Dear Reviewers, dear Editor,

We thank the referees for the insightful comments and suggestions. We addressed all the points raised in the review. In addition, we made a minor editorial modification that was not specifically requested by the referee: we inverted the order of Figs. 6 and 7 so that they now appear in the same order as their first citation in the text.

Second review of "From Strong Plates to Weak Boundaries: Strain Localization in the Lithospheric Mantle with Low- to High-Temperature Dislocation Creep" by Etienne Van Broeck, Fanny Garel, Catherine Thoraval, and D. Rhodri Davies.

By Antonio Manjón-Cabeza Córdoba

The authors have done a substantial re-write of the manuscript to address mine and the other reviewer's comments. I do not think this was needed, and the authors have introduced a few technical typos that could have been avoided had they done a more localized, surgical and subtractive revision. Nonetheless, the conclusions remain robust and I do not think that delaying the publication of this article any longer should be done on the sole base of my opinion. Unfortunately, when I accepted the second review I was not aware of the level of work put into the new version and I needed to ask for an extension once I realised I could not just check a few boxes. Due to personal reasons, it has been a bit frustrating to follow through with this review and I am not convinced of my objectivity (i.e., I may have been overly picky). For that I apologise to the authors and the editor.

Overall, the article is good and has improved. I still find the article overly long. In some parts it is repetitive and in others discusses issues that do not serve the conclusions. The manuscript features > 600 lines of the main text, 5 Appendixes and supplementary material; all this for conclusions that do not need that much argument (i.e., the more robust the conclusions the less discussion they need). However, some parts are definitely clearer now, and the figures are more synthetic and to the point. At the moment, I debate myself between recommending a technical review or a minor revision, and I pass the responsibility to the editor. Regardless, I think that I have contributed what I could contribute and, should another review be needed, I recommend someone else to do it.

ABSTRACT

I warn against the use of the word ductile altogether. Ductile is a rather phenomenological term that in numerical models can (and probably should) be changed by the word "viscous" whenever possible. If a distinction needs to be made, then I suggest to refer to the specific mechanism. In what the field scientists would classically call the "ductile regime", a combination of viscous creeps may apply (and even combined with some micro-brittle behavior). Note that later in the abstract dislocation creep is addressed explicitly (good).
We removed the term *ductile* from the Abstract, and revised the main text accordingly to your warning. In particular, occurrences of *ductile creep* were replaced by *creep*, *ductile deformation* by *creep deformation* and *ductile lithosphere* by *creeping lithosphere*. In the Discussion section (Sect. 5.3, lines 529-534), the term *ductile* is used in ‘brittle-*ductile* transition’ (e.g. Meyer et al., 2019). It also used later (Sect. 5.3 and 5.4, lines 545-550 and 580-587) to discuss how LT-HT dislocation creep can act as a ‘*ductile* strength limiter’ with reference to the term ‘*ductile* yield stress’ already used in Tackley, (2000) and Van Heck and Tackley (2008).

INTRODUCTION

Plate tectonics describes the motion of rigid lithospheric plates separated by narrow, weak boundaries (e.g. Le Pichon, 1968).
-> I don't understand this sentence. Plate tectonics describes? Maybe add "The hypothesis of plate tectonics"? Or "The theory of plate tectonics"? Or even "The law of plate tectonics"? Perhaps more technically "The paradigm of plate tectonics involves the motion of rigid lithospheric plates ..."?
We add ‘The theory of …’ , as firstly suggested.

"The lithosphere is expected to be strong under cold, ductile conditions". Ok so, at the very least, please define ductile. I have a rough idea of what the authors mean but a lot of people, particularly those working on the lithosphere will imagine that cold and ductile are an oxymoron. Hot and cold are relative, ok, but brittle is colder. Again another case for abandoning the word "ductile" altogether.
We changed the sentence to → « … the lithospheric mantle is expected to be strong in the creeping regime under low-temperature conditions. » and remove the term *ductile* (lines 32-33).

Line 49, low with respect to what? Maybe better, "lower than those predicted at x depth by assuming that the yield stress represents brittle deformation".
We rephrase the sentence : « However, the required stress limits are typically lower than 200-300 MPa (e.g. Richards et al. 2001 ; Crameri and Tackley, 2014 ; Mallard et al., 2016), and would correspond, assuming that yield stress represents brittle deformation, to effective friction coefficients ($\lesssim$ 0.2 e.g. Moresi and Solomatov, 1998 ; Crameri et al., 2012 ; Nakagawa and Iwamori, 2017) substantially below laboratory values ($\sim$ 0.6, Byerlee, 1978). » (lines 50-54).

