Review Report

Eagles - Overture for the Mandara and Vasuki Plates, TEKTONIKA, 2024.

Table of Contents

1st Round of Revisions

Decision Letter

Dear Dr. Eagles,

We have now received comments on your submission to TEKTONIKA (see attached) from two reviewers who provided constructive and (in one case) detailed comments on the text. The views of the suggested level of revisions to the text differed between the reviewers, but the points raised should be addressed prior to acceptance. These points largely relate to clarifying some of the limitations of the data used in the new reconstruction and some of the interpretations drawn from that data. For example, the basis for some of the ages used in the reconstruction or the kinematic interpretations used to displace some plates are unclear, which undermines the support for the new model. In addition, while the author proposes a new reconstruction, one of the reviewers notes a lack of hard evidence refuting some earlier reconstructions that were used as motivation for this work. Beyond this, both reviewers suggest minor editing of some sections of the text to focus the story.

After reading both the text and reviews, we agree with the suggestions from the reviewers that major to moderate revision of the text will be necessary prior to accepting the article for publication. Please take into account all suggestions by the reviewers, either by implementing them or explaining why you do not in cases where you disagree with their comments.

As editors, we are pleased to see that you have clearly identified sources of the data used in the text, as well as archived the new reconstruction GPlates project in Zenodo. If there are any changes to the project that occur during revision, please include the updated link in the text.

Submit your revised manuscript via the TEKTONIKA web site: under your manuscript's record you'll find a box named "revisions" with a way to upload your new files. Please also submit a detailed rebuttal letter explaining how you took into account reviewer's and editorial recommendations, and an additional manuscript version with the changes outlined.

Based on the scope of work suggested by the reviewers, we would hope that the revisions could be completed in approximately one month. If you have any concerns about this proposed timeline, or any other questions about how to proceed, you are most welcome to contact Executive Editor Craig Magee and/or Associate Editor David Whipp.

We will be anticipating the revised version of your paper and thank you for submitting to TEKTONIKA.

David Whipp, Associate Editor

Craig Magee, Executive Editor

Comments by Reviewer A (Valentina Espinoza) and Author response

All my comment are in attached peer-review form below.

Section A: Overview of manuscript

A1) Overall evaluation, general comments & summary

A1.1) Reviewer's comments

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

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

In this manuscript, the author introduces Vansuki and Mandara, two small tectonic plates believed to be situated in the Indian Ocean. The author offers a thorough explanation of the available reconstruction methods, highlighting that most of them, when considered individually, prove inadequate for the region and time at hand. Eagles employs a combination of seafloor observations to determine the past motion of the rigid segments within the basin, more accurately than ever before. These findings carry significant geological implications, such as shedding light on the former position of Sri Lanka prior to the Gondwana breakup, which further enhances the merit of this research for publication. The manuscript is well written, and though extensive, effectively persuades the reader of the existence of these newly identified plates. The supplementary material (GPlates files) was incredibly valuable for visualizing the proposed kinematic history of the basin.

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

- ☒ No changes required
- ☐ Rewriting
- ☐ Reorganising
- ☐ More data/figures
- ☐ Condensing
- ☐ Reinterpretation
- ☐ Other

Comments:

The submitted manuscript is easy to follow and could only benefit from some minor clarifications for readers who are not familiar with the methods.

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

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

Comments:

The text felt extensive at times, but it is thorough, so I would not suggest taking any text out. Figures are all appropriate, as well as the Supplementary material. A table with the outcome Euler-vectors of plate motion could be useful, but only if one could estimate uncertainties from the method.

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

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

Comments:

A1.2) Author(s) Responses:

Unfortunately, the visual-fitting technique used does not allow a straightforward path to calculating numerical uncertainty estimates. To explore permissible errors and get a feel for uncertainties, I would suggest that readers try manually changing the Euler parameters in the GPlates rotation file, or alternatively using its visual reconstruction tool, which enables users to click and drag reconstructed features and read off the corresponding rotation parameters.

A2) Summary of main merits and main points of improvement

A2.1) Reviewer's comments

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

The main merits I have found are...

The argument is compelling. The author resorts to a range of observations (seismic, magnetic and gravity records) with careful consideration of the options available, in order to produce the best possible reconstruction for a challenging time period, such as the Cretaceous. The manuscript is complemented by outstanding supplementary materials, enabling both experienced researchers and general readers to interactively explore the results.

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

Some minor clarifications that are addressed below, but are mainly posed for personal curiosity. The Figures are well-crafted and thoughtful, but are quite many and at times referenced at an odd pace. I recommend relocating some figures (even as supplementary material) for a more streamlined presentation.

A2.2) Author's responses:

See responses to specific comments, below.

Section B: Detailed evaluation of manuscript

B1) Title and abstract

B1.1) Reviewer's comments

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

The *Title* describes the main topic of the manuscript **accurately** — [YES] / [NO]

The *Title* describes the main topic of the manuscript **succinctly** — [YES] / [NO]

The *Title* includes **appropriate key terms** — [YES] / [NO]

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

The *Abstract* supports the rationale with **sufficient background information** — [YES] / [NO]

The *Abstract* includes a **well-balanced description of the methods** — [YES] / [NO]

The *Abstract* describes the **main results sufficiently and adequately** — [YES] / [NO]

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

The *Abstract* clearly states the **conclusions of the study** — [YES] / [NO]

The *Abstract* is **clear** and **well structured** — [YES] / [NO]

Comments:

Both title and abstract are concise and clear. I have no improvement to suggest.

B1.2) Author's responses

No action necessary.

B2) Introduction

B2.1) Reviewer's comments

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

The *Introduction* provides **sufficient background and context** for the study — [YES] / [NO]

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

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

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

The *Introduction* contains **relevant, suitable citations** — [YES] / [NO]

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

Comments:

I will include in this section my comments for the manuscript section "2. Rationale", as I interpreted it as more detailed introduction form. In general, the problem, its source and implications are laid out very clearly for the reader in this section.

Line 48 – The authors makes reference to "markers at the floor", that lead to recognize the two chains of plate motion, and that ultimately connect India to Antarctica. Though the two chains are clearly illustrated in Figure 1, it is not clear which "markers" entitle this separation of chains. I acknowledge they are listed in chapter 3, but could perhaps be mentioned early on here. Since the acknowledgement of these two distinct chains sets the structure for the rest of the text, I would suggest a little more dwelling on how these two are exactly defined.

Line 63 – The Kerguelen Plateau does not seem to be shown by a dashed outline, but rather a thin black continuous line.

B2.2) Author’s responses

Line 48: I have described the eastern and western chains in the text in such a way that the description more faithfully accompanies figure 1. The new description required some additions to the figure, which I have consequently had to enlarge to retain legibility.

Line 63: I altered the caption to figure 1 so it does not refer to a dashed outline.

B3) Data and methods

B3.1) Reviewer's comments

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

The *Methods* are described **concisely and with enough detail** for reproducibility — [YES] / [NO]

Necessary information about **data sources/acquisition/processing** is included — [YES] / [NO]

Data used are accessible via either supplementary files or links in the data availability statement — [YES] / [NO]

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

Comments:

Line 127 – The flow of the paper is hindered by the fact that no mention to Figure 2b and 2c are done prior to the first reference to Figure 3 (line 146). Figure 2c is referenced only in line 711, far after its appearance. I suggest some sort of rearranging to avoid coming back many pages (in the reading) for a referenced figure. I find the connection between the three panels in Figure 2 to be somewhat unclear.

Line 157 – The "observed fracture zone orientations at the blue disk" belong to the offshore Antarctic plate? If so, they can only be seen in Figure 1, correct? Do they have the same orientation as rotated offshore Indian plate fracture zone orientations (Figure 7)? I suggest a more explicit reference as to what is in the blue disk area in present day.

Line 269 - Does this mean using the same pole assigned to 34y, or does it also consider the previous known pole (33o in Eagles & Hoang, 2013). If the later, do the results and interpretations hold using a more recent geomagnetic timescale (e.g. GTS20 from Ogg2020)?

Line 315 - Does this process return a single best fitting pole location or an ensemble of statistically meaningful poles. Since it is done by visual inspection, I presume it is the former. If so, it would be interesting to develop a way to produce an ensemble of Euler poles that satisfies the error bounds, and hence provide uncertainties to the suggested motion of Vansuki. Though this is arguably well beyond the scope of this paper and not something I suggest the author to do, I wonder if it could be theoretically done with GPlates.

Line 330 – Replace “blue dashed lines” with “blue dashed arrows”.

Line 344 – Is it correct to say that the motion between India and Antarctica between ~133-126 to 95 remained steady (around an unchanged Euler pole)? For the benefit of the reader, I would suggest a more comprehensive explanation on the implications and underlying assumptions of the fit observed in the arrows of figures 3 and 4.

Line 382 – Replace “black lines” with “black dashed lines”.

B3.2) Author’s responses

The text I found at the line numbers given for these comments seems not to correspond with the contents of the comments. For the most part, the comments are clear enough for me to be able to respond anyway.

Line 127: I have split figure 2 into two separate figures, one that accompanies section 3.1 and the other to accompany section 3.2.

Line 157: I have added the blue disk to Figure 1, enabling readers to see the source of the fracture zone interpretations. As the *GPlates* project shows, the orientations of rotated Indian plate fracture zone are substantially the same, although it should be remembered that fracture zone pairs are pairs of one-sided products of plate divergence – not conjugate pairs – and should not be expected to align perfectly because of the presence of intervening transform faults.

Line 269: The updated manuscript and model have been adjusted to all use the GTS20 reversal timescale. This has caused some moderate changes in interpolated dates through the superchron.

Line 315: Yes, it is visual fitting for a single preferred rotation. It might be possible to generate formal error bounds on the rotations, but I agree that this could be a task for another day. For the meantime, as suggested in a previous response, *GPlates* might offer a context to attempt a similar task in a qualitative way.

Line 344: India/Antarctica motion in the 133-95 Ma period is entirely defined by the Tuck-Martin et al rotation parameters. These change only modestly, implying slow migration of the instantaneous Euler pole, and only small differences between the 133-126 Ma vectors and 126-95 Ma ones. I have added lightly to the description of the vectors in the text to more clearly portray my interpretation of the significance of their repeatability for readers' understanding of the relative accuracy of seafloor spreading-derived reconstructions and those derived from other data types.

Line 382: done.

