Reply to Referee Comment #2 (RC2)

Thank you for your feedback and suggested revisions. We appreciate your time and effort in reviewing our preprint.

We have considered the comments and taken action accordingly. We have made changes to address the majority of the issues raised by the reviewer.

The manuscript carefully distinguishes various ductile deformation stages and then focuses on the main deformation stage. This main stage, however, represents early (and in one case late) exhumation. My concern is to what extent shear stress during exhumation can be applied to rapid subduction as the title suggests.

Thank you for pointing this out. We have made the correction as follows:

The deformations we have studied were recorded during the early and late stages of the exhumation. However, the orogen-oblique stretching lineation of the Ds deformation is thought to reflect deformation closely related to rapid (24 cm/yr: Engebretson et al., 1985; Ishii & Wallis, 2020) and oblique subduction of the subducted Izanagi Plate (e.g., Wallis, 1992; Wallis et al., 2009). For these reasons we consider that the deformation under consideration formed as the result of rapid subduction.

Moreover, if deformation of subducted sediments were driven by a combination of Couette flow (simple shear) driven by the subducting plate, and Poiseulle flow (channelized flow) driven by a pressure gradient produced by the buoyancy of the subducted sediment (e.g., Fig.4 in Platt et al., 2018), it is possible that exhumation of these rocks is associated with stable subduction. In this case, what we observed can be the area close to the overriding plate within the plate boundary domain.

In the paper, additions will be made to Section 2.3 “Deformation of Shirataki unit during the main metamorphic stage” according to the description above.

The authors focus only on the quartz-rich regions. As outlined in the geological setting, the rocks are highly heterogeneous. The authors shortly address the fact that ultramafic bodies are minor and can be neglected.

Even if so, figure 3 clearly shows that the quartz shists are not the major lithology and that they are intercalated by pelitic and mafic shists. Such heterogeneities can cause stress concentration and result in larger scale stress gradients. Expanding the discussion in this direction as well as discussing relevant literature is needed.

Thank you for pointing this out. We have made the correction.

Tulley et al. 2020 compared the flow laws for various rocks with the strength of hydrous metabasalt inferred from the geological structure and quartz recrystallized grain size. The results showed that mica-containing metasediments can be harder or softer than hydrous metabasalt or amphibolite, depending on temperature conditions. It was also shown that the strength of hydrous metabasalt is reduced by pressure solution creep and slip of phyllosilicates, which plays an important role in deformation along the subduction boundary. Therefore, the discussion of deformation other than quartz schist, pelitic, and psammitic schist is important for the discussion of rock deformation at subduction boundaries. In this study, no microstructural observations or stress estimates of quartz schist and basic schist in the chlorite zone, or pelitic and basic schist in the garnet and albite zones have been made. However, previous studies showed that the basic schist in the oligoclase biotite zone appears to be less affected by Ds deformation than other rock bodies (e.g., Mori and Wallis., 2010), indicating that the associated strain is smaller. In addition, the quartz schist in the garnet zone has well-developed sheath folds (Wallis, 1990; Endo and Yokoyama, 2019), which are not observed in the surrounding lithologies suggesting that the strain in the quartz schist is particularly high. It is therefore possible that each rock body was deformed at a different strain rate and may have been deformed at the same stress. To investigate this, stress estimates should be made from the quartz domains for each schist, and the strength relationship between the other domains and the quartz domains in each schist should be investigated from structural and textual observations to constrain the deformation strength of each schist. If the flow laws of the constituent minerals are known, they may be combined to estimate the deformation of the entire rock body (Condit et al., 2022). It is also important to focus on lithological boundaries to confirm the presence or absence of structures that are attributable to strength contrasts, and this is a topic for future research.

Shear zones by antigorite serpentinite exist at the boundary between mantle wedge-derived serpentinite and pelitic schist (Kawahara et al., 2016). Although the area examined in our study is on the oceanic plate side of the subduction boundary region, it is possible that different minerals and different stress and strain conditions existed on the overriding plate side. Further research is needed on this as well.

In the paper, additions will be made to Section 4.1 “Stress recorded by quartz microstructure and in the subduction plate interface” according to the description above.