Line 53,54 "Low-temperature ductile deformation has been proposed as an alternative route to limiting lithospheric strength without imposing an ad-hoc stress cap throughout the ductile regime." This is true but even better with a citation.
Also, low-temperature ductile deformation... is the low-temperature relative to other ductile deformation mechanisms or is ductile used indistinctively from dislocation? I am intentionally being obnoxious precisely to point out the problem with using "ductile" within the context of geodynamic models.
We changed the sentence as follows : « Low-temperature creep deformation has been proposed as an alternative route to limiting lithospheric strength without imposing an ad-hoc stress cap throughout the creeping domain (Jain et al., 2017).» (lines 55-56).

Line 67 "Weinstein and Olson 1992: Thanks for this reference! Did not know it and it is indeed useful.
You’re welcome.

Line 91. Please be consistent, either sects 5 and 6 or sects Discussion and Conclusions
We consistently modified the references to sections : «(Sects. 5 and 6).» (line 93).

METHODS

Table 1. "Density" -> Reference density
Done.

Figure 1. "Freesurface" -> Free surface? Why? BTW?
We corrected the term ‘fressurface’ for ‘free-surface’ in the Figure 1. Note that the evolution of surface topography is not the primary focus of this study. Nevertheless, we discuss surface elevation variations and their influence on shallow stress build-up in the Supplementary Material, in the section «Lateral variations of free-surface topography and of shallow stresses » (Sect. S5.2).

Line 162-163. References are inline, probably unintentional.
Thank you, it was a mistake.

Table 2. Adisl ^{-7}? Is this correct? Please revise
Yes, we assume that you are refering to A_{diff} ? This value is correct, A_{diff} = 1e-7 Pa^{-1} s^{-1}, (see Garel et al, 2020). However, there was indeed another mistake, the correct unit for Adiff and A_{disl} are 'Pa^{-1} s^{-1}' and 'Pa^{-n} s^{-1}', it has been revised in Table 2.

The tanh label makes sense within the context of the previous publications (Garel 2020), but I'm not sure it is clear here as only one formulation is described in the methods
We remove the tanh label from Table 2.

Line 198. I am a bit confused. Some lines above the authors include an adiabatic correction for the viscosity, but now they claim they solve the problem in the Boussinesq approximation. I'm sorry I missed this in the previous review, but I don't think this is clear in the text.
It was indeed ambiguous, dT is a temperature correction applied only to viscosity calculations for diffusion and high-temperature dislocation creep, to mimic the effect of a 0.5 K/km adiabatic gradient within the incompressible approximation and partly balance the viscosity increase associated with pressure. We now explicit this in the Method section (Sect. 2.2.2 ,lines 151-153).

RESULTS

Line 254. I'm sorry if I missed this in the previous review, but F here is confusing. Earlier, a tectonic force F with units of N m^{-1} is defined but now these ratios seem dimensionless. I would expect the same capital letter should correspond to the same units.
Thank you, we acknowledge that it was confusing. We modify the tectonic force notation from 'F' to 'TF' and keept the F_T or F_{SR} labelling.

4D evolution actually means 5 D (i.e. 4D in time). I recommend either 3D evolution or 4D models. We do not mention 4-D evolution anywhere in the text, hence we are unsure how this comment applies to our manuscript.

Line 309 Formatting cmyr^{-1} -> cm yr^{-1}. And around this paragraph, the space between label terms disappear please be consistent throughout the text (Fig. 5 or Fig.5 but not both).
Done.

Figure 4: There may be a label missing in the vertical axis of panel b (km). Maybe this was intentional?
This was intentional, but you're right this was not clear. We revised the figure axis.

Line 352 Fig.9 -> Fig. 9: Line 355 Fig.9d -> Fig. 9d
Done.

Line 370 Fig.9 -> Fig 9: Table3 -> Table 3.
Done.

Lines ~360-374. Rather than a friction coefficient, the authors impose a temperature limit. Fine. But then they should not analyse a stiff layer that arises precisely because of this cutoff! A friction coefficient should indeed result on a stiffer part of the lithosphere, but the one in this manuscript is simply imposed. I think the authors overinterpret their results here (which they don't need at all).

