Review Report

Messager et al. , Late Pleistocene Fault Activity and Slip Rates in the Malarguë Fold-Thrust-Belt Front (Southern Central Andes, Argentina). TEKTONIKA, 2024.

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1st Round of Revisions

Decision Letter

13 September 2023

Dear authors,

We have now received two detailed reviews of your manuscript submitted to Tektonika. Our associate editor, Jack Williams, and myself have made a careful reading of your manuscript and of those reviews. Together with the reviewers, we agree that your work will be a very interesting contribution worth to be published in Tektonika but that it needs significant revision before a decision can be reached. You'll find below the associate editor's letter followed by the two reviews. Reviewers have also uploaded documents that you may access by connecting to your Tektonika space.

We look forward to receiving a revised version of your work for a second stage evaluation. We hope you will be able to submit it before the end of October. This is a non-strict deadline and we will not close the evaluation process at the due date. But we recommend not delaying too much the revision process. If needed, please keep us informed about your timetable.

When uploading your revision provide us with a full rebuttal addressing all reviewer's comments as well as the points highlighted by the associate editor, and also a second manuscript version with all changes clearly outlined.
Best regards

Robin Lacassin, TEKTONIKA executive Editor

--- Associate Editor:

Dear Authors

Thank you for submitting this manuscript to Tektonika. We have now received two reviews for this work, with both reviewers agreeing that this represents a novel and interesting contribution to Tektonika, however, revisions will be required prior to acceptance.

Having read through the manuscript and the reviewer's comments myself, I agree with this assessment. In particular, I note both reviewers have concerns about the lack of footwall constraints in you kinematic modelling of the Sosneado Thrust, and so whether you obtain are true or minimum slip rates. I also endorse Reviewer 2's requests for restructuring the manuscript, clarifiy some inconsistencies with the seismic reflection line in Figure 3, and more geomorphic context for the fluvial terraces.

Reviewer 1 has asked for further clarification when comparing your results with previous seismic hazard assessment in this region and your use of fault-scaling relationships. I would add here: (1) you should be aware that there are significant uncertainities when trying to estimate earthquake magnitudes from single measurements of co-seismic displacement (see for example Biasi and Weldon 2006; https://doi.org/10.1785/0120040172), and (2) I was surprised at how low your magnitude estimate (M 5.2) was for rupture of a 40 km long reverse fault, I think your value of b in equation 4 should be 1.49 not 0.49 (Table 2a in W&C94), which leads to a more 'reasonable' sounding M 6.9. That said, the W&C 94 scaling is now nearly 30 years old, and you may want to consider newer fault scaling relationships that have been developed from much higher resolution and robust rupture data (e.g., Leonard 2010: https://doi.org/10.1785/0120090189, Thinbaijam et al 2017: https://doi.org/10.1785/0120170017).

Reviewer 2 has noted that a lot of the references are conference proceedings (so not necessarily peer reviewed or accessible). Our recommendation is to use such references only if they are easily accessible and if the data provided are enough to check result's validity (for ex. an age is useless if it's only a number in an abstract). Citing short conference abstract is in general not acceptable.

I look forward to receiving a new version of this manuscript that has considered these reviewer's comments, and thank you again for supporting Tektonika!

Kind regards
Jack Williams, TEKTONIKA associate editor

Comments by Reviewer 1

(Carlos Costa)

The manuscript submitted by Messager et al. is a worthy contribution that present novel data on Quaternary thrusting in the South Central Andes. Estimation of thrust kinematics is addressed through a sound methodology involving modeling of off-fault deformation markers with surface and subsurface data, and determination of new numerical ages. The other main goal of this manuscript is to assess the seismogenic capability of this thrust system.

Their findings are significant because has been conceived traditionally that Quaternary-active thrusting, or at least their expressions at the surface, drastically decreases south of 33° South. These new data can be integrated with previous information north and south of the area studied here, providing a new picture of the neotectonic activity of the Andean Frontal Deformation Zone at these latitudes.

In terms of its general structure, this manuscript requires moderate changes, but in my opinion, a major revision is necessary to the seismogenic assessment.

The main issues that in my opinion require to be revised or better explained are indicated below. Complementary comments can be also found in the annotated file attached.

- Regional tectonic setting

It is claimed in the Abstact and in the Introduction, that Seismic Hazard in the back-arc broken forelands has been widely underestimated. There are no bibliography references supporting this strong statement, and there are also no further comments on broken forelands in the manuscript. Readers may wonder for instance; how these kinds of tectonic settings are defined along the Andes, their spatial distribution and characteristics. And of course which is the broken foreland zone in the study area. A full explanation is deemed necessary, not only because the back arc and the broken-forelands settings seem to be the main target of the manuscript, but also because according to the classy papers, the typical broken-foreland area is restricted to the flat-slab segment between 27 and 33 deg South (see annotated comments).
Thus, a regional tectonic setting is missing, which would be particularly useful for readers not familiarized with the Andean geology. The Neogene tectonic evolution should also be addressed because significant episodes in the geometry of the subducted Nazca plate took place during the las 8-10Ma. And those changes are considered to exert a significant control in the nature and distribution of neotectonic deformation

- Quaternary Geology of the study area

In Fig. 2, Quaternary morphostratigraphic units related to alluvial fans (Qn) and terrace deposits (Tn) are distinguished. What is the basis for this classification? How Q and T units are distinguished from each other? Are they related to not-constrained and constrained alluvial plain deposits respectively? Why small patches of Q5 in the Atuel Valley correspond to alluvial fan deposits and not to Tn? How were they distinguished from terrace deposits? How they have been correlated in between the AB and ST thrusts? The numerical ages of Q5 and T4 are very close. It is a analytical issue or a correlation issue? Authors claim that T4 is embedded in Q5. Perhaps a detailed strip map with Q units along the river valley and transversal cross-sections could help to better illustrate this point. Thus, I suggest better explaining the mapping criteria and providing a more detailed figure of the Quaternary units distinguished along the Atuel River valley. Profiles sketched in figures 7 and 3 could be displayed in such complementary graphics. Because the fault modeling relies in the plan-altimetry of these Quaternary units, they deserve a better explanation and a more detailed map representation.

- Methods

I endorse the methodological approach, but in my view, authors should better develop the following points:

a. The cross-section construction departs from different data sources, namely DEM data for topographic profiles of the Atuel River valley, with reasonable accuracy for the scale involved, and seismic lines for the subsurface geometry of the causative faults. Usual oil industry lines lack of good reflectors detail for the uppermost 1s t. time, which is precisely the most relevant part for neotectonic structures. The interpretation exposed in Fig 3a does not clearly show the ramp geometry at the surface, as indicated in Fig. 3b. Could this result in a source of uncertainty for shortening estimation? Do other surface ramp angle alternatives should be considered? Propagating thrusts tend to flatten upwards. Are there field data of the fault surface to contrast with? These points may affect modeling results, so a proper explanation should be introduced or an uncertainty assessment should be conducted.

b. The MT and ST are blind or emergent structures? Fig. 16 suggests that the tip point is already above T4 and probably Q5 for the ST. Has this question been considered for fold modeling?

c. Fig 16 suggests that shortening estimation has been conducted considering solely T4 and Q5 outcrops above the ST hanging wall. Is this correct? If so, since it sounds like no T4 and Q5 correlatives could be found in the foot wall, the assessed shortening seems to correspond to a minimum value because a pinpoint in the foot wall is desirable, particularly if the tip point of the propagating thrust has been already eroded.

Perhaps authors have already solved these questions, but I could not find them explained in the manuscript.

- Seismic Hazard Assessment . *Magnitude Assessment through coseismic slip.* If the interpretation of coseismic surface ruptures (line 727) refers to faults A and B in Fig 10, it bears in my view some core problems:

The field photos lack of the necessary detail and resolution for readers to properly judge the key assumption of coseismic surface slip caused by seismic events. But if one follows the interpretative sketches, faults A and B rather look like blind propagating structures. There is no evidence of colluvial wedges or similar lithofacial architecture which could suggest so. Please review

Even if assuming surface rupture, it does not come out clear from the sketches if the faults cumulated slip derived from one or more events.

So, even if the author's assumption was correct, I think it is not rooted in the proper grounds, or at least not supported by the data provided in the manuscript.

If authors attempt to use MD empirical relationships to estimate paleo-earthquake magnitudes, I suggest providing good quality pictures of the fault exposures, along with detailed loggings (not sketches), so readers can properly judge these statements.

Magnitude assessment through rupture length. In order to use the rupture length for magnitude assessment, a segmentation analysis of the thrust trace should be conducted, which I suspect could be quite troublesome here. See for instance the classic works in this regards, i.e.
Schwartz&Sibson, 1989, USGS OFR 89-315
Machette, et al, 1992. The wasatch fault zone, USA. Ann. Tect. 6, 5–39.
Du Ross et al, 2016. Fault segmentation: new concepts from the wasatch fault zone, Utah, USA. J. Geophys. Res. Solid Earth 121, 1131–1157. https://doi.org/10.1002/2015JB012519.
Or the classic paleoseismology book by McCalpin.

Unless very shallow hypocenter, thrust ruptures related to a M5.2 seems unlikely.

In Section 7, authors discuss the magnitudes assessed (5.2; 6.5, >7) without further analysis, even if the range of involved magnitudes implies an important difference in the seismogenic potential. Is there a preferred value/range?

Authors state (lines 750-751) that "*The estimation of the magnitude* (refers to M 5.2) *is consistent with the macroseismic observations in the area that suffered from earthquakes of such magnitudes during the instrumental period (Figure 1).*" It is intended to say that instrumental seismicity represents or captures the maximum credible earthquake (MCE) and therefore the seismogenic potential of the ST??

In the Conclusions (lines 772-775), the same outcome is suggested. "…*a high probability of earthquakes of Mw between 5.2 and 6.5*…" violating basic principles of the probabilistic seismic hazard assessment, namely; how much in terms of percentage means "high probability"? high probability in which temporal window? 50yrs? 500yrs? 5.000 yrs? Without specifications, the unspecific concept of "high probability" sounds very vague and more importantly, potentially misleading for future users. Most importantly, these conclusions seem to contradict the most provocative hypothesis launched at the beginning of the manuscript; which might tempt readers to plunge into this reading, namely: "*The back-arc broken forelands …. poses a hazard that has been widely underestimated*".

If maximum earthquake magnitudes estimated through geological data can be captured through the historical catalog, why the seismic hazard is being underestimated?

Also, the mean recurrence interval for MCE in structures with slip rate ~ 2 mm/a appears to be of several kyrs (see McCalpin 2009 or similar biblio), and thus with low chances of occurring during a temporal window <100 yrs….

It is possible that the manuscript intends to convey that the occurrence of earthquakes of magnitudes 5 and 6 during the historic record confirms the capability of the thrust system. But this is a completely different statement. What is important here, because is the ´promise´ of this submission, is the seismogenic capability derived from the geological data

General comment: This contribution bears very good data for shortening estimation and kinematics of thrusts during the Late Pleistocene. I suggest authors should be encouraged to consider the comments I have raised in this regard which could help to reach a more robust final version. But the shortcomings arising when trying to assess the seismic hazard are very evident to me; from the analysis of punctual field data used to derive MCE, to the general conclusion offered for this topic. I foresee a paved road produce a good-quality article through the structural modeling, but I fear a winding path to achieve sound results in assessing seismic hazard with these data and approaches.

Comments by Reviewer 2

(Martine Simoes)

I hereafter follow the guidelines proposed by Tektonika for the review process. In case not formatted correctly here, a PDF is provided hereafter.

Section A: Overview of manuscript

A1) Overall evaluation, general comments & summary
A1.1) Reviewer's comments

A1.1.1) General evaluation and publication suggestion – Required:
Please use this space to describe, in your own words, the core subject of the submission and your overall assessment of its suitability for publication.

The manuscript submitted by Messager et al. to Tektonika presents new work on the structure and Quaternary deformation of active faults at the front of the Malargue thrust belt (Argentina). The authors use an interesting approach, from structural geology and interpretation of seismic profiles to derive the geometry of active structures, to geomorphology to discuss the morphological record of recent deformation. The final discussion exploits the acquired results to quantify fault shortening and slip rates, and to evaluate the seismic potential of the investigated faults.
I appreciated much reading the manuscript, in particular the combination of these various approaches to better assess the seismic potential of this region. I look forward to see this work published, however, after some revisions; even though not major, these suggested corrections are quite substantial as the manuscript is long and uses a variety of approaches. Hereafter I further detail my various remarkes, hoping that will be useful to the authors to improve the manuscript and strengthen the presented results.

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 appreciated much of the work presented in this manuscript, and my suggestions are mostly to improve the readability of the manuscript. I have a specific correction to suggest as for the calculation of shortening/slip rates from terrace patterns. In details:
* Rewriting: I am not a native English-speaking person, but I have the feeling that some corrections for English are sometimes needed and may impede at places a detailed understanding of the ideas and reasoning. Even though I do not think that this a major point, if the authors have in hand native English-speaking colleagues that could give a final reading to the manuscript, that may be helpful.
* Reorganising: I felt sometimes lost with the overall organisation of the manuscript, and if found appropriate to the authors, I have the following suggestions for re-organisation:
1 Introduction
1 Geological Setting (and not as section 3– as such by further presenting the context, after the introduction, the authors can better expose what is known, what is unknown and

therefore further present and justify the scientific motivation of their work. This would go together with Figures 1 and the geological map of Figure 2)

3 Methods and Data (and not as section 2)
4 Structure of the Sosneado Thrust
5 Atuel River morphology
1 Quaternary tectonic activity (and not as section 7, as it is a good follow up to the previous section)
2 Cosmogenic ages (and not as section 6)
8 Discussion
+ additional suggestions of re-organisations within some of these sections.

* More figures: For an easier reading of the color code, I would break the map of Figure 2 in 2 maps: a geological map (with underlying geology) and a geomorphological map (with the most recent alluvial deposits used in the morphotectonic investigation, ie Q5 and younger). The different yellow/orange/brownish colors, with or without hatching, from recent Cenozoic deposits in the geology to the terraces in the geomorphology are too hard to distinguish and read from the figure as it is now. Figure 7 could also be simplified as panels a and b are repetitive, but it could be complemented by a figure where the terrace profiles are not shown in terms of elevation, but in terms of incision (ie altitude with respect to the present-day river, along a longitudinal profile), and indicating the places where there is presently incision but also aggradation (ie mostly at the front of active frontal thrusts). The same for Figure 13. At the end, I missed a final graph where the derived incremental shortening / average fault slip is compared to ages for each terrace level, with uncertainties on each parameter, to better visualize the evolution of shortening/slip over time. Much more helpful than table 3 where rates are calculated for specific time intervals, without illustrating the possible tendency – in fact, the reader needs to generate this figure from the table to get this understanding.
* Reinterpretation: I have some suggestions here and there, but my main concern is in the calculation of incremental shortening or slip for each terrace above the Sosneado Thrust. As far as I understand how the calculations were done, I have the feeling that incision only is considered - ie the uplift of the terraces above the river, and therefore in a reference frame related to the river (or to an ad hoc paleo-river). However, in determining fault shortening or slip rates, we should be a structural reference frame, where the uplift of the hanging wall is calculated with respect to the footwall. In other words, a correction for river base level is missing, ie for sedimentation over the Sosneado thrust footwall (see for example Figure 7 in Lave & Avouac, 2000). If there is no way to estimate sedimentation at the river base level, then the rates proposed here are only minimum values. Also I would suggest commenting the river signal from incision profiles (indicating where aggradation is ongoing) and not only elevation profiles.

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:
I suggest additional explanations and some modifications to some figures as explained previously, but also hereafter in details.

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...
- Morpho-tectonic analysis of the recent deformation across active thrusts in a back-arc setting

- Interesting combination of structural geology, geomorphology and geochronology
- New data

The main points of improvement I have found are...
- English writing to verify if possible
- Reorganisation of some sections of the manuscript.
- additional information and explanations on some of the analyses, verify interpretations (details above, and hereafter)
- simplify some of the figures, eventually by splitting some of them in 2; add information on some other figures (terrace incision), add a final figure (shortening / slip vs. terrace age).
- citations: some of the cited work corresponds to congress abstracts, ie to simplified (non-peer-reviewed) interpretations with no access to the raw data that motivated these interpretations. Does Tektonika accept such type of citations ? I know it is common practice in the geoscience community in S. America, but I do not think this is to be encouraged and some journals now refuse..

<u>Section B: Detailed evaluation of manuscript</u>

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]
The *Title* describes the main topic of the manuscript **succinctly** — [YES]
The *Title* includes **appropriate key terms** — [YES]
The *Abstract* includes a **clear aim and rationale** — [YES]
The *Abstract* supports the rationale with **sufficient background information** — [YES]
The *Abstract* includes a **well-balanced description of the methods** — [YES]
The *Abstract* describes the **main results sufficiently and adequately** — [YES]
The *Abstract* clearly describes the **importance/impact of the study** — [YES]
The *Abstract* clearly states the **conclusions of the study** — [YES]
The *Abstract* is **clear** and **well structured** — [YES]

Comments:

Title fine, just a suggestion: complement the title with "Late Pleistocene <u>fault</u> activity and slip rates…", just to clarify that the main focus is on faults right from the title.
The abstract is also fine and balanced with respect to the main text. My comments relate rather to some of the conclusions and will be detailed later.

B2) Introduction

B2.1) Reviewer's comments
*These statements are a **guide** to what good* Introductions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*

The *Introduction* provides **sufficient background and context** for the study — [YES]
The *Introduction* describes the **aim/hypothesis/rationale** clearly, providing **sufficient context** — [YES]
The *objective/hypothesis/rationale* **flows logically from the background** information — [YES]
The *Introduction* describes the study's **objective and approach** (last paragraph) — [YES]
The *Introduction* contains **relevant, suitable citations** — [YES]

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

Comments:

Here I consider together sections 1 (Introduction) and 3 (geological context). Section 3 should be placed as a section 2 to provide sufficient geological background information, after the general introduction, and before getting into the details of this work (data, methods, results, etc). Additional minor comments in Section C of this review.

