nature portfolio

Peer Review File

Deep crustal deformation driven by reaction-induced weakening

Corresponding Author: Dr Mathieu Soret

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The paper describes a very nice set of experiments at high temperature and pressure where reactions and deformation are intertwined. The microstructures clearly indicate that the main deformation mechanism is dissolution-precipitation creep. The fact that these experiments are performed on mineral assemblages, pressures and temperatures relevant for deep crustal sheer zones makes the study an important contribution.

On line 252-3 the authors state that "heterogeneous compositions of reaction products are inconsistent with diffusive transfer from the dissolving sites."

Maybe this is obvious to the authors, but I do not understand from the text and figures why diffusive mass transfer is incompatible with "heterogeneous compositions of reaction products". I suppose the latter means several different mineral types. The question is then more about the stability of the minerals and their relative growth kinetics (that can change with time doe to changing stresses in the shear zone...) I also do not understand why advection would make any difference compared to diffusion.

Which assumptions of fluid pathway cross-sections, growth kinetics, etc have led the authors to calculate and conclude that "length-scales and anisotropy of crystallization tails and shear bands are incompatible with fluid-assisted diffusion rates"? (lines 254-255)

The hypothesis of advection controlled DPC seems somewhat unfounded and I fail to find mention of any such process in ref 26 referred to in lines 262-263.

The conclusion in lines 271-273 "The rapid and dramatic stress drop (up to 74% of the peak stress) during strain localization can, therefore, be explained by fast reaction kinetics and efficient mass transfer" seems well founded. But the Authors have not demonstrated that this fast reaction kinetics and efficient mass transfer must be due to advection as opposed to diffusion.

With only 0.2% water in the samples I find it hard to imagine "fluid flow" as described in the introduction (line 26)

The experiments are well conducted, they show interesting shear weakening due to DPC and the implications for subduction zones seem plausible, even without invoking a speculative "advection" DPC.

Reviewer #2

(Remarks to the Author)
This manuscript presents exciting new experiments demonstrating the importance of fluid-mediated chemical reactions in controlling the strength of subducted material, and plate boundaries more generally. A comparison of low and high strain experiments clearly illustrates the evolution of the system from incipient reaction to interconnection of reaction products and drastic weakening. Spatially-resolved analysis of textures and mineral compositions provides important insights into the governing deformation mechanisms. The feedbacks between chemical reactions, fluids and deformation presented here are at the cutting edge of subduction zone science, structural geology, and rock deformation, and these experiments meaningfully advance this topic.

Major comments:
-Influence of metasomatism: Much of the discussion around the implications for these experiments focus on the influence of metamorphic reactions within a given lithology and how that might affect the location of the plate interface and distribution of seismicity. However, plate interfaces are near-universally characterized by extensive metasomatism between slab and mantle wedge lithologies that are much farther from equilibrium than any metamorphic reaction in a single lithology. The processes outlined in this manuscript certainly apply in that case as well, though the precise expression might differ because of the distinct initial geometry of metasomatic exchange across a boundary vs. metamorphic reactions within a rock. These rocks would also significantly modify the proposed model of interface down-stepping dependent on metamorphic reactions and fluid release. Discussion of the implications of these experiments in natural subduction zones, necessitates some consideration of metasomatic processes and metasomatized plate interface shear zones observed worldwide.

-Extrapolation to natural systems/subduction zones: These experiments are run at higher temperatures and stresses than expected in modern subduction zones. While this is an unavoidable side-effect of deformation experiments at human timescales I believe it warrants some comment. What strain rates could be accommodated by these processes at stresses expected in subduction zones and how do they compare to rates for aseismic creep, slow slip, etc? How would colder temperatures impact the kinetics of reaction and mass transport? How should the strain localization observed at this small-scale be expressed at the scale of the downgoing slab? Some brief comments on extrapolation to natural systems would strengthen the implications of this work.

-Differences between experiments: Figure 1 shows three different high strain experiments with different stress-strain curves. What controls the differences between these experiments (mineralogy, strain rate, etc.)?

-Figure clarity: In Figure 3, nanopores are not clearly visible in any of the images where they are labeled. Please include a more zoomed in image showing these features. In Figure 3, some images also lack annotation for all phases/domains. While I was able to figure out what each corresponded to eventually, additional labels might improve readability/clarity for the audience. In Fig. 3i, can the shear plane be marked for comparison with grain shape?

-Implications and Figure 5: How were the plotted Alpine rocks chosen? Are they specifically from the plate interface or just a survey of mafic and sedimentary Alpine samples? For the mafic rocks, is MORB a good representation of their composition? Slow spreading oceanic crustal setting, significant seafloor alteration, limited basalt volumes, and unusual gabbro compositions (e.g., Mg-Al and Fe-Ti gabbros) probably make these Alpine mafic rocks a poor fit for classic MORB.

Minor comments, questions and food for thought:
-Line 84: How was this water added and could it escape during the experiment? Approximately how much of the added water was eventually consumed to form hydrous minerals (zoisite, paragonite)?

-Lines 155-157: Later it is noted that the difference in layer thickness between Pl-derived and Px-derived reaction products reflects strength contrast as well.

-Lines 385-387: Water content was kept to a moderate level to prevent pore pressure effects, however significantly fluid pressure fluctuations are expected from many metamorphic reactions and due to fluid influx at the plate interface. How would increasing fluid pressure influence the deformation of a reacting system like this?

-Lines 242-244: Also shown in analog experiments by Ladd & Reber (2020).

-Lines 252-255: What are the expected intergranular diffusion rates? Not sure I understand how anisotropy plays a role.

-Line 262: I did not find a mention of advection-controlled DPC or the impact of advection versus diffusion in the cited reference.

Grammar/spelling suggestions with no bearing on the quality of the manuscript:
Line 49: “like” suggests that the minerals are not load-bearing in experiments but are in crustal rocks. “in” might better convey your intended meaning.
Line 56: Additional “,” after “present-day plate tectonics”.
Line 71: “on crustal rheology” rather than “on the crustal rheology”
Line 125: It is a little unclear what is referred to by “The fine-grained mixtures”.
Line 177: “interconnected by” not “interconnected between”.
Line 186: “forming” not “structuring”.
Line 281: Maybe “departure from” rather than “distance to”.
Line 400: “for” not “from”

Reviewer #3

(Remarks to the Author)

The reasons for the existence of plate boundary shear zones, and the phenomena of their initiation and persistence are critically important aspect of geodynamics and incompletely understood. A major reason for this uncertainty is a poor understanding of the rheology of the typical polyphase rocks that exist at depth. The authors of the submitted article describe several high-strain deformation experiments on polyphase rocks at high pressure and temperature outside of metamorphic equilibrium. During the experiments, the initial mineral phases broke down in areas of deformation and recrystallized as new phases (or compositions). While several experimental conditions (e.g. grain size and strain rate) are quite different from natural conditions, the experiments produce microstructures that look like naturally deformed samples. Key observations from the experiments include 1) considerable strain weakening, 2) strengths lower than those of mono-phase aggregates (although this isn't quantified in the paper), and 3) occurrence of recrystallized mineral phases in fine-grained, polyphase, highly-strained layers. Although only four experiments are presented, these are interesting results. However, I found the discussion of the implications of the results over-reaching: I don't find many of the big ideas put forward well supported by the results and previous literature.

A couple concerns of central importance:

1) Part of the hypothesis put forward in the paper (fig. 5c insets) suggests that weakening is followed strengthening once reactions cease. The strengthening is not observed in the experiments, and I doubt it would occur if they went to larger strain (are there no experiments in the literature or from this lab on such fine-grained aggregates to compared to?). Two critical observations: 1) "Incipient strain localization and weakening at peak-stress conditions coincides with the interconnection of reaction coronas of fine-grained reaction products" (197), and 2) Metamorphic reactions occurred from the beginning of the experiments, including during the high stress phases. Based on these two observations, the obvious interpretation to me is that it is the interconnected fine-grains that are responsible for weakness, not the "thermo–hydro–mechanical–chemical processes" or "chemical disequilibrium" as suggested by the authors. My interpretation is a rather conventional idea (grain size reduction causing weakening, e.g. quartz regime 1 (Hirth and Tullis, 1992)) but it isn't considered in the paper. If it were true though, then it undermines another hypothesis in the paper related to plate interface processes:

2) The authors conclude the paper with the hypothesis that weakening associated with metamorphic phase changes (like they see in their experiments) controls the location of the upper surface of subducting slabs. It seems likely based on the experiments presented that metamorphic changes accelerate weakening, but a quick calculation sheds some light on their hypothesis: The weakening associated with metamorphic reactions in the experiments occurs after shear strains of ~3 to 5. Extrapolated to the plate scale, where plate interfaces are ~1 km thick, the temporary (if it is in fact temporary) weakening associated with a significant metamorphic phase change would thus occur in a region ~3 to 5 km in length. That's an order of magnitude smaller from what would be needed to explain the gradual change in plate boundary location over ~40 km pictured in figure 5c. In other words, the (supposedly) transient weakening effect they document would appear to be "used up" far too quickly to have large-scale implications as the authors suggest.

The work is full of what seem like logical inconsistencies or oversimplifications (see below). Thus, I do not find that the experimental work supports the conclusions/discussion, and I do not recommend publishing.

—

Line by line comments:

21. "groundbreaking" is overly enthusiastic/biased for a science manuscript.

23. Somewhere in the paper this should be quantified (comparison of the new stresses with dislocation creep stresses).

24-27. Text like this in an abstract suggests that evidence for such things was documented in the document, but it is not (e.g. no quantification of the effect of (supposed/undocumented) dynamic permeability).

30. "Inception" of plate interfaces, i.e. subduction initiation? This topic is not discussed in the document.

32. "rooted in … chemical disequilibrium and fluid concentrations of the slab materials." Cut "fluid concentrations". There is a figure (5a) that depicts H_2O concentrations of different metamorphic rocks that likely exist in subduction zones, but there isn't enough discussion of fluid concentrations to warrant being included in the abstract.

58-66. There is a large and unjustified leap taken here. It is stated that metamorphic reactions and deformation are sometimes associated (58-59), and also that dissolution-precipitation creep (DPC) promotes localization (60-62). It does not follow from this that "the development of crustal shear zones...should then be driven by...chemical (dis)equilibrium…and… mass transfer along grain boundaries For one thing, there are lots of other factors that influence the development of crustal shear zones and localization (like grain size reduction). Also, many crustal shear zones occur at PT conditions where the deforming minerals are stable. (Also, if this were well established or self-evident as presented here, it would undermine the justification for the paper.)

128-129. "Dissolution dominates on the interfaces of primary phases subnormal to the shortening direction," The evidence for this should be given.

136-137 Cut "along the short diagonal". I find this text confusing and unnecessary. I can see from figure 2b what's meant.

141-142. There are other important factors not mentioned, e.g. rotation.

