1 • The authors interpret variations of the strength and parameters correlated as function of temperature and crustal thickness changes. To this purpose, we should consider that in the model an uniform stain rate is assumed and lateral variation of rheology is not included. These parameters could influence the strength variations and their possible effects should be discussed. Furthermore, since the Alps and surrounding areas are tectonically active, thermal steady state conditions are likely not present. The authors refer to the possible effects of the slab as well as of the fluids, but processes such as exhumation/erosion/sedimentation can affect the thermal field, especially that of the sedimentary layer.

We thank the reviewer for these helpful suggestions. We have now fully addressed all of these points in the newly added ‘Workflow Limitations’ section from lines 403 – 425.

2 • The authors should also specify if they calculated the strength for compressional or extensional stress conditions.

We thank the reviewer for pointing out this omission, it has been calculated in a compressional regime and this has been added to the manuscript at line 190.

3 • Section 2 Method: Line 120-125: The authors state referring to the Peierls creep mechanism: “however this was found to not affect the ductile strength of the plate…” What do you mean precisely? They cited Katayama and Karato (2008), but this article refers to an experiment on olivine under water satured conditions, which may not represent the conditions of the study area. However, other experiments (e.g., Demouchi et al., 2013) derived the Peierls creep mechanism on ‘dry’ olivine as well.

We thank the reviewer for their suggestion. He/she is correct in stating that we refer to a specific form of low temperature plasticity as derived from experiments on wet olivine. Our choice for a wet rheology stems from the following reason. Peierls creep results in a weakening of the plastic strength of the mantle rock at higher stress, which could potentially influence the depths at which the transition between frictional brittle behaviour and ductile deformation occurs. As such, the BDT would be located at shallower depths than those computed based on conventional power law creep. Recent studies (e.g. Katayama, 2021) have highlighted the sensitivity of lithospheric strength and modes of deformation to the effect of water. This is particularly relevant while addressing the sensitivity Peierls creep shows to wet conditions. The main implication here is that the effective role of Peierls creep to weaken the strength of the mantle would be higher in the presence of fluid, therefore by considering a bulk wet rheology for the most abundant mantle mineral, e.g. olivine. In this regard, a main limitation is the “upscaling” of currently adopted flow laws from the size of the sample in the laboratory to the scale of the lithosphere, where also we lack any control on the effective boundary conditions (the latter are controlled in the laboratory). There is also no evidence of scale invariant behaviour of power law creep rheologies (whether diffusion, dislocation or Peierls), given their exponential dependence on the applied strain rate. The latter parameter also differs within at least ten orders of magnitude between laboratory and natural conditions. Therefore, such models can only propose end-member study cases. Back to the “wet versus dry” bulk rheology issue, within the range of uncertainties in flow law parameterization, we attempted in our manuscript to quantify how robust the modelling results would have been to the additional weakening from low temperature plasticity. Therefore, we tested an end member model where these effects are “maximized”, thus our choice for a wet rheology, and we found that this additional deformation mechanism did not affect the main implications derived from our study. This would also be valid if considering a dry rheology, for which the weakening would be even less pronounced.

When stating that, “... this was found to not affect the ductile strength of the plate…”, we are showing that Peierls creep did not affect the spatial distribution of the brittle to ductile transition, where the strength contrast is controlled by Peierls creep mechanism at those levels.

4 • There is a recent thermal model of the European lithosphere of Limberger et al., 2018 (Global and Planetary Change) with which the authors can compare their results.
We thank the reviewer for this suggestion. As the thermal field was not generated within this work (see Spooner et al., 2020) we did not think it relevant to discuss differences between thermal field at length in the discussion. However, we have added a paragraph in the ‘Structural Model and Thermal Field’ section from lines 124 - 136 to explain why the thermal field we have used is preferential to Limburger et al. (2018) and compare the differences between the two.

5 • In Table 1, the authors display the rheological paramters used, but they do not specify the rocks’ conditions (dry or wet), except for the sediments.
We thank the reviewer for pointing out this omission. Table 1 has now been updated to reflect that the other parameters were dry.

6 • Equation 3: Please, add a reference to the equation of the effective solid viscosity.
We have some problems in following the reviewers reasoning here. The solid viscosity which he/she is referring to is not a material property, rather an effective parameter that is derived to recast the main constitutive law for secondary creep linking (differential) stress to (differential) strain rate in a more concise manner, thereby integrating all non-linear dependencies (described in equation 2 in the main text). This said, we are unsure which reference, if any, could apply here.

7 • Section 3 Results: About the strength results displayed in Fig. 5, it would be better to display the two figures using the same le range of values, possibly with another color scale (the one in use is too dark) to better compare them. At the moment, the lithospheric strength looks almost equal to the crustal strength.
As the range of lithospheric strengths (0.9 $\log_{10}$ Pa m) is almost 4 times larger than the range of crustal strengths (0.25 $\log_{10}$ Pa m), plotting them on the same scale whilst showing the heterogeneity of crustal strengths is not possible. The intent of these figures (now figure 7) is to show lithospheric and crustal strength heterogeneity and each has been scaled to highlight this. We have however changed the colour scale slightly from before in order for it to appear less dark, as per the reviewer’s suggestion, which we are thankful for. For a common scaling showing how much the crust contributes to lithospheric strength please see, Figure 8, which we have used in the manuscript to discuss how the crust provides very little of the lithospheric strength except in the orogenic root, with the upper lithospheric mantle providing the majority of lithospheric strength in both forelands. We have also added a new figure (figure 11) which shows the crustal contribution to overall strength making these comparisons even easier.