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 — [NO]
Necessary information about **data sources/acquisition/processing** is included — [NO]
Data used are accessible via either supplementary files or links in the data availability statement — [NO]
The *Dataset and/or Methods* are **organized effectively** — [NO]
Comments:
I suggest to slightly re-organize this section (here section 2, but suggested to move to a section 3), so as to mirror the subsequent presentation of results. As structural results are presented first (interpretation of seismic line), I would rather start this section with the presentation of the seismic line (3.1 structural seismic interpretation), followed by the various maps and topo data (3.2 morphotectonic analysis) and the chronological constraints (3.3 TCN dating).
My main concerns here in terms of data sources or processing are:
- seismic interpretation: there is little said about the seismic line used here. Is it accessible ? probably not, but mention it, or indicate how to access it, here or in the data availability statement as this information on the accessibility of the line is at the moment not provided. Did you get only the line-drawing shown on figure 3 ? or did you access the depth-interpreted line ? or the raw data (two-way travel time) and processed the depth conversion ? or in other words, what was your own processing of the data you accessed ? can we have information on the depth conversion of the line ? where are the wells (on the map of Figure 2 and along the line of figure 3) ?
- TCN dating: the authors used surface samples, and combined two cosmogenic nuclides. But why didn't they sample along depth-profiles to better constrain exposure ages and average inheritance ? my own (limited and modest) experience with surface samples is that they tend to have very scattered ages, in excess when compared to depth profiles…
Also, are 1-5 samples per surface sufficient to date the surface abandonment ?
- TCN dating: why not consider the classical 2sigma uncertainties, even though larger ?

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** — [NO]
The text in the *Result* section **cites tables and figures appropriately** — [YES]
The *Results* directly **relate to the study objectives** — [YES]
The *Results* present **data for all the approaches** described in the *Methods* section — [YES]

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

Comments:

Here my comments concern sections 4 (structural interpretation), 5 (morphology of Atuel river), 6 (dating) and 7 (Quaternary tectonic activity).

As mentioned earlier, I would suggest to re-organize slightly these results sections, with:

1 Structural interpretations
2 Morphology of the Atuel river
3 Quaternary activity (present section 7)
4 Dating (present section 6)

as the section on the evidence for Quaternary activity follows nicely that on the morphology of the Atuel river, and does not yet need the chronological results. As such the results would also nicely mirror the data & methods section.

More detailed appreciation for each one of these sections:

Structural interpretations (section 4).
My main concern is here on the interpretation of the seismic line, maybe too simplistic when compared to surface geology. Indeed, the region above the two most frontal interpreted fault segments (km 7 to 10 on figures 3 and 6a) does not match surface geology as intuited from Figure 2. Indeed on Figure 2, this part of the seismic line seems to cross 3 main thrusts (+ additional 1-3 minor thrusts) whereas this is not shown in the final interpretation. It should be noted that the line drawing of Figure 2 suggests that reflectors may be more deformed than interpreted here. The final structure here is expected therefore to be more complex, and the total shortening (1500m probably higher) when the balancing the section. By the way, what is the meaning of the shallow red line on figures 3 and 6, at 1 km asl ?

Morphology of the Atuel river (section 5)
I think that one very important information is missing on the nature of the surveyed terraces. Are these strath terraces, and if so how thick are the deposits ? are these fill terraces ? or cut-and-fill ? This information is important to understand where the present-day river is incising / aggrading (information missing on figures 7 and 13), whether it is incising into bedrock (with probable long-term uplift) or into its own deposits (if readjusting to climate or hydrological changes for instance). For instance, surface e (Q5) is described as erosional (lines 368-370), while the rest of Q5 is described to be 30 to 100 m thick (lines 367-368): the information on where the transition from incision/erosion to deposition/aggradation occurs and how it correlates (not not) spatially with faults is worth being mentioned. A field picture of a typical terrace profile (in section) could be useful.
I think the discussion on the remnants of Q6-Q7 useless (lines 387-388, Figure 7), as we are not sure to pick the top of the deposits (or else the same depositional horizon) all along the profile. In any case, you do not use these geomorphic/sedimentary markers in your subsequent analysis.
Line 389: why have you mapped intermediate levels for T2 and T3 ? you never use them in your analysis, and this results also in a complex geomorphological map (Figure 2).

Evidence for Quaternary tectonic activity (section 7, but suggested for section 6).
I suggest to start this section with the tectonic record of the terraces of the Atuel river (section 7.3, to be changed to a section 6.1) as it flows naturally after the morphological description of section 5 (in fact, these two sections could be merged ?), followed by the other two remaining sections.

For what concerns the section on the terraces of the Atuel river (7.3 to be moved to 6.1), I would suggest to quantify and discuss here the incision pattern – and not the sole elevation of the terraces above the present-river. Incision is a good (potential) indicator of terrace uplift in a river

reference frame, ie by taking the present-day river as a proxy for the geometry of the paleo-river (Lave & Avouac 2000) – provided that we're dealing here with strath terraces! This would avoid some confusion. For instance, at line 529 there is the mention of steeper terrace flanks on the eastern side of T4 above the AB thrust… whereas the flanks would be steeper (and shorter) to the west when considering incision, as the slope on the western side is reversed compared to the present river. The "deformation" pattern would be more direct – and correct - from incision profiles.
This said, I believe that there 3 main points of attention here, maybe not enough emphasized:
- overall the pattern of incision / aggradation is spatially correlated to the location of faults, indicating that active tectonics controls, to the first order, this pattern. An important conclusion, which is never really mentioned. Use the figures to illustrate this, by also indicating where you have active incision and where you have active deposition !
- there are two ~7 km wide "bulges" around some of the faults – you already mention it. However, I have a hard time understanding the first one above the AB thrust as the steeper and shorter flank to the west (when you imagine the related incision profile) would indicate deformation above a westward thrust (?). Also above the LC thrust the pattern drawn on figure 13b indicates that T4 is higher on the eastern (footwall) side of the thrust when compared to the western (hanging wall) side, in contradiction with my understanding of the field pictures of figures 11-12. Maybe there is the need to clarify things here.
- there is a "residual" gradient upstream for terraces T1-T4 that you do not comment much. Is this hydrological ? or related to large-scale (thick-skinned) folding upstream ? A (paleo)gradient of 4% for T4 is unlikely to be (fully) hydrological as these kind of values are found in small mountain-front rivers. So part of the signal is probably to be also exploited for tectonics, even just qualitatively.

Section 7.1 (to be moved to a section 6.2 or 6.3)
I do not understand well the point here. Indeed the field observations concern the deformation of older synorogenic deposits (Mid Miocene Agua de la Piedra formation ?), according to the location of these field observations on Figure 2. I am therefore not convinced that it is useful here to compare the deformation of such old deposits to the Quaternary deformation at the heart of this manuscript. Faults A and B described in Figure 10 are Mid-Miocene (as intuited from the geological map of Figure 2) or Quaternary (as suggested) ?

Dating (section 6 to be moved to a section 7)
I understand that too young Al ages for Q5, when compared to Be ages, could be an indicator that the Q5 Be age is wrong. I am however surprised that Be ages are well distributed and not more scattered, as they are in fact for T4.
Could we imagine that the Q5 age of 74 ka is fine, but that of T4 is wrong/too old ? indeed, T4 is entrenched within Q5 deposits, and some of the T4 pebbles used for dating could be remobilized pebbles from Q5 – and therefore show biased old ages ? depth-profiles would have been useful…

B5) Discussion and conclusions

B5.1) Reviewer's comments
*These statements are a **guide** to what good* Discussions *and* Conclusions *include. Please select YES or NO to the statements below if you wish and detail in the free form box below your reasons for any box checked with NO, or to comment on any other matter.*
The *Discussion* is **focused on the objectives** of the study — [YES]
The *Discussion* **addresses all major results** of this study, which are shown in *Results* — [YES]
The *Discussion* section makes **comparisons with other studies** that are relevant and informative — [YES]
The *Discussion* section properly identifies all **speculative statements** — [NO]
The *Discussion* section presents the **implications of the study** persuasively — [YES]
The *Discussion* section **highlights novel contributions** appropriately — [YES]
The *Discussion* section **addresses the limitations** of the study appropriately — [NO]
The *Discussion* section is **organised effectively** — [YES]
The *Conclusions* are **consistent** with and **summarise** the rest of the manuscript — [YES]

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

Comments:

The discussion section (section 8) could maybe be improved by further discussing the uncertainties and the limitations of the various proposed interpretations. In more details:

8.1 Chronology
- lines 569-571: can you further elaborate on this interpretation of a cover lost for 100s kyr ? what other regional observations support this ? how would an alluvial fan, finally interpreted to be 174 ka old, loose cover for several 100s of kyr ? I am confused…
- lines 584-586: why does T1 need to be younger than 13 ka ? Here also some more explanations are needed to follow completely the reasoning.

8.2 Morphostructural evolution of the mountain front
The text is overall a little bit confusing and may need some re-writing. I would suggest to further take support on Figure 15 for this.
As of Figure 15, which is the center of this discussion, I would suggest to keep at depth (along the sectional views) the same colors as the corresponding surface deposits, for an easier comparison of the different panels. Where growth strata (ie most of the time), these should be better highlighted on sectional views. Also from panels B to C, why/how does the “southern” part of the mountain front in the figure “lose” most of the previous deposits (Q7) before deposition of Q5-Q6, even though only the northern part (where Q7 is preserved) is subjected to higher structural uplift above faults ? How do you explain the (major) change from panel C to D, ie aggradation of the fan Q5 to incisional terraces T4 and subsequent ones ? Hydrological ? tectonic ?
I think more discussion is needed here.

8.3 Slip rate estimates
First and most importantly, you need to estimate structural uplift before quantifying shortening or fault slip. As already mentioned, the incision profiles are (in most cases, if no major hydrological changes) only minimal views of structural uplift because sedimentation at the base level of the river – and fault footwall – is not considered (Lave and Avouac 2000). So I recommend correcting for this first, as much as possible – or discussing the implications of this simplification if there is no proper way to quantify sedimentation rates.
Then the structural model to consider for the analysis may be revised if the final structural section is corrected (see previous comments). In any case the area balance approach (uplifted – and not incised – area vs. area displaced above the fault at the back) is independent of the structural model and should be robust for shortening estimates, as long as the decollement depth is fine – and the uplift (and not incision) pattern is used.
I have a hard time evaluating if the model fits well the data as I have the feeling that only model results are shown on figures 16 and 17, and that data are not represented. Could model and data be represented ? in particular for the reconstructions of Figure 17 ? for a better readability, maybe only show the appropriate terrace level for each panel of Figure 17 (and not all levels), playing with a variable vertical exaggeration above the river surface.
A graph showing derived incremental horizontal shortening (or average fault slip) and terrace age would be useful (much more than the 2nd column and the last 5 columns of table 3) to discuss rates over time.
Uncertainties on shortening/slip estimates are not discussed… so some of the variability in the rates over time may be an artefact if within result uncertainties.

8.4 seismic potential
Lines 722-736 may be simplified. First, because the reasoning is made on secondary faults, that may have been active by Mid-Miocene (see my previous comments). Second, because the authors even prefer the reasoning that follows.

For what follows by lines 740-747, it would be worth expliciting the values considered for the calculations, and their possible uncertainties. For instance, what rupture length is considered to estimate a RLD of 40 km ?
Lines 751-754: there is something that seems wrong to me. The amplitude of terrace folding depends on the rates of deformation and on the terrace ages (incremental deformation), not on the moment magnitudes of past earthquakes. The same terrace deformation may have been reached after a few large magnitude earthquakes, or several average magnitude earthquakes, or even by fault creep – as long as the total cumulated deformation is equal. So this is not the proper argument in favor (or not) of possible M 7 earthquakes

A suggestion: once you have shortening/slip rates and potential earthquake magnitudes (and therefore average coseismic slip by scaling laws), you may also determine the possible recurrence time of these earthquakes (on the investigated fault) and compare it to the existing catalog. As in the discussion of Simoes et al (2007) – that you already cite in this manuscript.

B6) Figures, tables and citations

B6.1) Reviewer's comments

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

Tables and *Figures* are **ordered logically** and **numbered sequentially** — [YES] / [NO]
Tables and *Figures* have **captions that explain** all their major features — [YES]
Tables and *Figures* have **captions that complement** the information in the main text — [YES]
Tables and *Figures* present data that **relate** to the study objective — [YES]
Tables and *Figures* present data that are **consistent** with and support the description of results — [YES] / [NO]
Tables and *Figures* have **succinct and informative titles** — [YES]
Figures are **accessible** (elements are clearly labelled, accessible colour palettes, colour contrasts, font size legible, etc.…) — [NO]
Figures with **maps or cross-sections** contain all **elements to be understood** (north arrow orientation, scale, visible coordinates, sufficient coordinate grid intercepts) — [YES]
Figures with **maps** have **sufficient location information** (in the map or caption) — [NO]
Cross-sections have clear labels for **scale and coordinates** at ends and within-section kinks — [NO]
Citations throughout are relevant, suitable, and comprehensive — [NO]

Comments:

Some comments, following the guidelines above:
- If the authors find my suggested re-organisation of the manuscript appropriate, the numbering of the figures will have to change at places. Beware of typos, in particular in figure/table captions.
- Tables and figures present data in support of presented results, but, as mentioned previously, I have the feeling that there should be additional points addressed (and therefore illustrated) when using terrace record to quantify Quaternary deformation: more specifically, terrace incision, together with where river aggradation is ongoing is expected to be informative and should also appear in figures.
- As of the color palettes, I believe that Figure 2 should be split in 2 for readility as some colors and symbols used to visually distinguish Cenozoic synorogenic deposits or terrace levels are hard to differentiate and read. Also, I do not get the meaning of the bizarre greenish color over the Malague Basin.
- I noticed that some elements presented in the text are not systematically well located on maps. Some examples:

The seismic line of Figure 3 is 12 km long, the section of Figure 6a is 17 km long, and they both are located over the same line and distance in the map of Figure 2 (where, given the scale, I even have the feeling that the drawn line is longer).
A well is mentioned in the text, only appears in the section of Figure 6 but is not shown and located on the map of Figure 2.
The cross-section shown in Figure 5 is not located on the maps of Figures 1 and 2.
The starting and ending points of the river longitudinal profile (Figure 7) is not located on maps.
- As of citations, I did not notice any specific missing citation, even though I am not particularly familiar with the investigated area. I just noticed a high number of cited abstracts or conference proceedings – ie non-peer reviewed and mostly unaccessible work and (raw) data. I know this is common practice within some communities, I do not know what are the rules for Tektonika, but I do not recommend using these studies as the work presented during a conference talk or poster is not accessible in details (in particular for what concerns raw data to be critically evaluated) and cannot be rigorously discussed.

Section C: Additional comments

C1) Minor/line-numbered comments
C1.1) Reviewer's comments

- lines 79-80: the folding of recent geomorphic markers does only indicate if a fault has been active recently, not that the fault is seismogenic. Indeed there is no indication on the slip mode (seismic vs creep) from landscape analysis… unfortunately!
- lines 88-89: the deformation did not originate from the terraces, but was recorded by the terraces.
- line 95: you determine slip rates, but discuss possible estimates of maximum displacement and moment magnitude.

- line 153: what is the bracketing numbering technique ?

- Lines 234: cumulated scarp, not to be confused with a seismic scarp since 500 m high.
- lines 245-247: a topographic profile across the maps of Figures 1 and 2 (and added to these figures) could be helpful to better accompany the text.
-lines 305-316: shouldn't this be moved to the section about the geological context ?

-lines 364-365: Diamante river is not shown on Figure 2

- lines 565-564: no, it implies mostly high incision rates between Q5 and T4, that may relate or not to intense deformation rates. In any case, there is a major change from aggradation (Q5 alluvial fan) to incision (T4 terrace).
- lines 589-590: homogenize the writing of interpreted ages: XX-XX (age interval) or XX +/- XX (statistical uncertainty); This is quite confusing.

- line 642: how do you estimate the geometry of paleo-rivers ? where are these paleo-rivers within the footwall of the fault and therefore what is the total throw across the faults ? for this last point, you need to estimate sedimentation at the river base level / fault footwall.

- lines 709-716: are all these faults structurally comparable to those studied here ? why would we expect (or not) similar rates ? wouldn't this paragraph better fit within the previous section ?

Figure 1: the greyish color for depot-centers is not easily readable. In any case in general I find the terminology "depot-center" very confusing. I understand it comes from the "basin geology" community, who considers the previous initial basins. But today these units are no more basins but rather thrust belts… so calling them "depot-centers" is really confusing for most readers. An alternative: "inverted depot-centers" to please everyone ? or just "thrust belt"…

Figure 2: What are the "unassigned alluvial deposits" ?

Sub-levels for T2, T3 and Middle Miocene formations are not distinguishable (see previous comments).
Here, you use “Principal Cordillera”, here and there you mention ‘Cordillera Principal” in the text.. homogenize the names and labels for a better readability.

Figure 3: add surface geology information above the seismic section, by using your geological map: name and color-code of the formations, possible dip angles measured in the field, thrust faults encountered at the surface in the field or on maps. What do the two red lines represent ?

Figure 5: other structural interpretations are mentioned in the text. Would it be worth illustrating here these other interpretations ? just to better visualize the structural uncertainties….

Figure 14: MPT and LGM explicited in the figure caption but they do not appear on the figure. The two first columns indicate the results of this study, but Gosse 1994 cited… quite confusing: this study ? or Gosse 1994 ?

C2) Other remarks
C2.1) Reviewer’s comments

Sorry for this very long review… I appreciated reading the paper, so my numerous questions reflect the fact that I found it interesting and wanted to go further in the discussion.
I therefore hope that my comments will be meaningful and helpful to complement and improve this nice work, and from there increase its impact.

M.Simoes

Authors’ Replies to Reviews

General comments to the revision

We deeply appreciate the high-quality review, which undoubtedly enhances the manuscript's overall quality. We have carefully considered and implemented all the suggested edits. Below, you will find detailed comments in response to Carlos Costa (Reviewer 1) and Martine Simoes (Reviewer 2)'s reviews.

The resulting manuscript is considerably modified from the first version. Here are some general comments raised by both reviewers. Follow detailed answers to the comments of each reviewer.

- **Refocus of the main goal of the paper**:

We fully acknowledge the insightful comments made by Reviewer #1 regarding the primary impact of our contribution, particularly in providing valuable data for estimating shortening and understanding the kinematics of thrusts during the Late Pleistocene. This is noteworthy because traditionally, it has been believed that Quaternary active thrusting, or at least its surface expressions, diminish significantly south of 33° South.

Both reviewers commend the authors for utilizing a multi-faceted approach that includes field observations, structural interpretation of a seismic line, new TCN ages, and analytical structural modeling to achieve their goals.

However, the reviewers and the editor also observed certain limitations in utilizing punctual field data to assess seismic hazards and derive MCE and scaling laws for earthquake magnitudes. As a result, they expressed concerns about the reliability of the assessing the seismic hazard obtained through these approaches and data sources.

