Review Report

Dauteuil et al. - Present-day Deformation in the Incipient Strike-slip Basin of the Okavango Delta (Botswana): Impact on the Ecosystem, TEKTONIKA, 2024.

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1st Round of Revisions

Decision letter

13/06/2024

Dear Dr. Olivier Dauteuil and coauthors,

Your manuscript submitted to the Tektonika has been reviewed by two referees. The reviewers agree that this contribution is important in understanding rift system evolution and its association with drainage system in the Okavango Graben. We agree that it is an interesting piece of work that shows the control of rift propagation at the southern end of the East Africa Rift on the delta system migration. Yet while the reviewers' reports were generally favorable, areas of concern were found. The reviewers' concerns should be addressed before your manuscript can be accepted for publication in the journal. In particular, we ask you pay attention to the description of hydraulic cycle correction of geodetic data and additional support of GNSS data from the Okavango delta. Additionally, the writing and figures require some improvement to improve clarity.

Sincerely yours,

Hongdan Deng, Associate Editor

Craig Magee, Executive Editor

Comments by Reviewer A

(Daniele Maestrelli)

[Comments taken from Tektonika review form]

This paper aims to illustrate the structural control of regional tectonics on the dynamics of the Okavango drainage system. The Authors make use of GNSS data obtained from 7 stations installed in the area to calculate 3D displacement and produce a coherent model for surface deformation to be compared with other data (e.g., swath profiles, flood maps). In the Author's view, the tectonic uplift has controlled the asymmetric sediment deposition toward the NE of the Okavango rift, influencing through time areas subject to flooding. Likely, envisaging continuation of this tectonic control and consequent shift of the system toward NE will induce the Okavango river to be captured by the Selinda river, transforming an endoreic system into an exoreic one, with possible consequences for environment and human life and business.

The hypothesis proposed by the Authors is plausible and supported by data. As far as I can understand, the quality of the produced data is good and the interpretation brings the Authors to provide and extremely interesting evolution for the Okavango area, with important implication for the environment. To my understanding, the work performed by the Authors has no flows and the impact of this manuscript is high. It was a pleasure to read it and I believe it represents an interesting contribution both for the journal and for the research community working on tectonics and environment (*sl*). I strongly encourage acceptance of this manuscript.

The manuscript is very well written and easy to read. The figures are extremely well prepared, useful and clear. For both text and figures I only suggest <u>very minor changes</u> that can be find in the annotated version of the manuscript.

The merit of this manuscript is to highlight the strong (and often disregarded) interaction between tectonics and environment. The Authors are able to demonstrate in a concise and effective way how regional tectonics may shape the future of a key ecosystem such as the Okavango Delta.

I have not so much to suggest. There are only a few parts of the text that requires checking and a few minor adjustments to figures. See annotated manuscript.

Maybe some info could be provided for the realization of the two SWATH profile. It is a well-known approach, but a few words may be appropriate.

Comments by Reviewer B

(Yanyan Wang)

The authors presented a geodetic study of the Okavango Delta system in Botswana and characterized the spatial and temporal patterns of ground motion within the period of 2010-2023 from GNSS data. Their geodetic results reveal that the lateral motion is consistent with a right-lateral displacement of the transtensive system of the region. The vertical displacement of their results demonstrated the modern subsidence center of the delta system is close to the apex of the modern fan. The authors further investigated the river course which intermittently connects the active channels of the Okavango delta and the Linyanti delta during wet seasons. They argued that the tectonic uplift (subsidence) pattern in the Okavango delta system will help to build a permanent connection between the two fans, and therefore the fully integration of the Okavango drainage system into the river network that flows to the Indian Ocean.

In general, I think the scientific ideas explored are interesting and the manuscript is acceptable for publication if the following scientific questions can be illustrated better in the manuscript, and provided that the English should be polished. Also, given the current scientific coverage in the manuscript, I think the title of the manuscript oversells the idea of impact on the ecosystem.

First of all, the hydraulic cycle correction of the geodetic data is under described. The authors mentioned the correction but I feel this correction needs more words to present the correction workflow and key technical details. It is worthwhile to put it at least in a supplemental file to document the methods and the observational data of the 8 stations. By providing these details, the argument that the observed ground motion is tectonicorigin will be better supported in section 5.1.

