Dear Reviewers, dear Editor,

Following the reviewers' constructive and insightful comments, we have substantially revised and reorganized the manuscript. The main modifications are summarized below, and detailed point-by-point responses are provided afterwards.

- The Introduction has been revised to clarify more clearly the motivation, scope, and strategy of the study, including the rationale for the mantle-only configuration and for the weakening diagnostics introduced here. We also rebalanced and expanded the reference framework.

- The Results section has been substantially reworked. We added a short preamble to clarify the structure of the section; introduced a new appendix figure defining the criteria used to classify localization outcomes across simulations; revised the figure and presentation of the two-stage localization analysis; added a new appendix figure showing the temporal evolution of thermo-mechanical vertical profiles, which complements the theoretical profiles discussed in the main text; moved the former Fig. 11 to the Appendix; and reduced repetition in the final Results subsection. We also corrected a mistake in the report of "D-$Y_{(500\ \mathrm{MPa},\ 950\ \mathrm{K})}$" simulation results in Table 3, where the final state outcome was "inc. loc" instead of "distr def".

- Following the Referees' pieces of advise, additional simulations have been performed to strengthen the interpretation and test the robustness of our conclusions, including simulations with depth-dependent yield stress and with temporally variable extension boundary conditions. Some technical material has been moved from the Appendix to the Supplementary Material, which now also includes further analyses supporting our responses to the reviewers' comments, for example regarding boundary conditions, dynamic pressure, and a posteriori estimates of shear heating.

- The Discussion has been extensively restructured. Section 5.1 now begins with the two-stage localization pathway and its comparison with previous numerical studies and selected natural rift systems. Sections 5.2 and 5.3 then compare localization feedbacks across rheological parameterizations and discuss the interpretation of models without dislocation creep or with vertically uniform yield stress, including whether yield stress can be regarded as a proxy for low-temperature plasticity and how the results might change with a more realistic shallow brittle rheology. Section 5.4 emphasizes the two main implications of the study: first, that localization timescales may be overestimated in the absence of dislocation creep; and second, that the new weakening diagnostics (F_T and F_SR) may be useful in other geodynamical contexts and for rheologies with additional state-dependence. We also discuss the principal limitation of the present study, namely the absence of crustal layering.

- We have refined the language throughout the manuscript, with particular attention to the Abstract, and we now include in the Appendix the simulations featuring depth-dependent yield stress.

Our answers are colored in blue, in the following, we indicate the line numbering from the marked-up manuscript version.

<u>Reply to Referee 1, Antonio Manjón-Cabeza Córdoba:</u>

Dear editor,
this technical paper presents a series of numerical experiments to address the role of rheology on deformation localization during extensional tectonics. While several detailed models of extension and rifting exist, evaluation of localization and adequate rheological mechanism is mostly absent in modern literature. In addition, they add the role of low-temperature plasticity, a continuation of their own work (Gouriet 2019; Garel 2020)
They show that considering multiple rheological mechanisms, in particular non-linear rheologies, is important to reproduce localization processes as we imagine them on Earth. They could specify a bit more on whether this was ever called into question, or what are the consequences of ignoring one rheology or the other, not just for their models, but for the wider community that will be forced to take simplifying assumptions regardless.
The manuscript is quite technical but the methodology is well explained. I have little to say there.

Because Solid Earth has a wide focus - inasmuch as the deep Earth can be called a wide field – the article would benefit for a slightly more careful contextualization (e.g. what advances or new evidence motivate this study? How does this work compare with other studies? What are the implications for other geophysicists/geologists? etc).
The introduction has been rewritten to reinforce the motivation of the study, and provide a more comprehensive context. We hope that the paper now clearly present the study's objectives, the methodological approach, and a brief outine of the paper's structure.

But this is not a criticism of the science of the manuscript itself.
Overall, I have few important comments to make and the majority of them are oriented to increase the reader interest on the manuscript rather than a criticism of the science of the manuscript.
We thank you a lot for these comments, which indeed greatly helped to improve how the main messages the manuscript are conveyed.

As a matter of taste, I find that they describe too many details of their results; these are simplified far-from-reality numerical experiments, and describing too many details may have little relevance. Perhaps summarizing a bit would improve the manuscript (i.e. in some sections there are some details that would be obvious to most people, like with faster extension comes faster thinning).
The Results section 4.4 has been tightened, with section 4.3 streamlined to focus on the main contributions of our study. We have added a short preamble to the results section outlining the key points, and improved transitions for smoother read.