150. Euhedral isn't the correct word. Lot's of the grains shown in figure 3i have curved boundaries.

157-159. This observation (high densities of defects) indicates that dislocation creep occurred during early phases of deformation. This may be an important part of the time-dependent rheology of the experiments that should be discussed further.

163. "Red arrows point to areas of total Pl1 consumption" This could be better explained. Do you mean "Red arrows point to small Pl1 grains, which we interpret as likely sites of near-total Pl1 consumption"?

186, 188. From my (limited) knowledge of the cited locations, referring to them as "fossil plate interfaces" is an exaggeration that undermines the meaning of the term "plate interface." These are simply rocks deformed near plate boundaries, no different than many (entire) mountain belts like the Alps. "Plate interface" is commonly understood to be a zone no more than 1-2 km thick (e.g. Agard 2018, fig.2).

187. The experiments look similar to natural samples. "Suggests" would be a more appropriate term than "highlights"

191. "Shear zone infancy is characterized by insignificant strain". This confuses me. By definition the infancy of a shear zone involves minimal strain, so what is the purpose of stating this? Needs rephrasing.

192. "indicating that the coarse-grained starting material is too strong to deform by dislocation creep at experimental strain rate conditions". This is directly contradicted by the results which show significant concentrations of dislocations in minerals existing from the beginning of the experiments (fig. 3g). This seems problematic for an important message of the paper—Apparently, some non-trivial amount of strain (shear strains of 2 to 4, fig.1) at high stress, likely associated with dislocation creep occurs before the weakening documented here begins to dominate.

200. The word "instabilities" has specific rheologic meaning that might not be intended here. I suggest just using the word "weaknesses".

219. The term "in contrast" is unclear. The previous paragraph mentioned "Increasing strain localization," here we are also talking about that (sheared reaction products).

230-232. Just because fluid-assisted mass transfer is faster, and there is some fluid in your experiments doesn't prove that it is the main mechanism. It seems likely, but jumping to conclusions that should be better based on comparison of experiments with different fluid contents (or some other evidence).

234-235. These experiments are a good example of "strain localization". There is a zone of higher strain through the sample, but this appears most obviously related to the position of the pistons (fig. 2), i.e. to changing boundary conditions such that there is no reason, after a certain amount of strain, for material outside of this central zone to continue deforming. "Strain localization" suggests that strain weakening of the material is causing localization. Certainly, the material is weakening (Fig.1), but whether or not that causes localization is not determined in these experiments. Thus, I don't see evidence here to argue that "localization in shear zones may not necessarily require brittle precursors".

233-246. This paragraph seems quite speculative on topics outside the main scope of the paper.

248. Section on DPC. I had to re-read this several times to make sense of this paragraph.
There is a bold hypothesis here that fluid-assisted diffusion isn't the most important process, instead "advection processes induced by…pressure gradients, dilatancy and brittle failure". This needs more evidence and clearer explanation. I think it would be helpful to explicitly differentiate between fluid-assisted diffusion and fluid-assisted(?) advection (or whatever term the authors want to use). I needed to read this several times, and I'm still not 100% sure I follow, e.g. at first I thought the authors were invoking pressure gradients as a means of transfer of solid material. The last sentence of the paragraph (263-265) though talks about a standard understanding of pressure gradients, so I guess the authors are talking about the advection of fluid containing ions rather than the diffusion of ions through a fluid. This should be stated much more directly.

259. The citations about dilatancy and brittle failure are unnecessary and distracting here—everyone knows what these processes are; citations in this paragraph instead shed some light on advection-controlled DPC if possible. This is one example of many in the paper of (nonstandard) use of citations almost like web-links (e.g. an underlined word on a website that links to a wikipedia article or some definition of a term). What's needed in this document are more citations used to support assertions that are made. In the example of this sentence, if there's a citation present it should demonstrate some precedent for the main idea of this sentence (mass transfer being controlled by advection processes)—has that been considered or proposed before? If not, then I expect more explanation for it. As it stands, I don't know if this is a new idea or not.

252. "heterogeneous compositions of reaction products are inconsistent with diffusive transfer from the dissolving sites." Why would heterogeneous compositions be more consistent with advection-controlled DPC than fluid assisted diffusion? Can you support this with citations? Also, I don't see compositional variation in figure 3f (other than between Cpx1 and Cpx2)? And as far as figure 4b and 4c go, given the large scale, it's hard for me to be certain that I see compositional variation of

reaction products (maybe some arrows would be useful). For example, I can see some fine-grained areas are more yellow or blue, but is that a result of compositional variations in recrystallized minerals (as the authors imply), or because of local variations in the concentrations of one mineral species versus another, or because there is local variation in the abundances of the various fine-grained remnant grains?

253-256 “Moreover, length-scales and anisotropy of crystallization tails and shear bands are incompatible with fluid-assisted diffusion rates, especially in environments where porosity is only locally and transiently connected.” This is quite arm wavy but probably could be quantified with some back-of-the-envelope calculation. Also, given that the rates from the Carlson reference are from natural samples, are those rates directly comparable to those inferred from the current experiments? A better comparison perhaps would be to rates of diffusion in fluids in static experiments (perhaps the work of C. Manning is relevant here). Additionally, If the authors are, as I understand, invoking fluid as the medium within which ions are advected (from sites of dissolution to sites of precipitation), then the second part of this sentence doesn’t make sense to me since advection of ions in a fluid also requires porosity (i.e. local and transiently connected porosity also sounds like a problem for “fluid-assisted advection” invoked by the authors). Finally, what does any of this have to do with anisotropy (“anisotropy of crystallization tails”)?

262. The authors make a strong contrast between the traditional understanding of DPC and…some other behavior they believe they see (lines 250-25e). This new mechanism of mass transfer proposed by the authors “advection-controlled DPC” either needs some citations or a much better description. Also, there is no mention of “advection-controlled DPC” in the cited paper (Mansard et al., 2020) (which presumably was cited as evidence that this process dramatically weakens rocks).

248 I’m quite confused by this section. Did the authors simply rename an already established process “advection-controlled DPC”, or are they really proposing something new?

259. But it was said earlier that there isn’t much brittle behavior

267. and/or differences in strength and strain?

271-272. “The rapid and dramatic stress drop…during strain localization can, therefore, be explained by fast reaction kinetics and efficient mass transfer”. This seems like a bold claim that hasn’t been really justified. There was more weakening in the plag-rich areas relative to pyroxene ones, but there might be other reasons for this than fast reaction kinetics. For example, maybe the pyroxene is just a stronger phase? Or maybe the strength reduction is due to a faster rate of grain size reduction in the plag-rich areas (partly due to metamorphic instability)?

271-272 Which mass transfer is being referred to? Strain involves mass transfer, so does the advection-controlled DPC referred to in the previous paragraph.

309. Re: the title of the last section “Implications for plate interface processes, from long-term to short-term” (and also line 64): Presumably the authors are referring to earthquakes being short term processes, whereas all the rest of the section is about rather “long term” processes. However the only mention of earthquakes is a reference to the location of the seismogenic zone. “Long-term to short-term” is an interesting topic about how short timescale things add up to “geologic” features, but there’s nothing about that in this section. Cut (and try to avoid unnecessary “buzz words”…)

311-313. “Our study confirms that existing flow laws for non-reactive materials tend to overestimate the strength of the lithosphere deformed in viscous / semi-brittle shear zones.” There should be some quantification of this—what flow laws are being considered?

316-317. “continuously located” is ambiguous. Does it mean continuous over time? Or it changes systematically with location? … “Compilation of exhumed materials from fossil subduction zones (e.g., Alps; Fig. 5a) outlines that plate interfaces are not continuously located within sedimentary horizons.” This might be better said something like this: “The abundance of both metasedimentary and mafic lithologies underplated during subduction (e.g. Agard, 2021) indicates that the plate interfaces are in many places not located within sedimentary horizons.”

318. “Outlines” doesn’t seem like quite the right word, maybe “suggests”? However, the sentence is incomplete—simply “compilation of exhumed material”, by itself, isn’t enough to draw this conclusion, there must be comparison with some other information.

318. What is “it”? Plate interfaces? Figure 5 doesn’t help much with this because Plate Interface is not labeled in the figure. However, the cited reference of Agard et al., (2018) defines plate interface as “the domain between the lower and upper plates, including their boundaries.” In this interpretation, the sentence seems to contradict itself then: If the plate boundary is the boundary of the plates, how can the boundary also occur within the subducting plate (“dip…into the slab crust and mantle”)? Figure 5 seems to show the seismicity boundary (red) dipping into the sedimentary crust and mantle of the lower plate (consistent with the—in my mind, questionable—cartoon of Agard et al., (2018)), so perhaps “it” refers to the seismicity boundary? (e.g. Agard et al., (2018) discuss a low-velocity horizon observed in receiver-function studies, and state that this zone “seems to dip into the slab and die out once the blueschist to eclogite transformation is completed”). But then, the seismicity would “dip beyond the seismogenic zones” which also doesn’t make any sense…

318. What does “dip beyond” mean (“dip beneath”?)

323. Drop this acronym (THMC). It's only used a couple times and most every reader is going to have to hunt for what it means at this point, since it was introduced and used only once in the introduction.

322-323. "Such a remarkably similar THMC evolution in subduction zones…". What is "remarkably similar" occurring in multiple subduction zones? The reference 44 (Van Keken et al., 2018) only refers to one place (Japan), and also shows counter examples where the blueschist-to-eclogite transition doesn't correspond to a cessation in seismicity.

325. The references here are confusing. Behr and Burgman (2021) describe many different potential mechanisms and modes of deformation, not a "common" mechanism. The Oncken et al reference argues that "cataclastic flow is the dominant physical mechanism governing transient creep episodes such as slow slip events" (which isn't the mechanism discussed here).

330-331 A quick look at the reference cited here indicates it has something to do with transient changes in deformation and involves rheology, but it looks like a completely different topic than discussed in this paper. It's a puzzling choice of a reference here, and doesn't seem relevant.

346-347. "the kinematic plate interface into the mafic crust inferred at ≥ 80 km depths; Fig. 5c" I think for this scenario, subduction zones must be continuously retreating and underplating at these depths. That might be worth mentioning

Figure 5c.:

1) It's a little hard for me to tell what some of the lines and labels are intended to indicate here. This is particularly problematic because of the importance of this figure for understanding the section "Implications for the plate interface processes...". Main issues are 1) "Seismogenic zone" seems clearly associated with a thick black line that becomes dashed with depth and then is labeled "recoupling" with the same font treatment at depth… So, I suspect these labels don't actually refer to the thick line (which should be labeled as "plate interface"??), but they should be oriented horizontally along with the text "viscous/semi-brittle" to indicate the locations of various things.

2) Also, the figure 5 caption doesn't explain what the circled (1) and (2) are, but the coloring suggests they might refer to the (unlabeled) insets for sediments and mafic rocks which include blue "subd. interface 1" and red "subd. interface 2". The connections between these things should be made more explicit.

