Review Report

Wang et al. - The Importance of Anisotropic Viscosity in Numerical Models for Olivine Textures in Shear and Subduction Deformations, TEKTONIKA, 2024.

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1st Round of Revisions

Decision Letter

Dear Yijun Wang,

Thank you for submitting your manuscript to Tektonika. We have now received 2 reviews of your manuscript. I agree with and support the assessment of Associate Editor Maelis Arnould, below; i.e. this is an interesting manuscript and one we think will become a good contribution to Tektonika. From my own personal assessment, I would urge you to consider how someone unfamiliar with the presented models may read this work - I think it would be worthwhile spending some time elaborating on ideas, model set-ups, results, etc... to place everything within a framework that someone new to the field could follow this work fully.

AE Arnould:

Both reviewers acknowledged the suitability of your manuscript for publication in Tektonika, after revision.

Based on both reviews and our own evaluation of your manuscript, we have reached the decision that your manuscript is suitable for publication in Tektonika, after revision. In particular, both reviewers agree to say that this manuscript would benefit from giving more details, especially about the methods and results sections. We therefore encourage you to submit a suitably revised manuscript. Along with your revision, please also include a point-by-point response to all reviewer/editor remarks using the review forms as well as a "tracked-changes" (changes highlighted or otherwise noted) version of the manuscript. We also encourage you to use the Tektonika templates (https://tektonika.online/index.php/home/manuscript_guidelines) when you resubmit your manuscript.

We would like to thank you for the comments. We agree that our description of the methods could be improved, specifically in terms of being clearer about what we do or don’t include. We have rewritten part of the method section to provide a more understandable description of our methods. As for the results section, we worked on making more references to specific parts of the figures, using accurate numbers instead of vague adjectives, and more clearly emphasizing the main point. We hope that the modified manuscript will provide enough information for potential audiences.

Additional editorial comments:

Line 27 "In different regions". Be a bit more precise?

We've changed this phrase to "regions experiencing different deformation".

L.87 "Rayleigh-Taylor" instead of "Raleigh-Taylor"?

We've fixed this typo.

L.125: "and boundary" instead of and-boundary"?

We've fixed this typo.

L.164-166: Equation 1: provide more information on what is \gamma and Q.

We've added definitions to \gamma and Q here (now equation 2).

L.190. Provide more details on the "single particle".

We've added what we use this particle for in the next sentence. In ASPECT, olivine textures are computed on the particles. As a result, our analysis of texture prediction methods all uses information that is stored on particles, such as stress, strain rate, velocity gradients, and temperature.

L.193-195: It would be good to discuss the rates at which the models are deformed. Why this choice? Is it an appropriate choice? How should we compare these deformation rates with the typical ones from the lab? From estimates in real geodynamic settings?

The deformation rate or strain rate will affect the amplitude of viscosity but does not have an impact on the effect of anisotropic viscosity. In our investigation of the effect of anisotropic viscosity, the amount of deformation or strain is more important. We have left this information out of this manuscript so that the main discussion is focused on strain rather than strain rate.

L.203 Even if the setting is the same as an existing study, I would suggest to detail a bit more the setup. In particular, what is the resolution? What are the differences btw the oceanic and continental domains?

We've added boundary conditions and labeled the materials in Figure 2a. The material properties are included in Supplementary Information. We use adaptive mesh refinement in the subduction model, the maximum cell size being 5*5 km and the minimum cell size being 0.625*0.625 km. This information is added to the description.

L.204: "in particular…" There is a missing word. 'locations' maybe?

We've fixed this sentence.

L.220-222: Why this choice of rheology and choice of rheological parameters shown in supplementary? How would the choice of rheology affect the stress patterns later *used by the authors to estimate AV? This needs a bit more discussion I think (at L. 431-434 I guess).*

We've added to the method section that the reason we use composite rheology in the subduction model is that both mechanisms are present in subduction conditions. We include a brief description of the rheology in the main text and the effect of this rheology on our results is discussed in the discussion section. In the discussion section we also added that if other deformation mechanisms that do not generate LPO are used or implemented, the effect of LPO-induced AV could be reduced.

L.225: Why 75? And why only 2 particles are shown then? What about the others?

We think 75 particles are enough to identify some regional patterns in the key regions (below the slab and in the mantle wedge above the slab) while keeping the analysis fast and easy to present in the manuscript. We've added that we chose 2 particles that are representative of the key regions (above and below the slab) and have enough deformation that the anisotropy is strong enough for analysis.

L.235-236: "while the MDM+AV…AV" Explain this choice to non-expert readers.

We've rewritten the description of our method, especially that we ran geodynamic models using ASPECT, and all texture computations (D-Rex, MDM, and MDM+AV) are post-processors that do not feedback to the geodynamic model by changing the deformations.

L.291-295: "effective viscosity…". Show this on a figure. As pointed out by a reviewer, this example would help non-experts to better understand the role of AV in a simple setting.

We added a figure of AV/IV vs. accumulated strain to Figure 3c for the shear box model. In this figure, we plot the ratio of the effective viscosity calculated with the effect of AV or with an isotropic texture, and as the particle is being deformed, the AV/IV ratio drops to about 0.6.

Figure 4: Why not adding a figure with the ratio eta_diff /eta_disl to show where both mechanisms dominate?

We did not include such a figure to keep the manuscript concise. In the discussion section, we mentioned that on both particles examined, deformation by dislocation creep dominates their evolution, which agrees with our expectation. We've also added in the discussion section that if diffusion creep or other mechanisms dominate, the effect of AV could be reduced.

Line 488-489: "where deformation … under various circumstances." This part of the sentence is rather vague. Consider rephrasing/developing it a bit?

We've rephrased this sentence to emphasize that the impact of AV is greatest in the upper mantle where dislocation creep dominates.

Comments by Reviewer A (Lucan Mameri) and Author response

Major concerns:

1) *The work lacks references. It primarily relies on a few citations from co-authors. It is essential for the work that the authors strengthen their findings by referencing relevant external research, besides, it provides context, validates results, increases potential citations, and acknowledges other's works.*

We agree that we did not provide enough references to related works and we have made some adjustments throughout the manuscript to reflect this.

In the introduction and discussion of our methods, we added references to previous implementations of Hill's parameters to represent anisotropic viscosity, such as Signorelli et al. (2021), Mameri et al. (2019), etc. In the discussion of our results, we added references to other studies that agree with our results, such as Tommasi & Vauchez (2001), Knoll (2009), etc.

2) *To significantly enhance the work's impact and relevance, the authors should consider comparing their findings with VPSC models. There's no need to run these models; they could examine how their results relate to those in Hansen et al. 2016, who conducted a comparative analysis between D-rex and VPSC, explore their work in the context of recent studies by Mameri et al. (2019, 2023), which utilized VPSC to model viscous anisotropy at different scales and also used Signorelli et al. 2021 implementation, or Lev and Hager 2011, which, using the director method as the present studied used, show how viscous anisotropy induces temperature instabilities in a subduction zone.*

Thanks for the advice. In the introduction, we've added references to the above-mentioned VPSC models and that our models are different in that AV is calculated from an evolving LPO. In the discussion section, we've added that our results agree with Mameri et al. (2019, 2023).

3) *The author mentions effective viscosity within their shear box models, yet the corresponding results are absent from figures. In this context, the simplicity of the model geometry is an advantage and is likely to offer a clearer connection to experimental shear. I strongly recommend incorporating a figure that demonstrates the evolution of effective viscosity on the shear box, which would not only enhance the research's logical structure but also elevate its overall impact.*

We agree that the shear box model discussion lacks support without showing the results and we have added a plot of AV/IV ratio plotted against the accumulated strain to

Figure 3c and referred to this figure when we mentioned the drop in effective viscosity in the text.