Taking these views into consideration, we have decided to restructure the manuscript to emphasize the key findings of this study: the identification of active thrusts and the estimation of their slip rates. Our results indicate the presence of a potentially hazardous fault system in the Andean retro-arc foreland south of 33ºS. Accordingly, we have revised the abstract and introduction to address the larger regional issues of comparing seismic activity, observed active tectonics, and tectonic evolution in the zone of the Pampean flat-slab to our study area located at 35ºS. Furthermore, we have included a new section in the geological setting that discusses the regional tectonics.

Additionally, we have rewritten the last section of the discussion, removing all analytical results related to magnitude estimation. Instead, we evaluate our structural style and fault kinematics findings in comparison to similar studies conducted in the Pampean flat-slab region. Based on our analysis, we conclude that the Malargüe FTB represents a system of potentially significant hazardous faults.

- **Updated Title**

We have updated the tittle accordingly to the refocus of the main goal of the paper and to reviewer #2 comment. The new title is now: “*Late Pleistocene fault activity and slip rates in the Malarguë FTB front, south of the Pampean Flat Slab of the Central Andes (35ºS, Argentina)”*

- **Removed section**

We have decided to remove the section of the discussion that focuses on the morphostructural evolution of the Malargüe FTB front. The reviewers' comments highlighted the need for further study on the nature of the surveyed terraces and their sedimentary aspects, such as fluvial terraces vs. alluvial fans, in establishing a clear link between tectonic uplifts and the morphogenesis of the alluvial sequences. We believe that a robust model should incorporate climatic and autogenic processes in addition to tectonic uplifts. However, we currently lack sufficient interpretation and data to discuss this matter without detracting from our main study results.

- **Reorganization of the paper**:

We followed the recommendations of the reviewer #2 to reshape the organization of the manuscript.

Old version	**New version**
Abstract **1. Introduction** **2. Methods and data** **2.1.** Morphotectonic analysis **2.2.** Structural seismic interpretation **2.3.** TCN dating **3. Geological setting** **3.1.** Morphostructural setting **3.2.** Neogene to Quaternary migration of the orogenic front **4. Structural configuration of the Sosneado Thrust at depth** **5. The Atuel River and its deposits** **6. Absolute dating of T4 and Q5** **7. Quaternary tectonic activity of the Malargüe f.t.b. thrust front** **7.1.** Folding and faulting above the Sosneado Thrust north of the Atuel River **7.2.** Folding and faulting above the Sosneado Thrust south of the Atuel River **7.3.** Record of the tectonic by the terraces of the Atuel River **8. Discussion** **8.1.** Alluvial chronology and regional correlations **8.2.** Morphostructural evolution of the Malargüe f.t.b. thrust front **8.3.** Slip rate estimation on the Sosneado Thrust 8.4. Seismic potential of the Sosneado Thrust **Conclusion**	**Abstract** **1. Introduction** **2. Geological setting** **2.1.** Regional tectonic framework **2.2.** Morphostructural settings of the Malargüe FTB **2.3.** Neogene to Quaternary migration of the orogenic front **3. Methods and data** **3.1.** Structural seismic interpretation **3.2.** Morphotectonic analysis **3.3.** TCN dating **4. Structural configuration of the Sosneado Thrust at depth** **5. The Atuel River and its deposits** **6. Neotectonic deformation at the front of the Malargüe FTB** **6.1.** Morphological record of recent tectonic activity: the Atuel River's terraces **6.2.** Folding and faulting of the Atuel River terraces above the Sosneado Thrust **6.3.** Folding and faulting in adjacent quaternary alluvial deposits above the Sosneado Thrust **7. Absolute dating of Q4 and Q5** **8. Discussion** **8.1.** Alluvial chronology and regional correlations **8.2.** Slip rate estimation on the Sosneado Thrust 8.3. New insights on hazardous fault systems at 35ºS **Conclusion**

- **Reinterpretation**:
  - <u>Uplift analysis through incision profiles</u>: as suggested by reviewer #2, we have incorporated incision profiles with regards to a theoretical concave river profile linking the tips of the antiforms segments of the river and terraces' longitudinal profiles. This initially provides a good indication of contemporaneous gradual uplift above crustal and thin-skinned thrusts (Figure 9B). To assess the true wavelength and amplitude of the uplifts, we focused on the incision profiles projected orthogonally to the fault strike, i.e., parallel to the shortening direction. It is worth noting that due to zones of aggradation both downstream and upstream of the uplifted areas, the values obtained are considered minimal, as also highlighted by the reviewers. The values obtained using this method are slightly different from the ones of the first version of the manuscript. Values are updated in Table 3.
  - <u>Slip rate estimations</u>: We employed the same methodology to analyze the total folding area recorded by each alluvial surface above the Sosneado Thrust. This yielded slightly different results

for slip rates compared to the previous version, which ranged between 1.1 and 1.8 mm/yr. The updated findings are more consistent with slip rates of 1.1-1.3 mm/yr.

- Uncertainties on Q5: As noted by the reviewers, the alluvial units Q5 exhibit greater uncertainties in their age, slip rates, and profile reconstruction, primarily due to their limited and sparse remnants. Additionally, the TCN ages indicate a short time lapse between Q4 and Q5 concerning the incision between them, raising concerns about the quality of the obtained ages.

  - *Age*: Initially, we considered that the age of Q5 was significantly underestimated, suggesting a post-depositional removal of aeolian sediments. However, as pointed out by reviewer #2, this scenario seemed unlikely, especially considering that the results for Q5 do not appear worse than those for Q4. We now believe that the ages of the two units are likely close to their true values. Given the uncertainties in the calculations, we estimate that the maximum difference in age between Q4 and Q5 would be around 20 kyr.

  - *Slip rates*: The Late Pleistocene average slip rate for Q5 (Q0-Q5) stands at 1.2 mm/yr, falling within the range of results obtained for Q2, Q3, and Q4. However, as recommended by reviewer #2, the analysis of the incremental slip rate (Q4-Q5) indicates significantly higher values compared to the Q2-Q4 trend. This discrepancy suggests that either the age or folded area of Q5 has been underestimated. We are currently able to provide a range of uncertainties.

  - *Structural restorations*: Geomorphic observations indicate aggradation both downstream and upstream of the Sosneado Thrust, suggesting a theoretically limited incision process above the thrust. By utilizing analytical results for fault displacement, we have unfolded and unfaulted the terrace profiles to reconstruct their paleo-riverbed relative to the present-day riverbed (Figure 19). The findings reveal that Q2-Q4 profiles lie slightly below the present-day river profile. Considering that our results provide minimal values for displacements, we can infer that without tectonic uplifts, the depositional regime would be aggradational, which aligns with geological observations.

    However, Q5 stands above the present-day profile and that of Q4, regardless of uncertainties regarding its age and folded area. This indicates a significant incision occurred between Q5 and Q4.

- **Figures' modifications and additions**

  - Figure 1: To emphasize that our study area lies south of a region characterized by intense seismic activity and hazardous faulting, as documented in the literature, we have included a regional map of southern South America. This map depicts the Juan Fernández Ridge and its associated flat slab geometry in the Andean retro-arc foreland. Additionally, we have provided a visualization of intra-crustal earthquakes (<30 km-deep) to highlight areas with high seismicity related to this geodynamic context. Furthermore, a cross-section of the Andean broken-foreland across the intense seismic zone illustrates the structural style of the source faults of these earthquakes. Our intention is to demonstrate that our study area shares a similar tectonic setting (characterized by intense seismicity and structural styles) albeit being closer to the Andean Cordillera.

  - Figure 2: As requested by both reviewers, we have removed the detailed map of the alluvial surfaces of the Atuel River from the geological map. Additionally, to streamline the number of figures, we have included the cross-section across the study area (originally Figure 5) in Figure B.

  - Figure 3: This new figure has been created in response to the request from Reviewer #1, aiming to provide perspective views of the Sosneado thrust front. This visualization enhances the correlation between the spatial distribution of the alluvial deposits of the Atuel River and the morphology of the thrust front. Additionally, in accordance with the suggestion from Reviewer #2, topographic profiles have been added to complement the textual explanations.

- Figure 4: We have made slight revisions to the figure of the seismic interpretation to enhance the clarity of the connection between surface markers (such as faults and stratigraphic intersections) and their relation to well data. Additionally, we have depicted potential subsidiary faults in the near subsurface to illustrate potential displays and duplexes associated with the main ramp. Initially, we simplified the seismic intersection to emphasize its deepest portion. However, both reviewers requested clarification regarding the near subsurface interpretation and its consistency with field observations.

- Figure 5: This is the updated figure depicting the Pleistocene terraces of the Atuel River in the study area, as per the request of both reviewers. In response to a comment from reviewer #2, we have omitted the intermediate terraces from the map.

- Figure 6: unchanged

- Figure 7: The cross-section and its restoration now illustrate the subsidiary faulting in the hanging wall of the main ramp, as depicted in Figure 4.

- Figure 8: This new figure comprises field images aimed at elucidating the characteristics of the surveyed terraces (cut-fill, cut-and-fill, strath terrace), along with the aggradational area. The figure serves to complement the text and the incision profiles presented in Figure 9B.

- Figure 9: As recommended by reviewer #2, we have replaced one of the regional longitudinal profiles with longitudinal incision profiles of the surveyed terraces. This adjustment aims to emphasize the convex uplifted areas more effectively.

- Figure 10: We have subsequently revised this figure, which previously displayed zoomed longitudinal profiles of the two uplifted areas. Instead, we now present a zoom-in on the incision profiles projected perpendicular to the fault strikes. This adjustment allows for the illustration of gradual folding and provides new estimates of uplifts for each terrace level.

- Figure 11: We have made slight modifications to this figure, which now combines previous Figures 11 and 12. For Figure 11A, we have selected different field images with more suitable lighting. Additionally, we have included a field picture of the northern bank of the Atuel River (Figure 11B). The objective is to provide a visual representation of the field environment, aiding potential readers interested in visiting the study area.

- Figure 12: Unchanged, former figure 9.

- Figure 13: Unchanged, former figure 10.

- Figure 14: Formerly figure 8, we have removed all reference to climatic MIS which are not discussed in the manuscript.

- Figure 15: Formerly Figure 14, we have revised the regional correlation for the terraces' age to now focus solely on a comparison with the work of Baker et al. (2009) on the Atuel River. The other regional correlations at higher distances have been omitted as they were deemed not relevant, as suggested by reviewer #1.

- Figure 16: We have included the projected remnants of Q5 on Figure 16B to emphasize the constraints on the profile reconstruction, as requested by Reviewer #2. Additionally, we have updated the representation of the paleo-riverbeds to illustrate their connection between the two tips of the folded profiles.

- Figure 17: We have eliminated the analytical results for the shallowest and steepest segment of the ramp (formerly segment 5). Additionally, we have removed the restored terrace profiles, as depicted in the subsequent figure, to improve visibility and clarity.
- Figure 18: As requested by Reviewer #2, on this new figure we have included incremental displacements and slip rates plotted against the incremental ages between two terraces. This figure effectively illustrates that Q5 deviates from the slip rate tendency of 1.1 mm/yr. Figure 18A facilitates estimating the range of possible combinations of incremental slip and ages required for Q5 to align with a consistent late Pleistocene average slip rate.
- Figure 19: This new figure illustrates the structural restoration of the terrace profiles using the analytical results of incremental displacements. It demonstrates that the Q2-Q4 interval lies below the present-day riverbed, indicating a potential aggradation regime in the absence of tectonic uplift. Conversely, Q5 is notably higher than Q4, suggesting a significant period of incision between the two terraces.
- Table 3: We have removed the analytical results of ramp segment 5 and updated the values associated with the most recent analytical results.

Edits to Reviewer 1 (Carlos Costa):

- Their findings are significant because has been conceived traditionally that Quaternary active thrusting, or at least their expressions at the surface, drastically decreases south of 33° South. These new data can be integrated with previous information north and south of the area studied here, providing a new picture of the neotectonic activity of the Andean Frontal Deformation Zone at these latitudes.
  We thank the reviewer to this key comment. We have reshaped the introduction and discussion to highlight the fact that:
    - **the flat-slab segment is responsible for intense intra-crustal seismic activity in the retro-arc broken foreland of southern Central Andes (31-33ºS)**
    - **Earthquakes of magnitudes>6 are recorded in this zone and correlated with crustal scale faults**
    - **Field studies deduced that such earthquake magnitudes are correlated to slip rates between around 2 $mm.yr^{-1}$ on these crustal faults.**
    - **Our own observations and slip rates estimations on secondary fault (thin-skinned Sosneado Thrust) implies minimal slip rates of 2 $mm.yr^{-1}$ along the crustal thrust front of the Malargüe FTB (35ºS), which is an intense seismogenic zone.**
    - **We conclude that deformation rates and style in the broken-foreland of the flat-slab zone and along the crustal thrust front of the Malargüe FTB are similar. Therefore, this zone should be considered as a hazardous thrust system with potential magnitudes exceeding 7 by comparison of the two areas.**
- The abstract and introduction claim seismic hazard in back-arc broken forelands is underestimated but lack supporting references and explanation. Readers seek clarification on the definition, distribution, and characteristics of these settings along the Andes, including identification of the zone in the study area. Clarification is necessary as the manuscript focuses on these settings, particularly in the flat-slab segment between 27 and 33ºS.
  We fully agree with that comment, and we have removed our initial statement from the abstract and introduction to insist on the previous comment of the reviewer.
- It is also required a regional tectonic setting to address t of the changing geometry of the subducted Nazca plate from the last 8-10Ma and the impact on the distribution of neotectonic deformations.
  We have added a "*Regional tectonic framework*" to the "*Geological setting*" to show the timing of shortening phases in the retro-arc foreland of the southern Central Andes in relation to the slab steepness evolution. We emphasize more particularly the subduction of the Juan Fernandez Ridge

triggering a flat-slab segment at 27-33ºS since 8-10 Ma and a specific slab steepening south of 33ºS since the Pliocene. This explains the reason why the Tectonic activity is believed to be reduced in our study area at 35ºS and wrongly estimated as a zone of non-hazardous fault.
We also detailed the role of the crustal inheritance in shaping the structural styles at 33 and 35ºS (we have added a regional cross section at 33ºS in Figure 1). This idea is to show that the mechanism of the structural deformations is similar. This is key to show that similar deformations processes with similar shortening rates may trigger similar earthquake magnitudes.

- **Quaternary Geology of the study area**

  Quaternary morphostratigraphic units related to alluvial fans (Qn) and terrace deposits (Tn) are distinguished.

  - What is the basis for this classification? How Q and T units are distinguished from each other? Are they related to not constrained and constrained alluvial plain deposits respectively? Why small patches of Q5 in the Atuel Valley correspond to alluvial fan deposits and not to Tn? How were they distinguished from terrace deposits?
    We admit that the sedimentary descriptions of the alluvial units are not strong enough to distinguish alluvial terraces from alluvial fans. We therefore refer all units as Qn to avoid any confusion. Nevertheless, we introduced "Strath Terrace" or "cut-and-fill/cut terrace" to discuss their morphogenesis related to uplifts. That was also a request from reviewer 2. We develop these new descriptions in section 5 illustrated by new field pictures in Figure 8.

    - How they have been correlated in between the AB and ST thrusts?
      Q0 to Q4 present a clear topographic continuum along the profile of the river and their correlation is strong. Q5 correlation is more uncertain. Nevertheless, these alluvial remnants are spread all along the river, they do not spatially overlap and therefore stand alone between Q4 and the Invernada/Mesones formations. In addition, their spatial correlation mimics the topography of Q4. We therefore assume that they belong to the same unit.
      We acknowledge and discuss the uncertainty of such sparse data into the reconstruction of the folded profile and the consequences on the estimation of the slip rates in section 8.2.

    - The numerical ages of Q5 and T4 are very close. It is an analytical issue or a correlation issue?
      This is also a key comment. In the light of Reviewer 2 comments, we do not exclude that the two ages are corrects considering a maximum difference in age of 20 ka due to their analytical uncertainties (see section 8.1).
      The analytical results of the slip rates in section 8.2 allow further discussion as illustrated by the relationship between average incremental displacement and the ages of terrace (Figure 18). This discussion was requested by the Reviewer 2. Results suggest that the age of Q5 might be underestimated in the range of 10-15 ka. Also, the uncertainty in the reconstruction of the topographic profile of Q5 leads to an overestimation and underestimation of the incremental shortening of 10's of meters. Eventually, we have added a new figure of the structural restoration of the paleo-profile to assess the elevation Q2-Q5 paleo-river beds through time. This shows that a significant incision is needed between Q4 and Q5, whatever the uncertainties in the age and Q5 reconstruction. Therefore, Q4 and Q5 are likely close in age. A significant autogenic process is needed to account for the incision rates. Nevertheless, we are not able to discuss this further since this require more morphosedimentary analysis of the alluvial units.

    - Authors claim that T4 is embedded in Q5.
      We do not have a clear figure and notes from the field campaign to illustrate this statement, so we have removed it from the text.

- Perhaps a detailed strip map with Q units along the river valley and transversal cross-sections could help to better illustrate this point.
  Thus, I suggest better explaining the mapping criteria and providing a more detailed figure of the Quaternary units distinguished along the Atuel River valley. Profiles sketched in figures 7 and 3 could be displayed in such complementary graphics. Because the fault modeling relies in the plan-altimetry of these Quaternary units, they deserve a better explanation and a more detailed map representation.
  We have added field pictures to illustrate the arrangement of the terraces (Figure 8) and we have split the geological map of the study area (Figure 2) and the map of the Quaternary units (Figure 5). Also, as requested by reviewer 2 we show the incision profiles for the Q2-Q4 interval within which we show the arrangement of the alluvial units along the Atuel River's profile as observed on the field (Figure 9). Unfortunately, we do not have a field picture showing the Q4 strath terrace regarding Q5 above the Sosneado Thrust. We hope that will be good enough.

- **Methods**

I endorse the methodological approach, but in my view, authors should better develop the following points:

- The cross-section construction departs from different data sources, namely DEM data for topographic profiles of the Atuel River valley, with reasonable accuracy for the scale involved, and seismic lines for the subsurface geometry of the causative faults. Usual oil industry lines lack of good reflectors detail for the uppermost 1s t. time, which is precisely the most relevant part for neotectonic structures. The interpretation exposed in Fig 3a does not clearly show the ramp geometry at the surface, as indicated in Fig. 3b. Could this result in a source of uncertainty for shortening estimation? Do other surface ramp angle alternatives should be considered? Propagating thrusts tend to flatten upwards. Are there field data of the fault surface to contrast with? These points may affect modeling results, so a proper explanation should be introduced, or an uncertainty assessment should be conducted.