3) there are two thin dashed lines that I think are labeled "brittle-viscous transition (BVT)" that extend, parallel to, and below, the plate interface, to 100 km depth. "Viscous / semi-brittle" is also drawn in red at ~50 km above the plate boundary. I find this confusing (multiple boundaries between similar things labeled differently in different locations).

3) the coloring of the blue and red zones is too similar, and could be confused with, the grey color of the mantle wedge.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
The authors have addressed all reviewer comments and improved the clarity and strength of the arguments presented with their modifications. The updated explanation regarding evidence for advection and advection-controlled DPC in particular is clear and well-supported. The authors have also strengthened the section on implications for subduction zone processes.

My only substantive comment, relating to clarity rather than content, is that the paragraph from Lines 325 to 346 is still somewhat opaque. After reading it several times I gather that the argument being made is that Pl1 domains are further from equilibrium, react more, and thus become weaker than Px1 domains producing "block-in-matrix" structures. However, this is not entirely clear and the paragraph could use reworking in general to better communicate the argument. A few specific comments:
Line 325: "more important" seems to be describing the increase grain size reduction in Pl1 domains relative to Px1 domains. Perhaps "more extensive" or "more developed" would better communicate this point?
Line 327: "is rather controlled by solubility differences" is confusing because there is not a clear statement this is in contrast to (use of "rather"), and feels out of place because this is the only mention of solubility in the paper. Rewording to avoid using "rather" would make this statement clearer. I assume solubility is meant to refer to the greater disequilibrium experienced by Pl vs. Px (this terminology is used further down in the paragraph) and greater reaction modulated by the ability of Pl to dissolve and reprecipitate. Perhaps use "large chemical disequilibrium" here as is used below for clarity?
Line 337: "confronting" suggests the authors see contradictory results. "Comparing" might better communicate the similar process responding to differing degrees of disequilibrium.

Additional comments below are largely cosmetic but would improve readability:
Line 69: THMC is still listed as an abbreviation here despite being removed later on in the manuscript.

Fig. 1, Line 78 and Line 90: Stated strain rates differ between these three locations. I would suggest modifying so they are consistent or explain why they are listed different in each case.

Fig. 3B: Does this appear somewhere in Fig. 3A? Or is it just a zoomed in view of a comparable area?

Fig. 3-5: In figures Cpx and Opx are distinguished while in the text Px is used, I'm assuming to refer to both. Perhaps note where Px is first used that it refers to 1st or 2nd generations of both Opx and Cpx.

Line 248: "The latter predicts" is confusing since the previous sentence didn't include a list of two things, unless this refers to wet Qz specifically vs. wet Px in the parentheses. Maybe instead "These maps predict" would be clearer?

Fig. 7 and 8: I found at some points in the last paragraph I had to flip back and forth between these figures. Perhaps they could be combined since they are all related to the implications of this work.

Reviewer #4

(Remarks to the Author)
Review of the manuscript entitled “Deep crustal deformation driven by reaction-induced weakening” by Soret et al.

This manuscript reported experimental investigations on reaction-induced weakening of a simulated mafic fault gouge. The results illuminate that the interconnected thermo–hydro–mechanical–chemical processes underpinning crustal shear zone development, regardless of the mineral aggregate plastic strength. These weakening could be an origin of the weakness of the subduction plate interfaces beyond the seismogenic zone. I feel the authors revised the manuscript based on the first review very well. However, I found some comments/questions on the manuscript. I hope this help to improve the manuscript.

1. My biggest concern is the effect of melting. The experimental condition is quite close to or beyond the hydrous/dehydration melting point of the mafic rock (e.g., Hacker et al., 2003, JGR; Rushmer, 1995, JGR). Especially, Rushmer (1995, JGR) found melts in amphibolite at 1.8 GPa and 850˚C, which is very close to the experimental condition of this study. I’m very curious whether the weakening in this study is not by hermo–hydro–mechanical–chemical processes, but actually by a weakening by partial melting (e.g., Hirth & Kohlstedt, 1995, JGR; Holtzman et al., 2003, EPSL) of the sample. I should emphasize that only a few vol% of melt could drastically weaken the rock, so the authors need to double check that there are really no melt in the recovered samples. I'm not an expert, but to me the mixtures of white and black spots in Fig. 4 and the PX1-rich domain in Fig. 5 all look like quenched melts... Based on the discussion in the manuscript, there is no melt at the PT condition of the natural subduction zone, so this issue is a critical in the entire discussion in the manuscript.

2. Fig 2b: To shear a 1 mm thick layer (line 444 in the manuscript) to a shear strain of 10 requires a displacement of 10 mm. The length of the sample relative to the shear direction is probably less than 10 mm, making it impossible to earn such a displacement. Even if the sample is 0.5 mm thick (which is likely from Fig. 2b), a displacement of 5 mm is required and the portion of the sample deformed to shear strain 10 must be very limited. Therefore, the top-right schematic in Fig. 2b is clearly wrong and the overlap between the upper and lower pistons should be much less. The actual amount of shear strain in “high shear strain zones” in Fig. 2b, which look to last for several mm in the figure, should be less than expected.

3. Relating to the comments #1 by Reviewer #3 and the reply to it, finding the evidence of dissolution and precipitation in the recovered sample doesn’t mean that the mechanism of the weakening. It could be still possible that the overall strength of the sample is controlled by the dislocation creep of quartz, because the quartz flow law shows lower stress than that observed in the experiments. For example, Getsinger and Hirth (2014, Geology) found that the rheology of the hydrating basalt to amphibolite is controlled by not reaction-induced process but the diffusion creep of plagioclase. I would personally like to support the author's hypothesis, but the evidence is insufficient. I do think that we should at least try to obtain additional strain rate dependence of mechanical data to prove that the sample is not deformed by dislocation creep.

Other comments:
Fig. 1: Translucent lines/squares are better than stars for quartz and feldspar strengths.

Fig. 1: add a scale in Fig 1b.

L36-37 and L392-393: It’s not exactly correct. The downdip limit of the seismogenic zone is depending of the subduction zone. In some cold subduction zones, plate boundary seismogenic zone extends to a middle of the mantle wedge (e.g., Kita et al., 2010, Tectonophysics). In hot subduction zones, seismogenic zone is constrained by the mantle wedge (see Oleskevich et al., 1999, JGR; Obara and Kato, 2916, Science).

Version 2:

Reviewer comments:

Reviewer #4

(Remarks to the Author)
Now I've read the revised manuscript and the rebuttal, and I found that the authors have addressed reviewers' comments. No further comments on the manuscript from me. I think the manuscript is ready for the publication.

Detailed answers to all comments can be found below in blue. The text and the figures have been significantly improved and clarified. Please note that portions that have been changed are highlighted in red in the new text.

Here is a quick overview of the main changes brought to the manuscript, to answer the reviewers' criticism and improve it:

Reviewer #1:

- We have added robust evidence for fluid advection in the main shear zones (e.g., the zoisite content testifies to local fluid concentration by a factor 3).
- We have deepened our thinking about the role of fluid advection on reaction kinetics and provided quantitative values for mass transfer rates as a function of the strain intensity. It is now more clearly demonstrated that fluid advection enhances mass transfer and reaction kinetics, and thereby dissolution – precipitation creep (DPC). A large section of the discussion is now dedicated to this demonstration.

Reviewer #2:

- We agree that metasomatism plays a key role in strain localization along viscous plate interfaces. Our study shows that strain localization by DPC can enhance fluid flow, fluid–rock interactions and thus metasomatism. As correctly pointed out by the reviewer, metasomatism can in turn enhance chemical disequilibrium, reaction kinetics and thus DPC. This is now explicitly stated in the manuscript.
- We have improved the upscaling of our results to natural systems by comparing our mechanical data to existing flow laws. This reveals that transforming eclogites can become nearly as weak as wet quartzite, thus showing that strain localization (at a "natural" strain rate of 10^{-12} s^{-1}) can require only a few tens of MPa of differential stresses (and not hundreds of MPa). The final figure has been corrected accordingly.

Reviewer #3:

- We have clarified that strain weakening is controlled by thermo-hydro-mechanical-chemical processes rather than dislocation creep, contrary to the "conventional idea". The fine-grained aggregates in shear zones nucleated after dissolution of the primary grains and mass transfer through a fluid phase. It is important to note that there is no evidence for significant grain size reduction by dynamic recrystallization in our experiments.
- We have better connected our results to field-based studies indicating that DPC is the dominant deformation mechanism controlling viscous strain localization at great depths. The geodynamic model discussed at the end of the manuscript has been largely corrected and improved, with a better link with the geological rock record, thermodynamic modelling and existing flow laws.
- We have carefully checked all the citations used in the manuscript. The few inconsistencies have been meticulously corrected.

Sincerely,
Mathieu Soret, on behalf of all co-authors

==========

Reviewer #1 (Remarks to the Author):

The paper describes a very nice set of experiments at high temperature and pressure where reactions and deformation are intertwined. The microstructures clearly indicate that the main deformation mechanism is dissolution-precipitation creep. The fact that these experiments are performed on mineral assemblages, pressures and temperatures relevant for deep crustal sheer zones makes the study an important contribution.

On line 252-3 the authors state that "heterogeneous compositions of reaction products are inconsistent with diffusive transfer from the dissolving sites." Maybe this is obvious to the authors, but I do not understand from the text and figures why diffusive mass transfer is incompatible with "heterogeneous compositions of reaction products". I suppose the latter means several different mineral types. The question is then more about the stability of the minerals and their relative growth kinetics (that can change with time doe to changing stresses in the shear zone...) I also do not understand why advection would make any difference compared to diffusion. Which assumptions of fluid pathway cross-sections, growth kinetics, etc have led the authors to calculate and conclude that "length-scales and anisotropy of crystallization tails and shear bands are incompatible with fluid-assisted diffusion rates"? (lines 254-255). The hypothesis of advection controlled DPC seems somewhat unfounded and I fail to find mention of any such process in ref 26 referred to in lines 262-263.

We agree that the length-scales and anisotropy of crystallization tails cannot be directly used as evidence for fluid advection. This constraint has been removed.

Following the reviewer's suggestion, we now provide a more detailed demonstration of our approach highlighting the contribution of fluid advection in high-strain domains:

1) (ll.293–301) The content (~30 vol.%) of zoisite (the only hydrous phase in our experiments) in the core of the main shear zones implies local fluid concentration by a factor 3 compared to the amount of added water in the starting material. Such fluid concentration during strain localization corroborates previous experimental findings (e.g., Précigout et al., 2019) and a plethora of field-based studies (some examples worldwide are now cited in the manuscript).
2) (ll.280–293) We have compared the widths of reaction fronts in low and high-strain domains with existing values of intergranular diffusivities of Al (Carlson, 2010). Al is inferred to be the least mobile element in our system and thus to be rate-limiting in the dissolution and precipitation processes. We show that the width in low-strain domains are compatible with reaction kinetics controlled by diffusion near fluid-saturated conditions. However, reaction kinetics are too fast (by at least ~2 orders of magnitude) to be solely explained by intergranular diffusion, even under fluid-saturated conditions.