First you can find the main comments to be addressed by the authors, below you can find a series of minor suggestions that the authors are free to address or not, or even ignore during review/reply process.

Formatting
I'm sure this will be fixed at production stage, but please check spacing between paragraphs.
Extra paragraph spaces: 268, 294, 305, 324, 351, 361, 364, 371, 408, 441,460,481,502,519, 537, 556, 561. At least those, I think. If these are intentional, then correct paragraphs without break (e.g. 465-466; 469-470…)
The revised manuscript formatting has been carefully reviewed to remove most of unwanted spaces.

Abstract
I hate to make this comment (I myself am not a great writer), but please revise the language of the abstract. Some constructions are confusing. I have tried to suggest some changes, but I am not a native speaker and at least some of the coauthors are. After reading the whole manuscript I have noticed this is something very punctual, and that the quality of the writing is good overall. Probably the abstract was written last, and a quick revision would make the manuscript much more attractive.
The abstract has been revised by a native English-speaking author to enhance the quality of the writing.

Introduction
One of the highlights of the manuscript is a new way to measure localization. Beyond the weird wording (we were inspired by -> we build on?), it seems that the main analysis of localization goes

through the new diagnostics described in the manuscript. And yet, the justification pertaining to this new diagnostic is quite lacking. What was wrong with previous ways to measure localization? What new information does this diagnostic bring? This is something that can be addressed in 2-3 lines as a motivation in the introduction section, and would give way to another couple of sentences in the discussion defending the diagnostic role on improving our knowledge of localization (rather than justify the diagnostics a posteriori).

Thank you for this very constructive comment. We have added such clarification in the introduction to emphasize the need for time-dependent diagnostics tracking over time and space both the viscosity decrease and its underlying thermal and mechanical control (lines 101-103), and now explicitly state that this study addresses weakening processes within the lithospheric mantle (lines 115-116).

Please, check my comment below on the references.

This comment has been taken into account (please see below).

Methods

The methods are very thorough in the description of the model, which I appreciate. I would appreciate a plot showing that the Arrhenius description of high-T dislocation creep converges at high T with the tan-description of low-T-high-T dislocation creep. Otherwise, we could be comparing apples with oranges. But this can be left for the supplementary material or one of the appendixes (note to editor: the equations are described well enough that the reader can plot this him/herself).

The similarity between high temperature (HT) dislocation creep and low- to high-temperature (LT-HT) dislocation is now explicitely stated in section 2.2.2 (lines 220-222), since it is suggested by the vertical viscosity profile in Fig. 9 (Sect. 4.3). This convergence at high temperature is also more clearly illustrated in a new figure S12 in the Supplementary Material S.1.3.3), which compares LT-HT and HT dislocation creep used in our study to other parameterizations.

Lines beyond 215: I think this definition would be clear(-ish) to most geodynamicists but there are better ways to write this. As it stands, one could argue that if Ft and Fr add to 100% (which they don't necessarily do unless normalized, which it is not explicit in the formulas), then hardening on one of the parameters (Ft or Fr) should result in a negative percentage in one of the parameters and a value greater than 100% in the other (white areas in figures 6, 11?). Then the reader may or may not reach the (incorrect or correct) conclusion that the authors use the absolute values of the partial derivatives in equation 14 (not explicit) and in equation 15, so that the values are necessarily constrained to 0-100, but then being unable to distinguish between weakening or hardening (not to mention that Delta eta/ Delta t would not be representative of deta / dt). I can't help but feel I am overthinking this, but the authors could help the reader by being a bit more explicit/explanatory.

Thanks a lot for pointing this ambiguity in the definition of F_T and F_SR. We now clarify in Section 3.3 that F_T and F_SR are only computed in regions undergoing weakening, and that one of the two may exceed 100 % even if F_T + F_SR = 100%. We detail in Figure 8 (and other figures using the same color-coding Figs. D1 and D2) that the F_SR colorscale is saturated between 0 and 1. A simulation where F_T<0 and F_SR > 1 is now mentioned in section 4.4 and Appendix D (lines 642-643 and 1240-1242, respectively) in some cases of low extension rates: thermal hardening due to plate cooling competes with mechanical weakening associated with plate extension.

