**Response to comments of 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. The lines numbers I've referenced for the new version of our text are line numbers in the annotated text online.

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 **new lines 62-64, 553-555 and 753-755**, 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 154**. 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 (**new lines 556-558**). We decided to use a kinematic definition, using the term LAB for the base of the moving plate that may (in the case of the Adriatic Plate, **new lines 757-760**) or may not (in the case of the European Plate, **new lines 756-757**) coincide with the seismological LAB (**new lines 1595-1601**).

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, depth 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, **new lines 1429-1461**.

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, **new lines 1427, 1461, 1597-1598**) 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 lines 282-295, 308-317; **new lines 550-560 and 748-751** in sections 3 and 4). These observations indicate kinematic coherence of both positive and negative kinematic anomalies above c. 200 km depth (Figs. 2, 3 and 4, old lines 423-442, new text lines cited above). 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 heterogeneous (**new lines 748-757 and referred to above**).

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 (**new line 1417**). 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) 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 (**new line 900**).

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 **new lines 156, 174**.

Mark Handy on behalf of all co-authors

**Response to comments of Laurent Jolivet (RC2)**

Thank you for your review, Laurent, which makes some important points that we've taken into account. The revised text has benefited from both of your remarks.

As in my response to RC1, I've copied in your text (black) and respond directly to your comments (red print). The lines numbers referenced for the new version of our text are the line numbers in the annoted text online.

This paper presents a new interpretation of the mantle structure underneath the Alpine arc based on a recent tomographic model. The goal of this work is perfectly clear and the outcome are potentially important given the current debate of the geometry of slabs below this well-known orogen. The attribution of the hanging pieces of slab seen in tomographic model beneath the Alps, European or Adriatic, is indeed debated and this has obviously major consequences for understanding the geodynamic evolution of this emblematic belt. Rightly stating that the long-established geological knowledge of the Alps has to be taken in consideration when interpreting deep structures illuminated by seismic rays, the authors conclude that there is only one main slab beneath the Alps and that it is European. Its geometry is precisely described and the implications of its detachment along most its length are discussed. This is thus potentially a major paper.

BUT, the question of the nature of slabs is mixed up with considerations about the thickness and rheology of the subducting lithosphere with emphasis on a "tectosphere" instead of "lithosphere". While the authors state at some stage that the so-called tectosphere almost equals lithosphere in terms of rheology, they also say that it is entirely different and changes the interpretation of tomographic images.

I have two main comments on this question, which really undermines the whole paper and makes it far from convincing:

- "Tectosphere" is by no way a new concept and I do not see why it is useful here. In my recollection, tectosphere was first discussed by Elsasser in 1967. In that paper it was used as the major stress-guide controlling plate motion in subduction zones. It is also behind the earlier models of Hess of a "tectogene" that was the early understanding of what we now call subduction. I do not really understand why it is useful to go back to this concept when lithosphere is a perfectly adaptable concept with the current corpus of knowledge on mantle and crustal rheology.

The point raised here is well taken, and, fortunately, there is a simple way out: We have eliminated the term "tectosphere" and now revert to the term "lithosphere". Without delving into the origins of "tectosphere" here (first defined by Jordan, neither used nor meant by Hess, Elsasser or Morgan, see below), we define lithosphere in the widely accepted sense as a piece of mantle that moves coherently with respect both to other pieces of lithosphere and with respect to the asthenosphere (**new lines 62-64**). Thus, "lithosphere" includes both orogenic lithosphere (upper mantle+crust) and slabs (upper mantle with/without crust still attached). Implicit in this definition is that the base of the lithosphere is a shear zone that accommodates relative motion (**new lines 96-98**). The reference frame used is the Alpine Front. We note in the text that this kinematically defined base -which is perforce also a weak zone- does not necessarily correspond with seismologically defined base of the lithosphere, which is often taken to be the bottom of the high-velocity layer. The reason we used "tectosphere" for European lithosphere was not to claim it as something new, but to emphasize the chemical/mineralogical heterogeneity of thick, non-convecting mantle layers involved in subduction. Jordan (1975, 1981) proposed such heterogeneity to account for differences in seismic velocity (see further comments below). In reverting to "lithosphere", we emphasize that our aim is to remain clear and to avoid confusing people.

