., Reference :

Laurencin, M., Graindorge, D., Klingelhoefer, F., Marcaillou, B., & Evain, M. (2018). Influence of increasing convergence obliquity and shallow slab geometry onto tectonic deformation and seismogenic behavior along the Northern Lesser Antilles zone. Earth and Planetary Science Letters, 492, 59-72.

Figure 11 : where are these in the arc? Please add a map!! The map has been added.

Authors' Reply to Reviewer 2

Section A: Overview of manuscript

A1) Overall evaluation, general comments & summary

A1.1) Reviewer's comments

A1.1.1) General evaluation and publication suggestion – Required:

Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.

The manuscript details a 2D valocity model across the Northern Lesser Antilles obtained from forward modeling of refracted arrival times and joint analysis of multichannel seismic reflection (MCS), in an area where the subducting oceanic crust was formed at an amagmatic spreading center. The profile is compared to existing profiles south and north-west of the area and shows a similar arc crust. An oceanic crustal structure compatible with a high serpentinized peridotite content is in agreement with the low seisimicity of this subduction zone segment, which is explained as the result of fluid dehydration flowing upward through the plate interface. This manuscript is suitable for publication in Tektonika but more work is necessary to better demonstrate how the new data exactly confirms or improves the final interpretation that was already derived in other papers. This suggestion would greatly increase the impact of the paper

First of all, we would like to thank the reviewer for a detailed and very constructive review. We are very sorry to not have seen the comments in our first resubmission. The responses to each point are given in blue directly after the reviewer comments. We felt this would be more clear. As wide-angle seismic models are non-unique by nature, we have added conclusions from heat-flow and bathymetric data sets to additionally constrain our findings. For this we have added text and 2 figures. Our different error estimations show how well each region of the model is constrained. We also added a description of the seismic phases used in our study for the different layers. We hope this revised manuscript convinces the reader that based on these different datasets the nature of the downgoing slab is varying from our study region southward. We then, in the discussion section propose this can be explained by differences in the fluid content of the crust dues to its serpentinite content and how this might interfere with seismicity.

A1.1.2) What does the submission need to be publishable? (select as needed; comment for all cases)

- ☐ No changes required
- x Rewriting
- x Reorganising

x More data/figures
☐ Condensing
☐ Reinterpretation
x Other:

Comments:

Sections numbering are almost impossible to understand. There is even a numbering reset (Section 3 is the first section, then 4, then 1) and the reader is unsure whether some parts are sections or subsections. The text therefore needs to be reorganized. Why not including a “methods” section?

We corrected the section numbering.

More details are needed on the a-priori structures of the velocity modeling and fit to the data. In particular, the paper should show very clearly how the data is better modeled by a unique oceanic crustal gradient, and how the slab dip “kink” at km 90 is constrained.

We do not think the “kink” is highly constrained as it is located in a deep part of the model, and it does not present substantial new information. We have taken this out of the discussion

A1.1.3) Can the submission be improved by reducing/adding any of the following? (select as needed; comment for all cases)

☐ Text
☐ Table
x Figures
x Supplementary material

Comments:

Additional ***Figures are needed to see details of the oceanic crustal structure fit to the data. In the main paper or in supp. Mat.The text needs to be reorganized***

We have added two figures, one presenting a reflection seismic profile characterised by high amplitude crustal reflection in the downgoing slab and one comparing fluid extrusion feature in both regions. We also reorganised the corresponding text and added two paragraphs.

A1.1.4) Please complete the following section if you recommend that the submission is NOT appropriate for publication (select as needed; comment if a box is selected)

☐ Quality is poor
☐ Research is not reproducible

☐ Other

Comments:

A1.2) Author(s) Responses:

We hope that the revised manuscript more clearly presents the importance the results from our wide-angle seismic modeling, its contribution to better understanding the seismicity in the study region, and its connection to existing studies. The unique contribution a wide-angle seismic data is the calculation of seismic velocities which allow to propose a lithology of the different layers in the subsurface.

A2) Summary of main merits and main points of improvement

A2.1) Reviewer's comments

Please describe below in a few sentences (100 to 300 words) the main merits of the submission and suggestions for improvements.

The main merits I have found are...

The manuscript figures are clear and the amount of work to build the model is very significant.

Thank-you !

The main points of improvement I have found are...

The introduction is followed by two sections numbered 3 and 4 (while Introduction has no number) and these sections also look like introduction, with sometimes an added discussion "flavor". I think it indicates the difficulty for the authors to explain what their profile exactly brings when compared to previous studies like, for instance, Marcaillou et al. (2021). I think that the authors should emphasize the role of refraction studies in constraining, beyond structural analysis, the nature of rocks and hydration through velocity analysis. The introduction should much further highlight the importance of the specific location of the profile and how it will complement the other existing profiles. The information is mostly there, but currently the introduction and additional sections (objectives and previous studies) are disorganized and look like too much like lists of informations with a lack of a central thread.

We have corrected the numbering and reworked the introduction. As proposed by the reviewer we added an explanation :

"The main objective of this study was to image seismic velocities in a region were reflection seismic data imaged height amplitude reflectors dipping towards the Mid-Atlantic Ridge in the Lesser Antilles. Although published reflection seismic data (*Maracaillou et al., 2021*) constrain well the geometry of the subduction here, wide-angle seismic data bring additional information about the nature of the crust. Although other profiles exist (*Laurencin et al. 2018*), the profile we present is up to day the only profile located in this region of unusual oceanic basement. It is has been acquired in the Northern Lesser Antilles, to the north-west of Barbuda Island, along the 200-km-long wide-angle and reflection seismic line AN06 spanning from the trench to the inner forearc."

