Dear Editor, dear Anne

Thanks again for your comments, which have resulted in further (and now final) refinement of the manuscript. In the following, we respond to all comments directly in the text lines of the editor and reviewers. These are marked in red.

Regards,
Mark Handy on behalf of the co-authors

Dear Mark, dear authors,

After reading the two reviews of the revised version of your manuscript, I conclude that the manuscript only requires minor revisions before it can be accepted.
Both reviewers consider that the most controversial element of the original version, which was the use of the term "tectosphere" for what looked very much like the lithosphere, has now been corrected. However, you remain firm on your definition of a European lithosphere that is almost twice as thick as what is generally considered on the basis of seismological constraints. I agree with the reviewers that your evidence is weak, weaker than usual seismological evidences provided for example by absolute S-wave velocity profiles or the depth of the boundary between isotropic lithospheric mantle and anisotropic asthenospheric mantle. As topical editor, I however respect the choice you have made to stimulate the debate on that issue.
Thank you, Anne, for respecting the prerogatives of the authors. You maintain that the evidence we present for a thick European lithosphere is “weak, weaker than usual seismological evidences”. Yet, already in the first version and even more in the second, we present evidence and arguments that the convention of equating the base of the positive-velocity anomaly with the base of lithosphere does not adequately explain the observed layering beneath the European foreland and the Central Alps. A correlative of this is that negative velocity anomalies do not in every case correspond with positive thermal anomalies, but can instead be attributed to seismic anisotropy and/or compositional effects. Indeed, this is where solely seismological criteria for defining lithosphere break down and the observations must be interpreted in a broader tectonic context. In adopting this approach for the European lithosphere, we differ from colleagues who assume à priori that lithosphere = high-velocity mantle. I guess we differ in how to weight certain observations. This point is now further emphasized in the Abstract.

The minor revisions that are requested before the manuscript is accepted are listed in L. Jolivet’s report (Rev. #2). The 2 most important ones are as follows:

1- The ordering of Chapters 3 and 4 should be reversed, since the arguments on which the interpretation is based should be placed before that interpretation. In Chapter 3, we have removed (and marked in red) the few phrases that appear too interpretational and placed them in Chapter 5. However, we wish to keep the order of chapters precisely because the observations in Chapter 3 form the basis for the discussion of alternative interpretations of anomalies in Chapter 4. Chapter 3 contains no interpretations unless explicitly stated where

they refer to later text. The large-scale tectonic interpretations come in Chapter 5, based on the observations in Chapter 3 and the arguments for-and-against alternatives advanced in Chapter 4.

2- As noticed by both reviewers, the schematic 3-D diagram of Fig. 11 should be corrected to account for a thick lithosphere. Yes, thank you for noticing this. We have fixed it. It was an oversight which stemmed from an earlier version of the diagram.

There is a typo in l. 17 and 456, where anomolously shoud be replaced by anomalously.
Done
P
lease refer to L. Jolivet's report for other suggested changes.

Please note two other changes:

1. In lines 946-960 we have added reference to Müntener et al. (2021) who argue against Tertiary slab breakoff in the Alps on geochemical grounds.
2. We have shortened the title of the manuscript to: **"Orogenic lithosphere and slabs in the greater Alpine area - Interpretations based on teleseismic P-wave tomography"**

I look forward to reading the final version of your manuscript.

Regards,

Anne Paul

Report #1

Submitted on 16 Sep 2021
Referee #1: Stephane Guillot, stephane.guillot@univ-grenoble-alpes.fr

This new version of the manuscript has evolved well, most of the remarks have been taken into account, in particular the change from Tectosphere to Lithosphere and I obviously agree on the proposed definition of the lithosphere which corresponds to the decoupling zone between the upper plate and the convective mantle.
There is still one major point of disagreement, concerning the thickness of the lithosphere. I am convinced that the authors are seriously mistaken about the thickness of the European lithosphere and moreover their interpretation is not compatible with the final 3D diagram where finally they draw a normal lithosphere (about 100 km). I suggest redrawing the final diagram to make it consistent with the paper.

Thanks for catching this. It was an oversight, based on adapting an older version of the diagram.

It is interesting to note that in his paper in Earth Science Review Artvevia (2019) shows that the European lithosphere under the Variscan basement has a noram thickness of about 100-125 km and not 200 km.

