Dear Reviewers, dear Editor,

Following the reviewers' constructive and insightful comments, we have substantially revised and re-organized 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-$Y_{(500\ MPa,\ 950\ K)}$" 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.

Reply to Referee 2, Vojtech Patocka :

The manuscript by Van Broeck et al. studies the effects of rheological parameterization on the efficiency of strain localization in models of plate extension.

Systematic tests of several rheological parameterizations are performed, particularly also the low-temperature dislocation creep is included (Gouriet et al., 2019), and the relative importance of yielding, dislocation, and diffusion creep is quantified during a plate boundary formation. Despite the vast existing literature on numerical models of extensional tectonics, it seems like a useful task: There is a well known discrepancy between the yield stress values adopted in geodynamical models with plate tectonics-like behavior and the laboratory-derived values. The discrepancy stems from the simplifications that need to be adopted when brittle deformation is modelled at large spatial and temporal scales, but the yield stress values are often tuned ad hoc, while the present manuscript clarifies the interplay between dislocation creep parameters and the adopted yield stress values.

The manuscript is well written and the figures are clear. However, there is a certain discrepancy between Introduction and Results (see the first two Specific comments below) and the Results section is a bit exhaustive, with too many observables describing essentially quite similar behavior. The results report a robust, two-stage process of plate-boundary formation, obtained in nearly all models with plastic yielding.

We warmly thank Referee 2 for his thorough and constructive review. His comments were very useful to improve the submitted manuscript and the presentation of the experiment analysis.

According to the two reviewers recommendations, we rewrote the results section to focus on our main fidings.

My main objection is that, in all the performed simulations, the extension is driven by a prescribed velocity that mimics Couette flow, computed for a vertical viscosity profile following only the diffusion creep rheology (Appendix A). As a result, mechanical weakening below the plate is restricted near the vertical boundaries of the box (velocity is prescribed there), probably narrowing the range of extensional scenarios that are obtained (and limiting the feedback between surface plate velocity and the amount of below-plate mechanical weakening, Patočka et al., 2024).

Thank you for this relevant comment. We run additionnal tests with variable boundary conditions (Supplementary Material S.1.2.3), in particular a velocity profile stemming from a viscosity profile mimicking the below plate weakening you mentioned (*Patočka et al., 2024,* green line in Figure S4 and S5). Despite a change in the flow pattern (Figure S3-b), the localization scenario remains similar to "*ref-1"* reference simulation (Figure S5).

Given the Discussion (lines 520-525 and 555-560), the manuscript would benefit from testing also a constant tectonic force at the vertical sides, acting on the plate under extension.

We agree with this comment, but the finite-element Fluidity code cannot use 'constant-force' boundary conditions. Neverthless, we performed a complementary set of simulations in which extension rate increase with time (Supplementary Material S1.2.5) for which the tectonic force remains roughly constant for a longer time, these simulations are also mentioned in the discussion 5.1 (lines 726-735).

In summary, the manuscript is a contribution to the literature, providing a diagnostic tool for quantifying the relative amount of thermal and mechanical weakening, and demonstrating that dislocation creep and plastic yielding result in different efficiency of strain localization (although the obtained overall patterns of deformation are similar). The model set-up is a bit simplistic (2D, no shear heating, single-composition material, pressure approximated by the lithostatic pressure), but given the general focus of the study it can be justified. Nevertheless, the employed simplifications should be discussed in a more quantitative manner - for instance, the amount of shear heating and thus thermal weakening can be estimated from the obtained strain rates and viscosities;

According to your suggestion, we calculated *a posteriori* the heat dissipation rate, which is now shown in supplementary S2.2, with a first-order estimation of temperature increase of resulting shear heating (< 50 K) mentioned in the discussion section in discussion 5.3 (lines 841-850).

dynamic pressure can be compared to the lithostatic one.

