

Caribbean biodiversity shaped by subduction zone processes along the Lesser Antilles arch

Corresponding Author: Mr Leny Montheil

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

**** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors ****

Dear Mr Montheil,

Your manuscript titled "Caribbean biodiversity shaped by Paleogene subduction zone processes" has now been seen by 4 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

In particular, please ensure that the revised manuscript meets the following editorial thresholds:

****Provide firm and sufficient geological evidence for demonstrating that the islands emerged around 42 Ma and subsided again at 24 Ma. If possible, provide additional geological data to support the results from the GPlates reconstruction.**

****Expand the discussion of your reconstruction by appropriately acknowledging previous advances on kinematic, tectonic, and geodynamic reconstructions of the regional Caribbean geology as pointed out by various reviewers.**

****Expand the justification for your methods and assumptions, and discuss the limitations, uncertainties, and potential biases.**

When resubmitting, please provide a point-by-point to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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**** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first ****

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor
Communications Earth & Environment

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Recommendation (Required): I recommend publication upon addressing comments

Significant: Yes, the paper is a significant advance or contribution.

Supported: Yes

Referencing: Yes

Quality: The organization of the manuscript and presentation of the data is appropriate

Data: Mostly Yes. However, the GPlates static polygons and rotations files are not included in the supplementary materials. This makes difficult the reproducibility of declination predictions.

In this manuscript titled "Caribbean biodiversity shaped by Paleogene subduction zone processes", the authors present an updated paleogeographic reconstruction of the Caribbean and the Northwestern Andes from 50 Ma to the present day, using the GPlates software. The model integrates novel paleomagnetic data collected in the Puerto Rico, Virgin and Northern Lesser Antilles islands (GrANoLA block), which is not implemented in previous reconstructions of the region. The author proposes that shortening and a rapid ~50m sea level drop around the Eocene-Oligocene (associated to Antarctic glaciation) in the GrANoLA islands created a land bridge that connected the islands with South America between 42-38 Ma. This land bridge is referred to as GrANoLA-land, and it is suggested to have enabled the dispersal of terrestrial and freshwater biotas, such as amphibians, landmammals, arachnids, reptiles, fresh water-fish, from South America to the eastern Caribbean islands. The author argues that GrANoLA-land was broke up into a series of islands between 38-24 Ma following regional subsidence triggered by arc magmatism cessation and trench-perpendicular extension.

I believe that the manuscript is well-written, organised and easy to read. Furthermore, the GPlates reconstruction honours most of the available constraints for the Caribbean and the Northern Andes. However, it is not clear the geodynamic mechanisms that explain the sudden cessation of magmatism and subsidence between 38-24 Ma. Also, it is not clear why

the Granada-Tobago basin behaves as a “ductile” block during basin-opening and tectonic escape of the Venezuela ophiolites (48-35 Ma), but behaves as a rigid block immediately after (35-25 Ma). I consider these issues can be addressed after better considering the subduction dynamics occurring beneath the surface and the crust.

The authors provide a comprehensive review of the geological evolution of the Northwestern Andes, Central America and the Caribbean. It also presents a comprehensive compilation of paleomagnetic data and kinematic markers of the region, such as strike-slip and shortening estimations. However, I think it would be useful to include the sea level curve of Miller et al., (2020) that further support the emergence of GrANoLA-land during 42-38 Ma.

It would be beneficial to include a paragraph in the supplementary materials that introduces and briefly describes the content of the two presented sections. This would make the reader follow better the manuscript.

I think the manuscript is a contribution of significance because it documents how the rapid horizontal and vertical displacements caused by subduction processes can rapidly (<10 Ma) create/destroy migration pathways, which can have a profound impact in the evolution of biodiversity assemblages. The study successfully explains how endemic biotas in the eastern Caribbean islands were colonised by South America species in the Eocene. Finally, this work highlights the importance of bringing together the paleo-biological and geological communities, which will be instrumental for the advancement of our understanding of the feedbacks between the biosphere and geological processes in deep time. I believe this work will spark interest in the bio-geo communities regarding the use of paleogeographic reconstructions to unravel the evolution of life on regional and global scales.

Below, I provide specific comments for the authors to address:

Technical comments

It is not clear what reference frame was used in the reconstruction, paleomagnetic or mantle reference frame?. Also, it is not mentioned if the updated reconstruction is integrated to any global tectonic reconstruction. What are the motions used for North America, South America and the Caribbean?. Can you provide the static polygons and the rotation files?. The performed paleomagnetic declination tests cannot be reproduced without the GPlates rotations.

The study showcases how the interior of oceanic plates is not rigid, and how this can result in complex dynamics such as opposing block rotations and in the creation of migration pathways. What are your comments on this?, I believe this is a call to starting the transition to dynamic polygon reconstructions.

169-171 The authors propose a saloon-door style opening for the Grenada-Tobago back-arc basins in the Caribbean plate interior. The text claims that slab roll-back explains the opposite block rotations between the GrANoLA blocks (counter-clockwise) and the Caribbean interior (clockwise), and the extrusion of the Venezuela ophiolites between 50-20 Ma. This is a process that requires the Caribbean interior to break-up and shear into itself, and I don't believe slab roll-back can solely explain this complex process. In Figs. 3C-D, a similar scenario is shown in the Greater Antilles, where the Caribbean plate breaks-up and shears into itself after being “squished” between the Bahamas Banks and South America.

What are your thoughts on the role of the Bahamas Banks in the strike-slip motion of the GrANoLA blocks?

What about additional processes relevant to subduction zones, such as the mantle toroidal flow?. (e.g. Schellart and Moresi, 2013 and Kiraly et al., 2017)

Furthermore, I am surprised that after 35 Ma, the time when lithospheric stretching ceases in the Grenada-Tobago basin, there is no evidence of basin inversion in the back-arc basins. The rationale is that the thin oceanic crust in the Grenada-Tobago back-arc basin should be mechanically weaker than the GrANoLA blocks.

How would you explain the absence of basin inversion during 30-25 Ma?

178-179 Figure 2 is referred in reversed order. In Figure two, (A) displays the configuration of the Caribbean at present day, but the text refers to the present day as (D).

199-200 The paleogeographic maps of Figure 3 show the shortening (Fig. 3A) and subsidence (Fig. 3B) episodes in the GrANoLA block that lead to the creation and destruction of GrANoLA-land. However, the GPlates animation displays continuous back-arc extension and slab roll-back during this time. I suggest including the back-arc spreading centers to Figs. 3A-B for improving visualization and providing a complete picture on the link between biodiversity and subduction processes like back-arc basin extension.

218-223 The author suggests that the GrANoLA blocks submerged and tectonically fragmented during a period of late Oligocene to Miocene regional subsidence, triggered by the cessation of arc volcanism and trench-perpendicular tectonic extension. The sketches in Figure 3A depict active subduction beneath the GrANoLA blocks (i.e. Antigua and Virgin Islands), indicating that subduction continued during arc magmatism cessation. Furthermore, the saloon door-style opening requires continuous slab-roll back and active subduction. Although the text references a similar scenario in Japan (Ref 8), it does not explain why or how arc magmatism ceases between the late Oligocene and Miocene. What mechanism explains this hiatus in magmatism? Is there any sealevel rise at

this time?. Or did the arc just jumped into the Caribbean interior?. Normally, the jump of a magmatic arc occurs in the direction of slab roll-back (eg.g. Izu-Bonin)

Minor comments

663-665 In the Northern South America section of the supplementary materials, the text mentions that Caribbean affiliated terranes are exposed in the Quebradagrande Complex (Central Cordillera, Colombia). However, detailed geological mapping and geochemical (Jaramillo et al., 2017) and geochronological studies conducted in magmatic (Jaramillo et al., 2017) and detrital zircons (Pardo et al., 2020) of the area, suggest that the oceanic rocks of the Quebradagrande Complex are part of a distinct accreted intra-oceanic arc. Can this be clarified in the text?

800 The start of the Hispaniola section of the supplementary materials is missing the topic sentence. It seems the first sentence is the continuation of some other section.

Reviewer #2 (Remarks to the Author):

I was going through the paper by I am supposed to review (Montheil et al Caribbean biodiversity..., and I found a MAJOR PROBLEM.

1. This paper just ignores the two recent contributions which change the perspective they have in his comments about GAARLANDIA's land bridge. In fact there is a couple of papers BY ITURRALDE-VINENT AND MACPHEE demonstrating that the Aves ridge was uplifted during the Middle Eocene to the Miocene. In these papers we also comment about THEIR GRANOLA.

I will just reject such a contribution which is not updated concerning the major facts about this subject.

Also I found that the references are numbered in the body of the paper, but such numbers are not found in the reference list, so is hard to track them.

The MISSING CONTRIBUTIONS are:

---Iturralde-Vinent, M.A., MacPhee, R.D.E., 2023. New evidence for late Eocene-early Oligocene uplift of Aves Ridge and paleogeography of GAARlandia. *Geologica Acta*, 21.5, 1-10.

---Iturralde-Vinent, M.A. y MacPhee, R.D.E., 2024. Remarks on: J.R. Ali and S.B. Hedges, 2024. Paleogeography of the Aves Ridge and its potential role as a bio-colonization pathway linking South America and the Greater Antilles in the mid-Cenozoic. *Revista Maya de Geociencias*, Agosto, pp. 90-95.

2. ADICIONAL PROBLEM:

---Supplementary figure 3, refers as Eocene-Oligocene arc rocks in Sierra Maestra of Cuba. The arc rocks in this location actually are Paleocene.early Middle Eocene.(Rojas-Agramonte, Y., Neubauer, F., Bojasr, A.V., Hejl, E., Handler, R., y García-Delgado, D., 2006. Geology, age and tectonic evolution of the Sierra Maestra mountains, southeastern Cuba: a review. En: Lidiak, E.G., e Iturralde-Vinent, M., (Editores) Geodynamic evolution of the Caribbean region. *Geologica Acta*, 4, (1-2): 123-150.; --Rojas-Agramonte, Y., Neubauer, F., García-Delgado, D., Handler, R., Friedl, G., y Delgado Damas, R., 2008. Tectonic evolution of the Sierra Maestra mountains, SE Cuba, during Tertiary times: From arc-continent collision to transform motion. *Journal of South American Earth Science*, 26: 125-151.; --Rojas-Agramonte, Y., Neubauer, F., Handler, R., García-Delgado, D., y Delgado Damas, R., 2001. Las deformaciones ocurridas en la pendiente sur de las montañas de la Sierra Maestra: evolución tectónica de la falla transformante Oriente. En: IV Congreso Cubano de Geología y Minería, *Memorias Geomin* 2001, La Habana, marzo 19-23.; --Rojas-Agramonte, Y., Neubauer, F., Kröner, A., Wan, Y.S., Liu, D.Y., García-Delgado, D., y Handler, R., 2004. Geochemistry and early Palaeogene SHRIMP zircon ages for island arc granitoids of the Sierra Maestra, southeastern Cuba. *Chemical Geology*, 213: 307-324, doi:10.1016/j.chemgeo.2004.06.031.

Reviewer #3 (Remarks to the Author):

Summary:

This study presents a new Gplates-driven paleogeographic reconstruction of the eastern Caribbean region from the Eocene onwards. The new model improves plate reconstructions by implementing recently published paleomagnetic and other geological data. The main modification is a >45° rotation of the Lesser Antilles forearc in the late Eocene to Oligocene, moving northward from a position near South America toward its present-day location at the northern cusp of the subduction zone. The main conclusion of the study is that the rotation of the forearc means that the Greater and Northern Lesser Antilles (GrANoLA) were much closer to South America than previously thought and, thus, could allow overland migration of terrestrial land mammals to the islands, rather than via an emerged Aves Ridge (GAARLandia hypothesis). This is the first manuscript primarily focused on this idea; however, it has been proposed in Montheil et al. (2023) and as a potential dispersion route in, e.g., Cornée et al. (2023), Fig. 1 in the latter article. Thus, this article represents an expansion of this previously proposed concept through a GPlates reconstruction and visualization. The authors motivate the article by pointing out that previous paleogeographic interpretations, namely the GAARLandia hypothesis, lack geological support and a geodynamic mechanism. However, it is unclear how their interpretation is any more supported by data than the previously

proposed options, nor do they provide any discussion on a geodynamic mechanism, missing key citations to recent geodynamic studies (e.g., van Benthem et al., 2014; Cerpa et al., 2021; Conrad et al., 2024) – the latter of the listed studies also discuss these hypotheses in terms of the implications of their modeling results). The article instead focuses on relatively one-sided and lofty claims about Caribbean biogeography without thoroughly discussing uncertainties, assumptions, implications, or alternative explanations and comparisons with other hypotheses. Notably, the proposed hypothesis only provides a new avenue for mammals to follow but does not provide any evidence for the continuous emergence of GrAnoLA. From my reading, the only support provided are citations to articles that indicate subaerial exposure of areas presently in and offshore Venezuela, even in the supplemental. Solely partial emergence is briefly mentioned as a possibility in the article but should be stated more clearly. There are also quite a few grammatical issues in the paper that affect clarity. For these reasons, which I expand on below alongside additional comments, I recommend that the article be rejected with encouragement for resubmission. However, I want to emphasize that with improved analysis and discussion, I believe this new reconstruction will significantly contribute to helping us better understand the geo- and biological history of the Caribbean realm.

1. On lines 66-67, the authors point out that previously proposed hypotheses are not well supported but do not describe the debate and the shortcomings of each hypothesis. Due to the lack of supporting evidence, they imply that an alternative explanation is required. While providing such an alternative is fine, their presentation makes it seem that this new hypothesis is a leading solution rather than a third alternative amongst two other, still perfectly reasonable solutions. I may be missing something about issues with these models that preclude their viability, but, in this case, it should be described more clearly.

2. On line 66, the authors also write that no geodynamic mechanism has been identified, and later on line 257 say that this paper provides a mechanism. I may have missed something, but I did not see a geodynamic mechanism identified and explained that supports their hypotheses. There is only a discussion on saloon door tectonics. They do not mention that at least three geodynamic mechanisms have been proposed that explain shortening in the northern Lesser Antilles: the effects of an arcuate slab corner (Cerpa et al., 2021), a slab edge push (van Benthem et al., 2014), and slab anchoring leading to subduction at the Lesser Antilles (Conrad et al., 2024), of which, the latter explains that the beginning of LA subduction could explain more continuous emergence and later subsidence along the length of the arc (e.g., Faccenna et al., 1999; Toth & Gurnis, 1998). The discussion would be improved if the geodynamic implications of their model were considered in the context of existing work. I suggest the authors include a deeper discussion and additional references to numerical and analog studies of subduction zone processes and how their proposed model fits within what we can learn from those studies.

3. The article does not acknowledge the uncertainties, assumptions, other possible explanations, and general lack of data to support their claims. This is likely due to limits in available constraints. As mentioned on lines 78-80 and discussed throughout the manuscript, the proposed model hinges on the paleomagnetic data provided in Montheil et al. (2023) and Philippon et al. (2020). The authors write that these data indicate that GrAnoLA moved hundreds of kilometers relative to the Caribbean Plate interior and North and South America. However, they do not mention that this is one end member solution proposed by those authors, including an oroclinal bending end member solution/block rotation in the forearc. Furthermore, paleomagnetic data are not evidence that GrAnoLA land emerged as a continuous strip of land. Thus, while providing an alternative avenue, they do not solve the issue of an overland pathway.

4. Claims such as that on lines 72-74 are not substantiated by data, as there is no clear evidence that the islands were uplifted and formed a “connected and emergent ‘GrAnoLA-land’.” On lines 205 to 209, the authors argue for the subaerial exposure of GrAnoLA, however, critically, evidence for widespread subaerial emergence seems lacking. While sea level fall cited on line 209 is an important consideration, neither of the cited papers in the preceding lines (Deville et al., 2015; Pindell et al., 2009) present data that support widespread subaerial exposure of GrAnoLA. At best, they suggest that portions of the forearc farther south may have been exposed during Oligocene times. Pindell et al. suggest around Serranía, Venezuela, leaving hundreds of kilometers of sea to be traversed, requiring something like a rafting hypothesis to explain. Thus, unfortunately, the proposed hypothesis is also hindered by our scarce geological knowledge and cannot stand apart as being more supported than other explanations in terms of overland transport. While important information is included in the supplemental material, with critical bits of evidence nested throughout, most of these are not mentioned in the main text. There is also no mention of quantitative evidence for such strike-slip plate motion, nor its relationship with the timing and amount of extension and seafloor spreading in the Granada basin (e.g., Aitken et al., 2011; Garroq et al., 2021), which is thought to have occurred mainly in the early-middle Eocene.

5. Lines 27-30 suggest that this paper presents new data that reveals new insights into the evolution of the eastern Caribbean. However, to me, this is more of a compilation paper emphasizing paleomagnetic data and ideas presented in those studies and using GPlates as a tool to demonstrate the concept. I do believe that the GPLATES integration of new constraints on the evolution of the eastern Caribbean is valuable and worthy of publication in some form. While the insight provided by the new reconstruction shows that the GrAnoLA hypothesis is convincingly well supported by paleomagnetic evidence, new data and ideas are generally lacking. I think that this manuscript would be improved with more quantitative analyses. GPlates is a powerful software that provides many tools for quantitative plate tectonic analysis, not to mention the option of deforming meshes, which, if included, would be a demonstration of the robustness of geometries and kinematics built into the reconstruction and the resulting strain patterns in the eastern Caribbean. This would set the stage for future modeling studies and explorations for new data to support the model. I noticed that in the supplemental, the authors discuss that they opt to use a static plate reconstruction, and I emphasize that the deforming mesh is solely a suggestion. However, I think GPlates can still be better harnessed to quantitatively describe the model, including plots synthesizing the timing and styles of motion on faults. Also, the supplemental movie is a screen capture. It could be improved by exporting frames and combining them into a movie, for example, with FFmpeg, GMT, MATLAB, or other programming languages.

6. Besides the Gplates integration, many ideas have already been discussed in the previous articles cited in the manuscript. For example, Montheil et al. (2023) says concerning the paleomagnetic data, "The potential southeastern position of the Northern Lesser Antilles block and the presence of middle Eocene to early Oligocene emergent land thus define the Northern Lesser Antilles as a potential pathway for South American fauna dispersals. This represents an alternative or complementary solution to the GAARlandia hypothesis that considered the Aves ridge as the only potential pathway for South-American faunas (Itturalde-Vinent and McPhee, 1999). This scenario is not contradictory to the GAARlandia hypothesis but indicates that the PRVI and NoLA blocks may have played a prominent role in terrestrial fauna dispersal during the Cenozoic". To better communicate how this model stands apart from previous studies, I suggest that a figure is included showing how the new reconstruction compares with previous models regarding both tectonics and paleogeography. A figure summarizing the constraints would also be beneficial as a composite addition to the reconstruction figure.

Additional line-by-line/figure comments:

Line 61: Somewhat awkward sentence. Maybe just list the numbers or the Epochs? Saying Paleogene to Neogene is effectively saying just Cenozoic.

Line 75: As mentioned above, the manuscript never fully describes the tectonic and geodynamic factors that cause shortening and uplift.

Line 77: initiate --> initiated

Figure 1: This diagram is missing the Southern Caribbean fold belt (e.g., Pindell and Kennan 2009). Furthermore, from my recollection, most present-day motion is accommodated on the Bocono fault in the Merida Andes, defining the Maracaibo block. I think this was left out for simplicity, but I thought it was worth noting for accuracy.

Line 103: "to actual volcanic arcs"?

Line 106: This sentence is difficult to read and grammatically incorrect.

Line 110: I am not sure that "oceanic" is needed.

Line 112: complexes --> complex

Line 113-114: Unclear

Line 118: add "ago" for clarity

Line 133-135: Revise for clarity and correctness

Line 137: Revise for clarity

Line 138: Revise "allow explaining"

143-144: Here you mention the motion on strike-slip faults but never really address how total displacement is distributed.

Line 146-147: Missing citation

Line 148: I don't remember the Muertos fault being definitively connected to the EPGFZ(?)

Line 150: I would be careful here with the word connected. I know what you mean, but maybe use a word like transferred instead. To me, I read connected as a single discrete structure, whereas in these cases there are probably complex transfer zones between more discrete structures.

Line 163: The use of the word lengthening is confusing here.

Lines 165-168: This sentence could be improved/split for clarity. It is hard to read and understand.

Line 169: Can remove "such a"

Line 186: Maybe add which relative direction they moved (south, right?)

Figure 2: Isn't the Granada basin extension considered more trench-perpendicular (Garroq et al., 2021)? Maybe potential uncertainties around this should be discussed in the main text.

Line 194: Remove umlaut

Line 201: 50 --> fifty

Lines 211-214: How could the pathway be semi-continuous and completely overland? Probably should make something like

“partially overland”.

Figure 3b: Garrocq et al. show the highest amount of extension in the southern Grenada basin, which remains until the Oligocene. Do you disagree with that in this model? In place, you show a narrow, shallow sea.

Line 234: I would remove the word novel in most places in this manuscript since this idea has already been proposed in Montheil et al. (2021).

Methods:

Line 460: As mentioned above, I think this would improve the manuscript's impact (and it wouldn't be too hard; you could even use rigid blocks or marker features representing unknown areas).

Supplemental:

Line 610: Missing reference

Line 620: Then why include it in graphics? Or is it quantifiable and major offshore?

Line 646: There are many references to Boschman et al. 2019. I get that that paper is a review and contains many references (which should be stated with “and references therein”), but Boschman et al. were not the first to compile all of these data; for example, much of Pindell's work. I suggest substituting many of the Boschman citations for the original work presenting those data and ideas. In this case, the work of S. Whattam for example.

Lines 755 – 759: While a key citation suggesting subaerial exposure of the forearc, this is the only mention of the Barbados prism in the compilation of constraints for the reconstruction.

Line 767: Subheadings almost in the middle of the page?

Line 803-804: The SFZ is not transpressional along its entire length.

Line 858-859: Do you mean to say “from” instead of “since”?

Lines 870-872: The timing cited here is incorrect. Cooling is shown to occur from the Eocene to Oligocene.