Regarding the results and methods, although it is useful, the Monte-Carlo uncertainty estimations should be more carefully introduced. The method does not totally solve the issue of a-priori input and non-uniqueness in forward models (neither in forward modeling nor in tomography studies, to be fair). For instance, the number of layers is already an a-priori that is not varied in Vmontecarlo (to my knowledge). Monte-Carlo approaches often perturb the models only around the preferred model, in both forward and inverse methods. What I think is lacking in the manuscript is to show the data in order to convince the reader that the more important features of the model can be supported by the data: what exactly in the data constrains the single oceanic crustal gradient? Which layers are constrained with more than wide angle reflections? What data exactly constrain the sudden slab dip change in the model?

A2.2) Author's responses:

We certainly agree with the reviewer and have added the following cautioning phrases: "However, some parameters are generally not varied by Monte Carlo methods such s the number of layers. Instead they perturb around the pre-defined preferred models, and are thus not completely user-independent."

All data are freely available to the highly interested reader in a SEANOE data depository, so we think adding more figures with data sections to the 6 representative ones is helpful, and might lengthen unnecessarily the manuscript.

Section B: Detailed evaluation of manuscript

B1) Title and abstract

B1.1) Reviewer's comments

*These statements are a **guide** to what good* Titles *and* Abstracts include*. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Title* describes the main topic of the manuscript **accurately** — [Not really]

Although I am not sure if the focus on the amagmatic oceanic crust really is included in "Island arc". If we strictly consider the island arc structure, there is barely any lateral variation in all the profiles. The difference is in the slab properties

Yes, this is true so we changed the titel to :

“Lateral evolution of the deep crustal structure of the Lesser Antilles subduction zone from wide-angle seismic modeling.”

The *Title* describes the main topic of the manuscript **succinctly** — [yes]

The *Title* includes **appropriate key terms** — [No]

Please see first remark

We changed the title accordingly

The *Abstract* includes a **clear aim and rationale** — [NO]

The abstract states that previous studies show properties that this study also show, but it does not explain why it has an added value.

Yes, that is true so we added :

“This study gives insight into lateral changes of the Lesser Antilles subduction zone by comparing the new dataset to existing wide-angles seismic profiles, and into the lithology of the subducting oceanic crust by gravity modelling and comparison to heat flow measures.”

The *Abstract* supports the rationale with **sufficient background information** — [YES]

The *Abstract* includes a **well-balanced description of the methods** — YES

The *Abstract* describes the **main results sufficiently and adequately** — YES

The *Abstract* clearly describes the **importance/impact of the study** — [NO]

See the above point about clear aim and rationale

The *Abstract* clearly states the **conclusions of the study** — [YES]

The *Abstract* is **clear** and **well structured** — [YES]

Yes, but there the issues of the introduction are also in the abstract

Comments:

B1.2) Author’s responses

B2) Introduction

B2.1) Reviewer's comments

*These statements are a **guide** to what good* Introductions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [YES]

The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [NO]

We have reworked the introduction to better show the two main objectives of this work. The main aim of this study was to image seismic velocities in a region were reflection seismic data imaged height amplitude reflectors dipping towards the Mid-Atlantic Ridge in the Lesser Antilles.
A second objective is the comparison of the structures found along this profile to other wide-angle seismic profiles and observe variability of the structures in the fore-arc.

The *objective/hypothesis/rationale* **flows logically from the background** information — [NO]

Please, see the above point about clear aim and rationale

The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES] and [NO]

We have reworked the introduction to better show the two main objectives of this work.

The *Introduction* contains **relevant, suitable citations** — [YES]

The *Introduction* is **organized effectively** — [NO]

We have reworked the introduction and corrected the section numbers.

Comments:

As already stated in the general comments, the strange "sections" 3 and 4 (objectives and previous work) are expected in the introduction itself, and the rationale should flow from it. In its current form, the "composite" introduction is somewhat repetitive, include elements that could be discussion and lacks rationale.

B2.2) Author's responses

We hope our restructured introduction gives more flow to the manuscript and better presents the main objectives

B3) Data and methods

B3.1) Reviewer's comments

*These statements are a **guide** to what good* Method *sections include and good practices for* Dataset *accessibility. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Methods* are described **concisely and with enough detail** for reproducibility — [YES]

Necessary information about **data sources/acquisition/processing** is included — [YES]

Data used are accessible via either supplementary files or links in the data availability statement — [YES]

The *Dataset and/or Methods* are **organized effectively** — [NO]

Comments:

The Methods are not sufficiently detailed before the results section. I think the methods should be in the Dataset and methods section and should first describe the refraction and Wide Angle reflection phases and how they constrain the most important layers. In particular, the dataset section should show how the oceanic crustal structure is constrained and how the deep a priori structure is built (the shallow constraints from MCS data are well described but what are the a priori at depth? Is the slab kink an apriori? If not how is it constrained?). Then the results sections can focus more exclusively on velocity gradients and error analysis (more or less as it is)

B3.2) Author's responses

We added a short text on the phases we picked in the data, detailed numbers of picks per phase and RMS errors are given in the Table 1. We think that the Monte-Carlo inversion and other error estimations give a good overview of how well layers are constrained. Again, we do not think that the "kink", which is located at a depth where the layer geometry starts to depend on mainly reflected phases, is highly constrained. So although necessary to fit our data, we do not draw conclusions on it.

Added description of the phases :

"Among the phases used in this study are the main refracted p-wave arrivals from the oceanic crust, the fore-arc sediments and crust and the upper mantle. Reflected arrivals were picked for all sedimentary and crustal layers. However, once the oceanic crust penetrates at depths larger than 20km its geometry is mainly modeled from reflections from the top and bottom of that layer. Arrivals from the seismic reflection data were used model the detailed layer geometry in the sediments. No s-wave arrivals were used in this study due to the poor quality of the arrivals.“

B4) Results

B4.1) Reviewer’s comments

*These statements are a **guide** to what good* Result sections *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Results* findings are **supported by data** — [NO]

By adding the heat-flow data and seafloor morphology, we additionally constrained the main point of our results, that the oceanic crust in our study region differs from the one in the southern Lesser Antilles. We added two new figures and explanations to the manuscript.