Results

The results are very thoroughly described. Little to add here. If anything, I'd suggest to simplify the results. Obviously, the authors can present a lot of good data, but I wonder how much of it is needed to justify the conclusions of the paper. To some extent, some of the data could be a bit of an overkill (See number figures/panels in Figure 8, 10, etc). In general I would summarize section 4.3 (it was difficult to read, in addition, although to be fair that could be my own limitations).

We agree that the result sections were sometimes too detailed with regard to our main conclusions. We extensively rewrote the Results section. In particular section 4.4 has been significantly shortened, and a short preamble now presents the section structure. Section 4.3 presenting the influence of the different rheologies brings the main results of the study. We have added the new figure C1 in Appendix C (new appendix section) to highlight the importance of a thin stiff layer, a message that is further interpreted in the Discussion section 5.3.

Discussion

We restructured the discussion section, with a short preamble presenting its new outline:

5.1 A two-stage pathway to plate-boundary formation

5.2 Importance of enabling thermo-mechanical weakening through dislocation creep

5.3 Implementing yield-stress or LT-dislocation creep as ductile strength-limiters

5.4 Implications for geodynamical models and limitations of the present study.

Note that the section numbers mentioned in the reviewer's comments may therefore have changed.

Limitations

The authors find that dislocation creep and yield stress are not interchangeable, which implies that both, non-linear rheology and a yield stress are necessary to reproduce rifting/ridges scenarios. This, however obvious it may seem, is an important result that greatly aids the geodynamics community (I am opinionated in this regard). But it turns out that the only existing physical process close to our models' yield stress is brittle failure. Our continuum models are inherently limited to volumetric (as opposed to planar) failure, and therefore can only reproduce this form of deformation approximately. While I don't believe so, it could be argued that considering planar failure would change the results. In addition, the classical approach to yield stress does not converge at the resolution of our models (although the relevant associated parameters may indeed converge), and more advanced approaches may give different results (Duretz et al., 2020; 2023). These considerations are arguably more important than the limitations highlighted in the discussion section at the preprint stage, and I suggest adding something along these lines. Nonetheless, I'd like to point out that these minor details do not disqualify the calculations of the paper.

This is an important remark, thus we have added a substantial discussion paragraph (5.4, lines 1041-1080).

We now discuss how the yield stress does not fully capture the brittle failure process that should produce discontinuous bands where deformation would be focused, which is not observed in our experiments. We acknowledge that this simplification could change the deformation pattern close to the surface, still we expect that these effects would not affect our main result at first order. Because the latter are focused on ductile deformation patterns occurring in the mid- to sub-lithospheric layers, where creep flows largely dominate. Visco-elasto-(Visco)plastic rheologies simulate deformation patterns related to brittle failure much more accurately, as pointed by Referee 1, which should result in a quite different distribution of the shallow deformation. These processes and effects are now discussed in the last part of Section 5.4 where some limitations of our modeling approach are detailed (lines 1059-1065). We are convinced that the scenario of strain localization modeled in our experiments as well as the effect of including dislocation creep, promoting strain localization in a narrow zone at lithospheric scale, remain robust results.

The cases with yield stress include a constant yield stress with depth. I believe this is a major limitation that should be discussed. As with the previous paragraph, the only physical mechanisms similar to the yield stress used here is the brittle failure, whose friction coefficient implies an increase in the strength with depth. Note that I am not suggesting that the model setting is not adequate (this simplification may aid in the disentangling of the different mechanisms/weakenings), but the limitation itself needs to be discussed.

Thank you for pointing out this important limitation in the initial submitted manuscript. To address this point, we performed two additional simulations featuring a depth-dependent yield stress (Appendix E), choosing to keep the same « peak » stress as in the constant 500-MPa yield stress simulations(discussed in sections 5.2, lines 832-838, and 5.3, lines 890-891, 900). These tests confirm that the localization duration and pattern is similar to a constant yield-stress parameterization if the peak lithospheric strength remains the same, whatever the shallow values – for both D-Y and D-d_{LT-HT}-Y rheologies. Only minor differences in topography and stresses are observed (Supplementary Material S2.1, Figs S15 & S16).

