nature portfolio

Peer Review File

Impact of dehydration on chlorite and intermediate-depth earthquakes

Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality

22nd Jun 23

Dear Professor Jung,

Your manuscript titled "Impact of dehydration on chlorite and intermediate-depth earthquakes" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns. Specifically, we require that you re-evaluate and clarify your interpretation of the data based on previous work and place your results in the context of recent developments in the field.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

[Link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Sylvain Barbot
External Editor
Communications Earth & Environment

Joe Aslin
Senior Editor
Communications Earth & Environment

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements (Download the link to your computer as a PDF.)

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear Authors,

Please find attached the full review file, containing the letter to the editor, the major comments and the line-by-line comments.
The paper is not OK as it is, as I tried to explain as carefully as possible. I hope it makes sense.
The key is the discussion on volume changes, and I am a bit supersized that you did not mention it, as you are among the most cited author about the relevance of this parameters at intermediate depths.
Normally you should be able to do the revisions relatively quickly.
I am not worried.

Best wishes,
Thomas P. Ferrand

communications earth & environment – manuscript COMMSENV-23-0561-T

Impact of dehydration on chlorite and intermediate-depth earthquakes
Letter to editor and review file – Dr. Thomas P. Ferrand

Dear CEE editorial team,

Thank you very much for contacting me to review this manuscript, and sorry for the delay.

I have read the manuscript COMMSENV-23-0561-T and I can now provide a detailed review. The topic is directly related to my research of the past ten years. Consequently, I have knowledge about the literature and about the up-to-date boundary between facts and interrogations on the topic.

The authors question the triggering mechanism of earthquakes of the lower Wadati-Benioff plane with high-P, high-T experiments. They propose that the chlorite instability (in chlorite-bearing peridotites) is the cause of these events and that the triggering mechanism is dehydration embrittlement, i.e. fluid-induced rupture nucleation similar to fracking (Raleigh & Paterson, 1965). From either field (e.g. Scambelluri et al., 2017), mechanical (e.g. Ranero et al., 2003) petrological (e.g. Ferrand et al., 2017) or mineral physics (e.g. Burnley et al., 2013) perspectives, it is very relevant to study slightly chloritized peridotites. This is a meaningful follow-up to the studies on slightly serpentinized peridotites (Xia, 2013; Ferrand et al., 2017).

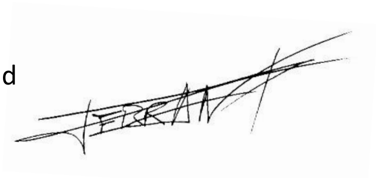
Overall, this is a good scientific contribution because of the quality of the experimental series and the quality of the presentation of the data. However, in my opinion, it is not publishable as it is because of a lack of consideration for several achievements in the field (including mine) and connected disciplines. Because of this lack of consideration for the literature and lack of precision regarding key concepts, most of the conclusions of the paper are incorrect. Nonetheless, all can be easily fixed relatively fast.

To help the authors, I provide a detailed review pointing out the issues, notably including the problematic interpretation based on the ‘dehydration embrittlement’ model and the flawed setting aside of the serpentine in the interpretation of the lower plane. I detail the reasons why the reported data do not support the interpretations, Finally, I urge the authors to recall alternative mechanisms from a fair review of the literature.

I hope the authors understand my comments and consider them useful.

Best regards,

Thomas P. Ferrand



NB: *As this work deals with a topic to which I have contributed in recent years, I refer to my papers in this review. Here I declare that my goal is not to increase my citation score, but provide constructive comments to improve the quality of the work. For sake of transparency, I always sign my reviews.*

Major comments:

NB: *These major comments are complemented by detailed line-by-line comments, fully referenced, for the authors to strengthen their manuscript. I apologize for any redundant comments.*

Major comment #1: Dehydration embrittlement (hydrofracking) *vs* stress transfers (DDST/TDST)

From the beginning to the end of the manuscript, the authors claim in favour of the “dehydration embrittlement” model, which relies on fluid overpressure (i.e. hydrofracking). Conceptualized from low-pressure experiments (Raleigh & Paterson, 1965), hydrofracking is not demonstrably accurate at high pressures (Ferrand, 2019a). Yet, it has been a working hypothesis in several papers, seriously contradicted by both experimental and natural data, as detailed hereinafter. This flaw (among others) seems to originate from a lack of consideration from the literature (see ***major comment #2***).

From experimental results, the dehydration-driven stress transfer (DDST) model was formulated. The main difference with the "dehydration embrittlement" model is that the dehydration fluids are not the main control of the mechanical instability generation (Ferrand et al., 2017). A key argument to support the DDST is that seismic ruptures were triggered within olivine aggregates by local serpentine dehydration for either positive or negative volume changes. Seismicity associate with negative Clapeyron slopes, and thus negative volume changes (solid+fluid) are absolutely incompatible with the idea of fluid overpressure (e.g. Jung et al., 2004; Jung & Green, 2004; Ferrand et al., 2017). In the present study (see **modified Table 1** hereinafter), faults are described in samples deformed in conditions where chlorite dehydration is characterized by both positive (P < 2 GPa) and negative (P > 2 GPa) volume changes. Consequently, the dehydration embrittlement model cannot be applicable as a common explanation for the entire dataset, contrary to the DDST. The authors should rather consider one of the key conclusions of our work: only a very small/local amount of unstable minerals is required at the tip of 'serpentinized' faults (lower Wadati-Benioff plane) and regions with too high fractions of weak minerals are not seismogenic, i.e. interplane volume (Ferrand et al., 2017; Ferrand, 2019a). This reconciles previously antagonist studies, showing for example that the subducting mantle is mostly dry or with a very minor hydrous fraction (e.g. Reynard et al., 2010). Seismic ruptures are triggered within fresh peridotite volumes in between the serpentinized faults (Ferrand et al., 2017).

Table 1. Experimental conditions and results for deformation experiments of chlorite peridotite.

Sample	Pressure (GPa)	Temperature (°C)	Shear strain (γ)	Strain rate (s^{-1})	Maximum differential stress (MPa)	Fault development	inverse Clapeyron slope	volume change
JH101	**0.5**	**650±15**	2.1±0.5	2.65×10^{-6}	254±15	X	> 0	> 0
JH116	**0.5**	**650±15**	4.5±1.0	9.68×10^{-6}	348±25	X		
JH127	**1.0**	**650±15**	3.5±0.8	6.30×10^{-6}	120±20	X		
JH100	**1.0**	**720±20**	2.1±0.5	5.48×10^{-6}	131±20	X		
JH138	**1.5**	**650±15**	3.3±0.8	6.26×10^{-6}	352±35	O		
JH218	**1.5**	**650±15**	3.1±0.8	6.03×10^{-6}	415±35	O		
JH146	**2.0**	**500±10**	3.3±0.8	6.90×10^{-6}	354±20	X	0	0
JH154	**2.0**	**570±10**	2.8±0.6	6.41×10^{-6}	259±15	X		
JH139	**2.0**	**600±10**	3.8±0.9	6.19×10^{-6}	346±15	X		
JH140	**2.0**	**650±15**	3.8±0.9	6.36×10^{-6}	173±20	X		
JH220	**2.0**	**670±15**	4.9±1.5	6.77×10^{-6}	363±30	X		
JH228	**2.0**	**730±20**	3.5±0.8	7.33×10^{-6}	489±35	O		
JH156	**2.5**	**540±10**	1.6±0.3	6.53×10^{-6}	238±20	X	< 0	< 0
JH152	**2.5**	**630±10**	4.0±1.0	6.51×10^{-6}	171±20	X		
JH221	**2.5**	**690±15**	2.1±0.5	6.81×10^{-6}	130±20	X		
JH225	**2.5**	**750±20**	4.9±1.5	7.12×10^{-6}	169±20	O		

Although the DDST model comes from experiments on serpentinized peridotites, the authors should be aware of 2 things: [1] the DDST is supported by several field works (e.g. Dunkel et al., 2017; Scambelluri et al., 2017) and seismological data (e.g. Kita & Ferrand, 2018); [2] the DDST is by definition applicable to any lithology containing an unstable phase, i.e. any transformation (TDST, Ferrand 2019a). Both dehydration fluids and local critical stress distortions are consequences of the dehydration reaction, but it does not mean that the fluids are the cause of the dynamic ruptures. Field work also revealed that fluids escape from dehydrating peridotites through channel-forming reactive porosity (Plümper et al., 2017a). Such transient fluids have recently been evidenced at high pressure by seismological data (Bloch et al., 2018). Fluid percolation has also been described during hydrothermal alteration of the continental crust through feldspar nanopores (Plümper et al., 2017b). Large-scale fluid percolation would impact the stress field within subducted materials (Craiu et al., 2022).

The distinction between the triggering mechanism (generation of a local mechanical instability) and the rupture itself (sudden release of elastic energy throughout the sample) should also be introduced (see ***major comment #3***). Importantly, significant stress distortions can build up in natural systems due to rheological contrasts, e.g. serpentinized faults within subducting slabs, as supported by field observations (e.g. Scambelluri et al., 2017; Campbell et al. 2020), laboratory experiments (e.g. Burnley et al., 2013; Ferrand et al., 2017) and numerical modelling (Toffol et al., 2022). Note, however, that dehydration-induced mantle earthquakes are also reported within the Vrancea locked slab (Ferrand & Manea, 2021), which means that active subduction is not required for stress preservation.

Major comment #2: Lack of consideration for the literature on the lower Wadati-Benioff plane

The authors state that serpentine would not be stable in the P-T conditions of the lower Wadati-Benioff plane, and that chlorite would therefore explain the lower-plane seismicity. This reasoning is based on incorrect statements on mineral stability, and artificially builds the relevance of chlorite dehydration in subduction seismicity. I suggest that the authors rewrite the paper in a way that makes more sense with regard to thermodynamics and previous experimental achievements.

The lower Wadati-Benioff plane (Hasegawa et al., 1978) is demonstrably correlated with serpentine destabilization (Ferrand, 2019a, and references therein). Numerous geophysical studies have helped better understand the nature and dynamics of double-seismic structures (e.g. Hasegawa & Nakajima, 2017; Bloch et al., 2018; Cai et al., 2018). The location of the lower plane (distance from the upper plane) is directly related to plate age (Brudzinski et al., 2007; Florez & Prieto, 2019) as it corresponds to the brittle-ductile transition of the oceanic plate before subduction (Ranero et al., 2003; Ferrand, 2019a; 2019b). The triggering mechanism of the lower-plane seismicity may be due to various mineral destabilizations (including chlorite). The very last trial to interpret the lower-plane seismicity is a numerical study considering thermal runaway processes affecting local carbonate minerals within the serpentinite faults during subduction (Prakash et al., 2023). Anyways, most of the lower-plane seismicity appears to be related to serpentine dehydration for either actively subducting slabs (e.g. Ferrand, 2019a) or locked slabs (Ferrand & Manea, 2021). If the authors want to argue against all of this, they would need to provide a detailed and convincing explanation. Making a big controversy out of some results by ignoring a significant part of previous achievements is not reasonable.

Importantly, in most experimental condictions, clinochlore (the Mg-rich chlorite) is expected to be stable, as recalled by the authors. At pressure ranging from 1.5 to 2.5 GPa, chlorite becomes unstable at temperatures above 850°C (MASH system, e.g. Staudigel & Schreyer, 1977). In contrast chlorite-bearing parageneses start destabilizing around 720-750°C (see review Ferrand, 2019a). This certainly explains why only 'partial' dehydration is observed in the experiments, and it should definitely be discussed in the present paper.

Major comment #3: Numerical simulations should be used with care to avoid misinterpretation

The authors artificially exclude serpentine minerals from the possible causes of dynamic ruptures upon dehydration in the lower Wadati-Benioff plane based on some pseudosections. They seem to consider the numerical models as directly informative about natural rocks at depth, but the models only consider simplified compositions with achieved thermodynamic equilibrium, and irrelevantly ignore serpentine minerals. The authors do not discuss mineral destabilizations within serpentinized faults in terms of local equilibria, and totally forget the kinetic issues in rocks at relatively low temperatures. Throughout the manuscript, several inaccurate statements or simplifications can be listed. I have tried to describe these in details in the ***line-by-line comments***.

Many studies have tried to refine thermodynamic calculations to fit experimental data in internally consistent datasets (e.g. Berman et al., 1986; Berman, 1988; Holland & Powell, 1998; Wunder & Schreyer, 1997) and this work is still progressing. Further updates of reaction curves positions are likely. It should be recalled here that the experimental stability curves (e.g. Ferrand, 2019a; Ferrand & Manea, 2021) fundamentally differ from pseudosections. Experimental P-T curves are deduced from direct observations of samples after experiments mimicking the geological processes; pseudosections, constrained from experimental results, are based on thermodynamic calculations in equilibrated systems. Used in many petrological and mineralogical studies (e.g. Bretscher et al., 2018; Fumagalli & Poli, 2005; Padrón-Navarta et al., 2013), pseudosections are a powerful tool to understand metamorphic parageneses. Nonetheless, pseudosections only consider phases that are defined as an input in the calculation. Moreover, considering relatively low temperatures (e.g. 600 °C) in subduction contexts and variable amount of minor hydrous/carbonated phases within fault gouges, local equilibria within serpentinized faults should be considered.

The timing of mineral destabilizations notably depends on local parageneses, chemical variability, and fluid concentrations/mobility/availability (Ferrand, 2019a). Most importantly, fluid percolation is controlled by fractures connectivity and by the structure of either hydrous minerals themselves (e.g. Tutolo et al., 2016) or transforming zones (e.g. Plümper et al., 2017a; 2017b). Reactions associated with negative volumes changes might help generate fluid percolation pathways and thus favour further dehydration (e.g. Craiu et al., 2022).

The enhanced diffusivity in transforming aggregates could explain the observed Ca mobility and thus the formation of tremolite within faults as a product of the metamorphic transformation. An alternative possibility could be the transient melting and fast crystallization, which are expected during dynamic ruptures under pressure (e.g. Incel et al., 2017; Ferrand et al., 2017; 2018; 2021; Ohuchi et al., 2020; 2022; Mao et al., 2022; Gasc et al., 2022). In either nature or experimental analogues, seismic ruptures under high pressures are systematically associated with a transient lubrication (usually dynamic melting, fossilized as pseudotachylytes) and elastic waves radiation. There is no acoustic recording in the present study, but the authors should provide some observation of former melt that would have been quenched as glassy and/or fine-grain materials. If there is no seismic rupture, or no documented proof of it, then the authors should discuss it, and mention that the study remains relevant anyways as it tackles at least part of the process.

Finally, it might be good to clarify the uncertainty on pressures, and to what extent these can be considered stable during the experiment. What is the level of assumptions? Is it equal to the isotropic part of the stress tensor? A quick reminder about the notions of confining pressure, mean stress and pressure-depth conversion could help the reader.