The conclusion in lines 271-273 "The rapid and dramatic stress drop (up to 74% of the peak stress) during strain localization can, therefore, be explained by fast reaction kinetics and efficient mass transfer" seems well founded. But the Authors have not demonstrated that this fast reaction kinetics and efficient mass transfer must be due to advection as opposed to diffusion.

The effect of fluid advection on reaction kinetics, thereby on DPC is now better detailed in the discussion. In low-strain domains, reaction kinetics is limited by diffusion-controlled mass transfer. In high-strain domains, reaction kinetics is no longer limited by diffusion because fluid advection dominates and allows faster intergranular mass transfer. Faster reaction kinetics leads to faster grain size reduction, thus faster weakening.

With only 0.2% water in the samples I find it hard to imagine "fluid flow" as described in the introduction (line 26).

We acknowledge that the term “fluid flow” might be confusing. The permeability at high-pressure conditions is extremely small but our experiments corroborate previous experimental and field-based studies as they show that viscous and brittle deformation can drive transient local fluid flow (Fusseis et al, 2009; Menegon et al, 2015; Marti et al, 2017; Locatelli et al, 2018; Précigout et al, 2019). Transient fluid flow may also arise from densification reactions (Plümper et al, 2017; Putnis, 2021; Bras et al, 2023). This is now clarified in the discussion section (ll.302–307).

The experiments are well conducted, they show interesting shear weakening due to DPC and the implications for subduction zones seem plausible, even without invoking a speculative "advection" DPC.

The presence of fluid advection is an important new constraint but it does not represent the only outcome of our study. We agree with the reviewer that the main outcome of our study is the fact that strain localization in transforming mafic rocks at HP conditions is due to DPC, with strong implications for the subduction zone dynamics.

==========

Reviewer #2 (Remarks to the Author):
This manuscript presents exciting new experiments demonstrating the importance of fluid-mediated chemical reactions in controlling the strength of subducted material, and plate boundaries more generally. A comparison of low and high strain experiments clearly illustrates the evolution of the system from incipient reaction to interconnection of reaction products and drastic weakening. Spatially-resolved analysis of textures and mineral compositions provides important insights into the governing deformation mechanisms. The feedbacks between chemical reactions, fluids and deformation presented here are at the cutting edge of subduction zone science, structural geology, and rock deformation, and these experiments meaningfully advance this topic.

Major comments:
-Influence of metasomatism: Much of the discussion around the implications for these experiments focus on the influence of metamorphic reactions within a given lithology and how that might affect the location of the plate interface and distribution of seismicity. However, plate interfaces are near-universally characterized by extensive metasomatism between slab and mantle wedge lithologies that are much farther from equilibrium than any metamorphic reaction in a single lithology. The processes outlined in this manuscript certainly apply in that case as well, though the precise expression might differ because of the distinct initial geometry of metasomatic exchange across a boundary vs. metamorphic reactions within a rock. These rocks would also significantly modify the proposed model of

interface down-stepping dependent on metamorphic reactions and fluid release. Discussion of the implications of these experiments in natural subduction zones, necessitates some consideration of metasomatic processes and metasomatized plate interface shear zones observed worldwide.

We fully agree with this comment. It is well-known that metasomatism can significantly modify the disequilibrium state of the rocks and therefore can promote the efficiency of dissolution precipitation processes (e.g., John et al., 2008; Angiboust et al., 2014; Bebout and Penniston-Dorland, 2016, Locatelli et al., 2018). Positive feedback with deformation commonly occurs as metasomatism requires the opening of transient permeability to allow the infiltration of external fluids. In the previous version of the manuscript, mineral reactions were implicitly assumed to encompass metasomatic processes, but this has been clarified. The end of the discussion (ll.404–408) now includes a more comprehensive section outlining the close relationships between metasomatism and the development of viscous shear zones.

-Extrapolation to natural systems/subduction zones: These experiments are run at higher temperatures and stresses than expected in modern subduction zones. While this is an unavoidable side-effect of deformation experiments at human timescales I believe it warrants some comment. What strain rates could be accommodated by these processes at stresses expected in subduction zones and how do they compare to rates for aseismic creep, slow slip, etc? How would colder temperatures impact the kinetics of reaction and mass transport? How should the strain localization observed at this small-scale be expressed at the scale of the downgoing slab? Some brief comments on extrapolation to natural systems would strengthen the implications of this work.

We agree that colder conditions would result into lower reactions kinetics. Yet, it is noteworthy that deformation experiments are conducted at high temperature conditions because experimental strain rates are 6-7 orders of magnitude faster than in nature. For instance, the “classical” flow laws of anorthite and wet quartzite, defined by Rybacki and Dresen (2000) and Hirth et al. (2001), are based on experiments conducted at ≥ 900°C.

To further extend the applicability of our findings to natural systems, we now provide a comparison of our mechanical data with existing flow laws (ll. 382–390). This shows that the strength of a transforming eclogite tends to that of a wet quartzite. We therefore infer that transforming eclogites have the potential to accommodate relatively fast strain rates (10^{-12} s^{-1}) at relatively low differential stresses (only few tens of MPa) – values that are expected along viscous plate interfaces.

Moreover, there is a growing body of field-based evidence indicating that reaction kinetics can be much faster than previously assumed if fluid-assisted mass transfer is efficient. For instance Beinlich et al. (2020) found that “the velocity of the fluid-driven reaction fronts can propagate at up to 10 cm per year, equivalent to the fastest tectonic plate motion and mid-ocean-ridge spreading rates” (ll.300–301).

-Differences between experiments: Figure 1 shows three different high strain experiments with different stress-strain curves. What controls the differences between these experiments (mineralogy, strain rate, etc.)?

Differences in strength among samples reflect differences in samples thickness (modifying the effective strain rate). This information is now provided in the text (l.90) and a new table (table S1) given as supplementary information.

-Figure clarity: In Figure 3, nanopores are not clearly visible in any of the images where they are labeled. Please include a more zoomed in image showing these features. In Figure 3, some images also lack annotation for all phases/domains. While I was able to figure out what each corresponded to eventually, additional labels might improve readability/clarity for the audience. In Fig. 3i, can the shear plane be marked for comparison with grain shape?

OK. Figure 3 has been reshaped. New microphotographs have been added (new Fig. 4 and 5). The phase labelling has been improved and nanopores on TEM images are now more visible.

-Implications and Figure 5: How were the plotted Alpine rocks chosen? Are they specifically from the plate interface or just a survey of mafic and sedimentary Alpine samples? For the mafic rocks, is MORB a good representation of their composition? Slow spreading oceanic crustal setting, significant seafloor alteration, limited basalt volumes, and unusual gabbro compositions (e.g., Mg-Al and Fe-Ti gabbros) probably make these Alpine mafic rocks a poor fit for classic MORB.

The plotted Alpine rocks belong to a compilation made by Agard et al. (2021). They correspond to sedimentary, mafic, ultramafic rocks metamorphosed at HP conditions during the Alpine subduction. Determining their precise location relative to the plate interface is challenging as the plate interface is always lacking (potentially lost during exhumation). Yet, most of these rock units exhibit viscous and brittle deformation features formed at (near) peak metamorphic conditions (e.g., Angiboust et al., (2012) and Locatelli et al. (2018) for the MonViso unit; Herviou et al. (2022, 2023) for the Schistes Lustrés) at or near the kinematic plate interface (Agard et al., 2018). This is now outlined in the manuscript (l.375).

The Alpine gabbros exhibit a large range of compositions. Yet, in average they closely resemble to a MORB, and small chemical differences do not affect the position of the main mineral reactions. The transition between the blueschist and eclogite facies consistently occurs at ~500°C (Angiboust et al., 2012; Locatelli et al., 2018). Moreover, Hacker (2008) demonstrated that the main dehydration reactions take place within the temperature range of 500–600°C, regardless of the mafic rock composition (MORB, gabbro, troctolite, etc…) and the initial amount of water (see his Figs.1 and 2).

Minor comments, questions and food for thought:

-Line 84: How was this water added and could it escape during the experiment? Approximately how much of the added water was eventually consumed to form hydrous minerals (zoisite, paragonite)?

The water was added to the sample powder prior to the experiments. There is no reason for the water to escape as the sample and shear pistons (also called forcing blocks) are placed in a weld-sealed platinum jacket with a nickel foil insert.

Paragonite only occurs in trace amount but zoisite can represent up to 30 vol.% of the mineral assemblage in the high-strain zones. This testifies to local fluid contents (~0.6 wt.%) exceeding by a factor 3 the amount of added water in the experiments. Such fluid concentration represents a robust evidence for fluid flow from the low-strain zones to the high strain zones. This is now detailed in the manuscript (ll.291-295).

-Lines 155-157: Later it is noted that the difference in layer thickness between Pl-derived and Px-derived reaction products reflects strength contrast as well.

Later in the manuscript it is noted that Px-rich domains are mechanically stronger than Pl-rich domains due to slower reaction kinetics. In the high-strain zones, relics of primary Px grains are common, while primary Pl are completely dissolved and replaced.

-Lines 385-387: Water content was kept to a moderate level to prevent pore pressure effects, however significantly fluid pressure fluctuations are expected from many metamorphic reactions and due to fluid influx at the plate interface. How would increasing fluid pressure influence the deformation of a reacting system like this?

Pore pressure effects are expected to occur for water contents higher than 0.5 wt % (Kronenberg and Tullis, 1984). Fluid concentration may therefore lead to local pore pressure effects and brittle deformation in our experiments as well as in nature. This is noted in the discussion (l.273).

-Lines 242-244: Also shown in analog experiments by Ladd & Reber (2020).

Indeed. We had missed this one. It is now cited.

-Lines 252-255: What are the expected intergranular diffusion rates? Not sure I understand how anisotropy plays a role.

The intergranular diffusion rates in presence of fluids are given by Carlson (2010) based on the widths of reaction coronas around partially resorbed garnet crystals.
The role of anisotropy is no longer discussed in the revised version of the manuscript.

-Line 262: I did not find a mention of advection-controlled DPC or the impact of advection versus diffusion in the cited reference.

OK. The cited article shows that DPC in hydrated mafic rocks can account for strain rates of 2–3 orders of magnitude higher than the ones predicted by diffusion creep flow laws. The reviewer is correct that the article does not mention the presence of fluid advection. The sentence has been deleted.

Grammar/spelling suggestions with no bearing on the quality of the manuscript:
Line 49: “like” suggests that the minerals are not load-bearing in experiments but are in crustal rocks. “in” might better convey your intended meaning.

OK. Done.

Line 56: Additional “,” after “present-day plate tectonics”.

OK. Done.

Line 71: “on crustal rheology” rather than “on the crustal rheology”

OK. Done.

Line 125: It is a little unclear what is referred to by “The fine-grained mixtures”.

Sentence corrected. “The fine-grained mixtures of reaction products consist of”.