General comments:

Abstrat

L.22 – consider replace no-AV for 'isotropic'

We replaced 'no-AV' with 'isotropic' as suggested.

L.26 – As I will point out below, the information regarding the 40% drop in viscosity in not shown

We added a figure of AV/IV vs. accumulated strain to Figure 3c. In this figure, we plot the ratio of the effective viscosity calculated with the effect of AV or with an isotropic texture, and as the particle is being deformed, the AV/IV ratio drops to about 0.6.

Introduction

L.34-36 – This is rather inaccurate. It is certain that mantle properties significantly affect deformation rates, but the nature of the feedback between physical-chemical processes between crust and mantle remains unclear, notably when times scales are taken into account. There are studies that demonstrate that deformation in the crust can control/change rates of deformation in the mantle. For example, crustal thickening may result in reduced thickness of the strong mantle below resulting in localized thinning of the lithospheric mantle (Dunbar & Sawyer, 1988), reduction of the friction coefficient in the crust can lead to higher strain localization in the mantle (Mameri et al. 2021), strain concentration in the tip of frictional faults might increase strain rates into the ductile layer below (Ellis and Stockhert, 2004), which can transfer down to the mantle, notably in regions with a strong lower crust.

This is a good point. In this manuscript, we mainly worry about how mantle properties, especially AV, can affect deformation in the mantle. To be more accurate, we've changed this to "exert major control over".

L.43-44 – In the given context, "Dislocation glide" is a more appropriate term than "Dislocation creep.", since strong crystal preferred orientation, as in olivine aggregate, actually rise from glide in a limited number of slip systems, while dislocation creep encompasses a series of mechanism that primary accommodate strain. In addition, *nucleation during dynamic recrystallization in fact tend to disperse the olivine CPO, as supported for instance by the slow development of olivine CPO in experiments at higher strains, and the inverse relationship between recrystallized volume and CPO strength (Lee et al. 2002; Falus et al. 2011; Hansen et al. 2014; Lopez-Sanchez et al. 2021).*

We agree. We've changed 'dislocation creep' to 'dislocation glide' and instead of 'lead to the development of LPO' we used 'modify'. We've also added references in this comment.

L.47-49 – It is not clear what the authors mean by 'internal' crystallographic properties. Please clarify.

We initially used 'internal' to distinguish the intrinsic seismic anisotropy caused by LPO from extrinsic seismic anisotropy caused by mineral layering. We changed 'internal crystallographic properties' to 'LPO'.

L.77-79 – The sentence in the introduction states that viscous anisotropy significantly affects the 'resulting texture'. However, they did not cite any article to support the claim. In addition, this is precisely the scientific question being investigated in the present study. So please, if there are already studies that show the impact of VA in texture development, as mentioned, cite them in this paragraph so the reader can evaluate the previous work and open questions that motivate the present study. This seem to be covered in the paragraph that follows (the Blackman citation?). So, to me, both paragraphs need some re-ordering/writing for better connection, or merge them in one single paragraph.

This is a good point. The feedback that is mentioned here is that viscous anisotropy changes deformation rates (Hansen et al. 2016a) and the deformation rates, which are different from those under isotropic conditions, will lead to a different texture. We found that the original sentence mentions multiple different processes that are difficult to discuss in one sentence. We've separated the first sentences into three parts: a) olivine exhibits viscous anisotropy; b) viscous anisotropy can change deformation rates and the deformation rates which are different from those under isotropic conditions, will lead to a different texture (Hansen et al. 2016a); c) viscous anisotropy has an influence on mantle flow dynamics. We've rearranged the following two paragraphs to support the three sentences above and to address this comment and the comment below.

L.85-89 – Previous numerical simulations are cited to show some of the impact of olivine viscous anisotropic. However, there is no mention to the new approach to parametrize viscous anisotropy by Signorelli et al., (2021), which was used in the *present work, or to the recently numerical studies on viscous anisotropy of olivine at rock-scales (Mameri et al. 2019) and fossil mantle shear zones (Mameri et al. 2021, 23), this latter also used the Signorelli's approach. Citing this article is important for two reasons: it provides recent contributions to the topic of olivine's viscous anisotropy, which is a relatively understudied area, and it also provide context to the parametrized approach to compute viscous anisotropy developed used in the present study.*

We've added references to the method (Signorelli et al. 2021), examples of implementations of viscous anisotropy as comparisons to rock experiments (Mameri et al. 2019) and to study fossil mantle shear zones (Mameri et al. 2021, 23).

Methods

L.111-112 - Could you cite the previous articles which explored the texture evolution cited?

We've added citations for previous studies on texture evolution using D-Rex, VPSC and MDM here (Boneh et al., 2015; Boneh & Skemer, 2014; Hansen et al., 2016a; Hansen et al., 2016b; Mameri et al., 2019).

L.111 – It would be instructive if the author reminded the reader of what the MDM acronym stands for, perhaps in the first paragraph of the method section. In its current form, the reader must revisit the introduction to find this information, even though the acronym is actually more relevant in the method section.

We've added the full name of MDM (modified director method) to the first mention of MDM in the method section.

L.112 – The AV was added to the second method, giving rise to a third method: a modified MDM method. To me, the current sentence form 'inclusion of AV in our third method' suggest a fourth method.

We've rephrased this sentence as "Here we use D-REX and MDM, as well as the extension of MDM, where the texture evolution model is coupled with a micromechanical model to include the role of anisotropic viscosity (AV) on LPO development" to avoid confusion.

L.146-148 –While temperature can influence material properties and hence deformation behavior, it is not the primary factor determining the director's alignment. I would suppress the reference to the temperature dependent rheology.

This sentence is included here to provide a full description of the development of the MDM method. We agree that temperature is not the main focus of the MDM method and in the latter part of rheology in our models, there isn't a good place to fit in this detail. We removed this sentence.

L.155-156 – The utilization of the same set of critical resolved shear stresses (CRSS) is consistent between MDM and MDM-AV. However, it remains unclear if the CRSS in D-Rex aligns with those. The author mentions following the CRSS values by Hansen et al. (2016) without explaining its meaning. It is imperative to clearly state which slip systems are considered the weakest and the hardest. This information holds significance as it indicates that the lattice-scale parametrization across all models conforms to the expected relative strength of slip systems during high temperature deformation, where [100] glide is the most favorable direction. It's worth noting that in subduction zones this is not so straightforward, notably due to the influence of water (Jung et al. 2017).

To clarify, our MDM does not use CRSS in its texture prediction and D-Rex in ASPECT uses the criteria by Karato et al. (2008) to determine the olivine fabric type based on water content and stress state, and the values of RRSS (reference resolved shear stresses) come from Table 1 in Kaminski (2002) and Table 1 in Kaminski et al. (2004). Only MDM+AV uses CRSS. We've added the above information in the method section of the description for D-Rex, MDM, and MDM+AV respectively.

L.152 – Why is the 'MDM' acronym mentioned at the end of the sentence?

We removed the MDM acronym which was a typo.

L.178-179 – The authors wrote '….while the effect of AV on the velocity gradient tensor does not feedback to the geodynamic model evolution'; Does it mean that the anisotropic viscosity tensor does not evolve? In the approach by Signorelli et al. 2021 this is the case; the Hill parameters remains constant. Is this the case the present work? -In this regard, I'm have some difficulty comprehending the approach, because anisotropic viscosity tensor affects the texture, but the anisotropic viscosity tensor (the hill parameters) itself depends on the texture. Are the hill parameters updated interactively to account for this mutual influence? Could you please further clarify?

We agree that the description of our method MDM+AV lacks important details that could lead to misunderstanding. We have made modifications to the method section to improve clarity, including adding an equation (equation 1=. In particular, what we want to say here is that the MDM+AV is applied as a post-processing step to recalculate textures and strain rate based on initial textures and deformation (stress, velocity gradient, and T) computed in the geodynamic models (ran with ASPECT, using D-Rex). We've changed the description of our method throughout the main text to emphasize that the texture computations (D-Rex, MDM and MDM+AV) are post-processors that do not change the deformation computed by the geodynamic model using ASPECT.