In fact, Artimieva`s (2019) LAB is much less than 100-125 km deep, only 50-75 km deep beneath the Alpine foreland (see Figs. 7 and 8). That's some 100 km shallower than the depth of the LAB beneath the European foreland of the Alps in our study. So perhaps it`s important to look more closely at the methods used and why they lead to such disparate results. Artemieva (2019) modeled the thermal structure (what she calls thermal isostasy) of the lithosphere based on a comparison of Moho depth and topography. In this approach, the crust is assumed to be at isostatic equilibrium and differences in lithospheric thickness are attributed solely to variations in the temperature field in the underlying mantle lithosphere. Compositional and anisotropic characteristics such as addressed in our manuscript are subsidiary, if indeed regarded at all. As Artimieva's approach is applied to a much larger scale than our study (which is based solely on high-resolution AlpArray data), it doesn't appear able to account for phenomena like the layered velocity anomalies down to 180 km in the downgoing European foreland of the Central Alps.

I think the authors forget that at the end of the Variscan orogeny (between the Upper Carboniferous and the Triassic) there was a major extensional event that thinned and rejuvenated the European lithosphere. In the Permian, Europe was a complete peneplain and in the Triassic the sea covered a large part of Europe. Furthermore, the entire Alpine region and the eastern edge of the Variscan range including the Alpine region was thinned again, which is perfectly incompatible with the preservation of a thick lithosphere. It is a pity that the authors do not take into account this evolution which in my opinion is very well seen in their tomographic images. Too bad.

We agree with the reviewer that the European Variscan crust away from the Alpine margins was thinned and/or equilibrated in post-Variscan (Permian) time, as evidence by the fairly uniform, c. 30 km Moho depth beneath the Variscides (except below the Rhine-Bresse graben system where it´s shallower). We also agree that the Alpine Tethyan margins (which have largely been subducted and are now located in the slab beneath the Alps, not in the foreland) were also thin. Where we part company is in assuming that this thinning is necessarily expressed by the depth of the present LAB. I would expect the LAB to have equilibrated downward with time after extension, analogous to what is observed at spreading ridges: after thinning and buoyant uplift, the lithosphere slowly thickens and subsides due to gradual cooling and thermal accretion. In the Alpine foreland, this post-Variscan equilibration would have occurred by the beginning of Triassic time, the period of epi-continental lacustrine sedimentation that the reviewer refers to. Note that this was at least 40-50 Myr after Permo-Carbonifereous extension, plenty of time to re-equilibrate lithospheric thickness. We can´t decide this dilemma here, but we think it`s worth being open-minded.

Report #2

Submitted on 24 Sep 2021
Referee #2: Laurent Jolivet, laurent.jolivet@sorbonne-universite.fr

The revised version is clearly an improvement and the authors have addressed many of the two reviewers' comments. The paper can, in my opinion, go in the press with some minor modification, although I am still not convinced by the interpretations of the tomographic images. The final geodynamic interpretation in terms of subduction polarity is logical given the interpretations of seismological data and also given the surface geology. Despite my disagreements I think that the publication of this paper can foster some useful debates.

One of the main improvements is that the notion of a "tectosphere" has disappeared in the new version. It was clearly not different from what we usually call "lithosphere" and its use was blurring the main message. By the way, I would like to stress the fact that Elsasser (1969) indeed used the term tectosphere and even showed figures stressing that it meant a rigid lid moving coherently above a weaker asthenosphere, in contradiction with the authors's reply to one of my comments. The paper corresponds to a chapter in a book edited by Runcorn. The reference is:

Elsasser, W. M.: Convection and stress propagation in the lower mantle, in: The application of modern physics to the Earth and planetary interiors, edited by: Runcorn, W. K., Interscience, New York, 223-246, 1969.

Thanks for providing the reference. It`s worth pointing out that Elsasser used the term "tectosphere" (later "lithosphere" in Elsasser 1971a, b) in a very different sense than Jordan (1975, 1981), contrary to the reviewer's claim in his first review. As explained before, we originally borrowed Jordan's term only to emphasize a key point that we still maintain: the European lithosphere in the Alpine foreland is seismically heterogeneous and its base does not everywhere coincide with the base of the seismic high-velocity zone. However, we`ve already abandoned the term "tectosphere", so the discussion here is moot.