B4) Results

B4.1) Reviewer's comments

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

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

The *Results* findings are presented **clearly and succinctly** — [YES] / [NO]

The text in the *Result* section **cites tables and figures appropriately** — [YES] / [NO]

The *Results* directly **relate to the study objectives** — [YES] / [NO]

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

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion.* — [YES] / [NO]

The *Results* section is **organised effectively** — [YES] / [NO]

Comments:

Line 546 – I would have liked to see the plate boundaries (pink lines) included in the supplementary GPlates material.

Line 604 – It is a bit of a shame that the kinematic history of the proposed Mandara plate could not be better constrained. In the supplementary movie it simply pops into existence without any modelled motion. Still, I understand the author's choice not to impose an interpretation, given the uncertainties detailed in section 5.2.

Line 648 – Is the onset and end of Vansuki's motion related to the activity and life of the Crozet plume? The location of this plume (ignoring advection) coincides with the spreading boundary of the newly born Vansuki at ~93 Ma. In the GPlates model it even appears only a few million years prior (~100 Ma). As a reader, I am left to wonder on the relationship of the two features.

B4.2) Author's responses

Line 546: I have added an illustrative set of plate boundaries, drawn at 2 Myr intervals after 130 Ma and at 5 Myr intervals beforehand, in a new *GPlates* feature collection file.

Line 604: Including plate outlines in the *GPlates* project allowed me to calculate rotations for the interpreted Mandara plate with respect to its neighbours, and include them in the updated rotations file. I feel that, despite the large uncertainties involved, this is preferable to having the Mandara plate “pop into existence without any modelled motion”.

Line 648: I appreciate the comment, but understand that too little is known about the age of the Crozet plume to be able to answer this question concisely in the current manuscript.

Comments by Reviewer B (Douwe van Hinsbergen) and Author response

Dear editor,

I have spent a lot of time on reviewing this paper, but in the end I find it not very convincing to be honest. The author basically shows that an assumption of India not moving to Madagascar could be reconciled if one speculates about other possibilities in the east Indian ocean, and then uses that model to exclude a reconstruction whose underpinning data are not mentioned. I find the conclusions overstated and unsupported by the evidence. I have tried to write a constructive and detailed review, and I hope the author will use it to upgrade the manuscript. However, I can't help thinking that the main conclusion of this manuscript on plume-plate interactions was already reached before the analysis described in the paper was performed.

I am not a specialist in marine geophysics, and I hope one of the other reviewers is. I am curious what they will think.

Cheers,

Douwe

Comments are provided in attached peer-review form.

Section A: Overview of manuscript

A1) Overall evaluation, general comments & summary

A1.1) Reviewer's comments

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

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

Dear editor,

I have read the paper of Graham Eagles with interest and in detail. I have commented line by line, and the review has become a bit longer than I intended, but I hope the author will be able to use it to clarify his manuscript.

The paper starts by showing that existing reconstructions of the west and east Indian ocean are not in agreement. In particular, existing models infer that India moved along Madagascar prior to the end of the Cretaceous superchron.

Then the author summarized existing data from the southeast Indian ocean, and much of the new model builds on an interpretation of two bathymetric images, Figures 6 and 7. In the Enderby basin, the author infers that there may be more continental lithosphere than previously interpreted, and in Figure 7, the author argues for a large-scale strike-slip displacement of Sri Lanka, and oceanic lithosphere to the north of that, changing the interpreted opening direction by some 45° within the Cretaceous superchron.

With those new interpretations, the author proposed a new model that reconciles the opening of the eastern Indian ocean with a static India-Madagascar block prior to 95 Ma, and argues that with this new model, initiation of subduction documented between the Indian Plate and Arabia cannot be linked to a minor plate rotation induced by a mantle plume.

Because this paper promised to offer evidence against our reconstructions, I read it with interest and in great detail but I do not find hard evidence that sends me back to the drawing board on plume-plate interaction or subduction initiation. That does not mean that the new interpretation of the SE Indian ocean is not interesting – although I don't think this interpretation is unique or unequivocal. I hope the suggestions, questions, and remarks will be of use to improve the manuscript.

Cheers,

Douwe van Hinsbergen

September 18, 2023

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

☐ No changes required

☒ Rewriting

☒ Reorganising

☒ More data/figures

☐ Condensing

☐ Reinterpretation

☐ Other

Comments:

I have several main questions and concerns that the author may want to address.

1) The author uses the age of 95 Ma as the onset of India-Madagascar motion, and this age features also in the Bengal Basin and elsewhere in the model. It is unclear to me what the evidence for this age of onset of motion is, what the age is based on, and what the uncertainties of this age are.
2) The author excludes motion between India and Madagascar prior to 95 Ma, but does not address the evidence for such motions that Gaina et al (2015) inferred from the geological records of ophiolites and underlying melanges along the NW Indian Plate.
3) The most important new inference that underpins the new model is an inferred transform motion of Sri Lanka along the southeast Indian margin between 95 and 84 Ma. First, the basis for these ages and their uncertainties are not clear to me. Second, the evidence for this strike-slip motion if the map of Figure 7, which argues that India and Sri Lanka are separated by a 700 km long transform fault on which there are releasing bends. The amount of strike-slip may be estimated from the amount of the extension in the releasing bends, and the total with of the releasing bend is then a maximum for the amount of extension. There is no evidence presented in the paper to evaluate whether these are indeed releasing bends, but assuming they are, the bends indicated on Figure 7 are perhaps 150 km wide. However, the author estimates 825 km of displacement and argues later that Sri Lanka ends up 1000 km farther east in Gondwana than previously shown. I do not understand where these much larger numbers came from. I do not see how the evidence presented in Figure 7 supports the new model.
4) To satisfy evidence for subduction initiation between 105 and 96 Ma from ophiolites to the west of India along a plate boundary that connects to the Madagascar-India boundary, van Hinsbergen et al. (2021) invoked a 1-2° rotation of India versus Africa. The author excludes this ‘large-scale’ rotation but nowhere quantifies the uncertainties of his reconstruction. I do not see any argument in the paper that excludes this rotation. The author only shows that he does not need to invoke this rotation to explain the observations of the east Indian ocean sea floor, in which there are no magnetic anomalies and

there is no detailed age information. So the new model argues against the interpretation of our 2021 paper but does not mention the underlying data and does not offer an alternative explanation. I think the conclusions on plume-plate interaction (not only in this paper, but also in Lucia's previous paper) are overstated, as further explained in the relevant section below.

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

☒ Text

☐ Table

☐ Figures

☐ Supplementary material

Comments:

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

☐ Quality is poor

☐ Research is not reproducible

☐ Other

Comments:

[Free form box]

A1.2) Author(s) Responses:

First, and most importantly, I would like to express my sincere gratitude to Douwe van

Hinsbergen for choosing to write a constructive review of my manuscript, despite the critical assessment of some of his previous conclusions that I presented in it.

Understandably, the review focuses on the manuscript through the lens of Douwe's previous work, which dealt with the notion of 'plume push', or ideas concerning the effects that mantle plumes can have on the balances of forces driving tectonic plates. It refers repeatedly to a plume push paper published in 2021 in Nature Geoscience, which is also cited in the main text, because that paper dealt with a similar period and focus area. For space reasons I will refer to that paper from here on as "PP2021".

Whilst I am happy to respond here in some detail by discussing PP2021 further, I have decided not to match the volume of that discussion in the revised manuscript. This is because the treatment of plume push in my first version manuscript was proportionally quite small, and I would like to try and avoid affecting the balance of the revised version.

There are some unfortunate misconceptions in the review that I will describe and comment on as they appear. The first one appears in §A1.1.1, which states that "much of the new model builds on an interpretation of two bathymetric images". The manuscript text and figure captions, and some of my comments later in this form, instead set out clearly how those figures present information based on multiple data sets derived from multiple geophysical techniques. In fact, none of the figures presents any bathymetric data.

§A1.1.2 sets out four comments and questions that might need to be addressed for the manuscript to be publishable. I will give mostly synoptic replies in this part of the form, and more detailed responses later where those comments and question recur in later comments.

1) *Comment on the source of a date (95 Ma) that is widely used in the manuscript*.

   The ~95 Ma age for onset of India-Madagascar motion was based on the oldest (92-89 Ma) dates of rocks in the Morondava large igneous province, suggested to be emplaced under slight extension, by simply adding on three million years for this extension to have started. In the updated version of the manuscript, I describe a less arbitrary approach that makes use of seafloor spreading rate considerations to estimate an earliest possible date of 94 Ma for opening of the basin. In this form, I will continue to refer to a date of 95 Ma, for clarity.

2) *Comment on not addressing inferences of India/Madagascar motions prior to 95 Ma, based on ophiolites and underlying melanges along the NW Indian Plate, by Gaina et al (2015)*.

   The comment refers to rocks of the Waziristan-Khost ophiolite caught in collision of India with the Kabul block during closure of the Neotethys ocean, involving subduction initiation at 96-90 Ma. The 96-90 Ma dates have yet to be published. PP2021 took the 96 Ma age, assumed that the plate convergence preceding subduction started at ~105 Ma, and suggested controversially that this closure occurred along the African/Indian plate boundary (by placing the Kabul Block on the African plate at the time). The placement is controversial because (i) the Waziristan-Khost ophiolite is the sole observational constraint on the western Neotethyan plate boundary configuration at 105-96 Ma and (ii) the 105-96 Ma date depends on unpublished dates, and an assumption about the time required for convergence to proceed to subduction. As well as referring to this directly, the revised manuscript also clarifies that plausible competing schemes exist in the literature, citing two of them.

3) *Assorted criticisms:*

(i) Criticism of the manuscript's interpretation of the timing of plate motion along the east coast of India.

The manuscript text cites peer-reviewed research that enables the timing of displacement to be inferred from multiple published resources that present Cenomanian-Turonian faulted rift sequence strata, backstripping evidence for rapid late Cretaceous subsidence, and ~90 Ma-onset radiometric dates of basaltic layers in the fills of the Cauvery and Mannar basins, as well as apatite fission track analyses revealing late Cretaceous uplift along observed faults in onshore locations in Sri Lanka. Without a detailed set of reasons why the findings of all these studies should not be interpretable in terms of an active late Cretaceous plate boundary between Sri Lanka and India, I am not ready to consider changing that interpretation.

(ii) *Criticism of the manuscript's estimate of the amount of Vasuki plate motion past the east coast of India.*

The comment is based around a displacement estimate, derived from a consideration of the width of basins affected by extension as the Vasuki and India plates diverged, that is ultimately based on a misconception of the proposed kinematic setting of the basins. I think the misconception was possible because of how I drafted one of the figures, and so I have worked to ensure it should not be repeated in the minds of other readers. The comment also fails to recognize that extension-based estimates are unlikely to generate estimates for the full amount of relative plate motion, because crustal extension is unlikely to have been the sole mechanism for its accommodation. A later reply will provide more detail on these responses.