8 • Lines 140-155: It would be intresting to correlate the ratio crustal/mantle strength with crustal thickness and temperature to better understand which of the two parameters influences more the strength.

As mentioned in the above response, figure 11 has been added in order to shows the crustal contribution to the overall lithospheric strength. This has also been discussed in a significantly reworked paragraph in the 'Mechanical Strength' section at lines 278 -289.

9 • Line 165: 'The distribution of seismic event epicentres in the southern foreland strongly correlates spatially with the computed integrated lithospheric strength (Figure 5a) and not with crustal strengths,…' This is hard to say, according to the colour scale used for Figure 5. Furthermore, if the earthquakes occur in the crust, their distribution should correlate more with the crustal strength variations.

As has been mentioned in a prior response, the colour scaling was scaled to highlight the relative strengths of either the crust or lithosphere, as what is important is whether the crust is strong compared to other places in the crust. To the reviewers second comment, what we are trying to demonstrate is that that is not always the case, with plate dynamics having an impact, resulting in lots of seismicity in certain regions where the crust appears homogenously strong, as the integrated lithospheric column is weak there due to representing the edge of a rigid and rotating indenter.

10 • Line 175: 'all cross sections show that the majority of seismicity occurs within the strongest region of the upper crust (~ 1 GPa),' I do not think that you can link the depth of seismicity with a strength value (~ 1 GPa), since this value is derived from a model based on assumptions, such as a fixed strain rate.

We thank the reviewer for bringing this to our attention. We have addressed this by making it clear in the 'Results' section that these strength values are merely values derived from the calculations undertaken in this workflow (see lines 242, 245 and 247). In addition, we have added a further section on 'Workflow Limitations' (see lines 403 – 425) to discuss the impact that depth dependent strain rates may have and that not enough data is available at present in order to utilise them across the whole study area.

11 • Line 183 up to the end of the section: Since, as expected, there is a strong correlation between lateral strength and viscosity variations, I suggest to discuss these results together.

We agree with the reviewer that there is a clear link between the lithospheric strength and the mantle viscosity which has already been emphasised in the text when discussing the mantle viscosities. However, there is also a very strong link between the lateral strength variations and the strength cross sections. As such we have opted to retain the original layout as we feel it already represents the most efficient way to talk through the results of the study.

12 • Section 4.1 Mechanical strength: The concept that a thick crust (e.g., that one characterizing the orogens) retains more strength than the mantle lithsophere has been also discussed in previous studies (e.g., Tesauro et al., 2009, Tectonophysics for Europe and more recent studies 2 on global and regional scale). About the relationship between crustal thickness, temperature, and integrated strength check also Mareschal and Jaupart, 2013, (Tectonophysics).

We thank the reviewer for this suggestion. We have now included these references in a significantly reworked paragraph in the 'Mechanical Strength' section at lines 278 – 289.

13 • Lines 215-218: A lower geothermal gradient can result also in an increase of the maximum depth seismicity, due to the deepening of the BDT, and not necessarily in 'less seismicity'.

Whilst we agree with the reviewer that this is of course a possibility, it is not observed within our results as almost no seismicity occurs beneath the upper crust in the orogen. Instead, the lower geothermal gradient results in a stronger crust which appears to result in less seismicity. We have reworded this section and moved it to the 'Relation to Seismicity' section at lines 298 - 303 in order to add clarity.

14 • Section 4.2 Relation to sesimicity: The location of seismicity at the boundaries of tectonic features having different rigidity/strength has been already observed in previous studies that the authors can check (e.g., Craig et al., 2011, Geophys. J. Int; Sloan et al., 2011, Geophys. J. Int.; Tesauro et al., 2015, G3).

We thank the reviewer for bringing these studies from other regions to our attention and have incorporated them into the manuscript at line 312.

15 • Lines 275-278: The presence/absence of decoupling conditions are more intuitive looking at the profiles of the strength variations than at those of viscosity variations.

We thank the reviewer for bringing this to our attention. We agree and have made some changes to the wording in the paragraph at lines 345 – 356 in order to reflect this.

16 • About the seismicity depth: it can be influenced by the presence of fluids, as in case of the Molasse basin, where the maximum depth is close to that of the Moho (check the study of Deichmann, 1992, Phys. Earth Planet.), besides by the strain rate (a higher strain rate than the one assumed by the authors would increase the BDT depth). Then, the temperature is not the only parameter that influences the seismicity depths.

We thank the reviewer for bringing this work to our attention. We have incorporated it into the discussion at lines 400 – 402. Unfortunately, we cannot comment on the likelihood of fluid flow as it has not been modelled within this work. We do however point out that the location of this deeper observed lower crustal seismicity that occurs at higher than expected temperatures aligns well with the location of alpine slabs, suggesting that the slabs might provide a regional cooling effect due to not being in thermal equilibrium that we have not accounted for. Other papers cited in our work (Singer, J., Diehl, T., Husen, S., Kissling, E. and Duretz, T. Alpine lithosphere slab rollback causing lower crustal seismicity in northern foreland. Earth and Planetary Science Letters, 397, pp.42-56, https://doi.org/10.1016/j.epsl.2014.04.002, 2014) also pose slab rollback as a different hypothesis for this deep seismicity.