L.192 – Please add the 'constant' before 'velocity.'

We added 'constant'.

L. 192 – The mention to the magnitude of the Z-coordinate is unclear. To impose a simple shear geometry, the velocity in the Z-direction should be zero.

The z-coordinate (depth) defines the amplitude of the velocity. We've changed "parallel to the x-direction" to "in the x-direction" to avoid confusion.

L. 203 – It would be preferable, for sake of efficiency of reading and flow, to clearly separated the shear box and subduction model setup in two sections, as in the results section. In the current form, for gathering information on the subduction zone model setup only, the reader needs to read through the section 2.4 (Model Setup) to find information on the subduction zone only from the third paragraph.

Thank you for this suggestion, we have now separated the shear box model settings and subduction model settings into two sections.

L.208 – For the sake of scientific rigor, please add the 'constant' before 'velocity.'

We added 'constant'.

L.211 - 212 – The comparison stablished using 'lower' in the sentence is confusing. In the current form it seems that cohesion is 1x104Pa lower in the crust compared to the surrounding lithosphere. I don't think this is the case.

We changed 'lower' to 'which is lower' in the sentence to avoid confusion.

L.213 - 214 – What are the conditions at the bottom boundary of the box?

We added a more complete description for the boundary conditions of the box: The model employs free-slip boundary conditions for its top, open boundary conditions for the four vertical side surfaces beneath 100 km depth, and no-slip boundary conditions for the bottom. A velocity boundary condition is defined for the lithospheric layer of 100 km.

L.228-229 – Since 'sub-slab' and 'mantle wedge' are first mentioned in the given lines, a brief, concise explanation of their location within the subduction zone (especially for sub-slab) would be much appreciated. You can use a short and simple parenthesis like '(the region right below and above the subducting slab, respectively)'. I consider this important because the article extensively explores particles within these regions throughout the results.

We've added "the region right behind and in front of the subducting slab, respectively" to the text.

Results

L.265-267 – The results section references Random-score outcomes, but they are absent from Figure 3c. I recommend including this data in Figure 3c to maintain completeness and consistent with Figure 3b. In addition, incorporating the R-score, along with the P- and G-scores, offers a more comprehensive and clearer representation (than Fig. 3b) of the modeled texture changes as a function of strain This information potentially holds key value for interpreting texture resetting and cycling on the field.

We think that since pointiness + girdleness + randomness score = 1, having the pointiness and girdleness score already includes the necessary information provided by this set of scores and the mentioned observation. Further, information about the randomness score is included in part b of the same figure. Sub-figure b as a tertiary diagram works better to provide information on the co-development of the pointiness, girdle-ness, and randomness scores. Instead of adding a figure, we've rephased this sentence as "The girdle-ness score of the textures reaches its peak around an accumulated strain ($\varepsilon_{ii}^{n} = \varepsilon_{ii}^{n-1} + dt\ \dot{\varepsilon}_{ii}$, where n is time step and $\dot{\varepsilon}_{ii}$ is the second invariant of strain rate and starts decreasing (Figure 3c), while the randomness score (R = 1 – P – G) decreases from the initial value of ~1 to less than 0.2 at an accumulated strain of 2 (Figure 3b)". The intent here is to point to figure 3 for guidance to understand the relationship between the P, G and R scores and to look at the R scores from Figure 3b.

L.274 – The word 'pattern' is more appropriate than 'shape' (e.g., L.275, L.277) for referring to crystallographic textures.

We've changed 'shape' to 'pattern' in our text.

L.274 – I would remove 'beyond this point.' This expression does not add to the comprehension and might lead the reader to believe there is a specific position/points *in strain space at which the author refers to, yet the previous sentence vaguely refers to an 'early-stage'. Alternatively, specify what the authors meant by 'early-stage' (e.g., strain of ~1).*

We've removed 'beyond this point' and changed a few ambiguous references to accumulated strain values throughout the results section.

L. 275 - 277 – Even though is true that girdle component exists in MDM-type models, in all models, the P-type pattern still dominates after ~6 strain. Could you rephrase the sentence to reflect this? Something like: "Yet, the point pattern dominates in all models after significant strain, the girdle shape in the textures predicted by MDM and MDM+AV persists until the end of the model….''

We've added one sentence to emphasize that the point pattern still dominates.

L. 292-295 – Results on effective viscosity on shear box are reported but not shown. I suggest adding a simple figure showing the evolution of effective viscosity as a function of strain, similar to Fig. 6a, b (if the author prefer, they could add in the supplementary material, yet I think in the main text would be better). I consider this would greatly enhance the potential of the work to be cited and hence the impact of the work.

We agree and thank you for the suggestion. We've added a panel to Fig. 3c with AV/IV plotted against accumulated strain.

L. 297. The first paragraph of the subduction section doesn't mention the D-rex model, which corresponds to the models used in Figure 4. Please briefly mention the use of the D-rex texture model, e.g., "Figure 4 illustrates a sequence of snapshots at ASPECT using the D-rex texture model captured at 10 My."

Figure 4 is snapshots from the geodynamic model from ASPECT. Although D-Rex is implemented into ASPECT for texture prediction, it does not feed back to the geodynamic model to modify the deformation, and specifically in this figure, there is nothing related to D-Rex or olivine texture. We've added in the section 2.1 for D-Rex that the textures predicted by D-Rex do not feed back into the deformation of the geodynamic model. Here, instead of mentioning D-Rex, we describe figure 4 as "of the subduction zone model from ASPECT".

L. 308 – Using 'beneath' might imply that the particle is situated below the lithosphere, in the upper mantle. Please, consider rephrasing to something like '…is located 100 km deep within the lithosphere.'

The particles are indeed located in the upper mantle below the lithosphere, as shown in Figure 4 by the pink (mantle wedge particle) and (sub-slab particle) blue dots.

L.313-315 – Similar to Figure 3a, please indicate in the text, or the figure caption, that the pole figure in Figure 5a shows only the [100] axis of the pole figure.

We've added this information to the figure caption of Figures 3, 5 and 7.

L.315-318 – Please clearly indicate to the reader which of the plots in Figure 5 the reader should investigate. The text vaguely refers to Fig.5 in line 315, and do not refer the reader to any figure in the description ending in line 318.

We added a reference to Figure 5a here.

L. 341 – Why mentioned the method section in this line? It appears irrelevant.

We've removed this mention from the method section here.

L. 342-344 – It would be very helpful to refer to the accumulated strain at which the observed changes occurred in Figure 6. This will eliminate any ambiguity and provide clarity regarding the specific points of change within the figure.

We've added the accumulated strain to the observations referred to in this sentence.

L. 348-349 – Similarly to the comment above, in order to guide the reader, please refer to the accumulated strain at which the mentioned changes occurred in Figure 6, for instance, at around 0.9, the drop in effective viscosity occurs. Otherwise, the reader kind of have to guess.

We've noted the exact values of the accumulated strain.

L. 350-352 – Similarly to the comment above, at which point, in strain space, can the misalignment and weakening be observed in Figure 6? The paragraph needs better references to the strain-space to guide the reader, especially because the description refers to data in both Figure 5 and Figure 6.

We've noted the exact values of the accumulated strain.

L.353 – The statement '...is not very strong' lacks the necessary precision. Perhaps the authors intended to convey 'moderate strength' (which is not that precise either)? Consider specify the mindex numbers.

We've changed 'not very strong' to 'has moderate strength' and referred to M-index values.