  We do think that the subsurface data on Figure 3A show clear markers of the geometry of the ramp:

  - **some seismic reflectors are clearly discontinuous in Figure 4A. Also, the key seismic reflectors numbered 10-60 from seismic facies, display a consistent offset with a reverse displacement on the ramp.**
  - **the ramp is highlighted by a clear change in the dip of the seismic reflectors between the west dipping reflectors in the hangingwall and horizontal reflectors in the footwall. The kink bands delimit dip domains that can be observed from the ramp to near surface dip of the reflectors in the hanginwall of the ramp as shown on Figure 3B.**

  Moreover, despite that the data near the surface is not of the best quality, they clearly show a variability of dip values which are consistent with surface structural observations:

  - **the ramp at surface links to the trace of the Sosneado thrust in the field which is marked by the topographic ridge and the exhumation of the late Cretaceous/Paleogene formation.**
  - **reflectors dip westward in the immediate hangingwall of the Sosneado thrust as shown on Figure 11A and C.**

  For all these reasons, we believe that the structural interpretation is accurate enough to perform an analytical analysis and restorations.

The near subsurface interpretations are more challenging in the hangingwall of the Sosneado Thrust. Seismic reflectors are more chaotic what reflects a more complex structural setting with duplex and splay faults (Figure 4B). This is consistent with the subsidiary faults observed at surface and illustrated in Figures 10, 11, 12 and 13. Therefore, we believe that this is related to structural complexity rather than poor data acquisition.

We have added some of these faults in the cross section (Figure 4B and 7) to show this complexity. The faults from the seismic interpretation only are in dashed lines. Nevertheless, an accurate interpretation of these zones requires additional field data and measurements. We have additional evidence from the field and satellite images to map several secondary faults in the ramp hangingwall. However, their description and analysis would overload the current manuscript.

In that regard, we agree that the slip rate in the shallowest segment of the ramp (segment 6 in the first version) will not reflect the true slip rates since it is spread into several secondary faults. We have therefore removed the values of the segment 6 in the Table 3.

We are working on a new manuscript on this regard. In this paper, we concentrate on the average slip of the Sosneado Thrust at depth and we have removed any analysis of slip rates on the shallowest segment of the ramp in section 8.2.

We have modified the text (section 3.1 "*Structural seismic interpretation*") in the light of the above comments.

- The MT and ST are blind or emergent structures? Fig. 16 suggests that the tip point is already above T4 and probably Q5 for the ST. Has this question been considered for fold modeling?

The Mesón thrust is blind on the section but the Sosneado Thrust is emergent as shown on the seismic section, exhuming the late Cretaceous/Paleogene formation. We agree that this was not clearly stated in the first version of the manuscript. We have added a few sentences in section 4, line 447.

We state in the section 8.2 that the analytical method based on the conservation area of the fold, does not consider the footwall deformation as the fault tip of the Sosneado Thrust is emergent. We also refer to the fact that the upstream and downstream tips of the folded area might be buried in aggradational area. We therefore argue that the values obtained for shortening and slip on the ramps are minimal (Lines 783-786).

Moreover, the unfaulting approach in the structural restoration of the paleo-rived beds (Figure 19) consider an emergent fault tip.

- Fig 16 suggests that shortening estimation has been conducted considering solely T4 and Q5 outcrops above the ST hanging wall. Is this correct? If so, since it sounds like no T4 and Q5 correlatives could be found in the foot wall, the assessed shortening seems to correspond to a minimum value because a pinpoint in the foot wall is desirable, particularly if the tip point of the propagating thrust has been already eroded.
- Perhaps authors have already solved these questions, but I could not find them explained in the manuscript.

We fully agree. The shortening values are estimated for Q2, Q3, Q4 and Q5. As discussed in the previous point, both the emergence of the fault tip and the aggradation downstream and upstream the folded area blur the whole amplitude of the deformation. This leads to an underestimation of the folded area, and therefore the shortening and slip estimation. We hope that this is clearer in the text now.

- **Seismic Hazard Assessment.**

  We agree 100% with the comments on this section. We are not going through all the comments since we have entirely reconsidered this section. Now we consider that the seismic hazard assessment in the 1st version obscures the strong outcomes of the paper: evidence of quaternary fault activity and the related slip rate estimations. We have therefore reshaped this section into a section 8.3 "*New insights on hazardous fault systems at 35ºS*". We consider the San Juan and Mendoza provinces further north, with similar structural styles and slip rates, as a good proxy to estimate that earthquake of magnitudes higher than 7 can occur in the region of Malargüe, along the crustal thrust front of the Principal Cordillera.

  We agree with the reviewer to assess that the mean recurrence of the earthquakes, regarding the average slip rate for the late Pleistocene, exceeds the historical records of earthquakes and that the magnitude 6 earthquakes recorded historically in the area should not be considered as a proxy of the maximum magnitudes earthquake.

<u>Comments per line (original document)</u>

Section 1: Introduction

- Line 42: I suggest to state the grounds supporting this affirmation, including corresponding sources. Also, aside of the Andes; which other broken forelands around the P R of F endorse this point?

  We agree with the reviewer that this statement is not appropriate. We therefore refocused the introduction toward a more regional problematic as nicely suggested by the reviewer: south of 33ºS, faults in the Andean retro-arc foreland are usually not considered as hazardous contrary to the Pampean flat-slab further north which has been subject of many publications.

- Line 46-47: From where this systematic comes from? Please explain and cite corresponding sources, because it does not follow or is not consistent with the traditional tectonic segmentation of the Andes. See for instance.
  - Jordan, T., Isacks, B., Allmendinger, R., Brewer, J., Ramos, V., Ando, C. 1983. Andean tectonics related to the geometry of subducted Nazca plate. Geological Society of America Bulletin 94, 341 361.
  - Jordan, T., Allmendinger, R. 1986. The Sierras Pampeanas of Argentina-A modern analogue of Rocky Mountains foreland deformation. American Journal of Science 286, 737-764.
  - Ramos, V., 2009. Anatomy and global context of the Andes: Main geologic features and the Andean orogenic cycle. Mem. Geol. Soc. Am. 204, 31–65.
  - Ramos, V.A., Cristallini, E.O., Pérez, D.J. 2002. The Pampean flat-slab of the Central Andes. Journal of South American Earth Sciences 15, 59-78.

  This sentence has been removed while reshaping the introduction. However, we have used these references in the text to describe the regional tectonic/structural setting. We did not cite Jordan et al. (1986) because we were not able to find the source document.

- Line 61: I think authors refer to Mendoza province, located at the orogenic front, where both active seismicity and tectonism are located. The latitudes encompassing these phenomena correspond to 30-33 degrees south. 27-33 deg corresponds to the entire Pampean Flat slab. If referring to active fault kinematics , more specific antecedents should be added, i.e:
  - Schmidt, S., Hetzel, R., Mingorance, F., & Ramos, V. (2011). Coseismic displacements and Holocene slip rates for two active thrust faults at the mountain front of the Andean Precordillera (~33°S). Tectonics, 30, TC5011. https://doi.org/10.1029/2011TC002932

- Rockwell, T., Ragona, D., Meigs, A., Owen, L., Costa, C. y Ahumada, E., 2014. Inferring a thrust-related earthquake history from secondary faulting: A long rupture record of La Laja Fault, San Juan, Argentina. Bull. Seism. Soc. Am., 104, 1, 269-284 doi:10.1785/0120110080.
- Costa, C., Schoenbohm, L., Brooks, B., Gardini, C., Richard, A., 2019. Assessing Quaternary Shortening Rates at an Andean Frontal Thrust (32°30′S), Argentina. Tectonics, 38, 3034–3051 http://dx.doi.org/10.1029/2019TC005564.
- Rockwell, T., Costa, C., Meigs, A., Ragona, D., Owen, L., Murari, M., Masana, E., Richard, A., 2022. Paleoseismology of the Marquesado-La Rinconada thrust system, Eastern Precordillera of Argentina. Frontiers in Earth Sc. DOI 10.3389/feart.2022.1032357

Yes, we are referring to the Mendoza and San Juan provinces. We thank the reviewer for this comment and this exhaustive list of publications. We have been widely citing these references in the introduction, regional settings, and discussion to compare the structural settings and Pleistocene kinematics.

- Line 66: This contribution corresponds to northern Pampeanas area. Not relevant here.

Right! We have removed the reference to Alvarado and Ramos (2011).

- Lines 75-76: What exactly this means? The seismic catalog suggests o records severe events? What a severe event would be? This paragraph is too imprecise. Please explain/rephrase.

This sentence has been removed in the new version of the Introduction.

- Lines 89-90: Check this paragraph. The deformation does not originate in the terraces. I suppose it is intended to mean that the goal is to use alluvial terraces as geometric markers for quantifying propagating deformation in Q deposits.

We have reshaped the sentence as follows (Lines 141-142): "*In this study, we aim to quantify the deformation recorded by the Quaternary terraces along the thrust front of the northern Malargüe F.T.B.*"

- Lines 95-96: The precise location of the ongoing deformation or of the fault sources driving active morphogenesis?

We have reshaped the sentence as follows (Lines 112-114): "*By doing so, we aim to clarify the precise location of the fault sources driving active morphogenesis along the front of the Malargüe F.T.B.*"

- Line 97: Check my comments in the conclusion section

Since we do use anymore the scaling laws to estimate the magnitude of an earthquake, which is not the goal of the paper anymore, we have removed this sentence.

- Figure 1 – Caption:
  - Malargüe Basin=Malargüe Depocenter?

    This has been modified in the figure and the caption.
  - Any particular trace attribute to distinguish the Malargüe FTB?

    We have added in the text:
    - **Lines 182-184: "*…the Malargüe fold-thrust belt (f.t.b.; 34°30'S – 36°30'S) inverts the northern and narrowest section of the Neuquén Basin (Figure 1B).*"**
    - **Lines 187-190: "*South of latitude 34°30'S, the narrow thrust belts of the Frontal Cordillera and Precordillera disappear, along with the narrowing broken foreland (Jordan et al., 1983). This appears to be linked to variations in the basement configuration (Figure 1C-D).*"**

- Figure 2 – Caption: Do all the thrust traces correspond to the Malargüe ftb? Which is the thrust front? Please give more specific details. Differences between the Malargüe basin and the Rio Grande basin

(Fig. 1)? Modify Malargüe Bassin vs Basin in the map. Is the Malargüe Basin a Mz or Cz depocenter? Both? I suggest providing more background data for readers not familiarized with the area.

Yes, we describe here the northeastern Malargüe F.T.B.. We now make the link between the geological map and the cross section which makes more understandable the different structural domains of the Malargüe F.T.B.. The Malargüe Depocenter is Mesozoic as stated in the text. We do not deal with the Malargüe Depocenter in the manuscript, so we do not intend to differentiate this depocenter with the Río Grande Basin. Additionally, we removed all references to a "*Sosneado Depocenter*", and we rather refer to "*Neogene synorogenic deposits*".

Section 3: Geological setting

- Line 220: A regional tectonic framework is missing in this section and it is deemed essential for properly understanding the setting of these neotectonic deformation, particularly for readers not familiarized with the regional geology. For instance, the manuscript is focused on the seismogenic potential of the broken forelands of the Andean back-arc, but this key concept is not defined/explained. What is the spatial distribution of the Andean broken forelands and which are their characteristics? Broken forelands have been defined along flat-slab subduction segments (see biblio references indicated in previous comments), and some authors consider the San Rafael block as a possible broken foreland at these latitudes inherited from previous subduction geometries. But this is the broken foreland authors are referring to? The explained tectonic framework should hopefully outline the Neogene tectonic evolution in this area, where variations in space and time of the geometry of the subducted plate are considered to exert the main control on the style and location of neotectonic structures.

  We thank the reviewer for his very good comment. The pre-Andean structural inheritance (Proterozoic orogenic suture zones, Mesozoic rift basins) localizes crustal uplifts all along the Andean retro-arc foreland. Some of those crustal blocks are now incorporated in the Principal Cordillera. In that sense, we can consider the entire Andean retro-arc foreland as a broken foreland which are inverted all along the compressional tectonic episodes since the Late Cretaceous with either in- or out-sequence migration of the major uplifted domains (see Huyghe et al., 2015; Garcia-Morabito et al., 2020; Messager et al., 2023 – references in the manuscript).

  Also, we fully agree that the relationship between variable slab steepness and the intensity of barck-arc uplift was missing. Particularly, the subduction of the Juan Fernandez since 8-10 Ma led to increase the Neogene back-shortening at 31-33ºS, and to maintain a Pampean flat-slab and intense shortening in this area after the Pliocene. This denotes from other areas to south of 33ºS where the steepening led to reduce back-arc shortening. We have stated that in the Introduction and particularly in the "*Regional tectonic framework*". We have also added a Figure 1A to show the relationship between the subduction of the oceanic plateau, the flat slab geometry, and the present-day retro-arc intense intra-crustal seismicity.

Section 4: Structural configuration of the Sosneado thrust at depth

- Line 315-316: The A. de La Piedra Fm. Corresponds to Miocene alluvial fans? A comma is missing here?

  We have removed this sentence.

- Line 326: please ckech if hanging-wall/hanging wall better fits editorial format criteria

  We have replaced "*hanginwall*" by "*hanging wall*" in the whole text.

Section 5: The Atuel River and its deposit

- Line 357: meaning " to the east of "?

  We have rewritten the sentence with: "*East of El Sosneado locality*" (line 460)

- Line 358: What Q0 and T0 means? Which are the differences between them? This cannot be recognized in Fig. 2. A detailed strip map of the Quaternary morphostratigraphic units related to the Atuel river alluvial plain is necessary as a companion of these descriptions. Otherwise, it is confusing to follow the description.

Good comment. To clarify the observations, we just now consider Q0 as the present-day alluvial fan east of the Sosneado Thrust and of the present-day alluvial plain of the Atuel River. We do not need to differentiate the two entities in the scope of the manuscript.

- Line 366: "Manga Th" in Fig 2. Homogeinize

  We have "Manga Thrust" By "La Manga Thrust" in the whole text and figures.

- Line 370: Fig. 2 is confusing as this regard. What units e, j, g. etc means?

  We have rephrased this sentence with "*Q4 remnants are found all along the river (Figures 4 and 8) and consist of coarser conglomerates with pebbles of 20 cm in diameter.*" (lines 474-475) instead of "*T4 remnants are ~~wider~~ all along the river and made of coarser pebbles of 20 cm*".

  As written in the caption of Figure 5, letters *a* to *k* are landmarks of the alluvial remnants projected orthogonally to the Atuel River longitudinal profile (Figure 9A).

- Line 372: Cannot find evidences of Q6 and Q7 in Fig. 2. Aside of this; how these alluvial fans have been identified? It seems to remain small patches of Q5, hot they have been correlated?

  Q6 and Q7 are the Mesones and La Invernada formations assigned to the Mid-Pleistocene although it is not clear how these ages were obtained. They correspond to the brownish units in Figure 2 and are described in the literature as fluvio-glacial deposits. Although these units are of a high relevance to discuss the role of climate in the morphogenesis of the Pleistocene alluvial fans and morphodynamical evolution of the drainage network, we do not address these topics here. Therefore, we do not discuss these formations in the text, and we removed this Q6-Q7 labels.

- Lines 395-396: It would be nice to see these maps

  Considering comments of Reviewer #2 on the fact that we are not using these intermediate intervals for the purpose of our study, and we do not document them enough, we have removed them from the maps and the text.

- Figure 7 – Caption: Wouldn´t be better and less confusing to refer to the proper morphostratigraphic units instead?

  The aim of the letters are only landmarks to make a spatial correlation between the map and the longitudinal profile, to facilitate the reader to know where remnants on the map are located along the stream. We can remove them if confusing.

Section 7.1: Folding and faulting above the Sosneado thrust north of the Atuel River

- Lines 446-448: Cannot get this straight. The alluvial fans are 20-30 cm thick and interbedded with shales and sandstones? Please try to rephrase.

  We have replaced the sentence: "*The hangingwall of the Sosneado thrust is covered by alluvial fans consisting of 20-30 cm-thick unconsolidated alluvial channels composed of small angular pebbles (<10 cm), interbedded with shale and sandstone sequences.*" by "*The hanging wall of the Sosneado Thrust is covered by subhorizontal, unconsolidated alluvial deposits dominated by channels bedforms (20-30 cm-thick) filled by angular pebbles up to 10 cm in diameter, and interbedded with shaly and sandy sequences*" (lines 604-606 in the new version)

- Figure 10 – Caption: Conglomerates´s?

  That was not clear, indeed. We have replaced "*Conglomerates alluvial beds…*" by "*channelized conglomerates* …" (caption of now Figure 12)**

Section 7.2: Folding and faulting above the Sosneado Thrust south of the Atuel River

- Line 503: How the remnants of terrace and alluvia fan deposits are distinguished from each other? How remnants of Q5 are correlated along the Atuel valley being ca. 20 km apart? The Quaternary geology of the area needs to be much better developed, explained and supported through suitable cartography.

We have limited observations and data to discuss the variable sedimentary facies within the alluvial depositional environment. Therefore, we do not have strong argument to discuss if Q5 is either a fluvial terrace or alluvial fan. Therefore, we do not differentiate anymore a fluvial terrace (Tn) from an alluvial fan (Qn). We label all alluvial units Q. We now write in the text: "*The Loma Cohueico Ridge spans over a length of 10 km and a width of 5 km, south of the Atuel River. It is capped by Q4 and Q5 strath terraces (Figures 3, 5 and 11A) , at its highest elevation point.*" (lines 568-570)

- Line 511: It is a subsidiary thrust or might be an out-of-sequence thrust? Is LCT represented in Fig 2?

  The wavelength of the deformation related to the LCT is of lower amplitude (~1 km-wide; Figure 10) and the topographic scarp is around 50 m (new Figure 3A). Also, the cross-section shows that the faults do not link to significant ramp at depth, so it roots at shallow level (Figure 4 and 7). Therefore, it is a subsidiary fault. Also, the gradual folding related to the the LCT and of the larger wavelength fold of the Sosneado thrust (Figure 10) shows that the two thrusts are concomitant.

  To support this, we wrote in section 6.1 (lines 556-558): "*Additionally, the Loma Coihueco Thrust appears to exhibit further syn-depositional deformation of lower wavelength in the hanging wall of the Sosneado Thrust. Both thrusts are therefore contemporaneous.*" Also in section 6.2 (lines 586-590): "*This subsidiary deformation in the hanging wall of the Sosneado Thrust, located south of the Atuel River, provides evidence of its activity after the deposition of Q5 and Q4. These observations align with the deformation patterns associated with the Loma Coihueco Thrust, as evidenced in the longitudinal and incision profiles of the terraces along the Atuel River (Figures 9 and 10B).*"

- Figure 11 – Caption: It is important to show the detail of the tectonic contact with an inset or another pic. Even with a zoom in, the present photo might not be enough to convincing for skeptic readers.