Furthermore, heterogeneities also occur on a micro scale. The piezometers were applied to quartz-only domains. The authors argue that the presence of sheet silicates inhibits grain growth and might cause wrong estimates on differential stresses. The authors argue further that sheet silicates do not form a network. However, in figure 6a it seems the sheet silicates form a continuous layer. Again, such heterogeneities can cause stress gradients. It would be interesting to see how much variation in shear stress is obtained between quartz-only domains and more heterogenous domains. And if significant these uncertainties should be included into the discussion. Knowing that additional measurements need time and effort, I think the manuscript would already benefit if these points were addressed theoretically in the discussion.

Thank you for your comments regarding the deformation of quartz-rich metasediments that also contain significant amounts of mica. We propose the following revisions.

●Sample ASM2,3,4

The estimated stresses are almost identical to the stresses received by the rock body. However, the stresses received by the mica minerals may be even smaller, as the strength of the mica is assumed to be lower than the strength of the quartz dislocation creep under the temperature conditions treated in our study.

Detail of the above discussion is stated in lines 32–65 of the reply comment for RC1.

●Sample ASM1

It is likely that the obtained stress is considered to be largely representative of the stresses undergone by the pelitic and psammitic schists of the chlorite zone. Such situations are only likely to occur when the deformation conditions are located near the boundary between the dislocation creep domain and the pressure solution creep domain. The change in the deformation mechanism between the vein/fringe and microlithon domains can be attributed to the difference in the degree of grain growth inhibition and activation of pressure solution creep due to the presence or absence of the quartz-mica boundary.

Detail of the above discussion is stated in lines 66–163 of the reply comment for RC1.

In the revised paper, the above text, figures, and tables (lines 32–163 of the reply comment for RC1) will be added to Sec 4.1 "Stress recorded by quartz microstructure and in the subduction plate interface".

Line 70: "Shear stress is equal to half the differential stress." Only the maximum shear stress is equal to half the differential stress. Indeed, on line 37 the author write that shear stress is used for absolute maximum shear stress. I would suggest to strictly write maximum shear stress. The data presented are estimates on the maximum shear stress and for the discussion it is crucial to use accurate terms.

Thank you for pointing this out. We have made the correction.

Figure 1: the unit boundary of the smaller eclogite units is hardly distinguishable from small ultramafic bodies. I suggest using different colors for the boundary and the ultramafic bodies. (Actually, the color for ultramafic bodies in figure 3 is different)

Thank you for pointing this out. Lithology information will be deleted, and a geological map of the same area will be produced and placed side by side.

Figure 2c: Can you add PT values here? Or otherwise plot the ductile deformation stages in 2a.

Thank you for your valuable comments. Deformation temperature pressure conditions vary according to metamorphic grade, making it difficult to fill in specific values. Therefore, the text has been amended as follows:

Line 136 “The main metamorphism that formed the Shirataki unit has four recognized ductile deformation phases, named Dr, Ds, Dt, and Du deformation, respectively (Wallis, 1990; Fig. 2b, 2c).”

⇒”Each of the four metamorphic zones formed by the main metamorphic stage has a unique PT path (Fig. 2a). Moreover, all metamorphic zone has four recognized ductile deformation phases, named Dr (burial), Ds (exhumation near the peak metamorphic condition), Dt (exhumation after the peak metamorphic condition), and Du (Slightly burial after exhumation) deformation, respectively (Wallis, 1990; Fig. 2b, 2c). ”

Line 151 “(c) Main metamorphism P–T–D path of the Shirataki unit (Aoya, 2001) modified by Kouketsu et al. (2021).”
⇒ “(c) Deformation phases in the Shirataki unit (after Kouketsu et al., 2021). This P-T path corresponds to each metamorphic zone P–T path in the main metamorphism in Fig. 2a.”

The Fig. 2c was modified to clarify the correspondence between Fig. 2a and Fig. 2c.

Figure 4: Can you also add pole figures?

Thank you for pointing this out. We have made the correction.

Table 3: The Cr+Ho data are a based on a corrected version of the Cross et al. piezometer after Holyoke et al. This is only mentioned in the discussion part. Please add some details also in the method section for better understanding of the present table.

Thank you for pointing this out. The following text has been added to the method section.

“In this study, we also used the piezometer of Cross et al. (2017) with a correction for measured values by Griggs apparatus, which is proposed by Holyoke and

Kronenberg (2010). In this case, the stress value is 0.73 times the value obtained by piezometer of Cross et al. (2017).”

Once again, we sincerely appreciate the opportunity to address your comments and concerns. If you have any further comments or queries, please do not hesitate to contact us.

Yours sincerely

Authors.

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