L.370 – It doesn't appear to me that sigma 1 is perpendicular to a-axis maxima at a strain of ~6 in Fig 7a. For a perpendicular orientation of sigma 1 to the axis, the dot black dot (at 1.46 strain) should be in the center of the principal stress circle, right? Sigma 3 is kind of at high angle (roughly ~80-60°), sigma 2 is perpendicular to a-axis maxima, while sigma 1 seems to be at low-to moderate obliquity angle (~30-10°) to a-axis maxima.

-Could the observed hardening in AV be attributed to the low angle of the principal stress sigma 1 in relation to the a-axis maximum? Sigma 1 is closely aligned with or perpendicular to the a-axis is an unfavorable loading geometry, see Figure 2b of Signorelli et al. (2021).

The mention of "1.46 strain" in this comment seems to refer to Figure 5a. To avoid confusion, we added a reference to Figure 7a in this sentence. We think that sigma1 and sigma3 are becoming more and more perpendicular to the point maximum in the texture around the end of the model in Figure 7a, which is in agreement with Signorelli et al. (2021). In the case of the sub-slab particle, strain is lower, and the girdle-like texture might be a cause of the weaker rheology.

Discussion

L.376 – I prefer 'models' instead of 'method', as it agrees with the terminology used earlier in the text.

We've changed 'methods' to 'models'.

L.395-399 – As previously mentioned in the results section, the authors did not provide data on effective viscosity for the simple shear box (they only reported in written form). The data on effective viscosity is valuable information, which, when compared to previous findings, validates the authors' implementation and could significantly increase potential citations for this article.

Please provide the data.

We've added a panel showing AV/IV plotted against the accumulated strain for the shear box model in Figure 3c to make the discussion section supported.

L. 402 – Which 'dynamic model' do the authors refer to? It's not clear from the text what they mean. Please clarify.

As addressed in comments to L. 178-179, this dynamic model refers to the geodynamic model we ran with ASPECT. D-Rex, implemented within ASPECT to compute the olivine texture on the particle, does not feed back to the model the effect of texture on deformation. Our other post-processing methods MDM and MDM+AV also do not change the deformation paths from the geodynamic model. We've added this information that all texture computation methods we analyze in this study are post-processes that do not change the geodynamic model and implement the feedback effect of AV.

L.403-406 – The statement on '…more anisotropy and weakening are expected…' might be true for the case of a simple shear box, but as demonstrated by the authors, it might not be that straightforward for more complex geometries/kinematics. To accommodate this consideration, consider adding 'in the simple shear box' at the end of the sentence, such as '…if we implement AV with D-Rex textures (as in ASPECT) in the shear box model.'

We've added a reference to the shear box model at the end of this sentence.

L. 417 – Why is expected that pre-existing texture result in more deformation? It's not necessarily the case. If the preexisting texture is poorly oriented to the principal stress direction, less deformation should occur.

This is a good point. We've changed this sentence to say that preexisting texture will lead to a stronger anisotropy initially and the resulting difference between having anisotropy or not is larger (Hansen et al. 2016b; Mameri et al., 2023), not more deformation.

L. 417 – Could you briefly elaborate why a trench-retreating subduction would result in more deformation? As also for the comment above, can you support the claim referencing to relevant articles?

We've separated this sentence into two parts and expanded it to make it clearer. In general trench retreat induces more lateral slab movement, resulting in more rigorous mantle flow (Di Giuseppe et al., 2008; Schellart and Moresi, 2013).

L.423 – For sake of clarity, please add something like 'using the same model' at the end of the sentence, such as '…is around twice as large using the same model.'

We've added 'using the same model' to the end of the sentence.

L.423-426 – The sentence is somewhat confusing. It begins by drawing a comparison between MDM-type models and then use a contrasting transitional world (‘nevertheless’) to describe a feature in D-rex, but to argue for another feature in MDM+AV. I suggest some re-rewriting.

We’ve rewritten this sentence as suggested.

L.432 – I understood that the ratio ηDisl/ηDiff lower than one (1) indicate that the viscosity of dislocation creep is lower than that of diffusion creep. However, momentarily crossed my mind, wrongly, that the ratio was an 'activation ratio', implying that diffusion is more activated than dislocation, contrary to the text. The use of the viscosity symbol η might be somewhat subtle in the ratio. To mitigate a potential confusion, I suggest a further clarification of this term in the text.

In addition, for readers primarily interested in the dynamics of subduction zones rather than the underlying mechanisms, there's a possibility they may have forgotten or are not familiarized with the meaning of η.

We’ve added an explanation of viscosity from dislocation creep and diffusion creep at the end of this sentence.

L. 437-442 – The weakening when the a-axis bisects sigma 1 is consistent with previous models on viscous anisotropy of olivine (e.g., Tommasi & Vauchez 2001; Knoll, 2009; Mameri et al., 2023), which demonstrated that a high obliquity load to the maximum concentration of fast axes of olivine results in the most significant weakening notably of at 45°. Considering there are very few citations in the discussion (mostly of co-authors), it would be valuable to cite articles that support the authors' findings.

We’ve added references to Tommasi & Vauchez (2001), Knoll (2009) and Mameri et al. (2023) here.

L.457-459 – This observation is also supported by previous studies mentioned above.

We’ve added references to the recommended articles.

L.459 -461 – The present study is NOT the first to implement viscous anisotropy in a numerical code. Maybe the first to implement VA in the ASPECT code. Mameri et al. (2021, 2023), which is also derived from the development by Signorelli et al. (2021), implemented viscous anisotropy in a thermomechanical geodynamic code. Knoll et al. (2009), Tommasi et al. (2009) implemented it in a numerical code, all based on VPSC predictions to study fossil shear zones. Lev et al. 2011 indeed implemented the director *method in the context a subduction zones. The present work did not cite none of this article. Well, there is also the approach by Kirally et al. 2020, co-author in the present study.*

We wrote “the first step” to mean “the first step in this project which provides a foundation for future work”. We’ve rephrased this sentence to reflect this meaning. We’ve added citations to the above-mentioned studies when we are introducing viscous anisotropy, VPSC, and our method in the previous sections.

Conclusion

L.480 – This is in agreement with finding by Mameri et al. 2019, which introduce isotropic deformation mechanism to VPSC The authors say that their results in consistent with previous results, but as mentioned previously, there is clear lack of cited comparisons.

We’ve added citations to Mameri et al. 2019 in the previous discussions but here we think that Mameri et al. 2019 is not strongly related to our point. We have also moved this statement to the discussion section, where we are mainly discussing dislocation creep viscosity vs. diffusion creep viscosity and the possible effect of fully implementing AV into ASPECT with composite rheology.

Figures

Is there any special reason why markers color in Fig. 3c and Fig. 7c is kind of similar, but different from those in Fig.5c?

The choice of color in Fig.5c and Fig.7c is supposed to correspond with the particle color in Fig 4, where the blue particle is located beneath the slab and the pink particle is located in the mantle wedge. We’ve changed the color scheme now so that the shear box model (Fig.3c) has a red theme, the sub-slab particle in the subduction model (Fig.5c) has a blue theme and the mantle wedge particle in the subduction model (Fig.7c) has a pink scheme, while D-Rex, MDM, MDM+AV are colored with different shades of the theme color.

Sigma 2 in figure 5a is kind of hidden. It could be closer to the dot.

We’ve made this change to Figure 5a.

Why strain-space explored Fig. 6a and 6b is so different? It is because the particles experienced less strain. Better (shortly) state this in the results. It is interesting observation in itself.

We’ve added one sentence that describes the different strains and deformation on these two particles from different regions in the beginning of section 3.2

Figure 2a needs some improvements, it seems very rough (similar to the visualization software). The reader should be able to understand the boundary conditions (at least the main aspects) without having to refer to the text. Here are some suggested improvements:

- Indicate that the velocity key refers to the arrows (e.g., velocity vectors or velocity arrows), because dark color in the code as similar, and can vary depend of the screen used to visualize.