The *Results* findings are presented **clearly and succinctly** — [YES]

The text in the *Result* section **cites tables and figures appropriately** — [YES]

The *Results* directly **relate to the study objectives** — [YES]

The *Results* present **data for all the approaches** described in the *Methods* section — [YES] / [NO]

See above comment on : The Results findings are supported by data

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion.* — [YES]

The *Results* section is **organised effectively** — [YES]

Comments:

This section is well done, but the authors could show much more the key features of the data supporting the main modeling results in a method section before this results section.

B4.2) Author’s responses

We have added a description of the phases as proposed, and also a panel of the seismic reflection data.

B5) Discussion and conclusions

B5.1) Reviewer's comments

*These statements are a **guide** to what good* Discussions *and* Conclusions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Discussion* is **focused on the objectives** of the study — [YES]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [YES]

The *Discussion* section properly identifies all **speculative statements** — [YES] / [NO]

We did rework the discussion and did delete some of the rather speculative statements in the section 6.2 (along arc variability of the structure of the northern Lesser Antilles fore-arc crust.

The *Discussion* section presents the **implications of the study** persuasively — [NO]

The *Discussion* section **highlights novel contributions** appropriately — [NO]

The *Discussion* section **addresses the limitations** of the study appropriately — [YES] / [NO]

The biggest limitation of this study is, that the wide-angle seismic profiles do not reach far onto ocean crust. On one hand we additionally constrained our propositions with knowledge from heat-flow and bathymetric data and on the other hand we added some text about the limitations of our study.

"However, both profiles in this comparison do not reach far onto oceanic crust, which might show a high variability, and precision of the wide-angle seismic data below 15-20 km is naturally less accurate than in the region were oceanic crust is at smaller depths. These two points might hinder us to precisely constrain the size of the Jacksonville patch along our profile, but the additional constraints from other datasets clearly show the difference in nature between the two regions."

The *Discussion* section is **organised effectively** — [YES]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES]

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES]

The *Conclusions* are **clear and concise** — [YES]

Comments:

The discussion is easy to read and informative. However, it is difficult to extract the novelty of the results and how these improve previous interpretations: Why was it important to have a new refraction velocity model at this particular location (in intro, but it should be then discussed again here)? How exactly does this new dataset answers limitations of previous interpretations? In its current form, the discussion shows how the new results are in agreement with previous results for the incoming plate and for the arc (with no link between them), with no further implications. The slab geometry dip change in the model is cited but not really discussed, and I think it really should.

B5.2) Author's responses

We have added three paragraphs to the discussion, to make a comparison to other existing wide-angle seismic profiles (Davy et al., 2022), to explain the relationship between our model and heat-flow measurements from the region and bathymetric data. We then also discuss possible interaction between the fluid content and the seismicity of the region. Although this has been proposed earlier, and also for different regions, we think that our combined study of unpublished wide-angle seismic data, heat-flow and bathymetry gives additional constraints on this relationship. Following a proposition of the first reviewer we added a rough estimate of a possible degree of serpentinisation of the oceanic crust, a results that can not be deduced from seismic reflection data only.

B6) Figures, tables and citations

B6.1) Reviewer's comments

These statements are a ***guide*** *to what good* Figures *and* Tables *include and how they are presented. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

Tables and *Figures* are **ordered logically** and **numbered sequentially** — [YES]

Tables and *Figures* have **captions that explain** all their major features — [YES]

Tables and *Figures* have **captions that complement** the information in the main text — [YES]

Tables and *Figures* present data that **relate** to the study objective — [YES]

Tables and *Figures* present data that are **consistent** with and support the description of results — [YES]

Tables and *Figures* have **succinct and informative titles** — [YES]

Figures are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc.…) — [YES]
Please, check our [Figure guidelines]

Figures with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — [YES]

Figures with **maps** have **sufficient location information** (in the map or caption) — [NO]

We have added location information to figures 1 and 13 (former Figure 10).

Cross-sections have clear labels for **scale and coordinates** at ends and within-section kinks — [YES]

All georeferenced elements are provided in common format (.shp, .geotiff, .kml) [in an open-access repository] — [YES] / [NO]

OBS positions will be published along with the OBS data in a Seanoe depository as a text file.

https://www.seanoe.org/data/00455/56629/

Citations throughout are relevant, suitable, and comprehensive — [YES]

Comments:
All locations discussed in the introduction are not found in the maps. Also, a map would be necessary in the discussion, at least as insets of Figures 10/11 because the reader is lost and it is essential to the clarity of the comparison. For instance, ANTITHESIS 5 is not shown in the Figure 1 map, and even it was, a reminder would be necessary to locate all the discussed profiles

B6.2) Author's responses

We have added this information indeed necessary to the reader in Figures 1 and 13 (former Figure 10).

2nd Round of Revisions

Decision Letter

Both reviewers have evaluated the revised manuscript and while they both agree that it has been much improved, they still have concerns about certain aspects of the model, such as the modelling of the oceanic crust, consideration of uncertainties in the interpretation, and comparisons with other models. Some reorganizing of the introduction has also been requested. Given that both reviewers are echoing similar concerns, we feel that another round of revision is warranted. We look forward to your revised manuscript.

All the best,
Kim Welford and Jack Williams

Comments by Reviewer 1

Below are my comments to five of their responses that illustrate my main criticisms. I have kept their font coloring for clarity: black for my original comment, blue for their response, and green for my new comment.

#1

Line 28. "Oceanic crust... comprises one single layer". It is not demonstrated that this is required by the data; seems more like a consequence of simplification in the modeling procedure.

Not only would we expect a reflection from the top of a second layer, also the data do not show any change in velocity gradient. We used the minimum-parameter/minimum structure approach, to avoid inclusion of velocity or structural features into the model unconstrained by the data, and this led us to model the crust as one single layer. We added this explanation to the text.