The second problem is the inference of river connection through surface tilting. I have no problem in agreeing with the observed tilting from differential subsidence in space from the GNSS data of the Okavango delta. To extrapolated the spatial pattern, it needs some GNSS stations or geodetic observations in the Linyanti delta area such that the connection between the two deltas through the Selinda rive can be studied with better confidence. The surface is sloping at such low gradient in that a subtle variation in space can re-direct the water flow path. I think the connection idea of this manuscript needs to be strengthened with additional geodetic observation data in the Linyanti delta area, or with a regional geodetic (vertical surface motion at least) pattern of the broader geographic area. If the regional tilting pattern can be confirmed with additional data, I think the authors need to expand the last paragraph of section 5.2 to be an independent section 5.3 discussing how the river profile should respond to the tilting vertical tectonic rates. A 1-D simple river profile modeling will help to demonstrate the argued knickpoint migration (kp1) and the resultant permanent connection.2

Lastly, many minor problems in the figure captions and the main text should be fixed. The language needs to be polished especially for grammar. Please see the annotated pdf files of the manuscript and the figure files for details. Please note that I didn’t point out all the grammar-problematic places. The authors need to read carefully themselves to polish the whole manuscript.

Response to Reviewer A

Abstract: we considered the suggestions of the reviewer. Some sentences were rewritten, as suggested (lines 27,28, 29)

Introduction

Line 59. The GRACE data are available on the following website: https://grace.jpl.nasa.gov/. This site provides a tool that generates time series of water equivalent thickness that was used the compare the ground deformation to the annual precipitation cycle. The comparison is explained in Dauteuil et al. (2023). A reference was added.
Line 65: correction done.
Line 170: We added a table with the coordinates of the stations and a short description of their location (table 1). We updated the table numbers. The sentence was modified.
Line 282: we added the fault names on the figure 8, as suggested.

Response to Reviewer B

We thank the reviewer for the pertinent comments and suggestions he did. They largely improve the manuscript. We integrated the English mistakes and the English of the manuscript was checked by an official traductor.

Reply of the general comments in the first part of his review. .
third section of the general comments. The hydraulic cycle corrections are quite easy to do because they affected the whole study area as described by Dauteuil et al. (2023). The huge rainfall in Angola generated an increase of the mass loading because of the soil made of sands poorly consolidated that store water. This increase of mass generates a flexure of the ground recorded over a wide region. To remove this regional effect, we estimated the displacement relative to the station MAUA that submits the rainfall loading as all the semi-permanent stations of the network, and at the same time. The displacements of the MAUA station were adjusted to 0 every day, and relative positions of the others were estimated by a difference between MAUA and the stations.
This section was partially rewritten by longer explaining how we removed the regional effect.

In the fourth section, the reviewer suggested to complete the GNSS network with sites around the Linyanti Delta and along the Selinda stream. These new sites increase the survey cover and will reinforce this study. We completely agree with this suggestion that largely complete the deformation study. We applied several times a proposal to monitor this area with GNSS to National French Agency (ANR) without success, unfortunately. This area is difficult to access (2 or 3 days on bad tracks, crossing two countries) and there are few safe places to leave receivers during some days, especially along Selinda stream. Thus, it costs money that we don't have.

We agree with the reviewer about the split of the section 5.2. We propose the following new organization: 5.2. Ground deformation at the scale of the graben, and 5.3 5.3. Impact on the future dynamics of the ecosystem: toward exorheism?

The reviewer proposes to demonstrate the migration of the knickpoint from river profile modeling, such published by Dietricht and Seidl (1994), by Chatanantavet and Parker (2009) or by Giachetta abd Willett (2018), for instance. These numerical simulations need to integrate some physical parameters such as the water fluxes, the grain sizes, the erodibility of bedrock, the vertical displacements, the amount of sediment availability, and an accurate topographic profile (because of the flatness of the area). But we don't know them and it is difficult to get them on field because of the complicated access. We could do the parametric modeling but the large number of unknown parameters will not allow us to settle this important question. Furthermore, this is another study topics.

Reply of the comments annotated inside the PDF file.

Line 40: references were added.
Line 53,54: the sentence was modified.
Lines 148-149: the sentence was changed and a large part of this section.
Line 157: “Strain field” section. The conversion of displacement vector into strain field is classical with GNSS studies. A lot of works estimates the strain from the displacements of nodes belonging to a network. The theoretical method was established by Means in 1976. We slightly completed the introduction of this section that was modified and we added some references.
Line 172: effectively MAUA station slightly varies both in strike and in rate. This is due to its location on a block limiting by two faults, and more precisely close to the one of these faults. We chose it because it located on the border of graben in a safe place and it has a longer time series. The record of other semi-permanent stations is not simultaneous because of the number of the available receivers at the same time and because of the distance that separates them needs one or two days to install them. We need a unique reference for all the station. The choice of the reference obviously changes the analysis of the relative displacements, and the interpretation should be done considering the given reference. In our study, we would like to analysis the displacements and the deformation in the inner graben. MAUA station is the most appropriate station.
Line 173: The work of the Saria et al determined the displacement in an absolute frame.
Line 181: sorry it’s a typewriting error. The displacement is toward NE. We corrected it.
Line 186: correction done: we refer now to figure 4.
Line 197: (section Decadal section). The analyzed period is 2010-2023 as indicated in the section 3. We have recalled the period studied in the text.
Line 210-211: we agree with this remark. That's why we don't take a positive view of this conclusion. More stations would obviously be necessary. But there are problems to access to this zone and the equipment will not be safe due to wildlife. We tried INSAR data, but this did not give correct results due to vegetation and flooding, which induce a lot of noise in displacement images (Gaudaré et al., submitted).
Line 220: as written in the text, the given value is the fault scarp, and not the throw. The throw is impossible to estimate because geophysical data and boreholes reaching the basement are not available.
Line 231: the shortening previously described concerns the inner part of the graben, and not the southeastern border. This border of the graben is clear submitted to