Reviewer #4 (Remarks to the Author):

The paper is well written and illustrated, and interesting to read. It provides a new tectonic reconstruction that has developed as a follow-up study from recent palaeomagnetic and geological work in the northern Antilles (Montheil et al., 2023, *Tectonics*, *GSA Bulletin*) and builds upon significant geological and geophysical advances in the eastern Caribbean over the past 5 years. The study, although not using a novel method/approach and not providing a novel dataset, provides new and significant constraints on the tectonic feasibility of a recently proposed paleogeographic concept that represents an alternative or complementary solution to the GAARlandia hypothesis (as formerly stated in Montheil et al. 2023, *Tectonics*). This has large regional interdisciplinary relevance and is a great example of the significance of tectonics in controlling some biological processes. The tectonic reconstruction is supported by an extensive geological and geophysical Caribbean synthesis and seems overall very sound regarding tectonic implications in the eastern Caribbean. I only have a few questions below on the relevance of some regional geological aspects that are not explicitly discussed in this manuscript. Overall, it is an interesting and useful contribution that promotes a new tectonic interpretation of the eastern Caribbean and that is notably relevant for regional palaeobiology. As a field geologist I think there is an interesting and concrete added value in this study and, thus, I am supportive of its publication.

It could be argued that the study does not provide new critical observation/data to support or refute significant land emergence in the eastern Caribbean during the Palaeogene, and this could be regarded as a weakness if the main goal of the paper is to demonstrate the feasibility of a land connection (or string of large islands) linking N and S America in the Paleogene. The manuscript suggests that the situation remains unclear in terms of actual land connection due to relatively scarce availability of geological data in this mostly submarine area. It is a fair take because probably the only way to test this aspect would be to drill possible ancient islands in the area to directly document their existence and longevity - so sadly with the current state of the IODP, this seems out of reach for many years still. In this context, the study makes in my opinion the most out of available data to help address a long-standing paleogeographic issue. A corollary of the scarcity of geological data is that the paleogeographic debate will probably not be settled by this study. This point is however recognized by the authors and presented in the concluding remarks as a way to stimulate interaction between the geological and palaeobiological communities. I personally agree with them, there is value in this. In a way this reminds me the start of the huge debate in the last decade about the formation of the Isthmus of Panama, but with the important difference that in this new study the geological rationale (although partly embedded in other recently published studies) appears sounder than in the Panamanian case. Critically, this study avoids creating potential misunderstanding stemming from confusion between tectonics and paleogeographic concepts. As outlined in the manuscript it is very much a tectonic study, well supported by recently published geodata, with some interesting possible paleogeographic and biological implications. So, from my perspective, the positives clearly outmatch the negatives, and I am therefore supportive of publication in *Communications Earth & Environment*.

Main comments/questions:

1) As stated in L202-205: "a connection with South America was likely formed by either stepping stone islands or a continuous land connection over the northern Venezuela and Barbados accretionary prism and forearc (Fig 3B)". Are there observations supporting land emergence close to S America beyond the Barbados accretionary complex? The study builds upon new data from the northern (and older exposed) Antilles, but what about the geological record closer to S America in the Palaeogene? This should be discussed/synthesized somewhere, since at present it looks like an important remaining question mark, but which is not explicitly treated as such. For instance, is it possible that there were volcanic islands south of Antigua area in the Paleogene? Often an issue with volcanic arcs is that older volcanoes are buried under younger volcanic deposits. Could a large area without islands have subsisted in the Paleogene, and if so of which size? I understand that the paper tries to show the feasibility of a continuous string of islands between N and S America, but it looks like it could better critically embrace the lack of constraints in some areas - in other words if you can better show that any remaining area without islands would have been small anyway, this will strengthen the implications of the model, not weaken them.

2) In Montheil et al., 2023 (Tectonics) there is a discussion on the relative contribution of forearc sliver motion, internal block faulting, and oroclinal bending of the arc as possible causes to explain large block rotation in the northern Antilles, which is not taken forward, or at least only partly, in this new study. Why is that and could this be explained in the manuscript (other readers will also wonder if they read related bibliography)? The presented model is apparently an endmember in terms of both mechanism (e.g., no internal block faulting) and extent of rotation (only the most extreme statistically possible rotation values are considered) - the text should clarify what would be geometric implications if there was a combination of deformation mechanisms and/or less extreme rotation used in the tectonic reconstruction.

3) The Caribbean Plate is considered to have insignificant internal deformation in the reconstruction. However, Beata Ridge, which apparently records long oceanic plateau magmatism (Dürkefälden et al., 2019) and has been recently (moderately or insignificantly?) deformed, suggests that the plate was at least not perfectly rigid. There has been rumours for years that unpublished paleomagnetic data from the German (U. Barckhausen?) indicate a complex formation of the plateau (if it is all plateau). In 2019 I saw some (obviously unpublished) seismic data from the petroleum industry from the "oceanic plateau" close to Colombia-Venezuela with rifts and grabens that can only make one wonder how much internal deformation has actually occurred in this plate. Although this is obviously not peer-reviewed data and is technically unusable, the scarcity of constraints on the rigidity of the Caribbean Plate calls for prudence. So my question is: would internal deformation of the plate change the outcomes of the modelling regarding the evolution of the Antilles? This should probably not be totally disregarded considering that most regional experts (possibly also some of the co-authors?) remain very cautious regarding the nature and evolution of the interior of the Caribbean Plate. A small discussion or at least a mention of this would be welcome.

Addressing Points (2) and (3) above may perhaps benefit from testing a few alternative solutions to address the full possible variability of existing constraints to better outline the robustness of the presented (favoured) tectonic model/interpretation.

David Buchs, Cardiff University

Minor comments:

L118: Isn't this also proposed by Pindell and Kennan 2009?

L130: "subduction initiation" would require no previous subduction (at least for most readers), which seems difficult to have here since there is an older arc (and slabs) in the area. Do you mean resuming of arc volcanism and/or development/lateral propagation of a new volcanic front?

L197-198: Montheil et al. 2023 does not look like the correct reference for constructional volcanism from 42 to 38 Ma - as you know the oldest igneous rock dated in this paper is 35 Ma. The Basal Volcanic Suite of Antigua is apparently only age estimated in this paper. Is there other supporting evidence in the literature? This should probably be clarified in the manuscript.

L219-221: "GrAnoLA-land submerged and tectonically broke up during an episode of late Oligocene to Miocene regional subsidence triggered by cessation of arc volcanism⁸, and by trench-perpendicular tectonic extension²¹ (Fig. 3A).": reference 8 (Japanese paper) is not the correct one. Saying that regional subsidence was triggered by cessation of arc volcanism seems very brave here because it could also be related to tectonics, e.g., crustal tectonic processes or subduction dynamics, not just post-volcanic lithospheric subsidence. You should tone down the interpretation of the cause of the subsidence here considering regional tectonic complexity. Mantle and subduction dynamics close to a subduction zone will likely play a larger role than "simple" thermal subsidence that is more commonly a dominant process for intraplate volcanoes far from subduction zones.

L638-646: This is perhaps not critical for this paper, but I would like to provide some insight into this section given that I have worked in the area for 15 years (with >1 year of regional field work, still going on in 2025). Buchs et al. 2010 and Montes et al. 2012 are so far the only papers documenting cross-cutting relationships between the oceanic plateau and the arc. Buchs et al. 2010 is the only published study documenting old (73 Ma) arc dykes cross-cutting oceanic plateau sequences, with reliable (biostratigraphic) data on the older arc sequences. We have new, unpublished regional data stemming from 2 PhD projects (including lithostratigraphy, geochemistry, geochronology, and biostratigraphy) that support this stratigraphic relationship across Panama toward Colombia (>500 km along strike) - we are in the process of collecting the last

geochronological data for publication to constraints distinct newly-recognised magmatic stages during subduction initiation around 75 Ma. None of existing field observations and associated data support earlier arc volcanism in the area. Given the relatively widespread regional exposures of earlier arc sequences overlying an oceanic plateau basement, it is rather strange that no clear direct evidence has been found so far for an earlier volcanic arc if there had been one in this area. Nicoya area, close to the suture with the Chortis Block (e.g., Siuna, Santa Elena), is a complex tectonic assemblage that does not provide a clear insight into the Panama evolution, as is, e.g., suggested in Andjić et al. 2019, which is an interpretation alternate to that of Boschman et al. 2019 and that also makes a rather convincing use of newer local geological constraints. So far, >75 Ma subduction in Panama has been proposed based on distant geological constraints or hypothetical regional scale mechanisms but local geology does not support the occurrence of >75 Ma arc sequences there. If it is an issue for/wider scale tectonic constraints, I would be happy to discuss this further given my interest in helping solve early Panamanian palaeogeography. Note also that new results in the northern termination of the Andes are showing growing evidence for a complex tectonic history with multiple arcs and oceanic plateaus in the SW Caribbean (e.g., Correa-Restrepo et al., 2023; Spikings et al., 2024). The suture between Panama and S America is expected to be equally complex. Has this any incidence on eastern Caribbean tectonic reconstructions, though?

L723: this is only supported by references in these papers, not new data in these papers - add "and references therein"

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Version 1:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Montheil,

Your revised manuscript titled "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles arc" has now been seen by the original reviewers #1, #3, and #4 and by a new reviewer (#5), whose comments are appended below. You will see that they find your work of some potential interest, and they do recognize your revised version shows important improvements. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

In particular, please ensure that the revised manuscript meets the following editorial thresholds:

***Appropriately discuss your paleobiogeographical hypothesis in the context of relevant existing literature on Caribbean biogeography and paleontology, as suggested by reviewer #5.

***Expand your discussion to justify your hypothesis on subduction initiation in the Antilles in the Eocene and subsequent volcanic arc dynamics.

***Compellingly justify the novelty and advance in this study, and avoid regional geodynamic overinterpretations.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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**** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first ****

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Carolina Ortiz Guerrero, Ph.D.
Associate Editor,
Communications Earth & Environment
Consulting Editor, Communications Sustainability

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Recommendation (Required): I recommend publication upon addressing comments Significant: Yes, the paper is a significant advance or contribution.

Supported: Mostly Yes

Referencing: Yes

Quality: The organization of the manuscript and presentation of the data is appropriate

Data: Yes

The authors provided an updated version of the manuscript "Paleogene subduction zone processes tectonic deformation along the Lesser Antilles arc", now titled "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles arc". I confirm I was able to load the .gpml and .rot files in GPlates and visualize the tectonic reconstruction. However, I believe the animation should be done properly, maybe using FFMPEG or scientific software such as MatLAB, Python.

The new title clarifies that the paper focuses on the paleo-geography (physiography) and kinematics, not the geodynamics. However, the introduction of the paper heavily highlights the importance of subduction processes in inducing physiography changes. However, there is no significant discussion of these processes in the text. I think the intro should be adjusted to reflect the true focus of the paper.

The authors say that "We show in our paper a kinematic reconstruction, for which understanding the drivers of deformation is irrelevant - the drivers may be inferred from the reconstruction.". However, the drivers of the deformation should inform the reconstruction, not the other way around. Your reconstruction is developed in a plate tectonics geodynamic context, where subduction processes are very important (as you mention in the intro). Hence, the kinematics you propose must be at least geodynamically plausible. You don't need to "prove" your "saloon-door" mechanism is correct or perform geodynamic simulations in this paper, but you can at least provide your thoughts on what mechanism could explain this very complex kinematics. You already provided an explanation of it in your reply. Adding this to the manuscript and a supplementary figure will strengthen its scope and will harvest future discussions in the context of Caribbean tectonics and paleo-geographic reconstructions.

217-219 I think that any comparison with other back-arc basin subduction systems should include a discussion of the geodynamic mechanisms. Two different geodynamic settings cannot be compared solely based on kinematic similarities.

235-240 You provide evidence supporting land emergence around 45 Ma and suggest the existence of an island chain divided by narrow sea corridors, which may have exhibited a dynamic or fluctuating topography (i.e. Connection and disconnecting of narrow sea corridors over time). Also, you mention that land emergence can be related to a drop in sea level 50-70 m. Have you looked at short-term sea-level curves that could support these fluctuations in land emergence?. Part of the evidence you presented for validating the submergence phase is indeed the growth of carbonates and carbonates are affected by changes in the sea level. I wonder if this is the mechanism that the manuscript is missing.

260-265, you claim that the land pathway you reconstructed offers an explanation of the tectonic drivers of emergence. As I mentioned before, I don't think the current manuscript describes these processes anywhere in the text. Could it be that the mechanism that explains the emergence and submergence is not tectonic, but paleoclimatic ? (i.e., sea-level changes)?

267-268 Again, you are invoking trench-parallel extension to explain the "destruction" of land bridges. However, your reconstructions display slab roll-back in both the "emergence" (45-38 Ma) and "submergence" phases (35-25 Ma).

307-309 I probably missed it, but I didn't see where you provided the mechanisms of formation and destruction of the biological dispersal pathway. Are you saying the magmatism versus the absence of magmatism?. If yes, what mechanism explains the cessation of magmatism? . especially when slab-roll back never stops between 50-25 Ma.

Also, the conclusion mentions that "Our reconstruction provides solid geological evidence for a time window, mechanism of formation and destruction (i.e. tectonics and volcanism) of a biological dispersal pathway". However, there is no significant discussion of these "mechanisms" in the text. What do you mean by tectonics?, I don't see how volcanism explains the extension.

Technical questions

While the undeformable block approach you used facilitates comparison with geological data, it significantly hinders integration with automated computational workflows—severely limiting the scope and quantitative usability of your reconstruction for future applications such as geodynamic simulations. I think implementing closed polygons will increase the impact of your reconstruction and clarify how the kinematics you reconstructed might explain the land bridges you are proposing (if the causes are indeed tectonic).

Minor comments

Fig. S4 I think it would be beneficial to include the "saloon-door" model to facilitate comparison

Reviewer #3 (Remarks to the Author):

Review is attached since there are images included.

Reviewer #4 (Remarks to the Author):

This is my second review of this manuscript, and I am still finding it interesting and suitable for publication in *Communications Earth & Environment*. The reviewer's comments have generally been thoroughly and constructively addressed by the authors, and the clarity of the methods, argumentation and discussion has been improved. The quality of the manuscript is therefore also substantially improved. I have three main points that the authors should consider before publication, all three are minor given the breadth and extent of work already invested in this study and the thorough replies to the previous comments.

1) A reviewer commented on the lack of usage of geodynamic constraints/models which the authors rebutted on the basis that their approach is evidence-based, with an emphasis on the use of field geological constraints, seismic profiles and paleomagnetic data. This approach could be clarified in the methods and/or the manuscript as readers with an interest in geodynamics will most surely have questions similar to those of the reviewer. E.g., the authors could state why geodynamics constraints are not considered in their approach. It should clarify that it is not an omission, but a methodological choice (valid in my opinion).

2) The manuscript content and the replies to the reviewers' comments suggest that there was subduction initiation in the Eocene in the Antilles, although it is also recognised that there was an earlier subduction zone in the area that accounts for the formation of the Aves Ridge as an older volcanic arc. Specifically, it is indicated in the manuscript, l130: "[...] and incipient volcanism, forming the 'GrANoLA [...]'. However, existing geological data do not indicate if the GRANoLA includes older volcanic sequences that are not exposed and still contributed to much of the shallowing of the island arc to form stepping stone islands (i.e., new Eocene arc volcanoes that developed on older pre-Eocene arc volcanoes). In the reply to one of my previous comments on subduction initiation it is indicated that the Chen et al. 2024 study (added during the revision to the manuscript) support formation of the "modern slab" in the Eocene - there is some interesting re-interpretation of mantle tomography that suggests the existence of a flat slab under the Caribbean in this study, but I don't understand how this is addressing my point on subduction initiation and subsequent island arc evolution from a volcanic perspective (which might have been unclear in my previous comment). The existence of an older slab is not refuted by the dataset and interpretation of Chen et al. 2024. It is also unclear to me what is the meaning of "modern slab" in the authors' reply. My point here is that most people interested in the long-term (10s of My) dynamics of an intra-oceanic arc and its paleogeographic evolution may consider that the older Aves Ridge Arc (Great Caribbean Arc) may have been dissected/split by the opening of the Grenada back-arc basin, and that the basement of the GRANoLA included parts of the earlier arc (as suggested by e.g. the Jurassic radiolarites of La Désirade that could be part of such basement). This has not necessarily to be associated with subduction initiation, but a new pulse of volcanism that started in the GRANoLA on top of older arc volcanic sequences, thus contributing to volcanic growth and emergence in this area. I think it is the most logical explanation for the volcanic evolution of the area (followed by some other studies in the past), but somehow, I am, personally, not getting this idea very clearly through the revised manuscript and some of the authors' comments in the rebuttal document. I think however that it is an important concept as the GRANoLA hypothesis in this study is potentially competing with the GAARlandia hypothesis from former palaeogeographic studies. The possible splitting of an older volcanic arc and relocation of the volcanic front through time, associated with back-arc opening, seem critical in this discussion as they would contribute to controlling the evolution of the paleogeography of volcanic promontories on both sides of the back-arc basin (GRANoLA and GAARlandia, which are not mutually exclusive hypotheses as now more clearly stated in the revised manuscript). Given the role of the construction of an island arc in land emergence in the E Caribbean, how could it be that "back arc extension did not create land nor destroyed it", as written in the reply to the previous reviewers' comments?

3) Given the east Caribbean focus of the study, I didn't want to shift the scientific debate to the western side of the Caribbean, but the reply to my previous comment on the timing of subduction initiation in Panama confused me - I did not fully understand the plate tectonic argumentation provided in the reply and this led me to realise that there are some shortcomings in the presentation of the geology of Colombia in the revised (and possibly also original) manuscript, which need to be amended before publication.

It is commonly accepted that the Romeral Fault in Colombia is the boundary between (i) the Central Cordillera in the E, that includes the Quebradagrande Complex with an older volcanic arc that likely developed along S America, and (ii) the Western Cordillera in the W that includes an accreted Upper Cretaceous intra-oceanic volcanic arc with slivers of accreted Caribbean (?) Oceanic Plateau. Because of this complex geological assemblage, and in particular the age and current location of volcanic arc units in the Western and Central Cordilleras, it has been suggested by several research groups that there was a double verging subduction system (Molucca plate style) between the Caribbean Oceanic Plateau (and its intra-oceanic volcanic arc) and South America (and its continental volcanic arc) before collision of the plateau with the continent in the Campanian.

Issue 1: Figure 2 and other supplementary figures show plateau rocks on the wrong side of the Romeral Fault, or the Romeral Fault is misplaced/mislabeled - should it be instead the Uramita Fault Zone? This labelling issue should be corrected, and it may also be useful to the editor to clarify that this has no impact on the outcomes of the study.

Issue 2: In the supplementary text: "The Quebrada Grande complex also exposed rocks originating from a Proto-Caribbean Cretaceous to Palaeocene intra-oceanic arc (Jaramillo et al., 2017; Pardo-Trujillo et al., 2020)." However, these studies do not support this view but follows the double verging subduction zone model discussed above (as do also other more recent publications having to explain the occurrence of Paleogene boninites in this area, e.g. Correa-Restrepo et al. 2023). The Northern South America section in the supplementary file needs to be clarified accordingly.

General note for tectonic reconstruction of the W Caribbean (although probably mostly irrelevant for the main outcomes of this study on the E Caribbean): there is no evidence for a pre-upper Campanian volcanic arc between S Costa Rica and W Panama. There are some Coniacian-Santonian siliceous sediments interbedded in the oceanic plateau at the base of the arc, but these do not contain arc-derived (e.g. tuffaceous) materials; they are synchronous (interbedded) with oceanic plateau volcanism that is devoid of supra-subduction geochemical proxies. If geological constraints are used for tectonic reconstructions, these are important observations that should be taken into account when developing new regional tectonic reconstructions. It might be an issue, but I cannot see how an earlier arc could have been missed between S Costa Rica to W Panama (an arc is not a small volcanic feature) - puzzling but not to be ignored. It looks like there is scope for additional work in future studies to explain the tectonic evolution of the W Caribbean and northern S America. In the meantime it would be fair (with respect to the extensive field work done in the area) to state somewhere in the supplementary file that there remain uncertainties in the exact timing of subduction initiation in southern Central America, but that this has no incidence on the outcomes of this study (if needed I provided some relevant references in my previous review).

David Buchs, Cardiff University

Reviewer #5 (Remarks to the Author):

The manuscript "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles Arch" by Montheil and collaborators represent a significant contribution to the geology of the eastern Caribbean and its link with the origin of some elements of the biodiversity that inhabited the region. In general, the article is well written and organized, and there is a good flow between the different sections. I must disclose that my expertise and therefore, my comments, are not in the geology neither the tectonic on the region, although I found it rather fascinating. My comments focus on the biogeographical and paleontological components of this article and the implication that this work has for understanding the origin of the Caribbean biodiversity.

After carefully reading the manuscript, I am inclined to recommend it for publication because of their significant findings and biogeographical implications. I do, however, have several comments that hope will help this paper to clarify some sections and improve this important contribution.

In general, I found a little difficult to follow the use of the terms land connections, land bridge, transient land bridge, and stepping stone islands since they seem to be used as synonyms in some instances (e.g., lines 265, 288, 291). These concepts are very different to each other, and the expectations for biological dispersal capability from one to another can be very different.

Although the paper focuses primarily on the tectonic deformation along the Lesser Antilles arc, I encourage the authors to discuss in more detail how their new interpretation fits with the current biological and paleontological evidence available. Specifically, in the section of "Impact on the Caribbean biological evolution" in the Discussion, they only have one reference regarding dispersal of vertebrates (line 297). There is a large body of literature looking at the dispersal of plants, invertebrates, vertebrates and fossils which could be included here to make the discussion much more meaningful.

Specific comments

Line 1: The title of this paper is "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles Arch". The new paleogeographical reconstructions of the Caribbean by Montheil and collaborators, particularly in the Eocene, has major implications for the origin of the Caribbean biodiversity, but not necessary for the posterior evolution and radiation within the islands and posterior overwater dispersal events. I would suggest the authors to consider modifying their title to something like "Origin of Caribbean biodiversity shaped....".

Line 49: It sees that biodiversity hotspots are often found in regions associated with subduction, citing Myers et al., (2000). However, this is not a result presented neither discussed by Myers et al., (2000). I suggest finding other authors that have worked in the associated between biodiversity hotspots and geological history of these regions.

Lines 57-58: The authors mention several groups that of South America origin which colonized the Caribbean through the Cenozoic like land mammals, amphibians, reptiles and others. However, they only provided one reference, for each groups, and some of these works only provided support or has implications for a clade. For example, Tejada et al (2024) cited for mammals, only focus on sloths; Blackburn et al., (2020) cited for amphibians, reports the oldest record of Eleutherodactylus, but there are several other groups of amphibians in the region with a South American origin. I would recommend the authors that if they will provide references or each group, it should be more comprehensive.