I find this response inadequate. A layered oceanic crust does not necessarily imply that a reflection from the top of a second layer is expected. Layering can occur by changes in vertical velocity gradient without a discontinuity in Vp. Authors claim that "*data do not show any change in velocity gradient*"

. Then please show the data that support such claim.

As I pointed out in my original review, none of the ray diagrams and record sections in Figure 4 show oceanic crustal refractions. Table 1 indicates that 583 picks for turning rays within the subducting oceanic crust were used in the modeling. Why are authors not showing the data, fits, and ray paths for this phase if it is the one most relevant for the main conclusion of the paper (that the crust corresponds to amagmatically accreted lithosphere)? The statement below that readers can freely download the dataset and assess it by themselves is disingenuous at best. It is the responsibility of the authors to present the data that support their main conclusions, not of the readers to download and reanalyze data.

#2

#3

Figure 3. This is the only figure where the wide-angle data is shown. Quality of figure is poor. Authors should include a supplement with records sections at larger scale.

Unfortunately the data figures in most manuscripts are small. However, the data are freely available to the interested reader. (https://www.seanoe.org/data/00455/56629/)

See my comment above. Nothing prevents the authors from showing a zoomed-in of a record section where the oceanic crustal refractions are observed and showing a raypath diagram for oceanic crustal refractions in the model.

Line 352. Here it is claimed that lower crustal velocities at AN06 are ~0.2 km/s higher than forTRAIL. But 0.2 km/s is well below the uncertainty estimates show in Fig. 7, which are 0.6-0.9 km/s. Also, authors need to explain how are the locations of the velocity-depth profiles chosen. Since the velocity in the oceanic crust increases down-dip, how would the interpretation be different if the profiles are taken closer to the trench where velocities are lower? And in the same paragraph it is indicated that a constant velocity gradient for the oceanic crust in AN06 provided the best fit to the data. But this was a modeling choice; nowhere in the manuscript the authors show evidence that such single-gradient crust is required by the data or discuss other possible structures consistent with the data. For example, none of the record sections and raypaths shown in Fig. 3 include oceanic crustal refractions. This phase is the only one that can accurately constrain velocities in the oceanic crust. If the crustal velocity structure is primarily constrained by Moho reflections, as Fig. 3 seems to indicate, then there are huge trade-offs between average crustal velocity and thickness. These aspects are important because they are the basis for the main conclusion that the down-going slab is primarily serpentinized peridotite. I do not think such conclusion if supported by the evidence shown in the manuscript.

As we explained earlier, the one-layer crust of the oceanic crust is a result from combining forward modeling and minimum-parameter/minimum structure approach. We are showing the main constraint on the model, but this remains a model and can therefore be questioned. So instead of adding more error calculations we have added other data (seismic reflection, bathymetry and heat flow) which confirm our hypothesis, that the downgoing crust is of a different nature along AN06 and THALES P1.

I find this response inadequate. What is the point of conducting the Montecarlo uncertainty assessment if this information is not used in the interpretation? At the minimum, the authors should show the 1D profiles in Fig. 11E,F,G with +/- error bounds extracted from Fig. 8 so readers can assess if velocity differences between AN06 and TRAIL P2 are significant or not.

#4

Line 360. I find the statement that "Thicknesses and velocities in the AN06 model fit better to the outline of an upper mantle layer exhumed at the ocean floor" unconvincing. Red profile in Fig. 10C could be interpreted as well as falling within the light blue region, particularly given the 0.6-0.9 km/s estimated uncertainties.

We have added more evidence for the existence of fluids in this part of the subductions zone, in form of reflection seismic and bathymetric data showing clear differences of our two study regions.

Adding more evidence from independent studies to support their hypothesis is fine, but it does not change the fact that the statement that "*Thickness and velocities in the AN06 model fit better to the outline of an upper mantle layer exhumed at the ocean floor, while the TRAIL P2 profileshows a better agreement with the outline of normal magmatic oceanic crus*t" is unsupported if Vp uncertainties are taking into consideration.

#5

Lines 386-387. The statement that the AN6 results show that the Jacksonville patch extends down to mantle wedge depths is not supported by the data shown in the manuscript. As mentioned in my earlier comment, without showing that crustal refractions constrain well the oceanic crustal velocity structure at those depths, the statement lacks evidence.

We added a figure showing the deep reflectors in the downgoing ocean crust in the Jacksonville Patch to demonstrate its extend (Figure 2B).

The new figure 2B does not show the Jacksonville Patch extending into the mantle wedge beneath the forearc; it shows oceanic mantle reflectors beneath the frontal accretionary wedge. Thus, the statement in lines 440-441 remains unsupported by the AN06 WAS data and model.

Other minor comments:

Fig. 2B shows profile AN43, but label in Fig. 1B says it is AN44. Please correct.

111. Typo “th”

.

141. Typo “profil”

.

307. Typo “such s”

.

Please explain why oceanic crustal velocity is not considered in the resolution and error calculations (i.e., there are not red circles along the top and bottom of the oceanic crust layer in Fig. 7a).

Fig. 4D. Why are the ray and data fit diagrams not showing information for all of the colored phases shown on this record section?Fig. 7. Caption for panels b, c, d indicates “depth nodes (squares with size relative to…

”

However I cannot see any squares in panels b, c, d. Please reword caption or fix figure.

.

Fig. 11. Labels for blue arrows in panels A, C indicate “inset E/F”

. How about inset G?

Comments by Reviewer 2

The revised manuscript has been improved, however, I think there is more work to be done before it can be published in Tektonika. Please find below the main topics to improve/clarify:

A) Introduction: now that it is clearer, after renumbering, that the introduction does include some subsections, I insist on the fact that these do not help the introduction to be "straightforward" enough. I think there should be no subsections in the intro. Especially, I am puzzled by an introduction finishing by "previous work". The previous work could be earlier in the state of the are part of the intro, and if it is too long then some geological/geodynamical/regional background informations could be moved to a subsequent section after the introduction.