- Add the size of the box next to the respective axes, e.g., X (2500 km), Y (2000 km), Z (800 km).

- Indicate which side of the box is being pushed, and which one fixed;

-I’m confused with the ‘weak domain’:

> *1) In the Fig.2a, there is one ‘weak crust’, the text talk about two weak zones. Explicitly point it out (with an arrow?) the two weak zones mentioned in the text.*
>
> *2) I presume the red then is stronger than the light orange. Please indicate in the figure the stronger domain*
>
> *3) The ‘weak crust’ in Fig.2b, actually is colored coded in Fig2b with higher viscosity (yellow) than the surrounding red crust, therefore stronger.*
>
> *4) I prefer using ‘weaker’ and ‘stronger’ rather than absolute terms (e.g., weak)*

We’ve made major edits to Figure 2a to address these comments. To avoid ambiguity, we changed the name of the material to “subducting crust”, “weaker crust” and “fixed crust” respectively and gave them different colors and labels in Figure 2a. The color in Figure 2a is not related to strength while in Figure 2b, the viscosity is shown. Now the “weaker crust” in Figure 2a is colored coded (light brown) and is evident in Figur e2b with a smaller viscosity (more blueish and greenish).

Comments by Reviewer B (Neil Ribe) and Author response

1. Line 65. Please quantify "relatively high computational resources" by giving orders of magnitude of the cost of VPSC and D-Rex relative to MDM (=1).

We've included a reference to Hansen et al. (2021) which includes a detailed comparison of these methods.

2. Lines 68-69 and 141-148 These descriptions of the director method are not sufficiently detailed. How many directors per aggregate: one per crystal, or one for the whole aggregate? And if the latter, what does the director represent?

In the original director method, the director is normal to the slip surface when studying anisotropy created by LPO. To be clearer, here we changed "anisotropy planes" to "slip surfaces". We've added that for each available slip surface in grain, the normal vector to the slip surface is defined as a director.

3. Given the modest differences (factor of ≈ 3) among the CRSSs of the olivine slip systems and the relatively weak textures found in natural samples, it seems difficult to imagine where "orders of magnitude changes in viscosity" are going to come from.

To be more accurate, we changed "orders of magnitude" to "greater than an order of magnitude". This amount of difference has been shown in both laboratory experiments and numerical models since the relationship is not linear.

4. Lines 137-138. If I recall correctly, Kaminski found that values of Mm much greater than 10 were necessary to get a good match to laboratory measurements.

Kaminski and Ribe (2001) suggested Mm = 125 based on comparisons to experiments from Zhang and Karato (1995) with extensive dynamic recrystallization. We mentioned that other experimental and modelling studies suggest that Mm = 10 results in more comparable textures with their outcomes. We've also added that in our models we experimented with different Mm values and found that larger Mm values result in too much grain growth, leading to a texture controlled by a few very large grains, creating unrealistic textures.

5. Lines 159-161. "We first use the micromechanical model ..." is unclear. Also, Hill's coefficients need to be defined for nonexpert readers.

We've rewritten a large part of the method section to improve readability, and also in response to comments from Reviewer 1. Specific to this comment, we note that the

Hill's coefficients are defined below on the next page as in equation (1) there are a lot of terms defined by the following equations. The part that contains the Hill's coefficients is the last one.

6. Line 171. Again, non-expert readers will need more help understanding the fluidity tensor. Explain why it is a 6 × 6 matrix. Is it a true tensor, i.e. an object that transforms in a definite way under coordinate 1 transformations? And if so, with respect to what axes is eqn. (3) expressed?

Yes, the fluidity (the inverse of viscosity) is a full rank-4 tensor that is transformed into a rank-2 tensor. Due to its symmetry, we can reduce it to a 6*6 matrix. We've added this to the description of the equation and details about the symmetry of the tensor are included in the Supplementary Information.

7. Lines 172-176. This is really not clear, especially the bit about "scaling".

We have moved this information to the beginning of section 2.3 in the context of the general description of how MDM+AV works. We added "The subsequent texture evolution is predicted with a velocity gradient $(D_{ij\_anisotropy})$ that we obtain by scaling the velocity gradient that the particle experienced in the geodynamic model (D_{ij_model}) with the ratio between the anisotropic strain rate and the strain rate in the geodynamic model:

$$D_{ij\_anisotropy} = D_{ij\_model} * \frac{\dot{\varepsilon}_{ij\_anisotropy}}{\dot{\varepsilon}_{ij\_model}}, \quad (1)$$"

8. Line 180. What is the "LPO reference frame"?

The details of rotation and reference frame are included in the supplementary information. We've added a reference to the supplementary information here to avoid confusion.

9. Lines 186-187. Unclear.

Since this information is already provided in previous parts of the method section, we dedicated this section only to describe the shear box geodynamic model settings, thus deleting this paragraph (and the unclear sentence mentioned).

10. Line 190. Why is cubical domain needed? Isn't this a 2-D flow?

Yes, this is a valid point. The flow is 2-D, and we could run the texture prediction model in 3-D. But in this study, we allow the textures to develop out of the shear plane (in 3-D) as texture computations are usually in 3-D.

11. Lines 236-240. Unclear.

We've added the equation for calculating effective viscosity to this sentence.

12. Line 265. How is "accumulated strain" defined? Please give the relation between this quantity and the axial ratio of the finite strain ellipse.

We added an equation for the accumulated strain at the end of this sentence.

13. Line 294. Has "effective viscosity" been defined somewhere? If not, please do so.

We added an equation for effective viscosity here.

14. Fig. 5. D-Rex and MDM give hugely different texture predictions at 24 and 40 Ma. Can the authors explain this? Same remark for Fig. 7, where all three methods give very different textures.

This is a good point. The differences between D-Rex and MDM are expected from previous studies. We've added a comparison between the textures from D-Rex and MDM to the paragraph discussing the texture difference between D-Rex, MDM and MDM+AV in the discussion section: "Different from the results from the shear box model, for particles in a subduction model, olivine textures predicted by D-Rex and MDM exhibit differences in the dominant pattern and the mean direction after an accumulated strain of about 1 (Figures 5a, 7a). This could be a result of the emphasis on dynamic recrystallization in D-Rex or differences in parametrization between D-Rex and MDM."

15. Fig. 5 again. The figure provides insufficient insight into the deformation history experienced by the sub-slab particle. I strongly suggest that the authors add a new figure showing trajectories of the two particles (sub-slab and wedge) on a Flinn diagram so that readers can see at a glance the history of finite strain.

The principal stress plots (Figure 5a) provide similar information about extension and compression. We've included that "we follow the convention of positive tensional stress" in the caption of Figure 5 and 7 to guide the reader. For example, in Figure 5a at an accumulated strain of 1.46, sigma1 (positive) is in the x direction, which means that there is large extension in x direction. Similarly, sigma3 (negative) is in the z direction, which means that there is compression in the z direction.

16. Fig. 6. Why are the AV/IV curves so jagged? I can't believe that all these oscillations are physical. The authors need to discuss what features of their numerical method (insufficient resolution?) might lead to unphysical oscillations.

We’ve added the following discussion to the main text: It is important to notice that the AV/IV ratio is not a smooth curve as the accumulated strain increases. This reflects both the uncertainty in finding Hill’s parameters for computing the anisotropic viscosity tensor using the best-fit method and the changing relationship between the principal stress directions and the texture.

17. Lines 367-368. I don’t recall, but make sure AV and IV are clearly defined somewhere. How are these scalar quantities related to the tensorial viscosity?

Yes, AV and IV are defined in the method section and on the previous page. These are the effective viscosities that are calculated from the stress and strain rate.

18. Line 397. How does the present work represent an advance over Kiraly et al. (2020)? This is a question to discuss in the Introduction.