  We fully agree that a closer picture of the fault would have strengthen the observations. Unfortunately, despite we've been walking on the fault contact, the quality of the outcrop did not allow such observations. The outcrop is in a steep cliff, made of Miocene shale and fallen Quaternary pebbles.

- Figure 12: in my opinion, this figure is of little help for supporting what is described in the manuscript. The photo is too general with too many shadows. The sketch is too tight and details are not clear. Perhaps an oblique south-looking aerial view from Google Earth or a similar platform may be better. The LCT and Presumably the ST can be easily recognized with higher oblique angles. It would be more clear to represent T4 deposits in that kind of graphic support.

  The purpose of this figure is to show the morphology of the ridge but also to guide readers on the field to QC our observations. In that regards we think that this figure is still needed here. We have chosen a picture with a better light (from the North; Figure 11A) and added a similar field picture for the northern part of the ridge (Figure 11B).

  Also, following the reviewer comments, we have added perspective views from GoogleEarth (Figure 3) together with topographic profiles (comments from the reviewer#2) to illustrate both the morphology of the thrust front and the Quaternary deposits.

  We have merged figures 11 and 12 in a single figure (new Figure 11) to reduce the total number of figures.

Section 7.3: Record of the tectonic activity by the terraces of the Atuel River

- Lines 541-543: This paragraph does not sound clear to me. Try to rephrase.

  We have removed this sentence and do not discuss of the potential uplift amplitude in Q5 in this section.

- Line 554: Again, a proper image or cartography is needed to help readers relate the profiles and deposits & landforms along the Atuel valley.

We have added field pictures (Figure 8), the new Quaternary map (Figure 5) and added letters A-k to link the map to the longitudinal profile.

- Figure 13: What dotted and solid lines represent in the thrust traces?

   We have added in the figure caption: "*The solid lines represent the topographic data derived from the DEM, while the dashed lines depict theoretical terrace profiles, mitigating erosional effects.*"

Section 8.1: Alluvial chronology and regional correlations

- Line 572: Giving the ages obtained, it is important to explain the grounds to consider Q5 and T4 as different chrono-morpho stratigraphic units. A good map would help here. Also a suitable explanation on the basis on which alluvial fans and terrace deposits are distinguished. Also; which is the mismatch in altitude between T4 and Q5

   Very good comments also raised by the Reviewer #2. We cannot perform a better map of the terrace remnants neither discuss their stratigraphic relationship based on sedimentary observations. But we have added a discussed extensively this matter in different ways:

   - <u>Section 7 – Absolute dating of Q4 and Q5</u>: we consider that the TCN analytical results are strong enough and show that Q4 and Q5 ages are close to reality.

   - <u>Section 8.1 – Alluvial chronology and regional correlations</u>: after revising the TCN analytical results, we infer that the maximum difference in age between the two terraces should not exceed 20 kyr (lines 735-737).

   - <u>Section 8.2</u>: we use the analytical results for the slip rates to show that the incremental displacement on the Sosneado Thrust between Q5-Q4 diverge from the consistent trend established by Q2, Q3 and Q4. This allows us estimating a possible range of values for the incremental age versus displacement that should have been recorded by Q5 (Figure 18A). Also, the structural unfaulting/unfolding of Q5 (Figure 19D) suggests that regardless the underestimation for Q5 age of for the folded area recorded by Q5, there must have been an intense period of incision between Q4 and Q5.

- Line 586: Do not think Polanski (1963) obtained numerical ages for those units. From where these ages come from?

   That's right. We now just mention that "*Q5 likely postdates the middle Pleistocene Invernada and Mesones formations (Polanski, 1963).*" (Lines 739-740).

- Line 591: It is attempted here to correlate with similar processes at very distant places. I think it should be underscored that this is highly speculative.

   This is a good comment. So regional scale correlation should consider many other climatic and geomorphological considerations. Therefore, we just keep the correlation history with the Diamante River which is located only 40 km further North (Baker et al., 2009) (Figure 15).

Section 8.2: Morphostructural evolution of the Malargüe thrust front

- Line 633: How is it envisaged the morphogenesis of the terrace deposits (Tn) ¿ Are they constrained by the ST to the east?

   We have decided to remove this section because we lack strong sedimentary observations. Nevertheless, the description of the nature of the terraces (cut-fill/fill-and-cut, strath terrace and aggradation areas) as display on Figure 9B, suggest that the uplift of above the Sosneado Thrust clearly impact the incision pattern. This is summarized in the first section of the Discussion.

Section 8.3: Slip rate estimation on the Sosneado thrust system

- Line 679: Are there data of the ramp angle at the surface? How these data have been introduced in the models? Thrusts tend to decrease dip angle upwards, particularly when propagating through Q deposits. It would be advisable to explain/clarify this potential uncertainty source in shortening estimation.

   The strata at surface in the hanging wall of the Sosneado Thrust, are dipping around 40 degrees to the West. This may indicate a potential dip of the ramp at surface which is quite uncertain to

determine from the seismic data. This is because of the several fault play in the near surface as suggested by the reviewer.

Because of the more complex structural pattern within the shallowest ramp segment, we have removed all slip rate analysis on it. The results we initially gave was overestimated because the slip is spread among all the faults splay, including the LCT which is confirmed by the surface analysis.

Section 8.4: Seismic potential of the Sosneado Thrust

We fully concur with all the comments provided below. Furthermore, we adhere to the recommendation made by reviewer #2 regarding the removal of any empirical analysis concerning the magnitudes of potential earthquakes. Our preference lies in comparing structural styles and slip rates on crustal-scale faults to assess whether the thrust front of the Malargüe FTB should be regarded as a hazardous fault system, despite our location south of the Pampean flat-slab intra-crustal seismic zone.

Therefore, we do not reply to all the below comments.

- Line 718: Which is…..in yrs?
- Line 721: This does not corrspond to the Precordillera environment. You might better check Rockwell et al., 2014 and 2022
- Line 734: Meaning : surface rupture events? Explain the grounds
- Line 736: Cannot see this in Fig. 9
- Line 737: Do the authors refer to Fig 10? If so, in my opinion some problems arise here:
    1. The field photos lack of the necessary detail and resolution for readers to properly judge the key assumption of coseismic slip. But if one follow the interpretation sketches, faults A and B rather look as blind propagating structures. There is no evidence of colluvial wedges or similar lithofacial architecture which could suggest so.
    2. Even if assuming surface rupture, it does not come out clear from the sketches if the faults cumulated slip derived from one or more events.
    3. So, even if the authors assumption might be correct, I think it is not rooted in the proper grounds, or at least not supported by the data provided in the manuscript.

  If authors attempt to use MD empirical relationships to estimate paleo-earthquake magnitudes, I suggest to provide good quality pictures of the fault exposures, along with detailed loggings (not sketches), so readers can properly judge these statements.
- Line 742: For thrust faults, this means that the probability of surface rupture is about 0.6. See for instance: Lettis et al, 1997, BSSA,1171-1198; Moss&Ross, 2011 BSSA, 1542-1553; Baize et al., 2019, SRL, doi 10.1785/02 20 190144; Nurminen et al., 2022. Nature.com/scientific data
- Line 749: In order to use the rupture length for magnitude assessment, a segmentation analysis of the thrust trace should be conducted. See for instance the classic works in this regards , i.e. Schwartz&Sibson, 1989, USGS OFR 89-315; Machette, et al, 1992. The wasatch fault zone, USA. Ann. Tect. 6, 5–39. ; Du Ross et al, 2016. Fault segmentation: new concepts from the wasatch fault zone, Utah, USA. J. Geophys. Res. Solid Earth 121, 1131–1157. https://doi.org/10.1002/2015JB012519; Or the classic paleoseismology by McCalpin. Unless very shallow hypocenter, thrust ruptures related to a M5.2 seems unlikely,
- Lines 755-757: Careful…. It is intended to say thatinstrumental seismicity represents or captures the seismogenic potential of the ST??
- Lines 760-761: So, can this happens or not? How to conciliate this interpretation with the question I raised in my previous comment? More important: Which is at last the estimated seismogenic potential for the ST? M5.2, M6.5 or M>7? Readers might remain confused

Conclusion:

- Lines 782-785: Why 1.8 +-0.2 mm/a is considered a hich rate? In comparison to what? What ´high probability´means? So the take away lesson would be that the instrumental seismicity represents the seismogenic potential of this seismogenic source? It should be uncerscored that these data for deriving these interpretations were colledted on secondary structures as to the main thrust, and therefore these nterpretations may underestimate the seismogenic capability. Otherwise; 30 cm of interpreted coseismic slip , under SR=1.8 mm/a would give rise to unrealistic mean recurrence intervals

The reviewer is right. The new slip rate estimation on the Sosneado Thrust falls to 1.0-1.3 mm/yr, or to 2 mm/yr along the crustal faults of the Principal Cordillera if we consider the accumulated displacements along the Malargüe FTB front. As discussed in the previous comments, we agree that the surface expression of faulting is related to subsidiary faulting and the displacements do not reflect the displacement on the Sosneado Thrust. We have removed these considerations from the discussion and conclusion.

We should also consider that the Sosneado Thrust may not be the source of the earthquakes. The source fault should be more considered to be the crustal thrust front (La Manga Thrust?) which records most of the historical seismicity. The size of the crustal scale faults may trigger an earthquake magnitude above 7, by comparison to the broken foreland of the Pampean flat-slab zone. Nevertheless, the recurrence of such a magnitude with these slip rate estimate, would be of several hundred/thousand years.

Edits to Reviewer 2 (Martine Simoes):

What does the submission need to be publishable?

* Rewriting: I am not a native English-speaking person, but I have the feeling that some corrections for English are sometimes needed and may impede at places a detailed understanding of the ideas and reasoning. Even though I do not think that this a major point, if the authors have in hand native English-speaking colleagues that could give a final reading to the manuscript, that may be helpful.
Eric Blanc, co-author of the paper who has English-native writing skills, has dedicated a specific effort to review the English writing of the paper compared to the previous version.

* Reorganising: I felt sometimes lost with the overall organisation of the manuscript, and if found appropriate to the authors, I have the following suggestions for re-organisation:

1 Introduction

1. Geological Setting (and not as section 3– as such by further presenting the context, after the introduction, the authors can better expose what is known, what is unknown and therefore further present and justify the scientific motivation of their work. This would go together with Figures 1 and the geological map of Figure 2)

3 Methods and Data (and not as section 2)

4 Structure of the Sosneado Thrust

5 Atuel River morphology

1. Quaternary tectonic activity (and not as section 7, as it is a good follow up to the previous section)
2. Cosmogenic ages (and not as section 6)

8 Discussion

+ additional suggestions of re-organisations within some of these sections.
We have reorganized the manuscript following all the above recommendations and in the text.

* More figures:

- For an easier reading of the color code, I would break the map of Figure 2 in 2 maps: a geological map (with underlying geology) and a geomorphological map (with the most recent alluvial deposits used in the morphotectonic investigation, ie Q5 and younger). The different yellow/orange/brownish

colors, with or without hatching, from recent Cenozoic deposits in the geology to the terraces in the geomorphology are too hard to distinguish and read from the figure as it is now.
We have split the geological map of the study area (Figure 2) and the map of the Quaternary units (Figure 5).

- Figure 7 could also be simplified as panels a and b are repetitive, but it could be complemented by a figure where the terrace profiles are not shown in terms of elevation, but in terms of incision (ie altitude with respect to the present-day river, along a longitudinal profile), and indicating the places where there is presently incision but also aggradation (ie mostly at the front of active frontal thrusts).
  As suggested, we have removed former Figure 7A and keep only figure 7B (now Figure 9A). We have added incision profiles along the stream in Figure 9B for levels Q2, Q3 and Q4. Included are the along-stream nature of the terrace arrangement (strath terrace, cut-and-fill/fill-cut and aggradation).
  The along-stream regimes of terrace arrangement is supported by field pictures in the new Figure 8.

- The same for Figure 13.

  The figure 10 (former figure 13) shows zooms of the incision profiles above the La Manga and Sosneado thrusts. Contrary to Figure 9B, these profiles are projected perpendicularly to the strike of the thrusts. Therefore, they reflect better the structural uplifts.

- At the end, I missed a final graph where the derived incremental shortening / average fault slip is compared to ages for each terrace level, with uncertainties on each parameter, to better visualize the evolution of shortening/slip over time. Much more helpful than table 3 where rates are calculated for specific time intervals, without illustrating the possible tendency – in fact, the reader needs to generate this figure from the table to get this understanding.

  We have added this new figure 18 in the discussion of section 8.2. The figure is divided in two graphs:

  - **Figure 18A: average incremental slip and shortening values versus incremental age (between two alluvial units)**
  - **Figure 18B: incremental slip and shortening rates vs. incremental ages**

  Figure 18B show that the average slip incremental rates are around 1.1 mm/yr for Q2, Q3 and Q4 but differs significantly for Q5.

  Figure 18A is key and allows discussing the uncertainties on both the reconstruction of the Q5 profile from limited remnants and its age. Considering an average slip rate of 1.1 mm/yr during the late Pleistocene, and a maximum age difference of 20kyr between Q4 and Q5 (see discussion in section 8.1), we can estimate the underestimation window in incremental slip and shortening rates for Q5. This part of the discussion is new.

* Reinterpretation: I have some suggestions here and there, but my main concern is in the calculation of incremental shortening or slip for each terrace above the Sosneado Thrust. As far as I understand how the calculations were done, I have the feeling that incision only is considered - ie the uplift of the terraces above the river, and therefore in a reference frame related to the river (or to an ad hoc paleo-river). However, in determining fault shortening or slip rates, we should be a structural reference frame, where the uplift of the hanging wall is calculated with respect to the footwall. In other words, a correction for river base level is

missing, ie for sedimentation over the Sosneado thrust footwall (see for example Figure 7 in Lave & Avouac, 2000). If there is no way to estimate sedimentation at the river base level, then the rates proposed here are only minimum values. Also I would suggest commenting the river signal from incision profiles (indicating where aggradation is ongoing) and not only elevation profiles.

These are excellent comments. The analytical method is based on the conservation area of the fold as discussed in section 8.2. But this approach does not consider the footwall deformation as the fault tip of the Sosneado Thrust is emergent. We also refer to the fact that the upstream and downstream tips of the folded area might be buried in aggradational areas. We therefore state that the values obtained for shortening and slip on the ramps are minimal (Lines 783-786).

In the first version of the manuscript, the paleo-river of Q4 and Q5 were estimated via their structural restoration with regards to the unfolding of Q2 and Q3, although we think that was not clearly stated in the text… The consequence is that the paleo-river beds was slightly lower in elevation than the tips of the folded area. Since we consider the analytical values as minimal, we have considered the paleo-river as joining the two tips of the folded area for Q4 and Q5 (the paleo-river of Q2 and Q3 remains the present-day riverbed). This different approach has decreased the areas of folding for Q4 and Q5, and therefore the shortening and slip rates. We therefore obtain a slightly lower average slip rate of 1.0-1.3 mm/yr but which is now consistent for all levels.

To discuss the consistency of the results, we have added the Figure 18 showing the restoration of the terrace profiles through incremental unfaulting (slip on the ramp) and unfolding (horizontal shortening/heave). The results show that Q2, Q3 and Q4's paleo-river bed fall deeper than the present-day riverbed, what is consistent with an aggradation downstream the uplift. Q5 restoration, whatever the uncertainties in its age and reconstruction, remains higher than the present-day river which suggest a period of incision since its abandonment.

* Citations: some of the cited work corresponds to congress abstracts, ie to simplified (non-peer-reviewed) interpretations with no access to the raw data that motivated these interpretations. Does Tektonika accept such type of citations ? I know it is common practice in the geoscience community in S. America, but I do not think this is to be encouraged and some journals now refuse..

We have deleted all citations related to congress abstracts. We kept a few key PhD thesis and references to geological maps.

Title and abstract

Title fine, just a suggestion: complement the title with "Late Pleistocene fault activity and slip rates…", just to clarify that the main focus is on faults right from the title.

We have considered this comment in the new title which reflect the refocus of the paper.

Introduction

Here I consider together sections 1 (Introduction) and 3 (geological context). Section 3 should be placed as a section 2 to provide sufficient geological background information, after the general introduction, and before getting into the details of this work (data, methods, results, etc).

Additional minor comments in Section C of this review.

Very good suggestion. We have reorganized the manuscript accordingly to these comments and the detailed ones.

Data and methods

- I suggest to slightly re-organize this section (here section 2, but suggested to move to a section 3), so as to mirror the subsequent presentation of results. As structural results are presented first (interpretation of seismic line), I would rather start this section with the presentation of the seismic line (3.1 structural seismic interpretation), followed by the various maps and topo data (3.2 morphotectonic analysis) and the chronological constraints (3.3 TCN dating).

  Done

- My main concerns here in terms of data sources or processing are:

  - **Seismic interpretation**: there is little said about the seismic line used here. Is it accessible ? probably not, but mention it, or indicate how to access it, here or in the data availability statement as this information on the accessibility of the line is at the moment not provided. Did you get only the line-drawing shown on figure 3 ? or did you access the depth-interpreted line ? or the raw data (two-way travel time) and processed the depth conversion ? or in other words, what was your own processing of the data you accessed ? can we have information on the depth conversion of the line ?

    Although we have contacted relevant people in the Argentinean assets of Total SA and YPF (Argentinean oil and gas company), we did not manage to get the permission to publish the seismic line, neither where to get access to the data. This data is old and was not anymore in the database of Total so we did not get further information in the processing. We had access to the depth data and we present our own interpretation of the seismic line. We have clarified all this in the text (section 3.1, lines 286-291).

    Where are the wells (on the map of Figure 2 and along the line of figure 3) ?

    The well is now located on Figure 2.

  - **TCN dating**: the authors used surface samples, and combined two cosmogenic nuclides. But why didn't they sample along depth-profiles to better constrain exposure ages and average inheritance ? my own (limited and modest) experience with surface samples is that they tend to have very scattered ages, in excess when compared to depth profiles…
    We are aware that a depth profile provides slightly greater robustness (although it is not a magic solution) in assessing the (erosion/time) pair, but unfortunately this is a costly technique that we were unable to implement here.

  - Also, are 1-5 samples per surface sufficient to date the surface abandonment ?

    The idea is that 3 samples giving the same result are enough, so for a single surface 3-5 samples is ideal. Slightly less for a terrace sequence (see references). In the present case, this rule is verified for S2 and S4; S5 is somewhat less clear.

  - TCN dating: why not consider the classical 2sigma uncertainties, even though larger ?

    We use one sigma which is quite usual for TCN.

