Response to comments of reviewer Stéphane Guillot (RC1)

Thank you for your review, Stéphane, which brought some interesting points to our attention. In the following, I've copied in your text and responded directly to your comments in red print.

I note however two important elements for the further discussion:

(1) the fact that the tomographic model is very dependent on the initial crustal model which results in a rather poor resolution of the tomographic model in the very upper part and according to Paffrath et al (2021).

(2) the fact that the tomographic model is poorly defined in the range 175-210 km.

It's not clear what you mean by "the upper part", but if you are referring to the depth range 175-210 km (his second pt), then you are correct that resolution of the tomographic model is poorer in the 100-200 km depth range due to the limited azimuth and near-vertical incidence of teleseismic rays. To that end, we have added to Section 2 a better description of the method used to ensure resolution in this critical depth range; this section builds on the description already given in Paffrath et al 2021b. The resolution in our models is sufficient to establish the nature of connectivity of the slabs with the orogenic lithosphere, as corroborated by the models of Kaestle et al. (2018) and (2020) based on ambient noise/surface wave tomography, which has much better resolution in the 100-200 km depth range. In rewriting section 2 "Methodology", we have made the crustal correction procedure clearer for non-seismologists.

All the interpretation that follows in the manuscript is based on the concept of European Tectosphere (which by the way is in the title of the paper). So this will be my first and more important remark, if the concept of Tectosphere is well explained in the introduction of the paper, the attribution of Tectosphere for the European plate is based between lines 434 and 438 by the fact that the negative anomaly follows the superior (overlying) positive anomaly, this is the only argments for a Tectosphere rather than a Lithosphere below the European plate. We have eliminated reference to "tectosphere" and revert to the term "lithosphere", which is defined in a kinematic sense as coherent layers of positive and negative Vp anomalies (see response to RC2). Note that this is just a change in nomenclature, not in interpretation. The point remains that the seismic heterogeneity of the down-going mantle slab probably reflects seismic anisotropy and/or compositional heterogeneity.

On the seismological side, as noted in the paper, many European groups and more recently an uncited paper on absolute S-wave velocities (Lyu et al., 2017) have defined the LAB at about 100-110 km depth; these data are not discussed in the paper. Why?

We have now included reference to Lyu et al. (2017) in new line 174. As an aside, we refer to the LAB as the base of the kinematic lithosphere, not to the much shallower base of the seismological lithosphere, which most seismologists (including Lyu et al. 2017) equate with the high-Vp layer (see also Waldhauser et al., 2002, Lippitsch et al., 2003, Spada et al. 2013). We note that in our profiles the base of this high-Vp layer occurs at a depth of ≤ 100 km. If this would correspond to the LAB, it would make the lithosphere rather thin for down-going Variscan or pre-Variscan continental lithosphere. We also make it clearer in the text that the seismological definition of the LAB is not always the almost accurate way to define the base of what geophysicists call lithosphere. We decided to use a kinematic definition, using the term lithosphere for the base of the moving plate that may (in the case of the Adriatic Plate) or may not (in the case of the European Plate) coincide with the seismological LAB.

Moreover, the notion of coherence between the fast and slow velocity anomalies that would imply that the two anomalies are driven in the same way and thus would define a single plate is undermined by the SKS data. It is unclear what you mean by "driven in the same way" and "undermined". In general, we would caution against putting too much faith in the depth resolution of SKS, particularly the depth interval that they sample (see comments below). In fact, shear wave splitting measurements (Barruol et al., 2004, 2011; Salimbeni et al., 2008, 2013; Link et al., 2020) show that the crustal contribution is minor and reflect the anisotropy in the upper mantle between 100 and 200 km. Interestingly, the orientation of the anisotropy data in the upper mantle is oblique to the direction of pre-oligocene convergence (which we would expect for pre-Alpine anomalies), with an E-W direction in the central Alps becoming progressively N-S in the western Alps and turning to the SE in the direction of the Appenines, driven by the slab retreat. In other words, it notes (?) a very strong decoupling between the crust and the very shallow mantle (high velocity zone) and the mantle below which excludes the notion of kinematic coherence between the high speed zone (0-100 km) and the low speed zone (100-200 km). This is only true if one assumes that the SKS directions are Alpine features, not, however, if they are pre-Alpine structures. Thus, it is likely that some of the anistropy in the upper mantle was inherited from the Variscan period (Indeed!), however, there is good agreement between these data and recent kinematics of the Alps that suggest that the anisotropy in the mantle is recent, post Oligocene, and therefore implies a strong decoupling between the slabs visible in the tomography and the upper, asthenospheric, mantle, which goes against the notion of a Tectosphere. Stéphane proposes an interesting idea on using SKS directions to identify possible detachment horizons in the mantle. As noted above, this is contingent on faith in good depth resolution of SKS directions as well as on an Alpine age of these directions. Unfortunately, resolution is widely regarded to be poor and, in this instance, is thought to be no better than 100 km, with the source of the anisotropy located somewhere towards the bottom of the lithospheric or in the sub-lithospheric mantle (Barruol et al. 2004, 2011). Yet, even if one trusts their depth resolution, the SKS directions under the Alps are kinematically ambiguous because we don't know their age. Stéphane tacitly assumes that arcuation of the internal western Alps is a post-Oligocene feature. Whereas older paleomagnetic studies indicate that this may hold for some internal units of the Western Alps (Thomas et al. 1999, Collombet et al. 2002), external units of the Western Alps experienced little, if any, post-Oligocene rotation (Aubourg and Chabert-Pelline 1999). Recent geological-geophysical studies indicate a more complex history of arc formation involving several distinct stages of motion from Eocene to Oligocene time (e.g., Schmid et al. 2017). The arcuate SKS patterns around the Western and Central Alps might therefore be due partly, or even entirely, to an older, pre-Alpine anisotropy recorded in the lower lithosphere (100-180 km in our interpretation) beneath the Alps, rather than to purported detachment within the mantle or flow within the asthenosphere. We just don't know. In this sense, the SKS directions do not "undermine" or preclude subduction of thick (150-200 km)