We’ve changed the description of our method in the introduction section to “In this study we apply an extended version of the MDM method, which we call MDM+AV, where the texture evolution model is combined with a micromechanical model to incorporate the effect of AV similar to Király et al. (2020) but using the approach described by (Signorelli et al., 2021) based on Hill’s orthotropic yield criteria to model AV (Hill, 1948).” Compared to Kiraly et al. (2020), our study explores more complex deformation paths tracing particles from geodynamic models of subduction settings and comparing textures predicted using 3 different methods. This project is a also step toward integrating AV in a full-flow model.

19. Line 461. “If we assume ...”. Unclear.

We’ve removed “we assume that” to simply state that if shear direction is the same as the texture alignment.

20. Supp. Info. (SI) Why can you assume monoclinic symmetry for an olivine aggregate?

See below.

21. SI. Why can you assume orthotropic symmetry (please define) in the mean CPO reference frame?

To answer comments 20 and 21 together, an olivine single crystal has an orthorhombic structure, and thus it is expected that the viscosity tensor for a single crystal olivine will have orthotropic symmetry with 9 independent components in the CPO reference frame. In olivine aggregates with multiple single crystals with different orientations, the average viscosity tensor is expected to have a lower symmetry, for example, monoclinic (in macroscopic reference frame). We've added information to SI that reflects this explanation.

22. SI. The notation of the matrix RCPO needs some explanation.

We've added to SI that each component of RCPO represents one component of the eigenvectors associated with the largest eigenvalues for the a, b, and c axes of the olivine textures and R_CPO is the same as the R_ij in R_CPO_K.

23. SI section 3. While I appreciate the authors' attempt to provide a physical explanation, this paragraph seems out of place in the SI. I suggest moving it to the main text.

We think that the observation and discussion in SI section 3 are not directly related to the main objectives of the manuscript but offer a relevant and reasonable explanation if readers are interested. Including this in the main text would require an additional figure and table as supporting material. To keep the manuscript focused and concise, we include this information in SI.

2nd Round of Revisions

Decision Letter

Dear Yijun Wang,

Thank you for submitting your manuscript to Tektonika. We have now received 2 new reviews of your manuscript. Both reviewers agree with your corrections and recommend publication, after some minor corrections, which are detailed below.

We therefore encourage you to make the revisions asked by one of the reviewers. Along with your revision, please also include a point-by-point response to all reviewer/editor remarks using the review forms as well as a "tracked-changes" (changes highlighted or otherwise noted) version of the manuscript.

Comments by Reviewer A (Lucan Mameri) and Author response

The authors have adequately responded to the concerns raised in the previous review, leading to an enhancement in the overall relevance of the figures and text, thereby strengthening the potential impact of the study. By incorporating additional references related to the study of viscous anisotropy in olivine, a niche area of research, the authors have strengthened their findings. While the requested comparison with VPSC models was somewhat timid, it suffices for the purposes of the article. Additionally, the authors have included a figure that addresses the lack of visual support for the magnitude of anisotropy in the shear box. This inclusion ensures proper presentation of key aspects and enhances the potential for citation. Improvements have been made to Figure 2 and its text description, including boundary conditions, which now adequately fulfill its purpose. In overall, I think the author made significant improvements. The present work adds valuable insights to the relatively sparse literature on the viscous anisotropy of olivine, particularly its implications for subduction zones. With my previous major concerns addressed, I now offer some (or more) minor suggestions to further enhance the article.

We would like to express our gratitude for your feedback again. In response to the comments below, we have made changes to the descriptions of the VPSC method, fixed ambiguous descriptions of ratios in percentages, and made adjustments to Figure 6.

INTRODUCTION

L.44-51. This section of the introduction potentially divert attention from the primary focus of the paper by giving rather to much detail information on seismic anisotropy right at the beginning of the article. For example the percentage difference in seismic velocity. Consider reducing. The important here (for me) is to draw attention to relation between LPO and seismic anisotropy, and that it demonstrated the LPO is prevalent in mantle.

We agree that the sentence on seismic velocity does seem out of place in this paragraph and we've removed the sentence.

L.63-64. The cited issue of "higher VPSC anisotropy than experimental anisotropy" arises from traditional VPSC models that solely consider dislocation glide. However, this challenge has been addressed by integrating isotropic deformation mechanisms into VPSC, such as diffusion, through two approaches. Tommasi and Signorelli (2015) explicitly incorporated dynamic recrystallization into VPSC. Mameri et al. (2019) introduce pyramidal slip systems in a manner where they accommodate strain but do not generate rotation of the crystal lattice. Both methods led to a considerable enhancement in aligning the computational results with laboratory findings. Notably, the second approach demonstrated substantially higher computational efficiency compared to the first method.

We've modified this description of VPSC to be "Notably, VPSC requires relatively high computational resources (Hansen et al., 2021) and predicts both higher texture strength and larger anisotropy at high strains compared to experimental results (Hansen et al., 2016b). These challenges have been addressed by incorporating dynamic recrystallization into VPSC (Tommasi and Signorelli 2015) or by introducing pyramidal slip systems to accommodate strain while not generating rotation of the crystal lattice (Mameri et al., 2019)."

L.83-85. Is this sentence introducing the Blackman findings that follow? If so, consider beginning the sentence after that (L.85) with "For instance," for improved clarity.

We've added "For instance" to the beginning of sentence at L.85.

L.91. FYI, Mameri et al. (2019) paper dealt with VPSC models rather than geodynamical models. Yet it served as a crucial step in the implementation of rock-scale VA within the geodynamical models in Mameri et al. 2021 and 2023.

Thanks for mentioning this. We've changed this sentence to "AV related to olivine LPO has also been applied to lithospheric processes in VPSC models and (quasi) instantaneous numerical models based on VPSC to understand how LPO preserved in fossil mantle shear zones contributes to lithospheric fault reactivation (Mameri et al., 2019, 2021, 2023; Signorelli et al., 2021)."

L.91. Mameri et al. 2021 instead of 2020 (the publishing board would probably point that out).

Thanks for pointing this out. We've corrected the publication year.

L95-98. Consider slice it out for clarity. It is current form is confusing.

We've split this sentence into three sentences for clarity: In this study, we apply an extended version of the MDM method, which we call MDM+AV, for olivine texture prediction in a shear box setting and a subduction setting. MDM+AV combines the texture evolution model with a micromechanical model to incorporate the effect of AV. We follow an approach similar to that of Király et al. (2020), but use Hill's orthotropic yield criteria to model AV (Hill, 1948; Signorelli et al., 2021).

L104-107. This sentence, which states the focus of the work, should mention the 'subduction zones' somewhere, which is where VA is being investigated.

We added "in both shear box and subduction settings" to this sentence.

METHOD

L. 128-130. Does it mean the presented models with D-REX models implicitly take into account enstatite and GBM? Please clarify or remove the sentence.

Yes, D-Rex takes into account GBM and it is possible to include both olivine and enstatite. As a reply to L.139 below together, we've added that we use only olivine for the shear box model and both olivine and enstatite for the subduction model.

L. 131-132. The broad explanation of ASPECT in this lines seems out of place. I suggest it should be moved to the first part of the method (around line 116), where ASPECT is first mentioned in the context of the method section.

We've moved the general description of ASPECT to the first paragraph of the method section.

L. 132-133 - I would omit the reference to 'realistic' model setups. Such terminology is subjective and inaccurate, considering that a model inherently represents a simplification of reality.

Great suggestion. We've removed 'realistic' in this sentence.

L. 139. So, revisiting my previous point, the statment in this line means that the volume fraction of pyroxene is not being taken into account. Correct? Please explicitly state because, as you mentioned in lines 128-130, in the original D-REX implementation, this was included.