Results

Here my comments concern sections 4 (structural interpretation), 5 (morphology of Atuel river), 6 (dating) and 7 (Quaternary tectonic activity).

As mentioned earlier, I would suggest to re-organize slightly these results sections, with:

1. Structural interpretations
2. Morphology of the Atuel river
3. Quaternary activity (present section 7)
4. Dating (present section 6)

as the section on the evidence for Quaternary activity follows nicely that on the morphology of the Atuel river, and does not yet need the chronological results. As such the results would also nicely mirror the data & methods section.

Done

More detailed appreciation for each one of these sections:

- **Structural interpretations (section 4).**

My main concern is here on the interpretation of the seismic line, maybe too simplistic when compared to surface geology. Indeed, the region above the two most frontal interpreted fault segments (km 7 to 10 on figures 3 and 6a) does not match surface geology as intuited from Figure 2. Indeed on Figure 2, this part of the seismic line seems to cross 3 main thrusts (+ additional 1-3 minor thrusts) whereas this is not shown in the final interpretation. It should be noted that the line drawing of Figure 2 suggests that reflectors may be more deformed than interpreted here. The final structure here is expected therefore to be more complex, and the total shortening (1500m probably higher) when the balancing the section.
We fully agree with this comment. The structural interpretation has been deliberately simplified to analyze the slip rates solely on the main ramp of the Sosneado Thrust. Most of the superficial deformation linked to the surface observation developed within the Neogene units as shown on the seismic section (Figures 4 and 7). The near subsurface interpretations are more challenging in the core of the in the hangingwall of the Sosneado Thrust. Seismic reflectors are more chaotic what reflects a more complex structural setting with duplex and splay faults (Figure 4B). This is consistent with the subsidiary faults observed at surface and illustrated in Figures 10, 11, 12 and 13. Therefore, we believe that this is more related to structural complexity rather than poor data acquisition.

We have added some of these faults in the cross section (Figure 4B and 7) to show this complexity including the Mesón thrust which is here blinded and corresponds to the flat fault. The faults from the seismic interpretation only are in dashed lines. Nevertheless, an accurate interpretation of these zones requires additional field data and measurements. We have additional evidence from the field and satellite images to map several secondary faults in the ramp hangingwall. However, their description and analysis would overload the current manuscript.

However, the restoration in Figure 7 considers all subsidiary deformation, including those in the Neogene units and slip on the Mesón Thrust. Therefore, the total shortening should not be too much underestimated.

In that regard, we agree that the slip rate in the shallowest segment of the ramp (segment 6 in the first version) will not reflect the true slip rates since it is spread into several secondary faults. We have therefore removed the values of the segment 6 in the Table 3.

We are working on a new manuscript on this regard. In this paper, we concentrate on the average slip of the Sosneado Thrust at depth and we have removed any analysis of slip rates on the shallowest segment of the ramp in section 8.2.

We have modified the text (section 3.1 "*Structural seismic interpretation*") in the light of the above comments.

By the way, what is the meaning of the shallow red line on figures 3 and 6, at 1 km asl ?
This is the Mesón Thrust at its southern tip. It is blinded in this section. This ramp was needed to accommodate displacements within the Neogene units. It matches at surface the southern continuation of the Mesón Thrust at surface as represented on Figure 2. This intersection is added on Figure 4.

- **Morphology of the Atuel river (section 5)**

I think that one very important information is missing on the nature of the surveyed terraces. Are these strath terraces, and if so how thick are the deposits ? are these fill terraces ? or cut-and-fill ? This information is important to understand where the present-day river is incising / aggrading (information missing on figures 7 and 13), whether it is incising into bedrock (with probable long-term uplift) or into its own deposits (if readjusting to climate or hydrological changes for instance). For instance, surface e (Q5) is described as erosional (lines 368-370), while the rest of Q5 is described to be 30 to 100 m thick (lines 367-368): the information on where the transition from incision/erosion to deposition/aggradation occurs and how it correlates (not not) spatially with faults is worth being mentioned. A field picture of a typical terrace profile (in section) could be useful.,
Field observations show that some section along the stream are cut-and-fill or cut-fill (we can't decipher them) for the interval Q1-Q3 within the Principal Cordillera, above the Arroyo Blanco/La Manga thrusts and in a limited extend upstream and downstream the Sosneado Thrust. No clear evidence for this was observed regarding Q4 and Q5. Strath terraces for the entire interval Q1-Q4 are located above the Sosneado Thrust. Usptream the Sosneado Thrust, Q0-Q2 is aggradational as Q3 since to disappears within this wide valley. Dowstream the Sosneado Thrust, the Sosneado alluvial fan is currently aggradading and should have been the case since at least Q3 because of their along stream continuity.

We have added the Figure 8 with field pictures to illustrate these observations. The Figure 8B also represents those observations on the incision profiles to illustrate the relationship between tectonic uplifts and the morphogenesis of the terraces.

I think the discussion on the remnants of Q6-Q7 useless (lines 387-388, Figure 7), as we are not sure to pick the top of the deposits (or else the same depositional horizon) all along the profile. In any case, you do not use these geomorphic/sedimentary markers in your subsequent analysis.
We agree and we have removed the discussion on these remnants. We just refer now to the Mesones and Invernada formations within a geological context.

Line 389: why have you mapped intermediate levels for T2 and T3 ? you never use them in your analysis, and this results also in a complex geomorphological map (Figure 2).
We agree and we have removed any references and discussion to these intermediate levels both in the text and in the Figure 5.

- **Evidence for Quaternary tectonic activity (section 7, but suggested for section 6).**

I suggest starting this section with the tectonic record of the terraces of the Atuel river (section 7.3, to be changed to a section 6.1) as it flows naturally after the morphological description of section 5 (in fact, these two sections could be merged ?), followed by the other two remaining sections.
We agree and the text has been modified in that regards.

For what concerns the section on the terraces of the Atuel river (7.3 to be moved to 6.1), I would suggest quantifying and discuss here the incision pattern – and not the sole elevation of the terraces above the present-river. Incision is a good (potential) indicator of terrace uplift in a river reference frame, ie by taking the present-day river as a proxy for the geometry of the paleo-river (Lave & Avouac 2000) – provided that we're dealing here with strath terraces! This would avoid some confusion. For instance, at line 529 there is the mention of steeper terrace flanks on the eastern side of T4 above the AB thrust… whereas the flanks would be steeper (and shorter) to the west when considering incision, as the slope on the western side is reversed compared to the present river. The "deformation" pattern would be more direct – and correct - from incision profiles.

We have modified the Figure 13 (now Figure 10) using the incision profiles projected perpendicularly to the thrusts' strikes. We agree that this reflects better the gradual tectonic uplifts through the deposition of the alluvial surfaces. The profile above the La Manga Thrust fault shows now clearly that the western flank of the bulge is the steepest. Also, Inversely, the steepest flank along the Sosneado Thrust is to the east and the Loma Coihueco is there introduced since it shows a significant change in the slope in the core of the bulge.

We do not address here the discussion on the vergence of subsurface thrust around the Arroyo Blanco Thrust since we do not have surface or subsurface data to give weight to the conclusions. We rather emphasize on the fact the tectonic uplifts are synchronous to the deposition of the terraces and on describing the amplitudes of the uplift.

This said, I believe that there 3 main points of attention here, maybe not enough emphasized:

- overall, the pattern of incision / aggradation is spatially correlated to the location of faults, indicating that active tectonics controls, to the first order, this pattern. An important conclusion, which is never really mentioned. Use the figures to illustrate this, by also indicating where you have active incision and where you have active deposition !

  Very good point. The incision/aggradation pattern and the nature of the terrace surveys are represented by field pictures in the new Figure 8 and the along stream incision profiles in Figure 8B. We wrote the first oreder conclusion of the relationship between the terraces' morphogenesis and the tectonic uplifts in the first section of the Discussion (Lines 704-713).

- there are two ~7 km wide "bulges" around some of the faults – you already mention it. However, I have a hard time understanding the first one above the AB thrust as the steeper and shorter flank to the west (when you imagine the related incision profile) would indicate deformation above a westward thrust (?).

  Yes, we believe that this deformation refers to a west-verging thin-skinned thrusting. Antithetic thin-skinned thrust at the tip of crustal thrust develops within a fishtail structural style. This is common when the crustal thrust reaches a weak sedimentary cover. This has been observed in the study area as shown on Figure 2B (Giambiagi et al., 2008) but also commonly in the region (Rockwell et al., 2014, 2022; Fuentes et al., 2016; Lopasso et al., 2024 – all references are in the new version of the manuscript).

  But, since we are not performing any slip rate analysis on this fault and that we do not have any field observations and data to support this, we prefer not discussing the structural style of the deformation and we have removed any reference to the style of the deformation in the text.

- Also above the LC thrust the pattern drawn on figure 13b indicates that T4 is higher on the eastern (footwall) side of the thrust when compared to the western (hanging wall) side, in contradiction with my understanding of the field pictures of figures 11-12. Maybe there is the need to clarify things here.

  We do not see the issue here. On Figure 11C, Q4 in the hanging wall of the thrust overrides itself in the footwall and is therefore higher. Nevertheless, at the scale of the ridge (Figure 11A), the LCT is located at the apex of the fold. Q4 in the LCT hangingwall, is therefore tilted to the west and is less elevated than at the apex . The LCT is a secondary fault which roots as shallower

depth compared to the Sosneado Thrust. Therefore, the vertical uplifts and topographic effect will be primarily related to the Sosneado Thrust.

In the first version, we presented first the LCT and then the larger scale folding. This might have been the reason of the confusion here. We have reorganized slightly the text here and we first describe the morphology of the fold related to the Sosneado Thrust as recorded by the terrace deformation. This makes the link more straightforward with the description of the incision profiles in the previous section. Then we describe the LCT and shows that it results in a minor topographic scarp and overriding of Q4. We do not want to add more discussion not to overload the text. We hope that will be good enough.

- there is a "residual" gradient upstream for terraces T1-T4 that you do not comment much. Is this hydrological ? or related to large-scale (thick-skinned) folding upstream ? A (paleo)gradient of 4% for T4 is unlikely to be (fully) hydrological as these kind of values are found in small mountain-front rivers. So part of the signal is probably to be also exploited for tectonics, even just qualitatively.

This is a very good observation which is indeed not discussed here. Nevertheless, this discussion implies to zoom out at more regional scale to understand the morphodynamic response of the Atuel river to the deposition of the massive fluvio-glacial deposits (Mesones Fm.) of an uncertain age (Pliocene-Mid-Pleistocene), and to its abrupt incision probably related to climatic (Mid-Pleistocene Climatic Revolution?) and autogenic processes. The longitudinal profile extended further west in the Principal Cordillera, shows "fossils" knickpoints migrating westward. We do believe that this residual gradient is an autogenic process enhanced by climatic factors. There are no clear evidence of the role played by tectonic uplifts which must also be considered.

This discussion is out of the scope of the manuscript and can't be easily discussed. We have added these sentences in the text (lines 508-511):
"***We intentionally withhold the incision profiles in the Principal Cordillera because the discussion of their residual steepness gradient (Figure 9A) would require consideration for tectonic, climatic, and autogenic surface processes beyond the scope of this study.***"

- Section 7.1 (to be moved to a section 6.2 or 6.3)

I do not understand well the point here. Indeed the field observations concern the deformation of older synorogenic deposits (Mid Miocene Agua de la Piedra formation ?), according to the location of these field observations on Figure 2. I am therefore not convinced that it is useful here to compare the deformation of such old deposits to the Quaternary deformation at the heart of this manuscript. Faults A and B described in Figure 10 are Mid-Miocene (as intuited from the geological map of Figure 2) or Quaternary (as suggested) ?

No, the deformation affects Quaternary deposits, probably younger than the Mid-Pleistocene as stated in the text (lines 604-608). These deposits are nearly horizontal and overly the Miocene deposits which clearly dip westward. We have slightly modified the text to get it clearer (with underline):
"The hanging wall of the Sosneado Thrust is covered by subhorizontal, unconsolidated alluvial deposits dominated by channels bedforms (20-30 cm-thick) filled by angular pebbles up to 10 cm in diameter, and interbedded with shaly and sandy sequences. These alluvial deposits cap unconformably the westward-dipping beds of the Agua de la Piedra Fm. (Middle Miocene) and Malargüe Gp. (Upper Cretaceous-Paleocene) (Figures 2B and 5)."

We have also changed the title of this section so it's clearer: *"6.3 Folding and faulting in adjacent quaternary alluvial deposits above the Sosneado Thrust*".

- Dating (section 6 to be moved to a section 7)

I understand that too young Al ages for Q5, when compared to Be ages, could be an indicator that the Q5 Be age is wrong. I am however surprised that Be ages are well distributed and not more scattered, as they are in fact for T4. Could we imagine that the Q5 age of 74 ka is fine, but that of T4 is wrong/too old ? indeed, T4 is entrenched within Q5 deposits, and some of the T4 pebbles used for dating could be remobilized pebbles from Q5 – and therefore show biased old ages ? depth-profiles would have been useful…

We thank the reviewer for this wise observation. We have revised our discussion accordingly and now use a Q5 age of 75 kyrs and a close Q4 age (71kyrs). We discuss the fact that these ages are not precisely resolved and that their difference is most likely less than 20 kyrs. In fact, the coherence of the two estimates for Q4, as well as the coherence in the age data set makes it difficult to explain Q4 TCN concentrations with a more complex history. Finally, we agree that a depth profile would resolve this ambiguity, but this was not possible within the framework of this study.

We have added in the text (lines 735-737):

"In summary, our age estimates for Q5 and Q4 are 75±8 kyr and 71±7 kyr respectively. We cannot rule out the possibility dating errors, but it seems likely that their respective ages are in the range of 55-85 kyr with a difference of age less than 20 kyr."

Discussion and conclusions

The discussion section (section 8) could maybe be improved by further discussing the uncertainties and the limitations of the various proposed interpretations. In more details:

8.1 Chronology

- lines 569-571: can you further elaborate on this interpretation of a cover lost for 100s kyr ? what other regional observations support this ? how would an alluvial fan, finally interpreted to be 174 ka old, loose cover for several 100s of kyr ? I am confused…

We agree with the reviewer that an aeolian cover is not reasonable for several 100s of kyrs. If we don't rule out the possibility of an aeolian cover, it is no longer consistent with our best guest.

- lines 584-586: why does T1 need to be younger than 13 ka ? Here also some more explanations are needed to follow completely the reasoning.

This point was maybe not clear in the text. We rather refer to a terrace level younger than Q2, i.e. than 13 ka. (see lines 738-745)

8.2 Morphostructural evolution of the mountain front

The text is overall a little bit confusing and may need some re-writing. I would suggest to further take support on Figure 15 for this.

As of Figure 15, which is the center of this discussion, I would suggest to keep at depth (along the sectional views) the same colors as the corresponding surface deposits, for an easier comparison of the different panels. Where growth strata (ie most of the time), these should be better highlighted on sectional views. Also, from panels B to C, why/how does the "southern" part of the mountain front in the figure "lose" most of the previous deposits (Q7) before deposition of Q5-Q6, even though only the northern part (where Q7 is preserved) is subjected to higher structural uplift above faults ? How do you explain the (major) change from panel C to D, ie aggradation of the fan Q5 to incisional terraces T4 and subsequent ones ? Hydrological ? tectonic ?

I think more discussion is needed here.

Very good comment. We have removed this section as a detailed sedimentary analysis of the surveyed terraces is needed. It should be provided enough arguments of an alluvial fan in Q5 and establish relationship with the Mesones and Invernada formations. Tectono-climatic considerations would be a rigorous approach to assess the morphos-structural evolution of the study area, but we do not have the data and understandings.

Therefore, we prefer to maintain the main focus of the paper on active faulting evidence and slip rate estimations.

8.3 Slip rate estimates

First and most importantly, you need to estimate structural uplift before quantifying shortening or fault slip. As already mentioned, the incision profiles are (in most cases, if no major hydrological changes) only minimal views of structural uplift because sedimentation at the base level of the river – and fault footwall – is not considered (Lave and Avouac 2000). So, I recommend correcting for this first, as much as possible – or discussing the implications of this simplification if there is no proper way to quantify sedimentation rates.

This is a very good comment again. In sections 5 and 6.1, we describe the geometries of the projected incision profiles. The current Atuel river was used to generate the profiles of Q2 and Q3. We rather consider a concave-up profile from the Principal Cordillera to the Sosneado Fan connecting the two tips of the antiform areas for Q4. We have updated the text in lines 771-773.

Also, we described in more details the amplitude of the uplifted areas (uplift amplitudes, flanks steepness) to show the gradual folding (Figure10).

Also, we specifically mention that the uplift values are considered as minimal du to aggradation upstream and downstream (lines 554-556). We additionally mention in the discussion (sections 8.2) that the shortening and slip rates are minimal (lines 1783-786).

Then the structural model to consider for the analysis may be revised if the final structural section is corrected (see previous comments). In any case the area balance approach (uplifted – and not incised – area vs. area displaced above the fault at the back) is independent of the structural model and should be robust for shortening estimates, as long as the decollement depth is fine – and the uplift (and not incision) pattern is used.

The structural section remains unchanged, and the depth of the decollement is the same. We have removed the calculations for the shallowest segment of the ramps since the splay of the fault reduce the slip on the main ramp. Otherwise, we agree with the reviewer that the shortening and slip rates along the ramp at depth is robust as soon as the reconstruction of the terraces' profile, paleo-rivers and terraces' age are robust. The main uncertainties are on Q5 which is discussed in the text.

I have a hard time evaluating if the model fits well the data as I have the feeling that only model results are shown on figures 16 and 17, and that data are not represented. Could model and data be represented ? in particular for the reconstructions of Figure 17 ? for a better readability, maybe only show the appropriate terrace level for each panel of Figure 17 (and not all levels), playing with a variable vertical exaggeration above the river surface.

The profiles of the terraces are very well constrained for Q2, Q3 and Q4 as display on the incision profiles (Figure 9). Figure 16B shows the reconstructed profiles and we have added the remnants of the terraces in green to the show the main control points. It is possible to increase the vertical exaggeration of Figure 16B but that will make the figures much more complicated as it is now.

Moreover, adding the data on the restorations (Figure 19) would require reorganizing the figure showing the restoration of a single terraces with variable vertical exaggerations. To us, that would make hard to understand the restoration steps in which the oldest terraces deformed with regards to the restoration of the young one.

A graph showing derived incremental horizontal shortening (or average fault slip) and terrace age would be useful (much more than the 2nd column and the last 5 columns of table 3) to discuss rates over time.

Uncertainties on shortening/slip estimates are not discussed… so some of the variability in the rates over time may be an artefact if within result uncertainties.