Major comment #4: Triggering mechanisms *vs* rupture mechanisms

There is a confusion between triggering mechanisms (i.e. trigger of the critical mechanical instability, e.g. fracking or fluid-induced fault reactivation or reaction-induced grain size reduction) and rupture mechanisms (i.e. nucleation and dynamic propagation of the rupture). Note that these can be studied separately. It is important that the authors do not include rupture mechanisms among the triggering mechanisms, because the community is already a bit confused on earthquake mechanics. On one hand, among the triggers we have the classic (but invalid at high pressure) "dehydration embrittlement" involving fluid pressure (Raleigh & Paterson, 1965), the reaction-induced grain size reduction (e.g. Green et al., 2015; Incel et al., 2017; Thielmann, 2018b) and the TDST (Ferrand et al., 2017; Ferrand, 2019a). Note that the TDST is not incompatible with reaction-induced grain size reduction, as it is another way to talk about the same process: the reaction-induced weakening of the destabilizing mineral assemblage. The self-localizing thermal runaway (e.g. Thielmann et al, 2015; Thielmann, 2018a) accounts for the rupture nucleation and propagation but does not bring any insight about the triggering mechanism. It has been showed that an initial grain-size reduction is required to start the thermal runaway process (Thielmann, 2018b), and I have explained that the easiest way to reduce grain size is the destabilization of minerals (Ferrand, 2019a). On the other hand, among the rupture mechanisms we have for example melt-assisted lubrication (e.g. Di Toro et al., 2006; Ferrand et al., 2018; 2021), thermal pressurization (e.g. Wibberley & Shimamoto, 2005; Viesca & Garagash, 2015) and transformational faulting (e.g. Burnley et al., 1991; Schubnel et al., 2013). The latter can be either considered as a triggering or rupture process depending on published studies, as "transformational faulting" is still largely mysterious and concept-based (see a recent trial of clarification for deep earthquakes mechanisms thanks to machine learning: Mao et al., 2022).

Line-by-line comments

Lines 24-27 → This sentence opposing 'cold' upper-plane earthquakes and 'warm' lower-plane earthquakes is totally misleading, any many other statements throughout the manuscript. The coldest part of the slab is in between the two high-seismicity planes, where minor seismicity is observed.

Line 29: 'the dehydration embrittlement of chlorite' → 'the chlorite' dehydration'

> **WARNING: conceptual issue!!** Throughout the manuscript, the authors argue that they show observations supporting dehydration embrittlement (i.e. a specific mechanism) although they only report observations supporting that seismic ruptures can nucleate due to a dehydration reaction. This is fundamentally different, and this shortcut in the reasoning leads to conclusions that actually contradict observational data. → **Major comment #1.**

Lines 32-33: 'partial chlorite dehydration' → We need to know what 'partial' actually means in this context. As far as I understand, if only part of the crystal destabilized in P-T conditions close to the breakdown conditions for a specific composition (e.g. clinochlore in the MASH system), then it means that the volumes of chlorite with slightly different compositions have remained stable because the P-T conditions are still within their thermodynamic stability field. This is explained in details by Ferrand (2019a; 2019b). → **Major comment #2.**

Lines 34-36 → **WARNING** → As shown for active subductions zones in Japan, Cascadia and Chile (among others), lower-plane hypocentral locations fit the conditions of destabilization of serpentine minerals (Ferrand, 2019a). I would estimate to 5-10% the fraction of earthquakes actually due to chlorite dehydration. This is also the case for the locked subducted slab currently dehydrating beneath Vrancea (Ferrand & Manea, 2021).

Line 36: 'dehydration embrittlement' → **WARNING** → The word 'embrittlement' transforms a true observation-based sentence into a concept-based and incorrect statement…

Lines 45-46: 'the latter' → no!! Major conceptual issue. See the key nuance between 'dehydration embrittlement' and 'dehydration-driven stress transfer'. → **Major comment #1.**

Line 48 → We have demonstrated (Ferrand et al., 2017; Kita & Ferrand, 2018) that DDST is the most likely mechanism in the lower plane. → **Major comment #1.**

Lines 49-51 → incorrect, as explain by Ferrand (2019a).

Line 50 → Lawsonite is irrelevant for the lower plane.

Line 53: 'and their cause remains unknown' → This statement is incorrect and shows a profound lack of consideration for key achievements. The authors cannot write their paper as if nothing has happened in the last 10 years. This is not reasonable. → **Major comment #1.**

Lines 55-56: 'Chlorite has a much wider stability field than serpentine in subduction zones under high P-T conditions' → As it is, the sentence may sound either incorrect or unclear. From experiments, we know that antigorite demonstrably has the wider stability field in subducting lithospheric mantle rocks (see the review: Ferrand, 2019a). → clarification needed.

Lines 56-57: 'Chlorite peridotite is stable in the lower layer of the double seismic zone, but serpentinite is not.' → **WARNING: demonstrably incorrect**, as clearly shown for Japan, Cascadia and Chile (Ferrand, 2019a)!! → **Major comment #2.**

Line 60 → a reference to Ferrand (2019a) is required here.

Lines 64-66 → **no**: - not 'chlorite' but 'chlorite peridotite';
- not ''dehydration embrittlement' but 'dehydration'.
(as it is, it is a mix between data and concepts, leading to incorrect conclusions).

Lines 71-72: '0.5-2.5 GPa' → **WARNING:** around 2 GPa of confining pressure.

Line 72: '500-750°C' → **WARNING:** in the pressure range from 1.5 to 2.5 GPa, chlorite becomes unstable at temperatures above 850°C (MASH system, e.g. Staudigel & Schreyer, 1977). In contrast chlorite-bearing parageneses start destabilizing around 720-750°C (see review Ferrand, 2019a). This certainly explains why only 'partial' dehydration is observed in the experiments, and it should definitely be discussed in the present paper. → **Major comment #2.**

Lines 73-75 → This is doubtful because it would contradict all the experimentally-deduced databases (review by Ferrand, 2019a). Pseudosections are supposed to be based on experimentally deduced

stability fields within internally-consistent databases, but should be used with care in order to avoid misinterpretations. → **See major comment #3.**
In addition, if the authors really want to 'discredit' serpentine minerals, they should perform the same calculations on serpentine minerals, notably the various antigorites exhibiting significant shifts in maximum temperature limits with varying compositions.
Line 77 → Is vesuvianite really stable at pressures around 2 GPa? Are there any observations of it in this study of previous studies? → clarification needed.
Line 80 → Very interesting observation. → It would be useful to discuss the shape and distribution of the fluid inclusions depending on the sign of the volume change (e.g. positive at 1 GPa and negative at 2.5 GPa).
Line 88: 'In samples without faults' → 'In samples in which no fault could be observed'. Indeed, the risk to miss 2D structures on a polished surface from a 3D volume is high… → to be clarified.
Line 93: 'a fault gouge that developed at 2.5 GPa and 750 °C' → NB: Chlorite is stable in these conditions, and if T increases a bit it would destabilize with a significantly negative volume change, incompatible with 'dehydration embrittlement'. → **Major comment #1.**
Line 96: 'Ca was distributed along the fault' → How should that be interpreted? Was there enough diffusion at grain boundaries for large cations to find their way during the dehydration? Or was dynamic melt required? This chemical mobility issue should at least be mentioned. → **Major comment #3.**
Line 99 → agreed.
Lines 105-106 → The stress-strain curves should be shown for the entire experimental series!! Not data should be hidden. Everything is potentially interesting, including details that the authors have not thought about yet, and it is important that the raw data are accessible to the community.
Line 106: 'to observe frictional behaviour' → What does that mean? Such concept-based wording can be confusing for part of the community. A more factual description of the deformation data is better.
Line 108: 'showed episodic stick-slip behaviours with a stress …' → 'showed an episodic stick-slip behaviour with stress …'
Line 109: 'which is a characteristic of a seismic fault' → 'which is characteristic of seismic faults'
Line 113: 'partial dehydration products' → **Major comment #2.**
Line 114 → a paragraph about volume change should be added here, in order to comment on this parameter in the discussion.
Line 115 → For the sake of clarity, I would separate the discussion from the conclusions.
Line 119: 'Kuriles' → 'Kuriles, as well as within remnants of subducted slabs (Ferrand & Manea, 2021; Craiu et al., 2022)'. For the sake of clarity, I would separate the discussion from the conclusions.
Line 120: '200 km' → a range would be better.
Line 126: 'dehydration embrittlement' → 'dehydration'
Lines 127-127 → The significant scientific achievements of the last decade should be fairly acknowledged. The manuscript as it is, it is not acceptable. The proposed mechanisms should be listed, including dehydration embrittlement (fluid overpressure) and DDST/TDST. → **Major comments #1.**
Line 131 → remove 'embrittlement'
Lines 129-131 → NB: Mineral dehydrations are demonstrably associated with both warm and cold subduction zones (e.g. Abers et al., 2003; Ferrand, 2019a).
Lines 135-136 → Have these studies really proposed that the 'dehydration embrittlement' of chlorite would be at play or have they proposed that the 'dehydration' of chlorite would play a role? This nuance is very important!!
Line 138: 'stable' → Indeed… Clinochlore (and most chlorite compositions) are expected to be stable in these conditions. → This is why the impact of thermal runaway processes (which can locally heat the rock by strain localization) should also be discussed (e.g. Thielmann et al., 2015; Thielmann, 2018; Kelemen & Hirth, 2007; Prakash et al., 2023). → **Major comments #2.**
Lines 138-143 → The paragraph is oriented in a way that is not supported by the literature. Antigorite is demonstrably the mineral that shows the best correlation between destabilization and seismicity (Ferrand, 2019a; 2019b; Manea & Ferrand, 2021).
Line 144 → The Fig.1 is not showing anything clear enough that would support such a statement.

Line 145 → Demonstrably incorrect.
Line 146 → remove 'embrittlement'.
Line 149: '~30% chlorite and ~1% dehydration product.' → This is interesting. Such small amounts of destabilized minerals would then be incompatible with significant fluid production, and thus the 'dehydration embrittlement' concept would likely not apply at all. The DDST is more likely. → **Major comments #1.** + I suggest to discuss also the possibility of thermal runaway instabilities triggering local dehydration (Kelemen & Hirth, 2007; Magott et al., 2020) → **Major comments #4**
Lines 149-150 → This is a theoretical estimate, that has not been confirmed by natural petrology, so some discussion is required.
Lines 150-152 → To be rewritten considering all comments.
Lines 153-155 → no → remove 'for the dehydration embrittlement of chlorite peridotite' and add a sentence recalling that 'Chlorites (e.g. clinochlore) are among the mineral phases identified as likely involved in seismicity upon dehydration.'
Line 157 → add references to Brudzinski et al. (2007) and Ferrand (2019a).
Line 158 → remove 'embrittlement'.
Lines 157-159 → vague easy sentence.
Line 160: 'which is likely responsible for the lower plane of the double seismic zone' → incorrect.
Line 161 → remove 'may' (see review by Ferrand, 2019a).
Lines 162-163 → vague. NB: the distance between the plane is fully explained in publications (e.g. Ferrand, 2019a; Florez & Prieto, 2019). It corresponds to the fossilized trace of the brittle-ductile transition (Ranero et al., 2003), and not only one mineral can destabilize and trigger seismicity there (although serpentine minerals are leading at depths up to 100 km; Ferrand, 2019a).
Lines 167-168: '…°C) in a representative subduction zone' → '…°C), representative of subduction zones'
Line 168: 'a dehydration product (Ca-amphibole)' → Not simply a dehydration product of chlorite, but of the chlorite peridotite paragenesis (Ca-bearing). The mobility of Ca should be discussed.
Line 169: 'partial dehydration' → **See major comment #3.**
Lines 170-173 → vague and disappointing, although the experimental results are significant and meaningful…
Table 1 → The meaning of 'x' and 'o' should be explicitly written, so that the reader does not get lost.
Fig.1 → the reference to Hacker et al. (2003) should come after 'serpentine' (line 310).
NB: 'cold' vs 'warm' is not fully describing the distribution of subducting lithospheres. For instance, the Costa Rica slab is warm but relatively steep. Some additional discussion would be welcome.

References

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Reviewer #2 (Remarks to the Author):

The authors have conducted deformation experiments on chlorite under a range of high pressure (1.5-2.5 GPa) and high temperature (650 -750 °C) to try to reveal its links to intermediate-depth earthquakes in subducting slabs. The results are interesting and important. I have some suggestions or questions about the manuscript:

1. The main question is that what are the main differences between the previous studies of lab experiments and this work. I suggest the authors should state it clearly in the Introduction or Discussion section, and point out the Highlights or new findings of the present study.
2. In the summary, I suggest the authors should show the values for pressure and temperature, e.g., line 24 "at high pressures (~1.5-5 GPa?)" , line 31 "high pressure (1.5-2.5 GPa) and high temperature (650-750 °C)".
3. Lines 24-26, 46-49, 125-128, 169-171 and so on. The authors suggested the upper-plane intermediate-depth seismicity of the double seismic zone (DSZ) is attributed to the dehydration embrittlement of hydrous minerals such as serpentine and lawsonite. However, it confused me. I think the popular hypothesis of dehydration embrittlement is that the upper-plane intermediate-depth seismicity is possibly linked to brittle failure associated with dehydration reactions of hydrous minerals (e.g. gabbro, lawsonite, etc, but not serpentine) in the slab crust, and the dehydration reactions of hydrous minerals (e.g. antigorite, clinochlore, chlorite, etc) in the slab mantle is possibly associated with the lower-plane intermediate-depth seismicity. Is it right?
4. Lines 134-137, what are reasons for no evidence supporting the hypothesis? Is it very difficult for previous studies to conducted deformation experiments on chlorite under a high P and high T condition or not? Please state it clear.
5. For the figures, I suggested the authors should list the values of the pressure and temperature both in figure and figure caption.
6. Lines 105-109, as shown in Extended Data Fig. 3, (a) denotes the stress-strain curve for a unfaulted sample, while (b) and (c) denote the stress-strain curves for the faulted samples. The authors should make it clear in the text. However, it seems that (a) and (b) show similar styles but are different from the style of (c), why? And how to get the stress drop of ~30-50+-20 MPa by the figure?
7. Line 119 "20-40 km" should be "~20-40 km", line 120 "of 200 km" should be "of ~200 km", line 123 "of 150 km" should be "of ~150 km".

Response to the Reviews

Thank you for allowing us to submit a revised draft of our manuscript titled “Impact of dehydration on chlorite and intermediate-depth earthquakes to *Communications Earth & Environment*. We appreciate the time and effort each of the reviewers have dedicated to providing insightful feedback on ways to strengthen our paper. Thus, it is with great pleasure that we resubmit our article for further consideration. Author responses are given in blue and changes to the manuscript are highlighted in yellow in the revised manuscript.

Responses to major comments on the pdf file of Reviewer #1

Major comment #1: Dehydration embrittlement (hydrofracking) vs stress transfers (DDST/TDST)

From the beginning to the end of the manuscript, the authors claim in favour of the “dehydration embrittlement” model, which relies on fluid overpressure (i.e. hydrofracking). Conceptualized from low-pressure experiments (Raleigh & Paterson, 1965), hydrofracking is not demonstrably accurate at high pressures (Ferrand, 2019a). Yet, it has been a working hypothesis in several papers, seriously contradicted by both experimental and natural data, as detailed hereinafter. This flaw (among others) seems to originate from a lack of consideration from the literature (see major comment #2).