Implications

While I know this is not the objective of the authors, it would be of benefit to the paper to include a couple of paragraphs on how the inclusion (or exclusion) of certain rheologies may influence more realistic models. I am always puzzled when authors claim to match real systems even when ignoring/neglecting key pieces of the puzzle. In this case, I am sure low-T plasticity has been ignored in most of the literature. How does low-T plasticity influence wide or narrow rifting? What may classical models be underestimating/overestimating? This would be much more interesting to the rather than "[…] the yield stress parameterization in our models approximates a mechanical coupling […]" of specific models, which seems to me more a sentence to justify the model setting than implications (Note: IMO we should always design the models with an experiment/model in mind, not run the model and then find a suitable application; this may be too idealistic, the authors may disregard).

While it is not straightforward to predict the impact of mantle (high- or low-to-high temperature) dislocation creep on continental rifting dynamics, we tried to discuss in section 5.4 the expected feedbacks and consequences on the overall lithospheric behaviour in terms of deformation localization of such rheologies compared to the sole combination of {diffusion creep + yield-stress} (lines 949-959). We also provide a more detailed discussion on how LT-dislocation creep would act as a ductile strength-limiter comparable to yield-stress (discussion section 5.3, in particular lines 913-934).

As it stands, a lot of the implications sound more like a review of current knowledge of rifting, with too loose of a relation with this manuscript's experiments (e.g. in lines 538-545, whether this work would have predicted one thing or the other, this paragraph could be written exactly the same regardless of the results).

Thank you for this comment. Indeed both reviewers noticed that this part of the initial discussion was quite far away from our take-home messages. We removed the previous "*review of current knowledge of rifting*", and now only discuss the two-stage localization scenario observed in some natural rift systems (section 5.1, lines 719-745), and the limitation of the absence of a continental crust in section 5.4 (lines 997-1033).

Conclusions

I disagree with the use of the word demonstrate. While this word does not necessarily need to mean the same as in a math paper, I don't think these numerical experiments can demonstrate anything, not without discarding other theories/mechanisms/processes, as with any experimental work. What the calculations here described show is that within the assumptions of the model, we can discard one or two rheologies, at least to explain by themselves some form of localization. That does not amount to a demonstration to me. Just to clarify, the paper does demonstrate that complex rheology is indeed a good way to localize deformation, but this was never into question (as far as I know) and it does not demonstrate that other mechanisms not here considered are not viable. To sum up, without considering other mechanisms/processes this study cannot demonstrate "[…] how lithospheric weakening under extension emerges […]", just one of several possibilities.

Sorry for the lengthy paragraph, what I meant is the following: consider changing "demonstrates" for "shows" or a similar, less-charged word.
We deleted the term "demonstrate", and rephrased the conclusions.

The sentence "models restricted to diffusion creep plus yield stress tend to overestimate the duration of strain localization and the timescale of plate break-up" is perhaps the most important conclusion of this work, or at the very least the most impactful. Consider discussing a bit more about this in the discussion (e.g., could the authors find some works where they think this timescale has been overestimated? if so, consider discussing them, but I understand if the authors consider this too conflictive and therefore not suitable for this article).
Thank you for this constructive comment. The whole discussion have been restructured to clarify the scientific conclusions of this paper. Specifically, we highlight the overestimation of the timescale plate breakup in the discussion and 5.4. (lines 949-951). Regarding the timescale comparison, it appears that it is difficult to establish the "right" timescale. Based on our results, we expect that including dislocation creep may greatly reduce the timescales of deformation localization (by a factor close to 2). Still, the direct comparison to simulations modeling the behavior of plate tectonics in large scale mantle convection models is somewhat tricky, as dislocation creep also promotes sub-lithosphere weakening by rising strain rates locally. The latter may in turn decrease the mantle-lithosphere coupling and hamper plate-like behavior in convection models (e.g., Arnould et al., 2023, see Sect 5.4, lines 949-959).