(iii) *Criticisms of the manuscript's interpretation of the presence of transtensional basins ("releasing bends") along the east Indian margin.*

The interpretation of transtensional basins along the eastern Indian margin does not originate in my manuscript. Instead, the manuscript attributes it to peer-reviewed papers, based on deep seismic reflection images of basin-bounding faults that splay off the ends of the near-vertical crust-cutting fault along the Coromandel transform and pass into the Cauvery and Krishna-Godavari basins.

(iv) *Criticism of manuscript providing inconsistent estimates (1000 km and 825 km) for the amount of Vasuki plate motion.*

This criticism is based on another misconception. Only the "825 km" figure is explicitly described in the manuscript as an estimate of the Vasuki plate's motion. The "1000 km" measurement is described as an estimate of the resultant of that motion together with alternative models' ideas of a 'scissor rift' Mannar Basin.

4) *Comment on the lack of quantification of the uncertainties in the new reconstruction.*
Tuck-Martin et al (2018) give quantified uncertainties for the African-Antarctic relative plate motion parameters used in the reconstructions. The visual-fitting techniques used in the rest of the project do not straightforwardly permit calculation of quantitative uncertainty estimates. From the contexts in which comments like this one are repeated in the rest of the review, I suspect that this comment is intended to infer that the model may be formally incapable of resolving 1-2° of Madagascar-India rotation at 105-96 Ma, as in PP2021. Formally, of course, the new model is pathologically incapable of resolving such a rotation because I adopted the absence of observational evidence for any pre-95 Ma Madagascar-India motion as an explicit constraint on its architecture. An inverse test, however, of whether the PP2021 model (which does feature such a rotation) honours azimuthal constraints of the kind applied to build the new model, is given later in this form. PP2021 fails that test.

Finally, a more useful comment about uncertainties might instead have targeted the binary consideration implicit in my study's rationale: Is it more reasonable to build a model using the absence of evidence for relative motion as a specific constraint, or instead to permit a model to show relative motion under the assumption that its record is lost and/or cryptic? We can expect the likelihoods of the former to increase and of the latter to decrease along with the increase in magnitude of suspected (artefactual or lost/cryptic) relative plate motion. Using PP2021's *GPlates* model, this magnitude is about 200 km of plate divergence between Madagascar and India in the ~13 Myr period preceding the first observational signs of Mascarene Basin opening. Starting from our knowledge of the plate kinematic controls on extensional basins and continental margins worldwide, I contend that the most parsimonious interpretation of the lack of evidence for 200 km of plate divergence between India and Madagascar before 95 Ma is that no such divergence occurred.

Many of the review comments in the rest of this form repeat variations or elaborations on parts of these four comments and questions. This means my responses will necessarily also feature some repetition, which I have tried to keep concise.

A2) Summary of main merits and main points of improvement

A2.1) Reviewer's comments

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

The main merits I have found are...

[Free form box]

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

[Free form box]

A2.2) Author's responses:

[Free form box]

Section B: Detailed evaluation of manuscript

B1) Title and abstract

B1.1) Reviewer's comments

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

The *Title* describes the main topic of the manuscript **accurately** — [YES] / [NO]

The *Title* describes the main topic of the manuscript **succinctly** — [YES] / [NO]

The *Title* includes **appropriate key terms** — [YES] / [NO]

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

The *Abstract* supports the rationale with **sufficient background information** — [YES] / [NO]

The *Abstract* includes a **well-balanced description of the methods** — [YES] / [NO]

The *Abstract* describes the **main results sufficiently and adequately** — [YES] / [NO]

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

The *Abstract* clearly states the **conclusions of the study** — [YES] / [NO]

The *Abstract* is **clear** and **well structured** — [YES] / [NO]

Comments:

l. 16: After having read the paper, I think it's more accurate to say that you hypothesize these two plates. You do not show resolution or demonstrate displacements as far as I can see?

B1.2) Author's responses

I have adopted the suggested terminology in appreciation of the review's constructive intention. However, I should point out that, without evidence for displacements at some reliable resolution, there would be no need for a hypothesis to explain anything. In fact, the manuscript shows abundant evidence for material displacements at the boundaries of previously unsuspected plates, in the form of the discrepant azimuths of fossil mid-ocean ridge and transform fault features, all estimated using multiple data sets from multiple stretches of the margins. Citing multiple studies of such settings, it also confirms that the discrepancies in the compared azimuths exceed uncertainties in the observational data and interpretations used to observe them, and thus do indeed provide the necessary resolution to prompt the need for some explanation, in the form of hypotheses.

B2) Introduction

B2.1) Reviewer's comments

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

The *Introduction* provides **sufficient background and context** for the study — [YES] / [NO]

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

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

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

The *Introduction* contains **relevant, suitable citations** — [YES] / [NO]

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

Comments:

l. 32: This is a rather qualitative and subjective requirement. As accurate as possible may be entirely unreliable, or much more precise than required for reliability. What is or is not reliable depends on the question you want to answer with it.

l. 67: What is this 95 Ma number based on, can you give a reference? The break-up occurs sometime in the superchron and LIP-related basalts of 91-89 Ma occur on both sides of the Mascarene basin suggesting that the two margins were in each others vicinity in this time.

l. 69: As I explained above, the motions we inferred were very subtle, just enough to cause ~50 km of E-W convergence in NE Arabia in a 10 Ma time period that is required by the metamorphic sole records that we dated. This interpretation was not based on sea floor interpretations, but I simply did not see evidence in the available magnetic records to refute this option.

L. 72: This is not 'in contrast'. This is simply in addition. There is plume-related intrusion and extrusion in a zone of continental extension. That does not exclude that there is subtle pre-91 Ma extension.

l. 81: As I explained above, the full plate boundary does not end in the Mascarene basin, but continues between India and Arabia. And there are geological records there that demonstrate E-W convergence by 96 for sure in the form of SSZ ophiolites in Pakistan and Oman, and by 104 Ma by consistent Lu/Hf garnet ages in Oman as well as the continuation of the plate boundary to Turkey. So while it may be the easiest to conclude that nothing is going on from a sparsely documented geological record from Madagascar and India that does not have the stratigraphic resolution to exclude subtle motions before 92 Ma, the inclusion of well-documented and widely studied geological records between India and Madagascar demonstrate that there may well have been some motion between India and Madagascar.

l. 81: Can you quantify those motions? And can you indicate what the relevance of them is? For instance, the absence or presence of transform motion between India and Madagascar does not affect the rotation hypothesis discussed above.

B2.2) Author's responses

Line 32: Agreed. I have changed the text accordingly.

Line 67: The ~95 Ma age for onset of India-Madagascar motion was based on the 92 Ma date of oldest rocks in the Morondava LIP and the fact that these were emplaced along an active rift zone. I have altered the text in this section so that it gives a more robust set of considerations for the likely age of onset of Mascarene Basin opening, arrived at using considerations of how it must have affected seafloor spreading rates and azimuths in the plate circuit.

Line 69 & Line 72: I have already stated in this form, as well as in the original text, that PP2021 requires and portrays 200 km of divergence between India and Madagascar before 95 Ma. This divergence is an obligate corollary to that paper's "very subtle" 50 km of convergence to explain metamorphic soles in Arabia. Of course, this divergence cannot be refuted using seafloor magnetic records, because it plays out during the Cretaceous magnetic quiet period. However, by noting that it begins with the ~1500 km-long conjugate margins of India and Madagascar united, we can expect the 200 km of divergence to have been accommodated by continental extension and so to have produced or altered at least 300,000 square kilometres of surface area between them between 105 and 96 Ma. Although a 1-2° rotation may seem easy to describe as “subtle”, the same term is less easy to apply to 300,000 square kilometres or more of missing mid-Cretaceous real estate.

Line 81 #1: The second part of this comment contradicts itself, making it difficult to respond to. Either the geological record between India and Madagascar is “sparsely documented” or it is “well-documented and widely studied”, but it can’t be both. Regardless of what was meant, the ophiolite and other evidence cited in PP2021 for the action of a post-Somali basin and pre-Mascarene basin convergent plate boundary in Oman, Turkey and Pakistan is of course compelling. It does not, however, oblige that plate boundary to have acted between the Indian and African plates. Alternative interpretations for the plate boundary configuration in Neotethys exist, and are widely cited. Indeed, the complete absence of geological observations for the anything-other-than-subtle relative motion accommodated at the same margin’s southern continuation between Madagascar and India, or for that matter anywhere other than the Waziristan-Khost ophiolite, argues strongly against PP2021’s plate boundary interpretation.

Line 81 #2: The motions are all quantified in the paragraph above line 82 using measurements calculated using GPlates implementations of all the cited models.

B3) Data and methods

B3.1) Reviewer's comments

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

The *Methods* are described **concisely and with enough detail** for reproducibility — [YES] / [NO]

Necessary information about **data sources/acquisition/processing** is included — [YES] / [NO]

Data used are accessible via either supplementary files or links in the data availability statement — [YES] / [NO]

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

Comments:

l. 86: You review a series of reconstruction techniques for extensional systems. There are also reconstruction techniques for convergent systems, which I for instance summarized in van Hinsbergen & Schouten 2021, on which the small rotation of India vs Africa/Arabia between 105 and 96 Ma was based that we published in our 2021 Nature Geo paper. Whilst convergent margin reconstructions are always minimum-motion reconstructions, they provide direct constraints on plate motions in regions where lithosphere is lost. I appreciate that your paper is not focused on active margins, but for the conclusions you draw on the interpretations of plume-induced rotation, they are relevant, since that rotation follows not from the techniques you summarize, but from convergent margin reconstructions.

l. 96: The reason to include this section is not entirely clear to me. The interpretation of the architecture of an older orogen cannot be an argument to reconstruct a younger ocean. That should go the other way around, as you write in the last sentence where you refer to Boger's work. So why do you give this information here?

l. 130-138: In addition to the previous comment: can you explain in the opening remarks of this chapter what information you will present and why? I appreciate that there is 200 km of uncertainty in pre-drift extension reconstruction, but I'm a bit puzzled why you present that at this stage. That doesn't matter for how we reconstruct the ocean floor, does it, unless there is evidence that there is extension in the margins after ocean spreading started?