Line 63: The authors mention there was a pick in colonization between 40 and 30 million years ago, citing Ali and Hedges (2021). However, Ali and Hedges (2021) said in their Conclusion statement (page 2703), first paragraph, starting in the fourth line the following: "First, although 23 of the 35 clades have colonization intervals that straddle c. 34 Ma, the ranges are typically 30–50 Myr, with some appreciably larger. Thus, it is difficult if, not impossible, to peer through the 'data fog' to visualize the actual record; any analysis based on the colonization-interval midpoint ages provides a superficial view." Therefore, in their opinion because of the wide age ranges it is not possible to observe a clear pattern for many of the vertebrate groups, and this pick the authors mention can be a methodological artifact. I recommend the authors to reevaluate their statement about the pick of colonizations between 30-40 million years ago in this section and thought the rest of the manuscript (e.g., line 114, 292).

Line 126: It sees that these new interpretations may be relevant in interpreting the evolution of the Caribbean biodiversity hotspot, but it seems actually more relevant to the origin of the biota in the Caribbean biodiversity hotspot.

Line 241-245: This is a very long sentence that could likely be divided into two.

A general issue with most palaeogeographical reconstructions that suggest the existence of some sort of land connectivity, is the absence of a filter. The Caribbean biota today and in the recent fossil record (late Quaternary) is relative depauperated at high taxonomic level, lacking several groups of vertebrates that are/were in South America. A land bridge usually allows

the dispersion of large components of the biota, which is not what we see in the modern and recent fossil record in the region. There are two non-mutually exclusive explanations. First, there were geographic and/or ecological filters like the presence of marine barriers which reduced the number of taxa that dispersed into the Greater Antilles. Second, it is possible that a larger number of groups arrived to the Greater Antilles but went extinct before the late Quaternary. This last alternative is discussed in a paper that was just published (Vinola-Lopes et al., 2025) and would support the claims made here. The sentence in line 241-245 seems to be the right place to mention that the disconnection between different parts of GrAnOLA acted as a biodiversity filter.

Line 264: it seems that around 34 mya the land exposed potentially created a true land bridge, but in the paleogeographic reconstruction in Figure 2, there are large gaps of water separating them.

Line 265: see the comment above regarding the dispersal peak since there are no strong evidence of it based on molecular analysis (see Ali and Hedges, 2021).

Line 286: migration is not the correct term to use in the biogeographical context since it is more associated with the cyclical movements of animals from one region to another as part of their life history. Suggest to use dispersal.

Lines 285-286: This line is a little awkward. In the way that currently reads, it suggests that all Caribbean biota came from South America, which is not the case. Suggest rephrasing.

Lines 286-288: it seems that there is a 20 million year window of opportunity to overland dispersal via interconnected stepping stones islands. However, the authors also suggest the presence of a true land bridge around 34 mya (lines 264-265).

Furthermore, with a 20 million window for overland dispersal we should expect to see several other groups of vertebrates in the Caribbean which are not there today (see Ali and Hedges, 2021). This time is considerable shorter than GABI, but the number of species that disperses into the Caribbean is significantly smaller than those that disperse either into South America or North America. How do you justify it?

Lines 297-302: The influence of the breaking up of GrAnOLA in the allopatric speciation and high level of endemism in Puerto Rico and the northern Lesser Antilles seems very interesting. However, in the paleogeographical reconstruction of Cornée et al. (2021) there not seems to be islands permanently exposed in the northern Lesser Antilles until recently. This is also reflected in Figure 4 in this paper. If the pattern of allopatric evolution described here was the case, we should expect to see that the lineages in the northern Lesser Antilles diverges early on, however, this not seems to be the case (e.g., Tucker et al., 2017).

Figures

I think the figures are in general well designed and convey a large quantity of information, but this is not reflected in their legends, which are rather short. I recommend the authors to expand in more details the legends to explain better the figures, helping the readers to get the most out of them. I also made some more specific comments below.

Figure 1: In the legend of this figure, the authors do not explain to which group the animal and plant outlines correspond. I assume that these are examples of groups with South American origin. But, are these also examples of groups that arrived to the region in the 40-30 million years old interval? If these species correspond to groups that arrived in the 40-30 mya, there are some issues, for example, anolis lizards, arrived to the Caribbean much earlier while the only group of living rodents in the region, the hutias (Capromyinae), arrived much later in the early Miocene (see Courcelle et al., 2019; Ali and Hedges, 2021). Alternatively, there are fossil rodents with South American affinities in the Oligocene of Puerto Rico (~29 mya), but there are no living relatives of them in the Caribbean today (see Marivaux et al., 2021), and the shape should be different.

Furthermore, in Figure 1 there is shape of a fish which is present in the Caribbean and not in South America, what does it mean?

A final observation about Figure 1 that is mostly stylistically but still important for accuracy. The sloth represented in the shade is a giant sloth, my guess a megalonychid considering the knuckle walking. Caribbean sloth seems to have been rather plantigrades, and they are now in their own family, Megalocnidae. Please, consider updating the silhouette of the sloth for a more accurate one.

Figure 4. Generally the same issues of Figure 1 regarding the animal and plant shapes (e.g., anolis lizards already dispersed in the Caribbean, hutias dispersed much later). Furthermore, the authors indicated that they followed Iturralde-Vinent and MacPhee (2023) and Ali and Hedges (2023) for the reconstruction of the Greater Antilles. However, in the ~35 Ma map, Puerto Rico, Hispaniola, and eastern Cuba are disconnected by marine barriers, while in the maps from the authors they cite these regions are reconstructed as connected. This is important for understanding biotic dispersal between islands in the Greater Antilles. As their reconstruction stand, it indicates overwater dispersal from Puerto Rico to Hispaniola and Cuba contrary to vicariance as result of the subsequent separation of the island (see Iturralde and MacPhee, 1999; MacPhee et al., 2003).

Line 281-282: Ali and Hedges (2023) should be Ali and Hedges (2024).

Lastly, one of the contribution of this paper is the emergence of GrAnOLA during the middle Eocene (~45 mya) and the formation of a transit land bridge at this point. I strongly suggest the authors to then also include a paleogeographical reconstruction of the Caribbean region at 45 mya.

Small comments

Line 63: Cenozoic should be changes to Cenozoic.

Line 83-84: instead of saying: "Cenozoic overwater dispersal via flying, swimming, or floatsam" I suggest the authors to use "overwater passive and active dispersal through the Cenozoic" comprising all possible overwater mentioned by the authors and others.

Line 242: instead of a : , consider using a coma or semicolon.

Line 302: Scheider et al., 2021 is not in the references.

Line 309: should be pathways, in plural, because there are multiple different pathways mentioned throughout the paper.

References:

Courcelle, M., Tilak, M. K., Leite, Y. L., Douzery, E. J., & Fabre, P. H. (2019). Digging for the spiny rat and hutia phylogeny using a gene capture approach, with the description of a new mammal subfamily. *Molecular Phylogenetics and Evolution*, 136, 241-253.

Marivaux, L., Vélez-Juarbe, J., Merzeraud, G., Pujos, F., Viñola López, L. W., Boivin, M., ... & Antoine, P. O. (2020). Early Oligocene chinchilloid caviomorphs from Puerto Rico and the initial rodent colonization of the West Indies. *Proceedings of the Royal Society B*, 287(1920), 20192806.

Viñola López, L. W., Velez-Juarbe, J., Münch, P., Almonte Milan, J. N., Antoine, P. O., Marivaux, L., ... & Bloch, J. (2025). A South American sebecid from the Miocene of Hispaniola documents the presence of apex predators in early West Indies ecosystems. *Proceedings B*, 292(2045), 20242891.

Iturralde-Vinent, M., & MacPhee, R. D. (1999). Paleogeography of the Caribbean region: implications for Cenozoic biogeography. *Bulletin of the AMNH*: no. 238.

MacPhee, R. D., Iturralde-Vinent, M. A., & Gaffney, E. S. (2003). Domo de Zaza, an Early Miocene Vertebrate Locality in South-Central Cuba, with Notes on the Tectonic Evolution of Puerto Rico and the Mona Passage1. *American Museum Novitates*, 2003(3394), 1-42.

Best regards,

Lazaro W. Vinola-Lopez

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Version 2:

Decision Letter:

** Please ensure you delete the link to your author home page in this e-mail if you wish to forward it to your coauthors **

Dear Mr Montheil,

Your revised manuscript titled "Caribbean biodiversity shaped by subduction zone processes along the Lesser Antilles arch" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in *Communications Earth & Environment*, provided you acknowledge the occurrence of volcanic arc rocks in at least the southern part of the Aves Ridge and tune down your interpretation of the Aves Ridge as a volcanic arc, along the lines recommends by our reviewer 4.

If you can address this request, we therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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Best regards,

Alireza Bahadori, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Recommendation (Required): I recommend publication upon addressing minor comments

Significant: Yes, the paper is a significant advance or contribution.

Supported: Yes Referencing:

Yes Quality: The organization of the manuscript and presentation of the data is appropriate Data: Yes

The authors provided an updated version of the manuscript "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles arc", now titled "Caribbean biodiversity shaped by subduction zone processes along the Lesser Antilles arch". At this stage, I believe the manuscript is ready for publication, and I have only a few minor comments. I think the new discussion about the geodynamics opens the path for future investigations based on your updated model of the Caribbean. Furthermore, I agree with you in using the "rigid block approach" for reconstructing shortening and stretching. However, the use of closed topological boundaries is appropriate for creating plate boundaries, such as trenches, mid-ocean ridges, and continental-scale strike-slip fault systems (see <https://github.com/GPlates/gplately>). I agree with you that creating these plate boundaries is outside the scope of this work, but doing this will enable the integration of your model with other regional and global tectonic reconstructions. As the other reviewers mentioned, your model shows many complexities in the eastern Caribbean that are not observed in other reconstructions of the region. Beyond its biogeographic relevance, the reconstruction has important geodynamic implications, some discussed by you. In the future, it will be very interesting to test the impact of these differences in the mantle flow and the mantle structure of the Caribbean.

Ln 275: The upper mantle extends quite a bit in depth (0-660 km), do you mean the full depth (660 km)?

Ln 279: Do you mean the 660 km mantle transition zone?

Ln 287: It's great that you included a comparison with a similar tectonic setting.

Reviewer #3 (Remarks to the Author):

The authors have fully satisfied the comments outlined in my first two reviews. I believe that the manuscript is now ready for publication.

All the best,
Ethan Conrad

Reviewer #4 (Remarks to the Author):

It is my third review of this manuscript, and I can attest that the manuscript was improved in terms of explaining processes underpinning the tectonic evolution of the E Caribbean as well as clarifying some regional geological aspects. Improvements were notably made regarding the explanation of Colombian geology and how this relates (or does not relate) to the new tectonic model. It is clear from the supplementary section on Colombian geology that the authors are not specialists of this area, but the quality is now good enough to address the needs of this study on the tectonic and paleogeographic evolution of the E Caribbean. The timing of subduction initiation in southern Central America is now left uncertain, which is fairer than what was previously mentioned given existing geological constraints; it is also clear this has no important incidence on the main conclusions of this study.

My last remaining criticism is on the geological origin of the Aves Ridge and its potential implications regarding the paleogeographic evolution of the E Caribbean. The authors are strongly questioning the volcanic arc origin of this major bathymetric feature in the E Caribbean, although it is generally considered to be a volcanic arc in most of the existing geological literature.

In their rebuttal the authors mentioned "That the Aves Ridge would be arc is a speculation, there is not actual rock evidence for this. The only rocks that were studied are the ones dredged from the southernmost part of the Ridge, and they give mostly CLIP signatures - and perhaps some arc influence, which at this southern margin is to be expected."

The revised text of the manuscript includes in lines 304-314: "In the reconstructed configuration prior to 50 Ma, the Aves Ridge was behind, and at a high-angle to the Greater Antilles arc (Fig. 3D). Moreover, it was situated approximately 500 km west of the eastern Caribbean plate boundary. Even though this boundary was likely a subduction zone in the late Cretaceous (Pindell and Kennan, 2009; Boschman et al., 2014), this distance is much larger than for typical arcs, and may challenge the interpretation of the Aves Ridge as a Cretaceous volcanic arc, as long thought (Burke, 1988; Neill et al., 2011). Perhaps more likely, the thick crust of the Aves Ridge, from which direct observations only come from the southernmost and northernmost parts may instead consist of basaltic rocks related to the Caribbean Large Igneous Province (CLIP) (Neill et al., 2011), similar to the ~20 km thick Beata Ridge (Dürkefälden et al., 2019)."

Although I agree with the authors that there is very limited evidence for the existence of a drown volcanic arc along the - whole- of the Aves Ridge, it is incorrect to suggest that there is no, or too little, evidence for volcanic arc rocks. The study of Neill et al. 2011 provides strong (unambiguous) geochronological and geochemical evidence from igneous rocks dredged in the southern part of the Aves Ridge. Their data is of excellent quality and clearly document the occurrence of (undated) calc-alkaline supra-subduction mafic igneous rocks, compositionally similar in terms of immobile trace elements to (dated) Upper Cretaceous felsic igneous rocks with arc signatures, which are clearly not CLIP signatures as indicated in the rebuttal. Given these geochemical constraints that also include 2 Nd-Hf isotope analyses, these rocks almost certainly document a volcanic arc, with a Late Cretaceous age for at least some of its sequences. These rocks certainly do not correspond to oceanic plateau basaltic rocks like those reported by Dürkefälden et al. 2019. In fact, most of the studies on oceanic plateau basalts in the Caribbean do not report volcanic arc-like rocks such as those analysed by Neill et al. 2011, and if they do it is generally from sequences crosscut or overlain by such rocks (e.g., in Panama and Colombia). The authors must acknowledge the occurrence of volcanic arc rocks in at least the southern part of the Aves Ridge. It is the simplest and most logical explanation of the data in Neill et al. 2011.

Considering the very limited number of dredged and analysed samples from the rest of the Aves Ridge, the authors could decide to tune down the generally accepted interpretation of the whole of the Aves Ridge as a volcanic arc. However, considering this ridge has been interpreted for long as a volcanic arc based on geophysical constraints (e.g., seismic crustal velocities) and, more recently, the data by Neill et al. 2011, any attempt to tune down the volcanic arc hypothesis will probably be seen by the Caribbean community as a tentative to tune down the significance of the GAARlandia hypothesis to promote the new proposed model. As detailed in my last review, there are some important paleogeographic implications of having a volcanic arc in at least some parts of the Aves Ridge (a valid interpretation using Neill et al. 2011 data). It would in my view be more constructive and beneficial to this study to not use speculative regional arguments, like the possible lack of a volcanic arc in large parts of the Aves Ridge, to rule out the GAARlandia hypothesis- it reads like that now, even if not intentional. The GAARlandia hypothesis is not mutually exclusive with the new paleogeographic interpretation and is not hampering the value of this study. Yet it can provide a fairer account of the data of Neill et al. 2011, and other regional observations on the nature of the Caribbean oceanic plateau and volcanic arcs. As I have been suggesting for >15 years, Caribbean researchers would benefit much from new dredging of the Aves Ridge. Until then, the only direct insight into its origin comes from the arc rocks documented in Neill et al. 2011, and this needs to be acknowledged, not questioned, in this study.

David Buchs, Cardiff University

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POINT BY POINT RESPONSE

Reviewer #1 (Remarks to the Author):

Recommendation (Required): I recommend publication upon addressing comments

Significant: Yes, the paper is a significant advance or contribution.

Supported: Yes

Referencing: Yes

Quality: The organization of the manuscript and presentation of the data is appropriate

Data: Mostly Yes. However, the GPlates static polygons and rotations files are not included in the supplementary materials. This makes difficult the reproducibility of declination predictions.

In this manuscript titled “Caribbean biodiversity shaped by Paleogene subduction zone processes”, the authors present an updated paleogeographic reconstruction of the Caribbean and the Northwestern Andes from 50 Ma to the present day, using the GPlates software. The model integrates novel paleomagnetic data collected in the Puerto Rico, Virgin and Northern Lesser Antilles islands (GrANoLA block), which is not implemented in previous reconstructions of the region. The author proposes that shortening and a rapid ~50m sea level drop around the Eocene-Oligocene (associated to Antarctic glaciation) in the GrANoLA islands created a land bridge that connected the islands with South America between 42-38 Ma. This land bridge is referred to as GrANoLA-land, and it is suggested to have enabled the dispersal of terrestrial and freshwater biotas, such as amphibians, landmammals, arachnids, reptiles, fresh water-fish, from South America to the eastern Caribbean islands. The author argues that GrANoLA-land was broke up into a series of islands between 38-24 Ma following regional subsidence triggered by arc magmatism cessation and trench-perpendicular extension.

I believe that the manuscript is well-written, organised and easy to read. Furthermore, the GPlates reconstruction honours most of the available constraints for the Caribbean and the Northern Andes. However, it is not clear the geodynamic mechanisms that explain the sudden cessation of magmatism and subsidence between 38-24 Ma. Also, it is not clear why the Granada-Tobago basin behaves as a “ductile” block during basin-opening and tectonic escape of the Venezuela ophiolites (48-35 Ma), but behaves as a rigid block immediately after (35-25 Ma). I consider these issues can be addressed after better considering the subduction dynamics occurring beneath the surface and the crust.

We do not model the Granada-Tobago basin as a ductile block. We open it as a back-arc basin from 48 to 38 Ma according to Garrocq et al., 2020. We modeled this in our reconstruction with southeastward motion of the Tobago block (TOB) relative to the Grenada block (GR) that is attached to Caribbean

plate Interior (see Figure S1 for a detailed block list and location). Perhaps the reviewer perceived it as a ductile block because our reconstruction uses rigid blocks, which overlap when extension is reconstructed. We do this to illustrate the magnitude of the motion, which we now explain in the method section of the paper, but that does not modify the extension rate or amount in any way.

The authors provide a comprehensive review of the geological evolution of the Northwestern Andes, Central America and the Caribbean. It also presents a comprehensive compilation of paleomagnetic data and kinematic markers of the region, such as strike-slip and shortening estimations. However, I think it would be useful to include the sea level curve of Miller et al., (2020) that further support the emergence of GrANoLA-land during 42-38 Ma.

We added the reference.

It would be beneficial to include a paragraph in the supplementary materials that introduces and briefly describes the content of the two presented sections. This would make the reader follow better the manuscript.

We have added a method section to the main text that explains the reconstruction approach and the choices that we made and uncertainties involved, which should also serve to clarify the SI.

I think the manuscript is a contribution of significance because it documents how the rapid horizontal and vertical displacements caused by subduction processes can rapidly (<10 Ma) create/destroy migration pathways, which can have a profound impact in the evolution of biodiversity assemblages. The study successfully explains how endemic biotas in the eastern Caribbean islands were colonised by South America species in the Eocene. Finally, this work highlights the importance of bringing together the paleo-biological and geological communities, which will be instrumental for the advancement of our understanding of the feedbacks between the biosphere and geological processes in deep time. I believe this work will spark interest in the bio-geo communities regarding the use of paleogeographic reconstructions to unravel the evolution of life on regional and global scales.

Thank you, that is indeed the novelty of the paper, which we have explained in detail in the opening of this rebuttal and in the revised introduction of the paper.

Below, I provide specific comments for the authors to address:

Technical comments

It is not clear what reference frame was used in the reconstruction, paleomagnetic or mantle reference frame?. Also, it is not mentioned if the updated reconstruction is integrated to any global tectonic reconstruction. What are the motions used for North America, South America and the Caribbean?

We explained this in the new method section: a paleomagnetic reference frame is used for the figures. A mantle reference frame was used to infer trench migration relative to the mantle in the discussion. References to the plate circuit are cited in the main text now.

Can you provide the static polygons and the rotation files? The performed paleomagnetic declination tests cannot be reproduced without the GPlates rotations.

The static polygons and rotation files were available in Supplementary Data 2. Maybe there was a informatic bug here but the author provided these files during the submission process as supplementary files.

The study showcases how the interior of oceanic plates is not rigid, and how this can result in complex dynamics such as opposing block rotations and in the creation of migration pathways. What are your comments on this? I believe this is a call to starting the transition to dynamic polygon reconstructions. Exactly, the Caribbean plate is not an entirely rigid plate. Nevertheless, we favored undeformable polygons rather than dynamic polygons as motion of the Caribbean blocks are mainly dominated by strike slip motion and to locate the areas of opening (i.e. overlaps) or shortening (i.e. gaps) predicted by the model, that we could later check for geological consistency with available geological data. Using dynamic polygons is cosmetically pleasant, because it adjusts the interior shape of a polygon to the changing orientations of its boundaries. However, it also makes illustrating the net deformation that is accounted for less clear. By choosing rigid polygons, the resulting reconstruction is 'uglier', but underlying assumptions and uncertainties, are clearer. We have explained this in the new method section.

169-171 The authors propose a saloon-door style opening for the Grenada-Tobago back-arc basins in the Caribbean plate interior. The text claims that slab roll-back explains the opposite block rotations between the GrANoLA blocks (counter-clockwise) and the Caribbean interior (clockwise), and the extrusion of the Venezuela ophiolites between 50-20 Ma. This is a process that requires the Caribbean interior to break-up and shear into itself, and I don't believe slab roll-back can solely explain this complex process. In Figs. 3C-D, a similar scenario is shown in the Greater Antilles, where the Caribbean plate breaks-up and shears into itself after being “squished” between the Bahamas Banks and South America. What are your thoughts on the role of the Bahamas Banks in the strike-slip motion of the GrANoLA blocks?

What about additional processes relevant to subduction zones, such as the mantle toroidal flow?. (e.g. Schellart and Moresi, 2013 and Kiraly et al., 2017)

We show in our paper a kinematic reconstruction, for which understanding the drivers of deformation is irrelevant - the drivers may be inferred from the reconstruction. It is well possible that the friction of the Caribbean plate with its northern margin has effects and withheld the forearc sliver in the north

while it could move easier in the southeast. But whether that hypothesis is correct or not would not change the reconstruction. What is clear is that the opening of the Tobago-Grenada basin in the mantle reference frame requires slab roll-back (we added this to the discussion). The other components, including mantle flow, cannot be kinematically reconstructed and may be subject of future geodynamic modeling studies - for the key point of our paper, the formation of migration pathways, it is not of direct importance.