From experimental results, the dehydration-driven stress transfer (DDST) model was formulated. The main difference with the “dehydration embrittlement” model is that the dehydration fluids are not the main control of the mechanical instability generation (Ferrand et al., 2017). A key argument to support the DDST is that seismic ruptures were triggered within olivine aggregates by local serpentine dehydration for either positive or negative volume changes. Seismicity associate with negative Clapeyron slopes, and thus negative volume changes (solid+fluid) are absolutely incompatible with the idea of fluid overpressure (e.g. Jung et al., 2004; Jung & Green, 2004; Ferrand et al., 2017). In the present study (see modified Table 1 hereinafter), faults are described in samples deformed in conditions where chlorite dehydration is characterized by both positive ($P < 2$ GPa) and negative ($P > 2$ GPa) volume changes. Consequently, the dehydration embrittlement model cannot be applicable as a common explanation for the entire dataset, contrary to the DDST. The authors should rather consider one of the key conclusions of our work: only a very small/local amount of unstable minerals is required at the tip of ‘serpentinized’ faults (lower Wadati-Benioff plane) and regions with too high fractions of weak minerals are not seismogenic, i.e. interplane volume (Ferrand et al., 2017; Ferrand, 2019a). This reconciles previously antagonist studies, showing for example that the subducting mantle is mostly dry or with a very minor hydrous fraction (e.g. Reynard et al., 2010). Seismic ruptures are triggered within fresh peridotite volumes in between the serpentinized faults (Ferrand et al., 2017).

Although the DDST model comes from experiments on serpentinized peridotites, the authors should be aware of 2 things: [1] the DDST is supported by several field works (e.g. Dunkel et al., 2017; Scambelluri et al., 2017) and seismological data (e.g. Kita & Ferrand, 2018); [2] the DDST is by definition applicable to any lithology containing an unstable phase, i.e. any transformation (TDST, Ferrand 2019a). Both dehydration fluids and local critical stress distortions are consequences of the dehydration reaction, but it does not mean that the fluids are the cause of the dynamic ruptures. Field work also revealed that fluids escape from dehydrating peridotites through channel-forming reactive porosity (Plümper et al., 2017a). Such transient fluids have recently been evidenced at high pressure by seismological data (Bloch et al., 2018). Fluid percolation has also been described during hydrothermal alteration of the continental crust through feldspar nanopores (Plümper et al., 2017b). Large-scale fluid percolation would impact the stress field within subducted materials (Craiu et al., 2022).

The distinction between the triggering mechanism (generation of a local mechanical instability) and the rupture itself (sudden release of elastic energy throughout the sample) should also be introduced (see major comment #3). Importantly, significant stress distortions can build up in natural systems due to rheological contrasts, e.g. serpentinized faults within subducting slabs, as supported by field observations (e.g. Scambelluri et al., 2017; Campbell et al. 2020), laboratory experiments (e.g. Burnley et al., 2013; Ferrand et al., 2017) and numerical modelling (Toffol et al., 2022). Note, however, that dehydration-induced mantle earthquakes are also reported within the Vrancea locked slab (Ferrand & Manea, 2021), which means that active subduction is not required for stress preservation.

Response: We sincerely thank the reviewer’s helpful comments. Conventionally, dehydration embrittlement has been regarded as a mechanism for triggering earthquakes. Notably, Jung, Green and Dobrzhinetskaya (2004) conducted experimental studies under high pressures reaching up to 6 GPa and established that this mechanism remains viable even when associated with a negative volume change. In response to the reviewer's suggestion, we have taken into account recent research on stress transfer models (DDST/TDST). From our study findings, it is indeed justifiable to consider DDST/TDST as potential earthquake triggering mechanisms. To address this, we have made modifications to the manuscript, incorporating relevant discussions concerning volume change and stress transfer models.

→ Table 1.

→ Lines 43–47 on pages 1–2.

→ Lines 85–86 on page 3.

→ Lines 134–140 on page 5.

→ Lines 206–215 on page 8.

→ Lines 233–236 on page 9.

Major comment #2: Lack of consideration for the literature on the lower Wadati-Benioff plane

The authors state that serpentine would not be stable in the P-T conditions of the lower Wadati-Benioff plane, and that chlorite would therefore explain the lower-plane seismicity. This reasoning is based on incorrect statements on mineral stability, and artificially builds the relevance of chlorite dehydration in subduction seismicity. I suggest that the authors rewrite the paper in a way that makes more sense with regard to thermodynamics and previous experimental achievements.

The lower Wadati-Benioff plane (Hasegawa et al., 1978) is demonstrably correlated with serpentine destabilization (Ferrand, 2019a, and references therein). Numerous geophysical studies have helped better understand the nature and dynamics of double-seismic structures (e.g. Hasegawa & Nakajima, 2017; Bloch et al., 2018; Cai et al., 2018). The location of the lower plane (distance from the upper plane) is directly related to plate age (Brudzinski et al., 2007; Florez & Prieto, 2019) as it corresponds to the brittle-ductile transition of the oceanic plate before subduction (Ranero et al., 2003; Ferrand, 2019a; 2019b). The triggering mechanism of the lower-plane seismicity may be due to various mineral destabilizations (including chlorite). The very last trial to interpret the lower-plane seismicity is a numerical study considering thermal runaway processes affecting local carbonate minerals within the serpentinite faults during subduction (Prakash et al., 2023). Anyways, most of the lower-plane seismicity appears to be related to serpentine dehydration for either actively subducting slabs (e.g. Ferrand, 2019a) or locked slabs (Ferrand & Manea, 2021). If the authors want to argue against all of this, they would need to provide a detailed and convincing explanation. Making a big controversy out of some results by ignoring a significant part of previous achievements is not reasonable.

Importantly, in most experimental condictions, clinochlore (the Mg-rich chlorite) is expected to be stable, as recalled by the authors. At pressure ranging from 1.5 to 2.5 GPa, chlorite becomes unstable at temperatures above 850°C (MASH system, e.g. Staudigel & Schreyer, 1977). In contrast chloritebearing parageneses start destabilizing around 720-750°C (see review Ferrand, 2019a). This certainly explains why only 'partial' dehydration is observed in the experiments, and it should definitely be discussed in the present paper.

Response: We have taken into careful consideration the existing literature on the triggering of lower-plane seismicity via serpentine dehydration. In response to this, we have made the necessary textual modifications to emphasise serpentine as the potential source of lower-plane seismicity. Additionally, we have acknowledged chlorite as an additional mineral that may also play a role in triggering lower-plane seismicity. Furthermore, we have included discussions pertaining to the concept of 'partial' dehydration of chlorite, drawing from relevant insights provided by previous studies.

→ Lines 24–27 on page 1.

→ Lines 51–55, 61, and 64–68 on page 2.

→ Lines 122–129 on page 5.

→ Lines 171 and 180–183 on page 7.

→ Lines 199 and 201 on page 8.

→ Lines 216–218 and 233–236 on page 9.

Major comment #3: Numerical simulations should be used with care to avoid misinterpretation

The authors artificially exclude serpentine minerals from the possible causes of dynamic ruptures upon dehydration in the lower Wadati-Benioff plane based on some pseudosections. They seem to consider the numerical models as directly informative about natural rocks at depth, but the models only consider simplified compositions with achieved thermodynamic equilibrium, and irrelevantly ignore serpentine minerals.

Response: We have introduced a novel pseudosection in our study, specifically focusing on the volume percentage of antigorite. This pseudosection was derived from the same starting material (please refer to Supplementary Fig. 4). It is important to note that the expected stability field for antigorite lies beyond the range of our experimental conditions, with the exception of one sample, JH146, which was subjected to conditions of 2.0 GPa and 500 °C. Worth highlighting is the fact that none of the samples, including the initial material, exhibited any presence of antigorite. Given its complete absence, we were unable to consider the dehydration impact of serpentine in this study.

→ Supplementary Fig. 4.

→ Lines 129–133 on page 5.

The authors do not discuss mineral destabilizations within serpentinized faults in terms of local equilibria, and totally forget the kinetic issues in rocks at relatively low temperatures. Throughout the manuscript, several inaccurate statements or simplifications can be listed. I have tried to describe these in details in the line-by-line comments.

Response: As recommended by the reviewer, we have discussed mineral destabilisations within serpentinised faults (lines 129–133). We have also corrected the inaccurate statements as suggested by the reviewer (see line-by-line comments and responses)

Many studies have tried to refine thermodynamic calculations to fit experimental data in internally consistent datasets (e.g. Berman et al., 1986; Berman, 1988; Holland & Powell, 1998; Wunder & Schreyer, 1997) and this work is still progressing. Further updates of reaction curves positions are likely. It should be recalled here that the experimental stability curves (e.g. Ferrand, 2019a; Ferrand & Manea, 2021) fundamentally differ from pseudosections. Experimental P-T curves are deduced from direct observations of samples after experiments mimicking the geological processes; pseudosections, constrained from experimental results, are based on thermodynamic calculations in equilibrated systems. Used in many petrological and mineralogical studies (e.g. Bretscher et al., 2018; Fumagalli & Poli, 2005; Padrón-Navarta et al., 2013), pseudosections are a powerful tool to understand metamorphic parageneses. Nonetheless, pseudosections only consider phases that are defined as an input in the calculation. Moreover, considering relatively low temperatures (e.g. 600 °C) in subduction contexts and variable amount of minor hydrous/carbonated phases within fault gouges, local equilibria within serpentinized faults should be considered.

Response: Calculated pseudosections in this study were based on the starting material composition. However, we admit that there may be an inconsistency between theoretical pseudosection findings and experimental results.

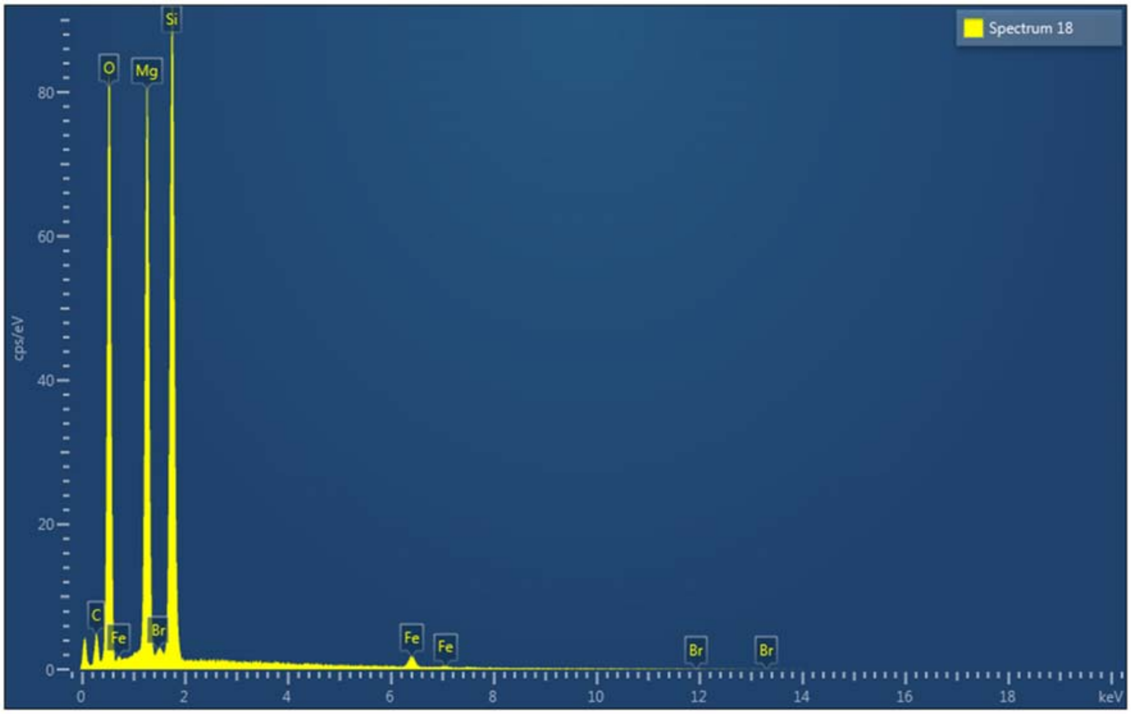
→ Line 75 on page 3.

→ Lines 122–133 on page 5.

The timing of mineral destabilizations notably depends on local parageneses, chemical variability, and fluid concentrations/mobility/availability (Ferrand, 2019a). Most importantly, fluid percolation is controlled by fractures connectivity and by the structure of either hydrous minerals themselves (e.g. Tutolo et al., 2016) or transforming zones (e.g. Plümper et al., 2017a; 2017b). Reactions associated with negative volumes changes might help generate fluid percolation pathways and thus favour further dehydration (e.g. Craiu et al., 2022).

The enhanced diffusivity in transforming aggregates could explain the observed Ca mobility and thus the formation of tremolite within faults as a product of the metamorphic transformation. An alternative possibility could be the transient melting and fast crystallization, which are expected during dynamic ruptures under pressure (e.g. Incel et al., 2017; Ferrand et al., 2017; 2018; 2021; Ohuchi et al., 2020; 2022; Mao et al., 2022; Gasc et al., 2022). In either nature or experimental analogues, seismic ruptures under high pressures are systematically associated with a transient lubrication (usually dynamic melting, fossilized as pseudotachylytes) and elastic waves radiation. There is no acoustic recording in the present study, but the authors should provide some observation of former melt that would have been quenched as glassy and/or fine-grain materials. If there is no seismic rupture, or no documented proof of it, then the authors should discuss it, and mention that the study remains relevant anyways as it tackles at least part of the process.

Response: We found pocketed glassy materials between fine grains of olivine near faults, which could have been associated with transient lubrication (Supplementary Fig. 5).



Commentary Fig. 1. Energy dispersive spectrometer peaks of glassy material. Atomic percentages of elements were Mg = 17.49%, Fe = 1.12%, Si = 19.16%, and O = 62.06%, which indicate silicate melt origin of glassy material.

Finally, it might be good to clarify the uncertainty on pressures, and to what extent these can be considered stable during the experiment. What is the level of assumptions? Is it equal to the isotropic part of the stress tensor? A quick reminder about the notions of confining pressure, mean stress and pressure-depth conversion could help the reader.

Response: The uncertainty of pressure estimation is caused by the friction and the application of a deviatoric stress and is less than ~10% (Mirwald, Getting and Kennedy, 1975) (already shown in the Method lines 270–272). To ensure the constancy of confining pressure throughout the experiments, we used a computer program at the Tectonophysics Laboratory, School of Earth and Environmental Sciences, Seoul National University. The degree of uncertainty associated with this process was measured to be less than approximately 50 MPa. Detailed information regarding pressure uncertainty is available in the Methods section, and the pressure-depth conversion has been added to the text (line 74). A pressure of 2 GPa is equivalent to a pressure at the depth of ~60 km (line 74).

→ Lines 73–75 on page 3.

→ Lines 142–143 on page 5.

→ Lines 270–272 on page 11.