extension with a set of normal faults forming blocks. The inner part of the graben submits another type of deformation.
Line 235: the labelling of the figure was changed.
Line 244: the sentence was rewritten to be clearer.
Line 263-264: We rewrote the first sentence, and complete the section with a discussion about the apparent contradiction between the subsidence distribution and the sediment thickness, as suggested by the reviewer. We propose that there is a very recent change in the subsidence distribution, with a shift toward the NW. This interpretation is compatible with 2 observations: i) the domain at the mouth of the panhandle is always inundated whatever the flood intensity, and ii) there very few islands or emerged lands in this domain, while it is higher than in the SE part of the swamps.
Line 268: we rewrote this sentence.
Line 298: the paleolake Deception (this is its formal name) is described in the section 2 (Geological overview). We added the paleo-shoreline on the figure 2.
Lines 301-302: we completely agree with the reviewer. But in our study area, the basement of the alluvial fan is the last deposits of a paleolake that dried up. Therefore, we reasonably assumed that this surface was horizontal.
Line 306a: see reply of line 298.
Line 306b: the sentence was rewritten.
Lines 311-312: the sentence was effectively confusing. It was rewritten.
Lines 320: actually, no quantitative study of the connection is available, unfortunately. We started this study one year ago. Therefore, it is impossible to provide quantitative values, sorry.
Lines 323…: we corrected knickpoint.
Lines 323-325: the slope values were checked and corrected.
Line 327: we agree that the “hill” is not clearly visible. It corresponds to a decrease in the slope, and maybe to an inversion of the slope. But the accuracy of the DEM is not sufficient to better observe this key point. On field, this area is difficult to reach because it is very isolated with few tracks, that could confirm the hypothesis. We rewrote this sentence. We noticed this segment on the profile of figure 10.
Line 328-329: we disagree with the reviewer (YW40.1) when he wrote that the knickpoints are stationary. All the knickpoints migrates upstream at different rates depending on the nature of the knickpoint (lithology, fault…). See for instance the works of Loget et al (2009), Begin et al. (1980), Gallen et al (2011), Bressan et al (2014), Struth et al (2019) ...
Yes, we propose that the knickpoint kp1 is the last obstacle before a permanent connection between the two rivers.
Line 329 (YW41): obviously the river will adjust its profile after a tilting. A tilting will increase the slope of the river profile that increases the flow rate of the water and so the erosive power of the river. This will remove the knickpoints by increasing the water flow energy and sediment fluxes.

Reply about the figures
Figure 1: the color bar was added and the source of earthquakes indicated (USGS) in the caption.
Figure 2: the shoreline of paleolake deception added and the caption completed.
Figure 3: the fault mapping provided here come from this work.
Figure 5: the left y-axis legend was added.
Figure 6: the suggested corrections were considered in the caption.
Figure 7: a detail lithology is impossible to propose because no deep borehole is available. The interpretative geological section results from a compilation and a synthesis of geological map, geophysical surveys, and field observations.

Acceptance letter

We have reached a decision regarding your submission to тектоniка, "Present-day deformation in the incipient strike-slip basin of the Okavango Delta (Botswana): impact on the ecosystem ".

Our decision is to: Accept Submission

I would like to apologise for the delay in getting this decision to you - unfortunately you caught me on a period of parental leave and holiday, and when I was back, Hongdan was on leave and busy. The manuscript is now moving to copyediting. As you may have seen from Hongdan's reply to a discussion converstion on the Tektonika website, we have debated the accuracy of some phrasing elements in the manuscript. Some of the text is not necessarily how we would phrase things, but it is understandable and broadly correct. However, in addition to the copyediting, we do recommend re-reading the manuscript and checking the clarity of the writing again.

Kind regards,

Craig Magee (Executive Editor)

Hongdan Deng (Associate Editor)