Furthermore, I am surprised that after 35 Ma, the time when lithospheric stretching ceases in the Grenada-Tobago basin, there is no evidence of basin inversion in the back-arc basins. The rationale is that the thin oceanic crust in the Grenada-Tobago back-arc basin should be mechanically weaker than the GrANoLA blocks. How would you explain the absence of basin inversion during 30-25 Ma? Actually there is basin inversion in the Grenada-Tobago basin that occurred during the late Oligocene to the middle Miocene (Escalona & Mann, 2011; Garrocq et al., 2021; Speed et al papers). This uplifted the Grenadines ridge. How much inversion there has been remains for now unclear in absence of published detailed paleomagnetic constraints, so it is not included, but the available evidence shows that the shortening is Miocene and younger, and thus largely postdates the biological dispersal.

178-179 Figure 2 is referred in reversed order. In Figure two, (A) displays the configuration of the Caribbean at present day, but the text refers to the present day as (D).

Thanks for pointing that out, we have modified it.

199-200 The paleogeographic maps of Figure 3 show the shortening (Fig. 3A) and subsidence (Fig. 3B) episodes in the GrANoLA block that lead to the creation and destruction of GrANoLA-land. However, the GPlates animation displays continuous back-arc extension and slab roll-back during this time. I suggest including the back-arc spreading centers to Figs. 3A-B for improving visualization and providing a complete picture on the link between biodiversity and subduction processes like back-arc basin extension.

Our goal with this figure was to focus on the paleogeography and to represent the mechanism (uplift and subsidence) responsible for land emersion and demise. As the back arc extension did not create land nor destroyed it, we choose to not represent it, similarly to all the inner Caribbean plate faults kinematics, that are represented in Figure 3 (former Figure 2). We only represented plate boundaries to be able to locate where the Caribbean plate ends. Furthermore, slab rollback is one of the geodynamic drivers but maybe not the only one so we decided to not represent it for more clarity, but rather discuss this in the text.

218-223 The author suggests that the GrANoLA blocks submerged and tectonically fragmented during a period of late Oligocene to Miocene regional subsidence, triggered by the cessation of arc volcanism

and trench-perpendicular tectonic extension. The sketches in Figure 3A depict active subduction beneath the GrANoLA blocks (.i.e. Antigua and Virgin Islands), indicating that subduction continued during arc magmatism cessation. Furthermore, the saloon door-style opening requires continuous slab-roll back and active subduction.

Although the text references a similar scenario in Japan (Ref 8), it does not explain why or how arc magmatism ceases between the late Oligocene and Miocene. What mechanism explains this hiatus in magmatism? Is there any sealevel rise at this time? Or did the arc just jumped into the Caribbean interior? Normally, the jump of a magmatic arc occurs in the direction of slab roll-back (eg.g. Izu-Bonin)

That is a very interesting topic that we addressed in a previous paper (Montheil et al., 2023). Indeed there is an arc cessation while continuous subduction recorded in the Lesser Antilles arc islands. However, what the drivers of the lull in arc magmatism may have been or not is not relevant for the observation that arc magmatism ceased in the old part of the arc, and that it took a long time for it to pick up in the modern arc. Adding our speculation as to what the drivers may be (which we did in Montheil et al., 2023 GSABull) does not aid the identification of the possible dispersion routes, so we did not discuss this further in this paper.

Minor comments

663-665 In the Northern South America section of the supplementary materials, the text mentions that Caribbean affiliated terranes are exposed in the Quebradagrande Complex (Central Cordillera, Colombia). However, detailed geological mapping and geochemical (Jaramillo et al., 2017) and geochronological studies conducted in magmatic (Jaramillo et al., 2017) and detrital zircons (Pardo et al., 2020) of the area, suggest that the oceanic rocks of the Quebradagrande Complex are part of a distinct accreted intra-oceanic arc. Can this be clarified in the text?

Sure, thanks for this specific correction, we changed the “northern south America” paragraph in the supplementary text accordingly. We also modified the Figure 2 to represent these Cretaceous arc rocks, and the one found in Venezuela and in the Leeward Antilles.

800 The start of the Hispaniola section of the supplementary materials is missing the topic sentence. It seems the first sentence is the continuation of some other section.

Thanks for pointing that out, we re-arrange the Hispaniola section accordingly.

Reviewer #2 (Remarks to the Author):

I was going through the paper by I am supposed to review (Montheil et al Caribbean biodiversity...,

and I found a MAJOR PROBLEM.

1. This paper just ignores the two recent contributions which change the perspective they have in his comments about GAARLANDIA's land bridge. In fact there is a couple of papers BY ITURRALDE-VINENT AND MACPHEE demonstrating that the Aves ridge was uplifted during the Middle Eocene to the Miocene. In these papers we also comment about THEIR GRANOLA.

I will just reject such a contribution which is not updated concerning the major facts about this subject.

This is a strangely aggressive review. Nonetheless, we have explained now in the introduction that the previous hypotheses were cast in the paradigm that the eastern Caribbean plate was essentially rigid. The novelty of our paper is the reconstruction of the documentation that it isn't rigid, and this opens alternative pathways to explain the biology. We also indicate that this does not disqualify Iturralde's hypothesis, but the new reconstruction offers ways of explaining the timing of the peak dispersion. We have added the references of Iturralde's recent papers on the hypothesis.

Also I found that the references are numbered in the body of the paper, but such numbers are not found in the reference list, so is hard to track them.

The MISSING CONTRIBUTIONS are:

---Iturralde-Vinent, M.A., MacPhee, R.D.E., 2023. New evidence for late Eocene-early Oligocene uplift of Aves Ridge and paleogeography of GAARlandia. *Geologica Acta*, 21.5, 1-10.

---Iturralde-Vinent, M.A. y MacPhee, R.D.E., 2024. Remarks on: J.R. Ali and S.B. Hedges, 2024. Paleogeography of the Aves Ridge and its potential role as a bio-colonization pathway linking South America and the Greater Antilles in the mid-Cenozoic. *Revista Maya de Geociencias*, Agosto, pp. 90-95.

Thanks for pointing that out, we have updated the reference list.

2. ADICIONAL PROBLEM:

---Supplementary figure 3, refers as Eocene-Oligocene arc rocks in Sierra Maestra of Cuba. The arc rocks in this location actually are Paleocene.early Middle Eocene.(Rojas-Agramonte, Y., Neubauer, F., Bojasr, A.V., Hejl, E., Handler, R., y García-Delgado, D., 2006. Geology, age and tectonic evolution of the Sierra Maestra mountains, southeastern Cuba: a review. En: Lidiak, E.G., e Iturralde-Vinent, M., (Editores) Geodynamic evolution of the Caribbean region. *Geologica Acta*, 4, (1-2): 123-150.; --Rojas-Agramonte, Y., Neubauer, F., García-Delgado, D., Handler, R., Friedl, G., y Delgado Damas, R., 2008. Tectonic evolution of the Sierra Maestra mountains, SE Cuba, during Tertiary times: From arc-continent collision to transform motion. *Journal of South American Earth Science*, 26: 125-151.; --Rojas-Agramonte, Y., Neubauer, F., Handler, R., García-Delgado, D., y Delgado Damas, R., 2001. Las deformaciones ocurridas en la pendiente sur de las montañas de la Sierra Maestra: evolución tectónica

de la falla transformante Oriente. En: IV Congreso Cubano de Geología y Minería, Memorias Geomin 2001, La Habana, marzo 19-23.; --Rojas-Agramonte, Y., Neubauer, F., Kröner, A., Wan, Y.S., Liu, D.Y., García-Delgado, D., y Handler, R., 2004. Geochemistry and early Palaeogene SHRIMP zircon ages for island arc granitoids of the Sierra Maestra, southeastern Cuba. Chemical Geology, 213: 307-324, doi:10.1016/j.chemgeo.2004.06.031.

Thanks for this comment, we have modified this figure accordingly (that is now in the main text as Figure 2) and modified the supplementary text accordingly (line X).

Reviewer #3 (Remarks to the Author):

Summary:

This study presents a new Gplates-driven paleogeographic reconstruction of the eastern Caribbean region from the Eocene onwards. The new model improves plate reconstructions by implementing recently published paleomagnetic and other geological data. The main modification is a $>45^\circ$ rotation of the Lesser Antilles forearc in the late Eocene to Oligocene, moving northward from a position near South America toward its present-day location at the northern cusp of the subduction zone. The main conclusion of the study is that the rotation of the forearc means that the Greater and Northern Lesser Antilles (GrANoLA) were much closer to South America than previously thought and, thus, could allow overland migration of terrestrial land mammals to the islands, rather than via an emerged Aves Ridge (GAARLandia hypothesis).

This is the first manuscript primarily focused on this idea; however, it has been proposed in Montheil et al. (2023) and as a potential dispersion route in, e.g., Cornée et al. (2023), Fig. 1 in the latter article. Thus, this article represents an expansion of this previously proposed concept through a GPlates reconstruction and visualization. The authors motivate the article by pointing out that previous paleogeographic interpretations, namely the GAARLandia hypothesis, lack geological support and a geodynamic mechanism. However, it is unclear how their interpretation is any more supported by data than the previously proposed options,

We have now explained this explicitly in the introduction: the previous papers demonstrated that the paradigm of eastern Caribbean plate rigidity is incorrect and directed towards possible implications. In this paper, we kinematically reconstruct the evidence, and demonstrate, rather than speculate, how the main paleogeographic pathways reconstruct through time.

nor do they provide any discussion on a geodynamic mechanism, missing key citations to recent geodynamic studies (e.g., van Benthem et al., 2014; Cerpa et al., 2021; Conrad et al., 2024) – the latter

of the listed studies also discuss these hypotheses in terms of the implications of their modeling results).

The hypothetical geodynamic mechanism is irrelevant for the tectonic or paleogeographic reconstruction, which follows geological evidence. See main point 3.

The article instead focuses on relatively one-sided and lofty claims about Caribbean biogeography without thoroughly discussing uncertainties, assumptions, implications, or alternative explanations and comparisons with other hypotheses.

Notably, the proposed hypothesis only provides a new avenue for mammals to follow but does not provide any evidence for the continuous emergence of GrANoLA.

We have added to the 'tectonic reconstruction' part an explanation for the uncertainties and alternatives for the new reconstruction. The reviewer does not offer alternative hypotheses for the reconstruction, but we have, which we discuss now, and explain why the reconstructed one is the only feasible one. We have also explained in the discussion that explaining the migration does not require a continuous emergence, but that a transient pathway of connecting and disconnecting islands has the same effect. In context of the geological setting, this is a more likely scenario.

From my reading, the only support provided are citations to articles that indicate subaerial exposure of areas presently in and offshore Venezuela, even in the supplemental. Solely partial emergence is briefly mentioned as a possibility in the article but should be stated more clearly.

We have added explanation that a transient landbridge of merging and breaking islands is a more likely scenario to develop a pathway.

There are also quite a few grammatical issues in the paper that affect clarity. For these reasons, which I expand on below alongside additional comments, I recommend that the article be rejected with encouragement for resubmission. However, I want to emphasize that with improved analysis and discussion, I believe this new reconstruction will significantly contribute to helping us better understand the geo- and biological history of the Caribbean realm.

We have addressed all issues above and below and thank the reviewer for their comments.

1. On lines 66-67, the authors point out that previously proposed hypotheses are not well supported but do not describe the debate and the shortcomings of each hypothesis. Due to the lack of supporting evidence, they imply that an alternative explanation is required. While providing such an alternative is fine, their presentation makes it seem that this new hypothesis is a leading solution rather than a third alternative amongst two other, still perfectly reasonable solutions. I may be missing something about issues with these models that preclude their viability, but, in this case, it should be described more clearly.

We addressed this now in general point 1: the weakness of the previous paleogeographic hypothesis is the underlying tectonic paradigm of eastern Caribbean plate rigidity. We explained that the three available hypotheses were perfectly logical, but the novel tectonic information offers new opportunities to explain the pathway, and the timing of its formation and destruction.

2. On line 66, the authors also write that no geodynamic mechanism has been identified, and later on line 257 say that this paper provides a mechanism. I may have missed something, but I did not see a geodynamic mechanism identified and explained that supports their hypotheses. There is only a discussion on saloon door tectonics. They do not mention that at least three geodynamic mechanisms have been proposed that explain shortening in the northern Lesser Antilles: the effects of an arcuate slab corner (Cerpa et al., 2021), a slab edge push (van Benthem et al., 2014), and slab anchoring leading to subduction at the Lesser Antilles (Conrad et al., 2024), of which, the latter explains that the beginning of LA subduction could explain more continuous emergence and later subsidence along the length of the arc (e.g., Faccenna et al., 1999; Toth & Gurnis, 1998). The discussion would be improved if the geodynamic implications of their model were considered in the context of existing work. I suggest the authors include a deeper discussion and additional references to numerical and analog studies of subduction zone processes and how their proposed model fits within what we can learn from those studies.

The drivers of the deformation are irrelevant for whether the deformation happened or not, and whether or not there was uplift. This follows from observations. We realize that we 'promised' the reader a geodynamic explanation, which we removed from the text, and we changed the title accordingly.

3. The article does not acknowledge the uncertainties, assumptions, other possible explanations, and general lack of data to support their claims. This is likely due to limits in available constraints. As mentioned on lines 78-80 and discussed throughout the manuscript, the proposed model hinges on the paleomagnetic data provided in Montheil et al. (2023) and Philippon et al. (2020). The authors write that these data indicate that GrANoLA moved hundreds of kilometers relative to the Caribbean Plate interior and North and South America. However, they do not mention that this is one end member solution proposed by those authors, including an oroclinal bending end member solution/block rotation in the forearc.

We have addressed the alternatives in the tectonic reconstruction section now, see comments above. In general point 1, we also explain now that what the reviewer perceives as 'the general lack of data to support our claims' is actually hard evidence against the decades-long paradigm that underpinned the entire biogeographic conundrum of the Caribbean region, and that a reconstruction satisfying those data sheds a novel light on this biological problem, and explains the timing.

Furthermore, paleomagnetic data are not evidence that GrANoLA land emerged as a continuous strip of land. Thus, while providing an alternative avenue, they do not solve the issue of an overland pathway.

We explained in the introduction which data explain what. The stratigraphic, structural, and seismic data demonstrate shortening, uplift, emersion, extension, and subsidence, and submergence and the timing thereof; the paleomagnetic data demonstrate the necessity for major horizontal motions. The reconstruction casts this evidence in regional tectonic context and offers the opportunity to translate these data into a paleogeographic scenario that opens novel ways to explain biology.

4. Claims such as that on lines 72-74 are not substantiated by data, as there is no clear evidence that the islands were uplifted and formed a “connected and emergent ‘GrANoLA-land’” On lines 205 to 209, the authors argue for the subaerial exposure of GrANoLA, however, critically, evidence for widespread subaerial emergence seems lacking.

There is evidence for emergence of the GRANOLA land in Cornée 2021 and Philippon, 2020. We have explained in the discussion that there well may never have been a continuous landbridge, but that a transient corridor may well have existed by merging and breaking islands. Nonetheless, it always remains possible that the dispersion was overwater, but our new reconstruction offers to explain the timing, and no earlier scenario could.

While sea level fall cited on line 209 is an important consideration, neither of the cited papers in the preceding lines (Deville et al., 2015; Pindell et al., 2009) present data that support widespread subaerial exposure of GrANoLA. At best, they suggest that portions of the forearc farther south may have been exposed during Oligocene times. Pindell et al. suggest around Serranía, Venezuela, leaving hundreds of kilometers of sea to be traversed, requiring something like a rafting hypothesis to explain. Thus, unfortunately, the proposed hypothesis is also hindered by our scarce geological knowledge and cannot stand apart as being more supported than other explanations in terms of overland transport. While important information is included in the supplemental material, with critical bits of evidence nested throughout, most of these are not mentioned in the main text.

It will never be possible to prove that a continuous land bridge existed, and overwater dispersion can never be ruled out. That is not the point of our paper. As we now explain, we develop a new paradigm on which the biology should be interpreted, namely of a highly mobile eastern Caribbean forearc that had a vastly different position in during dispersion than previously thought. Whatever the reviewer prefers to interpret should be cast in that paleogeographic context, and we find that this context offers novel opportunities to explain overland migration, and the timing thereof.

There is also no mention of quantitative evidence for such strike-slip plate motion, nor its relationship with the timing and amount of extension and seafloor spreading in the Granada basin (e.g., Aitken et

al., 2011; Garroq et al., 2021), which is thought to have occurred mainly in the early-middle Eocene. We postulate that the motion, is accommodated along the documented Monserrat-Harvers fault. Of course there is no quantitative evidence for the amount of displacement. If even enormous faults like the North Anatolian Fault have no conclusive evidence for its displacement, submarine faults like this one will never have it. However, the paleomagnetic data is quantitative evidence. We explain these data and we are not aware of an alternative scenario that could explain it in a very different way, nor is the reviewer offering one.

5. Lines 27-30 suggest that this paper presents new data that reveals new insights into the evolution of the eastern Caribbean. However, to me, this is more of a compilation paper emphasizing paleomagnetic data and ideas presented in those studies and using GPlates as a tool to demonstrate the concept. I do believe that the GPLATES integration of new constraints on the evolution of the eastern Caribbean is valuable and worthy of publication in some form. While the insight provided by the new reconstruction shows that the GrANoLA hypothesis is convincingly well supported by paleomagnetic evidence, new data and ideas are generally lacking. I think that this manuscript would be improved with more quantitative analyses. GPlates is a powerful software that provides many tools for quantitative plate tectonic analysis, not to mention the option of deforming meshes, which, if included, would be a demonstration of the robustness of geometries and kinematics built into the reconstruction and the resulting strain patterns in the eastern Caribbean. This would set the stage for future modeling studies and explorations for new data to support the model. I noticed that in the supplemental, the authors discuss that they opt to use a static plate reconstruction, and I emphasize that the deforming mesh is solely a suggestion. However, I think GPlates can still be better harnessed to quantitatively describe the model, including plots synthesizing the timing and styles of motion on faults. Also, the supplemental movie is a screen capture. It could be improved by exporting frames and combining them into a movie, for example, with FFmpeg, GMT, MATLAB, or other programming languages. We have addressed this comment above. Deforming meshes in GPlates are simply made by lines that move relative to each other and therefore change shape and area of the region they encompass. They just look nice, but are cosmetic. The rigid blocks that we used here are more insightful because they show where deformation must have occurred.

In general comment 1, and in the abstract and introduction, we have addressed the novelty of the paper, and how it shifts the paleogeographic paradigm with major implications for interpreting the origin of the Caribbean biodiversity hotspot.

6. Besides the Gplates integration, many ideas have already been discussed in the previous articles cited in the manuscript. For example, Montheil et al. (2023) says concerning the paleomagnetic data, “The potential southeastern position of the Northern Lesser Antilles block and the presence of middle Eocene to early Oligocene emergent land thus define the Northern Lesser Antilles as a potential

pathway for South American fauna dispersals. This represents an alternative or complementary solution to the GAARlandia hypothesis that considered the Aves ridge as the only potential pathway for South-American faunas (Itturalde-Vinent and McPhee, 1999). This scenario is not contradictory to the GAARlandia hypothesis but indicates that the PRVI and NoLA blocks may have played a prominent role in terrestrial fauna dispersal during the Cenozoic". To better communicate how this model stands apart from previous studies, I suggest that a figure is included showing how the new reconstruction compares with previous models regarding both tectonics and paleogeography. A figure summarizing the constraints would also be beneficial as a composite addition to the reconstruction figure.

The Montheil et al., 2023 paper showed paleomagnetic evidence, and in a few sentences argued that these findings could have implications for paleogeography discussions. In this paper, we placed those new findings in a kinematic context from Colombia to Cuba and demonstrate that it actually has, why, when, and where. There is no model, no reconstruction in Montheil et al., 2023. Just an illustration that paleomagnetism demonstrates major intraplate deformation. What that deformation is, and how it places all other findings on vertical and horizontal motions into paleogeographic context is shown here. The point of our paper is to evaluate the novel opportunities that this offers, not to choose between competing hypotheses. The main new finding is that the reconstructed forearc offers to explain the timing of peak dispersal, whereas no previous hypothesis could.

Additional line-by-line/figure comments:

Line 61: Somewhat awkward sentence. Maybe just list the numbers or the Epochs? Saying Paleogene to Neogene is effectively saying just Cenozoic.

We modified this sentence accordingly.

Line 75: As mentioned above, the manuscript never fully describes the tectonic and geodynamic factors that cause shortening and uplift.

The causes of the shortening have been addressed in the cited papers, and are not directly relevant here. We have cited some papers that offered dynamic hypotheses to explain the deformation history, but whether those did so correctly or not is of no consequence to the impact of our paper.

Line 77: initiate --> initiated

Corrected

Figure 1: This diagram is missing the Southern Caribbean fold belt (e.g., Pindell and Kennan 2009). Furthermore, from my recollection, most present-day motion is accommodated on the Bocono fault in

the Merida Andes, defining the Maracaibo block. I think this was left out for simplicity, but I thought it was worth noting for accuracy.

We added to the main text a figure (Fig.2) containing the main fault names, timing and displacement, that were formerly part of the Supplementary material.

Line 103: “to actual volcanic arcs”?

We changed “actual” for “present-day”

Line 106: This sentence is difficult to read and grammatically incorrect.

We modified this sentence.

Line 110: I am not sure that “oceanic” is needed.

Noted, we deleted it.

Line 112: complexes --> complex

Modified.

Line 113-114: Unclear

We have reformulated this.

Line 118: add “ago” for clarity

Done.

Line 133-135: Revise for clarity and correctness

We have moved this to the tectonic reconstruction paragraph and reformulate it.

Line 137: Revise for clarity

We have modified the sentence accordingly.

This paleomagnetic rotations fits with the model are also justified in Supplementary SX.

and Line 138: Revise “allow explaining”

Done.

143-144: Here you mention the motion on strike-slip faults but never really address how total displacement is distributed.

We have modified the text accordingly.

Line 146-147: Missing citation

Modified.

Line 148: I don't remember the Muertos fault being definitively connected to the EPGFZ(?)

This sentence has been deleted in the new version.

Line 150: I would be careful here with the word connected. I know what you mean, but maybe use a word like transferred instead. To me, I read connected as a single discrete structure, whereas in these cases there are probably complex transfer zones between more discrete structures.