1. It is extremely difficult to understand which observations allow the authors to place the base of the so-called tectosphere at a given depth below Europe or, else, decide that it is much thinner below the Eastern Alps. Quite simply, the criteria for drawing the base of the thick European lithosphere at c. 180 km (e.g., Figs. 3, 4A, 15A) is the

generally coherent pattern of both +Vp and -Vp anomalies to that depth (old lines 282-287 and 428-431; **new lines 550-560, 753-755, 1597-1600**). It is true that this criterion cannot be applied clearly in cross sections across the Eastern Alpine foreland (e.g., Figs. 4B, 6A), where the contrasts in seismic velocity structure are less pronounced (**new lines 641-644**). At some stage (figure 7) two alternative interpretations of one of the profiles are shown. One has the thick European tectosphere (Fig. 7B) and the other does not (Fig. 7C). But, in these alternative interpretations, the authors also show opposing interpretations of the nature of the slabs, either European or continental. It is not clear what you mean by "…either European or continental" (presumably you mean "Adriatic" rather than continental"). In our discussion of Figs. 7B and 7C in section 4, we merely weigh the implications of assuming that the base of the lithosphere is the base of the subhorizontal Vp layer as opposed to the base of the overlying +Vp layer. In the case of Fig.7C, this could lead to the conclusion that mostly Adriatic lithosphere was subducted, which is incompatible with the distribution and age of shortening in the Alps. Subduction of the European slab (7B) seems more realistic (and I fully agree with this conclusion) they then conclude that the interpretation with a tectosphere is also more likely, when the two questions are, in my understanding, totally independent. I see the problem, and we have added that another unrealistic feature, viz. using the +Vp base as the European LAB, would result in down-going lithosphere only 80 km thick! As pointed out in our response to RC1, we consider this unlikely for Variscan and pre-Variscan lithosphere in the foreland of the Alps (**new lines 934-940**).

The authors should (1) first clearly show why they draw the based of the "tectosphere" at a given depth on tomographic images (in some images the basal limit goes through anything, blue or red and one does not known why) and then, (2) once this is clearly established, discuss the consequences for the tectonic interpretation of images. As already pointed out, we explain why we draw the base of the lithosphere (formerly tectosphere) at a depth of c. 180 km (section 3, **62-64, 553-555 and 753-757**) and discuss these consequences in the following sections (**e.g., new lines 962-963, 1595-1597**). They should also clearly say the differences between tectosphere and lithosphere. We no longer use the term tectosphere. In my understanding there is no difference whatsoever. Actually, there is (see my remarks under the historical digression below). I do not see why you need this "new" concept (which is not new; we never claimed it was new!) for your interpretation. It blurs the whole paper and makes reading unconvincing. Agreed, that is why we have chosen to eliminate it.

The other major problem with this paper is that it is extremely difficult to follow. The description of the 3D structure is really not well organized and the connection between the text and figures is not clear, at least to me. At one stage, I stopped reading and moved directly to the discussion. Patience is a virtue amongst reviewers. There is a major effort to make for clarifying the presentation of the tectonic interpretation based on the tomographic images. Other colleagues have read the paper and made suggestions on how to improve the presentation. For example, we have amended the figures, both in the text body. Generally, however, no one found the overall structure and presentation unduly difficult.