We compare these pressures in Supplementary Figure S11 (dynamic pressure in the creep domain negligible compared to the lithostatic one), and mention this in methodology section 2.2.2.

The role of boundary conditions at the vertical sides should be investigated more thoroughly
As mentioned above, we performed additional tests presented in Supplementary 1.2.3, that show that altering the vertical side BCs does not impact the localization scenario.

and it should be clarified why a crustal layer is missing.
This was indeed not highlighted enough in the initial submission. We now explicitly mention this strong simplification in the introduction (lines 115-16), in the methods section 2.1 (lines 134-144), and we discussed how this simplification may influence strain localization in discussion section 5.4 (lines 997-1034).

Specific comments:
The term intra-plate deformation is used in Introduction in the context of distributed deformation occurring outside plate boundaries. Later, when discussing the performed 2D models, "intra-plate" is used to describe deformation within certain depths of the plate (lines 380-385). Are these two phenomena linked?
Thank you, indeed the term 'intra-plate' to refer to lithospheric deformation was ambiguous. We removed all occurrences that did not relate to the context of « laterally distirbuted deformation ».

Generally, a stronger link between Introduction and the obtained results would help.
We rewrote part of the introduction to emphasize this link.

Section 5.4 is mostly providing comparison to numerical studies rather than to natural observations, and in many ways the paragraphs there describe further "limitations of the present study" rather than "comparison with natural cases".
Indeed, both reviewers made similar comments. We thus simplified this part of discussion on "*current knowledge of rifting*", and now only discuss (i) potential two-stage natural riftings (section 5.1), and (ii) implications of the absence of crust (section 5.4, lines 997-1034).

Also, the Introduction mentions depth-dependent yield stress as the common modeling approach (line 37) and then a depth-constant yield stress is assumed.
Thank you for this important comment. We performed additional simulations with a depth-dependent yield stress. Our results highlight that the scenario of localization and timing remains similar independently of the shallow resistance if the creep rheology is the same (Appendix E). The differences concern the initial tectonic forces (Fig. E2-c), the lateral viscosity contrasts (Fig. E2-e), but also topography (Supplementary Figure S15) and shallow stresses (Supplementary Figure S16). This is now extensively discussed in section 5.2. (lines 832-838) and in Section 5.3. (lines 890-891, 900).

Lines 45-55 indicate that low-temperature dislocation creep is an important mechanism that will have a significant effect on the obtained results. However, from Table 3 it seems that the opposite is true and that yielding mostly overrules the low-temperature dislocation creep (see particularly the comparison of models D-d_HT-Y_500 vs ref-1; both models seem to perform nearly identically). If I read that correctly, the effect of including LT-HT is smaller than anticipated (a negative result) and it should be admitted in the Introduction (or Abstract). See also lines 290-291, where a model without LT-HT is present in group 2, and where dislocation creep is found responsible only for the strain localization speed rather than the overall style of extension.
Thank you for this comment that reveals that we did not make our point very clear in the initial submission. We clarified in discussion sections and 5.2 and 5.3 how yield-stress is (or not) comparable to LT-dislocation creep, with we hope a more impact argumentation.

The upper boundary is a free surface, but viscoelasticity is neglected. As shown in a different tectonic context, including a free surface while neglecting elasticity leads to a significant overestimation of shallow stresses (Patočka et al., 2019). In a compressional setting, the effects of viscoelasticity were demonstrated by Jaquet et al., 2016. In an extensional setting, the effect of neglecting viscoelasticity is probably not so dramatic, assuming the resulting topography is small, but it should be discussed.
Thank you for this relevent remark: this simplification is now explicitly mentioned in methods section (2.2.1, lines 195-197). We also addressed a specific discussion in section 5.4 on this important simplification , with a citation of your work (lines 1041-1080) and other implications regarding fault development.

Line 79, the boundary condition for the central inflow at the base of the model domain should be given. Even in Appendix B, it only says that the velocity is "left free to adjust", but since it is not one of the basic boundary conditions, the governing equation should be formulated.