L. 151-152: This sentence suggests that the continental margin reconstructions play a role in how we reconstruct the ocean floor, but it doesn't, does it? It only plays a role in the pre-drift extension reconstructions, which are always less certain than magnetic anomaly reconstructions (well, depending on magnetic anomaly frequency I suppose). From the introduction it wasn't clear why these passive margin reconstructions are important, perhaps you can clarify that a bit?

l. 153: Yes, this has long bothered me, it's interesting to see how you reconstruct Sri Lanka, I'm curious to see how you built that!

l. 155: add a reference behind Mannar basin

l. 174: 'eclipses' is a bit poetic: what do you mean here?

l. 175-176: idem: 'significant in proportion' is vague. Can you explain what you mean exactly?

l. 176-179: That may well be, but from the introduction and setting, it is not clear to me why this is important.

l. 182-194: I don't think this is very relevant. As you say, there are no reconstrutcions based on poles for the Mesozoic and younger of India, and nobody would do that anyway, because magnetic anomaly-based reconstrutcions have much lower uncertainties. We use the reconstructions in developing global APWPs like Torsvik et al 2012, not the other way around. That is only relevant for pre-Mesozoic reconstructions.

In addition, the 'reliability' of VGPs above an arbitrary cutoff of 25 directions, or whatever other number, is nothing but a guess. As with any dataset, the uncertainty decreases with increasing number of datapoints. We built a new APWP that steps by the artificial hierarchical level of poles recently (Vaes et al., ESR 2023, see also Vaes et al 2022 JGR for an analysis why this was necessary). But for your paper I don't think this matters much, I would simply leave the paleomagnetism argument out, because it isn't useful for the reconstruction, and hasn't been used.

L. 198: Well, dykes may well record PSV. But a conclusion of comparing a VGP-based pole with an APWP based on averaging poles compares apples and oranges anyway. We can take a look at whether that single pole suggests a rotation relative to India using the Vaes APWP, and with an approach that demonstrates at 95% confidence whether a paleomagnetic difference is geologically/tectonically significant (APWP-online.org, in revision for Tektonika). But I don't think you should go there in this paper. I'd simply delete the paleomagnetism section, since it is a lot of words to exclude one pole that isn't widely used anyway, and that is based on such a low number of datapoints that I don't think you'd be questioned for ignoring it.

I. 211: Perhaps start this chapter 3 with this section, delete section 3.1 and 3.3, and add 3.2 after this one?

I. 221: the concept of a plate circuit was not introduced in this paper, perhaps an addition of a seminal work on this concept is in place. Cox & Hart or so.

I. 224: I think Doubrovine and Tarduno (2008) developed those error propagation schemes for the first time?

I. 225: This is true, but this is also widely agreed upon. The way you structured this chapter, it reads as if the international community has been making reconstructions based on continental margin fits and pmag, and that you make a case that it would be better to use magnetic anomalies and fracture zones. But everyone agrees with this, so you could come to the point quicker and just explain your new analysis.

L. 232: 'strong reproducibility' and 'very high resolution' are qualitative descriptions, can you quantify the uncertainty?

I. 234: But I presume the extended margin reconstructions are only relevant for times prior to the ocean floor reconstructions, so for what problem is this remark important?

I. 262: Did you need all that text to make that point? There is mot much pre-83 Ma ocean floor between India and Madagascar, and as Gaina et al. (2015) showed, there's only an incomplete record of pre-83 Ma crust along the east African margin, and only ophiolites along the east Arabian margin because of convergence in episodes in the Cretaceous and Paleogene. So there are no models that infer India-Madagascar motion before 83 (why 95?) Ma based on the western chain. The eastern chain is all there is, right?

L. 269: better update to Gradstein et al 2020, the latest version of the timescale. Makes a difference, 121-83.5

l. 279: Isochrons are interpretations, not measurements, right? If not, can you define what the difference is of an isochron and how that is measured?

l. 291: This is interesting, but am I right in concluding that none of these techniques will have the power to exclude a 1-2° rotation of India relative to Africa over a period of some 10 Ma after 105 Ma?

l. 295: if you do this by visual inspection, how do you quantify the uncertainty?

l. 315: How do you know these are 95 Ma old? There are no 95 Ma anomalies. What is the age uncertainty there? I presume 47 Ma is a reversal, which?

l. 319: I find it difficult to see this from Figure 3. That figure has a lot of lines and a few vectors, but I don’t see the fracture zones there. Is there a way to illustrate this clearer?

l. 322: Am I right in thinking that this is the real problem that you will solve in this paper? It could help the reader if you explain this mismatch between the predicted and observed fracture zone orientations much earlier in the paper, and that you aim to resolve this by proposing a new reconstruction for the eastern chain.

l. 324: Farther instead of further?

l. 325: Again, how do you know they’re pre-95 and not just pre-83.5?

l. 326: Please refer to a figure

l. 322: What was the reason for others, like Gaina, to infer that there must have been motion between India and Madagascar before 95 Ma? I presume there was an analysis underlying that conclusion and I think it’s relevant to explain that in the ‘previous work’ section.

l. 348: An inset in the Figure to show the location in the Indian Ocean would be helpful.

l. 384: What produces the ‘reversals’ that were interpreted as magnetic anomalies if this is continental crust? There are no dredges or other direct data to establish whether this is oceanic or continental crust?

l. 384: just ‘consensus’.

l. 387: Is the crustal thickness known of the part of Enderby that you interpret as continent?

l. 395: Farther instead of further?

l. 409: ‘can be’ or ‘is tentatively’. I presume you mean that you interpret it this way?

l. 429: The train of basins may be 700 km long, but the pull-apart basins you draw in the inset is no more than about 150 km long. And is this basin is deforming continental crust, the displacement is of course much less than 150 km. So this strikes me as a secondary fearture, isn’t it? The main opening history is parallel to the IA-AA direction you indicate I presume?

l. 432: Dated how?

l. 433: How is 94-85 dated?

l. 436: can you quantify ‘relatively low temporal resolution’? And are the extensional faults observed or inferred from the cooling?

l. 476: Is this a new interpretation? I thought this was the way the 85E ridge was explained in the first place, by the Kerguelen plume stepping from the Australian-Antarctic system westwards over a transform to the Indian plate where due to rapid northward motion a long chain forms, until it steps back to the Australian plate around 40 Ma or so? So it makes sense that the chain overlies a transform zone. Hasn’t every plate reconstruction treated it this way?

B3.2) Author's responses

Line 86: The convergent margin reconstruction 'technique' referred to is, to directly quote the paper that the comment cites, better described as a set of "rules of thumb". The comment itself states that these rules of thumb provide minimum motion estimates. The "small rotation of India vs. Africa" in PP2021, which the comment also states was derived using such rules, is thus to be regarded as a minimum estimate. In other words, it might realistically be expected to imply *more than* 200 km of pre-95 Ma cryptic plate divergence between Madagascar and India. As 200 km is implausible on its own already, I do not intend to over-lengthen my manuscript by rehearsing these additional arguments there also. Instead, I have included a more general observation about the imprecision of convergent margin reconstructions.

Line 96: I agree with the reviewer that the cart is put before the horse when Precambrian correlations are used to define reconstructions. It does, nonetheless, happen. A prominent example of this appears in the paper by Yoshida et al, cited in the manuscript. With 115 previous citations, that paper is the second most highly cited of all papers in the Scopus database to include both "Sri Lanka" and "Gondwana" in their titles. Given all this, I have retained the section.

Line 130-138: I have added a statement about how these data can be used, in combination with seafloor isochrons, for defining the earliest stage of plate divergence to have been accommodated by seafloor spreading.

Line 151-152: The comment is essentially the same as that given to lines 130-138 and requires no further response.

Line 153: No action required.

Line 155: No references added to the text because extensive references are given in the caption to the figure accompanying the statement. Instead, I have called out the figure at a slightly earlier point.

Lines 174-179: I have changed the poetic word for something more prosaic, and added text that I hope makes the intended meaning clearer.

Lines 182-198: I agree with the reviewer that the paleomagnetic tool has been misused and its results overinterpreted for determining Sri Lanka's position in Gondwana. Our shared opinion does not change the fact of their misuse and – in the study area here, at least – the prominence of papers (Yoshida et al., 1992, again) presenting it. This makes it necessary to keep the section in the revised manuscript.

Line 211: for reasons given in previous responses I have opted to retain the overall structure of section 3 in the revised manuscript.

Line 221: No changes made. Despite the comment's implication, the citation is not intended to direct readers to a description of the concept of plate circuits but, as the text containing it describes, to a description of the use of plate circuit solutions to constrain continental extensional processes independently of reconstructions based on interpretations of the shapes of extended continental margins.

Line 224: The error propagation schemes used in the cited reference go much further back than 2008. I have included a new citation of what in my view is the most pertinent of the early papers on the subject, and later in the section added further references for instances of its application in the study region (my intention with the original single citation).

Line 225: No change made. I think that this paragraph is not too lengthy an excursion to present the properties and implications of this data source compared to the paleomagnetic and extended margin fitting methods, whose results seem to hold unnecessary sway in the minds of many users of plate reconstruction models for the eastern Indian Ocean.

Line 232: In the revised text, I have quantified the uncertainty for the example given in Figure 4.

Line 234: See the response above to the earlier comment about lines 130-138.

Line 262: See the responses to the comments about lines 96, and 192-198, above.

Line 269: Agreed. I have updated the model and text to use the Gradstein et al. (2020) timescale.

Line 279: I'm not sure what the "difference" of an isochron is that I am being asked to define here. If the comment is referring to an isochron's orientation, as the text at line 279 does, then I would prefer to conclude for now that this is a property for which Earth scientists will need no explicit definition.

Line 291: The comment muses on whether it would be possible to conclude that observations of plate motion azimuths in fault and isochron data might, within their uncertainties, be incapable of resolving the 1-2° rotation in PP2021. I tested this possibility using the *GPlates* model supplement from PP2021, which includes such a rotation, by generating small circles about its India-Antarctica rotation for comparison to the corresponding traces of India-Antarctica plate divergence (fracture zones) on the flanks of the central Indian Ridge in the 105-96 Ma period. If the azimuthal methods discussed at and before line 291 were to lack the power to resolve the 1-2° rotation, then we could expect the orientations of the small circle arcs near that ridge all to lie within 6° of the orientations of similar aged fracture zone segments on its flanks (section 3.5 cites an influential study as the source of the 6° sensitivity bound). Whilst I found that some of the fracture zones agreed moderately well (east of the northern end of the 85° East Ridge), the azimuthal misfit rapidly degrades southwards and westwards to reach values in the region of 25° (figure 1). This pattern is a strong indicator that the India-Africa Euler pole around which the 1-2° rotation is applied is located too close to the ridge crest. The test confirms that azimuth observations indeed have the power to rule out near-pole rotations like the one in PP2021, within their stated resolution.