Major comment #4: Triggering mechanisms vs rupture mechanisms

There is a confusion between triggering mechanisms (i.e. trigger of the critical mechanical instability, e.g. fracking or fluid-induced fault reactivation or reaction-induced grain size reduction) and rupture mechanisms (i.e. nucleation and dynamic propagation of the rupture). Note that these can be studied separately. It is important that the authors do not include rupture mechanisms among the triggering mechanisms, because the community is already a bit confused on earthquake mechanics. On one hand, among the triggers we have the classic (but invalid at high pressure) “dehydration embrittlement” involving fluid pressure (Raleigh & Paterson, 1965), the reaction-induced grain size reduction (e.g. Green et al., 2015; Incel et al., 2017; Thielmann, 2018b) and the TDST (Ferrand et al., 2017; Ferrand, 2019a). Note that the TDST is not incompatible with reaction-induced grain size reduction, as it is another way to talk about the same process: the reaction-induced weakening of the destabilizing mineral assemblage. The self-localizing thermal runaway (e.g. Thielmann et al, 2015; Thielmann, 2018a) accounts for the rupture nucleation and propagation but does not bring any insight about the triggering mechanism. It has been showed that an initial grain-size reduction is required to start the thermal runaway process (Thielmann, 2018b), and I have explained that the easiest way to reduce grain size is the destabilization of minerals (Ferrand, 2019a). On the other hand, among the rupture mechanisms we have for example melt-assisted lubrication (e.g. Di Toro et al., 2006; Ferrand et al., 2018; 2021), thermal pressurization (e.g. Wibberley & Shimamoto, 2005; Viesca & Garagash, 2015) and transformational faulting (e.g. Burnley et al., 1991; Schubnel et al., 2013). The latter can be either considered as a triggering or rupture process depending on published studies, as “transformational faulting” is still largely mysterious and concept-based (see a recent trial of clarification for deep earthquakes mechanisms thanks to machine learning: Mao et al., 2022).

Response: As the reviewer suggested, we have distinguished triggering mechanisms from rupture mechanisms.

→ Lines 41–42, and 43–47 on pages 1–2.

→ Lines 134–147 on pages 5–6.

Responses to line-by-line comments on the pdf file of Reviewer #1

Lines 24-27 → This sentence opposing 'cold' upper-plane earthquakes and 'warm' lower-plane earthquakes is totally misleading, any many other statements throughout the manuscript. The coldest part of the slab is in between the two high-seismicity planes, where minor seismicity is observed.

Response: We have modified the text to avoid confusion.

Line 29: 'the dehydration embrittlement of chlorite' → 'the chlorite' dehydration'

WARNING: conceptual issue!! Throughout the manuscript, the authors argue that they show observations supporting dehydration embrittlement (i.e. a specific mechanism) although they only report observations supporting that seismic ruptures can nucleate due to a dehydration reaction. This is fundamentally different, and this shortcut in the reasoning leads to conclusions that actually contradict observational data. → Major comment #1.

Response: We modified the text from 'dehydration embrittlement' to 'dehydration', as suggested.

→ Line 28 on page 1.

Lines 32-33: 'partial chlorite dehydration' → We need to know what 'partial' actually means in this context. As far as I understand, if only part of the crystal destabilized in P-T conditions close to the breakdown conditions for a specific composition (e.g. clinochlore in the MASH system), then it means that the volumes of chlorite with slightly different compositions have remained stable because the P-T conditions are still within their thermodynamic stability field. This is explained in details by Ferrand (2019a; 2019b). → Major comment #2.

Response: We have added relevant text to discuss partial dehydration of chlorite, as suggested.

→ Lines 122–129 on page 5.

Lines 34-36 → WARNING → As shown for active subductions zones in Japan, Cascadia and Chile (among others), lower-plane hypocentral locations fit the conditions of destabilization of serpentine minerals (Ferrand, 2019a). I would estimate to 5-10% the fraction of earthquakes actually due to chlorite dehydration. This is also the case for the locked subducted slab currently dehydrating beneath Vrancea (Ferrand & Manea, 2021).

Response: We have modified the text to avoid confusion.

Line 36: ‘dehydration embrittlement’ → WARNING → The word ‘embrittlement’ transforms a true observation-based sentence into a concept-based and incorrect statement…

Response: We have modified the text from ‘dehydration embrittlement’ to ‘dehydration’, as suggested.

→ Line 36 on page 1.

Lines 45-46: ‘the latter’ → no!! Major conceptual issue. See the key nuance between ‘dehydration embrittlement’ and ‘dehydration-driven stress transfer’. → Major comment #1.

Response: We have modified the text, as suggested.

Line 48 → We have demonstrated (Ferrand et al., 2017; Kita & Ferrand, 2018) that DDST is the most likely mechanism in the lower plane. → Major comment #1.

Response: We have added this in the text, as suggested.

Lines 49-51 → incorrect, as explain by Ferrand (2019a).

Response: We have modified the text, as suggested.

Line 50 → Lawsonite is irrelevant for the lower plane.

Response: Yes, lawsonite is relevant at the subducting oceanic crust.

Line 53: 'and their cause remains unknown' → This statement is incorrect and shows a profound lack of consideration for key achievements. The authors cannot write their paper as if nothing has happened in the last 10 years. This is not reasonable. → Major comment #1.

Response: We have modified the text per the reviewer's suggestion. Specifically, we have incorporated relevant prior research that discusses serpentine as a potential source of lower-plane seismicity. Additionally, we have acknowledged chlorite as an additional mineral that may also have the capability to trigger lower-plane seismic events.

→ Lines 47–48 and 51–55 on page 2.

Lines 55-56: 'Chlorite has a much wider stability field than serpentine in subduction zones under high P-T conditions' → As it is, the sentence may sound either incorrect or unclear. From experiments, we know that antigorite demonstrably has the wider stability field in subducting lithospheric mantle rocks (see the review: Ferrand, 2019a). → clarification needed.

Response: We have modified the text to avoid confusion.

Lines 56-57: 'Chlorite peridotite is stable in the lower layer of the double seismic zone, but serpentinite is not.' → WARNING: demonstrably incorrect, as clearly shown for Japan, Cascadia and Chile (Ferrand, 2019a)!! → Major comment #2.

Response: We have removed the incorrect and vague expressions, as suggested.

→ Lines 56–58 on page 2.

Line 60 → a reference to Ferrand (2019a) is required here.

Response: We have added the reference, as suggested.

→ Line 61 on page 2.

Lines 64-66 → no: - not ‘chlorite’ but ‘chlorite peridotite’;

- not ‘’dehydration embrittlement’ but ‘dehydration’.

(as it is, it is a mix between data and concepts, leading to incorrect conclusions).

Response: We have modified the text, as suggested.

→ Lines 64–68 on page 2.

Lines 71-72: ‘0.5-2.5 GPa’ → WARNING: around 2 GPa of confining pressure.

Response: As the reviewer suggested, we have modified the text.

→ Lines 73–75 on page 3.

Line 72: ‘500-750°C’ → WARNING: in the pressure range from 1.5 to 2.5 GPa, chlorite becomes unstable at temperatures above 850°C (MASH system, e.g. Staudigel & Schreyer, 1977). In contrast chlorite-bearing parageneses start destabilizing around 720-750°C (see review Ferrand, 2019a). This certainly explains why only ‘partial’ dehydration is observed in the experiments, and it should definitely be discussed in the present paper. → Major comment #2.

Response: Per the reviewer’s suggestion, we have added a discussion on the partial dehydration of chlorite, as mentioned above.

→ Lines 126–129 on page 5.

Lines 73-75 → This is doubtful because it would contradict all the experimentally-deduced databases (review by Ferrand, 2019a). Pseudosections are supposed to be based on experimentally deduced stability fields within internally-consistent databases, but should be used with care in order to avoid misinterpretations. → See major comment #3.

In addition, if the authors really want to ‘discredit’ serpentine minerals, they should perform the same calculations on serpentine minerals, notably the various antigorites exhibiting significant shifts in maximum temperature limits with varying compositions.

Response: We acknowledge the possibility of inconsistency between theoretical pseudosection findings and our experimental results. To address this concern, we have made appropriate textual modifications to elucidate the limitations of pseudosections. For detailed information regarding the antigorite pseudosection and its absence in our samples, we kindly refer you to our responses provided in response to Major Comment #3.

→ Line 75 on page 3.

→ Lines 122–129 on page 5.

Line 77 → Is vesuvianite really stable at pressures around 2 GPa? Are there any observations of it in this study of previous studies? → clarification needed.

Response: Unfortunately, minor minerals such as vesuvianite could not be observed because its expected volume percentage was too low (~0.5%).

→ Lines 80–82 on page 3.

Line 80 → Very interesting observation. → It would be useful to discuss the shape and distribution of the fluid inclusions depending on the sign of the volume change (e.g. positive at 1 GPa and negative at 2.5 GPa).

Response: According to the reviewer's suggestion, we have added relevant text regarding fluid trails and volume change: there was no difference.

→ Lines 85–86 on page 3.

Line 88: 'In samples without faults' → 'In samples in which no fault could be observed'. Indeed, the risk to miss 2D structures on a polished surface from a 3D volume is high… → to be clarified.

Response: Per the reviewer's suggestion, we have modified the text and added a discussion.

→ Line 93 on page 3.

→ Line 122–129 on page 5.

Line 93: 'a fault gouge that developed at 2.5 GPa and 750 °C' → NB: Chlorite is stable in these conditions, and if T increases a bit it would destabilize with a significantly negative volume change, incompatible with 'dehydration embrittlement'. → Major comment #1.

Response: Yes, if T increases a bit , chlorite will destabilise with a significant negative volume change.

Line 96: 'Ca was distributed along the fault' → How should that be interpreted? Was there enough diffusion at grain boundaries for large cations to find their way during the dehydration? Or was dynamic melt required? This chemical mobility issue should at least be mentioned. → Major comment #3.

Response: We found pocketed glassy materials (melt) between fine grains of olivine near faults, which could explain the rapid diffusion of large cations (Ca). We have modified the text and added a figure to show the existence of glassy materials (Supplementary Fig. 5).

→ Supplementary Fig. 5.

→ Lines 148–159 on page 6.

→ Line 232–233 on page 9.

Line 99 → agreed.

Lines 105-106 → The stress-strain curves should be shown for the entire experimental series!! Not data should be hidden. Everything is potentially interesting, including details that the authors have not thought about yet, and it is important that the raw data are accessible to the community.

Response: As the reviewer suggested, we have added stress-strain curves of all samples.

→ Supplementary Fig. 2.

→ Lines 110–115 on page 4.

Line 106: ‘to observe frictional behaviour’ → What does that mean? Such concept-based wording can be confusing for part of the community. A more factual description of the deformation data is better.

Response: We have removed the vague expression as per the reviewer’s suggestion.

→ Lines 110–111 on page 4.

Line 108: ‘showed episodic stick-slip behaviours with a stress …’ → ‘showed an episodic stick-slip behaviour with stress …’

Response: We have modified the text for the description of stress-strain curves of samples.

→ Lines 110–115 on page 4.

Line 109: ‘which is a characteristic of a seismic fault’ → ‘which is characteristic of seismic faults’

Response: As the reviewer suggested, we have modified the text.

→ Lines 114–115 on page 4.

Line 113: ‘partial dehydration products’ → Major comment #2.

Response: According to the reviewer’s suggestion, we have added a discussion on the partial dehydration of chlorite.

→ Lines 122–129 on page 5.

Line 114 → a paragraph about volume change should be added here, in order to comment on this parameter in the discussion.

Response: Per the reviewer's suggestion, we have added a discussion regarding volume change.

→ Table 1.

→ Lines 85–86 on page 3.

→ Lines 134–147 on pages 5–6.

Line 115 → For the sake of clarity, I would separate the discussion from the conclusions.

Response: We have separated the discussion from the conclusions as per the reviewer's suggestion.

Line 119: 'Kuriles' → 'Kuriles, as well as within remnants of subducted slabs (Ferrand & Manea, 2021; Craiu et al., 2022)'. For the sake of clarity, I would separate the discussion from the conclusions.

Response: We have modified the text and separated the discussion from the conclusions.

→ Line 171 on page 7.

Line 120: '200 km' → a range would be better.

Response: We have modified the text.

→ Line 173 on page 7.

Line 126: ‘dehydration embrittlement’ → ‘dehydration’

Response: We have modified the text from ‘dehydration embrittlement’ to ‘dehydration’.

→ Line 178 on page 7.

Lines 127-127 → The significant scientific achievements of the last decade should be fairly acknowledged. The manuscript as it is, it is not acceptable. The proposed mechanisms should be listed, including dehydration embrittlement (fluid overpressure) and DDST/TDST. → Major comments #1.

Response: We have added and acknowledged the scientific achievements of the last decade including DDST/TDST.

→ Lines 180–183 on page 7.

Line 131 → remove ‘embrittlement’

Response: We have removed ‘embrittlement’.

→ Line 190 on page 7.

Lines 129-131 → NB: Mineral dehydrations are demonstrably associated with both warm and cold subduction zones (e.g. Abers et al., 2003; Ferrand, 2019a).

Response: We have modified the text.

→ Lines 186–187 and 191–192 on pages 7–8.

Lines 135-136 → Have these studies really proposed that the ‘dehydration embrittlement’ of chlorite would be at play or have they proposed that the ‘dehydration’ of chlorite would play a role? This nuance is very important!!

Response: Dehydration involving mechanisms, including dehydration embrittlement and DDST/TDST could be possibly responsible for intermediate-depth seismicity. Dehydration embrittlement at negative volume change was experimentally demonstrated by Jung, Green and Dobrzhinetskaya (2004) at pressures up to 6 GPa (please see answers to the Major Comments #1). To avoid confusion, we have modified the text from 'dehydration embrittlement' to 'dehydration'.

→ Line 190 on page 7.

Line 138: 'stable' → Indeed… Clinochlore (and most chlorite compositions) are expected to be stable in these conditions. → This is why the impact of thermal runaway processes (which can locally heat the rock by strain localization) should also be discussed (e.g. Thielmann et al., 2015; Thielmann, 2018; Kelemen & Hirth, 2007; Prakash et al., 2023). → Major comments #2.

Response: We have added a discussion on thermal runaway as a rupture mechanism.

→ Lines 45–47 on pages 1–2.

→ Lines 160–167 on page 6.

Lines 138-143 → The paragraph is oriented in a way that is not supported by the literature. Antigorite is demonstrably the mineral that shows the best correlation between destabilization and seismicity (Ferrand, 2019a; 2019b; Manea & Ferrand, 2021).

Response: We have modified the text to address serpentine as the source of the lower-plane seismicity at low temperature and stated chlorite as an additional possible mineral to trigger lower-plane seismicity at high temperature (please see answers to the Major Comments #2).

→ Line 199 on page 8.

Line 144 → The Fig.1 is not showing anything clear enough that would support such a statement.

Response: We have removed the vague expression, as suggested.

→ Line 199 on page 8.

Line 145 → Demonstrably incorrect.

Response: We have removed the text, as suggested.

Line 146 → remove 'embrittlement'.

Response: We have removed 'embrittlement'.