This sentence has been deleted in the new version.

Line 163: The use of the word lengthening is confusing here.

Lengthening means getting longer. It appears to be the more accurate in that case.

Lines 165-168: This sentence could be improved/split for clarity. It is hard to read and understand.

We re-arrange this paragraph and delete this sentence.

Line 169: Can remove "such a"

Reformulated.

Line 186: Maybe add which relative direction they moved (south, right?)

We have moved this to the geodynamic paragraph and reformulated it for more clarity.

Figure 2: Isn't the Granada basin extension considered more trench-perpendicular (Garrocq et al., 2021)? Maybe potential uncertainties around this should be discussed in the main text.

As the subduction trench is curved, the Grenada-Tobago basin is opening perpendicularly to the northern part of the trench but parallel to the southern part (see Fig.12b of Garrocq et al., 2021).

Uncertainties are discussed in Garrocq et al., 2021 and we choose here to go with their preferred model. Nevertheless, the 15° CW rotation of the model have changed their original opening directions from NW-SE to W-E, as discussed in the tectonic reconstruction section now.

Line 194: Remove umlaut

Removed.

Line 201: 50 --> fifty

Done.

Lines 211-214: How could the pathway be semi-continuous and completely overland? Probably should make something like “partially overland”.

We explained this now as a 'transient landbridge': This thus may have formed a transient land bridge: perhaps there was never a continuous land bridge, but an interconnected chain of large islands, separated by narrow and shallow sea corridors, that may have connected and disconnected in various configurations over time, effectively allowing biological dispersal.

Figure 3b: Garroq et al. show the highest amount of extension in the southern Grenada basin, which remains until the Oligocene. Do you disagree with that in this model? In place, you show a narrow, shallow sea.

We do not disagree with this model, we have modified the Fig. 3B accordingly, increasing the extend of Grenada-Tobago related deep sea areas.

Line 234: I would remove the word novel in most places in this manuscript since this idea has already been proposed in Montheil et al. (2021).

The ideas were already proposed in precedent paper but the tectonic and paleogeographic reconstructions are new, we have thus leaved this adjective when we are talking about the reconstructions.

Methods:

Line 460: As mentioned above, I think this would improve the manuscript's impact (and it wouldn't be too hard; you could even use rigid blocks or marker features representing unknown areas).

As responded above, the deformable-plates option in kinematic modeling is just cosmetic and makes the resulting reconstruction nicer to look at, but less clear. It can easily be adjusted to deforming topologies if someone would want to use it in a numerical model.

Supplemental:

Line 610: Missing reference

Reference added.

Line 620: Then why include it in graphics? Or is it quantifiable and major offshore?

Because this fault have been proposed as northern Caribbean plate boundary in some reconstruction.

We now discussed it line XX.

Line 646: There are many references to Boschman et al. 2019. I get that that paper is a review and contains many references (which should be stated with “and references therein”), but Boschman et al. were not the first to compile all of these data; for example, much of Pindell's work. I suggest

substituting many of the Boschman citations for the original work presenting those data and ideas. In this case, the work of S. Whattam for example.

We added and reference therein.

Lines 755 – 759: While a key citation suggesting subaerial exposure of the forearc, this is the only mention of the Barbados prism in the compilation of constraints for the reconstruction.

We mention this in the main text, there in the supplementary and in Figure 2.

Line 767: Subheadings almost in the middle of the page?

Yes, these are subparagraphs.

Line 803-804: The SFZ is not transpressional along its entire length.

We reformulated accordingly.

Line 858-859: Do you mean to say “from” instead of “since”?

Indeed, thanks, we reformulated this sentence.

Lines 870-872: The timing cited here is incorrect. Cooling is shown to occur from the Eocene to Oligocene.

This timing is for the plutons in Puerto Rico but the Virgin Islands ones cools and exhumes during the early Miocene as stated in the abstract of Roman et al., 2021: “In contrast, the northern Virgin Islands experienced rapid cooling and exhumation in the early Miocene (ca. 24–21 Ma) associated with the extensional opening of the Anegada Passage ”

Reviewer #4 (Remarks to the Author):

The paper is well written and illustrated, and interesting to read. It provides a new tectonic reconstruction that has developed as a follow-up study from recent palaeomagnetic and geological work in the northern Antilles (Montheil et al., 2023, Tectonics, GSA Bulletin) and builds upon significant geological and geophysical advances in the eastern Caribbean over the past 5 years. The study, although not using a novel method/approach and not providing a novel dataset, provides new and significant constraints on the tectonic feasibility of a recently proposed paleogeographic concept that represents an alternative or complementary solution to the GAARlandia hypothesis (as formerly stated in Montheil et al. 2023, Tectonics). This has large regional interdisciplinary relevance and is a great example of the significance of tectonics in controlling some biological processes. The tectonic reconstruction is supported by an extensive geological and geophysical Caribbean synthesis and seems overall very sound regarding tectonic implications in the eastern Caribbean. I only have a few

questions below on the relevance of some regional geological aspects that are not explicitly discussed in this manuscript. Overall, it is an interesting and useful contribution that promotes a new tectonic interpretation of the eastern Caribbean and that is notably relevant for regional palaeobiology. As a field geologist I think there is an interesting and concrete added value in this study and, thus, I am supportive of its publication.

We thank the reviewer for these positive and supportive comments and appreciate that they see the novelty in providing a kinematic tectonic reconstruction to illustrate a novel paleogeographic insight.

It could be argued that the study does not provide new critical observation/data to support or refute significant land emergence in the eastern Caribbean during the Palaeogene, and this could be regarded as a weakness if the main goal of the paper is to demonstrate the feasibility of a land connection (or string of large islands) linking N and S America in the Paleogene. The manuscript suggests that the situation remains unclear in terms of actual land connection due to relatively scarce availability of geological data in this mostly submarine area. It is a fair take because probably the only way to test this aspect would be to drill possible ancient islands in the area to directly document their existence and longevity - so sadly with the current state of the IODP, this seems out of reach for many years still. In this context, the study makes in my opinion the most out of available data to help address a long-standing paleogeographic issue. A corollary of the scarcity of geological data is that the paleogeographic debate will probably not be settled by this study. This point is however recognized by the authors and presented in the concluding remarks as a way to stimulate interaction between the geological and palaeobiological communities. I personally agree with them, there is value in this. In a way this reminds me the start of the huge debate in the last decade about the formation of the Isthmus of Panama, but with the important difference that in this new study the geological rationale (although partly embedded in other recently published studies) appears sounder than in the Panamanian case. Critically, this study avoids creating potential misunderstanding stemming from confusion between tectonics and paleogeographic concepts. As outlined in the manuscript it is very much a tectonic study, well supported by recently published geodata, with some interesting possible paleogeographic and biological implications. So, from my perspective, the positives clearly outmatch the negatives, and I am therefore supportive of publication in Communications Earth & Environment.

Main comments/questions:

1) As stated in L202-205: "a connection with South America was likely formed by either stepping stone islands or a continuous land connection over the northern Venezuela and Barbados accretionary prism and forearc (Fig 3B)". Are there observations supporting land emergence close to S America beyond the Barbados accretionary complex? The study builds upon new data from the northern (and older exposed) Antilles, but what about the geological record closer to S America in the Palaeogene?

This should be discussed/synthesized somewhere, since at present it looks like an important remaining question mark, but which is not explicitly treated as such. For instance, is it possible that there were volcanic islands south of Antigua area in the Paleogene? Often an issue with volcanic arcs is that older volcanoes are buried under younger volcanic deposits. Could a large area without islands have subsisted in the Paleogene, and if so of which size? I understand that the paper tries to show the feasibility of a continuous string of islands between N and S America, but it looks like it could better critically embrace the lack of constraints in some areas - in other words if you can better show that any remaining area without islands would have been small anyway, this will strengthen the implications of the model, not weaken them.

Thanks for this comment, we expanded the paleogeographic implications paragraph to outline in a better way the data available for closer to South America geological record (i.e. Northern Venezuela, Leeward Antilles and southern Aves ridge region).

2) In Montheil et al., 2023 (Tectonics) there is a discussion on the relative contribution of forearc sliver motion, internal block faulting, and oroclinal bending of the arc as possible causes to explain large block rotation in the northern Antilles, which is not taken forward, or at least only partly, in this new study. Why is that and could this be explained in the manuscript (other readers will also wonder if they read related bibliography)? The presented model is apparently an endmember in terms of both mechanism (e.g., no internal block faulting) and extent of rotation (only the most extreme statistically possible rotation values are considered) - the text should clarify what would be geometric implications if there was a combination of deformation mechanisms and/or less extreme rotation used in the tectonic reconstruction.

Actually, we do not include the most extreme scenario. Reconstructing the $\sim 40^\circ$ rotation that we document for the northeastern Caribbean region back to the late Eocene automatically satisfied the 70° rotation in the early Eocene by reconstructing the Grenada basin. We have explained the (im)possibility of the alternative scenario that we identified in Montheil et al., 2023, (see response to Rev 3), also in the text and added the Figure S4 accordingly.

3) The Caribbean Plate is considered to have insignificant internal deformation in the reconstruction. However, Beata Ridge, which apparently records long oceanic plateau magmatism (Dürkefälden et al., 2019) and has been recently (moderately or insignificantly?) deformed, suggests that the plate was at least not perfectly rigid. There has been rumours for years that unpublished paleomagnetic data from the German (U. Barckhausen?) indicate a complex formation of the plateau (if it is all plateau). In 2019 I saw some (obviously unpublished) seismic data from the petroleum industry from the "oceanic plateau" close to Colombia-Venezuela with rifts and grabens that can only make one wonder how much internal deformation has actually occurred in this plate. Although this is obviously not peer-reviewed data and is technically unusable, the scarcity of constraints on the rigidity of the Caribbean

Plate calls for prudence. So my question is: would internal deformation of the plate change the outcomes of the modelling regarding the evolution of the Antilles? This should probably not be totally disregarded considering that most regional experts (possibly also some of the co-authors?) remain very cautious regarding the nature and evolution of the interior of the Caribbean Plate. A small discussion or at least a mention of this would be welcome.

In our comprehension of the available bibliography, the Colombia-Venezuela rifts are inherited from the Farallon plate oceanization that happened during the Jurassic and Cretaceous, before being partly covered by the CLIP and therefore before the timeframe of the reconstruction, which is why we did not mention this in the manuscript. In our reconstruction we have restored 50 km of shortening in the Beata ridge to facilitate the 15° clockwise rotation of the Caribbean plate interior. Nonetheless, any deformation west of the Beata Ridge is not relevant to the outcome of the reconstruction or the biological dispersion discussion - only the failure of the paradigm of eastern Caribbean plate rigidity is. We have explained this in the introduction and discussion.

Addressing Points (2) and (3) above may perhaps benefit from testing a few alternative solutions to address the full possible variability of existing constraints to better outline the robustness of the presented (favoured) tectonic model/interpretation.

David Buchs, Cardiff University

Minor comments:

L118: Isn't this also proposed by Pindell and Kennan 2009?

That is correct, we have added this reference.

L130: "subduction initiation" would require no previous subduction (at least for most readers), which seems difficult to have here since there is an older arc (and slabs) in the area. Do you mean resuming of arc volcanism and/or development/lateral propagation of a new volcanic front?

There is evidence for Cretaceous subduction below the Caribbean plate, but the modern slab clearly formed in the Eocene. We added a recent slab reconstruction of Chen et al. (2024) as further support and clarification. The Cretaceous and older subduction occurred when the Caribbean plate was located west of Colombia.

L197-198: Montheil et al. 2023 does not look like the correct reference for constructional volcanism from 42 to 38 Ma - as you know the oldest igneous rock dated in this paper is 35 Ma. The Basal Volcanic Suite of Antigua is apparently only age estimated in this paper. Is there other supporting evidence in the literature? This should probably be clarified in the manuscript.

That is correct, we added other references (Philippon et al., 2020 and Noury et al., 2021) accordingly.

L219-221: "GrANoLA-land submerged and tectonically broke up during an episode of late Oligocene to Miocene regional subsidence triggered by cessation of arc volcanism⁸, and by trench-perpendicular tectonic extension²¹ (Fig. 3A).": reference 8 (Japanese paper) is not the correct one. Saying that regional subsidence was triggered by cessation of arc volcanism seems very brave here because it could also be related to tectonics, e.g., crustal tectonic processes or subduction dynamics, not just post-volcanic lithospheric subsidence. You should tone down the interpretation of the cause of the subsidence here considering regional tectonic complexity. Mantle and subduction dynamics close to a subduction zone will likely play a larger role than "simple" thermal subsidence that is more commonly a dominant process for intraplate volcanoes far from subduction zones.

The cause of subsidence was removed from the paper, as it is not relevant. The relevance is the evidence for the subsidence.

L638-646: This is perhaps not critical for this paper, but I would like to provide some insight into this section given that I have worked in the area for 15 years (with >1 year of regional field work, still going on in 2025). Buchs et al. 2010 and Montes et al. 2012 are so far the only papers documenting cross-cutting relationships between the oceanic plateau and the arc. Buchs et al. 2010 is the only published study documenting old (73 Ma) arc dykes cross-cutting oceanic plateau sequences, with reliable (biostratigraphic) data on the older arc sequences. We have new, unpublished regional data stemming from 2 PhD projects (including lithostratigraphy, geochemistry, geochronology, and biostratigraphy) that support this stratigraphic relationship across Panama toward Colombia (>500 km along strike) - we are in the process of collecting the last geochronological data for publication to constraints distinct newly-recognised magmatic stages during subduction initiation around 75 Ma. None of existing field observations and associated data support earlier arc volcanism in the area. Given the relatively widespread regional exposures of earlier arc sequences overlying an oceanic plateau basement, it is rather strange that no clear direct evidence has been found so far for an earlier volcanic arc if there had been one in this area. Nicoya area, close to the suture with the Chortis Block (e.g., Siuna, Santa Elena), is a complex tectonic assemblage that does not provide a clear insight into the Panama evolution, as is, e.g., suggested in Andjić et al. 2019, which is an interpretation alternate to that of Boschman et al. 2019 and that also makes a rather convincing use of newer local geological constraints. So far, >75 Ma subduction in Panama has been proposed based on distant geological constraints or hypothetical regional scale mechanisms but local geology does not support the occurrence of >75 Ma arc sequences there. If it is an issue for/from wider scale tectonic constraints, I would be happy to discuss this further given my interest in helping solve early Panamanian palaeogeography. Note also that new results in the northern termination of the Andes are showing growing evidence for a complex tectonic history with multiple arcs and oceanic plateaus in the SW

Caribbean (e.g., Correa-Restrepo et al., 2023; Spikings et al., 2024). The suture between Panama and S America is expected to be equally complex. Has this any incidence on eastern Caribbean tectonic reconstructions, though?

We thank the reviewer for this discussion. The difference between a 90-100 or 75 Ma inception of subduction in the western Caribbean subduction zone is that in the latter case, the Caribbean plate was part of the Farallon plate, and was thus rapidly converging with South America at that time. What would be puzzling about that scenario is that the western South American margin was buried in a subduction zone around 100 Ma already, which would be impossible if the Caribbean plate was still located 1000 km or so to the west at that time. So the underthrusting of South America below the Caribbean plate is an alternative constraint, and suggests that after this time, there cannot have been full plate convergence between Farallon and South America at a plate boundary east of the Caribbean plate. Hence, this calls for the existence of a subduction zone to the west by that time. This would imply that it takes a while for the Central American volcanic arc to start. For the current paper, however, the inception of subduction initiation in the western Caribbean subduction zone in the Cretaceous is of no consequence.

L723: this is only supported by references in these papers, not new data in these papers - add "and references therein"

Done

Reviewer #1 (Remarks to the Author):

Recommendation (Required): I recommend publication upon addressing comments

Significant: Yes, the paper is a significant advance or contribution.

Supported: Mostly Yes

Referencing: Yes

Quality: The organization of the manuscript and presentation of the data is appropriate

Data: Yes

The authors provided an updated version of the manuscript "Paleogene subduction zone processes tectonic deformation along the Lesser Antilles arc", now titled "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles arc". I confirm I was able to load the .gpml and .rot files in GPlates and visualize the tectonic reconstruction. However, I believe the animation should be done properly, maybe using FFMPEG or scientific software such as MatLAB, Python.

Thanks for pointing that out, we have now updated the videoclip in a more appropriate way (file name : Caribbean_Reconstruction_50-0Ma.mp4).

The new title clarifies that the paper focuses on the paleo-geography (physiography) and kinematics, not the geodynamics. However, the introduction of the paper heavily highlights the importance of subduction processes in inducing physiography changes. However, there is no significant discussion of these processes in the text. I think the intro should be adjusted to reflect the true focus of the paper.

The authors say that "We show in our paper a kinematic reconstruction, for which understanding the drivers of deformation is irrelevant - the drivers may be inferred from the reconstruction.". However, the drivers of the deformation should inform the reconstruction, not the other way around. Your reconstruction is developed in a plate tectonics geodynamic context, where subduction processes are very important (as you mention in the intro). Hence, the kinematics you propose must be at least geodynamically plausible. You don't need to "prove" your "saloon-door" mechanism is correct or perform geodynamic simulations in this paper, but you can at least provide your thoughts on what mechanism could explain this very complex kinematics. You already provided an explanation of it in your reply. Adding this to the manuscript and a supplementary figure will strengthen its scope and will harvest future discussions in the context of Caribbean tectonics and paleo-geographic reconstructions.

We fundamentally disagree with the statement that interpreted drivers of deformation should inform the reconstruction - it really is the other way around. Reconstructing plate tectonics does not require understanding the drivers of plate tectonics: reconstructing the motions of the plate informs us about those drivers. For the Caribbean case, the finite strain patterns may give insights on plausible drivers and our interpretation of those drivers should never lead to adjustments of the reconstruction, for we would introduce circular reasoning. We acknowledge that such drivers could be further used as input parameters in geodynamical numerical models to test our hypothesis but that is beyond the scope of our work. We also would like to reassure that the entire reconstruction process was guided by principles of geometric consistency. Only where two geometries could explain the data, geodynamic plausibility may be used as argument, which, for example, led us to rule out the

oroclinal bending model (see Fig. S4).

That said, we fully agree with Reviewer 1 that subduction-related processes must have played a fundamental role in driving upper plate deformation. In response to the repeated request of the reviewer to discuss the geodynamic drivers of the Caribbean history that underpins the biotic exchanges, we have therefore added a section to the discussion in which we interpret the potential drivers of the deformation and magmatism that we reconstructed:

“Interpretation of the drivers of eastern Caribbean deformation

The reconstructed sliver motions of the GrANoLa and Northern Venezuela–Leeward Antilles arc and forearc blocks, concurrent with the opening of the Tobago-Grenada back-arc, the increase in trench length, and the eastward retreat of the southeastern Caribbean trench in a mantle reference frame (Doubrovine et al., 2012; Fig. 3) suggest that slab rollback played a significant role in the tectonic evolution of the eastern Caribbean plate. This phase of slab roll-back occurred in the first ~10 Ma after initiation of the Lesser Antilles subduction zone (Fig. 3C, 3D). Before 50 Ma, Caribbean plate motion relative to the Americas was NE-ward, accommodated by SW-ward subduction below the Greater Antilles (Pindell and Kennan, 2009; Boschman et al., 2014). The subduction trench at 50 Ma extended from Cuba to the Virgin Islands (Fig. 3D), also shown by the occurrence of Cretaceous to early Eocene arc-related rocks on these islands (e.g., Hu et al., 2022). During this time, the eastern Caribbean plate margin was dominantly a transform fault (Fig. 3D). However, during the ~50 Ma change to eastward relative motion of the Caribbean Plate, this transform became a new subduction zone. Our reconstruction shows that the roll-back of this slab is mostly concentrated in the period of opening of the Grenada-Tobago basin, and occurred during the first hundreds of kilometers of subduction below the eastern Caribbean plate, during which period the slab was still only located in the upper mantle. We hypothesize that rapid roll-back during the late Eocene opening the Grenada-Tobago Basin occurred as long as both poloidal upper mantle flow (below the slab) and toroidal flow (around the slab) was possible. Once the slab reached the base of the upper mantle, poloidal flow impeded and roll-back ceased, after which the Caribbean plate became nearly mantle-stationary, and relative motion with the Americas was accommodated by the westward advance of the Americas. During the late Eocene roll-back phase, the relative motion between the eastern Caribbean plate margin and the buoyant and rigid Bahamas Bank indenter and the South American promontory (Wallace et al., 2009; Martin, 2013) was highest. We infer that the contrast between friction in the north and south with roll-back in the center may have caused the Venezuela and GrANoLa sliver motion. Analogous tectonic histories may be found in the western Mediterranean region, where roll-back of the Calabrian and Gibraltar slabs led to the opening back-arc basins accompanied by opposite rotations of forearc accretionary units on Sicily and the southern Apennines, and in the Betic and Rif orogens, respectively (Cifelli et al., 2007; 2008; van Hinsbergen et al., 2020). These motions continued but slowed down once roll-back ceased, driven by ongoing relative Caribbean-Americas motion.”

217-219 I think that any comparison with other back-arc basin subduction systems should include a discussion of the geodynamic mechanisms. Two different geodynamic settings cannot be compared solely based on kinematic similarities.

We have included this in the new discussion on the drivers. The setting of the Caribbean case and the Calabrian and Gibraltar cases is both kinematically and geodynamically very similar.

235-240 You provide evidence supporting land emergence around 45 Ma and suggest the existence of an island chain divided by narrow sea corridors, which may have exhibited a dynamic or fluctuating topography (i.e. Connection and disconnecting of narrow sea corridors over time). Also, you mention that land emergence can be related to a drop in sea level 50-70 m. Have you looked at short-term sea-level curves that could support these fluctuations in land emergence?

We have added an appropriate reference (Katz et al., 2008) in the manuscript. These authors conclude, based on a site located in the Gulf of Mexico, that during the Eocene-Oligocene Transition "relative sea-level decrease of approximately 105 m, which equates to a 67 m eustatic fall".

Part of the evidence you presented for validating the submergence phase is indeed the growth of carbonates and carbonates are affected by changes in the sea level. I wonder if this is the mechanism that the manuscript is missing.

Marine limestones are indeed affected by fluctuation of sea-level but are nevertheless unequivocal evidence of the presence of sea, and we use them as sea-indicators only. We added the geodynamic mechanism in response to the general comment.