We moved this appendix in the supplementary material (S1.2.2), and removed the term “free vz” from the BC description in section 2.1 and Figure 1.

Similarly, in Section 2.2.2, Eq. C3 should be included to make the rheological description complete.

Thanks, we added the previous eq C3 in methods 2.2.2 (now Eq. 7, line 229)

Also, in Eq. 6, the total strain-rate is denoted as edot_creep, but later, in Eq. 7 and 13, it is labeled simply as edot. I assume that only one total strain-rate is involved, this being attributed either fully to yielding or to creeping, with the distinction being based on whether eta_creep or eta_yield is bigger (Eq. 8 and line 131). These details should be clarified. Note also that, generally, edot is not a very good label for strain-rate. Although widely used in geodynamical literature, it is not used in mathematical literature, because the symmetric part of the velocity gradient (i.e. strain-rate) is not the material derivative of any standard measure of strain (such a relation is only approximative, valid in the linearized theory of deformations).

Thank you for spotting this detailed issue: indeed, the total strain rate is the strain rate second invariant. We unified the notations in section 2.2.2.and supplementary material S1.3.2 , and removed the term “epsilon_dot_creep” to avoid confusion. Note that we keep the epsilon dot notation.

Line 139: Defining 500 MPa as "the upper bound for the validity of the LT-HT dislocation creep" is not very clear. This value is larger than the typical values needed to obtain plate-like behavior in geodynamical models, and so it is promising that localized plate boundaries are obtained with this parameterization, but the motivation behind the particular value could be elaborated a bit more (given that most of the presented results employ this value).

We clarified this in method section 2.2.3 (lines 259-261): the value of 500 MPa corresponds to the upper limit of data points of LT-HT dislocation creep from Gouriet et al. (2019).

Lines 195-200: It would help to clarify how Eq. 11 is related to the traction acting on the vertical boundary and why the employed stresses are evaluated 100 km away from the boundary.

We added precisions in the methods (section 3.2, line 348), to explain why the force is evaluated at 100 km from the domain sides (Supplentary material S1.4.2).

If I understand it correctly, Fig. 9 is computed not from the performed simulations, but solely from the rheological equations (i.e. from the viscosity as a function of temperature and strain-rate). This should be stated in the text and not only in the figure caption. It is not fully clear how the time-derivative of temperature and strain-rate is determined in that figure (i.e. what exactly is meant by the employed 50 to 20 Myr plate age scenario, over what time is this change prescribed?).

Thank you for your comment, we have better explained how these theoretical profiles were obtained, how they are proxies for Stages 1 or 2, and how to interpret them in terms of rheology-controlled mechanical or thermal driven weakening (Section 4.3, lines 541-571) and in sect. 5.2 (lines 814-819).

Line 448-450: The nearly identical behavior in models that differ only in the dislocation creep parameters does not imply that "Low-temperature dislocation creep and yield stress seem to play analogous roles in reducing the high lithospheric strength in the temperature range 800-1100K". Instead, it indicates that the low-temperature dislocation creep does not get activated very much, perhaps because yield stress dominates the region, or am I missing something? The sentence would be true if yield stress was not included in the d_LT-HT simulation, but it is.

Thank you for this remark that showed us we needed to clarify how yield stress and LT-HT creep were playing comparable (but not equivalent) roles, it is discussed in sections 5.2 and especially in Sect. 5.3. Low temperature dislocation creep might be activated in the temperature range 800 K to 1100K (Figure 8).

Given the extensive discussion of the absence of a crustal layer (lines 505-535), why isn't a crustal layer prescribed in the simulations? Is it because Fluidity does not allow it, or because it was desired to reduce the number of tested parameters (with crustal rheology introducing new ones)? This should be clarified.