This sentence appears to be less relevant to the paragraph and leads to confusion. We've removed this sentence and added "While the evolution of volume fractions for olivine and enstatite are both possible in D-Rex, we only use olivine in our prediction of LPO and LPO-induced AV to be consistent with other methods (MDM and MDM+AV)."

L. 154-156. It seems is missing a comma either after '(denoted by a)' or 'evolution model'.

Thanks for pointing out this and we've added a comma after 'evolution model'.

L. 164-166. I found the sentence dificul to follow. What exactly was calibrated toguether…? The model texture or the micromechanical models?

The micromechanical model was calibrated with MDM. To reflect this, we've changed this sentence to "To model texture evolution with the influence of AV, we combine the LPO development provided by the MDM model with the micromechanical model, which has been calibrated with MDM to fit laboratory deformation experiments on olivine polycrystals (Hansen et al., 2016b).".

L. 164. Consider phrasing MDM LPO as "..LPO development provided by the MDM model.." or something similar.
We've changed "MDM LPO" to "LPO development provided by the MDM model".

L. 168. The respective differences in relative strength (RSS) needs to be specified here. I assume it follows those outlined by Hansen et al. (2016), where (010)[100] is considered the easiest slip system, as expected in most upper mantle deformation. However, this may not be the case for subduction zones, notably in the mantle wedge, where water might induce transtion from A- to B-type olivine fabric (Ohuchi et al. 2012). Therefore, the reader needs to be aware of the choice of relative strength of slip systems in order to evaluate the impact of this assumption.
We've mentioned the resolved shear stress for different slip systems in the previous section 2.2 (one paragraph above). Here we've added "as described above for MDM (section 2.2)" to the end of this sentence.

L. 177. Instead of using "mechanical" anisotropy, which encompasses elastic behavior, stick to "viscous" anisotropy, as it precisely reflects the focus of the investigation in the article.
We've changed "mechanical anisotropy" to "viscous anisotropy".

L. 207. Please add 'shear' to 'strain' (e.g., total shear strain), saying something like 'henceforth referred to as strain'.
We've changed "total strain" to "total shear strain" and added "(henceforth referred to as strain)".

L. 229. In its current form, the sentence appears to specify a velocity boundary condition precisely (an only) at a depth of 100 km. I suggest rephrasing it as: "A velocity boundary condition is defined for the lithospheric layer extending up to 100 km."
We've taken the suggestion (with one small change) and changed the sentence to "A velocity boundary condition is defined for the lithospheric layer extending down to 100 km."

L. 241-243. The part that starts the paragraph in the cited lines, "With the rheological behavior," better fits the previous paragraph. However, the part "We place 75..." after "(Figure 2)" introduces a completely new idea, and should be done ones starting the new paragraph.
We agree that the previous paragraph talks about the rheology and the sentence starting with "with the rheological behavior" best belongs to the previous paragraph. We've

moved this sentence to the end of the previous paragraph (and also changed “with” to “for”).

RESULTS

L. 285-287. I agree that the girdle persists in the other methods relative to D-Rex. The magnitude seems rather small (0.23; about one order magnitude higher than D-Rex). Nonetheless, I bet remains (very) important for viscous anisotropy because the girdle in MDM-based models will results in more grains in (un)favorable orientations compared to D-Rex. It would be beneficial to have the authors shortly comments on this there.
We’ve added one sentence here: “This difference could be important for viscous anisotropy because the girdle in MDM-based models will result in more grains in favorable or unfavorable orientations as compared to D-Rex, potentially modifying the deformation rate.”

L. 292-294. Adding the AV component does not significantly change the predicted texture." This is because the texture is already well-oriented for deformation, as I indicated by the reduction in effective viscosity. I presume that texture evolution would be substantially affected by VA if it was unfavorably oriented, as it would necessitate the activation of harder slip systems and other mechanisms.
We’ve changed the sentence to “Adding the AV component does not significantly change the texture predicted by MDM+AV because the texture is already well-oriented for the applied deformation”. We’ve also added one sentence to L.422 in the discussion section in the paragraph discussing the effect of AV in the shear box model: “The texture developed in the shear box model is already well-oriented for the applied deformation, while a strong unfavorably oriented texture as occurred in the subduction model will be more significantly impacted by AV.”

L. 295. Please indicate that in the initial time step mentioned, the models is ISOTROPIC.
We’ve added “where the model is isotropic” after “relative to the initial time step”. We also separated the comparison to Kiraly et al. (2020) into another sentence.

L. 294-296. The ratio between AV/IV goes from 1 to 0.4, this means a reduction ‘by’ 60%, not ‘by’ 40% as mentionend (I think the authors intented to say ‘to’ 40%).
Thanks for bringing this up. We meant a reduction by 60% and we’ve changed “by 40%” to “by 60%” both here and in the remaining text as well.

L. 294-296. Still in the topic above. For a complete description that althous could mention that in fact after reduction by 60% ocurs up to gamma of 2 , then viscosity recovers slighly. The author have a idea why is that?

We've added "up to an accumulated strain of 2" to this sentence.

L. 299. Please add 'perpendicular to the trench' or 'along the X-direction' right after 'cross-section'.

We've added "along the x-direction" after "cross-section". See the comment below for details.

L. 299-302. I suggest break this sentence in smaller chuncks for clarity. As bellow: Figure 4 illustrates a sequence of snapshots of a cross-section perpendicular to the trench in the middle of the subduction zone model. The snapshot were captured at 10-Myr intervals and shows the viscosity (left) and strain rate (right) fields. Velocity vectors (white arrows) are displayed, in which the paths of analyzed particles are highlight as blue and pink arrows.

We've split the sentence into this: "Figure 4 illustrates a sequence of snapshots of a cross-section along the x-direction in the middle of the subduction zone model. The snapshots are captured at 10-Myr intervals and show the viscosity (left) and strain rate (right) fields. Velocity vectors (white arrows) are displayed, in which the paths of analyzed particles are highlighted as blue and pink lines."

L. 330. Please add 'for the time and rheology imposed...' before or after 'the accumulated strain within the mantle wedge can be 4 times larger'. This will ensure the results is restrict to the observed/modelled window, instead of potentially misleading the reader to think it is a general finding.

We've added "for the time and rheology imposed" to the end of the sentence.

L. 332-335. The author describes the overall contrast in MDM-(AV) with D-REX, but there is no mention or description of the fact that around 1 strain, D-REX predicts a significantly different a-axis symmetry. In D-REX, the a-axis points subparallel to the Z-axis of the pole figure, while in MDM, it is roughly parallel to the X-axis. I noticed later in the reading that the authors quickly address this in the discussion. Therefore, please describe the change at 1 strain in the results section, as mentioned above.

Great suggestion. We've added this comparison to the beginning of this paragraph and now the first two sentences become: "For the less deformed sub-slab particle, our results show that around an accumulated strain of 1, D-Rex predicts a significantly different a-axis symmetry, where the a-axis points subparallel to the z-axis, while for MDM and MDM+AV, the texture is roughly parallel to the x-axis (Figure 5a). Apart from this difference, both MDM and MDM+AV predict a similar texture evolution, characterized by a more girdle-like shape of the a-axis distribution compared to D-Rex's prediction, which transitions into a more point-like shape starting from a strain of 0.8 (Figure 5a)."

L. 341. Describing the magnitude of viscous anisotropy in terms of percentage (alone) can be confusing for most people, as strength, stress, or strain rate comparisons are often reported as a "factor" or "order of magnitude" difference. In addition, in line 341 for instance, the authors say that "AV decreases from 100% to about 80% of the IV." However, this implies that the model transitions from isotropic to a viscosity weaker than the isotropic viscosity, which means that the sentence above does not make sense because the models is in fact isotropic in the beginning. To address this without changing figures, why not also include in parentheses the factor by which viscosity is changed?