The objectives should be at the end of the intro and it is not necessary to announce them.

B) Discussion: One of the main criticism made in the first review was that the data needed to support more directly the main conclusion about the hydration of the plate around this profile. The addition of heat flow data helps strenghten the discussion but two issues remain, in my opinion, regarding the oceanic crustal velocities:

1) I understand that, in AN06 profile, the crustal velocities are mainly constrained by wide angle reflections, explaining why one Velocity gradient can be constrained. In TRAIL P2, the oceanic crust is modeled with two layers but how are they constrained? Are both gradients constrained? Would it be possible to model TRAIL P2 oceanic crust with also one gradient only? Since AN06 and TRAIL P2 are chosen to be representative of the structure within and away from Jackson Patch, I think it is important to show exactly what in the data leads to the different oceanic Velocity structure rather than just presenting TRAIL P2 model as different. When comparing with reference oceanic structures, of cours the two layer model will match a normal crust much more easily.

2) ANO1 profile also shows an oceanic structure with one layer/ one gradient. It is, if I understand well, out of the area where the oceanic crust is known to be formed at an amagmatic spreading center. If we compare AN01 and AN06, would we find a clear Velocity difference? Would gravity modeling indicate less serpentinization there? This would support the conclusions much more than the comparison with the TRAIL P2 two layer oceanic model, at least as long as we do not know how these two layers are constrained.

C) English: Many sentences, especially in the new text, have grammar problems (no subject, absence of punctuation…). Please carefully reread the text to make sure the meaning of sentences is clear.

In conclusion, I encourage the authors to do another revision as it is important to publish this dataset. I hope these comments can help.

line 99: no subject in the sentence (is "heat flow measurements" missing?)

lines 321-322: "instead they perturb the predefined…" I would delete this sentence. Although I agree, some people will argue that this is why minimum structure preferred model from tomography is neeeded…

Figure 11: it would be much easier to have a blue Arrow indicating the TRAIL P2 vz profile and a red Arrow indicating the AN06 vz profiles. The labels should be insets E/F/G.

line 478: "and lower than values in the trench"

line 484: "Finally… (Figure 12)": there is a grammar problem, I do not understand the sentence. comas missing?

line 487: "but": I'm not sure I understand. And how were these features created exactly, how is it related to cold fluid inputs?

In this part about the heat flow, fluid input in the crust is justified, but not fluid outputs affecting the seismicity. So this information is important to justify serpentinization and effects on seismic velocities, but not the dewatering.

Line 524: "similar structure": well, if they are so similar, then the amagmatic oceanic crust is also to the north? I think it would be important to see that AN01 oceanic Velocity is lower than AN06 right? (see main comment)

line 572: "high Velocity". A comparison between AN06 and AN01 would show that even more clearly?

Authors' Reply to Reviewer 1

Reviewer A:

Recommendation: Revisions Required

Below are my comments to five of their responses that illustrate my main criticisms. I have kept their font coloring for clarity: black for my original comment, blue for their response, and green for my new comment. We added our answers in mauve colour.

#1

Line 28. "Oceanic crust... comprises one single layer". It is not demonstrated that this is required by the data; seems more like a consequence of simplification in the modelling procedure.

Not only would we expect a reflection from the top of a second layer, also the data do not show any change in velocity gradient. We used the minimum-parameter/minimum structure approach, to avoid inclusion of velocity or structural features into the model unconstrained by the data, and this led us to model the crust as one single layer. We added this explanation to the text.

I find this response inadequate. A layered oceanic crust does not necessarily imply that a reflection from the top of a second layer is expected. Layering can occur by changes in vertical velocity gradient without a discontinuity in Vp. Authors claim that "data do not show any change in velocity gradient". Then please show the data that support such claim. As I pointed out in my original review, none of the ray diagrams and record sections in Figure 4 show oceanic crustal refractions. Table 1 indicates that 583 picks for turning rays within the subducting oceanic crust were used in the modeling. Why are authors not showing the data, fits, and ray paths for this phase if it is the one most relevant for the main conclusion of the paper (that the crust corresponds to amagmatically accreted lithosphere)? The statement below that readers can freely download the dataset and assess it by themselves is disingenuous at best. It is the responsibility of the authors to present the data that support their main conclusions, not of the readers to download and reanalyze data.

Wide-angle seismic models are inherently non-unique as are gravity models. However, deep crustal structure stays out of reach of direct sampling efforts, so an approach of combining different modelling techniques might in our view be the most promising way to image the deeper layers of the Earth. As both reviewers again express their impression that our model is unconstrained, we have additionally undertaken tomographic inversion of the first arrivals using the FAST code of Colin Zelt. As no information from secondary arrivals, synthetic modelling or gravity modelling have been used for this model, we chose to notpresent this as our final model, but just to present it to clarify that automatic, "black box" like approaches will lead to similar results. It is presented in the electronic supplements part of the manuscript, and we show it here for information:

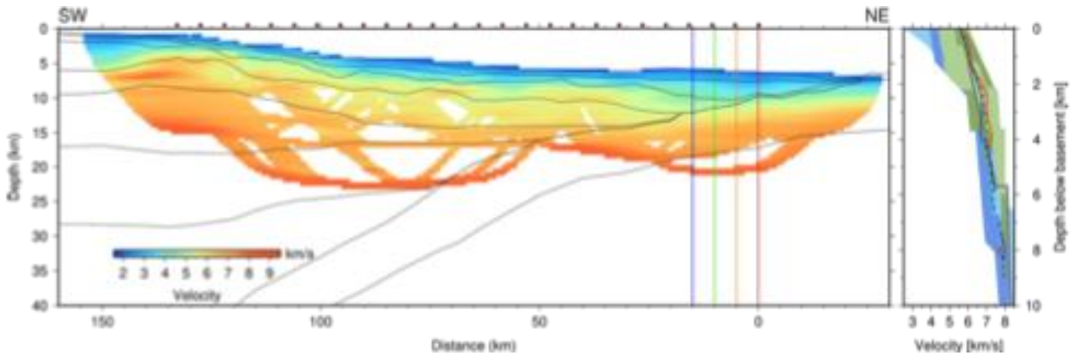

Figure 1: Velocity depth profiles extracted from a tomographic model of all first arrivals, using the FAST (Zelt and Barton, 1998) software. We then included the velocity-depth profiles from this automatic tomography for information of the reader into Figure 11 of the manuscript.