→ Line 201 on page 8.

Line 149: '~30% chlorite and ~1% dehydration product.' → This is interesting. Such small amounts of destabilized minerals would then be incompatible with significant fluid production, and thus the 'dehydration embrittlement' concept would likely not apply at all. The DDST is more likely. → Major comments #1. + I suggest to discuss also the possibility of thermal runaway instabilities triggering local dehydration (Kelemen & Hirth, 2007; Magott et al., 2020) → Major comments #4

Response: We have added a discussion on thermal runaway instability triggering a local dehydration.

→ Lines 160–167 on page 6.

Lines 149-150 → This is a theoretical estimate, that has not been confirmed by natural petrology, so some discussion is required.

Response: We have added a discussion on chlorite content in natural upper mantle peridotite.

→ Lines 205 and 206–215 on page 8.

Lines 150-152 → To be rewritten considering all comments.

Response: We have rewritten the sentence.

→ Lines 206–215 on page 8.

Lines 153-155 → no → remove ‘for the dehydration embrittlement of chlorite peridotite’ and add a sentence recalling that ‘Chlorites (e.g. clinochlore) are among the mineral phases identified as likely involved in seismicity upon dehydration.’

Response: We have modified the text.

→ Lines 216–218 on page 9.

Line 157 → add references to Brudzinski et al. (2007) and Ferrand (2019a).

Response: We have added the references.

→ Lines 221 on page 8.

Line 158 → remove ‘embrittlement’.

Response: We have removed ‘embrittlement’.

Lines 157-159 → vague easy sentence.

Response: We have removed the vague sentence.

Line 160: ‘which is likely responsible for the lower plane of the double seismic zone’ →

incorrect.

Response: We have removed the sentence, as suggested.

Line 161 → remove ‘may’ (see review by Ferrand, 2019a).

Response: We have removed ‘may’.

Lines 162-163 → vague. NB: the distance between the plane is fully explained in publications (e.g. Ferrand, 2019a; Florez & Prieto, 2019). It corresponds to the fossilized trace of the brittle-ductile transition (Ranero et al., 2003), and not only one mineral can destabilize and trigger seismicity there (although serpentine minerals are leading at depths up to 100 km; Ferrand, 2019a).

Response: We have removed the vague sentence and added the references.

→ Line 221 on page 8.

Lines 167-168: ‘…°C) in a representative subduction zone’ → ‘…°C), representative of subduction zones’

Response: We have modified the text.

→ Line 231 on page 9.

Line 168: ‘a dehydration product (Ca-amphibole)’ → Not simply a dehydration product of chlorite, but of the chlorite peridotite paragenesis (Ca-bearing). The mobility of Ca should be discussed.

Response: We have modified the text and discussed the mobility of Ca.

→ Lines 148–159 on page 6.

Line 169: 'partial dehydration' → See major comment #3.

Response: We have added a discussion on the partial dehydration of chlorite.

→ Lines 126–129 on page 5.

Lines 170-173 → vague and disappointing, although the experimental results are significant and meaningful…

Response: We have removed the vague sentence and modified the text to address serpentine dehydration as a source of lower-plane seismicity at low temperature. We have added a discussion on stress transfer models.

→ Lines 233–236 on page 9.

Table 1 → The meaning of 'x' and 'o' should be explicitly written, so that the reader does not get lost.

Response: We have added a footnote for 'x' and 'o' in Table 1.

→ Table 1.

Fig.1 → the reference to Hacker et al. (2003) should come after 'serpentine' (line 310).

Response: We have modified the text in the caption of Figure 1.

→ Lines 531–532 on page 17.

Responses to the comments of Reviewer #2

1. The main question is that what are the main differences between the previous studies of lab experiments and this work. I suggest the authors should state it clearly in the Introduction or Discussion section, and point out the Highlights or new findings of the present study.

Response: We have described the purpose of this investigation and its difference from previous studies in both the Introduction and Conclusions sections.

→ Lines 64–68 on page 2.

→ Lines 233–236 on page 9.

2. In the summary, I suggest the authors should show the values for pressure and temperature, e.g., line 24 "at high pressures (~1.5-5 GPa?)" , line 31 "high pressure (1.5-2.5 GPa) and high temperature (650-750 °C)". Lines 24-26, 46-49, 125-128, 169-171 and so on.

Response: We have showed values for pressure and temperature.

→ Lines 24, 29, 30–31 on page 1.

→ Line 54 on page 2.

→ Line 223 on page 9.

3. The authors suggested the upper-plane intermediate-depth seismicity of the double seismic zone (DSZ) is attributed to the dehydration embrittlement of hydrous minerals such as serpentine and lawsonite. However, it confused me. I think the popular hypothesis of dehydration embrittlement is that the upper-plane intermediate-depth seismicity is possibly linked to brittle failure associated with dehydration reactions of hydrous minerals (e.g. gabbro, lawsonite, etc, but not serpentine) in the slab crust, and the dehydration reactions of hydrous minerals (e.g. antigorite, clinochlore, chlorite, etc) in the slab mantle is possibly associated with the lower-plane intermediate-depth seismicity. Is it right?

Response: Upper-plane seismicity is observed in both subducting oceanic crust and the upper mantle of slab (Fig. 1). The upper-plane intermediate-depth seismicity is possibly linked to brittle failure associated with dehydration reactions of hydrous minerals (e.g., lawsonite in the slab crust and serpentine and other hydrous minerals in the slab mantle). In this study, we showed that both upper- and lower-plane intermediate-depth seismicity can be linked to the dehydration of chlorite at high temperature conditions.

→ Lines 48–49 on page 2.

→ Lines 179–183 on page 7.

4. Lines 134-137, what are reasons for no evidence supporting the hypothesis? Is it very difficult for previous studies to conducted deformation experiments on chlorite under a high P and high T condition or not? Please state it clear.

Response: Conducting rock deformation tests under high P-T conditions is difficult. A modified Griggs apparatus established at Seoul National University allowed us to conduct high P-T deformation investigations with chlorite peridotite, which was previously impossible.

→ Lines 192–193 on page 8.

5. For the figures, I suggested the authors should list the values of the pressure and temperature both in figure and figure caption.

Response: We have added the P-T values of samples both in the figures and figure captions.

→ Figs. 2–4; Supplementary Fig. 5.

6. Lines 105-109, as shown in Extended Data Fig. 3, (a) denotes the stress-strain curve for a unfaulted sample, while (b) and (c) denote the stress-strain curves for the faulted samples. The authors should make it clear in the text. However, it seems that (a) and (b) show similar styles but are different from the style of (c), why? And how to get the stress drop of ~30-50+-20 MPa by the figure?

Response: Another reviewer suggested showing stress-strain curves for all samples; therefore, Extended Data Fig. 3 has been modified and presented in Supplementary Fig. 2. The description of stress-strain curves has been modified. Faulted samples showed a large stress drop after peak stress was reached.

→ Lines 110–115 on page 4.

→ Supplementary Fig. 2.

7. Line 119 “20-40 km” should be “~20-40 km”, line 120 “of 200 km” should be “of ~200 km”, line 123 “of 150 km” should be “of ~150 km”.

Response: We have modified the text.

→ Lines 172, 173, 176 on page 7.

References

Jung, H., Green, H.W., Dobrzhinetskaya, L.F., 2004. Intermediate-depth earthquake faulting by dehydration embrittlement with negative volume change. Nature 428, 545-549.

Mirwald, P.W., Getting, I.C., Kennedy, G.C., 1975. Low-friction cell for piston-cylinder high-pressure apparatus. Journal of Geophysical Research 80, 1519-1525.

24th Oct 23

Dear Professor Jung,

Your manuscript titled "Impact of dehydration on chlorite and intermediate-depth earthquakes" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

Please note that it may still be possible for your paper to be published before the end of 2023, but in order to do this we will need you to address these points as quickly as possible so that we can move forward with your paper.

EDITORIAL REQUESTS:

We note that the words 'such as serpentine' on line 199, appear to be out of place. Please check this.

Please review our specific editorial comments and requests regarding your manuscript in the attached "Editorial Requests Table".

*****Please take care to match our formatting and policy requirements. We will check revised manuscript and return manuscripts that do not comply. Such requests will lead to delays. *****

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If you have any questions or concerns about any of our requests, please do not hesitate to contact me.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Sylvain Barbot
Editorial Board Member
Communications Earth & Environment

Joe Aslin
Senior Editor,
Communications Earth & Environment
https://www.nature.com/commsenv/
Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

Dear authors,

Please find attached the review file.
I acknowledge the revisions, but list several remaining issues that should be addressed before publication.

Best wishes for the last revisions,
Thomas P. Ferrand

communications earth & environment – manuscript COMMSENV-23-0561-T

Impact of dehydration on chlorite and intermediate-depth earthquakes

Letter to editor and review file – Dr. Thomas P. Ferrand

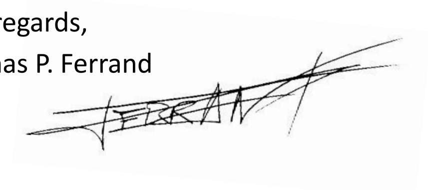
Dear CEE editorial team,

Thank you very much for contacting me to review this manuscript, and sorry for the delay.

I have read the manuscript and I can now provide a detailed review. As stated during the first round of review, this is a good scientific contribution because of the quality of the experimental series and the quality of the presentation of the data. The revised version of the manuscript is much better on many aspects, and very close to publishable. Nonetheless, some things remain to be solved, which includes some imprecisions on wording, concepts, or argumentation, as I have tried to explain in details in the following *Line-by-line comments*.

This review is made for the authors to be able to easily and rapidly fix all the little issues that remain. Especially, I have highlighted some warnings that, in my opinion, must be understood and answered by proper clarifications of the manuscript. I am sure that this paper will be well cited and I hope that you and the authors will consider my input as useful.

Best regards,
Thomas P. Ferrand



Major comments → **See line-by-line comments for details.**

- Several sentences remain that suggest that the upper Wadati-Benioff plane would be linked to serpentine dehydration while the lower plane would be due to chlorite dehydration, which is demonstrably incorrect.
- I acknowledge the much better referencing to the literature in the revised manuscript, but noticed that the worlds "suggest" and "speculate" are used, which sounds negative and somehow disrespectful towards the enormous efforts that have been made during the past decades.
- The authors describe a paper (Ferrand, 2019, Lithos) as pro-serpentine, although this paper was specifically written to explain the exact contrary: i.e. that, even though serpentine instability correlates with most intermediate-depth seismicity, it cannot explain alone its entire distribution. It is good to cite the relevant literature on the topic, but it should be made with caution to avoid misleading referencing. I recall that the link between chlorite (clinochlore) and intermediate-depth seismicity is neither new, nor surprizing. Instead, the authors could insist about the fact that even local/minor reactions can trigger extreme strain localization during deformation experiments.
- The authors explain that they could not observe vesuvianite but consider that it exists because it is predicted by thermodynamics. And they explain that they could not observe serpentine, which would demonstrate, according to them, that it does not exist (even in small amounts) in their samples. All of this does not sound serious and could weaken the paper. Instead, the authors should explain that they make a choice in the reaction they believe is the cause of the reaction-induced strain localization. This would be a reasonable interpretation, with which I could agree, but not a full demonstration… This is a serious issue.
- There is a problem in the interpretation of the formation of fluid inclusions trails, which are not the cause but the consequence of mode-I cracking… Similarly, there is a problem in the description of delamination of a chlorite crystal, which usually happens on chlorite basal plane and would certainly not record sigma 1 due to its highly anisotropic nature.
- There is still a problem regarding the "dehydration embrittlement" definition, which was initially described as due to dehydration-induced fluid overpressure (i.e. hydrofracking). Here I want to

insist about the fact that Jung et al. did not demonstrate that the 'dehydration embrittlement' mechanism would work at pressures above 2 GPa, but that dehydration-induced seismicity is also possible at such pressures (with negative volume changes), which has then be used to explain that the 'dehydration embrittlement' cannot be the only mechanism for intermediate-depth earthquakes, which actually made me formulate the DDST/TDST models.

- There are several wording/syntax issues, and clarifications to be made.

Line-by-line comments

Line 23 → To avoid repetition of "zones", I suggest to write "double seismic structures", for example.
Line 24 → The lower Wadati-Benioff plane can start around 1-1.2 GPa and merges with the upper plane around 4 GPa. Why 1.5-5.0?
Lines 25-27 → Once again, the upper plane is not only due to serpentine, but can be due to lawsonite and lots of other minerals, as the authors recall in the main text. Similarly, the lower plane is likely due to serpentine but also to chlorite and other minerals, as clearly explained in previous studies. This is known... This sentence is problematic because it still suggests that the upper plane would be associated with serpentine and the lower plane with chlorite, which is demonstrably incorrect.
Line 34: "suggest" → This word is not appropriate, because the correlation is already well established (e.g. Ferrand, 2019, and references therein). Here I would rather write: "Our findings confirm, together with correlation studies between seismicity and mineral stability, that a significant part of intermediate-depth seismicity in the lower plane of double seismic zones can be attributed to chlorite dehydration.
Line 48: "of upper mantle" → "in the upper mantle"
Line 49: "of subducting oceanic crust" → "in the subducting oceanic crust"
Line 49: "a" → "as a"
Line 50 → Here ref. 17, 18 and 19 could be added, and ref. 32 is not exactly adapted for this sentence.
Line 59: "speculate" → At this stage, this is more than "speculation"... → I would adapt the sentence as follows: "Based on laboratory experiments and seismicity correlation studies, researchers have inferred that seismicity in the lower plane of double seismic zones could be linked to the dehydration of hydrous minerals, including serpentine and chlorite."
Line 66 → Ref.17 (Ferrand, 2019) is cited here to claim the contrary of what it argued. Contrary to what the authors suggest here, ref.17 reviewed all possible hydrous and anhydrous phases that could be involved in TDST, including chlorite, and did not specifically favor serpentine minerals... The correlation of most seismicity of double-seismic zones with the instability of serpentine minerals is a fact that strongly support a causal link. In addition, ref.32 (Ferrand & Manea, 2021) also considered chlorite...
Lines 66-68 → Already highlighted by correlation studies between seismicity and thermal structure (Abers et al. 2013, Ferrand 2019 = ref.1 and ref.17). → I urge the authors to rephrase the paper in order to keep the story as honest as possible.
Line 72: "Fourteen experiments were conducted using simple shear" → "Fourteen simple-shear experiments were conducted"
Lines 79-80: formula → again, I do not understand why the authors consider such a complex equation. Are they sure about the presence of vesuvianite and Na-phlogopite? Is it possible to specify the amphibole? Is it systematically tremolite? Could there be cummingtonite or tschermakite for example?

> NB: Regarding the stability of clinochlore, I still wonder why the authors de not refer to the key references on the topic (i.e. Staudigel & Schreyer, 1977; Pawley, 2003), and prefer keeping fixed to a complicated multi-component equation involving mineral phases that they have not been able to observe. I understand the thermodynamic modelling argument, but I am afraid many readers will not feel satisfied by this interpretative side of the paper.