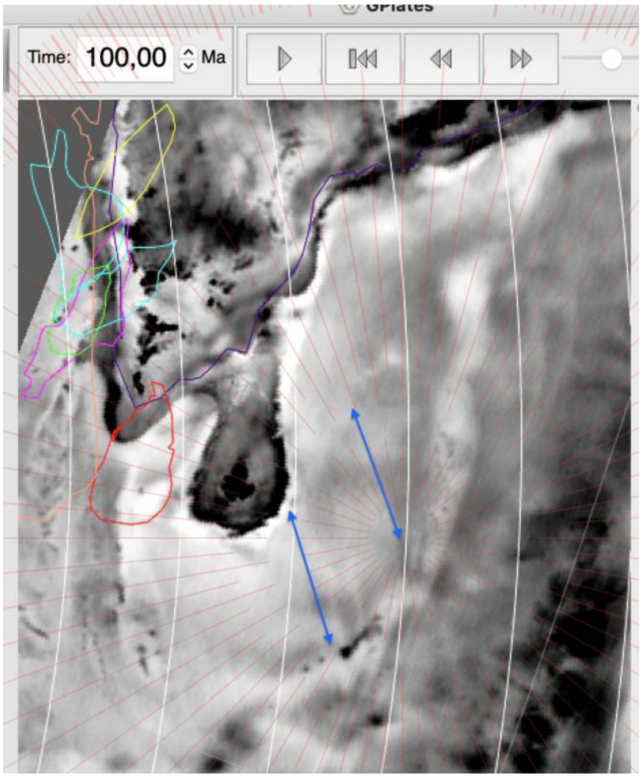

Fig. 1. Compare PP2021 model INDANT small circle arcs for 105-96 Ma (white lines) to orientations of similar-aged fracture zones on the central Indian Ridge flanks (gravity base image). Blue arrows highlight fracture zone segments whose azimuths diverge by 25° or more from the small circle arcs. Overlay graticule interval is 5°, using Red Lines app (https://apps.apple.com/de/app/red-lines-tools/id1469400117?mt=12).

Line 295: I think this comment is asking about how I ensured that plotted small circle segments and observed faults and isochrons coincided within the uncertainty estimates given in the text. In some instances, I calculated the orientations using my own C-shell script that applies the Haversine formula to line segments represented by lists of longitude-latitude points. In others, I used either an on-screen protractor-overlay app (as in figure 1) or a printed screen grab and plastic protractor.

Line 315: See a previous response about the 95 Ma age and its uncertainty. 47 Ma refers to the C21 isochron.

Line 319: A vector describing the observed fracture zone azimuths is plotted and labelled in an inset to the figure. I have added to the text so that readers are explicitly directed to the inset. I have also enlarged Figure 1 to make it easier to locate the observed fracture zones.

Line 322: I have added text to the introduction section to make it clear to readers that I will present a new model of the eastern chain.

Line 324: I think that the proposed distinction between "farther" and "further" can be useful in instances where "further" might be confused with "moreover". This is not one of those instances.

Line 325: See previous comments on the source of the 95 Ma age and the fracture zone bends.

Line 326: Added reference to figure 1.

Line 322: In the *GPlates* supplements to Gaina et al's paper, Madagascar's motion occurs implicitly as part of an African plate, whose motion is defined with respect to the North American plate. India's motion is listed with respect to Siberia but only by recalculation, having been defined originally with respect to Madagascar using rotation parameters that are listed by Torsvik et al (2008). These, in their turn, were attributed by Torsvik and co-authors to a manuscript in preparation by Müller and Gaina, which, as far as I can tell, was never published. I conclude, as there is no observational record of pre-95 Ma motion between India and Madagascar, that Gaina et al ultimately derived their pre-95 Ma India-Madagascar motion from an unpublished Indian Ocean plate motion model. I have decided not to include this description in the revised manuscript as it does not add to the existing message that such motions are likely artefactual.

Line 348: The figure's location within the Indian Ocean is shown in Figure 1.

Line 384: I can’t respond constructively to this comment because it is not clear to me what continental crust it is referring to. The sentence at this line does not mention any continental crust, nor does the paragraph it occurs in.

Line 384: removed “useful”.

Line 387: crustal thicknesses have been determined for the handful of refraction seismic profiles located in figure 9.

Line 395: Here, too, I see no risk of readers misinterpreting my “further” as a ‘moreover’.

Line 409: Correct, I made the interpretation tentatively.

Line 429: This comment is based on a mistaken assumption that the basin opening direction should be parallel to the IA-AA vectors in figure 7a. On this basis, the comment contends that the amount of plate motion responsible for the basins’ formation cannot exceed 150 km, their width parallel to that vector. I can understand why this mistake could be made, because I had plotted one of the IA-AA vectors so far across the smooth area of gravity field in the western Bay of Bengal that it reaches the Coromandel transform zone, next to the basins, on the eastern Indian margin. I have reduced the length and prominence of the IA-AA vectors in the figure so that it no longer invites such a misunderstanding. Using the intended blue V-I (Vasuki-India) vector in the inset, instead, allows the full length of the basin train (which a typographic error in the original manuscript unfortunately put as ~700 km) to be measured as 790 km. This is of course still less than the total cited V-I plate motion (825 km), and also includes a component of pre-extensional (i.e. pre-existing) length. To suggest, however, as the original comment does, that this should invalidate the plate motion estimate, requires an unrealistic assumption that the entire history of V-I motion ought to have been accommodated by crustal extension beneath the Cauvery and Mannar basins alone. A more complete kinematic budget should include estimated crustal extension in the 200 km-wide Krishna-Godavari basin at the opposing (northern) end of the Coromandel transform, slip along the transform itself accommodating V-I seafloor spreading and oceanic crust formation in the adjacent western Bay of Bengal basin, and possible slip along strike-slip faults within, between, and at the margins of the Cauvery, Mannar and Krishna-Godavari basins. Such a budget can be crudely completed here using existing data: Backstripping of wells drilled near the centre of the 790 km-long train returned crustal stretching factors in the region of 1.5 (e.g. Narasimha Chari et al, cited in the manuscript), implying a total of up to 330 km plate divergence if a similar factor is assumed for the 200 km-wide Krishna-Godavari basin. Oceanic crust, measured along the V-I azimuth adjacent to the Coromandel transform, is ~400 km wide. Hence, it is quite a straightforward task to account for 730 km of the 825 km total V-I motion estimate from the GPlates project. The ~95 km shortfall might plausibly be related to slip along strike-slip faults within and between the Cauvery and Mannar basins, and/or stretching factors that exceed 1.5 in the deeper, undrilled, possibly even oceanic, parts of the Mannar basin. Given their ‘back of the envelope’ character and the lack of more detailed maps of stretching factors, I have opted not to include these considerations in the revised text.

Lines 432 & 433: 94-85 Ma is intended to illustrate the approximate chronostratigraphic range of the cited Turonian-Santonian interval, for whose identification in the basins I have updated the text to give an idea of the methods used in the cited papers.

Line 436: Removed “relatively low temporal resolution”. The relative precision should have been clear from the terms “early- and mid-Cretaceous” as opposed to stage names elsewhere in the paragraph.

Line 476: The interpretation of the valleys at top basement as the traces of strike-slip faults is new, and as described in the text it contrasts with Choudhuri’s alternative interpretation of them as flexural basins. The sentence at line 476 did not deal with interpretations of the 85°E Ridge as a whole, whilst the reviewer comment on it does. For what it’s worth, the comment accurately describes one such interpretation, albeit one which is by no means definitive.

B4) Results

B4.1) Reviewer's comments

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

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

The *Results* findings are presented **clearly and succinctly** — [YES] / [NO]

The text in the *Result* section **cites tables and figures appropriately** — [YES] / [NO]

The *Results* directly **relate to the study objectives** — [YES] / [NO]

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

The *Results* **text belongs to the Results section**, not to *Introduction*, *Methods*, or *Discussion.* — [YES] / [NO]

The *Results* section is **organised effectively** — [YES] / [NO]

Comments:

l. 481: The only evidence you have indicated is the map of Figure 7, which is a bathymetric map on which you draw faults suggesting there are some pull-apart basins along the Indian coast. You did not show an amount of displacement, but that is obviously less than the N-S width of the pull-apart basins i.e. less than 150 km or so. Moreover, the ocean to the east of this basin has lineaments that are oblique to the margin. If these are not fracture zones, then what are they? And why do the pull-apart basins along the margin contraduct that oblique spreading direction? There could be strain partitioning, or one could have formed after the other: you did not demonstrate that the two are synchronous features.

l. 481: I'd say that you hypothesize the existence of a third plate. I don't see what you think is conclusive evidence.

l. 482-485: This is a very complex sentence, can you simplify? I thought you didn't believe there is an abandoned ridge crest there?

l. 485: rephrase: newly hypothesized. I think that ‘recognized’ is overstated if the evidence presented are two bathymetric maps, no direct age information, and no new independent evidence demonstrating that previous interpretations are false. It’s simply another view on existing evidence.

l. 487: can you give a link to some background reading on this story?

l. 488: So you already published this model elsewhere?

l. 502: the uncertainties are quantified by the moving hotspot models that you cite. Can you indicate those error bars and show how your hotspot motions relate to those previously estimated errors?

l. 509: Just out of curiosity, because it doesn’t matter for this paper, but what is the basis for the GA polygon that you draw, and its rifting from India? Where are its geological remains today?

l. 523: I’d refer to it as the reconstruction. A ‘model’ is such a vague term.

l. 526: But this is contradicted by the identification of anomalies up to 124 Ma in the West Somali Basin, and Albian ocean floor between that is found as relics in the melanges below the Waziristan-Khost Ophiolite of west Pakistan-east Afghanistan (Gaina et al., 2015). Perhaps we misinterpreted that information, but it is relevant for your review and reconstruction.

L. 531: It is not clear to me what the ages of 95 and 84 Ma are based on, can you clarify this in the review?

L. 536: In your model, there is continuous oblique divergence between India and Arabia throughout the late Cretaceous. However, the geological record of the Waziristan-Khost ophiolite demonstrates convergence that must have a strong E-W component between the Kabul Block and India. This is dated by the 96 Ma SSZ ophiolites that show that subduction was self-sustaining by this time, and the arrest of subduction by emplaceing the ophiolites over the Indian margin by ~80 Ma. The counterclockwise rotation of India vs Madagascar that Gaina inferred successfully explains this subduction history (and was not based on that subduction history), whereas your new model leaves those observations unexplained as far as I can see. I think this is relevant to discuss, here, or in the next section of the paper.