European lithosphere. We discuss the possible causes and age of the large negative Vp anomaly in the down-going lithosphere of the European Plate in section 6.3.

By the way, it is interesting to note that on figure 11, it is the lithosphere that is drawn so this figure, which is not consistent with what is discussed before in the manuscript. The caption to this figure specifies that slabs (sensu kinematically coherent pieces of descending mantle) are depicted, without any reference to the terms tectosphere or lithosphere. However, in the text and the figure, we now refer to these slabs as lithospheric slabs (see discussion above).

Similarly on figure A3 and A8, why the Tectosphere thins towards the south while on figure A16 and A17 we talk about lithosphere and not Tectosphere with a normal and coherent thickness of the lithosphere and its extension in the mantle. Please see discussion above.

Thus, to conclude, I agree that the upper mantle signature is inherited from the late Variscan event, which is marked by the thinning of the lithosphere and the upwelling of the asthenosphere (Malavieille et al., 1993; Schullman et al., 2015 : VanderHaegue et al., 2020). We state that the upper mantle signature is inherited (pre-Alpine, probably Variscan or pre-Variscan) without specifying whether this signature is late Variscan (Stéphane's interpretation). Lithospheric thinning may well have occurred in late or post-Variscan (Permian) time, but the European lithosphere in the sections we show is thick (150-200 km) and its Vp signature does not necessarily reflect such thinning. and finally, why put a limit around 200 km which is very poorly defined when the strongest velocity contract with a passage from +3 to -3 is rather around 80-110 km deep which is perfectly compatible with the geological inheritance suggesting a thin European lithosphere and a relatively shallow asthenosphere and incompatible with the concept of Tectosphere. The observations in support of placing the base of the subducting lithosphere at c. 180 km depth (rather than at the greatest change in Vp anomalies) are clearly laid out in the text (old text lines 282-295). These observations indicate kinematic coherence of both positive and negative kinematic anomalies above c. 200 km depth (Figs. 2, 3 and 4, old text lines 423-442). Again: It is important to distinguish between the structural/kinematic and seismological definitions of lithosphere (see comments above). We argue that the former rather than the latter is relevant for understanding the tectonics of European subduction in the Alps.

Then If you consider that the boundary between +3 and -3 anomalies are the LAB, it becomes more easy to interpret the deep high velocity anomaly you observed in the mantle and you don't have to cross cut the contours of the anomaly as you did in the figure 3, figure 4 , Figure 5, Figure 6 and Figure 7. Again, the lines we have drawn that define the base of the European slab DO NOT correspond strictly to + Vp and –Vp contours. Rather, these lines account for the structural and kinematic coherence of subducting mantle lithosphere that we interpret to be non-convective and compositionally heterogeneous.

The data presented is this paper are very important and have to be published, but I consider that the interpretation based on the existence of a Tectosphere is incorrect and it will be difficult to convince the geophysical and geological community. The only convincing figure is the Figure 11 in where the Tectosphere is not considered at all. Please see comment above regarding Fig. 11.

I propose that the authors reflect on this concept. If they persist in their interpretation then a stronger argument seems necessary and in particular to explain why in an arbitrary way, the contours of the anomalies in the mantle would be cut in the interpellation. The other solution which for me would be more convincing would be to consider the +3/-3 limit as the LAB and reinterpret the figures accordingly. I would be please to review a revuised version of this manuscript. See repeated comments above on this same point.

Specific points

Concerning the Adriatic slab, a comparison with the paper of Sun et al. (2019, EPS) is required. We have cited Sun et al 2019 (final paragraph of section 6.2). The main difference between our interpretations is that we show the Adriatic slab beneath the Northern Apennines to be largely detached from its orogenic lithosphere, in contrast to Sun et al. 2019 who interpret their images to show attachment of continental slabs without oceanic precursors.

Concerning the hydrated mantle, please refer to the paper of Malusa et al. (2021, G3) and refrences therien in where this point is thoroughly discussed. We have added quotes of Malusà et al. (2018) and Malusà et al. (2021) in this context.

The Zhao et al. (2015, Geology) is not referenced. Yes, they were cited in the old text, line 115. Zhao et al. (2015) is now additionally cited in the revised text..

Mark Handy on behalf of all co-authors