We are grateful to the reviewer for this comment. We have added the graph as it shows that the incremental displacements rates are consistent for Q2, Q3 and Q4 (Figure 18). Consequently, we conclude that a slip rate of 1.1 mm/yr for the late Pleistocene is a strong result. In addition, despite the slip rate for Q5 is similar, the incremental shortening/slip rate show that this result is more uncertain. We conclude that Q5 age with regards to Q4 and the reconstruction of its profile is misleading. The graph shows a range of combinations for Q5/Q4 age and slip/shortening rates to match the 1.1 mm/yr trend.

8.4 seismic potential

With regards to the comments of both reviewers, we have decided to remove the analysis of the seismic potential of the faults and use the scaling laws to estimate potential magnitudes on the crustal thrust. We are not going through all the comments since we have entirely reconsidered this section. Now we consider that the seismic hazard assessment in the 1st version obscures the strong outcomes of the paper: evidence of quaternary fault activity and the related slip rate estimations. We have therefore reshaped this section into a section 8.3 "*New insights on hazardous fault systems at 35°S*". We consider the San Juan and Mendoza provinces further north, with similar structural styles and slip rates, as a good proxy to estimate that earthquake of magnitudes higher than 7 can occur in the region of Malargüe, along the crustal thrust front of the Principal Cordillera.

We agree with the reviewer #1 to assess that the mean recurrence of the earthquakes, regarding the average slip rate for the late Pleistocene, exceeds the historical records of earthquakes and that the magnitude 6 earthquakes recorded historically in the area should not be considered as a proxy of the maximum magnitudes earthquake.

Figures, tables and citations

Some comments, following the guidelines above:

- If the authors find my suggested re-organisation of the manuscript appropriate, the numbering of the figures will have to change at places. Beware of typos, in figure/table captions.

This is done and we have been careful.

- Tables and figures present data in support of presented results, but, as mentioned previously, I have the feeling that there should be additional points addressed (and therefore illustrated) when using terrace record to quantify Quaternary deformation: more specifically, terrace incision, together with where river aggradation is ongoing is expected to be informative and should also appear in figures.

We have added the Figure 8 with field pictures of the nature of the terraces. Additionally, terrace natures (fill-cut, strath, aggradation) are represented on new profile incisions in Figure 9B.

- As of the color palettes, I believe that Figure 2 should be split in 2 for readility as some colors and symbols used to visually distinguish Cenozoic synorogenic deposits or terrace levels are hard to differentiate and read. Also, I do not get the meaning of the bizarre greenish color over the Malague Basin.

This is a share comments with reviewer #1 and the Quaternary geology is now represented in detail on Figure 5. Figure 2 shows a simplified map of the Quaternary deposits with a unique color (light yellow) for the Atuel River deposits and borwn for the Mesones/La Invernada Fms.

- I noticed that some elements presented in the text are not systematically well located on maps. Some examples:

The seismic line of Figure 3 is 12 km long, the section of Figure 6a is 17 km long, and they both are located over the same line and distance in the map of Figure 2 (where, given the scale, I even have the feeling that the drawn line is longer).

A well is mentioned in the text, only appears in the section of Figure 6 but is not shown and located on the map of Figure 2.

The reviewer is right. Updated trace for the Figures 3 and 6 and the location of the well are now in Figure 2A.

The cross-section shown in Figure 5 is not located on the maps of Figures 1 and 2.

The section in Figure 5 is now in Figure 2B and located in Figure 2A.

The starting and ending points of the river longitudinal profile (Figure 7) is not located on maps.

This has been updated on Figure 5 (ref to Fig. 9A).

- As of citations, I did not notice any specific missing citation, even though I am not particularly familiar with the investigated area. I just noticed a high number of cited abstracts or conference proceedings – ie non-peer reviewed and mostly inaccessible work and (raw) data. I know this is common practice within some communities, I do not know what are the rules for Tektonika, but I do not recommend using these studies as the work presented during a conference talk or poster is not accessible in details (in particular for what concerns raw data to be critically evaluated) and cannot be rigorously discussed.

We have removed all citations refereeing to conference abstract or governmental reports, which are indeed traditionally cited in publications in South American studies.

Additional comments

- lines 79-80: the folding of recent geomorphic markers does only indicate if a fault has been active recently, not that the fault is seismogenic. Indeed, there is no indication on the slip mode (seismic vs creep) from landscape analysis… unfortunately!

We fully agree and that might be the case for the Sosneado Thrust detaching in weak stratigraphic layers. Crustal faults are more prone to be seismogenic as described north of the study area. We meant here that there is a spatial correlation between earthquakes and crustal faults active during the Pleistocene. Therefore they might be active at Present-Day and seimogenic. We now write (lines 72-76):

"*Spatial correlations between seismogenic areas recorded in earthquake catalogues from INPRES, CERESIS, NEIC, and USGS and several crustal-scale thrusts in the southern Central Andes (Costa et al., 2006; Branellec et al., 2016a; Gregori and Christiansen, 2018; Olivar et al., 2023), suggest that crustal seismogenic faults appear to indeed accommodate shortening in the Frontal Cordillera and the Sierras Pampeanas further north (Figure 1B and D).*"

- lines 88-89: the deformation did not originate from the terraces, but was recorded by the terraces.

This was because of miswriting. We have rewritten the sentence as follow (Lines 106-108): "*… we aim to quantify the deformation recorded in the quaternary terraces along the thrust front of the northern Malargüe FTB (Figure 1B and D).*"

- line 95: you determine slip rates but discuss possible estimates of maximum displacement and moment magnitude.

Right. We now only discuss the fact that the Malargüe FTB represents a hazardous fault system (Lines 113-115): *"This allows us to better estimate slip rates along the thrust front of the Principal Cordillera and discuss its hazardous nature."*

- line 153: what is the bracketing numbering technique ?

We have rephrased the text to make this clearer in section 3.1, lines 294-305.

- Lines 234: cumulated scarp, not to be confused with a seismic scarp since 500 m high.

We are not sure to understand this comment.

- lines 245-247: a topographic profile across the maps of Figures 1 and 2 (and added to these figures) could be helpful to better accompany the text.

We have added a new Figure 3 showing perspective views of the thrust front north and south of the Atuel river including topographic profiles. The perspective views are a request from Reviewer #2.

-lines 305-316: shouldn't this be moved to the section about the geological context ?

We compare here our results to previous studies so we think that this should stay in this section.

-lines 364-365: Diamante River is not shown on Figure 2

This line has been removed.

- lines 565-564: no, it implies mostly high incision rates between Q5 and T4, that may relate or not to intense deformation rates. In any case, there is a major change from aggradation (Q5 alluvial fan) to incision (T4 terrace).

The reviewer is right. We now write in the text (lines 722-725): "*With incision up to 200 m-deep of Q4 relative to Q5, as their longitudinal profiles suggest (Figure 9A), such time interval of 4±0.8 ka would imply a considerable incision rate of approx. 50 mm.yr^{-1}, a high rate that raises concerns about the reliability of the ages obtained.*"

Also, we consider that the age of Q5 is not dramatically wrong and can be underestimated up to 20 ka. The restoration of Q5 paleo river bed in Figure 19D shows that a significant period of incision is needed between Q5 and Q4, even if the tectonic uplift is also underestimated.

- lines 589-590: homogenize the writing of interpreted ages: XX-XX (age interval) or XX +/- XX (statistical uncertainty); This is quite confusing.

We have removed the age interval of Q6-Q7 since we do not discuss these last intervals anymore.

- line 642: how do you estimate the geometry of paleo-rivers ? where are these paleo-rivers within the footwall of the fault and therefore what is the total throw across the faults ? for this last point, you need to estimate sedimentation at the river base level / fault footwall.

Good point. We now consider the paleoriver for Q4 and Q5 as a concave-up curve matching the topographic profile outside of the convex areas and linking the two tips of the folded terraces in the fault hanging wall. We have modified the text as discussed above.

- lines 709-716: are all these faults structurally comparable to those studied here ? why would we expect (or not) similar rates ? wouldn't this paragraph better fit within the previous section ?

This is a good comment. In addition, the comparison between the different study areas

- Figure 1: the greyish color for depot-centers is not easily readable. In any case in general I find the terminology "depot-center" very confusing. I understand it comes from the "basin geology" community, who considers the previous initial basins. But today these units are no more basins but rather thrust belts… so calling them "depot-centers" is really confusing for most readers. An alternative: "inverted depot-centers" to please everyone ? or just "thrust belt"…

  I have increased the thickness of the boundaries of these depocenters, and it should be easier to see them on the map.

  I agree with the reviewer that naming "depocenter" without giving an age or its relationship with any tectonic phases can be confusing. It might be hard to understand if the depocenters are old, inverted or stull functioning at Present-Day. The "mesozoic depocenters" are better defined in the text and put back in the regional context in the "*Introduction*" and "*Regional Tectonic Settings*". Their segmentation is key, and they localize the deformation during the Tertiary. Most of the hazardous crustal faults at Present-Day in the Andes are related to their inversion. Therefore, we prefer not to use fold-belts to describe them since this is specifically a consequence of their inversion.

  When referring to those depocenters, we have been cautious to always refer to either "the mesozoic depocenter of…", "mesozoic xx depocenter", or "inverted mesozoic depocenter of xx"…

- Figure 2: What are the "unassigned alluvial deposits" ?

  We have removed that mention from Figure 2 and replace them by "*Piedmont deposits*".

  Sub-levels for T2, T3 and Middle Miocene formations are not distinguishable (see previous comments).

  We have removed them from the map since they are not needed in the text and discussion.

  Here, you use "Principal Cordillera", here and there you mention 'Cordillera Principal" in the text.. homogenize the names and labels for a better readability.

  Thanks for spotting that, we have homogenized the text and the figures and kept Principal Cordillera which is more the English writing (versus Spanish writing).

- Figure 3: add surface geology information above the seismic section, by using your geological map: name and color-code of the formations, possible dip angles measured in the field, thrust faults encountered at the surface in the field or on maps. What do the two red lines represent ?

  We have added the intersection of key surface geology markers above the section (stratigraphic formations, fault traces).

  We have no dip measurements for the specific section measured o the field.

  The red lines are the faults. Straight lines are from both seismic and surface mapping, and dashed lines from seismic only. We have added the fault name for the Meson and Loma Coihueco thrusts.

- Figure 5: other structural interpretations are mentioned in the text. Would it be worth illustrating here these other interpretations ? just to better visualize the structural uncertainties….

  We agree that it would be nice to have them as a figure. Nevertheless, we may have already too many figures and we prefer not adding them since the readers can still refer to the relevant publications. It's just a matter of prioritization: we prefer showing our own interpretations.

- Figure 14: MPT and LGM explicited in the figure caption but they do not appear on the figure. The two first columns indicate the results of this study, but Gosse 1994 cited… quite confusing: this study ? or Gosse 1994 ?

  Sorry for that! A remnant of a previous draft dealing with climatic forcing to explain the terraces morphogenesis. This part have been removed from the submitted paper but there was a left over in the figure caption.

2nd Round of Revisions

Decision Letter
27 March 2024

Dear Grégoire,

Your revised manuscript has now been evaluated by one of the former reviewers (Martine Simoes) and by our associate editor (Jack Williams). See their comments below plus few remarks from me. I agree with Jack's recommendation on the need of another minor revision step to address the comments raised by the reviewer, whereafter we will consider the revised text for publication in TEKTONIKA. Given the limited scope of the requested revision, we would like to receive your revised text and rebuttal within less than about a month, thus before the end of April.

Best regards,

Robin Lacassin

----- Associate Editor (J. Williams) :

Dear authors,

Thank you for the resubmission of your article on the Malargüe Fold-and-Thrust Belt. Following resubmission, one of the original reviewers has examined this revised manuscript. They consider that the this is a significant improvement on the initial submission, however, some revisions are still required prior to acceptance. In particular, some clarification is needed for what is represented in Figure 9, and what the exposure ages of surfaces Q4 and Q4 imply. Having examined the revised manuscript and reviewer's comments myself, I agree with this assessment.

Given the relatively minor nature of the revisions, I therefore ask that you complete these revisions within 1 month (i.e., by 30th April).

Please note that the reviewer has also provided comments on the word document (that you will be able to download from the Tektonika managing system), which you will need to address too.

I look forward to receiving the revised manuscript soon, and thank you again for supporting Tektonika!

Jack

----- Executive Editor (R. Lacassin) additional remarks:

• Title: Avoid using abbreviations like FTB, in title, as they are undefined. Also your title is very long (especially since you have to write FTB in full). Consider shortening it. Although I agree with the reviewer on its importance, I'm not sure it's necessary to mention the flat slab in the title.

• I fully agree with reviewer's comments on the description and meaning of the terraces and on Figure 9. Also consider their comments on Figure 18, and on the conclusion.

• The two tables are complicated. It will be difficult to fit them in the PDF pages. Few readers will need all the details and numerical results they contain. I would suggest to move those tables to supplementary data (in both PDF and XLS formats), and replace them in the core of the paper with a simpler and more synthetic table.

• Although it's not mandatory, you may consider to provide a short plain langage abstract.

• Don't forget to submit all files, including the annexes and supplementary data (absent from the revision). Please also include a .bib file (exported from your reference manager) with all the references.

Comments by Reviewer 2

(Martine Simoes)

First of all, I thank the authors for their extensive revision of their manuscript, which reads now much better and more linearly, with easy-to read-figures. The manuscript is now much more improved and, I believe, almost ready for publication, with still a few needed corrections or complements that I detail hereafter. English is also now much more readable, I only noticed a few typos and mistakes in the headings of the different sections: since the editing of the manuscript is done by the authors and later once the papier is accepted by colleagues that volunteer for it, I proposed some corrections directly in the manuscript in a « track-changes » mode in Word. I also added directly some comments on the sides of the text.

I therefore suggest to go through my comments in the commented manuscript, and only detail hereafter those that I find more important:

=> Introduction and main conclusions of the paper

I appreciate much the way the authors have re-focused their work. The introduction and discussion have been substantially modified. One of the main findings now is that the studied region is actively deforming, as other parts of the backarc foreland further north. According to the authors and previous reviews, such active deformation was dismissed previously because of the steep geometry of the underlying subduction slab. Therefore, the authors should now insist more on this finding in their final conclusion: active deformation (and related seismic hazards) in the backarc foreland is not related to the underlying slab geometry. Whether you have a steep of a flat slab does not change much the seismogenic potential of the backarc faults.

Even though this is presented now as the main motivation of the manuscript in the introduction, the authors do not get back sufficiently to this important finding of theirs in the abstract, discussion and conclusion of the paper.

=> Geomorphology of the Atuel River (section 5)

The authors have nicely simplified the description of alluvial surfaces and things read now much more nicely. However, because this is most important for the later interpretation, a clear description of the nature of the mapped surfaces is missing. As far as I understood from my previous reading of the manuscript and my guess of the revised one: Q0 is the present-day river, Q1 -> Q4 are strath terraces (at least above the Sosneado thrust, fill-and-cut mostly elsewhere) and Q5 is the top surface of an abandoned fan. Is that right ? If so, clearly describe this in the text, do not have the readers only guess it from Figure 9 B.

And from there emphasize more that spatial variability of the nature of these geomorphological features is correlated with underlying mapped faults, meaning that active tectonics on these faults exerts a first-order control on the river response, implying that this river response may be used to get some info on active tectonics. There is some reference to this in the revised manuscript, but I believe not clear enough yet.

Also the different nature of Q5, relative to Q4, is most probably relevant to understand and discuss the very similar ages the authors get for both surfaces.

=> Incision of mapped surfaces (Section 5, figure 9b)

Figure 9B is a very nice figure that illustrates the river response to active fault slip and deformation. However, even though the authors write that Figure 9b represents terrace incision… it cannot be! Incision is by definition the difference in altitude of a surface (top of terrace, top of fan, etc) above the present-day active river. Therefore, from figure 9a, we would expect that terrace incision is very high in the upstream portions of the river (ie from kom 0 to km 12), where the surfaces show a very high residual gradient. In fact terrace incision is highest in these upstream portions - even higher than above the investigated faults… but it appears null on figure 9b!!

My guess (but it's only a guess…) is that the authors represented here the « anomaly » above an a priori paleo-river, and that this a priori paleo-river is estimated from an exponential profile that integrates the upstream high residual gradients of the terraces. This is nowhere explained and detailed, nor how a potential pale-river has been quantitatively estimated.

If my guess is correct, there are some issues here that the authors need to account for. In fact, the high terrace gradients in the upstream portions of the river may have different origins and as such may not be taken as simple proxies for non-deformed paleo-river remnants. In fact if these high gradients (lowering down with terrace ages) were paleo-river remnants, it means that the river gradient dramatically decreased over time, from 4% to 1%. Such decrease could be related to hydrological changes that are in line with the transition from an aggrading river (deposition of Q5 alluvial fan) to an incisional river (terraces Q4 ->Q1). But a gradient of 4 % is quite high for paleo-river Q4, and may be found in the upstream portions of small frontal mountain rivers - not in the relative distal parts of through-going rivers as the Atuel river. So what are/could be the evidence for this ? how is this high gradient comparable to other rivers in the region ? Alternatively part of this high gradient may also integrate broad active uplift in the more internal parts of the mountain range… and be therefore a (partly) deformed feature, not a non-deformed paleo-river.

I understand that the authors may not have all the clues in hand to answer these various questions, but I feel that these questions need to be posed, and these various hypotheses need to be discussed. My suggestions:

- Show surface incision (terrace elevation - present river elevation) as this is a classical thing to do, and is a basic observation to have, together with the various types of river's response (strath, cut-and-fill, aggradation…)

- Point and discuss the upstream high incision.

- Define a paleo-river profile as you most probably did, and explain how you did the calculation. Calculate the elevation of the surfaces above this theoretical profile (but do not refer to this as « incision » but as something like « height above theoretical paleo-river »). This could be a figure 9c.

- If you can't have clues on the precise origin of the residual upstream gradient, discuss then how the various hypotheses on this origin impact you estimate of the « height above theoretical paleo-river » and your interpretation of it.

=> Ages of Q4 and Q5, and implications

Even though Q4 and Q5 have a high elevation difference above the present-day river, the estimated ages are not much different. As pointed out by the authors, there could be issues in the dating of Q5, which would be expected to be much older. Or not.

Take the fact that the ages are correct. This means that incision has been dramatic from Q5 to Q4, an idea that does not seem to please authors. However, if Q5 is an alluvial fan surface and Q4 a strath terrace, this is not unlikely. Indeed, the abandonment of the aggrading fan (Q5) is expected to be followed by a transient period of high incision into the alluvial deposits… before hitting bedrock and incising into it to form strath terraces Q4 to Q1. To my sense not unlikely, just a representative of the transient incision during the transition from aggradation to incision. Then the question would be: why this transition ? if climatic, it should be observed regionally. Otherwise, it could be related to (local) changes in the upstream drainage system (captures, etc). In any case, probably something to relate to the observed residual gradients.