L. 534: It looks like your Vasuki plate is placing Sri Lanka ~750 km to the northeast along the Indian margin (in present-day coordinates). I don't see how the evidence presented, which I think is only Figure 7, justified this. The margin has a few step-overs that could be consistent with right-lateral strike-slip, but while the fault may be 750 km long, the step-overs as you draw them certainly did not accommodate 750 km of motion, but less than their 150 km width. If they had 750 km of extension, you'd expect something like the Cayman Trough. The fabric of the sea floor to the east of the SE Indian margin is oblique to the trend you're showing and does not provide evidence for the motion, and therefore the existence, of a Vasuki plate, so I think you should explain in more detail why you interpret a 700 km displacement along this margin in the data section.

L. 542: Farther west (distance rather than figurative)? Check throughout text.

I. 568: Because there's no section 5.1.2, I'd not number this section. In addition, the uncertainty you discuss is only about the age and not the Euler pole positions or rotation angles. I suggest to just integrate this paragraph in the text above.

I. 578: What is the uncertainty on this age?

I. 586: I think this interpretation is slightly misleading. There is 92-89 Ma LIP volcanism whose products are found on Madagascar and in southern India and that is thought to relate to a mantle plume. The sea floor is consistent with breaking up not long after this epidose. But the rifting may have started earlier, there's nothing precluding that, and the breakup didn't cause the magmatism.

L. 589: How do you know it's around the same time, there are no magnetic anomalies in this interval. Or do you assume it's at the same time in your model?

I. 605: Are there any mid-ocean ridges today that cannot be recognized in the bathymetry, or ridges that are solely identified by mirrored anomalies that support this? How was the absence of a ridge in this region previously explained? That it was reactivated as a 90E transform or so?

I. 611: as before, in absence of a section 5.2.2., I'd delete this as a heading and just continue the discussion in a new paragraph.

l. 613: ‘Relatively precisely’ is vague. What are these numbers based on? Why 15° and 5°?

l. 616: This argumentation is better given in the description around Figures 6 and 7. See comments there: your argument for transform motion comes from transtensional stepovers that certainly did not accommodate 700 km of motion. ‘on the order of 100-200 km’: what is this based on? The width of the step-overs is less than that. The documentation of this displacement is critical to the interpretation of this paper, and the discussions that you build on it, so please explain these estimates where you provide the underlying documentation.

L. 622: While the model movie looks elegant, I do not understand where these numbers come from. The number 825 is given here for the first time. How did you arrive at it, or the ages?

l. 628: Please give this argumentation in the section where you describe the geology and evidence. The oldest Marion plume-related rocks on Madagascar and India are 92 Ma, so where does this 95 Ma come from? Which rocks are dated?

l. 637: If you’d continue the spreading longer, what consequences would that have for the model? I presume you’d start another ridge later, or partition motion over two ridges?

l. 650: How can crust be entrained in a mantle during extension? I don’t really understand this concept. What is the evidence that was explained by this model? Could you bring this series of interpretations, done by others, to the section where you describe the previously acquired arguments for a continental basement below the Kerguelen?

l. 656: Figure 9 is missing in the manuscript.

l. 666: If the displacement of Sri Lanka of 825 km would instead be 100 or 150 km as suggested by the widths of the restraining bends, would you then still need the Kerguelen to be continental?

l. 667: Do the western chain rotations take the evidence for Cretaceous spreading in the Somali Basins summarized by Gaina et al 2015 into account?

I. 668: Is this quantified by Tuck-Martin et al? Or is this your estimate of that resolution?

I. 677: Please introduce this graben in the review?

I. 680: Give refs.

I. 684: I'm a little confused by the numbers. In the description of the fault that you use for the displacement you mention that it's 700 km long. Then you reconstruct 825 km of displacement to end up 1000 km farther east than previously estimated. Can you explain in the analysis how you ended up with these numbers?

I. 690: Can you indicate what the resolution is? 1 vs 10 km? 10 vs 100? 100 vs 1000?

I. 691: Based on the evidence you present, it is impossible for a reader to judge whether your statement on the Kuunga orogen is accurate. If you want to make that claim, then I'd expect a full review on the structure and architecture of that orogen, and the awkward position within that orogen of Sri Lanka when using classical reconstructions. I'd say that this is a subject for a separate paper, and I'd simply indicate here that the position of Sri Lanka has repercussions for reconstructions of earlier orogens, and that at first sight, based on [cited key evidence] your fit may work well.

L. 693: In absence of numbers and error bars, how can the reader judge the resolution? As far as I can see, you simply shift Sri Lanka along the Indian margin until a kink in that margin, and you don't really show supporting geological evidence for it. So how do you determine the resolution?

I. 714: I can't remember this evidence from the review section, did you present it there? If not, please do.

B4.2) Author's responses

Line 481: I strongly disagree with the inference that bathymetric maps have low value as constraints on tectonic structures. This aside, it should have been clear from the figure caption that the map in Figure 7 is not a bathymetric map with some faults “drawn on”. It shows the vertical gradient of gravity, a product that is widely understood, valued, and used for its capacity to show buried and/or remote tectonic features in the world’s oceans in a level of detail that is very suitable for the interpretation of plate boundary products and processes, with fully georeferenced fault traces that were originally interpreted from high quality deep sounding seismic data that have supported multiple peer-reviewed publications. I have responded to the comments on pull-apart basins where they appeared earlier (in the reviewer comment on line 429 of the manuscript) and will not repeat that response here. Finally, the vertical gradient of gravity data clearly show that NW-trending fracture zone lineaments in Bay of Bengal basin end well offshore of the Coromandel margin, leaving a broad area of smooth fabric next to the margin itself in the western part of the basin.

Line 482-5: I have rewritten and added punctuation marks to the sentence so that it should now be easier to parse its meaning.

Line 485: The comment starts out by again stating incorrectly that the study only uses bathymetric data, and adds (without noting the cited evidence from synrift sequences in seismic and outcrop data, backstripping and fission track analyses, radiometric dates and, in the case of Mandara, a magnetic polarity reversal isochron) that the plate motions are not directly dated. Bizarrely, to my mind at least, the comment also seems to suggest that the manuscript’s new ideas and concepts would be somehow more valuable if they were to have been generated using new data. Aside from all this, the comment’s purpose is to recommend referring to Vasuki and Mandara as ‘newly hypothesized’, rather than “newly recognized” plates. I have changed my terminology accordingly.

Line 487: The Wikipedia entry on the Samudra Manthana is a good starting point.

Line 488: this model has not been published elsewhere before.

Line 502: It is difficult to understand what the comment is intended to achieve, or why. The sentence already quantifies the errors in the advection-based studies and makes clear that the hotspots shown in the figure are assumed not to move within the mantle.

Line 509: The northern edge of the Greater India-Argoland polygon is filled merely by assuming a relatively continuous-curvature northern edge of Gondwana between western Australia and Arabia in the predrift reconstruction. I appreciate that there are a range of opinions that can be adopted about the present-day location of Greater India, but having not worked or read seriously on the issue I don’t currently strongly favour any of them.

Line 523: accepted.

Line 526: This comment has two parts. The second part repeats an earlier comment (to line 81) about the Waziristan-Khost ophiolite, and requires no further response here. The first part is about the West Somali Basin, where pre-95 Ma relative motion between Africa and Madagascar. The exact timing of this spreading is the subject of a debate, stemming from contrasting “young” and “old” interpretations of the age of spreading cessation from magnetic isochrons in the basin, that both originated in the early 1980s. These different timings allow for different interpretations of the plates each side of the ridge in the basin, and have been influential as described next. “Young” cessation interpretations imply spreading rates between Madagascar and Africa that are slower than contemporary rates between Antarctica and Africa. They thus require and define an independent plate bearing Madagascar plate during some or all of the basin’s opening. This plate is attractive to some workers because it allows eastern chain reconstructions to produce tight margin-to-margin fits of eastern India and East Antarctica with little or no intervening Kerguelen Plateau continental material. However, there are difficulties reconciling those margin fits with observations of seafloor spreading features along the eastern chain that, in turn, have needed to have been rectified by further subdividing the ‘Madagascar-India’ plate along the line of the future Mascarene Basin during the lifetime of the West Somali Basin. I have already remarked on the absence of geological observations that record any such motions in the period to 95 Ma. ‘Old’ interpretations of magnetic isochrons in the West Somali Basin, in contrast, imply Madagascar-Africa spreading rates the same as those between Africa and Antarctica, and so describe the basin’s opening and abandonment simply in terms of relocation of the east/west Gondwana boundary from north to south of Madagascar, which stays fixed to India at all times before the Mascarene Basin opens. I have not rehearsed these comments in the revised text because they are already given elsewhere in the literature it cites, and because the fact of the debate about the West Somali Basin’s age does not affect the lack of observations that would confirm pre-95 Ma motions between Madagascar and India that one side of that debate requires should exist.

Line 531: the attribution of these dates and some discussion of their uncertainties is given in a later section.

Line 536: Although formulated differently, this comment repeats earlier comments (to line 81) about the Waziristan-Khost ophiolite. Its correctly observes that my model shows “continuous oblique divergence between India and Arabia throughout the late Cretaceous” and so does not reproduce the ophiolite emplacement scenario put forward in PP2021. This observation is indeed inconsistent with PP2021’s assertion of a long 105-96 Ma Neotethyan plate boundary between Indian and African plates. On the other hand, it is not inconsistent with the alternative, intra-Tethyan, contexts for the Waziristan-Khost ophiolite that exist in the literature, and which I cite in the revised manuscript, nor with the 96-90 Ma range of metamorphic sole-cooling dates for its inception, which overlaps strongly with the onset of its India/Africa plate motion. Hence, whilst the new model continues not to show a specific subduction-onset scenario for the Waziristan-Khost ophiolite, it does allow room to speculate on one.

Line 534: I responded to remarks very like these already (reviewer comment about the accommodation of strain in transtensional basins on line 429; reviewer comment contending the existence of NW-trending FZs reaching the Coromandel margin segment at line 481) and will not repeat those remarks here.

Line 542: See earlier responses.