260-265, you claim that the land pathway you reconstructed offers an explanation of the tectonic drivers of emergence. As I mentioned before, I don't think the current manuscript describes these processes anywhere in the text. Could it be that the mechanism that explains the emergence and submergence is not tectonic, but paleoclimatic ? (i.e., sea-level changes)?

Sea level changes may have played a role, as we indicate, but it was the geodynamic drivers of the deformation that brought the eastern Caribbean in a position where such subtle sea level effects could have had an effect. We modified the paragraph accordingly, and expanded a sentence that was already there stating that drop in sea level probably favoured land connection.

267-268 Again, you are invoking trench-parallel extension to explain the "destruction" of land bridges. However, your reconstructions display slab roll-back in both the "emergence" (45-38 Ma) and "submergence" phases (35-25 Ma).

First, the slab roll-back phase is only the first one - after that, it slows down. Nonetheless, the roll-back does not one-to-one correlate with the formation of individual faults, it only provides a large-scale driver of deformation. Hence, crust has been previously magmatically and tectonically thickened (Philippon et al., 2020 PlosOne and Montheil et al., 2021 GSA Bull.) became affected by the trench-parallel extension until it was thin enough to be submerged.

307-309 I probably missed it, but I didn't see where you provided the mechanisms of

formation and destruction of the biological dispersal pathway. Are you saying the magmatism versus the absence of magmatism?. If yes, what mechanism explains the cessation of magmatism? . especially when slab-roll back never stops between 50-25 Ma. We modify the sentence according to the reviewer comment: "Our reconstruction provides solid geological evidence for a time window, mechanism of formation and destruction (i.e. tectonics and volcanism) of a physiographic pathway that served as a biological dispersal corridor. " and add sentences in the Cenozoic paleogeography section to better outline how tectonic history explain the paleogeographic evolution of the eastern Caribbean region. For example "Based on our reconstruction, we interpret that the GrANoLA land creation most probably resulted from the combined action of the northern Lesser Antilles magmatic flare-up and of contractional tectonics due to interaction with the Bahamas bank."

Also, the conclusion mentions that "Our reconstruction provides solid geological evidence for a time window, mechanism of formation and destruction (i.e. tectonics and volcanism) of a biological dispersal pathway". However, there is no significant discussion of these "mechanisms" in the text. What do you mean by tectonics?, I don't see how volcanism explains the extension.

We do not state that volcanism explains extension. We state that vertical land motion is tectonically and magmatically driven. The extension is triggered by roll back as well understood by the reviewer in regards to his comments above, and as covered in the added interpretation section.

Technical questions

While the undeformable block approach you used facilitates comparison with geological data, it significantly hinders integration with automated computational workflows—severely limiting the scope and quantitative usability of your reconstruction for future applications such as geodynamic simulations. I think implementing closed polygons will increase the impact of your reconstruction and clarify how the kinematics you reconstructed might explain the land bridges you are proposing (if the causes are indeed tectonic).

We acknowledge that the close polygon approach may be of interest for further studies but the present study is large scale and provides a first order idea of the sliver block motion along the trench explaining potential land connection with South America. The closed-polygon approach that the reviewer here suggests would numerically require regionally distributed shear, which is not accurate. The reconstruction that is presented in this paper is being included into a global model that includes orogenic deformation, made in such a way that modelers using automated computational workflows do not misrepresent the deformation history. This is beyond the scope of the current paper and not relevant for the conclusions we draw here.

Minor comments

Fig. S4 I think it would be beneficial to include the "saloon-door" model to facilitate comparison

Done

Reviewer #3 (Remarks to the Author):

2nd review of: Caribbean biodiversity shaped by Paleogene subduction zone processes
The revised manuscript and rebuttal submitted by Montheil et al., fully satisfies the comments/criticisms raised in my first review. The authors have been more explicit with their uncertainties and the improved feasibility of their model over existing reconstructions. One important new clarification is that they are not implying that full emergence is necessary or that there are necessarily issues with current models, but that the islands/rafting hypothesis is probably more likely. Also, the omission of the comment on geodynamics and addition of citations to the geodynamics articles that describe potential geodynamic mechanisms is important and appreciated.

Overall, in this new version, I think that the aim of the paper is outlined much more effectively, focusing on the tectonic synthesis, Gplates implementation of paleomagnetic rotation and other recent geological data, and the extensive discussion of the links between the new reconstruction, tectonic implications and potential faunal dispersal pathways. In my original review what was meant by “general lack of data”, is that this paper does not really add any new data to constrain the reconstruction or strengthen the argument that there was a full or partially emerged landbridge. The important contribution is really the new reconstruction, and I think now the manuscript clearly explains that the paper is a synthesis of an ongoing debate supported by that new regional reconstruction. The added emphasis on the improved timing and new segmented and highly mobile forearc model for the eastern Caribbean explains this well.

[We thank reviewer 3 for acknowledging the improvement made on our manuscript following the first round of comments and suggestions by the reviewers.](#)

However, while the supplemental does broach this subject a bit, I do think that the authors could include additional explicit explanation in the main text of how their reconstruction stands apart from, and is an improvement on, previous models (e.g., Boschman). For example, the authors redrew and subdivided blocks (e.g., split Caribbean plate interior into basins), completely redid the Puerto Rico rotations, corrected some plate boundaries, etc. These are very valuable improvements, and I am eager to use it as it helps explain new thermochronological data in Hispaniola I have in a publication currently under review. I offer some additional suggestions below.

[We followed the reviewer's suggestions and added more details on the improvement made in our study compared to previous models.](#)

Just a note of GPlates deforming meshes: In my first review I mentioned the potential to use a deforming mesh. The authors pushed back, and I agree that the rigid block approach is fine for the purposes of the article, that was mainly just something pointed out for additional consideration. However, for the sake of the GPlates developers, it should be noted though that the deforming mesh is not just “cosmetic”, but uses deforming boundaries and interior points to calculate strain quantities which can be analyzed and linked to geological or geophysical data, e.g., gravity, seismic stratigraphy, and tomography (see Gurnis et al., 2018). For example, one neat application that I have recently seen is using the deforming mesh framework to invert present-day crustal thicknesses in a volume restoration sense to evaluate plate models, which is far beyond “cosmetic”. That said, with the authors' modifications I do think the rigid block approach is fine., although I think there are necessary

changes to the text concerning the use of the term rigid for the eastern Caribbean (see below).

We acknowledge the reviewer comments and agree with the potential of the deformable polygons approach to evaluate plate models. As already replied to the Reviewer 1 comment the present study is large scale and provides a first order idea of the sliver block motion along the trench explaining potential land connection with South America. The application that the reviewer mentions here is precisely why quickly adding a 'deformable plate' model should be handled with care - it will immediately be interpreted in model space without considering the underlying geological history. This will be done thoroughly, globally, and in another paper as pointed out above.

At this point, I think that the manuscript is ready for publication after the consideration of a few more (minor) points:

Use of "rigid" in abstract and throughout text

In the abstract on line 25 it is stated: "However, tectonic reconstructions have always assumed a mostly rigid eastern Caribbean plate, such that migration routes must either have been overwater, or overland through vertical motions emerging and submerging oceanic crust."

I understand what you're saying in some sense, but I think it is necessary to add something like coherent or unbroken and mobile (as stated already on line 230, e.g.), since rigid alone does not necessarily mean that the plate is unbroken. I think this is especially necessary since you mention rigid blocks in reference to the GPlates model and without a deforming mesh, you are still modeling the eastern Caribbean as rigid, just broken into more blocks. [We have modified the text accordingly.](#)

Suggestions: Emphasis on differences between existing and new Gplates models

Looking at the Zahirovic/Müller or Boschman reconstruction in detail, it is striking how different the rotations and block definitions are. I think this new model is indeed a significant improvement to current GPlates implementations in the eastern Caribbean and this could be emphasized. In particular, PRVI motion is totally redone, e.g., in the other models it is weirdly tacked on to southeastern Hispaniola then rotates clockwise away, while the PRVI block in this model is in the forearc to the east and rotates to its present position in what seems to me a more sensible way given cited paleomag and geological data.

[The differences with previous reconstruction were already briefly introduced and discussed but we now emphasize it more by adding sentences in the approach and tectonic reconstruction paragraph.](#)

As an idea, one simple way to show this, that could go in the supplemental, is to use the kinematics tool in the GPlates GUI to track that rotation or changing lat lon position for each model through time, then in map view and/or time vs. rotation space plot how the models are different (in mantle or NA reference frame, for example). This will show how significant this change is, as before there is no way to get the PRVI block in the correct position for a landbridge. For example, I quickly made some map view examples showing the comparison from 66 Ma to present with different reference frames (below). The new reconstruction also does a MUCH better job of classifying and subdividing the blocks compared to Zahirovic/Müller, which could be highlighted.

The Gplates GUI track is a very interesting idea, thanks for pointing that out. We think that the added text is a sufficient editing for the reader to grasp the difference with previous reconstruction, and that these Gplates GUI comparison could be made in future (review) studies.

Line by line comments and figure comments:

Body

Line 46: basin basins

Corrected

Line 63: remove umlaut over i in Cenozoic

Corrected

Figure 1: Is there any meaning to the color of the blocks? Perhaps clarify in the legend or caption.

Thanks for pointing that out, we have added the color code to the caption.

Line 141: “Approach” seems to be out of place

It is in place, it is a subheading

Line 158: indicators indicators

Corrected.

Line 167: Subduction indicators consistency consistency of subduction indicators

Corrected.

Line 174: Builded built

Corrected.

Line 181: maybe add the word “implemented”

Corrected.

Line 207: add “The” before Venezuelan

Corrected.

Line 221 – 223: Yes, but this was not the only cause of shortening and uplift at the time and shortening and uplift also occurred in Hispaniola earlier in the late Eocene – Oligocene. This comment may read a bit nitpicky because the statement is not false, but the authors may want to reword to clarify that this is just one event among others causing deformation in Hispaniola. Also, the reference to Wessels is probably not the best to use since that is only a review. I think Dolan et al. (1998) is better since the interactions between the Bahamas Bank and northern Hispaniola are discussed at more length.

The reference has been corrected accordingly and the text modified too.

Line 231: I suggest rephrasing “invites...” It is a bit awkward phrasing.

Invites has been replaced by allows

Line 309: consider changing to “provide a synthesis of independent evidence”

Corrected according to the reviewer comments.

Supplemental:

GPlates files: There are a few errors loading the file. Doesn't affect function, but would be good to clear if possible

We have fixed these errors, the topography layer was missing.

Supplemental movie: This is fine, but, I want to say again, that it would look much sharper, more professional, etc., if the movie is made by exporting individual frames and combining using ffmpeg, an online converter, Matlab, Python, etc. Here is a script I have written for other purposes that should work if you would like to use it:

Thanks for sharing this, we have now updated the videoclip in a more appropriate way (file name : Caribbean_Reconstruction_50-0Ma.mp4).

```
% clear; close all; clc
%% Movie making script from images
% Written by Ethan Conrad
% Desc: create a movie from frames. Export frames in GPlates using export tool at
% whatever Myr increments you want. Make sure unique beginning of file
% name. That needs to be input in the command line to pick the images.
% might be issue with path, just make sure to add everything and should
% work fine

% Change these things:
videoquality = 75; % this will make your file bigger or smaller, balance with quality fps = 1; %
frames/sec - set this for movie speed
fileout = 'Recon'; % name output mp4 imagetype = '.jpg'; %input image type

% change the first two entries to move the XX Ma annotation in the movie annotposition =
[0.0813875968992247 0.15 0.11915503875969
0.0574625828554855];

% Set up
selpath = uigetdir(path); % select path in gui

cd(selpath) % switch to that directory
prompt = {'First few letters of file name?'}; % enter in command line unique name

% dialog box settings dlgtitle = 'Input';
dims = [1 35];
definput = {'Pic'}; % default
Fname = inputdlg(prompt,dlgtitle,dims,definput); Fname = Fname{1};
str = sprintf('%s%s', Fname, imagetype); beginningnum = 0;
startnum = 1;
start = startnum - beginningnum; files = dir(str); % select files nImages = length(files);

%% set up video writer
```

```

writerObj = VideoWriter(fileout,'MPEG-4'); writerObj.Quality = videoquality;
writerObj.FrameRate = fps; open(writerObj);
h = figure('units','normalized','outerposition',[0 0 1 1]);

% Create movie
for idx = start:nImages-1
%%
image = imread(files(idx).name);
im = imshow(image); % show image str = sprintf('frame#: %d', nImages-1); fig = gcf;
% create Ma annotation annotation(fig,'textbox',... annotposition,...
'String',{sprintf('%s Ma', files(idx).name(7:10))},... 'FontSize',24,...
'FontName','Helvetica',...
'FitBoxToText','on',...
'BackgroundColor',[1 1 1], "FaceAlpha", 0.5);

%%
set(fig, "Color", 'w') f=getframe(h);
writeVideo(writerObj,f); % write statement cnt = cnt +1;
end
% close everything close(writerObj) delete(writerObj) close all

---
Ethan Conrad

```

Reviewer #4 (Remarks to the Author):

This is my second review of this manuscript, and I am still finding it interesting and suitable for publication in Communications Earth & Environment. The reviewer's comments have generally been thoroughly and constructively addressed by the authors, and the clarity of the methods, argumentation and discussion has been improved. The quality of the manuscript is therefore also substantially improved. I have three main points that the authors should consider before publication, all three are minor given the breadth and extent of work already invested in this study and the thorough replies to the previous comments.

[We thank reviewer 4 for acknowledging the improvement made on our manuscript following the first round of comments and suggestions by the reviewers.](#)

1) A reviewer commented on the lack of usage of geodynamic constraints/models which the authors rebutted on the basis that their approach is evidence-based, with an emphasis on the use of field geological constraints, seismic profiles and paleomagnetic data. This approach could be clarified in the methods and/or the manuscript as readers with an interest in geodynamics will most surely have questions similar to those of the reviewer. E.g., the authors could state why geodynamics constraints are not considered in their approach. It should clarify that it is not an omission, but a methodological choice (valid in my opinion). [We explain in the reconstruction approach how we made the reconstruction. We don't understand why the reviewers are so surprised that we do not mix our kinematic reconstruction with dynamic interpretation: this is standard practice in geodynamic analysis](#)

and avoids publishing self-fulfilling prophecies. Dan McKenzie did not come up with the theory of plate tectonics because he figured out slab pull. Forsyth and Ueda figured out slab pull because Dan McKenzie reconstructed plate tectonics.

By adding a section with the interpretation of the drivers of deformation and magmatism, we cover all the questions on drivers that this and the previous reviewers have, and it is placed after the reconstruction so that it is clear what is based on what.

2) The manuscript content and the replies to the reviewers' comments suggest that there was subduction initiation in the Eocene in the Antilles, although it is also recognised that there was an earlier subduction zone in the area that accounts for the formation of the Aves Ridge as an older volcanic arc. Specifically, it is indicated in the manuscript, l130: "[...] and incipient volcanism, forming the 'GrANoLA [...]". However, existing geological data do not indicate if the GRANoLA includes older volcanic sequences that are not exposed and still contributed to much of the shallowing of the island arc to form stepping stone islands (i.e., new Eocene arc volcanoes that developed on older pre-Eocene arc volcanoes). In the reply to one of my previous comments on subduction initiation it is indicated that the Chen et al. 2024 study (added during the revision to the manuscript) support formation of the "modern slab" in the Eocene - there is some interesting re-interpretation of mantle tomography that suggests the existence of a flat slab under the Caribbean in this study, but I don't understand how this is addressing my point on subduction initiation and subsequent island arc evolution from a volcanic perspective (which might have been unclear in my previous comment). The existence of an older slab is not refuted by the dataset and interpretation of Chen et al. 2024. It is also unclear to me what is the meaning of "modern slab" in the authors' reply. My point here is that most people interested in the long-term (10s of My) dynamics of an intra-oceanic arc and its paleogeographic evolution may consider that the older Aves Ridge Arc (Great Caribbean Arc) may have been dissected/split by the opening of the Grenada back-arc basin, and that the basement of the GRANoLA included parts of the earlier arc (as suggested by e.g. the Jurassic radiolarites of La Désirade that could be part of such basement). This has not necessarily to be associated with subduction initiation, but a new pulse of volcanism that started in the GRANoLA on top of older arc volcanic sequences, thus contributing to volcanic growth and emergence in this area. I think it is the most logical explanation for the volcanic evolution of the area (followed by some other studies in the past), but somehow, I am, personally, not getting this idea very clearly through the revised manuscript and some of the authors' comments in the rebuttal document. I think however that it is an important concept as the GRANoLA hypothesis in this study is potentially competing with the GAARlandia hypothesis from former palaeogeographic studies. The possible splitting of an older volcanic arc and relocation of the volcanic front through time, associated with back-arc opening, seem critical in this discussion as they would contribute to controlling the evolution of the paleogeography of volcanic promontories on both sides of the back-arc basin (GRANoLA and GAARlandia, which are not mutually exclusive hypotheses as now more clearly stated in the revised manuscript). Given the role of the construction of an island arc in land emergence in the E Caribbean, how could it be that "back arc extension did not create land nor destroyed it", as written in the reply to the previous reviewers' comments?

Thanks for this detailed comments a for sharing these thoughts on arc evolution, which we indeed must have misunderstood in the previous review. The reviewer is right: there was an earlier phase of westward subduction below the eastern Caribbean plate, in the late Cretaceous. This phase ended around 80-75 Ma due to collision with South America at the

modern position of Colombia. There was indeed a slab involved, which broke off and is now located below Venezuela (van der Meer et al., Nature Geo 2010). Nonetheless, for this paper, this is too much detail: what is important is that our reconstruction places the Aves ridge 500 km west of the eastern Caribbean plate margin - and in the late Cretaceous this was likely even more since units that were left behind in Colombia may have to be added.

Hence, we take a different route in our interpretation section that we added in response to the first reviewer. That the Aves Ridge would be arc is a speculation, there is not actual rock evidence for this. The only rocks that were studied are the ones dredged from the southernmost part of the Ridge, and they give mostly CLIP signatures - and perhaps some arc influence, which at this southern margin is to be expected. For the rest of the Aves ridge, there is no evidence that it is an arc. Instead, the thick crust of this region could simply be CLIP material, like it is the case for the Beata Ridge. Further light on these questions necessitates an update of the Cretaceous to Jurassic kinematic evolution of the Caribbean region and our group is working on such reconstructions. Nonetheless, this illustrates why it is important to make kinematic reconstructions based on data, and not on speculations of drivers of formation: decades of reconstructions have drawn subduction zones close to the Aves ridge because Burke (1988) speculated it could be an arc, without any evidence. Our reconstruction, based on kinematic evidence unavailable to Burke in 1988, shows that that interpretation faces the challenge that the Aves Ridge was located 500 km west of the nearest plate boundary.

3) Given the east Caribbean focus of the study, I didn't want to shift the scientific debate to the western side of the Caribbean, but the reply to my previous comment on the timing of subduction initiation in Panama confused me - I did not fully understand the plate tectonic argumentation provided in the reply and this led me to realise that there are some shortcomings in the presentation of the geology of Colombia in the revised (and possibly also original) manuscript, which need to be amended before publication.

It is commonly accepted that the Romeral Fault in Colombia is the boundary between (i) the Central Cordillera in the E, that includes the Quebradagrande Complex with an older volcanic arc that likely developed along S America, and (ii) the Western Cordillera in the W that includes an accreted Upper Cretaceous intra-oceanic volcanic arc with slivers of accreted Caribbean (?) Oceanic Plateau. Because of this complex geological assemblage, and in particular the age and current location of volcanic arc units in the Western and Central Cordilleras, it has been suggested by several research groups that there was a double verging subduction system (Molucca plate style) between the Caribbean Oceanic Plateau (and its intra-oceanic volcanic arc) and South America (and its continental volcanic arc) before collision of the plateau with the continent in the Campanian.

Issue 1: Figure 2 and other supplementary figures show plateau rocks on the wrong side of the Romeral Fault, or the Romeral Fault is misplaced/mislabelled - should it be instead the Uramita Fault Zone? This labelling issue should be corrected, and it may also be useful to the editor to clarify that this has no impact on the outcomes of the study.

Issue 2: In the supplementary text: "The Quebrada Grande complex also exposed rocks originating from a Proto-Caribbean Cretaceous to Palaeocene intra-oceanic arc (Jaramillo et al., 2017; Pardo-Trujillo et al., 2020)." However, these studies do not support this view but follows the double verging subduction zone model discussed above (as do also other more recent publications having to explain the occurrence of Paleogene boninites in this area, e.g. Correa-Restrepo et al. 2023). The Northern South America section in the supplementary file

needs to be clarified accordingly. General note for tectonic reconstruction of the W Caribbean (although probably mostly irrelevant for the main outcomes of this study on the E Caribbean): there is no evidence for a pre-upper Campanian volcanic arc between S Costa Rica and W Panama. There are some Coniacian-Santonian siliceous sediments interbedded in the oceanic plateau at the base of the arc, but these do not contain arc-derived (e.g. tuffaceous) materials; they are synchronous (interbedded) with oceanic plateau volcanism that is devoid of supra-subduction geochemical proxies. If geological constraints are used for tectonic reconstructions, these are important observations that should be taken into account when developing new regional tectonic reconstructions. It might be an issue, but I cannot see how an earlier arc could have been missed between S Costa Rica to W Panama (an arc is not a small volcanic feature) - puzzling but not to be ignored. It looks like there is scope for additional work in future studies to explain the tectonic evolution of the W Caribbean and northern S America.

In the meantime it would be fair (with respect to the extensive field work done in the area) to state somewhere in the supplementary file that there remain uncertainties in the exact timing of subduction initiation in southern Central America, but that this has no incidence on the outcomes of this study (if needed I provided some relevant references in my previous review).

Thanks for pointing that out, there is indeed a very complex geology in Central and northern South America and these precisions were needed to the text and figures. We have now edited Figure 2 and the Northern South America section of the supplementary text, and amends in the paragraph "extended methods" the fact that we mainly focused on the eastern side in this study but that there is room for future regional reconstruction of the western part of the Caribbean plate : "Uncertainties persist regarding the precise timing of subduction initiation, as well as the kinematic evolution of arc-related blocks in Central America (Buchs et al. 2010 ; Montes et al., 2012; Andjić et al. 2019). While these uncertainties do not affect the results of the present reconstruction, which focuses on the eastern Caribbean plate, they should be taken into account in future kinematic analyses of the western Caribbean region."