Line 177: “interconnected by” not “interconnected between”.

OK. Done.

Line 186: “forming” not “structuring”.

OK. Done.

Line 281: Maybe “departure from” rather than “distance to”.

OK. Done.

Line 400: “for” not “from”

OK. Done.

==========

Reviewer #3 (Remarks to the Author):

The reasons for the existence of plate boundary shear zones, and the phenomena of their initiation and persistence are critically important aspect of geodynamics and incompletely understood. A major reason for this uncertainty is a poor understanding of the rheology of the typical polyphase rocks that exist at depth. The authors of the submitted article describe several high-strain deformation experiments on polyphase rocks at high pressure and temperature outside of metamorphic equilibrium. During the experiments, the initial mineral phases broke down in areas of deformation and recrystallized as new phases (or compositions). While several experimental conditions (e.g. grain size and strain rate) are quite different from natural conditions, the experiments produce microstructures that look like naturally deformed samples. Key observations from the experiments include 1) considerable strain weakening, 2) strengths lower than those of mono-phase aggregates (although this isn't quantified in the paper), and 3) occurrence of recrystallized mineral phases in fine-grained, polyphase, highly-strained layers. Although only four experiments are presented, these are interesting results. However, I found the discussion of the implications of the results over-reaching: I don't find many of the big ideas put forward well supported by the results and previous literature.

A couple concerns of central importance:

1) Part of the hypothesis put forward in the paper (fig. 5c insets) suggests that weakening is followed strengthening once reactions cease. The strengthening is not observed in the experiments, and I doubt it would occur if they went to larger strain (are there no experiments in the literature or from this lab on such fine-grained aggregates to compared to?). Two critical observations: 1) "Incipient strain localization and weakening at peak-stress conditions coincides with the interconnection of reaction coronas of fine-grained reaction products" (197), and 2) Metamorphic reactions occurred from the beginning of the experiments, including during the high stress phases. Based on these two observations, the obvious interpretation to me is that it is the interconnected fine-grains that are responsible for weakness, not the "thermo–hydro–mechanical–chemical processes" or "chemical disequilibrium" as suggested by the authors. My interpretation is a rather conventional idea (grain size reduction causing weakening, e.g. quartz regime 1 (Hirth and Tullis, 1992)) but it isn't considered in the paper. If it were true though, then it undermines another hypothesis in the paper related to plate interface processes:

We respectfully disagree with the reviewer's interpretation.

Our study shows that the "conventional idea" of rock mechanics cannot be directly applied to polymineralic rocks undergoing mineral reactions. The "conventional idea" is based on deformation experiments conducted on monomineralic, unreactive aggregates (e.g., quartz by Hirth and Tullis). In such conditions, strain is typically accommodated in the dislocation creep regime. Contrary to the reviewer's statement, we do consider this deformation mechanism in our study. We do indicate that the primary porphyroclasts from the starting material exhibit plastic deformation but the microstructures clearly indicate that it is not the dominant deformation mechanism. Shear zones predominantly consist of fine-grained mixtures that do not show any plastic deformation. This lack of plastic deformation is well documented in the revised text and new Fig. 5. Moreover, we present multiple lines

of evidence demonstrating that the fine-grained mixtures formed by dissolution and precipitation processes driven by the strong chemical disequilibrium state of the starting material at HP conditions. Once chemical equilibrium is reached, grain coarsening leading to rock strengthening is expected as dissolution precipitation creep ceases. This evolution has been already proposed based on field observations (e.g., Koons et al., 1987; Rubie, 1998; Stünitz et al., 2020; Bras et al., 2021).

2) The authors conclude the paper with the hypothesis that weakening associated with metamorphic phase changes (like they see in their experiments) controls the location of the upper surface of subducting slabs. It seems likely based on the experiments presented that metamorphic changes accelerate weakening, but a quick calculation sheds some light on their hypothesis: The weakening associated with metamorphic reactions in the experiments occurs after shear strains of ~3 to 5. Extrapolated to the plate scale, where plate interfaces are ~1 km thick, the temporary (if it is in fact temporary) weakening associated with a significant metamorphic phase change would thus occur in a region ~3 to 5 km in length. That's an order of magnitude smaller from what would be needed to explain the gradual change in plate boundary location over ~40 km pictured in figure 5c. In other words, the (supposedly) transient weakening effect they document would appear to be "used up" far too quickly to have large-scale implications as the authors suggest.

Not sure what the reviewer means here. Weakening occurs in our experiments at a strain rate 6 times faster than in nature. It is also essential to note that, unlike our experimental setup, P-T conditions (and bulk compositions if metasomatism) continuously evolve during rock burial, thereby maintaining chemical disequilibrium through time.

Line by line comments:

21. "groundbreaking" is overly enthusiastic/biased for a science manuscript.

OK. Swapped for "novel"

23. Somewhere in the paper this should be quantified (comparison of the new stresses with dislocation creep stresses).

OK. Done.

24-27. Text like this in an abstract suggests that evidence for such things was documented in the document, but it is not (e.g. no quantification of the effect of (supposed/undocumented) dynamic permeability).

The abstract has been clarified.

30. "Inception" of plate interfaces, i.e. subduction initiation? This topic is not discussed in the document.

The topic of subduction initiation is now more discussed in the text.

32. "rooted in … chemical disequilibrium and fluid concentrations of the slab materials." Cut "fluid concentrations". There is a figure (5a) that depicts H2O concentrations of different metamorphic rocks that likely exist in subduction zones, but there isn't enough discussion of fluid concentrations to warrant being included in the abstract.

The topic of fluid flow is now discussed in more details in the text.

58-66. There is a large and unjustified leap taken here. It is stated that metamorphic reactions and deformation are sometimes associated (58-59), and also that dissolution-precipitation creep (DPC) promotes localization (60-62). It does not follow from this that "the development of crustal shear zones...should then be driven by...chemical (dis)equilibrium…and…mass transfer along grain boundaries For one thing, there are lots of

other factors that influence the development of crustal shear zones and localization (like grain size reduction). Also, many crustal shear zones occur at PT conditions where the deforming minerals are stable. (Also, if this were well established or self-evident as presented here, it would undermine the justification for the paper.)

The last sentence has been removed. The other deformation mechanisms (dislocation creep, diffusion creep, brittle deformation) are now explicitly listed as potential driving mechanisms for the development of shear zones.

128-129. "Dissolution dominates on the interfaces of primary phases subnormal to the shortening direction," The evidence for this should be given.

This section has been rephrased. It is now mentioned that plastic deformation could be an alternative mechanism to dissolution in the development of the primary grain aspect ratio (ll.132–135). Unfortunately, EBSD analyses yielded insufficient mineral indexation.

136-137 Cut "along the short diagonal". I find this text confusing and unnecessary. I can see from figure 2b what's meant.

OK. Done.

141-142. There are other important factors not mentioned, e.g. rotation.

Not sure what the reviewer means.

150. Euhedral isn't the correct word. Lot's of the grains shown in figure 3i have curved boundaries.

This sentence has been rephrased to highlight that most grains display euhedral to subhedral shapes.

157-159. This observation (high densities of defects) indicates that dislocation creep occurred during early phases of deformation. This may be an important part of the time-dependent rheology of the experiments that should be discussed further.

We do discuss the role of dislocation creep. We show that it does not accommodate a significant part of the strain, even during the first stages of the shear zone development (l.246–250).

163. "Red arrows point to areas of total Pl1 consumption" This could be better explained. Do you mean "Red arrows point to small Pl1 grains, which we interpret as likely sites of near-total Pl1 consumption"?

OK. The sentence has been modified as follow: "Red arrows point to Pl1-rich domains that have undergone full reequilibration in the vicinity of a C-type shear band".

186, 188. From my (limited) knowledge of the cited locations, referring to them as "fossil plate interfaces" is an exaggeration that undermines the meaning of the term "plate interface." These are simply rocks deformed near plate boundaries, no different than many (entire) mountain belts like the Alps. "Plate interface" is commonly understood to be a zone no more than 1-2 km thick (e.g. Agard 2018, fig.2).

OK. The sentence has been modified as follow: "formed at or close to plate interfaces", following the terminology of Agard et al. (2018).

187. The experiments look similar to natural samples. "Suggests" would be a more appropriate term than "highlights"

OK. Swapped for "indicates".

191. "Shear zone infancy is characterized by insignificant strain". This confuses me. By definition the infancy of a shear zone involves minimal strain, so what is the purpose of stating this? Needs rephrasing.

This section has been completely rephrased to underscore the high strength of mineral aggregates deforming by dislocation creep.

192. “indicating that the coarse-grained starting material is too strong to deform by dislocation creep at experimental strain rate conditions”. This is directly contradicted by the results which show significant concentrations of dislocations in minerals existing from the beginning of the experiments (fig. 3g). This seems problematic for an important message of the paper—Apparently, some non-trivial amount of strain (shear strains of 2 to 4, fig.1) at high stress, likely associated with dislocation creep occurs before the weakening documented here begins to dominate.

Correct. We now outline that strain localization by dislocation creep requires unrealistically high differential stresses (> GPa), since plagioclase is unstable at high-pressure conditions and much weaker than the other minerals predicted to be stable.

200. The word “instabilities” has specific rheologic meaning that might not be intended here. I suggest just using the word “weaknesses”.

OK. Done

219. The term “in contrast” is unclear. The previous paragraph mentioned “Increasing strain localization,” here we are also talking about that (sheared reaction products).

OK. Swapped for “instead”

230-232. Just because fluid-assisted mass transfer is faster, and there is some fluid in your experiments doesn’t prove that it is the main mechanism. It seems likely, but jumping to conclusions that should be better based on comparison of experiments with different fluid contents (or some other evidence).

The result and discussion sections now provide a more comprehensive examination of the influence of fluid on mass transfer and its interplay with deformation.

234-235. These experiments are a good example of “strain localization”. There is a zone of higher strain through the sample, but this appears most obviously related to the position of the pistons (fig. 2), i.e. to changing boundary conditions such that there is no reason, after a certain amount of strain, for material outside of this central zone to continue deforming. “Strain localization” suggests that strain weakening of the material is causing localization. Certainly, the material is weakening (Fig.1), but whether or not that causes localization is not determined in these experiments. Thus, I don’t see evidence here to argue that “localization in shear zones may not necessarily require brittle precursors”.

Disagreed. The peak-stress experiment clearly shows that grain size reduction by dissolution and precipitation is enough to trigger strain localization. At gamma = 1, there is no “high-strain zone” in the sample. Our experimental configuration resembles the geological scenario of a highly reactive mafic lens embedded within a less reactive granulite (like the well-documented example of Holsnoy, Norway).

233-246. This paragraph seems quite speculative on topics outside the main scope of the paper.

Disagreed. The stage of strain weakening is associated with extensive brittle deformation that needs to be described and discussed. The mechanisms driving brittle deformation at high-pressure conditions in nature remain largely elusive – partly due to the scarcity of experimental data. Some hypotheses are discussed in the text, based on our experiments and the literature (ll.269–276).