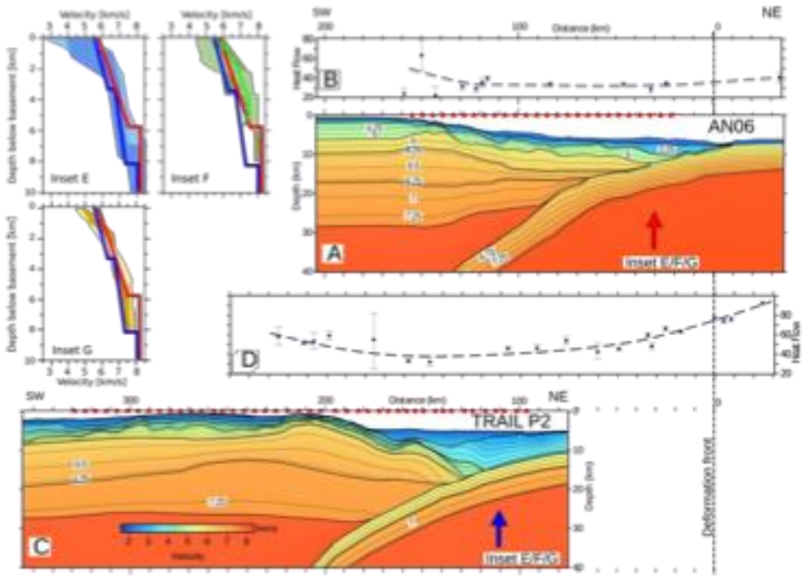

Figure 2: (A) and (C) Wide-angle seismic models for line AN06 and the TRAIL P2 profile (Kopp et al., 2011) respectively, contoured at a 0.25 km/s interval. The position of the deformation front is marked by a green dashed line. The blue arrows correspond to the position of the vz-profiles shown in the insets (E), (F) and (G). (B) and (D) Heat flow values from Ezenwaka et al., 2022. For location see Figure 1. Insets: velocity-depth (vz) profiles extracted beneath the basement for the ANTITHESIS (red lines) and TRAIL P2 (blue lines) profiles. . (E) Vz-profiles extracted from the oceanic crust region and comparison to typical Atlantic (144-170 Ma) and Pacific (29-140 Ma) crust (blue polygons - White et al., 1992). (F) Vz-profiles extracted from the oceanic crust region and comparison to velocities of exhumed upper mantle (light green polygon - Dean et al., 2000, dark green polygon - Van Avendonk, 2006). The green line marks the velocity distribution from tomographic inversion of first arrivals. The complete model is shown in electronic supplement S1. (G) Comparison to segment centers and segment ends from Davy et al., 2020.

Figure 2: (A) and (C) Wide-angle seismic models for line AN06 and the TRAIL P2 profile (Kopp et al., 2011) respectively, contoured at a 0.25 km/s interval. The position of the deformation front is marked by a green dashed line. The blue arrows correspond to the position of the vz-profiles shown in the insets (E), (F) and (G). (B) and (D) Heat flow values from Ezenwaka et al., 2022. For location see Figure 1. Insets: velocity-depth (vz) profiles extracted beneath the basement for the ANTITHESIS (red lines) and TRAIL P2 (blue lines) profiles. . (E) Vz-profiles extracted from the oceanic crust region and comparison to typical Atlantic (144-170 Ma) and Pacific (29-140 Ma) crust (blue polygons - White et al., 1992). (F) Vz-profiles extracted from the oceanic crust region and comparison to velocities of exhumed

upper mantle (light green polygon - Dean et al., 2000, dark green polygon - Van Avendonk, 2006). The green line marks the velocity distribution from tomographic inversion of first arrivals. The complete model is shown in electronic supplement S1. (G) Comparison to segment centers and segment ends from Davy et al., 2020.

#2

Figure 3. This is the only figure where the wide-angle data is shown. Quality of figure is poor. Authors should include a supplement with records sections at larger scale.

Unfortunately the data figures in most manuscripts are small. However, the data are freely available to the interested reader. (https://www.seanoe.org/data/00455/56629/)

See my comment above. Nothing prevents the authors from showing a zoomed-in of a record section where the oceanic crustal refractions are observed and showing a raypath diagram for oceanic crustal refractions in the model.

We really do think that opening all data free to zoom and model is the best way to make science transparent, rather than multiplying figures by adding zooms of all different features mentioned in the text.

#3

Line 352. Here it is claimed that lower crustal velocities at AN06 are ~0.2 km/s higher than for TRAIL. But 0.2 km/s is well below the uncertainty estimates show in Fig. 7, which are 0.6-0.9 km/s. Also, authors need to explain how are the locations of the velocity-depth profiles chosen. Since the velocity in the oceanic crust increases down-dip, how would the interpretation be different if the profiles are taken closer to the trench where velocities are lower? And in the same paragraph it is indicated that a constant velocity gradient for the oceanic crust in AN06 provided the best fit to the data. But this was a modeling choice; nowhere in the manuscript the authors show evidence that such single-gradient crust is required by the data or discuss other possible structures consistent with the data. For example, none of the record sections and raypaths shown in Fig. 3 include oceanic crustal refractions. This phase is the only one that can accurately constrain velocities in the oceanic crust. If the crustal velocity structure is primarily constrained by Moho reflections, as Fig. 3 seems to indicate, then there are huge trade-offs between average crustal velocity and thickness. These aspects are important because they are the basis for the main conclusion that the down-going slab is primarily serpentinized peridotite. I do not think such conclusion if supported by the evidence shown in the manuscript.