Line 568: Section numbering is a style issue for the journal copy editor to decide on, should this manuscript go on to be accepted. I have already stated that the visual fitting procedure does not lend itself to a straightforward quantification of uncertainties in the Vasuki/India Euler rotation parameters. I note that it has nonetheless been possible to set upper and lower bounds on the likely amount of relative motion between the two (cf. response to comment on Line 616, below). In comparison, the 1-2° 'Waziristan-Khost' rotation, based on a set of rules of thumb for convergent plate margin settings, is a lower bound estimate only, and fails a test of its significance for pre-95 Ma seafloor spreading directions in the Indian Ocean (Figure 1).

Line 578: I have indicated an uncertainty by citing the range of available dates for the Indian-margin breakup-related volcanism. The formal laboratory uncertainties on the individual ages reported within that range are much smaller than the range and therefore of little importance for the uncertainties in Mandara motion timing discussed here.

Line 586: The text at this line is not an interpretation, but a statement of an association that can be understood from the literature. Storey et al, 1995 explicitly state that the LIP volcanism accompanied crustal extension, citing its distribution along what appears to be a linear rift zone parallel to the western margin of the Mascarene Basin and its dating from the period immediately before seafloor spreading there. To be "misleading", there would need to be later, presumably geochemical, studies that somehow disprove any link to active extension. To my knowledge, there are no such studies, nor are there studies reporting observations from Madagascar or eastern India that would support or refute the comment's assertion that the "breakup didn't cause the magmatism".

Line 589: The date is applied based on the observations that fracture zones south of the curved basins have the same orientations as fracture zones in neighbouring seafloor south of the ~95 Ma bend, whilst those north of it parallel fracture zones that are younger than the bend.

Line 605: One modern example of such a ridge, cited in the manuscript, is the East Pacific Rise, which is only bathymetrically evident as a long-wavelength feature. Previous studies have not recognized the need for a Vasuki Plate, and therefore did not require an abandoned Vasuki/India ridge in the western Bay of Bengal basin.

Line 611: See an earlier response. I am happy to defer to the copy editor on this issue.

Line 613: Removed “relatively precisely”. The numerical ranges are justified with citations in section 3.5.

Line 616: I am confident that these arguments, which concern likely uncertainties in the modelled motion of the Vasuki plate and not – as the comment would have – justifications for assuming its existence, are given in the right place. The argument for transform motion does not come primarily from the interpretation of transtensional basins but from the orientations of near-vertical faults along the Coromandel transform segment of the eastern Indian margin and of basement strike-slip faults at the eastern margin and in the basement of the 85°E Ridge. Similarly, to reprise a previous response, it would be a mistake to assume it possible to estimate the full amount of Vasuki-India motion from observations of the transtensional basins. The source of the estimated uncertainty of 100-200 km is clearly described in the same sentence as it is introduced. It is based on the typical widths of continental shelf gravity edge anomalies and their juxtaposition for purposes of reconstruction.

Line 622: I have modified the text to make clear that the 825 km value was measured using *GPlates*. The dates chosen are extensively justified, with multiple citations, in the slightly revised second part of the same paragraph.

Line 628: The suggestion to include this material in the section on “geology and evidence” was unnecessary because the citations on this line are already present in the section on the timing of Cauvery and Mannar Basin tectonics.

Line 637: Continuing the spreading for longer would merely see the same relative configuration of ridges endure for longer, requiring the Vasuki plate to move more slowly with respect to India between them. The revised version of the model and manuscript, which show the Vasuki plate’s motion continuing until 80 Ma, illustrate this.

Line 650: My understanding of the entrainment mechanism from my readings of the two cited papers is by erosion, for example by delamination, of the base of the crust during localized convection or Rayleigh-Taylor instability in the uppermost plume mantle spreading out beneath the microcontinent. As the paragraph explains, it was advanced to explain geochemical evidence for continental contamination of some of the Kerguelen Plateau basalts. As my interpretation of the Kerguelen microcontinent is based on consideration of these and other results within the context of, rather than for deriving *a priori* constraints on, the new reconstructions of the eastern path, I remain confident that this material is correctly placed here.

Line 656: The uploaded version certainly started out with a figure 9 embedded. The figure also survived being downloaded again to me following the first review round.

Line 666: If the displacement were only 100-150 km, then Sri Lanka would overlap intolerably with the Napier Peninsula in East Antarctica, increasing the area of underlap that it otherwise partially occupied. The widened underlap would require more, not less, continental material to be interpreted beneath the Kerguelen Plateau to fill it.

Line 667: See a previous response, to the comment on manuscript line 526.

Line 668: It is quantified using Tuck-Martin et al's formal uncertainty ellipses around the Euler rotation poles.

Line 677: Here, again, the graben plays no leading role in generating the main features of the model, but is instead interpreted within the model context. Hence, I disagree that it is necessary to have introduced the graben any earlier than in this section.

Line 680: The desired references are included in the key to figure 9.

Line 684: The numbers are clearly described in the existing text and there should be no need for confusion. Summarizing, the N-S train of transtensional basins in the floors of the Cauvery and Mannar basins is 790 km long (with apologies again for the typographic error of "700 km" in the original manuscript). The basins' formation can be related to movement of the Vasuki plate with respect to India, but the length of the train and the basins' cumulative widening is unlikely to record all of this movement because of the likelihood of slip along the bounding strike-slip faults and the strike-slip faults to the north of the basins, and extension in the Krishna-Godavari basin. The magnitude of the Vasuki/India vector in the *GPlates* model is 825 km, quantified after fitting Sri Lanka into a gap in the India/East Antarctica reconstruction, and its azimuth is near north-south, observed in strike-slip fault orientations at the East Indian margin and 85°E Ridge. The difference between the placements of Sri Lanka in most older reconstructions (which use 'scissor rift' motion with respect to India across the Mannar Basin) and in the new one (involving Vasuki motion) is about 1000 km. The 1000 km value is therefore not an estimate of the magnitude of the Vasuki plate motion vector but instead an estimate of its sum together with reconstructed closure of the Mannar Basin.

Line 690: Done.

Line 691: I would agree with this comment if the statement I made at line 691 had been one to the effect that the new reconstruction produces a definitive map of this part of Kuunga. Such a statement would indeed require extensive justification and a lengthy review of the tectonic and metamorphic evidence that have been presented for various segments of the orogen. However, the statement at this line was intended to convey that the new reconstruction permits reconstruction of a continuous candidate Kuunga in the region, which is close to the meaning that the reviewer suggests I should adopt. I have slightly altered the text to make this clearer.

Line 693: The statement in the manuscript at line 693 was about azimuthal resolution. The scissor rift interpretations in Figure 2b, despite their range and variety, all achieve closure of the Mannar Basin by some criterion, but do so along azimuths that vary between approximately east-west and southeast-northwest, that is, by about 45°. In contrast, the motion of the Vasuki plate is constrained to within 15° of azimuth by the strike of the Coromandel transform margin segment, and to within 5° by the basement valleys over strike-slip faults along the 85°E Ridge.

Line 714: Again, this is an interpretation that becomes possible because of the new reconstruction, and not a starting constraint on building it, and I am confident that the observations it starts from are presented in the appropriate segment of the manuscript.

B5) Discussion and conclusions

B5.1) Reviewer's comments

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

The *Discussion* is **focused on the objectives** of the study — [YES] / [NO]

The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES] / [NO]

The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [YES] / [NO]

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

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

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

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

The *Discussion* section is **organised effectively** — [YES] / [NO]

The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES] / [NO]

The *Conclusions* are **supported by the data** in *Results* and **follow logically** from the *Discussion* — [YES] / [NO]

The *Conclusions* are **clear and concise** — [YES] / [NO]

Comments:

l. 724: Is this really what you did? Or did you perform a thought experiment to see how you could reconcile the eastern chain with the western one assuming the western one is accurate and well-dated (and see earlier remarks on the Somali basins and ophiolites analyzed in Gaina et al 2015). I'm asking because the argumentation you present is not conclusive in its own right. The evidence for microcontinents is not unequivocal. The strike-slip of Sri Lanka along the margin is not rock-solid. The ages are not well-defined. They are perhaps permissible within the uncertainty of the data, but they are not high-resolution novel constraints I think. I enjoy the thought experiment – after all, plate tectonics is simple and options are limited, but the reconstruction reads as that: a thought experiment. If it was that, then it may be better to present it as such, and 80% of my questions above are solved.

l. 725: What plate motions within India or Madagascar are you referring to? I don't know of any significant motions that were proposed, just a few minor rifts?

l. 726: I think you'll be more convincing to the reader if you leave the judgement of that to them. Just go on and discuss it.

l. 735: You have nowhere described Tibet or its ophiolites, so I'd simply avoid mentioning anything about Greater India. The reconstruction of western Australia necessarily involved Argoland, which is now forming parts of Myanmar, Java, and Borneo, and it conflicts with the large polygon you indicate in your reconstruction. But as this is irrelevant for the new reconstruction in this paper, probably best to leave this out of the paper. Or include it in the review and explain your choices.

L. 731-741: This is part of the description of the reconstruction in the previous section, better to get it out of the discussion.

l. 748: But there are plenty of plumes away from divergent plate boundaries and plenty of divergent plate boundaries far from plumes. Some plumes kickstart ridges, and if they do, they're in each other's proximity, but there's no rule requiring that, is there?

l. 754: That orogen was half a billion years old at the time of rifting, is there any evidence it was still high? I find it hard to judge the validity of the correlations to the Kuunga orogen in this paper, since the orogen is nowhere described in any detail.

l. 756: Why do you need to infer excess gravitational potential energy of an old orogeny as driver for extension here? Isn't the presence of weak zones enough to explain the location of break-up?

l. 759: I don't think you have any observations to make inferences of the plate-scale stress field. It is also not clear to me where you want to go with this, none of these inferences follow from your new reconstruction.

L 762-772: I don't think that the statements you make in this section are supported by the evidence you presented in this paper, for the following reasons:

1) The ‘success’ of your new model is basically that you argue that existing evidence in the eastern Indian ocean may be reconciled with the assumption that India never moved relative to Africa pre-95 Ma. The evidence you show, however, nowhere precludes that it did, you provide no uncertainties, not in age, nor in position, for pre-95 positions or motions.
2) The 95 Ma age is nowhere explained in any detail, nor is an uncertainty range indicated
3) There are records of oceanic spreading between India and Asia in the Cretaceous, summarized in Gaina et al 2015, that you don’t mention nor alternatively explain
4) The records of convergence between India and Kabul, and in Oman and farther west, demonstrate E-W convergence. Your model does not explain this convergence, you simply state that it doesn’t exist in your model. But the evidence, which you nowhere mention nor explain in an alternative way, is still there.
5) You refer to a ‘large-scale rotation’ of the Indian Plate. Our reconstruction infers a 1-2° rotation in a time window of 10 Ma. That is not a large-scale rotation, and I really do not see how your new reconstruction, which is nowhere quantitative, is able to exclude that.