248. Section on DPC. I had to re-read this several times to make sense of this paragraph. There is a bold hypothesis here that fluid-assisted diffusion isn’t the most important process, instead “advection processes induced by…pressure gradients, dilatancy and brittle failure”. This needs more evidence and clearer explanation. I think it would be helpful to explicitly differentiate between fluid-assisted diffusion and fluid-assisted(?) advection (or whatever

term the authors want to use). I needed to read this several times, and I'm still not 100% sure I follow, e.g. at first I thought the authors were invoking pressure gradients as a means of transfer of solid material. The last sentence of the paragraph (263-265) though talks about a standard understanding of pressure gradients, so I guess the authors are talking about the advection of fluid containing ions rather than the diffusion of ions through a fluid. This should be stated much more directly.

OK. This section has been entirely rewritten to provide additional constraints justifying the presence of local transient fluid advection in the high-strain domains. We think that the terms "fluid advection" and "fluid flow" are clear enough, as they are commonly used in the literature (297–307).

259. The citations about dilatancy and brittle failure are unnecessary and distracting here—everyone knows what these processes are; citations in this paragraph instead shed some light on advection-controlled DPC if possible. This is one example of many in the paper of (nonstandard) use of citations almost like web-links (e.g. an underlined word on a website that links to a wikipedia article or some definition of a term). What's needed in this document are more citations used to support assertions that are made. In the example of this sentence, if there's a citation present it should demonstrate some precedent for the main idea of this sentence (mass transfer being controlled by advection processes)—has that been considered or proposed before? If not, then I expect more explanation for it. As it stands, I don't know if this is a new idea or not.

We thank the reviewer for the constructive feedback. This section has been rewritten.

252. "heterogeneous compositions of reaction products are inconsistent with diffusive transfer from the dissolving sites." Why would heterogeneous compositions be more consistent with advection-controlled DPC than fluid assisted diffusion? Can you support this with citations? Also, I don't see compositional variation in figure 3f (other than between Cpx1 and Cpx2)? And as far as figure 4b and 4c go, given the large scale, it's hard for me to be certain that I see compositional variation of reaction products (maybe some arrows would be useful). For example, I can see some fine-grained areas are more yellow or blue, but is that a result of compositional variations in recrystallized minerals (as the authors imply), or because of local variations in the concentrations of one mineral species versus another, or because there is local variation in the abundances of the various fine-grained remnant grains?

This section has been removed.

253-256 "Moreover, length-scales and anisotropy of crystallization tails and shear bands are incompatible with fluid-assisted diffusion rates, especially in environments where porosity is only locally and transiently connected." This is quite arm wavy but probably could be quantified with some back-of-the-envelope calculation. Also, given that the rates from the Carlson reference are from natural samples, are those rates directly comparable to those inferred from the current experiments? A better comparison perhaps would be to rates of diffusion in fluids in static experiments (perhaps the work of C. Manning is relevant here). Additionally, If the authors are, as I understand, invoking fluid as the medium within which ions are advected (from sites of dissolution to sites of precipitation), then the second part of this sentence doesn't make sense to me since advection of ions in a fluid also requires porosity (i.e. local and transiently connected porosity also sounds like a problem for "fluid-assisted advection" invoked by the authors). Finally, what does any of this have to do with anisotropy ("anisotropy of crystallization tails")?

Because Al is one of the least mobile components in our system, we assume that it will act as the rate-limiting mechanism in the Pl_1-rich domains.

The width of reaction products (> 5 µm) in our peak stress experiment (~140h) suggests transfer rates around $10^{-18,-17}$ $m^2.s^{-1}$ (following Carlson's equation). In the high-strain zones (formed in 20–30h, from peak stress conditions), total dissolution of primary plagioclase (with a radius of 50 µm) suggests transfer rates around 10^{-15} $m^2.s^{-1}$. The slow rate in low-strain domains is consistent with intergranular diffusivities of Al estimated in systems reaching near fluid-saturated conditions (Carlson, 2010). However, reaction kinetics are too fast (by at least ~2 orders of magnitude) to be solely explained by intergranular diffusion, even under fluid-saturated conditions (ll.291–292).

262. The authors make a strong contrast between the traditional understanding of DPC and…some other behavior they believe they see (lines 250-25e). This new mechanism of mass transfer proposed by the authors "advection-controlled DPC" either needs some citations or a much better description. Also, there is no mention of "advection-controlled DPC" in the cited paper (Mansard et al., 2020) (which presumably was cited as evidence that this process dramatically weakens rocks).

The term "advection-controlled DPC" is no longer used in the manuscript.

Mansard et al. (2020) clearly showed that DPC was the dominant mechanism controlling strain weakening in their experiments, capable of accounting for strain exceeding those predicted by diffusion creep flow laws by 2–3 orders of magnitude higher than the ones (see their Fig. 13). We agree with the reviewer that it would have been beneficial to add a reference to Précigout et al. (2019, Scientific Report) who experimentally demonstrated fluid concentration (therefore advection) from low- to high-strain zones. It is worth noting that fluid advection in shear zones is not a novel concept since it has been documented in many fossil shear zones across various geological settings worldwide.

248 I'm quite confused by this section. Did the authors simply rename an already established process "advection-controlled DPC", or are they really proposing something new?

See our response to the previous comment.

259. But it was said earlier that there isn't much brittle behavior

This section refers to brittle deformation in high-strain domains. Earlier in the manuscript it is noted that there is not much brittle deformation in low-strain domains, particularly at peak conditions (prior to substantial strain localization).

267. and/or differences in strength and strain?

Grain size reduction is controlled by dissolution and precipitation processes.

271-272. "The rapid and dramatic stress drop…during strain localization can, therefore, be explained by fast reaction kinetics and efficient mass transfer". This seems like a bold claim that hasn't been really justified. There was more weakening in the plag-rich areas relative to pyroxene ones, but there might be other reasons for this than fast reaction kinetics. For example, maybe the pyroxene is just a stronger phase? Or maybe the strength reduction is due to a faster rate of grain size reduction in the plag-rich areas (partly due to metamorphic instability)?

We respectfully disagree with the reviewer's comment. Pyroxene is inherently a stronger phase compared to plagioclase but plastic deformation does not drive strain weakening. It is clearly demonstrated in our study that the rate of grain size reduction is dominantly controlled by dissolution – precipitation processes, therefore by reaction kinetics and associated mass transfer.

309. Re: the title of the last section "Implications for plate interface processes, from long-term to short-term" (and also line 64): Presumably the authors are referring to earthquakes being short term processes, whereas all the rest of the section is about rather "long term" processes. However the only mention of earthquakes is a reference to the location of the seismogenic zone. "Long-term to short-term" is an interesting topic about how short timescale things add up to "geologic" features, but there's nothing about that in this section. Cut (and try to avoid unnecessary "buzz words"…)

We respectfully disagree with the reviewer's comment. Short-term refers to the brittle deformation reported in our experiments. It is unclear how "short-term" can be a "buzz word".

311-313. "Our study confirms that existing flow laws for non-reactive materials tend to overestimate the strength of the lithosphere deformed in viscous / semi-brittle shear zones." There should be some quantification of this—what flow laws are being considered?

OK. Quantitative values are now provided.

316-317. "continuously located" is ambiguous. Does it mean continuous over time? Or it changes systematically with location? … "Compilation of exhumed materials from fossil subduction zones (e.g., Alps; Fig. 5a) outlines that plate interfaces are not continuously located within sedimentary horizons." This might be better said something like this: "The abundance of both metasedimentary and mafic lithologies underplated during subduction (e.g. Agard, 2021) indicates that the plate interfaces are in many places not located within sedimentary horizons."

OK. The sentence has been rephrased.

318. "Outlines" doesn't seem like quite the right word, maybe "suggests"? However, the sentence is incomplete—simply "compilation of exhumed material", by itself, isn't enough to draw this conclusion, there must be comparison with some other information.

OK. The sentence has been rephrased.

318. What is "it"? Plate interfaces? Figure 5 doesn't help much with this because Plate Interface is not labeled in the figure. However, the cited reference of Agard et al., (2018) defines plate interface as "the domain between the lower and upper plates, including their boundaries." In this interpretation, the sentence seems to contradict itself then: If the plate boundary is the boundary of the plates, how can the boundary also occur within the subducting plate ("dip…into the slab crust and mantle")? Figure 5 seems to show the seismicity boundary (red) dipping into the sedimentary crust and mantle of the lower plate (consistent with the—in my mind, questionable—cartoon of Agard et al., (2018)), so perhaps "it" refers to the seismicity boundary? (e.g. Agard et al., (2018) discuss a low-velocity horizon observed in receiver-function studies, and state that this zone "seems to dip into the slab

and die out once the blueschist to eclogite transformation is completed"). But then, the seismicity would "dip beyond the seismogenic zones" which also doesn't make any sense…

OK. This section of the discussion has been rewritten. "It" was referring to the kinematic plate interface, which eventually localizes in the mafic crust (see Agard et al. in 2018 (Fig. 12) and 2020 (Fig. 15)).

318. What does "dip beyond" mean ("dip beneath"?)

OK. The sentence has been removed.

323. Drop this acronym (THMC). It's only used a couple times and most every reader is going to have to hunt for what it means at this point, since it was introduced and used only once in the introduction.

OK. Done.

322-323. "Such a remarkably similar THMC evolution in subduction zones…". What is "remarkably similar" occurring in multiple subduction zones? The reference 44 (Van Keken et al., 2018) only refers to one place (Japan), and also shows counter examples where the blueschist-to-eclogite transition doesn't correspond to a cessation in seismicity.

OK. The reference to van Keken's work has been removed in the revised version of the discussion.

325. The references here are confusing. Behr and Burgman (2021) describe many different potential mechanisms and modes of deformation, not a "common" mechanism. The Oncken et al reference argues that "cataclastic flow is the dominant physical mechanism governing transient creep episodes such as slow slip events" (which isn't the mechanism discussed here).

The reference to Behr and Burgmann (2021) has been removed. The reference to Oncken et al. (2021) is tied to sentences about brittle deformation, slow slip events.

330-331 A quick look at the reference cited here indicates it has something to do with transient changes in deformation and involves rheology, but it looks like a completely different topic than discussed in this paper. It's a puzzling choice of a reference here, and doesn't seem relevant.

OK. The reference has been removed.

346-347. "the kinematic plate interface into the mafic crust inferred at ≥ 80 km depths; Fig. 5c" I think for this scenario, subduction zones must be continuously retreating and underplating at these depths. That might be worth mentioning

Correct. We have revised our final model. It is now simplified. In this model, strain localization does not require continuous retreating and underplating.