Otherwise, I am still puzzled by the interpretation of the thickness of the European lithosphere. The main argument used in the paper is that the tomographic profiles show a package of coherent anomalies dipping toward the internal zones and the base of this package (i.e. below the low velocity anomaly) should be the base of the lithosphere (see figures 3C or 7B for instance). I can indeed see this "package" but I do not see any information telling us where the base of the lithosphere is. It is said page 13, lines 406-407, that this positive-negative anomaly layering makes up a "coherent kinematic entity". The profile does not show any kinematic information and I cannot clearly see what in the seismological information tells that the base of the lithosphere should be placed there.

Already in our previous reply (as well as in our original and revised manuscripts), we state clearly that the marker used to determine relative motion of mantle with respect to the orogenic crust and upper plate is the Northern Alpine Front (NAF). Note that we only say that there was displacement of the lithosphere to the SSE during subduction, not how

much displacement there was, because we don't know the original position of the negative anomaly beneath the Alpine foreland. We only see the obliquity of the negative anomaly pattern in map view with respect to the Northern Alpine Front in Fig. 2 and the location of layered anomaly descending beneath and beyond the NAF. However, what alternatives are there? We could propose that Alpine subduction was in another direction than SSE (absurd) or that the strong, layered anomalies are post-subduction features of the downgoing European plate (extremely unlikely). Neither of these alternatives is viable and would raise more than a few eyebrows. To clarify at the least the latter, we have added a sentence (lines 437-438) on the age of the strong negative anomaly extending from the Alpine foreland to beneath the Alps.

I have nothing a priori against the idea- that the European lithosphere is thicker than usually thought, but if I look at the tomographic data alone, I do not see it. It seems that you have decided a priori that it should be there or, else, I totally missed your reasoning.

The observed seismic layering down to about 180 km depth is interpreted to preclude large-scale mantle convection above this depth for the rather obvious reason that convection would not be expected to cause or to preserve subhorizontal layering. This is not à priori reasoning. Note that the lack of any pronounced layering below this depth in the Central Alps suggests that the mantle there may be (or may have been) convecting asthenospheric mantle. However, the velocity anomalies below c. 180 are weak, so we really can´t say for sure.

Chapter 4 (choice of the interpretation of seismic structure) should in my opinion come before you actually describe your interpretation, which is presently done in chapter 3. It would not change the conclusions but it would be more logical.

We see your point and have removed from Chapter 3 the few sentences that are interpretational and inserted them in Chapter 5. Chapter 3 now contains observations and only occasional references to later interpretation, which are separated from the observations. The observations in Chapter 3, especially of the seismic layering in some profiles down to c. 180 km, are key for the discussion of alternative interpretations in Chapter 4 and for the large-scale tectonic interpretation in Chapter 5. This is why we prefer to stick to the sequence of chapters used from the outset.

In the regional tectonic interpretation (from line 474) you mention that you marked boundaries (thick black lines) around "kinematically coherent images". Once more, what does tomography tell us about kinematics ? Please see comment above.

Lines 848-849: you suggest that "the Vp layer in the European lithosphere is structurally anisotropic and may have accommodated viscous flow". Is it still the lithosphere then?

Yes, we state this explicitly in the same sentence. In the next line, we state that our interpretation of lithospheric flow is contrary to the previously published view that viscous flow leading to seismic anisotropy in the 100-200 km depth interval occurred in the asthenosphere.

Lines 857 and following: the European lithosphere is not as old as you say. After the Permian thermal overprint it was also affected by the Triassic rifting and then the Liassic rifting event. Please see our response to the similar comment of the first reviewer.

Figure 11: this nice sketch is not in agreement with your conclusions as the European lithospheric lid is not thicker than 100km, instead of 200. Yes, thanks for detecting this oversight. See also response to editor and first reviewer.

Minor typos:

Line 17: please change “anomalously” to “anomalously”. Check throughout manuscript, the same misspelling occurs at least one more time. Thanks, we´ve done this.

Line 576: “Becculava” is probably “Beccaluva”, please check. Have checked, it`s indeed Beccaluva.

Laurent Jolivet