Line 81: "vesuvianite were difficult to observe" → **But were they observed at all??** → If no, the authors should provide a better argumentation to support the decision they make, and be very clear about what observations are lacking. Importantly, the way the authors rule out the possible presence of minor fractions of serpentine minerals (**lines 129-131**) is totally inconsistent with the way they claim for the existence of minor (non-observable) amounts of vesuvianite… → This is weakening the manuscript, and could be solved by a be

<u>NB:</u> During the first round of review, I was surprised by the importance of vesuvianite in the paper, although it is not documented in the starting material. But I would buy it if the authors add a reference about its stability at such P-T conditions. I found one in the literature: Dey et al. (2019). The authors might find it useful to better argue in favor of their interpretation.

Line 82: "Of" → "In"
Line 83 → remove "in all of them"
Line 86: "absent" → "not observed"
Line 87: "are evidence" → "are interpreted as evidence"

<u>NB:</u> The authors can indeed consider these fluid inclusions as 'evidence' of dehydration during deformation only after presenting the detailed observations of **lines 88-92**.

Lines 91-92 → **WARINING**: Here there is a critical mistake in the interpretation. First, fluid inclusions do not form anything; they are what remains after coeval dehydration and deformation. Second, the presence of fluid, together with elastic strain can trigger mode-I cracks subparallel to sigma 1, indeed, but only in case of fluid saturation (expected upon chlorite dehydration). → I request that the authors correct the observational mistake and invert the sentence in order to present these results in a physically logical manner.

<u>NB:</u> Under non-negligible confining pressures, cracks tend to form about 30-40° from sigma1 in the absence of pressurized fluids (fluid saturation conditions locally expected upon dehydration with a positive Clapeyron slope), and this angle can decrease to about 0-10°C in case of fluid pressurization (positive Clapeyron slope), which, obviously, is not the case here. In addition, the fluid inclusion trails are about 30° from sigma 1... which is expected in the absence of pressurized fluids, consistently with the negative Clapeyron slope.

Lines 94-95: "and their sheets were aligned at a slight angle with respect to their shear direction" → **WARNING**: the delamination of the documented chlorite crystal (Fig.3a) occurred along its basal plane... which orientation is mostly due to the initial orientation of the crystal, not to sigma1... The authors could write instead that the deformation can be accommodated by shearing along the basal plane of well-oriented chlorite crystals.
Line 97: "The" → "A"
Lines 97-98 → remove "of the chlorite peridotite"
Lines 101-102: "optical photomicrograph" → This image was produced using an optical microscope? or secondary electron imaging (SE2)? → clarification needed.
Lines 104-105: "which was not observed in unfaulted samples" → This part of the sentence is useless.
Line 107: "micro analyser" → "microanalyser"
Line 113: "after peak-stress reached" → "after peak stress"
Line 115: "characteristic of seismic faults." → **WARNING**: Macroscopic stress drops in deformation experiments are characteristic of sample-scale faulting, but it is does not tell anything about the seismic or aseismic nature of faulting events. Acoustic receivers are required to go further (e.g. Gasc et al., 2011, 2017; 2022; Schubnel et al., 2013; Incel et al., 2017; Ferrand et al., 2017). For instance, in our previous study (Ferrand et al., 2017), a macroscopic stress drop is observed upon dehydration-induced aseismic deformation (50% of serpentine) but no sample-scale stress drop is observed in seismogenic samples (5% of serpentine).
Line 119: "fault" → "faults"
Line 119: "a partial dehydration product" → "one of the dehydration products"
Line 119 → The authors could add a line about the observed differences for positive and negative volume changes... It is a very important topic, and we need to highlight such observations, because they are very important for numerical modelling and tectonic-scale interpretations (e.g. Scambelluri et al., 2019; Craiu et al., 2022).
Line 124: "fluid" → "fluid inclusions"
Line 125: "the dehydration product" → "a well identifiable dehydration product"

Line 125: “Results” → “Our results”
Line 126: “triggers” → “likely contributes to”
Lines 127-128: “the temperature conditions of chlorite to be destabilized” → The sentence is not well written + There is a conceptual issue. In a purely chemical sense, the reason for “partial” dehydration is because the other reactive phases are in low fractions. Thus, most chlorite remains stable in the end. A chemist would say that chlorite is in excess compared to other reactive species in the studied material. The studied reaction does not correspond to the actual destabilization of chlorite…
Line 128: “complete instability” → “actual instability”
Lines 129-131 → Here the authors claim that there was/is no serpentine in their samples because they could not observe it. But they could not observe vesuvianite or phlogopite either, so this argument sounds totally unreasonable…
Lines 134-135: “Conventionally, dehydration embrittlement […]” → **WARNING:** It is no convention! Dehydration embrittlement is a mechanism based on fluid overpressure. This is the definition. The authors should admit that the 'dehydration embrittlement’ model is not appropriate at high pressures, neither for serpentine nor for chlorite.
Lines 135-137 → **Incorrect.** As clearly explained by Ferrand 2019, negative volumes changes imply impossible hydrofracking!
Lines 139-143 → These sentences are correct, and make the previous sentences (**lines 129-137**) sound a bit odd…
Line 143 → remove “conditions”
Line 145: “volume change conditions” makes no sense → replace with “volume changes”
Lines 150-152 → repetition of "alternatively" and "alternative"
Line 155: “wave” → “waves”
Line 159: “supports the notion of” → “would be consistent with the”
Line 159: “rapid fast” → choose between “rapid” and “fast”
Lines 155-158 → The observation of amorphous material is important because it allows the authors to argue for dynamic melting, and thus for the seismic nature of the faulting event. But this does not demonstrate that all observed faults were formed by seismic events. Furthermore, whether the reported experimental faulting events are seismic or not is not really a problem. Seismic and aseismic faulting events both correspond to extreme localization of the deformation, which can lead to larger ruptures and is essential to understand regional seismic and aseismic events, from earthquakes to tremors and slow-slip events...
Lines 160-161 → This is correct, but the logical link with the next sentence is unclear. The authors should write that the presence of amphibole in faults documents coeval fluid percolation and faulting.
Line 179: “of upper mantle” → “of the upper mantle”
Line 179: “of subducting” → “of the subducting”
Lines 181-183 → again, ref.17 (Ferrand, 2019) does not only consider serpentine minerals but a vast diversity of hydrous and anhydrous minerals that could exist in subducting slabs under intermediate-depth conditions. And the same applies to the upper plane... → correction needed.
Lines 186-187: “under deeper depth” → “deeper” or “at larger depths” or “in deeper conditions”.
Lines 192-193 → incorrect: it is not more difficult than experiments on serpentine or other minerals...
Lines 195-199 → The sentence is too long and ends with a confuse double "such as". → I suggest to cut the sentence at least in 2. Especially, I do not understand what the authors mean by adding "such as serpentine" at the end of the sentence. → Clarification needed.
Line 200: “intermediate-depth earthquakes” → “most intermediate-depth earthquakes”

NB: the authors have not demonstrated that only these two mineral families are involved. Plenty of minerals can generate stress transfers upon transformation (Ferrand, 2019). I think about talc for instance, which stability also coincides with the studied P-T conditions. As far as we understand, what matters is the speed of the transformation and the elasticity of the mineral and surrounding materials.

Line 203: “~31% chlorite by volume” → “~31 vol.% of chlorite”

Lines 204-205: "∼30% chlorite and ∼1% dehydration product." → "∼30 vol.% of chlorite and ∼1% of dehydration products"
Lines 205-206: "10-35% chlorite volume percentage" → "a fraction of 10-35 vol.%"
Line 211: "reported ultrahigh-pressure […]" → "reported an ultrahigh-pressure […]"
Lines 216-217 → This sentence should be in the introduction…
Line 219: "subducting" → "subduction"
Line 223: "the upper (~600 °C) and lower (~700 °C) planes" → This over-simplification is not reasonable. This would make non-specialists readers believe that the upper plane always around 600°C and the lower plane around 700°C, which is demonstrably incorrect (e.g. Ferrand, 2019, and ref. therein).
Line 225 → remove "global" or rephrase to avoid confusion.
Line 225: "However," → "In contrast,"
Lines 231-233: "A fault was developed where a dehydration product (Ca-amphibole) and a melt of the chlorite peridotite were observed" → Very confused sentence → I suggest to rephrase as follows: "Faults developed and are filled with a dehydration product (Ca-amphibole) and an amorphous material suggesting dynamic melting of the chlorite peridotite".

<u>NB:</u> it is very important not to write "melt" to described a glass. A melt is liquid.

Lines 233-236: "The result supports dehydration involving mechanisms (dehydration embrittlement and DDST/TDST) that are applicable for not only well-known serpentine but also chlorite to explain intermediate-depth earthquakes of subducting slabs." → suggestion for better grammar and fluidity: "The results support that dehydration-induced faulting mechanisms (e.g. dehydration embrittlement or DDST/TDST) are applicable to not only well-known serpentine minerals, but also to chlorite to explain intermediate-depth earthquakes within subducting slabs."
Line 241: "used as the starting material." → "used as starting material."
Line 245: "micro analyser (EPMA)." → "microanalyser (EPMA)"
Line 248: "Nickel foil" → "A nickel foil"
Line 270: "controlled constantly" → "constantly controlled"
Lines 274-276 → repetitive with **lines 258-260**. → correction needed.
Line 281: "decreasing the pressure" → "the pressure was decreased"
Lines 286-289 → For the sake of clarity, the authors could add: "using an internally consistent dataset built from our up-to-date knowledge on mineral phases."

<u>NB:</u> This is a big assumption. Using such thermodynamic models means that we assume that all potentially stable mineral phases are known in the studies system, the overall chemistry of which is also somehow uncertain.

Line 291: "assuming a water saturated condition" → "assuming water saturation"

<u>NB:</u> Water saturation is a strong assumption. This is risky and should be discussed in the discussion, especially in the paragraph about volume changes.

Line 295: "solid solutions" → I suggest to rephrase this, because there is a confusion with the concept of solid solution. → suggestion: "The mineral phases considered in the model were…"

<u>NB:</u> Thermodynamically speaking, a solid solution is a mixture of two minerals forming an homogeneous crystal. This is not what the authors mean at **line 295**.

Line 302: "using field-emission …" → "using a field-emission …"
Line 305: "accelerating" → "acceleration"
Line 310: "current probe" → "probe current" (?)

Line 529 (Table 1 caption): "of chlorite" → "on chlorite"
Line 536 (Fig.1 caption): "match only" → "only match"
Line 538 (Fig.2 caption) → It is unclear what "36" and "33" mean on the figure. → add to caption.
Line 547 (Fig.2 caption): "a Mode-I crack along the fluid inclusion trail" → "a Mode-I crack that left a fluid inclusion trail"
Lines 556-557 (Fig.4 caption): "optical photomicrograph" → Only electron microscopy is mentioned in the Methods. → confirmation needed.

Line 573 (Fig.S1): “Nickel foil” → “A nickel foil”
Line 575 (Fig.S1): “stress” → “stresses”
Line 577 (Fig.S2) → remove “curve”
Line 577 (Fig.S5): “(a melt)” → “(most likely a solidified/quenched melt)”

Fig.1 → In Fig.1a, it appears that most lower-plane earthquakes occur in the stability field of chlorite peridotite... The authors should make it clear (maybe into parentheses) that the instability of chlorite would explain only a fraction of this seismicity (around the transition between the yellow and red area zones). → Maybe a dashed line could be added on the figure to help the reader?
Apart from that, I would say that these new **lines 51-55** are much clearer and more accurate than in the previous version of the manuscript.

Fig.4 → Interesting observation without description/discussion: Fig.4c and Fig.4f have almost the same scale, but exhibit different microstructure. At 2.5 GPa (Fig.4c), the Claperyon slope is negative and thus reaction products can easily form within a relatively thick fault gouge (about 10 µm). At 1.5 GPa (Fig.4f), the Clapeyron slope is slightly positive and the fault is much thinner (<1µm).

Fig.S2 → Problem to distinguish JH127 and JH228 → color adjustment required.

References used in the review file:

- Abers, G.A., Nakajima, J., van Keken, P.E., Kita, S. & Hacker, B.R. (2013). Thermal-petrological controls on the location of earthquakes within subducting plates. *EPSL* **369**, 178–187.
- Craiu, A., Ferrand, T. P., Manea, E. F., Vrijmoed, J. C. & Mărmureanu, A. (2022). A switch from horizontal compression to vertical extension in the Vrancea slab explained by the volume reduction of serpentine dehydration. *Scientific Reports* **12**(1), 22320.
- Dey, A., Choudhury, S. R., Mukherjee, S. … & Sengupta, P. (2019). Origin of vesuvianite-garnet veins in calc-silicate rocks from part of the Chotanagpur Granite Gneiss Complex, East Indian Shield: the quantitative PTX CO_2 topology in parts of the system CaO-MgO-Al_2O_3-SiO_2-H_2O-CO_2 (+ Fe_2O_3, F). *American Mineralogist* **104**(5), 744-760.
- Ferrand, T. P. & Manea, E. F. (2021). Dehydration-induced earthquakes identified in a subducted oceanic slab beneath Vrancea, Romania. *Scientific Reports* **11**(1), 10315.
- Ferrand, T. P. (2019). Seismicity and mineral destabilizations in the subducting mantle up to 6 GPa, 200 km depth. *Lithos* **334**, 205-230.
- Ferrand, T. P., Hilairet, N., Incel, S., Deldicque, D., Labrousse, L., Gasc, J., ... & Schubnel, A. (2017). Dehydration-driven stress transfer triggers intermediate-depth earthquakes. *Nature communications* **8**(1), 15247.
- Gasc, J., Daigre, C., Moarefvand, A., Deldicque, D., Fauconnier, J., Gardonio, B., ... & Schubnel, A. (2022). Deep-focus earthquakes: From high-temperature experiments to cold slabs. *Geology* **50**(9), 1018-1022.
- Gasc, J., Hilairet, N., Yu, T., Ferrand, T., Schubnel, A. & Wang, Y. (2017). Faulting of natural serpentinite: Implications for intermediate-depth seismicity. *EPSL* **474**, 138-147.
- Gasc, J., Schubnel, A., Brunet, F., Guillon, S., Mueller, H. J. & Lathe, C. (2011). Simultaneous acoustic emissions monitoring and synchrotron X-ray diffraction at high pressure and temperature: Calibration and application to serpentinite dehydration. *PEPI* **189**(3-4), 121-133.
- Incel, S., Hilairet, N., Labrousse, L., John, T., Deldicque, D., Ferrand, T., ... & Schubnel, A. (2017). Laboratory earthquakes triggered during eclogitization of lawsonite-bearing blueschist. *EPSL* **459**, 320-331.
- Pawley, A., 2003. Chlorite stability in mantle peridotite: the reaction clinochlore + enstatite = forsterite + pyrope + H2O. *Contributions to Mineralogy & Petrology* **144** (4), 449-456.
- Scambelluri, M., Cannaò, E. & Gilio, M. (2019). The water and fluid-mobile element cycles during serpentinite subduction. A review. *European Journal of Mineralogy* **31**(3), 405-428.
- Schubnel, A., Brunet, F., Hilairet, N., Gasc, J., Wang, Y. & Green, H. W. (2013). Deep-focus earthquake analogs recorded at high pressure and temperature in the laboratory. *Science* **341**(6152), 1377-1380.
- Staudigel, H., Schreyer, W., 1977. The upper thermal stability of clinochlore, $Mg_5Al[AlSi_3O_{10}](OH)_8$, at 10-35 kb PH_2O. *Contributions to Mineralogy & Petrology* **61** (2), 187-198.