Figure 5c.:

1) It's a little hard for me to tell what some of the lines and labels are intended to indicate here. This is particularly problematic because of the importance of this figure for understanding the section "Implications for the plate interface processes...". Main issues are 1) "Seismogenic zone" seems clearly associated with a thick black line that becomes dashed with depth and then is labeled "recoupling" with the same font treatment at depth… So, I

suspect these labels don't actually refer to the thick line (which should be labeled as "plate interface"??), but they should be oriented horizontally along with the text "viscous/semi-brittle" to indicate the locations of various things.
2) Also, the figure 5 caption doesn't explain what the circled (1) and (2) are, but the coloring suggests they might refer to the (unlabeled) insets for sediments and mafic rocks which include blue "subd. interface 1" and red "subd. interface 2". The connections between these things should be made more explicit.
3) there are two thin dashed lines that I think are labeled "brittle-viscous transition (BVT)" that extend, parallel to, and below, the plate interface, to 100 km depth. "Viscous / semi-brittle" is also drawn in red at ~50 km above the plate boundary. I find this confusing (multiple boundaries between similar things labeled differently in different locations).
4) the coloring of the blue and red zones is too similar, and could be confused with, the grey color of the mantle wedge.

Former Fig. 5 has been extensively revised to address the reviewer's feedback.

Please find below our answers (in blue) to the 2 reviewers comments (in black) along with changes made to the manuscript (in green). We thank the reviewers for their useful comments that we have carefully taken into account.

Reviewer #2 (Remarks to the Author):

The authors have addressed all reviewer comments and improved the clarity and strength of the arguments presented with their modifications. The updated explanation regarding evidence for advection and advection-controlled DPC in particular is clear and well-supported. The authors have also strengthened the section on implications for subduction zone processes.

My only substantive comment, relating to clarity rather than content, is that the paragraph from Lines 325 to 346 is still somewhat opaque. After reading it several times I gather that the argument being made is that Pl1 domains are further from equilibrium, react more, and thus become weaker than Px1 domains producing "block-in-matrix" structures. However, this is not entirely clear and the paragraph could use reworking in general to better communicate the argument. A few specific comments:

Line 325: "more important" seems to be describing the increase grain size reduction in Pl1 domains relative to Px1 domains. Perhaps "more extensive" or "more developed" would better communicate this point?

⇨ Agreed. “More extensive” seems more accurate. Change made.

Line 327: "is rather controlled by solubility differences" is confusing because there is not a clear statement this is in contrast to (use of "rather"), and feels out of place because this is the only mention of solubility in the paper. Rewording to avoid using "rather" would make this statement clearer. I assume solubility is meant to refer to the greater disequilibrium experienced by Pl vs. Px (this terminology is used further down in the paragraph) and greater reaction modulated by the ability of Pl to dissolve and reprecipitate. Perhaps use "large chemical disequilibrium" here as is used below for clarity?

⇨ Agreed. We greatly appreciate the reviewer’s input. The section has been revised accordingly (l.327-347).

Ll334–340: “Differences in chemical equilibrium between the reactants explain the strength contrasts observed within the block-in-matrix” shear zones. Reaction-induced grain size reduction is more pronounced in Pl_1-rich domains than in Px_1-rich domains. Due to the grain-size sensitivity of the deformation mechanisms, fully transformed Pl_1-rich domains are weaker and become eventually larger and better-connected than less reactive Px_1-rich domains (Fig. 4b–d). The rapid and dramatic stress drop (up to 74% of the peak stress) during strain localization is, therefore, primarily attributed to fast reaction kinetics and efficient mass transfer in Pl_1-rich domains.”

Line 337: "confronting" suggests the authors see contradictory results. "Comparing" might better communicate the similar process responding to differing degrees of disequilibrium.

⇨ Agreed. “Comparing” is more accurate. Change made.

Additional comments below are largely cosmetic but would improve readability:

Line 69: THMC is still listed as an abbreviation here despite being removed later on in the manuscript.

⇨ Indeed. We removed the abbreviation.

Fig. 1, Line 78 and Line 90: Stated strain rates differ between these three locations. I would suggest modifying so they are consistent or explain why they are listed different in each case.

⇨ Indeed. The strain rates varied between ~1 to 5 $x10^{-5}$ s^{-1}. This is now clarified throughout the text (l.93) and in Fig. 1.

Fig. 3B: Does this appear somewhere in Fig. 3A? Or is it just a zoomed in view of a comparable area?

⇨ Correct. Fig. 3b is now located in Fig. 3a.

Fig. 3-5: In figures Cpx and Opx are distinguished while in the text Px is used, I'm assuming to refer to both. Perhaps note where Px is first used that it refers to 1st or 2nd generations of both Opx and Cpx.

⇨ OK. Px_1 is now defined in the description of the starting material (l. 96).

Line 248: "The latter predicts" is confusing since the previous sentence didn't include a list of two things, unless this refers to wet Qz specifically vs. wet Px in the parentheses. Maybe instead "These maps predict" would be clearer?

⇨ Agreed. Change made.

Fig. 7 and 8: I found at some points in the last paragraph I had to flip back and forth between these figures. Perhaps they could be combined since they are all related to the implications of this work.

⇨ We understand the concern but Fig. 8 already includes a substantial amount of information. We are not convinced that merging Fig. 7 and 8 would help the reader. However, we will ensure that both figures are placed close to each other in the final version of the manuscript.

Reviewer #4 (Remarks to the Author):

Review of the manuscript entitled "Deep crustal deformation driven by reaction-induced weakening" by Soret et al.

This manuscript reported experimental investigations on reaction-induced weakening of a simulated mafic fault gouge. The results illuminate that the interconnected thermo–hydro–mechanical–chemical processes underpinning crustal shear zone development, regardless of the mineral aggregate plastic strength. These weakening could be an origin of the weakness of the subduction plate interfaces beyond the seismogenic zone. I feel the authors revised the manuscript based on the first review very well. However, I found some comments/questions on the manuscript. I hope this help to improve the manuscript.

1. My biggest concern is the effect of melting. The experimental condition is quite close to or beyond the hydrous/dehydration melting point of the mafic rock (e.g., Hacker et al., 2003, JGR; Rushmer, 1995, JGR). Especially, Rushmer (1995, JGR) found melts in amphibolite at 1.8 GPa and 850˚C, which is very close to the experimental condition of this study. I'm very curious whether the weakening in this study is not by hermo–hydro–mechanical–chemical processes, but actually by a weakening by partial melting (e.g., Hirth & Kohlstedt, 1995, JGR; Holtzman et al., 2003, EPSL) of the sample. I should emphasize that only a few vol% of melt could drastically weaken the rock, so the authors need to double check that there are really no melt in the recovered samples. I'm not an expert, but to me the mixtures of white and black spots in Fig. 4 and the PX1-rich domain in Fig. 5 all look like quenched melts... Based on the discussion in the manuscript, there is no melt at the PT condition of the natural subduction zone, so this issue is a critical in the entire discussion in the manuscript.

⇨ Our experimental conditions were specifically designed to prevent partial melting, given its dramatic effect on rock mechanics – as outlined by the reviewer. We carefully chose the H_2O content added to the starting material (0.2 wt.%) to maintain water-undersaturated conditions. The *post mortem* micro-textures do not show any evidence of melt in either Px-rich or Pl-rich domains, whether at the μm-scale in the BSE images or at the nm-scale in the TEM images. The mixtures of white and black spots in Fig. 4 reflect a very fine-grained assemblage of zoisite, quartz and kyanite (see new Fig. 8c), whereas the Px-rich domains consist of secondary Na-rich pyroxene (see new Fig. 8b). It should be also noted that the absence of melt is consistent with previous experimental studies conducted under similar conditions of temperature, mineral assemblage and water content (Marti et al., 2018; Mansard et al., 2020). **This supplementary information is now added in the text.**

ll.450–453. "Water content was chosen to increase the reactions kinetics and mass transfer during the experiments, while not inducing partial melting or mechanical pore pressure effects[81]. The lack of melt in the reaction products is consistent with previous experimental studies conducted under similar conditions of temperature, mineral assemblage and water content[82,83]."

⇨ The studies of Hacker et al. (2003) and Rushmer (1995) reported melt formation, but the experiments were done on amphibolites made of ≥ 50% of amphibole. Amphibole is a hydrous mineral composed of 2 wt.% of H_2O. In our experiments amphibole is only present in trace amounts (< 5%) in the starting material. Their experiments were thus conducted with a water content in the reactive bulk compositions 5 to 10 times higher than in our experiments. This substantial difference in water content likely accounts for the melt observed in their *post mortem* samples. Moreover, Hirth and Kohlstedt (1995) added melt directly in their starting material, while Holtzman et al. (2003) conducted their shear experiments at 1200°C. Finally, the micro-textures of melts reported in these 4 studies (i.e., amorphous pockets filling triple junctions or cracks) entirely differ from the micro-textures reported in our study.

ll.100–101. "Moreover, none of the experiments has produced microstructures indicative for melt, such as glass films and/or pockets within grain boundaries or microfractures[41]"

2. Fig 2b: To shear a 1 mm thick layer (line 444 in the manuscript) to a shear strain of 10 requires a displacement of 10 mm. The length of the sample relative to the shear direction is probably less than 10 mm, making it impossible to earn such a displacement. Even if the sample is 0.5 mm thick (which is likely from Fig. 2b), a displacement of 5 mm is required and the portion of the sample deformed to shear strain 10 must be very limited. Therefore, the top-right schematic in Fig. 2b is clearly wrong and the overlap between the upper and lower pistons should be much less. The actual amount of shear strain in "high shear strain zones" in Fig. 2b, which look to last for several mm in the figure, should be less than expected.

- ⇨ The initial thickness was ~0.8 μm (and not 1 mm, as previously stated). This is now corrected in the text (l.534). Moreover, the drawing in Fig. 2b does not show the entire length of the pistons in the shear direction (~11 mm).
- ⇨ Shear experiments are characterized by sample thinning, which occurs as a linear function of the axial displacement (Marti et al., 2017). For instance, the final thickness of the sample OR101 is ~0.5 μm (See new Table S1). The sample material only escapes in the shear direction, the strain is taken to be plane strain. The shear strain is thus calculated from the continuous monitoring of two displacement transducers (LVDT) as the sum of individual increments of shear displacement divided by the instantaneous shear zone thickness (see the appendix given by Marti et al., 2017 (JSG) for more details). The progressive sample thinning during shearing explains the actual amount of shear strain between 7 and 10 (despite lateral displacements of 4-5 mm). **This information can now be found in the method section.**

ll. 464–468. "Because general shear experiment commonly experiences a significant sample thinning during deformation (see Table S1), the shear strain was calculated from the continuous monitoring of two displacement transducers (LVDT) as the sum of individual increments of shear displacement divided by the instantaneous shear zone thickness[32]. As a result, shear strain of 7–10 can be generated by lateral displacements of 4-5 mm.

3. Relating to the comments #1 by Reviewer #3 and the reply to it, finding the evidence of dissolution and precipitation in the recovered sample doesn't mean that the mechanism of the weakening. It could be still possible that the overall strength of the sample is controlled by the dislocation creep of quartz, because the quartz flow law shows lower stress than that observed in the experiments.