References
The cited references are a bit unbalanced.
I understand the affinity argument for citations, but I feel the citations are a bit too close to home. Although I am partial to succinct manuscripts, and the current citation number stands at ~120, I feel like the manuscript could be a bit more balanced in terms of research groups (and/or nationality of the cited researchers). I don't want to be overly disingenuous, though, and, to me, featuring more or less references is not a reason for which to reject this manuscript. References included are not just a way to justify claims, but also a way for the reader to catch up with the basics needed for reading this manuscript: they would benefit for a wider coverage of the topics. My recommendation is that the authors expand on the research groups cited even at the expense of some of the already cited references (and I know I may be shooting myself on the foot, as I could find me name in the references). Here are some suggestions
We have expanded the references. We tried to include more diversity in research groups (e.g. "Mazzotti and Gueydan, 2018; Fuchs and Becker, 2022; Brune et al., 2023; Foley and Bercovici, 2014; Heron et al., 2016; Zhou et al., 2020; Chen et al., 2020; Gerya, 2024" line 46 or "Moresi and Solomatov, 1998; Trompert and Hansen, 1998; Tackley, 1998; Korenaga, 2010; Nakagawa and Iwamori, 2017; Coltice et al., 2019" line 73, in introduction).

- *<u>Plate behavior in convection models</u>. Only works with a particular code are cited. And only two main researchers (Nick Coltice and Paul Tackley). Even if I should be a bit more 'loyal' to these groups, the fact is that this issue has been a topic addressed all over the world. The authors may indeed leave some of the references cited, but they can also add Trompert and Hansen (1998), Moresi and Solomatov (1998), or any other suitable group (my suggestion is that they try balancing the continents included).*
Thank you for these precise suggestions. We added these references, along with several others in the introduction. We have included in the revised manuscript all the quotations listed by Referee 1.

- *<u>Low temperature plasticity</u>. It is understandable that the previous work by the authors (e.g. Garel et al., 2020) takes a prominent role. And yet, I cannot help but bark at the exclusion of some groups out there. In particular, I was surprised to see no reference to the most recent (to my knowledge, I am not an expert) experiments on low temperature plasticity (e.g. Hansen et al. 2019; Warren and Hansen, 2023; admittedly the latter is a review, please check references therein). Of course, the scope of that literature is different to that of this manuscript, but the total absence of some of this work is surprising, at the very least. I am not a rock/mineral physicist, though, and there could be some reason that eludes me for which these works are not cited.*
Thank you for pointing out these lacking references. We now compare our LT-HT and HT dislocation parameterizations to other low-temperature flow laws (Mei et al 2010, Demouchy et al 2013, Jain et al

2017, and Warren and Hansen 2023) in a new supplementary figure (Fig S12), with corresponding references quoted in the introduction, methodology section (2.2) section as well as in section 5.4.

- Inheritance. Some authors that have worked on that topic will be absent by need (the authors cannot cover everything), but again, this is a bit unbalanced. By comparison, for example Fuchs is cited three times (probably deservedly so), while too many others are absent (e.g. Foley and Bercovici 2014; Heron et al., 2016; there are many more, these are just some that have done numerical models with similar scope; and I am not objective or without conflict of interest here). I could not find any reason for this omission, although perhaps there is one and I have misunderstood the paper somehow.
The studies on inheritance cited in the introduction (line 46) and in the discussion section 5.4 are now better balanced, although not numerous since our study does not consider properties with a memory of past deformation.

In contrast, the literature review on models of more specific rift systems is quite impressive (although, IMO, still a bit continent-centric).
As mentioned previously, we removed the detailed literature review on rift models, keeping only references to justify the absence of crust (Sect. 2.1, lines 134-144) and to discuss the applicability of the two-stage scenario of deformation localization highlighted in our experiments (section 5.1) or of faster strain localization in the presence of a crust (section 5.4).

Figures:

Consider increasing labels fonts when needed/possible (see below).
This has been done (e.g. Figs. 6 and 7).

Figure 9: Please clarify the axes (if parentheses mean units, they mean units, if they don't, they don't). Axes are confusing. Pa s vs. Pa.s notation differ in x-axis label in panels a and c. In these panels the parentheses seem to signal units while in panels b and d seem to signal conditions; please use brackets, parentheses, etc consistently.
We have modified the figures to be more consistent on labels and font size, especially for Figure 9: the unit labelling has been checked, the x-axis labels in panels (b) et (d) are more explicit, and the titles have been removed.

Appendix A: *I'm sure the boundary conditions are correct, but are not clear as written now. The authors mention a linear system for a non-linear rheology, a changing temperature profile, etc, which is not properly described.*
We moved the more detailed implementation of the kinematic boundary condition imposed on vertical sides modeling a Couette profile to the supplementary material (section S1.2.1).