Reviewer #2 (Remarks to the Author):

Revised form of the study is now acceptable for the publication.

Response to the Reviews

Thank you for allowing us to submit a revised version of our manuscript titled “Impact of dehydration on chlorite and intermediate-depth earthquakes to *Communications Earth & Environment*. We appreciate the time and effort of the reviewer have dedicated to providing insightful feedback on ways to strengthen our paper. Thus, it is with great pleasure that we resubmit our article for further consideration. Author responses are given in blue and changes to the manuscript are highlighted in yellow in the revised manuscript.

Responses to major comments on the pdf file of Reviewer #1

Major comments

Several sentences remain that suggest that the upper Wadati-Benioff plane would be linked to serpentine dehydration while the lower plane would be due to chlorite dehydration, which is demonstrably incorrect.

Response: We sincerely thank the reviewer’s helpful comments. The contribution of minerals on Wadati-Benioff plane is corrected in the text (see line-by-line comments and responses).

→ Lines 25–27 on page 1.

I acknowledge the much better referencing to the literature in the revised manuscript, but noticed that the worlds “suggest” and “speculate” are used, which sounds negative and somehow disrespectful towards the enormous efforts that have been made during the past decades.

Response: We modified the words (see line-by-line comments and responses).

→ Lines 34–36 on page 1.

→ Lines 59–62 on page 2.

The authors describe a paper (Ferrand, 2019, Lithos) as pro-serpentine, although this paper was specifically written to explain the exact contrary: i.e. that, even though serpentine instability correlates with most intermediate-depth seismicity, it cannot explain alone its entire distribution. It is good to cite the relevant literature on the topic, but it should be made with caution to avoid misleading referencing. I recall that the link between chlorite (clinochlore) and intermediate-depth seismicity is neither new, nor surprizing. Instead, the authors could insist about the fact that even local/minor reactions can trigger extreme strain localization during deformation experiments.

Response: We corrected the references as the reviewer suggested (see line-by-line comments and responses).

→ Line 67 on page 2.

→ Line 193 on page 7.

The authors explain that they could not observe vesuvianite but consider that it exists because it is predicted by thermodynamics. And they explain that they could not observe serpentine, which would demonstrate, according to them, that it does not exist (even in small amounts) in their samples. All of this does not sound serious and could weaken the paper. Instead, the authors should explain that they make a choice in the reaction they believe is the cause of the reaction-induced strain localization. This would be a reasonable interpretation, with which I could agree, but not a full demonstration… This is a serious issue.

Response: Unfortunately, we were unable to detect vesuvianite, as confirmed by BSE images and EDS mapping of the entire samples. We acknowledge the possibility of serpentine being present in the samples as a minor mineral, following the same logic. However, upon close examination using highly magnified (~2000x) EDS mapping in the faults, we found no serpentine. Therefore, we consider that serpentine likely does not play a significant role in faulting in this study, even if it can exist as a minor mineral in the samples. As the reviewer suggested, we have modified the text to reference Dey et al. (2019) regarding variable vesuvianite stability fields and provided an explanation for the possibility of serpentine being present as a minor mineral.

→ Lines 84–88 on page 3.

→ Lines 138–144 on pages 5–6.

There is a problem in the interpretation of the formation of fluid inclusions trails, which are not the cause but the consequence of mode-I cracking… Similarly, there is a problem in the description of delamination of a chlorite crystal, which usually happens on chlorite basal plane and would certainly not record sigma 1 due to its highly anisotropic nature.

Response: We have modified the sentence, mode-I cracks as the cause and fluid trails as the result. We also commented that fluid trails and cracks are up to 30° from σ_1.

→ Lines 95, 97, and 98 on page 4.

There is still a problem regarding the "dehydration embrittlement" definition, which was initially described as due to dehydration-induced fluid overpressure (i.e. hydrofracking). Here I want to insist about the fact that Jung et al. did not demonstrate that the 'dehydration embrittlement' mechanism would work at pressures above 2 GPa, but that dehydration-induced seismicity is also possible at such pressures (with negative volume changes), which has then be used to explain that the 'dehydration embrittlement' cannot be the only mechanism for intermediate-depth earthquakes, which actually made me formulate the DDST/TDST models.

Response: Jung et al. (2004) showed experimentally that dehydration of serpentine can make a fault at the pressures above 2 GPa. Yes, dehydration-induced seismicity with negative volume change is possible. We modified the word about dehydration-induced seismicity under negative volume change ($\Delta V < 0$) to avoid a confusion.

→ Lines 145–156 on page 6.

There are several wording/syntax issues, and clarifications to be made.

Response: We modified and clarified the texts following line-by-line comments.

<u>Responses to line-by-line comments on the pdf file of Reviewer #1</u>

Line 23 → To avoid repetition of "zones", I suggest to write "double seismic structures", for example.

Response: We have modified the text as the reviewer suggested.

→ Line 23 on page 1.

Line 24 → The lower Wadati-Benioff plane can start around 1-1.2 GPa and merges with the upper plane around 4 GPa. Why 1.5-5.0?

Response: We have modified the text as the reviewer suggested.

→ Line 24 on page 1.

Lines 25-27 → Once again, the upper plane is not only due to serpentine, but can be due to lawsonite and lots of other minerals, as the authors recall in the main text. Similarly, the lower plane is likely due to serpentine but also to chlorite and other minerals, as clearly explained in previous studies. This is known... This sentence is problematic because it still suggests that the upper plane would be associated with serpentine and the lower plane with chlorite, which is demonstrably incorrect.

Response: We have modified the text to avoid a confusion.

→ Lines 25–27 on page 1.

Line 34: “suggest” → This word is not appropriate, because the correlation is already well established (e.g. Ferrand, 2019, and references therein). Here I would rather write: "Our findings confirm, together with correlation studies between seismicity and mineral stability, that a significant part of intermediate-depth seismicity in the lower plane of double seismic zones can be attributed to chlorite dehydration.

Response: We have modified the sentence as the reviewer suggested.

→ Lines 34–36 on page 1.

Line 48: “of upper mantle” → “in the upper mantle”

Response: We have modified the text as the reviewer suggested.

→ Line 48 on page 2.

Line 49: “of subducting oceanic crust” → “in the subducting oceanic crust”

Response: We have modified the text as the reviewer suggested.

→ Line 49 on page 2.

Line 49: “a” → “as a”

Response: We have modified the text as the reviewer suggested.

→ Line 49 on page 2.

Line 50 → Here ref. 17, 18 and 19 could be added, and ref. 32 is not exactly adapted for this sentence.

Response: We have modified references as the reviewer suggested.

→ Line 51 on page 2.

Line 59: “speculate” → At this stage, this is more than "speculation"... → I would adapt the sentence as follows: "Based on laboratory experiments and seismicity correlation studies, researchers have inferred that seismicity in the lower plane of double seismic zones could be linked to the dehydration of hydrous minerals, including serpentine and chlorite."

Response: We have modified the sentence as the reviewer suggested.

→ Lines 59–62 on page 2.

Line 66 → Ref.17 (Ferrand, 2019) is cited here to claim the contrary of what it argued. Contrary to what the authors suggest here, ref.17 reviewed all possible hydrous and anhydrous phases that could be involved in TDST, including chlorite, and did not specifically favor serpentine minerals... The correlation of most seismicity of double-seismic zones with the instability of serpentine minerals is a fact that strongly support a causal link. In addition, ref.32 (Ferrand & Manea, 2021) also considered chlorite...

Response: We corrected the references.

→ Line 67 on page 2.

Lines 66-68 → Already highlighted by correlation studies between seismicity and thermal structure (Abers et al. 2013, Ferrand 2019 = ref.1 and ref.17). → I urge the authors to rephrase the paper in order to keep the story as honest as possible.

Response: We rephrased sentences as the reviewer suggested.

→ Lines 67–70 on page 2.

Line 72: “Fourteen experiments were conducted using simple shear” → “Fourteen simple-shear experiments were conducted”

Response: We have modified the text as the reviewer suggested.

→ Line 74 on page 3.

Lines 79-80: formula → again, I do not understand why the authors consider such a complex equation. Are they sure about the presence of vesuvianite and Na-phlogopite? Is it possible to specify the amphibole? Is it systematically tremolite? Could there be cummingtonite or tschermakite for example?

NB: Regarding the stability of clinochlore, I still wonder why the authors de not refer to the key references on the topic (i.e. Staudigel & Schreyer, 1977; Pawley, 2003), and prefer keeping fixed to a complicated multi-component equation involving mineral phases that they have not been able to observe. I understand the thermodynamic modelling argument, but I am afraid many readers will not feel satisfied by this interpretative side of the paper.

Response: The starting material for this study is natural peridotite, which comprises complex components, unlike the synthetic starting materials used in previous studies (Pawley, 2003; Staudigel and Schreyer, 1977). We have modified the text to explain the distinct dehydration reactions between the synthetic starting material and the natural starting material.

→ Lines 77–82 on page 3.

Line 81: “vesuvianite were difficult to observe” → But were they observed at all?? → If no, the authors should provide a better argumentation to support the decision they make, and be very clear about what observations are lacking. Importantly, the way the authors rule out the possible presence of minor fractions of serpentine minerals (lines 129-131) is totally inconsistent with the way they claim for the existence of minor (non-observable) amounts of vesuvianite… → This is weakening the manuscript, and could be solved by a be

NB: During the first round of review, I was surprised by the importance of vesuvianite in the paper, although it is not documented in the starting material. But I would buy it if the authors add a reference about its stability at such P-T conditions. I found one in the literature: Dey et al. (2019). The authors might find it useful to better argue in favor of their interpretation.

Response: Unfortunately, we were unable to detect vesuvianite, as confirmed by BSE images and EDS mapping of the entire samples. We acknowledge the possibility of serpentine being present in the samples as a minor mineral, following the same logic. However, upon close examination using highly magnified (~2000x) EDS mapping in the faults, we found no serpentine. Therefore, we consider that serpentine likely does not play a significant role in faulting, even if it can exist as a minor mineral in the samples. As the reviewer suggested, we have modified the text to reference Dey et al. (2019) regarding variable vesuvianite stability fields and provided an explanation for the possibility of serpentine being present as a minor mineral.

→ Lines 84–88 on page 3.

→ Lines 138–144 on pages 5–6.

Line 82: “Of” → “In”

Response: We have modified the text as the reviewer suggested.

→ Line 88 on page 3.

Line 83 → remove “in all of them”

Response: We have modified the text as the reviewer suggested.

→ Line 89 on page 3.

Line 86: “absent” → “not observed”

Response: We have modified the text as the reviewer suggested.

→ Lines 92–93 on page 3.

Line 87: “are evidence” → “are interpreted as evidence”

NB: The authors can indeed consider these fluid inclusions as 'evidence' of dehydration during deformation only after presenting the detailed observations of lines 88-92.

Response: We have modified the text as the reviewer suggested.

→ Line 93 on page 3.

Lines 91-92 → WARINING: Here there is a critical mistake in the interpretation. First, fluid inclusions do not form anything; they are what remains after coeval dehydration and deformation. Second, the presence of fluid, together with elastic strain can trigger mode-I cracks subparallel to sigma 1, indeed, but only in case of fluid saturation (expected upon chlorite dehydration). → I request that the authors correct the observational mistake and invert the sentence in order to present these results in a physically logical manner.

NB: Under non-negligible confining pressures, cracks tend to form about 30-40° from sigma1 in the absence of pressurized fluids (fluid saturation conditions locally expected upon dehydration with a positive Clapeyron slope), and this angle can decrease to about 0-10°C in case of fluid pressurization (positive Clapeyron slope), which, obviously, is not the case here.

In addition, the fluid inclusion trails are about 30° from sigma 1... which is expected in the absence of pressurized fluids, consistently with the negative Clapeyron slope.

Response: We have modified the sentence, mode-I cracks as the cause and fluid trails as the result. We also commented that fluid trails and cracks are oriented up to 30° from σ_1.

→ Lines 95, 97, and 98 on page 4.

Lines 94-95: "and their sheets were aligned at a slight angle with respect to their shear direction" → WARNING: the delamination of the documented chlorite crystal (Fig.3a) occurred along its basal plane... which orientation is mostly due to the initial orientation of the crystal, not to sigma1... The authors could write instead that the deformation can be accommodated by shearing along the basal plane of well-oriented chlorite crystals.

Response: We have modified the text as the reviewer suggested.

→ Lines 100–102 on page 4.

Line 97: "The" → "A"

Response: We have modified the text as the reviewer suggested.

→ Line 104 on page 4.

Lines 97-98 → remove "of the chlorite peridotite"

Response: We have modified the text as the reviewer suggested.

→ Line 105 on page 4.

Lines 101-102: "optical photomicrograph" → This image was produced using an optical microscope? or secondary electron imaging (SE2)? → clarification needed.

Response: We have modified the text to clarify that the image is optical microscope image.

→ Line 109 on page 4.

Lines 104-105: “which was not observed in unfaulted samples” → This part of the sentence is useless.

Response: We have modified the text as the reviewer suggested.

→ Line 111 on page 4.

Line 107: “micro analyser” → “microanalyser”

Response: We have modified the text as the reviewer suggested.

→ Line 113 on page 4.

Line 113: “after peak-stress reached” → “after peak stress”

Response: We have modified the text as the reviewer suggested.

→ Line 119 on page 5.

Line 115: “characteristic of seismic faults.” → WARNING: Macroscopic stress drops in deformation experiments are characteristic of sample-scale faulting, but it is does not tell anything about the seismic or aseismic nature of faulting events. Acoustic receivers are required to go further (e.g. Gasc et al., 2011, 2017; 2022; Schubnel et al., 2013; Incel et al., 2017; Ferrand et al., 2017). For instance, in our previous study (Ferrand et al., 2017), a macroscopic stress drop is observed upon dehydration-induced aseismic deformation (50% of serpentine) but no sample-scale stress drop is observed in seismogenic samples (5% of serpentine).

Response: We have removed the sentence to avoid a confusion.

→ Line 120 on page 5.

Line 119: “fault” → “faults”

Response: We have modified the text as the reviewer suggested.

→ Line 124 on page 5.

Line 119: “a partial dehydration product” → “one of the dehydration products”

Response: We have modified the text as the reviewer suggested.

→ Line 124 on page 5.

Line 119 → The authors could add a line about the observed differences for positive and negative volume changes... It is a very important topic, and we need to highlight such observations, because they are very important for numerical modelling and tectonic-scale interpretations (e.g. Scambelluri et al., 2019; Craiu et al., 2022).

Response: We have documented different fault microstructures under positive and negative volume changes.