- ⇨ We disagree with the reviewer's comment. The mechanisms driving weakening are discussed in details in the manuscript, with multiple lines of evidence arguing against the activation of dislocation creep during strain localization.
  1) TEM images do not show any evidence of intracrystalline deformation in the fine-grained material newly formed in the high-strain shear zones, including quartz/coesite (silica polymorphs are not differentiated in this study).
  2) Neither quartz/coesite nor plagioclase forms a load-bearing framework in the shear zones. The proportion of quartz/coesite in the high-strain domains was estimated to be < 5-10 % using the image processing software ImageJ. Quartz/Coesite occurs as isolated grains in the shear zones. This is confirmed by the TEM images (see new Fig. 8c). Moreover, no strength contrast is observed among the sheared newly-formed minerals – indicating that strain is unlikely accommodated within the grains since they are known to bear different mechanical strengths (even in the diffusion creep regime).
  3) Sheared newly-formed minerals are chemically heterogeneous from core to rim, some with a zoning testifying to oriented growth in the extension direction.

Nevertheless, the revised version of the manuscript now includes new EBSD analyses on a high-strain experiment. **The result and discussion sections have been thoroughly and extensively rewritten.** A large area of the main shear zone was successfully mapped (indexation rates ~80% of the surface area) despite the very fine-grained size of the mineral products. Unfortunately most quartz/coesite and plagioclase grains were too small to be indexed. Therefore, only data for Cpx from Px-rich domains, and zoisite and kyanite from Pl-rich domains are shown and discussed in the manuscript. Two figures are now added to the revised manuscript (new Fig. 6 and Fig. 7).

- ⇨ The EBSD data for zoisite and kyanite are consistent with the TEM observations. Both minerals lack of plastic deformation. They show a weak to moderate CPO (J-index < 3.8), but a strong SPO. Primary grains of CPX display evidence of subgrain boundaries (SGB) testifying to plastic deformation. Secondary Cpx also show some intragranular boundaries but the CPO remains extremely weak relative to the amount of strain – especially for the grains < 2 μm which represent > 60% of the surface area of the map. It is also worth noting that secondary cpx grains do not show any crystallographic relationships with the adjacent primary grains. Similar to zoisite and kyanite, secondary cpx exhibit a strong SPO with the long axis either oriented in the direction of minimum stress or the direction of shear (along C' shear bands). We also note that the CPO and SPO appear to be uncorrelated (the CPO pattern is not affected by the SPO rotation). Altogether, these findings are best explained by mineral reactions, heterogeneous nucleation processes, stress-controlled grain growth and rigid body rotation. The coarsest grains of secondary CPX may be affected by plastic deformation or brittle deformation (to explain the formation of intragranular boundaries) but it is clear that these mechanisms are not dominant in the shear zones. There is no evidence of new grains formed by dynamic recrystallization.

Result section: ll. 165–203.
"Electron backscatter diffraction (EBSD) analyses show that Cpx_1 porphyroclasts in high-strain shear zones have a high aspect ratio (root mean square (RMS) = 2.2), with the long axis generally oriented subparallel to the foliation plane (Fig. 6a). They also display intragranular misorientations, giving rise to subgrain boundaries (SGB) – i.e., pixel-to-pixel misorientations between 2° and 10° – and testifying to plastic deformation (Fig. 6b). Pole figures of Cpx_1 indicate a strong crystallographic preferred orientation (CPO) with [001] and [100] axes subparallel and subperpendicular to the foliation plane, respectively (Fig. 6b,c). In contrast, the reaction products have a very small

grain size (RMS equivalent diameter < 1.2 μm) and do not show any evidence of intragranular boundaries, except for Cpx_2 (grains < 5 μm in diameter), in which the distribution of SGB vorticity axis is statistically consistent with their CPO and the dominance of {110}<001> dislocation slip-systems[42] (Fig. 6d). Although kyanite and zoisite do not show any evidence of crystal plasticity, they display a weak to moderate CPO (M-index = 0.1, J-index < 3.8) with a common pattern with that of Cpx_2: the shortest principal crystallographic axis (i.e., [001] for Ky; [010] for Zo) and the longest one (i.e., [100] for both Ky and Zo) are oriented respectively subparallel and subperpendicular (to form a weak girdle) to the lineation (Fig. 6c).

Cpx_2 grains exhibit a moderate aspect ratio (RMS = 1.7) and a distinct shape preferred orientation (SPO), the long axes of which cluster around the minimum stress σ_3 (Fig. 6d). The SPO strengthens with increasing grain size – as does the CPO – although the latter remains weak overall with J-index lower than 2 (Fig. 7a). Grains below 2 μm in diameter, which represent more than 60% of the surface area indexed in the high strain shear zone, also have a long axis distribution primarily oriented around the direction of σ_3. In contrast, coarser grains (2–5 μm) have a higher aspect ratio (RMS = 2.0) and a long axis distribution strongly aligned in the stretching direction. Remarkably, the CPO pattern remains weak (M-index ≤ 0.06; J-index < 2) and unchanged despite the significant change of SPO for the coarser grains (Fig. 7a).

A close-up view of a representative area of the high-strain zone (Fig. 7b,c) reveals that Cpx_2 grains adjacent to a Cpx_1 porphyroclast (blue area in Fig. 7d) lack of CPO, despite clear evidence of crystal shearing at the porphyroclast rim (i.e., lattice transposition into the shear plane; Fig. 7b). The CPO of Cpx_2 grains in the sheared matrix strengthens with increasing strain in the stretching direction (highlighted in Fig. 7d at the favor of a C'-type shear band; orange and blue areas). However, the intensity remains very weak (M-index < 0.1, J-index < 3.4) regarding the amount of local shear strain ($\gamma > 5$ based on the angle between the foliation and shear plane[43]. For such a finite strain, a minimum strength of M = 0.3 and J = 10 would be indeed expected[44].

At the nm-scale, transmission electron microscopy (TEM) images indicate that reaction products lack of intracrystalline deformation, in contrast to primary phases (Fig. 8a,b). A compositional change is observed between defect-free Cpx_2 grains and a plastically deformed Cpx_1 grain. The sharp and irregular interface between them represents the reaction front. The Na enrichment in the new Cpx_2 compared to the partly dissolved Cpx_1 is consistent with the jadeite substitution associated with mineral reequilibration at high-pressure conditions. All reaction products in both Pl_1-rich and Px_1-rich domains display an elongated euhedral to subhedral shape preferentially aligned in the foliation plane but the crystallographic fabric remains weak (Fig. 8b,c), as observed at larger scale. Reaction products in Pl_1-rich domains show an intense phase mixing without any noticeable strength difference, except where Grt forms larger grains (10–30 μm) deflecting the main foliation (Fig. 4d)."

Discussion section: ll. 269–291.
"Available flow laws and deformation mechanism maps[8,9] predict that dislocation creep should dominate over diffusion creep at our experimental conditions. However, this result is inconsistent with the lack of plastic deformation for most of reaction products (zoisite, kyanite) and the weak CPOs relative to the amount of shear strain (Fig. 6, Fig. 8). Moreover, neither quartz/coesite nor plagioclase constitutes the load-bearing framework of shear zones (Fig. 8c). Instead, sheared reaction products show i) an intense phase mixing, ii) a very small grain size (≤ 5 μm), iii) local variations in chemical composition, iv) nano- to micro-pores along grain boundaries and v) no crystallographic relationships with the primary grains (Fig. 4d–f, Fig. 5b,c). Altogether, these findings are best explained by grain-size-sensitive (GSS) creep combined with fluid-assisted mineral reactions and heterogeneous nucleation processes[22,32,33]. Dominant dissolution–precipitation creep (DPC) associated with oriented growth and rigid body rotation here accounts for i) a strong stress-controlled SPO of the sheared reaction products, ii) no correlation between SPO and CPO for grains below 2 μm in size (which account for > 60% of the shear zone), and iii) an increase of aspect ratio with increasing grain size. The lack of dynamically recrystallized grains around the primary porphyroclasts further indicates that intracrystalline plastic deformation plays a negligible role in strain accommodation, particularly during the stage of drastic weakening. Our experiments therefore corroborate the growing body of field observations indicating that crustal shear zones are dominantly driven by DPC rather than dislocation creep[47–49]. Because intergranular fluid-assisted mass transfer is orders of magnitude faster than solid-state diffusion[50], DPC can explain strain accommodation at relatively high strain rates and low magnitude of differential stress, regardless of the mineral plastic strength[17,34]. Furthermore, the formation of quadruple junctions is inferred to be diagnostic of grain boundary sliding (GBS), which is an essential component of GSS creep, including DPC[51] (Fig. 8). Nucleation of fine-grained polymineralic assemblages and GBS enhance porosity and phase mixing, impede grain growth by grain boundary pinning and, therefore, are crucial for the long-term development of shear zones that deform by GSS creep[27,28,52]."

For example, Getsinger and Hirth (2014, Geology) found that the rheology of the hydrating basalt to amphibolite is controlled by not reaction-induced process but the diffusion creep of plagioclase. I would personally like to support the author's hypothesis, but the evidence is insufficient.

⇨ Our observations and interpretations are consistent with the fossil rock record (as noted in the discussion section). They are also compatible with the study of Getsinger and Hirth (2014, Geology) mentioned by the reviewer. In their study, they indicate that the rheology of hydrated basalt is controlled by reaction-induced processes. Their conclusion actually states that "amphibole-forming reactions may be an

important weakening mechanism at temperatures and pressures appropriate for the lower continental crust".

I do think that we should at least try to obtain additional strain rate dependence of mechanical data to prove that the sample is not deformed by dislocation creep.

- ⇨ Our samples are characterized by a continuous weakening. Unfortunately, the lack of steady state during strain localization excludes the calculation of a stress exponent (n) via strain-rate stepping experiments.

Other comments:

Fig. 1: Translucent lines/squares are better than stars for quartz and feldspar strengths.

- ⇨ OK. Done.

Fig. 1: add a scale in Fig 1b.

- ⇨ OK. Done.

L36-37 and L392-393: It's not exactly correct. The downdip limit of the seismogenic zone is depending of the subduction zone. In some cold subduction zones, plate boundary seismogenic zone extends to a middle of the mantle wedge (e.g., Kita et al., 2010, Tectonophysics). In hot subduction zones, seismogenic zone is constrained by the mantle wedge (see Oleskevich et al., 1999, JGR; Obara and Kato, 2916, Science).

- ⇨ Agreed. The brittle/ductile transition in subduction zones can be controlled by many factors (e.g., temperature, fluid pressure, lithology, reactivity, subducting rate). For a sake of simplicity, we adopt a commonly used approximations stating that the downdip limit of the seismogenic zone occurs around 350°C (e.g., Oncken et al., 2021). This isotherm is also consistent with the brittle/ductile transition predicted by the quartz flow laws used in many thermo-mechanical models – including the I2ELVIS code developed by T. Gerya (e.g., Menant et al., 2020).