- Line 594: "Incompressible Navier-Stokes equations […] for this 1-d profile simplify to:" -> change to "Incompressible Stokes equations […] for each of these layers simplify to:". Otherwise this would not be strictly correct, or/and not be able to result in 10th order polynomial later on.
Changes have been made accordingly in the supplementary material section S1.2.1.

I haven't checked the Garel and Thoraval paper (sorry) but I am going to assume this 10th order polynomial poses no problem and it is going to hold with a changing thermal structure in the lithosphere, about which I need to remain skeptical. Still, please clarify a bit better this part.
The 10th order polynomial is chosen so as to fit a smooth Couette vertical profile of horizontal outflow velocity, in particular the velocity gradient above and below z_{LAB}. We cite in S1.2.1 the paper by Garel and Thoraval (2021) for the definition of the "constant velocity" plate that serves to define z_{LAB}.

MINOR SUGGESTIONS

Line 2: "Viscosity is capped": viscosity is not capped, technically, what is capped is the stress (note correct use in line 14). This is a minor technicality that I am sure won't confuse geodynamicists, but may be hard to understand to other geoscientists outside this field.
We rephrased the abstract and added the sentence : « Numerical stability is maintained by imposing viscosity cut-offs of …» section 2.2.2 (line 243).

Line 3: "[…] have been proposed, among which low-temperature plasticity.". This construction may make sense in many languages (it does in mine), but I don't think it does in English. Usually, the subordinate clause after "among which" would need a verb. Probably just "among which low-temperature plasticity is the most discussed/interesting/promising", or something similar, is enough.
The main text has been extensively modified, and this sentence have been removed.

Lines 11-12. The use of respectively is peculiar. Although I don't dare to say it is wrong. Online, I could not find this use. https://www.merriam-ebster.com/sentences/respectively ; https://www.internationalscienceediting.com/how-to-use-respectively-correctly/ ; https://blog.mdpi.com/2022/11/10/how-to-use-respectively/ . Please double-check this, perhaps with a native speaker (some colleagues have commented to me they have indeed seen this before). Otherwise change to "[…] is either fully fully mechanical or fully thermal for temperatures lower or higher than 1300 K, respectively." See throughout the text.
Thanks for the comment. Most of the main text has been revised by our native English-speaking author.

Line 26. This is true according to that specific paper. However, note that lateral strength contrasts may arise as well from chemical heterogeneities (e.g. Bodinier and Godard, 2013) which may, or not, imply weakening or strengthening; topography (Turcotte and Schubert, 2014). Of course, weakening may still be needed for creating plate boundaries, but not necessarily to sustain lateral strength contrasts. I have realized this is not important for this paper, but notice that this sentence is an assumption, not a necessity.
We agree with Referee 1, the mention to weakening as a way to sustain lateral strength contrasts has been removed.

Lines 26-32: The authors may find useful the paper by Gerya (2024).
Thanks for the comments. We now quote this relevant paper in the introduction.

Line 39: These citations are very unbalanced, all of them by the same code. Consider removing some of these (not so relevant), and adding, e.g. (but not necessarily) Trompert and Hansen (1998); and/or Moresi and Solomatov (1998); and/or something by other groups (e.g. Höink and Lenardic 2010).
These quotations are now included, except *Höink and Lenardic 2010. We rather cite Richards et al., 2001.*

Line 182: I don't think the definition of plateness is needed, as the authors refer to it correctly, in case the authors wanted to summarize the manuscript.
We kept the 'plateness' definition, to ease the reading and understanding, but moved it to the Supplementary Material (Sect. S1.4.1).

Line 220. I'm not sure whether describing the sections in section 4.1 already makes sense. Wouldn't make sense to include a paragraph before 4.1, as an introduction after 4 before introducing the subsections?
Thank you: we followed your piece of advice and have included a preamble to the Results section to outline its structure.

Line 243: Note this is the use of respectively I am accustomed to and that I have seen elsewhere.

Line 245: (0-6.5 Myr for the reference case). This may seem redundant, but it is important to highlight that the duration of the stages is dependent on the case.
Highlight done in section 4.3.

Line 250. What are plates outskirts? I tried to figure out what this is, and I think it would be better to just change "plate outskirts" to "plates".
This term has been removed.