This choice is now clarified in the methodology section 2.1 (lines 134-144), by focusing on mantle-only models, we can isolate strain localization processes within the mantle and more easily compare our results with global plate-like models, which often rely on simplified rheologies and neglect the crust. We also discuss how we expect this important 'no-crust' simplification in section 5.4. (997-1034).

Technical comments:
Thank you very much : your technical comments were useful indeed to improve abstract, introduction, figures, and to focus the results on our main findings.

Line 3: among which low-temperature plasticity... a verb seems to be missing.
Rephased in abstract as "Yet, alternative mechanisms for limiting ductile strength, including low-temperature plasticity, have been proposed" (line 4)

Table 1: Volumic mass, isn't it more common to simply say density?
Indeed, changed to "density". (Table 1, Sect. 2.1)

Table 2: Activation volume, not energy (in the third rows)
Thank you for spotting this error (modified, Table 2, Sect. 2.2.2)

Line 221: the word "homogeneous" is often incorrectly used where "uniform" should be used instead.
We changed the occurrences of "homogeneous" to "uniform".

Line 236: "Highly deformed zone" is used for regions with strain-rate amplification > 1. But that is not a very strict criterion, if the number is not at least 2, calling the zone highly deformed may be misleading.
We indeed misemployed "highly", and now use "enhanced deformed zone"

Line 250: Why depth-invariance of the weakening rate results in small lateral viscosity contrast is not clear to me.
We have now clarified this in section 4.2 : "[…] weakening <u>at the domain center </u>[...] <u>exceeds off-center weakening</u>, producing a growing but moderate viscosity contrast." (line 451)

Fig. 4 caption: We represent only values where >60% ... but the color maps seem full, so it is not clear what this sentence is referring to.
We have now clarified the caption for Figure 4b (Sect. 4.2) :
"Orange shades indicate where weakening (W > 0) affects more than 60 % of lithospheric thickness, with plotted values representing the geometric mean at a given x and time t over all depths z where W (x, z, t) > 0. On the other hand, purple-blue shades indicate where weakening affects less than 40 % of lithospheric thickness, plotting the vertical geometric mean for depths where W ≤ 0 (hardening or neutral). Areas where weakening affects between 40 % and 60 % of the lithospheric thickness are left white."

Line 265: superimposed boundaries? Perhaps reformulate simply as "The dominant def. mech. are delimited by the dashed dark blue lines…"
This term ('superimposed boundaries') does not appear anymore in the new version of Section 4.2.

Fig. 5b: The initial evolution (<1 Myr) is not commented on. Is it perhaps an effect of the imposed BCs?
We chose not to comment on this initial evolution, that is never interpreted later in the paper, because the initial "jump" results from a combination of time derivative calculations (weakening rate) and of initial strain rates conditions (derived assuming null vertical velocity that will immediately evolve as free-surface deforms, hence this short apparent transient stage in the temporal evolution of weakening).

Fig 6 caption: White regions represent hardening?
Figure 6 us now Figure 8 (Sect. 4.3), and we have added in the caption that
"The diagnostics are calculated only in regions in weakening (W>0), with hardening or stable regions (W \< 0) shown in white"

Line 318: even if -> even though
this term has disappeared in new section 4.4

Line 355: The relative change in the width of the deformed zone does not seem so small, so perhaps giving rough numbers would help.
The sentence in section 4.4 has been rephrased, and Fig. D3 Indicate the evolution of plate thickness as a function of width for different ages/velocities.

Line 535: Two @ symbols
Thank you, that was a mistake !

References:
Patočka, V., Čížková, H., Tackley, P. (2019): Do elasticity and a free surface affect lithospheric stresses caused by upper mantle convection?, Geophys. J. Int., 216(3), 1740-1760.
Jaquet Y., Duretz T., Schmalholz S.M. (2016): Dramatic effect of elasticity on thermal softening and strain localization during lithospheric shortening, Geophys. J. Int., 204(2), 780–784.