David Buchs, Cardiff University

Reviewer #5 (Remarks to the Author):

The manuscript "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles Arch" by Montheil and collaborators represent a significant contribution to the geology of the eastern Caribbean and its link with the origin of some elements of the biodiversity that inhabited the region. In general, the article is well written and organized, and there is a good flow between the different sections. I must disclose that my expertise and therefore, my comments, are not in the geology neither the tectonic on the region, although I found it rather fascinating. My comments focus on the biogeographical and paleontological components of this article and the implication that this work has for understanding the origin of the Caribbean biodiversity.

We thank reviewer 5 for his positive comments on our manuscript.

After carefully reading the manuscript, I am inclined to recommend it for publication because of their significant findings and biogeographical implications. I do, however, have several comments that hope will help this paper to clarify some sections and improve this important contribution.

In general, I found a little difficult to follow the use of the terms land connections, land bridge, transient land bridge, and stepping stone islands since they seem to be used as synonyms in some instances (e.g., lines 265, 288, 291). These concepts are very different to each other, and the expectations for biological dispersal capability from one to another can be very different.

We agree with this comment that those terms are different and therefore their implications for biota dispersal. To make it clearer: land connections relate to connections with the mainland, and we use this term to refer to either a land bridge or stepping-stone islands. We removed the term “land bridge” and replaced it by “transient land bridge” because such land bridge is transient as it has not been permanent through the paleogeographical evolution of the Eastern Caribbean. We now explain the transient term in more detail in the paleogeography section.

We replaced the term “interconnected stepping stone islands” with “transient land bridge or stepping-stone islands” (in reference to the work of Iturralde and McPhee and our work since we can not disclose unequivocally the nature of GAARlandia and GrANoLA as either a land bridge or stepping-stone islands).

Although the paper focuses primarily on the tectonic deformation along the Lesser Antilles arc, I encourage the authors to discuss in more detail how their new interpretation fits with the current biological and paleontological evidence available. Specifically, in the section of “Impact on the Caribbean biological evolution” in the Discussion, they only have one reference regarding dispersal of vertebrates (line 297). There is a large body of literature looking at the dispersal of plants, invertebrates, vertebrates and fossils which could be included here to make the discussion much more meaningful.

We have modified this section accordingly.

Specific comments

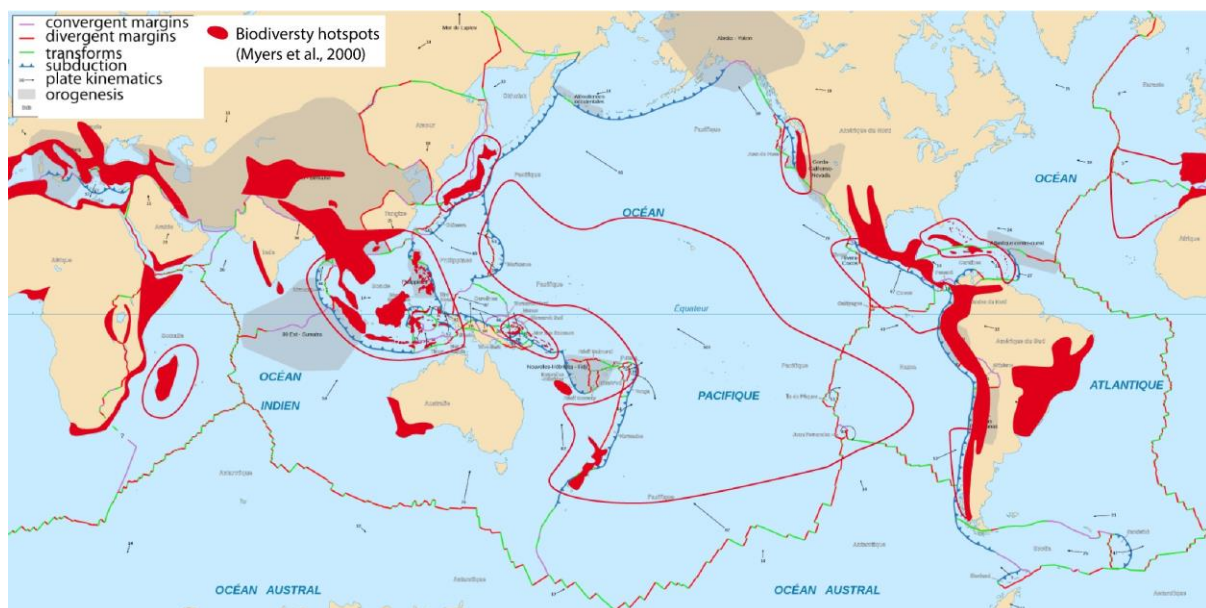
Line 1: The title of this paper is “Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles Arch”. The new paleogeographical reconstructions of the Caribbean by Montheil and collaborators, particularly in the Eocene, has major implications for the origin of the Caribbean biodiversity, but not necessary for the posterior evolution and radiation within the islands and posterior overwater dispersal events. I would suggest the authors to consider modifying their title to something like “ Origin of Caribbean biodiversity shaped....”.

We understand the point of the reviewer, but the alternative title is not accurate. An origin is not shaped. And the origin of the biodiversity is more than just a landbridge. We acknowledge in the paper that there are clearly more drivers of biological evolution and biodiversity in the Caribbean region. ‘shaped’ is sufficiently general so as not to claim that it was entirely driven by tectonic deformation.

Instead, we prefer to return to the original title “Caribbean biodiversity shaped by subduction zone processes along the Lesser Antilles arch”, because we now explain, in a new

interpretation section in response to reviewer 1, how the tectonic deformation as well as the magmatism are both the result of subduction zone processes.

Line 49: It seems that biodiversity hotspots are often found in regions associated with subduction, citing Myers et al., (2000). However, this is not a result presented neither discussed by Myers et al., (2000). I suggest finding other authors that have worked in the associated between biodiversity hotspots and geological history of these regions. Given that we are limited by the number of references we can cite, we have added one reference that modeled the relationship between plate tectonics and biodiversity hotspots of endemic species richness (Pellissier et al., 2017 doi.org/10.1093/biolinnean/blx125). As part of the novelty of our study, we present a new supplementary figure (new SI Fig. 1) comparing the locations of biodiversity hotspots as defined by Myers et al. (2000) and a map of plate boundaries. We classified each biodiversity hotspot into one of five geodynamical contexts (new SI Table 1) and estimated that 48% of the 25 hotspots are located along subduction zones.



Supplementary figure 1: Comparative map of the biodiversity hotspots by Myers et al., 2000 and plate boundaries. 48% of the 25 hotspots defined by Myers et al., 2000 are located along subduction zones (see SI Table 1).

Geodynamical context	Subduction zone	Rift	Orogen	Intra plate	Passive Margin
Tropical Andes	1				
Mesoamerica	1				
Caribbean	1				
Brazil's Atlantic forest					1
Choc/Darien/Western Ecuador	1				
Brazil's Cerrado				1	
Central Chile	1				
California Floristic Province	1				
Madagascar		1			
Eastern Arc and Coastal forest of Tanzania/Kenya		1			
Western African Forests					1
Cape Floristic Province					1
Succulent Karoo					1
Mediterranean Basin	1				
Caucasus			1		
Sundaland	1				
Wallacea	1				
Philippines	1				
Indo Burma					1
South Central China				1	
Western Ghats/Sri Lanka					1
SW Australia					1
New Caledonia	1				
New Zealand	1				
Polynesia/Micronesia				1	
Sum	12	2	1	3	7
Percentage of Biodiversity hotspots	48	8	4	12	28

SI Table 1 : 25 biodiversity hotspots defined by Myers et al (2000) and their geodynamical context.

Lines 57-58: The authors mention several groups that of South America origin which colonized the Caribbean through the Cenozoic like land mammals, amphibians, reptiles and others. However, they only provided one reference, for each groups, and some of these works only provided support or has implications for a clade. For example, Tejada et al (2024) cited for mammals, only focus on sloths; Blackburn et al., (2020) cited for amphibians, reports the oldest record of Eleutherodactylus, but there are several other groups of amphibians in the region with a South American origin. I would recommend the authors that if they will provide references for each group, it should be more comprehensive.

Since we are limited by the number of references used, we decided to include the taxon specific references at the end of the sentence instead of providing references after each taxonomic group.

Line 63: The authors mention there was a pick in colonization between 40 and 30 million years ago, citing Ali and Hedges (2021). However, Ali and Hedges (2021) said in their Conclusion statement (page 2703), first paragraph, starting in the fourth line the following: "First, although 23 of the 35 clades have colonization intervals that straddle c. 34 Ma, the ranges are typically 30–50 Myr, with some appreciably larger. Thus, it is difficult if, not impossible, to peer through the 'data fog' to visualize the actual record; any analysis based on the colonization-interval midpoint ages provides a superficial view."

Therefore, in their opinion because of the wide age ranges it is not possible to observe a clear pattern for many of the vertebrate groups, and this pick the authors mention can be a methodological artifact. I recommend the authors to reevaluate their statement about the

pick of colorizations between 30-40 million years ago in this section and throughout the rest of the manuscript (e.g., line 114, 292).

We modified the sentence to this: “Most of the colonization times, estimated from molecular clocks and the fossil record, occurred during the Cenozoic, between ~ 60 and 10 million years ago. Despite these broad colonization intervals and error margins ranging from 30 to 50 million years, colonization events appear to cluster predominantly during the Eocene–Oligocene interval (Iturralde-Vinent and MacPhee, 1999, 2023; Ali and Hedges, 2021).” Following the recommendations, we deleted the statement of a peak of colonizations between 30-40 million years ago throughout the manuscript and replaced it by Eocene to Oligocene cluster tendency.

Line 126: It sees that these new interpretations may be relevant in interpreting the evolution of the Caribbean biodiversity hotspot, but it seems actually more relevant to the origin of the biota in the Caribbean biodiversity hotspot.

The sentence has been modified accordingly and now reads as: “interpreting the origin and evolution of the Caribbean biodiversity hotspot biota”.

Line 241-245: This is a very long sentence that could likely be divided into two.

A general issue with most palaeogeographical reconstructions that suggest the existence of some sort of land connectivity, is the absence of a filter. The Caribbean biota today and in the recent fossil record (late Quaternary) is relative depauperated at high taxonomic level, lacking several groups of vertebrates that are/were in South America. A land bridge usually allows the dispersion of large components of the biota, which is not what we see in the modern and recent fossil record in the region. There are two non-mutually exclusive explanations. First, there were geographic and/or ecological filters like the presence of marine barriers which reduced the number of taxa that dispersed into the Greater Antilles. Second, it is possible that a larger number of groups arrived to the Greater Antilles but went extinct before the late Quaternary. This last alternative is discussed in a paper that was just published (Vinola-Lopes et al., 2025) and would support the claims made here. The sentence in line 241-245 seems to be the right place to mention that the disconnection between different parts of GrANoLA acted as a biodiversity filter.

We split into two sentences as suggested and included a discussion on GrANoLA acting as a biodiversity filter. It now reads:

These land masses may have formed a transient land bridge or a chain of large islands separated by narrow sea corridors. Fragments of GrANoLA may have connected and disconnected in various configurations over time, effectively allowing biological dispersal of some organisms or acting as dispersal barriers in others (Fig. 4B; Vinola-Lopez et al., 2025).

We also added a few sentences on the Discussion regarding the “Impact on Caribbean biological evolution” It now reads: While a land connection would have allowed the dispersal of South American biota, both the current Caribbean biota and its late Quaternary fossil record lack several groups of extant and extinct South American vertebrates. Moreover, dispersal into the Caribbean from continental America is significantly less than dispersal during the Great American Biotic Interchange, despite a similar time length of opportunity for colonization (Pelegrin et al., 2018). These observations suggest that either Caribbean biota was more strongly filtered by geographic or ecological filters (i.e. seaways reducing the number of taxa that dispersed into the Antilles), or that the transient and ephemeral nature of the land

connections to the Caribbean were not effective pathways for many organisms, or that a larger number of groups dispersed but went extinct before the late Quaternary (Vinola-Lopez et al., 2025).

Line 264: it seems that around 34 mya the land exposed potentially created a true land bridge, but in the paleogeographic reconstruction in Figure 2, there are large gaps of water separating them.

We think the reviewer refers to Figure 4, as Figure 2 is a geological map and not a paleogeographic reconstruction. In Figure 4 there are two colors defining emerged areas, one is yellow and represents the emerged land that we are sure of and the other is light and greyish yellow representing the zone that could potentially have been emerged but we cannot be 100% sure of due to lack of geological data. The presence of these uncertain areas explains why we use “transient” landbridge instead of landbridge in the text.

Line 265: see the comment above regarding the dispersal peak since there are no strong evidence of it based on molecular analysis (see Ali and Hedges, 2021).

We have deleted all statements that refer to a “peak” of dispersal. The sentence now reads:land bridge that may have facilitated biological dispersal.

Line 286: migration is not the correct term to use in the biogeographical context since it is more associated with the cyclical movements of animals from one region to another as part of their life history. Suggest to use dispersal.

The sentence has been rephrased: “Our reconstructed paleogeography offers a novel insight into a potential dispersal pathway of South American biota into the Caribbean realm”

Lines 285-286: This line is a little awkward. In the way that currently reads, it suggests that all Caribbean biota came from South America, which is not the case. Suggest rephrasing.

The sentence has been rephrased: “Our reconstructed paleogeography offers a novel insight into a potential dispersal pathway of South American biota into the Caribbean realm”

Lines 286-288: it seems that there is a 20 million year window of opportunity to overland dispersal via interconnected stepping stones islands. However, the authors also suggest the presence of a true land bridge around 34 mya (lines 264-265).

Furthermore, with a 20 million window for overland dispersal we should expect to see several other groups of vertebrates in the Caribbean which are not there today (see Ali and Hedges, 2021). This time is considerably shorter than GABI, but the number of species that disperses into the Caribbean is significantly smaller than those that disperse either into South America or North America. How do you justify it?

Following previous recommendations from this reviewer to improve the usage of the different land masses terms, we now define “land connection” as an island-continent land bridge **or** stepping-stone islands. Given the degree of uncertainty in the paleo reconstructions as of the exact morphology of the land masses (GAARlandia, GrAnOLA) we use the term “land connection” as an umbrella term to refer to both: a transient land bridge or a chain of islands. Sentence now reads: “We identified a roughly 20 Myr window of opportunity between 45 and 25 Ma for overland dispersal via a land connection in the Eastern Caribbean region”.

We think the reviewer made a mistake and meant to say that the GrAnOLA timeframe is considerably larger (not shorter) than GABI. We disagree with this comment since there is

fossil evidence of biotic interchange or dispersals between South and North America in birds and mammals that date back to the Oligocene-Miocene (Proto-GABI, Pelegrin et al. 2018) and therefore the time of opportunity for colonization has a similar duration in both cases . The earliest fossil evidence of mammal interchange from South to North America is that of *Panamacebus* from the lower Miocene in Panama (21 million years ago, Bloch et al. 2016). The comparison of dispersal facilitated by GrANoLA and GABI is, however, worth discussing under the section “Impact on Caribbean biological evolution”. It now reads:

“Moreover, dispersal into the Caribbean from continental America is significantly less than dispersal during the Great American Biotic Interchange, despite a similar time length of opportunity for colonization (Pelegrin et al., 2018). These observations suggest that either Caribbean biota was more strongly filtered by geographic or ecological filters (i.e. seaways reducing the number of taxa that dispersed into the Antilles), or that the transient and ephemeral nature of the land connections to the Caribbean were not effective pathways for many organisms, or that a larger number of groups dispersed but went extinct before the late Quaternary (Vinola-Lopez et al., 2025).”

Lines 297-302: The influence of the breaking up of GrANoLA in the allopatric speciation and high level of endemism in Puerto Rico and the northern Lesser Antilles seems very interesting. However, in the paleogeographical reconstruction of Cornée et al. (2021) there does not seem to be islands permanently exposed in the northern Lesser Antilles until recently. This is also reflected in Figure 4 in this paper. If the pattern of allopatric evolution described here was the case, we should expect to see that the lineages in the northern Lesser Antilles diverged early on, however, this not seems to be the case (e.g., Tucker et al., 2017).

There are Eocene to Oligocene exposed islands in the paleogeographic reconstruction of Cornée et al., 2021 and of this work. These islands were later submerged during the Miocene then emerged again more recently, to form the current remnants of GrANoLA land in the Lesser Antilles (i.e. Puerto Rico, Virgin Islands, Saint-Martin, Saint-Barthelemy and Antigua).

We disagree with the rationale of the expectation that lineages in the northern Lesser Antilles must have diverged early on or branched off first than other Caribbean species. Speciation via niche/ecological differentiation or other speciation mechanism could have occurred in the larger islands of Hispaniola and Cuba before the fragmentation of GrANoLA land which we claim created the conditions necessary for allopatric speciation. We are well aware that many lineages have early diverging branches distributed in the Greater Antilles not the Lesser Antilles, and our reflection is not meant to provide an order of speciation events in the different parts of the Caribbean.

Figures

I think the figures are in general well designed and convey a large quantity of information, but this is not reflected in their legends, which are rather short. I recommend the authors to expand in more details the legends to explain better the figures, helping the readers to get the most out of them. I also made some more specific comments below.

We expanded the legends and captions of figure 1, 3 and 4.

Figure 1: In the legend of this figure, the authors do not explain to which group the animal and plant outlines correspond. I assume that these are examples of groups with South

American origin. But, are these also examples of groups that arrived to the region in the 40-30 million years old interval? If these species correspond to groups that arrived in the 40-30 mya, there are some issues, for example, anolis lizards, arrived to the Caribbean much earlier while the only group of living rodents in the region, the hutias (Capromyinae), arrived much latter in the early Miocene (see Courcelle et al., 2019; Ali and Hedges, 2021).

Alternatively, there are fossil rodents with South American affinities in the Oligocene of Puerto Rico (~29 mya), but there are no living relatives of them in the Caribbean today (see Marivaux et al., 2021), and the sharing of the shape should be different.

Furthermore, in Figure 1 there is shape of a fish which is present in the Caribbean and not in South America, what does it means?

A final observation about Figure 1 that is mostly stylistically but still important for accuracy. The sloth represented in the shade is a giant sloth, my guess a megalonychid considering the knuckle walking. Caribbean sloth seems to have been rather plantigrades, and they are now in their own family, Megalocnidae. Please, consider updating the silhouette of the sloth for a more accurate one.

We appreciate the comment of making Figure 1 more accurate, going beyond aesthetics. We have now decided to represent lineages of plants and animals that could have used GrANoLA land as a dispersal pathway to arrive in the Caribbean from South America during the period of exposure revealed here: 45 to 25 million years ago (the 20 million-year window of dispersal opportunity) and detailed them in the caption of the Figure 1. We also represented the rodent shape in lighter colors to indicate that it is a fossil specie and not the living relatives.

According to the reviewer comment, the megalonychid has been replaced by Megalocnidae and the fish and arachnid shape has been added to the South American biota (it was a mistake)

Figure 4. Generally the same issues of Figure 1 regarding the animal and plant shapes (e.g., anolis lizards already dispersed in the Caribbean, hutias dispersed much latter).

Furthermore, the authors indicated that they followed Iturralde-Vinent and MacPhee (2023) and Ali and Hedges (2023) for the reconstruction of the Greater Antilles. However, in the ~35 Ma map, Puerto Rico, Hispaniola, and eastern Cuba are disconnected by marine barriers, while in the maps from the authors they cite these regions are reconstructed as connected. This is important for understanding biotic dispersal between islands in the Greater Antilles. As their reconstruction stand, it indicates overwater dispersal from Puerto Rico to Hispaniola and Cuba contrary to vicariance as result of the subsequent separation of the island (see Iturralde and MacPhee, 1999; MacPhee et al., 2003).

It does not, the greyish/yellowish region are defined as either emerged or shallow water. We decided to change these colors to green as there was repeated confusion on that light yellow colors and meaning from different reviewer indicating that this was not visually clear.

Line 281-282: Ali and Hedges (2023) should be Ali and Hedges (2024).

Corrected.

Lastly, one of the contribution of this paper is the emergence of GrANoLA during the middle Eocene (~45 mya) and the formation of a transit land bridge at this point. I strongly suggest

the authors to then also include a paleogeographic reconstruction of the Caribbean region at 45 mya.

The landbridge we inferred around 35 Ma, on par with the biological literature. The onset of the tectonic uplift and the first emergence of the forearc was around 45 Ma. Nonetheless, the patterns are not dissimilar from the 35 Ma, even though the position was slightly westwards, as can be seen in the tectonic reconstructions. Adding a 45 Ma slice does not add fundamental insight, so we prefer to leave the two reconstructions that represent 'landbridge' and 'destroyed connection'.

Small comments :

Line 63: Cenozoïc should be changed to Cenozoic.

Corrected.

Line 83-84: instead of saying: "Cenozoic overwater dispersal via flying, swimming, or floatsam" I suggest the authors to use "overwater passive and active dispersal through the Cenozoic" comprising all possible overwater mentioned by the authors and others.

Corrected according to the reviewer suggestions.

Line 242: instead of a : , consider using a coma or semicolon.

Corrected.

Line 302: Scheider et al., 2021 is not in the references.

The appropriate reference has been added in the text and reference list.

Line 309: should be pathways, in plural, because there are multiple different pathways mentioned thought time in the paper.

Corrected

References:

Courcelle, M., Tilak, M. K., Leite, Y. L., Douzery, E. J., & Fabre, P. H. (2019). Digging for the spiny rat and hutia phylogeny using a gene capture approach, with the description of a new mammal subfamily. *Molecular Phylogenetics and Evolution*, 136, 241-253.

Marivaux, L., Vélez-Juarbe, J., Merzeraud, G., Pujos, F., Viñola López, L. W., Boivin, M., ... & Antoine, P. O. (2020). Early Oligocene chinchilloid caviomorphs from Puerto Rico and the initial rodent colonization of the West Indies. *Proceedings of the Royal Society B*, 287(1920), 20192806.

Viñola López, L. W., Velez-Juarbe, J., Münch, P., Almonte Milan, J. N., Antoine, P. O., Marivaux, L., ... & Bloch, J. (2025). A South American sebecid from the Miocene of Hispaniola documents the presence of apex predators in early West Indies ecosystems. *Proceedings B*, 292(2045), 20242891.