Section 4.2: Two stages vs. three stage. The authors comment two stages, but in reality there are three (by their own definition). And regarding this… I am a bit of a stickler for steady state. While the authors mention stationary and not steady state, I would disagree with their evaluation. The duration of the third stage is not long enough to evaluate anything, let alone its stationarity (which again, I interpreted as steady state). Rather, it seems to me that this third stage is out of the scope of this work, and some rewording may be needed.
We agree that the system continues to evolve even after a plate boundary is achieved, and this is now described in section 4.2 (line 291). However, we have retained the « two-stage » formulation, because our study specifically focuses on progressive deformation localization up to the formation of a plate boundary.

Stage 1 and Stage 2: Add "for the reference case" inside the parentheses for the time spans. E.g. "[…] spatial focusing of the deformed zone (0/6.5 Myr for the reference case).

Line 278: Note that the yield stress approximation is one of several possibilities, and that the "need" in this line is not a hard "need" but a soft "need".
This has been modified in the revised manuscript (now in section 4.3, lines 479-480)

Line 332: I don't understand. Why another feedback? Why not the same non-linear feedback described before?
This has been modified in the revised manuscript. We trust that the description of feedbacks is now clearer in the Results sections 4.3 and 4.4, and well defined in the discussion section 5.2 (lines 752-765).

Line 344: Is it necessary to introduce what you are going to do in the section? I understand a small summary at the beginning of section 4 but this is a bit of redundancy on an article that is already a bit long.
The Result section has been extensively revised to improve the clarity and fluidity

Line 362,363: Are these lines needed if they are going to be commented about below?
These results are now in section 4.4

Line 365: Without boundary conditions that are fully consistent with the thermal state of the lithosphere, this is a very strong sentence. It may be true, but the initial plate age may not alter much the duration of strain localization.
These results are now presented in section 4.4. Since we have performed tests with different lateral boundary conditions (Supplementary material section S1.2.4) to assess the robustness of our findings, we are confident that the initial plate age indeed exerts little influence.

Line 372: velocity larger than what? (0.2 cm yr -1? 1 cm yr -1? Please specify). Perhaps change to "the larger the velocity the faster the plate thinning".
These results are now in section 4.4 (line376 et seq). The revised version is clearer.
Line 379. Are you sure you are not opverinterpretting the data? It seems to me that the "plateau" is defined by way too few points and it looks pretty linear in logarithmic scale to me. The scatter within a single velocity value (different plate ages) may be too much for this level of interpretation. Besides, how does this affect the overall implications or conclusions or the paper? I would consider summarizing this a next paragraphs (or perhaps even 1 or 2 before) into fewer, less interpretative ones.
The results section has been thoroughly revised, we trust that the presentation of our findings are now more descriptive.

Line 410. I agree, and this is useful. But why is it important to disentangle the thermal and mechanical weakening components? I think the paper would improve substantially if the authors were more specific on the why, rather than the how.

We think it is important to distinguish the relative effect of the strain rate or temperature because this may improve our knowledge about the time evolution of strain localization. In particular, how the resulting deformation is linked to the field evolution of the physical quantities associated to a specific geodynamical context (i.e. explicit the feedback loop). Behind these motivations, we added a sentence in introduction (lines 102-103) "..., but also why it decreases as thermal and kinematic fields evolve self-consistently."

Line 415. In ". Here, it highlights …". What does this "it" refer too?
We understand that it wasn't clear, "it" refers to F_T and F_SR diagnostics.

Lines 419-421. While these references are more diverse than in the "Introduction" section, they are not greatly used. The work by Maxim does not feature any weakening that is not purely thermal, ergo does not need any disentanglement; the work of Fuchs does not include grain size and should not be used to reference a grain size field (note that, if you'd like to diversify the references, this would be a good point to include some references I mentioned in the main comments)… I am of course glad to see these groups cited, but please make sure the references are properly used.
Thank you, It seems that some confusion have been made. The purpose of this reference list was to provide some exemples for other geodynamical settings and may provide different evolution of the physical fields than those modeled in our study. For example, in a subduction setting, the strain rate should increase, whereas temperature should decrease. In a plume-lithosphere interaction setting, the triggering of partial melting and of temperature increase should drive the development of deformation and weakening. Hence, when Ballmer et al., 2009 was quoted, It was to suggest that a numerical setting including small scale convection and partial melting the lithosphere bottom could involve other feedbacks. The latter may be very interesting to investigate.
Fuchs and Becker 2021 compared the deformation memory rheologies: damage-dependent and grain size sensitive, but not Fuchs et al., 2019: we quoted the wrong paper ... Thank you for having noticed this!