→ Lines 125–128 on page 5.

Line 124: “fluid” → “fluid inclusions”

Response: We have modified the text as the reviewer suggested.

→ Line 133 on page 5.

Line 125: “the dehydration product” → “a well identifiable dehydration product”

Response: We have modified the text as the reviewer suggested.

→ Line 134 on page 5.

Line 125: “Results” → “Our results”

Response: We have modified the text as the reviewer suggested.

→ Line 134 on page 5.

Line 126: “triggers” → “likely contributes to”

Response: We have modified the text as the reviewer suggested.

→ Line 135 on page 5.

Lines 127-128: “the temperature conditions of chlorite to be destabilized” → The sentence is not well written + There is a conceptual issue. In a purely chemical sense, the reason for “partial” dehydration is because the other reactive phases are in low fractions. Thus, most chlorite remains stable in the end. A chemist would say that chlorite is in excess compared to other reactive species in the studied material. The studied reaction does not correspond to the actual destabilization of chlorite…

Response: We have modified the text as the reviewer suggested.

→ Line 137 on page 5.

Line 128: “complete instability” → “actual instability”

Response: We have modified the text as the reviewer suggested.

→ Line 137 on page 5.

Lines 129-131 → Here the authors claim that there was/is no serpentine in their samples because they could not observe it. But they could not observe vesuvianite or phlogopite either, so this argument sounds totally unreasonable…

Response: As we answered for the comment of line 81, we acknowledge the possibility of serpentine being present in the samples as a minor mineral like vesuvianite and phlogopite in the starting material. However, upon close examination using highly magnified (~2000x) EDS mapping in the faults, we found no serpentine. Therefore, we consider that serpentine likely does not play a significant role in faulting, even if it can exist as a minor mineral in the samples. As the reviewer suggested, we modified the text to explain the possibility of serpentine being present as a minor mineral.

→ Lines 138–144 on pages 5–6.

Lines 134-135: "Conventionally, dehydration embrittlement […]" → WARNING: It is no convention! Dehydration embrittlement is a mechanism based on fluid overpressure. This is the definition. The authors should admit that the 'dehydration embrittlement' model is not appropriate at high pressures, neither for serpentine nor for chlorite.

Response: We have modified the sentence to avoid a confusion.

→ Line 145–146 on page 6.

Lines 135-137 → Incorrect. As clearly explained by Ferrand 2019, negative volumes changes imply impossible hydrofracking!

Response: Dehydration-induced faulting with negative volume changes (~6 GPa) was experimentally studied by Jung et al. (2004). We modified words about dehydration-induced seismicity under negative volume change ($\Delta V < 0$) to avoid confusion.

→ Lines 145–156 on page 6.

Lines 139-143 → These sentences are correct, and make the previous sentences (lines 129-137) sound a bit odd…

Response: We have modified sentences to avoid a confusion.

→ Lines 145–156 on page 6.

Line 143 → remove "conditions"

Response: We have modified the text as the reviewer suggested.

→ Line 153 on page 6.

Line 145: “volume change conditions” makes no sense → replace with “volume changes”

Response: We have modified the text as the reviewer suggested.

→ Line 155 on page 6.

Lines 150-152 → repetition of "alternatively" and "alternative"

Response: We have modified the word to clarify the sentence.

→ Line 162 on page 6.

Line 155: “wave” → “waves”

Response: We have modified the text as the reviewer suggested.

→ Line 165 on page 6.

Line 159: “supports the notion of” → “would be consistent with the”

Response: We have modified the text as the reviewer suggested.

→ Lines 169–170 on page 7.

Line 159: “rapid fast” → choose between “rapid” and “fast”

Response: We have modified the word to clarify the sentence. We chose “rapid”.

→ Line 170 on page 7.

Lines 155-158 → The observation of amorphous material is important because it allows the authors to argue for dynamic melting, and thus for the seismic nature of the faulting event. But this does not demonstrate that all observed faults were formed by seismic events. Furthermore, whether the reported experimental faulting events are seismic or not is not really a problem.

Seismic and aseismic faulting events both correspond to extreme localization of the deformation, which can lead to larger ruptures and is essential to understand regional seismic and aseismic events, from earthquakes to tremors and slow-slip events...

Response: We have written the sentence as the reviewer suggested.

→ Lines 157–158 on page 6.

Lines 160-161 → This is correct, but the logical link with the next sentence is unclear. The authors should write that the presence of amphibole in faults documents coeval fluid percolation and faulting.

Response: We have modified the text as the reviewer suggested.

→ Lines 172–173 on page 7.

Line 179: “of upper mantle” → “of the upper mantle”

Response: We have modified the text as the reviewer suggested.

→ Line 191 on page 7.

Line 179: “of subducting” → “of the subducting”

Response: We have modified the text as the reviewer suggested.

→ Line 191 on page 7.

Lines 181-183 → again, ref.17 (Ferrand, 2019) does not only consider serpentine minerals but a vast diversity of hydrous and anhydrous minerals that could exist in subducting slabs under intermediate-depth conditions. And the same applies to the upper plane... → correction needed.

Response: We apologize for misleading referencing. We corrected the references.

→ Line 193 on page 8.

Lines 186-187: “under deeper depth” → “deeper” or “at larger depths” or “in deeper conditions”.

Response: We have modified the text as the reviewer suggested.

→ Line 198 on page 8.

Lines 192-193 → incorrect: it is not more difficult than experiments on serpentine or other minerals...

Response: We have removed the sentence to avoid a confusion.

→ Line 204 on page 8.

Lines 195-199 → The sentence is too long and ends with a confuse double "such as". → I suggest to cut the sentence at least in 2. Especially, I do not understand what the authors mean by adding "such as serpentine" at the end of the sentence. → Clarification needed.

Response: We have modified the sentence as the reviewer suggested.

→ Lines 206–211 on page 8.

Line 200: “intermediate-depth earthquakes” → “most intermediate-depth earthquakes”

NB: the authors have not demonstrated that only these two mineral families are involved. Plenty of minerals can generate stress transfers upon transformation (Ferrand, 2019). I think about talc for instance, which stability also coincides with the studied P-T conditions. As far as we understand, what matters is the speed of the transformation and the elasticity of the mineral and surrounding materials.

Response: We have modified the text and mentioned a possibility for “other minerals” as the reviewer suggested.

→ Lines 211–213 on page 8.

Line 203: “~31% chlorite by volume” → “~31 vol.% of chlorite”

Response: We have modified the text as the reviewer suggested.

→ Line 214 on page 8.

Lines 204-205: "~30% chlorite and ~1% dehydration product." → "~30 vol.% of chlorite and ~1% of dehydration products"

Response: We have modified the text as the reviewer suggested.

→ Lines 215–216 on page 8.

Lines 205-206: "10-35% chlorite volume percentage" → "a fraction of 10-35 vol.%"

Response: We have modified the text as the reviewer suggested.

→ Lines 216–217 on page 8.

Line 211: "reported ultrahigh-pressure […]" → "reported an ultrahigh-pressure […]"

Response: We have modified the text as the reviewer suggested.

→ Line 222 on page 9.

Lines 216-217 → This sentence should be in the introduction…

Response: We have removed the sentence to avoid a confusion.

→ Line 227 on page 9.

Line 219: "subducting" → "subduction"

Response: We have modified the text as the reviewer suggested.

→ Line 229 on page 9.

Line 223: “the upper (~600 °C) and lower (~700 °C) planes” → This over-simplification is not reasonable. This would make non-specialists readers believe that the upper plane always around 600°C and the lower plane around 700°C, which is demonstrably incorrect (e.g. Ferrand, 2019, and ref. therein).

Response: We have removed the words to avoid a confusion.

→ Line 233 on page 9.

Line 225 → remove “global” or rephrase to avoid confusion.

Response: We have removed the word to avoid a confusion.

→ Line 235 on page 9.

Line 225: “However,” → “In contrast,”

Response: We have modified the text as the reviewer suggested.

→ Line 235 on page 9.

Lines 231-233: “A fault was developed where a dehydration product (Ca-amphibole) and a melt of the chlorite peridotite were observed” → Very confused sentence → I suggest to rephrase as follows: “Faults developed and are filled with a dehydration product (Ca-amphibole) and an amorphous material suggesting dynamic melting of the chlorite peridotite”.

NB: it is very important not to write “melt” to described a glass. A melt is liquid.

Response: We have modified the sentence as the reviewer suggested.

→ Lines 242–243 on page 10.

Lines 233-236: “The result supports dehydration involving mechanisms (dehydration embrittlement and DDST/TDST) that are applicable for not only well-known serpentine but also chlorite to explain intermediate-depth earthquakes of subducting slabs.” → suggestion for better grammar and fluidity: “The results support that dehydration-induced faulting mechanisms (e.g. dehydration embrittlement or DDST/TDST) are applicable to not only well-known serpentine minerals, but also to chlorite to explain intermediate-depth earthquakes within subducting slabs."

Response: We have modified the sentence as the reviewer suggested.

→ Lines 243–246 on page 10.

Line 241: "used as the starting material." → "used as starting material."

Response: We have modified the text as the reviewer suggested.

→ Line 251 on page 10.

Line 245: "micro analyser (EPMA)." → "microanalyser (EPMA)"

Response: We have modified the text as the reviewer suggested.

→ Line 255 on page 10.

Line 248: "Nickel foil" → "A nickel foil"

Response: We have modified the text as the reviewer suggested.

→ Line 268 on page 11.

Line 270: "controlled constantly" → "constantly controlled"

Response: We have modified the text as the reviewer suggested.

→ Line 280 on page 11.

Lines 274-276 → repetitive with lines 258-260. → correction needed.

Response: We have removed the sentence to avoid a confusion.

→ Line 284 on page 11.

Line 281: “decreasing the pressure” → “the pressure was decreased”

Response: We have modified the text as the reviewer suggested.

→ Line 289 on page 11.

Lines 286-289 → For the sake of clarity, the authors could add: “using an internally consistent dataset built from our up-to-date knowledge on mineral phases.”

NB: This is a big assumption. Using such thermodynamic models means that we assume that all potentially stable mineral phases are known in the studies system, the overall chemistry of which is also somehow uncertain.

Response: We have modified the text as the reviewer suggested.

→ Lines 294–295 on page 12.

Line 291: “assuming a water saturated condition” → “assuming water saturation”

NB: Water saturation is a strong assumption. This is risky and should be discussed in the discussion, especially in the paragraph about volume changes.

Response: We have modified the text as the reviewer suggested.

→ Line 300 on page 12.

Line 295: “solid solutions” → I suggest to rephrase this, because there is a confusion with the concept of solid solution. → suggestion: “The mineral phases considered in the model were…”

NB: Thermodynamically speaking, a solid solution is a mixture of two minerals forming an homogeneous crystal. This is not what the authors mean at line 295.

Response: We have modified the text as the reviewer suggested.

→ Lines 303–304 on page 12.

Line 302: “using field-emission …” → “using a field-emission …”

Response: We have modified the text as the reviewer suggested.

→ Line 311 on page 12.

Line 305: “accelerating” → “acceleration”

Response: We have modified the text as the reviewer suggested.

→ Line 314 on page 12.

Line 310: “current probe” → “probe current” (?)

Response: We have modified the text as the reviewer suggested.

→ Line 319 on page 13.

Line 529 (Table 1 caption): “of chlorite” → “on chlorite”

Response: We have modified the text as the reviewer suggested.

→ Line 545 on page 18.

Line 536 (Fig.1 caption): “match only” → “only match”

Response: We have modified the text as the reviewer suggested.

→ Line 555 on page 18.

Line 538 (Fig.2 caption) → It is unclear what “36” and “33” mean on the figure. → add to caption.

Response: The contour numbers on the figure represent the volume percentages of minerals.

We have modified the text for explanation.

→ Lines 559–560 on page 18.

Line 547 (Fig.2 caption): “a Mode-I crack along the fluid inclusion trail” → “a Mode-I crack that left a fluid inclusion trail”

Response: We have modified the text as the reviewer suggested.

→ Line 567 on page 18.

Lines 556-557 (Fig.4 caption): “optical photomicrograph” → Only electron microscopy is mentioned in the Methods. → confirmation needed.

Response: We clarified the use of an optical microscope as well

→ Line 311 on page 12.

Line 573 (Fig.S1): “Nickel foil” → “A nickel foil”

Response: We have modified the text as the reviewer suggested.

→ Line 605 on page 20.

Line 575 (Fig.S1): “stress” → “stresses”

Response: We have modified the text as the reviewer suggested.

→ Line 607 on page 20.

Line 577 (Fig.S2) → remove “curve”

Response: We have removed the “curve” as the reviewer suggested.

→ Line 609 on page 20.

Line 577 (Fig.S5): “(a melt)” → “(most likely a solidified/quenched melt)”

Response: We have modified the text as the reviewer suggested.

→ Lines 622–623 on page 20.

Fig.1 → In Fig.1a, it appears that most lower-plane earthquakes occur in the stability field of chlorite peridotite... The authors should make it clear (maybe into parentheses) that the instability of chlorite would explain only a fraction of this seismicity (around the transition between the yellow and red area zones). → Maybe a dashed line could be added on the figure to help the reader?

Apart from that, I would say that these new lines 51-55 are much clearer and more accurate than in the previous version of the manuscript.

Response: We have modified the text as the reviewer suggested.

→ Lines 550–551 on page 18.

Fig.4 → Interesting observation without description/discussion: Fig.4c and Fig.4f have almost the same scale, but exhibit different microstructure. At 2.5 GPa (Fig.4c), the Clapeyron slope is negative and thus reaction products can easily form within a relatively thick fault gouge (about 10 µm). At 1.5 GPa (Fig.4f), the Clapeyron slope is slightly positive and the fault is much thinner (<1µm).

Response: We have described different microstructures of the faults depending on volume changes.

→ Lines 125–128 on page 5.

Fig.S2 → Problem to distinguish JH127 and JH228 → color adjustment required.

Response: We have adjusted colors for the samples JH127 and JH228 as the reviewer suggested.

→ Figure S2.

References

Dey, A., Choudhury, S.R., Mukherjee, S., Sanyal, S., Sengupta, P., 2019. Origin of vesuvianite-garnet veins in calc-silicate rocks from part of the Chotanagpur Granite Gneiss Complex, East Indian Shield: The quantitative P-T-XCO2 topology in parts of the system CaO-MgO-Al2O3-SiO2-H2O-CO2 (+Fe2O3, F). American Mineralogist: Journal of Earth and Planetary Materials 104, 744-760.

Jung, H., Green, H.W., Dobrzhinetskaya, L.F., 2004. Intermediate-depth earthquake faulting by dehydration embrittlement with negative volume change. Nature 428, 545-549.

Pawley, A., 2003. Chlorite stability in mantle peridotite: The reaction clinochlore + enstatite = forsterite + pyrope + H2O. Contributions to Mineralogy and Petrology 144, 449-456.

Staudigel, H., Schreyer, W., 1977. The upper thermal stability of clinochlore, Mg5Al [AlSi3O10](OH)8, at 10–35 kb PH2O. Contributions to Mineralogy and Petrology 61, 187-198.