The authors have also proposed the possibility that such high transient high incision rates reflect increased slip rates on faults, and therefore higher seismic activity on the faults. I do not think this is a reasonable explanation: indeed, the high incision rates needed between Q5 and Q4 are to take place everywhere along the whole river profile (Figure 9), and are not found locally only above faults. This is a large-scale feature, and does not fit the spatial wave-length of faulting.

=> Figures: I also noticed a few details to be corrected (missing citations, missing locations, missing explanations in the caption…). Please refer directly to the commented manuscript.

More specifically about Figure 18 : I thank the authors for adding this figure, as an effort to answer some of my previous comments. But I apologize to say that the figure is not much more clear than the previous table… Indeed, why representing interval rates, vs durations of these intervals ? why not simply and directly calculated displacement (from figure 17 - ex for Q4: 74 m) vs surface age (ex for Q4: 71 ka) , with uncertainties on both parameters ? the shortening rate (and its variations over time) would then directly read from the slope of the curve. This is a very classical way to show things (ex figure 12 of Lave and Avouac 2000) and I do not know why the authors prefer this more complex view of the data.

I apologize for being picky about some things… I hope that my comments will be useful to correct for these few last things, and from there to strengthen the results obtained here. I attached a commented manuscript to this review.

I am looking forward to seeing the paper published and the documented recent fault activity further considered in regional hazards.

Authors' answers to reviews

Hi,

Many thanks for this very good revision. We have answered to all comments and modified the text accordingly. Below are some answers to the main questions. You will find detailed answers to the comments in the Word documents itself.

Note that we have slightly updated the Appendix accordingly to recent modification in the TCN dating sections. Also we have added a Figure 20 to clarify how we estimate shortening rate on the crustal ramp west of the thin-skinned Sosneado Thrust.

----- Executive Editor (R. Lacassin) additional remarks:

• Title: Avoid using abbreviations like FTB, in title, as they are undefined. Also your title is very long (especially since you have to write FTB in full). Consider shortening it. Although I agree with the reviewer on its importance, I'm not sure it's necessary to mention the flat slab in the title.
We agree with this. We came back to the title of the first version.

• I fully agree with reviewer's comments on the description and meaning of the terraces and on Figure 9. Also consider their comments on Figure 18, and on the conclusion.
We agree. See comments to M. Simoes.

• The two tables are complicated. It will be difficult to fit them in the PDF pages. Few readers will need all the details and numerical results they contain. I would suggest to move those tables to supplementary data (in both PDF and XLS formats), and replace them in the core of the paper with a simpler and more synthetic table.
Done. We have removed the Table 1 from the plain text and simplified the Table 2. The full versions are in the supplementary materials in .pdf and .xls.

• Although it's not mandatory, you may consider to provide a short plain langage abstract.

• Don't forget to submit all files, including the annexes and supplementary data (absent from the revision). Please also include a .bib file (exported from your reference manager) with all the references.

----- Reviewer's comments:

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I therefore suggest to go through my comments in the commented manuscript, and only detail hereafter those that I find more important:,
See the Word document to our answers to all individual comments.

=> Introduction and main conclusions of the paper

I appreciate much the way the authors have re-focused their work. The introduction and discussion have been substantially modified. One of the main findings now is that the studied region is actively deforming, as other parts of the backarc foreland further north. According to the authors and previous reviews, such active deformation was dismissed previously because of the steep geometry of the underlying subduction slab. Therefore, the authors should now insist more on this finding in their final conclusion: active deformation (and related seismic hazards) in the backarc foreland is not related to the underlying slab geometry. Whether you have a steep of a flat slab does not change much the seismogenic potential of the backarc faults.

Even though this is presented now as the main motivation of the manuscript in the introduction, the authors do not get back sufficiently to this important finding of theirs in the abstract, discussion and conclusion of the paper.

We 100% agree with this (see answers to the comments in the manuscript). We have been more specific in the Introduction, in the last section of the discussion and in the conclusion. We still note that despite Quaternary tectonics have been observed as far as 38ºS alongside with limited earthquakes of magnitudes <5, the present-day fault activity and seismic activity tend to decrease southward.
We have mention this in the last paragraph of the discussion.

=> Geomorphology of the Atuel River (section 5)

The authors have nicely simplified the description of alluvial surfaces and things read now much more nicely. However, because this is most important for the later interpretation, a clear description of the nature of the mapped surfaces is missing. As far as I understood from my previous reading of the manuscript and my guess of the revised one: Q0 is the present-day river, Q1 -> Q4 are strath terraces (at least above the Sosneado thrust, fill-and-cut mostly elsewhere) and Q5 is the top surface of an abandoned fan. Is that right ? If so, clearly describe this in the text, do not have the readers only guess it from Figure 9 B.

To be entirely honest, we haven't worked enough on the sedimentology of the alluvial deposits to elaborate too much on their interpretation. We therefore named all units from Q1 to Q5 as alluvial terraces. The word "terrace" does not have a very precise meaning. It refers primarily to a landform (flat) as observed on the field.
In addition, we initially interpreted Q5 as an alluvial cone. Indeed, it appeared to be 100 meters thick at the foot of the Cordillera, with metric-sized subangular blocks, and 30 meters thick at the Sosneado thrust with pebbles up to 30 cm. However, we were challenged by the Reviewer 1 (Carlos Costa) to show all the details, which we do not have. Furthermore, Q5 could also be nested within the glacial alluvial fans of the Middle Pleistocene (La Invernada and Mesones Formations). Therefore, we have decided to no longer consider this surface as an alluvial fan but rather to leave its nature open to question and refer only to an alluvial terrace.
Nevertheless, in the text and in now Figure 9C (former 9B), we tried to be more specific on the nature of each alluvial terrace with regards to their location along the stream.

And from there emphasize more that spatial variability of the nature of these geomorphological features is correlated with underlying mapped faults, meaning that active tectonics on these faults exerts a first-order control on the river response, implying that this river response may be used to get some info on active tectonics. There is some reference to this in the revised manuscript, but I believe not clear enough yet.

We have been more specific on that in the last paragraph of the section 5. We discussed the role of the uplift on the river morphodynamics and the morphogenesis of the strath terraces above the Sosneado Thrust, and the upstream (large valley) and downstream (Sosneado alluvial fan) aggradations. So it should be clearer now that the uplifts on the Sosneado Thrust have impacts on river and its deposits morphogenesis.
We also kept a mention of this in the first paragraph of the Discussion.

Also the different nature of Q5, relative to Q4, is most probably relevant to understand and discuss the very similar ages the authors get for both surfaces.
We did not discuss much this relatively to the lack of field observations to discuss their nature (see above comment). Nevertheless, we eventually demonstrate in the discussion that a significant hydrological incision happened between the deposition of Q5 and Q4. We discuss the potential role of mechanically weak (then easily erodable) Q5 alluvial fan, or more climatic (shift from MIS 5-Q5- to MIS 4-Q4) or autogenic process. We did not discuss this in details because we have no arguments, but we mention them as possible explanation to discuss this intense incision.

=> Incision of mapped surfaces (Section 5, figure 9b)

Figure 9B is a very nice figure that illustrates the river response to active fault slip and deformation. However, even though the authors write that Figure 9b represents terrace incision… it cannot be! Incision is by definition the difference in altitude of a surface (top of terrace, top of fan, etc) above the present-day active river. Therefore, from figure 9a, we would expect that terrace incision is very high in the upstream portions of the river (ie from kom 0 to km 12), where the surfaces show a very high residual gradient. In fact terrace incision is highest in these upstream portions - even higher than above the investigated faults… but it appears null on figure 9b!!
Yes, that was a mistake. We kept the 0-12 km data on the incision profiles (new Figure 9B) but we removed this data in the profiles of residual topographic anomalies related to a tectonic uplift (new Figure 9C- former 9B). For this latter, the comments are right: we can reconstruct the paleo-river bed in the Principal Cordillera since the residual gradient of the alluvial remnants can also register a certain amount of large scale tectonic uplift.

My guess (but it's only a guess…) is that the authors represented here the « anomaly » above an a priori paleo-river, and that this a priori paleo-river is estimated from an exponential profile that integrates the upstream high residual gradients of the terraces. This is nowhere explained and detailed, nor how a potential pale-river has been quantitatively estimated.
This is indeed how we determined the paleo-river bed. Thanks to the reviewer comments, we concede that we cannot use the profile in the Principal Cordillera to determine the paleo-river bed. Therefore, we now consider the paleo-river as a straight line linking the tips of the antiform determined in the profiles of the alluvial surface. We think that this gives a good approximation and the shortening and uplift calculation give the same results.
We detailed the method in section 5 (lines 550-566) and in Figure 9C, and we discuss quickly why we do this way regarding the method of Lavén and Avouac (2000).

If my guess is correct, there are some issues here that the authors need to account for. In fact, the high terrace gradients in the upstream portions of the river may have different origins and as such may not be taken as simple proxies for non-deformed paleo-river remnants. In fact if these high gradients (lowering down with terrace ages) were paleo-river remnants, it means that the river gradient dramatically decreased over time, from 4% to 1%. Such decrease could be related to hydrological changes that are in line with the transition from an aggrading river (deposition of Q5 alluvial fan) to an incisional river (terraces Q4 ->Q1). But a gradient of 4 % is quite high for paleo-river Q4, and may be found in the upstream portions of small frontal mountain rivers - not in the

relative distal parts of through-going rivers as the Atuel river. So what are/could be the evidence for this ? how is this high gradient comparable to other rivers in the region ? Alternatively part of this high gradient may also integrate broad active uplift in the more internal parts of the mountain range… and be therefore a (partly) deformed feature, not a non-deformed paleo-river.
We agree that these gradients are highs. We noticed from observations not shown here that there is a clear backward migration of a knickpoint registered by level Q1-Q4. We assume that there is a strong regressive erosion linked to variation in the base level. Our guess is that there was a reorganization of the drainage network related to the incision of the Mid-Pleistocene fans coming from the north and extending in our study area. But we agree that a large-scale uplift in the Principal Cordillera could contribute to such a gradient. We do not want to discuss this mater and we prefer not considering these profiles of the alluvial remnants in the text. We state this lines 562-566.

I understand that the authors may not have all the clues in hand to answer these various questions, but I feel that these questions need to be posed, and these various hypotheses need to be discussed. My suggestions:

- Show surface incision (terrace elevation - present river elevation) as this is a classical thing to do, and is a basic observation to have, together with the various types of river's response (strath, cut-and-fill, aggradation…).
We have included a new figure of the incision profiles in Figure 9B. The various types of alluvial deposits are displayed in Figure 9C because they are not uniforms among the alluvial units.

- Point and discuss the upstream high incision.

As said in the above comment, we do not want to discuss much this here. Nevertheless, we mention this high gradient and mention in lines 562-566 that hydrological, climatic and crustal scale tectonics may play a role.

- Define a paleo-river profile as you most probably did, and explain how you did the calculation. Calculate the elevation of the surfaces above this theoretical profile (but do not refer to this as « incision » but as something like « height above theoretical paleo-river »). This could be a figure 9c.
We have done this with the new Figure 9C and we explain how we get to this profile in lines 550-561 and in Figure 9C.

- If you can't have clues on the precise origin of the residual upstream gradient, discuss then how the various hypotheses on this origin impact you estimate of the « height above theoretical paleo-river » and your interpretation of it.
As mentioned above, we do not use an exponential theorical paleo-river but a straight line between the two tips of the folded top of the abandoned alluvial surface.

=> Ages of Q4 and Q5, and implications

Even though Q4 and Q5 have a high elevation difference above the present-day river, the estimated ages are not much different. As pointed out by the authors, there could be issues in the dating of Q5, which would be expected to be much older. Or not.

Take the fact that the ages are correct. This means that incision has been dramatic from Q5 to Q4, an idea that does not seem to please authors. However, if Q5 is an alluvial fan surface and Q4 a strath terrace, this is not unlikely. Indeed, the abandonment of the aggrading fan (Q5) is expected to be followed by a transient period of high incision into the alluvial deposits… before hitting bedrock

and incising into it to form strath terraces Q4 to Q1. To my sense not unlikely, just a representative of the transient incision during the transition from aggradation to incision. Then the question would be: why this transition ? if climatic, it should be observed regionally. Otherwise, it could be related to (local) changes in the upstream drainage system (captures, etc). In any case, probably something to relate to the observed residual gradients.
We consider that the ages of Q4 and Q5 are correct but regarding the uncertainties of the TCN dating, a maximum difference in age between the two levels is 20 kyr.
We perfectly agree that it must be an intense incision between Q4 and Q5. In Figure 19, we show the restoration the alluvial terrace (unfault and unfold - using results of the analytical solution). The restoration of Q5 shows that, what ever the uncertainties in the reconstruction of its profile or even with a maximum difference in age between Q4 and Q5, an incision rate between 4 and 22 mm/yr is required after removing the tectonic component. We conclude that either autogenic processes have played a role in the, or the transition between the interglacial MIS 5 (Q5) and MIS4 (Q4) may also have played a role (lines 931-940).

The authors have also proposed the possibility that such high transient high incision rates reflect increased slip rates on faults, and therefore higher seismic activity on the faults. I do not think this is a reasonable explanation: indeed, the high incision rates needed between Q5 and Q4 are to take place everywhere along the whole river profile (Figure 9), and are not found locally only above faults. This is a large-scale feature, and does not fit the spatial wave-length of faulting.
This is right, we have removed this part of the discussion.

=> Figures: I also noticed a few details to be corrected (missing citations, missing locations, missing explanations in the caption…). Please refer directly to the commented manuscript.
Sorry for that. We have made cleared that.

More specifically about Figure 18 : I thank the authors for adding this figure, as an effort to answer some of my previous comments. But I apologize to say that the figure is not much more clear than the previous table… Indeed, why representing interval rates, vs durations of these intervals ? why not simply and directly calculated displacement (from figure 17 - ex for Q4: 74 m) vs surface age (ex for Q4: 71 ka) , with uncertainties on both parameters ? the shortening rate (and its variations over time) would then directly read from the slope of the curve. This is a very classical way to show things (ex figure 12 of Lave and Avouac 2000) and I do not know why the authors prefer this more complex view of the data.
Sorry, there was a misunderstanding in the first review and we think that we should emphasize more on the incremental results from Table 3.
We have used a similar diagram than the Figure 12 of Lavén Avouac (2000). We use this diagram to discuss the uncertainties on Q5's age and cumulative shortening estimation.

I apologize for being picky about some things… I hope that my comments will be useful to correct for these few last things, and from there to strengthen the results obtained here. I attached a commented manuscript to this review.
The paper will be significantly improved thanks to all those "picky" comments. Thanks for taking the time to do this extensive review!

I am looking forward to seeing the paper published and the documented recent fault activity further considered in regional hazards.

3rd Round of Revisions

Decision Letter

(10 May 2024)

Dear Grégoire and co-authors,

Thanks for submitting your revision. We have carefully evaluated the rebuttal, and the changes done to the manuscript about the terraces description and the calculation of incision and uplift (e.g., Fig 9). We also asked the former reviewer (Martine Simoes) for a rapid evaluation, and she's satisfied with your answers and modifications on these subjects.

In the revised manuscript, you have also added a new section that relates the local shortening rates on the Sosneado Thrust to the larger thrust system (as depicted in Fig. 20). This is an interesting subject indeed, but we are sorry to say that the reasoning you make on page 59 (revised manuscript) and the new figure seems to us completely off base. Transferring it to the Himalayas for example, it's like to say that the ~2 cm/yr on the frontal thrust would resume to 0.2 cm/yr on the deep crustal thrust ramp, which would be completely odd. In fact, if our understanding is correct, you're doing a calculation that we would describe as squeezing toothpaste from a tube: slowly squeezing where the tube is wide (in your case above the deep crustal ramp) makes the paste run out much faster where the tube is thinner (on the Sosneado thrust). Pushing this reasoning further, at the very emergence of the frontal thrust you would get an absurd infinite rate. Think to another analogy like a pressure washer (Kärcher). This is fluid flow within a conduct of varying section. But the crust is not a fluid pushed by a piston through a conduct confined between something rigid at the base and a rigid, not permeable, lid at the surface (implying no surface deformation nor uplift, no relief building, no erosion, no exhumation, etc…). Such fluid flow mechanism is not applicable to fold and thrust tectonics. As far as we know nobody uses it. If you had applied it to your frontal fault, you wouldn't have formed a fold, nor deformed and uplifted the terraces because everything would have been blocked by the rigid and impermeable surface cover.

In FTB tectonics, a more standard approach would be to either: (1) keep the slip constant on the basal fault from its emergence at the surface to the deep crustal ramp – meaning that the horizontal shortening locally depends on the dip of the ramp or décollement beneath – or (2) you keep the horizontal shortening constant – and the slip on the fault beneath varies, again depending on its dip angle. In either case, the fault slip or horizontal shortening will be constant whilst the other parameter can vary based on the geometry of the underlying fault. However, the degree of these variations will not be in a proportion of 1 to 10. This is simple trigonometry.

We thus ask you to revise this section of the manuscript. You may choose to simplify it a lot while adopting a correct tectonic approach. Or, as you are not really using the calculated rates in the following discussion and conclusion, you may alternatively choose to simply delete this section and figure.

Your paper will be a really fine contribution to Tektonika, but we cannot accept it without correcting this important issue. We hope you will be able to revise it rapidly. No need to upload again all the different files, but be sure to clearly label your new revision and rebuttal.

All the best

Robin Lacassin, EE Tektonika
Jack Williams, AE Tektonika

Authors' answers to reviews

Hi Robin,

Thanks for your reply and relevant comments, once again.

I have resubmitted the manuscript following your recommendations.
The text and its related Figure 20 have been removed from the text.

We keep saying that the shortening rates obtained in this study is comparable to other studies in the Malargüe area and in the Pampean flat slab area.
We only mention that the shortening rate on the crustal ramp is equal or higher than the rates estimated on the Sosneado Thrust, taking into account that several additional thrusts should be taken into account.

I hope that this will be ok.

Best regards

Grégoire Messager et al.

Final decision

(23 May 2024)

Dear Grégoire,

Thanks for your final revision that take into account our last comments. We have accepted your manuscript today.
You will be contacted by our copy editing team in the coming weeks.

Thanks for submitting to Tektonika !

Robin Lacassin, Tektonika Executive Editor
Jack Williams, Tektonika Associate Editor