Line 434-435. "The colder heart of the mantle lithosphere". Not very clear to me, it is definitely quite nice metaphor, but I think the readers would prefer a word a bit more specific than "heart" (note that the rest of the manuscript is quite technical and precise, this is just a very specific note).
You are right and we now avoid this formulation throughout the whole manuscript.

Line 535. "@@": meaning?
Thank you, this was a mistake.

Figures :

Figure 5. Is the resp. in figure 5 "respectively"? Please check use and consider whether abbreviations help or not.
This has been corrected.

Figure 7. Again, I understand the template-to-final issues, but if the figure is meant to be this small, consider increasing the labels and legends font (note differences between figure 7 and 8 legend labels).
Thank you, we hope that the revised figure is now easiest to read.

Figure 8 is a bit dense, consider simplifying (are all panels needed to defend the conclusions of this manuscript?).
We acknowledge that this figure (now Figure 6) is little dense, yet, we think that all panels help us to describe the results and provide a simple framework to compare the different simulations results.

Figure 9: Axes are confusing. Pa s vs. Pa.s notation differ in x-axis label in panels a and c. In these panels the parentheses seem to signal units while in panels b and d seem to signal conditions; please use brackets, parentheses, etc consistently.
We reworked the whole figure and took into account your feedbacks. We hope this modification help the reader (Fig 9, Sect. 4.3).

Figure 10: Ten panels may be overkill for a vertical-placed figure. Consider place it horizontally or take two panels out (which would in reality mean only taking one panel out). Re-check font sizes then.
We agree, and we decided to move this figure in Appendix (Sect D, Fig. D1) because it only illustrates some second order differences for particular initial age/ extension rate combinations.

Figures 6 and 11. It is not clear what the white areas represent. Since the red-to-green colormap is a percentual map in the legend of Figure 6, it cannot be that these areas are beyond or below the color map values (saturation). I assume it means no weakening, but this is just an assumption (please specify in the legend of Figure 6, at the very least).
We actually forgot to describe in the initial figure's caption that white areas correspond to hardening. We added this precision in figures (Figs 8, D1, and D2).

Appendixes A and B

I am a bit worried about the choice of boundary conditions.
Particularly for extensional settings the choice between velocity and stress boundary conditions is often a matter of debate. Moreover, the bottom boundary injection of material may influence the results considerably, because Dynamic pressure is one of the variables for which we solve, strain rate (particularly) may be affected by the injection of material. I have no particular opinion on the right way to proceed, I am not that knowledgeable in extensional settings, but in the ideal world, the authors could show a preliminary model or test case with different boundary conditions showing that the results are not greatly affected. Nevertheless, if the authors do not find this feasible, this is not an issue that should preclude the publication of the paper.
Thank you for your comment. We set our boundary conditions according to preliminary tests, and we now described some of these tests in the supplementary material (sect S1.2.3). In all cases, the boundary conditions are robust, and do not modify the range of scenarios that are obtained.
For the bottom conditions: The material that is injected through the bottom boundary is not prescribed but is let free to adjust as a function of the free surface and the imposed side velocity outflow. We also added test results with a free-slip bottom boundary conditions in the supplementary material (Fig. S3-d). There is no significant differences in the modeled timing and scenario of weakening compared to the set up used in the main manuscript (Fig. S5 in supplementary material). The only difference is the x-location of the localized plate boundary, the bottom free-slip boundary setup leading to strain localization which is close to the sides of the box (Fig S3 d).
However, if the vertical bottom inflow is imposed (instead of being let free to adjust), we indeed observe a greater strain rate at the plate base, which accelerates strain localization (Supplementary, sect. S1.2.4., Fig. S8, lines 115-121)
We also compared the dynamic pressure to the lithostatic one (supplementary material S1.3.1.). The largest differences are in the yielding realm, that is, the shallow part of the lithosphere, where rheology is no pressure dependent (constant yield stress). Therefore, we expect that theses differences results in negligible effects.

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