As we explained earlier, the one-layer crust of the oceanic crust is a result from combining forward modelling and minimum-parameter/minimum structure approach. We are showing the main constraint on the model, but this remains a model and can therefore be questioned. So instead of adding more error calculations we have added other data (seismic reflection, bathymetry and heat flow) which confirm our hypothesis, that the downgoing crust is of a different nature along AN06 and THALES P1.

I find this response inadequate. What is the point of conducting the Montecarlo uncertainty assessment if this information is not used in the interpretation? At the minimum, the authors should show the 1D profiles in Fig. 11E,F,G with +/- error bounds extracted from Fig. 8 so readers can assess if velocity differences between AN06 and TRAIL P2 are significant or not.

As the reviewer seems to have doubts about the validity of forward modelling wide-angel seismic data and their commonly used error analysis, we hope to convince by additionally showing the above tomographic inversion coming to highly similar models. As also explained above, we prefer to keep our final model, as it includes information from secondary arrivals, gravity modelling ans synthetic amplitude calculations, missing from the forward model.

#4

Line 360. I find the statement that "Thicknesses and velocities in the AN06 model fit better to the outline of an upper mantle layer exhumed at the ocean floor" unconvincing. Red profile in Fig. 10C could be interpreted as well as falling within the light blue region, particularly given the 0.6-0.9 km/s estimated uncertainties.

We have added more evidence for the existence of fluids in this part of the subductions zone, in form of reflection seismic and bathymetric data showing clear differences of our two study regions.

Adding more evidence from independent studies to support their hypothesis is fine, but it does not change the fact that the statement that “Thickness and velocities in the AN06 model fit better to the outline of an upper mantle layer exhumed at the ocean floor, while the TRAIL P2 profile shows a better agreement with the outline of normal magmatic oceanic crust” is unsupported if Vp uncertainties are taking into consideration.

Again, we hope to better convince the reviewer by having calculated a tomographic inversion model and adding the resulting vz-profiles into figure 11.

#5

Lines 386-387. The statement that the AN6 results show that the Jacksonville patch extends down to mantle wedge depths is not supported by the data shown in the manuscript. As mentioned in my earlier comment, without showing that crustal refractions constrain well the oceanic crustal velocity structure at those depths, the statement lacks evidence.

We added a figure showing the deep reflectors in the downgoing ocean crust in the Jacksonville Patch to demonstrate its extend (Figure 2B).

The new figure 2B does not show the Jacksonville Patch extending into the mantle wedge beneath the forearc; it shows oceanic mantle reflectors beneath the frontal accretionary wedge. Thus, the statement in lines 440-441 remains unsupported by the AN06 WAS data and model.

This is true, which is why we carefully stated “probably into the mantle wedge”. To keep to the facts we have deleted this part of the phrase.

Other minor comments:

Fig. 2B shows profile AN43, but label in Fig. 1B says it is AN44. Please correct.

This has been checked and corrected to AN43.

111. Typo “th”. We corrected this.

141. Typo “profil”. We corrected this.

307. Typo “such s”. We corrected this.

Please explain why oceanic crustal velocity is not considered in the resolution and error calculations (i.e., there are not red circles along the top and bottom of the oceanic crust layer in Fig. 7a).

Although an older version of the manuscript exists with squares at the points of velocity nodes, in the version submitted the last time, we avoided these as they tend to confuse the reader. Resolution of the velocities can be studied in the resolution parameter shown in Figure 7d and the Monte-Carlo perturbation Figure 8.

Fig. 4D. Why are the ray and data fit diagrams not showing information for all of the coloured phases shown on this record section?Fig. 7. Caption for panels b, c, d indicates “depth nodes (squares with size relative to…”.

Sorry, the caption belongs to this older version of the figure.

However I cannot see any squares in panels b, c, d. Please reword caption or fix figure.

Fig. 11. Labels for blue arrows in panels A, C indicate “inset E/F”. How about inset G? Where are the red and blue profiles in G taken from?

We corrected the figure caption, sorry about the confusion.

Authors' Reply to Reviewer 2

Reviewer B:

The revised manuscript has been improved, however, I think there is more work to be done before it can be published in Tektonika. Please find below the main topics to improve/clarify:

A) Introduction: now that it is clearer, after renumbering, that the introduction does include some subsections,

I insist on the fact that these do not help the introduction to be "straightforward" enough. Ithink there should be no subsections in the intro. Especially, I am puzzled by an introduction finishing by "previous work". The previous work could be earlier in the state of the are part of the intro, and if it is too long then some geological/geodynamical/regional background informations could be moved to a subsequent section after the introduction. The objectives should be at the end of the intro and it is not necessary to announce them.

We rearranged the introduction to not contain subsections any more. Also the objectives were moved to the end of the introduction and are not announced, as the reviewer proposed.

B) Discussion: One of the main criticism made in the first review was that the data needed to support more directly the main conclusion about the hydration of the plate around this profile. The addition of heat flow data helps strenghten the discussion but two issues remain, in my opinion, regarding the oceanic crustal velocities:

1) I understand that, in AN06 profile, the crustal velocities are mainly constrained by wide angle reflections, explaining why one Velocity gradient can be constrained. In TRAIL P2, the oceanic crust is modeled with two layers but how are they constrained? Are both gradients constrained? Would it be possible to model TRAIL P2 oceanic crust with also one gradient only? Since AN06 and TRAIL P2 are chosen to be representative of the structure within and away from Jackson Patch, I think it is important to show exactly what in the data leads to the different oceanic Velocity structure rather than just presenting TRAIL P2 model as different.