Yes, that is exactly what we imply that this particle transitions from isotropic to an effective viscosity weaker than the isotropic viscosity. The reason we use percentages in the text is to be consistent with Figure 6 and throughout the text, since some percentages cannot be simplified to be a factor of an integer. We agree that "AV decreases from 100% to about 80% of the IV" can be misleading in some way. To avoid confusion, we've changed this sentence to "AV decreases to about 80% of the IV."

L.350. Can the authors be more specific here: 'The slight misalignment of 1''. In those stress figures, the approxiamte angle between sigma 1 the maximum can be found, right? For exemple, at the begginingg it seem about 45-50 degress, by the end it seems subparallel to the x-direction of the pole figure.

We've changed this sentence to be "The slight misalignment of sigma1 with the direction of the a-axis point maximum (about 10~20°)".

L. 350. Why is there still weakening after an accumulated strain of 1 if Sigma is not oblique to the 'apparent a-axis maxima'? Is this an effect of the girdle around the a-axis, which results in several grains still being well-oriented (oblique) to Sigma 1?

L. 385. Here is a clear example of how confusing it can be to describe the AV/IV ratio using percentages, and the mental gymnastics one must perform to simply understand that the differences are approximately a factor of 2. "a decrease of the AV to 50% of the IV," it means that the AV viscosity is half the magnitude of the IV viscosity. In other words, IV is twice the magnitude of the AV viscosity. Similarly, if AV is about 200% of IV, it means that the AV viscosity is twice the magnitude of the IV viscosity. Or conversely, the IV viscosity is half the magnitude of the AV viscosity. These examples underscores the importance of using more intuitive measurements to convery the relationships between anisotropic and isotropic viscosity.

Thanks for this suggestion. We've rewritten these relationships (50% and 200%) as "AV decreasing to about half of the IV (AV/IV≈50%)" and "reaching up to about twice of IV (AV/IV≈200%)". We've kept the percentages in parentheses to be consistent.

Discussion

L. 390-391. The word 'implication' begs the question of what is being implied. This is not clear in the given sentence. Maybe just remove the word 'implication' (?).

We've removed "implication of" from the sentence.

L.396. Since you were also talking about VPSC in the previous paragraph, specify which methods you are referring to when saying 'all three methods'. I assume you are referring to your models, D-REX, MDM, and MDM-AV."

We've changed "all three methods" to "D-Rex, MDM and MDM+AV".

L. 399. 'in' instead of 'In'

We've fixed this typo.

L. 402-403. The use of "decreasing" and "increasing," which, by the way, is not necessary, to describe the inverse relationship, makes understanding of the sentence/relation somewhat complicated. Why not clarify the meaning in plain language, as follows: "We observe an inverse correlation between the effective viscosity and the pointiness score, in other words, the lower the viscosity, the higher the pointiness score." Something like that.

We've changed this sentence to "In the shear box model, we observe an inverse correlation between the effective viscosity and the pointiness score of the olivine a-axis (Figure 3c); in other words, the lower the viscosity, the higher the pointiness score."

L. 404. The decrease is 'by' about 60%, leading 'to' 40%.

As mentioned in a previous reply, we've fixed this.

L. 406-411. I found the statement is thes lines confusing. Does the sentence starting with 'Using the MDM+AV...' have anything to do with the second period starting at 409? It doesn't seem to be the case, as the cited lines (406-411) first refer to the shear box, then geodynamic models, and then return to the shear box again. In any case, it needs clarification or rewriting."

Yes, the three sentences are related. The first sentence says that the effect of AV is not large since AV cannot change the strain rate in my model presented here MDM+AV is applied as a postprocessor to the geodynamic model. We've changed the second sentence to "If we incorporate MDM+AV into geodynamic modelling tools where AV can modify strain rate, the effect of AV can be amplified." Then the third sentence says that if we implement D-Rex to geodynamic modelling tools, the results could be different from implementing MDM+AV.

L. 418-419. I would stick to the term 'strain' throughout the text, or put 'strain' in parentheses right after 'intensity of the deformation'
We've added "(strain)" after "intensity of deformation".

L.426-439. I wish the author would discuss the differences predicted in CPO by D-REX compared to the other models, particularly in relation to natural expectations in subduction zones. Considering that a B- type fabric, as predicted by D-Rex in the subduction model, requires the [001] direction as the easy glide, while the A-type fabric requires [100], if the texture predicted by D-REX were to feed back viscous anisotropy and in turn the CPO interactively in the model, would it be a better choice than using the other models?
Thanks for bringing this up. At L.495 below, where implementation of AV to ASPECT is discussed, we added this sentence "These differences in texture prediction between MDM-based methods and D-Rex are expected since D-Rex accounts for the effect of water in activating different olivine fabric types, which could play a significant role in subduction systems."

L.448. Sigma 3 goes from negative values to positive values. What does it mean? Does it signify a change in regime? In other words, the reader does not know if the negative value still represents compressive stress, as mentionned, or effectively an extensional regime that develps, or if the negative values simply denotes a lower sigma 3 value than a background stress. Could you please clarify this in the text?
This information is included in the figure caption for Figure 5, where the principal stresses are first shown. We've also added this to the result section where we first talk about principal stresses "A positive value corresponds to extension, while a negative stress is compressive."

L.479-482. Confusing sentence with two central ideas. To finish the discussion in a more impactful way, I suggest breaking the sentence apart.
We've changed the first sentence to "Our study uses an application of MDM+AV to quantify the effect of AV in both simple and complex scenarios. Based on this, we build a solid foundation for incorporating AV into numerical methods where AV associated with existing textures can modify the strain rates that are used to predict new textures."

CONCLUSION

L.493. Well, 'typical' is debatable; 'simple', for sure.

We've changed the phrase to "in both simple shear box and subduction settings".

L.497. the MDM-AV is similar. In which sense? Need clarification.

We've changed this to "The strain evolution of MDM+AV texture is similar to that of MDM texture".

L.496-498. The results in the draft are time/strain evolution, the conclusion described in this part seems static, and do not reflect the time/strain evolution nature of the results.

As mentioned in the previous reply, we've added "the strain evolution of" to the description.

L.498. 'by' 60%, not 40%.

As mentioned in a previous reply, we've fixed this.

Figures

Figure 3. There is a IDM in the Fig., I believe is a typo, should be MDM.

We've fixed this typo.

I propose flipping the figure 6. Put the AV/IV plot below the respective viscosity x strain plot, since they have the same x-axis, the reader could straight compare the viscosity evolution path with strain evolution.

We agree that having the shared x-axis aligned will be easier for the reader to understand the plot and we've rearranged the subplots as suggested and changed the figure caption as needed.

Comments by Reviewer B (Neil Ribe) and Author response

Neil Ribe: Recommendation: Accept publication

The authors have made a serious and successful effort to respond to the comments of both reviewers. I judge that the manuscript can now be published.

Acceptance letter

Dear Yijun Wang, Agnes Király, Clinton P. Conrad, Lars Hansen, Menno Fraters:

Thanks a lot for addressing the reviewer's comments. We have decided to accept your submission for publication. Your manuscript will therefore soon be held by the typesetting/proofreading team. In the meantime, I noticed a few minor points that can already be addressed by you, to save time during the proofreading phase. They are listed below:

- one comment (about line 350) has not been addressed by the authors.

- there are still comments from Clint Conrad which appear in the manuscript. I would suggest the authors to delete them.

- the notation of \sigma_x is not consistent throughout the manuscript. I would advise the authors to use one that is consistent (either the numbers should always be in subscript, or not).

- there is an issue with the formatting of one reference: the one that references the data from the authors (Starts by "Scripts..."). Consider reformatting it (Wang, Y. (2023). Scripts to reproduce the results in manuscript1. Zenodo. https://doi.org/10.5281/zenodo.8247969)

Best,

Maëlis Arnould and Craig Magee,