So basically, your new model assumes that there are no motions between India and Africa, then reinterprets existing information in the eastern Indian ocean – without actually demonstrating that that interpretation is unique – to argue that your assumption cannot be excluded, and then you use your assumption to exclude a previous model without explaining the data that underpinned that previous model.

L. 773-774: I was surprised by the Perez-Diaz paper, and I wondered whether you guys read our arguments in van Hinsbergen et al 2011.
In our paper we introduced the concept of plume push based on the simple fact that when plumes arrive below plates, they have no choice but to spread out horizontally. This must cause tractions. We wondered whether those tractions could have been large enough to explain the acceleration of India based on the plate circuits as we understood them around 65 Ma, and, more importantly, whether the waning of plume push could explain the slow-down. That final bit was the main rationale, since I was wondering whether we’d need to invoke the India-Asia collision for that slow-down.
We put two numerical models to the max, added two plumes in a row, to maximize the plume push effect, and found that it is Unlikely that plumes would be able to accelerate plates by more than a few cm/a, probably less. So, we concluded that the the vast majority of the acceleration was NOT the result of plume push, but that lubrication of the base of the plate for instance could play a key role.

Lucia in her paper argued that the acceleration of India at 65 Ma was much smaller than we estimated in our paper. That then makes plume push a MORE likely driver of that acceleration. Not a less likely driver. And because magnetic anomalies played no role in the argumentation of the existence of plume push, not finding evidence in magnetic anomalies can never be an argument against that existence. It can only mean that you are not able to measure the effects with magnatic anomalies, which we never claimed you should.

As with this paper, I don't understand why you have such a big issue with plumes being able to affect plates. They have been known for decades to break up continents, they cause km-scale dynamic topographic uplift in regions of 100s to 1000s of kms wide, why would it be so surprising if they cause a 1-2° plate rotation, or an acceleration of a few cm/a? In any case, I don't see how your reconstruction in this paper is able to exclude our conclusions.

l. 780-782: "careful consideration and prioritization of the resolving powers of available constraints on past plate motions in the eastern Enderby and western Bay of Bengal basins".

You have basically reinterpreted a possible fault with a few restraining bends as a strike-slip fault with 70, 825, or 1000 km displacement for which I do not see the smoking gun, and for the rest, I don't think you have demonstrated that your new interpretations are in any way unique or unequivocal. Certainly not up to a point that you can exclude some India-Madagascar motion that is, as it stands, required by the geology of the west Indian ocean that you don't discuss.

B5.2) Author's responses

Line 724: After speculating on what drove me to carry out the study, this comment goes on to repeat a number of its precursor comments. There is no need to repeat my responses here.

Line 725: This comment, about evidence for plate motions between and within India and Madagascar, is based on a mistaken understanding of the sentence at line 725. That sentence does not refer to plate motions, but to artefacts that have appeared in previous models.

Line 726: Agreed, this was a clumsy way to introduce the section. I have altered the sentence.

Line 735: The comment suggests to remove both Argoland and Greater India from the model and its interpretations because there is no use of constraints from their remnants in the Tethyan collision zone. I disagree, as we have already established (i) the very low resolution of reconstructions built using observations from collision zones, (ii) the superior resolution of reconstructions built using seafloor spreading constraints, and (iii) the availability of such constraints from western Australia.

Line 731-741: Agreed.

Line 748: I'm not sure what this comment is intended to achieve or what action it should prompt from me. As it stands, I don't think the sentence, or the reference it cites and deals with, presents or understands a rule for all plumes. Instead, Whittaker et al's paper deals with plume/ridge interactions and so, to my mind, should really only be understood for plumes that rise beneath or migrate into the vicinity of active ridges. As the reviewer hasn't singled out what it was about it that led him to understand something different than I intended, and as the second reviewer made no corresponding comment on it, I have decided for now not to change the essence of line 748.

Line 754: The sentence refers not to high topography, but thick crust. In the absence of a detailed review of the relatively large literature on Kuunga, which would not be possible to give for space reasons, it should be clear from a comparison of the three Kuunga paths (reproduced from a widely cited review paper) in Figure 2a and the more detailed-scale reconstruction in Figure 10, that the new model's Kuunga can be considered at least as plausible.

Line 756: A gravitationally unstable region of thick crust *is* a weak zone.

Line 759: With knowledge of the distribution and senses of a plate's boundaries, where large body stresses are generated by evolving density contrasts related to lithospheric cooling and subduction, and the expectation of internal rigidity, it should indeed be possible to generate a first order idea of its internal stress distribution. Hence, it is an exaggeration to state that there are no observations to support the statement about the stress field under comment here. That said, the first-order distribution will undoubtedly be locally altered by gravitational contrasts related to variable lithospheric and crustal thicknesses (including the Kuunga orogen), and by loads and tractions applied from above and below (examples of both of which might reasonably be attributable to the Kerguelen plume), and the resultant of all these stresses is a force that will be responsible for maintaining or changing the plate's motion. This level of analysis would necessitate a new research project and a separate manuscript, so I have decided to remove the sentence at the end of section 6.1.

Line 762-772: The bulk of this comment repeats observations and objections that the reviewer brought up and repeated over several of the previous comments. I will not repeat my replies to those parts here except to note that I have rewritten the section under comment so that the revised manuscript can reflect the debate in this form.

Line 773-774: Much of this comment is used to discuss the paper by Pérez Díaz et al (2020), in which we examined multiple seafloor spreading models to examine whether the ~65 Ma acceleration of the Indian plate, which multiple studies have attributed to plume-related forces, is real or artefactual. The comment refers extensively to one of the reviewer's previous papers (in 2011), whose title makes clear it to be predicated on "Acceleration and deceleration of India-Asia convergence…". True to its title, the 2011 paper's abstract describes accelerations as occurring rapidly at 90 Ma and 65-50 Ma, and the deceleration to have followed the 65-50 Ma acceleration, occurring more slowly. Digging in deeper, the body of the 2011 paper goes on to make clear that these signals are all derived by addition of rotations in plate circuits, citing around a dozen papers that present those rotations. All of those papers are based on seafloor spreading data or, to use the comment's terminology, "magnetic anomalies". In the face of this, I found it quite perplexing to read the comment's suggestion that "not finding evidence [for plume push] in magnetic anomalies can never be an argument against [its] existence", when it was precisely this source of evidence that led the reviewer to assume the existence, and model the relative importance, of plume influences in the 2011 paper. Now, in Pérez-Díaz et al (2020), we produced and/or examined more detailed seafloor spreading models for many of the same divergent systems that the 2011 analysis covered, and concluded that the 65-50 Ma acceleration is largely or completely artefactual. The comment also directly contradicts its own perplexing take on seafloor spreading models (*i.e.* that they have nothing to say about plume effects on plate motion) by proposing that the lack of acceleration signals in the rates presented in such models by the 2021 paper somehow strengthens the reviewer's previous findings about plume push. Quite apart from the doublethink it requires its readers to practise about the usefulness of magnetic anomalies, I find this latter proposal baffling. Without a plate kinematic signal, what need is there to assume a plume push force, or forces, with magnitudes significant for plate kinematics, to apply at all?

Returning to the manuscript for *Tektonika*, part of the discussion examines the idea of an alternative proposed plume push signal, this time an azimuthal one in the form of 1-2° rotation of India relative to Africa at 105-96 Ma, as in PP2021. It shows that such a rotation does not enable regional models to reproduce the azimuthal records of Indian-Antarctic plate motions, nor to reconcile them with contemporary African-Antarctic motions. Furthermore, it highlights that such a rotation's inclusion instead requires one to accept that at least 300,000 km^2 of planetary surface area was either created and/or extensionally modified between Madagascar and India at 105-96 Ma without leaving a single geologically or geophysically observable trace of its existence.

None of this means I have a big problem with plumes exerting forces on the lithospheric plates they interact with. For many of these forces there are unavoidable physical considerations that mean they must, and strong observational suggestions that they do. The models in Pérez-Díaz et al (2021) and in the manuscript under review here have all been used to contribute high resolution observation-based quantifications or tests of what kinematic effects these forces might have had, or might have been possible. From that work, I am confident to retain conclusions consistent with the ideas that, whilst plume arrivals should indeed apply basal tractions and disturb gravitational body force balances on plates, and may indeed reduce drag at their bases by eroding asperities and/or lubricating them with low viscosity rocks, the combined effects of these changes on plate speed appear to be small enough to be somewhere between difficult and impossible to quantify against background noise, and that a plausible example of unequivocal effects on plate motion azimuth remains still to be identified.

Line 780-782: This comment repeats observations and objections brought up and repeated over several of the previous comments. There is no need to repeat my replies to those comments here.

Acceptance letter

Dear Dr. Eagles,

We would like to thank you once again for your efforts in revising your manuscript that has been submitted to TEKTONIKA. Given the considerable changes to some parts of the text and the large number of reviewer comments you have addressed, we had decided to send the manuscript out for a second round of reviews. Both reviewers from the first round provided some feedback. The first responded informally via email that although there were disagreements about elements of the manuscript, they felt it was worth publishing and allowing the community to continue the discussion in light of the work. The second reviewer was satisfied that their comments on the earlier text had been adequately addressed in the revision and had no further comments in a second review.

Given the reviewers' comments we are pleased to accept your manuscript for publication in TEKTONIKA. Our editorial staff will be in contact with you regarding the next steps.Thank you again for submitting your work to TEKTONIKA.

David Whipp, Associate Editor

Craig Magee, Executive Editor

Reviews:

Valentina Espinoza

Thank you for the opportunity to review this paper, I am looking forward to its publication. Please let me know if providing all my comments in the form is all right, or was I expected to write a more extensive paragraph above. As recommendation I selected "Revision Required" since to me it best resembled the spirit of "Return to author for minor revisions", hope that is the case.

Douwe van Hinsbergen

Hey Craig,

I am completely swamped this week, and I’m gonna take a break over Christmas, so I’d rather not re-review.

The way I see it, the authors have gotten my advice, and what they do with that is their choice. I'm sure they've accommodated some, disagreed elsewhere (there's a few issues Graham and I will not agree upon I think), and that's fine with me. Just publish it and let the community sort it out!

Cheers,

Douwe