When comparing with reference oceanic structures, of cours the two layer model will match a normal crust much more easily.

Wide-angle seismic models are inherently non-unique as are gravity models. However, deep crustal structure stays out of reach of direct sampling efforts, so an approach of combining different modelling techniques might in our view be the most promising way to image the deeper layers of the Earth. As both reviewers again express their impression that our model is unconstrained we have additionally undertaken tomographic inversion of the first arrivals using the FAST code of Colin Zelt. As no information from secondary arrivals, synthetic modelling or gravity modelling have been used for this model, we chose to not present this as our final model, but just to present it to clarify that automatic, “black box” like approaches will lead to similar results.

2) ANO1 profile also shows an oceanic structure with one layer/ one gradient. It is, if I understand well, out of the area where the oceanic crust is known to be formed at an amagmatic spreading center.

If we compare AN01 and AN06, would we find a clear Velocity difference? Would gravity modeling indicate less serpentinization there? This would support the conclusions much more than the comparison with the TRAIL P2 two layer oceanic model, at least as long as we do not know how these two layers are constrained.

The profiles AN01 and AN03 do not reach sufficiently out onto oceanic crust to be used for a valid comparison.

C) English: Many sentences, especially in the new text, have grammar problems (no subject, absence of punctuation…). Please carefully reread the text to make sure the meaning of sentences is clear.

In conclusion, I encourage the authors to do another revision as it is important to publish this dataset. I hope these comments can help.

We have asked a native speaker to correct our english.

line 99: no subject in the sentence (is "heat flow measurements" missing?)

We corrected this.

lines 321-322: "instead they perturb the predefined…" I would delete this sentence. Although I agree, some people will argue that this is why minimum structure preferred model from tomography is neeeded…

We deleted the sentence.

Figure 11: it would be much easier to have a blue Arrow indicating the TRAIL P2 vz profile and a red Arrowindicating the AN06 vz profiles. The labels should be insets E/F/G.

We changed the arrow colors and corrected the insets.

line 478: "and lower than values in the trench"

This was corrected.

line 484: "Finally… (Figure 12)": there is a grammar problem, I do not understand the sentence. Comas missing?

Sorry, we corrected this incomprehensible phrase to:

"During the cruise we acquired bathymetric data from the oceanic crust to image the variation of its nature and morphology along the trench (Figure 12)."

line 487: "but": I'm not sure I understand. And how were these features created exactly, how is it related to cold fluid inputs? In this part about the heat flow, fluid input in the crust is justified, but not fluid outputs affecting the seismicity. So this information is important to justify serpentinization and effects on seismic velocities, but not the dewatering.

We added more and better explanations :

These edifices were created before the plate entered the subduction and its associated forebulge, and might therefore might be associated to the nature of the accreted crust at the spreading center. Recent models of spreading mechanisms of slow spreading ridges propose the creation of core complexes and ridges close to the axis (Smith et al., 2006; Smith et al., 2008). These have a high serpentinite content and bring fluids into the subduction zone. The serpentine and the fluids might then interact with seismicity during subduction.

Line 524: "similar structure": well, if they are so similar, then the amagmatic oceanic crust is also to the north? I think it would be important to see that AN01 oceanic Velocity is lower than AN06 right? (see main comment)

line 572: "high Velocity". A comparison between AN06 and AN01 would show that even more clearly?

We are sorry, but the profiles AN01 and AN03 do not reach sufficiently out onto oceanic crust to be used for a valid comparison, due to technical problems we had only a short time for acquisition of these profiles.

Recommendation: Revisions Required

3rd Round of Revisions

Decision Letter

Frauke Klingelhoefer, Boris Marcaillou, Muriel Laurencin, Mireille Laigle, Jean-Frederic Lebrun, Laure Schenini, David Graindorge, Mikael Evain, Heidrun Kopp:

We have reached a decision regarding your submission to тектоnіка, "The Deep Crustal Structure of the Lesser Antilles Subduction Zone.".

Our decision is: Accept Following Minor Final Edits

We are satisfied that the reviewer's comments about the non-uniqueness of the wide angle seismic model have been addressed. However, we believe it would be better to introduce the tomographic inversion results earlier in the manuscript (currently at Lines 525-528, where line numbers refer to tracked change version of the manuscript). It could be particularly useful to mention these new results in Section 5.3 why, despite the Monte Carlo-derived uncertainity (~0.6 km/s) of the velocity model, you interpret distinct ~0.2 km/s differences in velocity structure from the TRAIL line.

-Introduction: We recognise that the authors have been given some conflicting advice from Reviewer B on how to structure this, so we sympathise with this. Nevertheless we feel that the section between Lines 132-170 should be moved into a distinct 'Tectonic Setting' setting that is distinct from the introduction. The objective section (Lines 171-190) should be moved above this section so that it is at the end of the introduction. This is also illustrated in the attached annotated pdf file.

-Table 1: Specify figure that the colors correspond to

-Line 365: Specify that the rho symbol indicates density

-Figure 9: If I'm interpreting the plot correctly, the legend text for the star symbol should be for serpentinised crust (i.e. low density), and the triangle symbol should be for unserpentinised crust (i.e. high density)

We look forward to receiving your final version of the manuscript within two weeks. Please let us know if this deadline will be problematic.

All the best,

Kim Welford and Jack Williams

Acceptance Letter

Frauke Klingelhoefer, Boris marcaillou, Muriel Laurencin, Mireille Laigle, Jean-Frederic Lebrun, Laure Schenini, David Graindorge, Mikael Evain, Heidrun Kopp:

We have reached a decision regarding your submission to τεκτonικa, "The Deep Crustal Structure of the Lesser Antilles Subduction Zone.".

Our decision is to: Accept Submission