Iturralde-Vinent, M., & MacPhee, R. D. (1999). Paleogeography of the Caribbean region: implications for Cenozoic biogeography. *Bulletin of the AMNH*: no. 238.

MacPhee, R. D., Iturralde-Vinent, M. A., & Gaffney, E. S. (2003). Domo de Zaza, an Early Miocene Vertebrate Locality in South-Central Cuba, with Notes on the Tectonic Evolution of

Puerto Rico and the Mona Passage¹. American Museum Novitates, 2003(3394), 1-42.

Best regards,

Lazaro W. Vinola-Lopez

Point by point response

Reviewer #1 (Remarks to the Author):

Recommendation (Required): I recommend publication upon addressing minor comments

Significant: Yes, the paper is a significant advance or contribution.

Supported: Yes Referencing:

Yes Quality: The organization of the manuscript and presentation of the data is appropriate

Data: Yes

The authors provided an updated version of the manuscript "Caribbean biodiversity shaped by tectonic deformation along the Lesser Antilles arc", now titled "Caribbean biodiversity shaped by subduction zone processes along the Lesser Antilles arch". At this stage, I believe the manuscript is ready for publication, and I have only a few minor comments. I think the new discussion about the geodynamics opens the path for future investigations based on your updated model of the Caribbean.

Furthermore, I agree with you in using the "rigid block approach" for reconstructing shortening and stretching. However, the use of closed topological boundaries is appropriate for creating plate boundaries, such as trenches, mid-ocean ridges, and continental-scale strike-slip fault systems (see <https://github.com/GPlates/gplately>). I agree with you that creating these plate boundaries is outside the scope of this work, but doing this will enable the integration of your model with other regional and global tectonic reconstructions. As the other reviewers mentioned, your model shows many complexities in the eastern Caribbean that are not observed in other reconstructions of the region. Beyond its biogeographic relevance, the reconstruction has important geodynamic implications, some discussed by you. In the future, it will be very interesting to test the impact of these differences in the mantle flow and the mantle structure of the Caribbean.

Ln 275: The upper mantle extends quite a bit in depth (0-660 km), do you mean the full depth (660 km)?

Thanks for pointing that out, we have clarified that the slab was still '*within*' the upper mantle". The point we are making with that is now further clarified in the next sentence, "*...as long as both poloidal upper mantle flow (below the slab, so before the slab reached the base of the upper mantle) ... was possible.*"

Ln 279: Do you mean the 660 km mantle transition zone? We mean the '*base of the upper mantle, around 660 km*', as we now clarified. The mantle transition zone is the interval between 410 and 660 km, in which there is still poloidal flow possible.

Ln 287: It's great that you included a comparison with a similar tectonic setting. Thanks.

Reviewer #3 (Remarks to the Author):

The authors have fully satisfied the comments outlined in my first two reviews. I believe that the manuscript is now ready for publication.

All the best,
Ethan Conrad

Reviewer #4 (Remarks to the Author):

It is my third review of this manuscript, and I can attest that the manuscript was improved in terms of explaining processes underpinning the tectonic evolution of the E Caribbean as well as clarifying some regional geological aspects. Improvements were notably made regarding the explanation of Colombian geology and how this relates (or does not relate) to the new tectonic model. It is clear from the supplementary section on Colombian geology that the authors are not specialists of this area, but the quality is now good enough to address the needs of this study on the tectonic and paleogeographic evolution of the E Caribbean. The timing of subduction initiation in southern Central America is now left uncertain, which is fairer than what was previously mentioned given existing geological constraints; it is also clear this has no important incidence on the main conclusions of this study.

My last remaining criticism is on the geological origin of the Aves Ridge and its potential implications regarding the paleogeographic evolution of the E Caribbean. The authors are strongly questioning the volcanic arc origin of this major bathymetric feature in the E Caribbean, although it is generally considered to be a volcanic arc in most of the existing geological literature.

In their rebuttal the authors mentioned “That the Aves Ridge would be arc is a speculation, there is not actual rock evidence for this. The only rocks that were studied are the ones dredged from the southernmost part of the Ridge, and they give mostly CLIP signatures - and perhaps some arc influence, which at this southern margin is to be expected.”

The revised text of the manuscript includes in lines 304-314: “In the reconstructed configuration prior to 50 Ma, the Aves Ridge was behind, and at a high-angle to the Greater Antilles arc (Fig. 3D). Moreover, it was situated approximately 500 km west of the eastern Caribbean plate boundary. Even though this boundary was likely a subduction zone in the late Cretaceous (Pindell and Kennan, 2009; Boschman et al., 2014), this distance is much larger than for typical arcs, and may challenge the interpretation of the Aves Ridge as a Cretaceous volcanic arc, as long thought

(Burke, 1988; Neill et al., 2011). Perhaps more likely, the thick crust of the Aves Ridge, from which direct observations only come from the southernmost and northernmost parts may instead consist of basaltic rocks related to the Caribbean Large Igneous Province (CLIP) (Neill et al., 2011), similar to the ~20 km thick Beata Ridge (Dürkefälden et al., 2019)."

Although I agree with the authors that there is very limited evidence for the existence of a drow volcanic arc along the -whole- of the Aves Ridge, it is incorrect to suggest that there is no, or too little, evidence for volcanic arc rocks. The study of Neill et al. 2011 provides strong (unambiguous) geochronological and geochemical evidence from igneous rocks dredged in the southern part of the Aves Ridge. Their data is of excellent quality and clearly document the occurrence of (undated) calc-alkaline supra-subduction mafic igneous rocks, compositionally similar in terms of immobile trace elements to (dated) Upper Cretaceous felsic igneous rocks with arc signatures, which are clearly not CLIP signatures as indicated in the rebuttal. Given these geochemical constraints that also include 2 Nd-Hf isotope analyses, these rocks almost certainly document a volcanic arc, with a Late Cretaceous age for at least some of its sequences. These rocks certainly do not correspond to oceanic plateau basaltic rocks like those reported by Dürkefälden et al. 2019. In fact, most of the studies on oceanic plateau basalts in the Caribbean do not report volcanic arc-like rocks such as those analysed by Neill et al. 2011, and if they do it is generally from sequences crosscut or overlain by such rocks (e.g., in Panama and Colombia). The authors must acknowledge the occurrence of volcanic arc rocks in at least the southern part of the Aves Ridge. It is the simplest and most logical explanation of the data in Neill et al. 2011.

Considering the very limited number of dredged and analysed samples from the rest of the Aves Ridge, the authors could decide to tune down the generally accepted interpretation of the whole of the Aves Ridge as a volcanic arc. However, considering this ridge has been interpreted for long as a volcanic arc based on geophysical constraints (e.g., seismic crustal velocities) and, more recently, the data by Neill et al. 2011, any attempt to tune down the volcanic arc hypothesis will probably be seen by the Caribbean community as a tentative to tune down the significance of the GAARlandia hypothesis to promote the new proposed model. As detailed in my last review, there are some important paleogeographic implications of having a volcanic arc in at least some parts of the Aves Ridge (a valid interpretation using Neill et al. 2011 data). It would in my view be more constructive and beneficial to this study to not use speculative regional arguments, like the possible lack of a volcanic arc in large parts of the Aves Ridge, to rule out the GAARlandia hypothesis- it reads like that now, even if not intentional. The GAARlandia hypothesis is not mutually exclusive with the new paleogeographic interpretation and is not hampering the value of this study. Yet it can provide a fairer account of the data of Neill et al. 2011, and other regional observations on the nature of the Caribbean oceanic plateau and volcanic arcs. As I have been suggesting for >15 years, Caribbean researchers

would benefit much from new dredging of the Aves Ridge. Until then, the only direct insight into its origin comes from the arc rocks documented in Neill et al. 2011, and this needs to be acknowledged, not questioned, in this study.

David Buchs, Cardiff University

The authors would like to thank again David Buchs for this very constructive and detailed comments. We agree that Aves ridge volcanic arc rocks occurrence was not sufficiently explained in the former manuscript. We have now corrected this adding a sentence to the text : *“unambiguous record of Cretaceous volcanic arc rocks in the Aves ridge is restricted to its southernmost (i.e. Los Hermanos spur) and northernmost (i.e. Saba bank) parts (Fox et al., 1971; Bouysse et al., 1985; Neill et al., 2011)”*. In this version we have clarified why we considered that a Cretaceous volcanic arc on the Aves ridge is unlikely : *“The 50 Ma back-arc position of the Aves Ridge, combined with the limited and spatially restricted Cretaceous arc samples, may challenge the long-standing interpretation of the entire Aves Ridge as a Cretaceous volcanic arc (Burke, 1988; Neill et al., 2011). Perhaps more likely, the thick crust of the central Aves Ridge, may instead consist of Caribbean Large Igneous Province (CLIP) related rocks (Neill et al., 2011) similar to the those found on the ~20 km thick Beata Ridge (Dürkefälden et al., 2019) on the central Caribbean Plate (Fig. 1). The Cretaceous arc volcanic rocks found on the northern and southern extremities of the ridge in the Los Hermanos and Saba Bank regions may have may instead be part of the ~E-W trending arcs of the Northern and Southern Caribbean plate, and may even have been tectonically juxtaposed against the central Aves ridge.”*

Finally, we do not agree with reviewer 4 stating that the absence of a Cretaceous volcanic arc on the Aves ridge is tuning down the significance of the GAARlandia hypothesis. During the Cenozoic, the Aves ridge unarguably consist of thick crust whatever its rock lithology. Consequent to this crust thickness the Aves ridge is a close-to-sea surface topographic high that can be emerged and drowned more easily than the surrounding oceanic crust by Cenozoic vertical motions. Whether this topographic high consists of Cretaceous arc material or of Caribbean Large Igneous Province has thus no impact on the Eocene paleogeographic history and does not preclude the existence of the GAARlandia land. Nevertheless it is of primary importance for the geodynamic evolution of the region, which is why we dedicated it a section according reviewer's suggestions during the previous reviews.

2nd review of: Caribbean biodiversity shaped by Paleogene subduction zone processes

The revised manuscript and rebuttal submitted by Montheil et al., fully satisfies the comments/criticisms raised in my first review. The authors have been more explicit with their uncertainties and the improved feasibility of their model over existing reconstructions. One important new clarification is that they are not implying that full emergence is necessary or that there are necessarily issues with current models, but that the islands/rafting hypothesis is probably more likely. Also, the omission of the comment on geodynamics and addition of citations to the geodynamics articles that describe potential geodynamic mechanisms is important and appreciated.

Overall, in this new version, I think that the aim of the paper is outlined much more effectively, focusing on the tectonic synthesis, Gplates implementation of paleomagnetic rotation and other recent geological data, and the extensive discussion of the links between the new reconstruction, tectonic implications and potential faunal dispersal pathways. In my original review what was meant by “general lack of data”, is that this paper does not really add any new data to constrain the reconstruction or strengthen the argument that there was a full or partially emerged landbridge. The important contribution is really the new reconstruction, and I think now the manuscript clearly explains that the paper is a synthesis of an ongoing debate supported by that new regional reconstruction. The added emphasis on the improved timing and new segmented and highly mobile forearc model for the eastern Caribbean explains this well.

However, while the supplemental does broach this subject a bit, I do think that the authors could include additional explicit explanation in the main text of how their reconstruction stands apart from, and is an improvement on, previous models (e.g., Boschman). For example, the authors redrew and subdivided blocks (e.g., split Caribbean plate interior into basins), completely redid the Puerto Rico rotations, corrected some plate boundaries, etc. These are very valuable improvements, and I am eager to use it as it helps explain new thermochronological data in Hispaniola I have in a publication currently under review. I offer some additional suggestions below.

Just a note of GPlates deforming meshes: In my first review I mentioned the potential to use a deforming mesh. The authors pushed back, and I agree that the rigid block approach is fine for the purposes of the article, that was mainly just something pointed out for additional consideration. However, for the sake of the GPlates developers, it should be noted though that the deforming mesh is not just “cosmetic”, but uses deforming boundaries and interior points to calculate strain quantities which can be analyzed and linked to geological or geophysical data, e.g., gravity, seismic stratigraphy, and tomography (see Gurnis et al., 2018). For example, one neat application that I have recently seen is using the deforming mesh framework to invert present-day crustal thicknesses in a volume restoration sense to evaluate plate models, which is far beyond “cosmetic”. That said, with the authors’ modifications I do think the rigid block approach is fine., although I think there are necessary changes to the text concerning the use of the term rigid for the eastern Caribbean (see below).

At this point, I think that the manuscript is ready for publication after the consideration of a few more (minor) points:

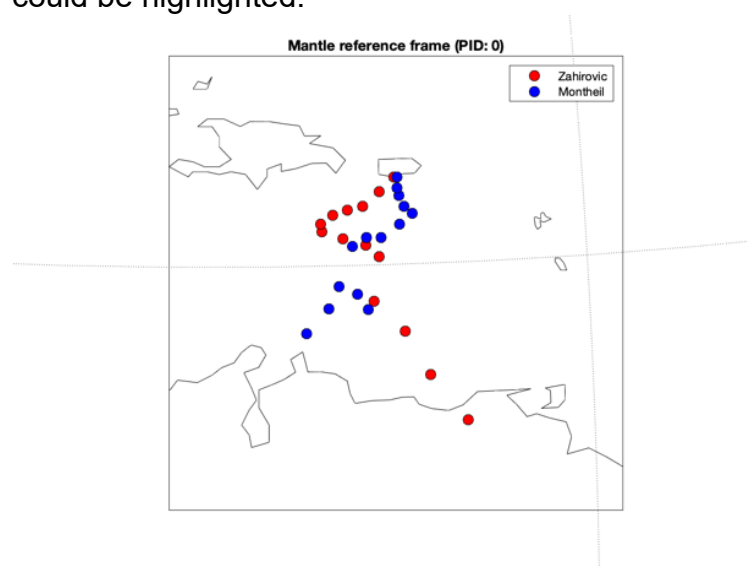
Use of “rigid” in abstract and throughout text

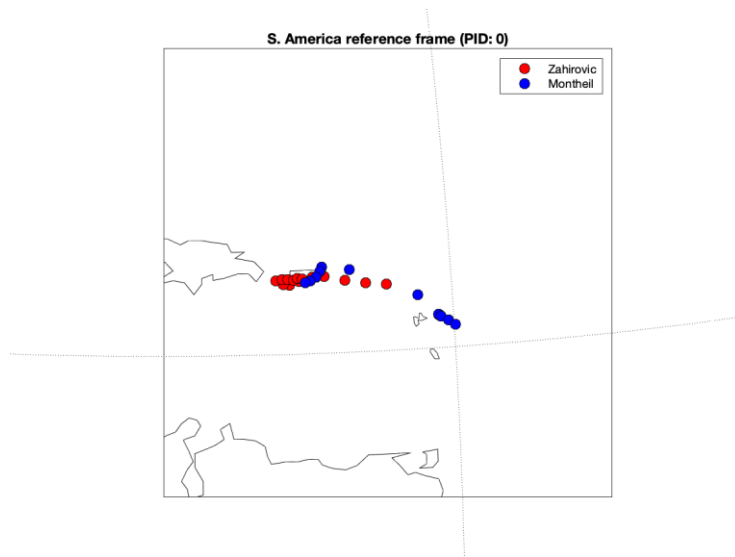
In the abstract on line 25 it is stated: “However, tectonic reconstructions have always assumed a mostly rigid eastern Caribbean plate, such that migration routes must either have been overwater, or overland through vertical motions emerging and submerging oceanic crust.”

I understand what you’re saying in some sense, but I think it is necessary to add something like coherent or unbroken and mobile (as stated already on line 230, e.g.), since rigid alone does not necessarily mean that the plate is unbroken. I think this is especially necessary since you mention rigid blocks in reference to the GPlates model and without a deforming mesh, you are still modeling the eastern Caribbean as rigid, just broken into more blocks.

Suggestions: Emphasis on differences between existing and new Gplates models

Looking at the Zahirovic/Müller or Boschman reconstruction in detail, it is striking how different the rotations and block definitions are. I think this new model is indeed a significant improvement to current GPlates implementations in the eastern Caribbean and this could be emphasized. In particular, PRVI motion is totally redone, e.g., in the other models it is weirdly tacked on to southeastern Hispaniola then rotates clockwise away, while the PRVI block in this model is in the forearc to the east and rotates to its present position in what seems to me a more sensible way given cited paleomag and geological data. As an idea, one simple way to show this, that could go in the supplemental, is to use the kinematics tool in the GPlates GUI to track that rotation or changing lat lon position for each model through time, then in map view and/or time vs. rotation space plot how the models are different (in mantle or NA reference frame, for example). This will show how significant this change is, as before there is no way to get the PRVI block in the correct position for a landbridge. For example, I quickly made some map view examples showing the comparison from 66 Ma to present with different reference frames (below). The new reconstruction also does a MUCH better job of classifying and subdividing the blocks compared to Zahirovic/Müller, which could be highlighted.





Line by line comments and figure comments:

Body

Line 46: basin → basins

Line 63: remove umlaut over i in Cenozoic

Figure 1: Is there any meaning to the color of the blocks? Perhaps clarify in the legend or caption.

Line 141: “Approach” seems to be out of place

Line 158: indidcators → indicators

Line 167: Subduction indicators consistency → consistency of subduction indicators

Line 174: Builded → built

Line 181: maybe add the word “implemented”

Line 207: add “The” before Venezuelan

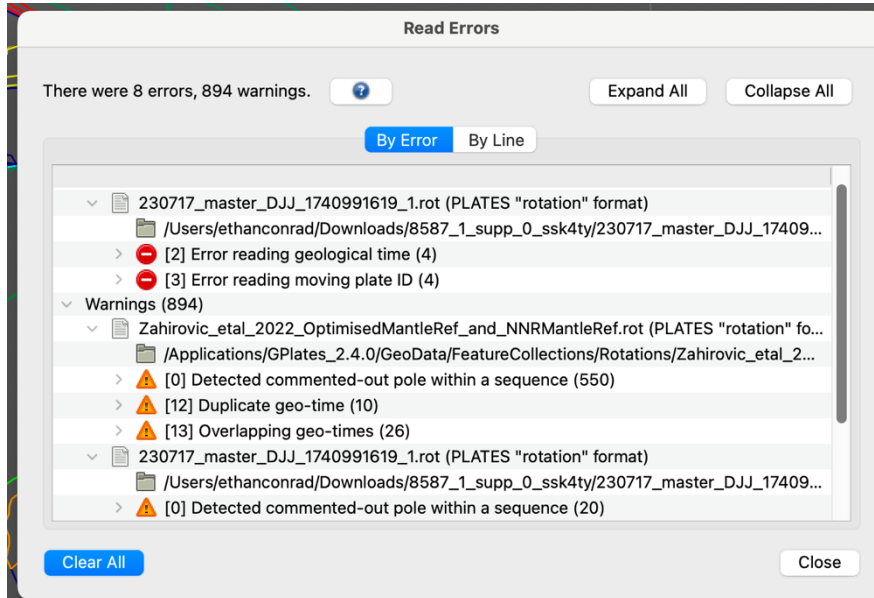
Line 221 – 223: Yes, but this was not the only cause of shortening and uplift at the time and shortening and uplift also occurred in Hispaniola earlier in the late Eocene – Oligocene. This comment may read a bit nitpicky because the statement is not false, but the authors may want to reword to clarify that this is just one event among others causing deformation in Hispaniola. Also, the reference to Wessels is probably not the best to use since that is only a review. I think Dolan et al. (1998) is better since the interactions between the Bahamas Bank and northern Hispaniola are discussed at more length.

Line 231: I suggest rephrasing “invites...” It is a bit awkward phrasing.

Line 309: consider changing to “provide a synthesis of independent evidence”

Supplemental:

Gplates files: There are a few errors loading the file. Doesn't affect function, but would be good to clear if possible



Supplemental movie: This is fine, but, I want to say again, that it would look much sharper, more professional, etc., if the movie is made by exporting individual frames and combining using ffmpeg, an online converter, Matlab, Python, etc. Here is a script I have written for other purposes that should work if you would like to use it:

```
% clear; close all; clc
%% Movie making script from images
% Written by Ethan Conrad
% Desc: create a movie from frames. Export frames in GPlates using export tool at
% whatever Myr increments you want. Make sure unique beginning of file
% name. That needs to be input in the command line to pick the images.
```

```
% might be issue with path, just make sure to add everything and should
% work fine
```

```
% Change these things:
videoquality = 75; % this will make your file bigger or smaller, balance with quality
fps = 1; % frames/sec - set this for movie speed
fileout = 'Recon'; % name output mp4
imagetype = '.jpg'; %input image type
```

```
% change the first two entries to move the XX Ma annotation in the movie
annotposition = [0.0813875968992247 0.15 0.11915503875969
0.0574625828554855];
```

```
% Set up
selpath = uigetdir(path); % select path in gui
```

```

cd(selpath) % switch to that directory
prompt = {'First few letters of file name?'}; % enter in command line unique name

% dialog box settings
dlgtitle = 'Input';
dims = [1 35];
definput = {'Pic'}; % default
Fname = inputdlg(prompt,dlgtitle,dims,definput);
Fname = Fname{1};
str = sprintf('%s*%s', Fname, imagetype);
beginningnum = 0;
startnum = 1;
start = startnum - beginningnum;
files = dir(str); % select files
nImages = length(files);

%% set up video writer
writerObj = VideoWriter(fileout,'MPEG-4');
writerObj.Quality = videoquality;
writerObj.FrameRate = fps;
open(writerObj);
h = figure('units','normalized','outerposition',[0 0 1 1]);

% Create movie
for idx = start:nImages-1
    %%
    image = imread(files(idx).name);
    im = imshow(image); % show image
    str = sprintf('frame#: %d', nImages-1);
    fig = gcf;
    % create Ma annotation
    annotation(fig,'textbox',...
        annotposition,...
        'String',{sprintf('%s Ma', files(idx).name(7:10))},...
        'FontSize',24,...
        'FontName','Helvetica',...
        'FitBoxToText','on',...
        'BackgroundColor',[1 1 1], "FaceAlpha", 0.5);

    %%
    set(fig, "Color", 'w')
    f=getframe(h);
    writeVideo(writerObj,f); % write statement
    cnt = cnt +1;
end
% close everything
close(writerObj)
delete(writerObj)
close all

```

Ethan Conrad