We acknowledge that the temperature limitation in the viscosity parameterization artificially introduces a stiff layer. In addition to methodology section 2.2.1, we have added in Section 4.3 a sentence to explain why we introduce such a the temperature cut-off to compare HT and LT-creep mechanisms (lines 367-368). We wish to keep in the paper the description of these simulations' results, because they illustrate that the yield stress cannot be interpreted solely as a proxy for brittle or semi-brittle in Discussion (Sect. 5.3, lines 520-526).

Figure 9: Notice that HT and LT-HT are not equivalent formulations at high t. The viscosity scale is logarithmic and therefore the difference is noticeable. Not sure I agree with the authors both cases are comparable (this does not hurt the general conclusions of the paper, but does put a limit on the strength of their remarks on LT-HT vs HT, which admittedly, they tuned down). Just to be clear, I do like the author's LT-HT formulation better than just HT, my comment is just on what's shown in the manuscript

We agree that LT-HT and HT dislocation creep are indeed not equivalent but « comparable » at high temperature and high pressure. Our focus is rather on the yield-creep transition at temperatures in range of 800 to 1000 K in which both paramaterizations result in different modes of weakening.

However, we disagree that the difference is significant at high pressure and high temperature conditions. Even on a logarithmic scale, the effective viscosities remain very similar (~2e20 versus ~1.7e20 Pa s at 120 km depth in Fig. 9).

DISCUSSION

"Taken together, our results show that lithospheric-scale strain localization in this extensional setting is controlled not simply by the integrated strength of the plate, but by where weakening occurs and how rheology couples deformation focusing to asthenospheric upwelling through rheology-modulated feedbacks linking deformation focusing, plate weakening and asthenospheric upwelling." -> This sentence is key and it is too hard to read. I would suggest dividing in 2-3 sentences. Otherwise readers may miss the main point of this work. What about... [...] our results show that [...] strain localization [...] is controlled by the integrated strength of the plate and by where weakening occurs. In addition, a key controlling effect is the rheological coupling of deformation focusing and asthenospheric upwelling [...] ...? (I'm sorry if I did not capture the original meaning, but at the very least I should have shown this wasn't easy to understand).

We revised this paragraph by following the Referee's piece of advice to cut the text into 3 shorter sentences (Sect. 5, lines 420-426).

Line 443 Potentially unintentional inline citation (Ulvrova, 2019).

Done.

Line 474 Fig.9 -> Fig. 9

Done.

Line 483 Fig.6c -> Fig. 6c

Done, but please note that here the quoted figure has been changed to Fig. 7c.

Line 574 "[...] approaches should distinguish [...]". Should distinguish for what? I understand that there is something missing when not including dislocation creep or a "ductile yield stress", but are the authors suggesting that all models that do not include this should be automatically scrapped? I would not agree with that, nor I think the authors have the results to make such statement. Instead, this sentence would be better written as "Models that would like to accurately estimate the absolute timescale of localization should distinguish [...]". That way this statement is precise and with real

actionable implications.
I apologise to the authors if this comment seems pedantic, but this sort of comments are too common and of little help in our community (For example, I work on melting calculations in this sort of models, I could argue that extension tectonics of this sort usually happens under strong magmatism, therefore casting doubt on the authors' results because "Models should include the effect of melting". But I honestly think that the authors' models are better because they are simplified and therefore could properly study creep-based localisation without the effect of melting.
Indeed we agree with the Referee's point of view. We modified the sentence using the proposal made by the Referee.

Line 577 "result" -> aspect (not technically a result).
Thanks for having noticed this, we followed your suggestion.

Line 585. I understand that the authors acted to address a reviewer's comment. But I wanted to highlight that by no means I believe this is the main limitation of the study. No action needed here.

CONCLUSIONS

Line 627 "In all simulations that successfully localize" Localize what? I may be overly picky here, the sentence should be understandable for most readers, but an object is missing.
You are right, we added a word : « In all simulations that successfully localize '*deformation*' ».

GENERAL COMMENTS

I suggest you review the order of the citations. This is not important, but at the moment, they are not ordered neither chronologically nor alphabetically in some parts of the text. When they do, they do so chronologically, which I like, so I suggest to be consistent throughout the text. Perhaps does not make that much of a change, but easier on the reader.
Thank you, we have revised the order of the citations by respecting a chronological sorting.

Many of the labels for the Figures do not have a space between the "Fig." and the number. Others do. I have pointed some out, but I am sure I missed some. This is just a technical issue but appears throughout the manuscript, please revise.
We have revised the space between 'Fig.' and the number of the corresponding figure, as well as for Sections or Tables.

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