Detailed remarks:

- Line 58: OK for Jordan but you should also refer to Elsasser (1967) or Morgan (1968). Laurent, thanks for recalling some classical papers from the beginning days of plate tectonics. After rereading my collection of papers from that era, I was rather baffled by your claim that Hess, Elsasser and Morgan came up with the idea of a tectosphere, or indeed, anything remotely like what Jordan proposed (1975, 1981). In attempting to explain the similarity of heat flow in continental and oceanic lithospheres, Elsasser made the assumption that the continental lithosphere has uniform thickness and homogeneous physical properties. This is quite the opposite of "tectosphere" sensu Jordan (1975), who attributed differences in surface-wave dispersion curves to mineralogical/chemical heterogeneity in cratonic lithosphere (analogous to what we proposed for the European lithosphere and what prompted us to use Jordan's term). In the 50s and early 60s, Hess invoked a modified form of Vening Meinesz's idea of "tectogene", which is a kind of downfolded or buckled crust and also has nothing in common with Jordan's tectosphere. Perhaps the similarily of the prefix "tectos" in these terms misled you. Or maybe you were thinking of Uyeda's work in the 70s, which focused on subduction. Yet, even he is unconcerned with heterogeneity of down-going plates, least of all the compositional heterogeneity that underlies Jordan's concept of a tectosphere. But as said, we have deleted the term "tectosphere" and now only refer to Jordan's idea of chemical/mineralogical gradients to explain the longevity of thermal anomalies in the mantle (**new lines 1470-1473**).
- Line 61: Jordan's definition of tectosphere is exactly what is meant nowadays by lithosphere. No. See my remarks above and read Jordan's papers if you're interested.
- Line 64 …: you cannot pretend using tectosphere in a purely kinematic sense when you draw arbitrary limits of the base of your tectosphere on the images. One does not know why you put them there. We have repeatedly referred to the coherence of laterally continuous positive and negative Vp anomalies as the criterion for defining the base of the lithosphere (see answers above). The difficulty in defining the base of the European lithosphere is greatest beneath the foreland of the Eastern Alps (old lines 526-528) where, in contrast to the Western and Central Alps, there is no horizontal layering of +Vp and -Vp anomalies (point already made in original text as well as in **new lines 642-644**). We now mention the criteria for drawing a speculative base of the lithosphere in such domains (**first paragraph of section 5)**. You also discuss the pre-Alpine history of these pieces of tectosphere, thus implying different chemical, lithological characteristics. Yes, indeed. Tomographic images shown velocity anomalies, not kinematic entities, and thus certainly not plates. We agree wholeheartedly with this latter statement. Again, the boundaries of our plates are not arbitrarily defined; they are based on considerations of coherence with respect to the surface geology (**new lines 958-961**), especially the Northern Alpine Front or Alpine orogenic front (**new lines 551-552, 745-755, 761, 907-908** in caption to Fig. 7, **935, 949-951, and 1599-1600**), plausible thickness of down-going pre-Alpine continental lithosphere (certainly more than 80 km, **new lines 934-936** and above) and the implausibility of high temperatures to explain the -Vp anomalies within laterally coherent layers of mantle (section 6.3, **new lines 1462-1473**).
- Lines 255-260: how do you define a plate boundary at this scale? A simple thrust is not a plate boundary, especially in a region like the Alps where deformation is widely distributed. We define the plate boundary as the limit of deformation, which does not

preclude distributed deformation behind (internal to) the orogenic front. Plate boundaries should be used for kinematic purposes not for geological description of small orogen like the Alps. We agree conditionally. Although it is clear that plate boundaries are difficult, if not impossible to define on short time scales (due to distributed deformation), they are certainly definable on longer time-scales of, say, several million years by using crustal provenance and kinematic markers as criteria (e.g., the Adria-European plate boundary in the Alps-Carpathians since the Miocene).

- Line 282-295: this paragraph is incomprehensible. Please help the reader. We have amended the text to read (**new lines 550-560**): "A striking feature in horizontal slices at 100 to 220 km depth is the lateral continuity of $-V_p$ anomalies of up to 5-6% which reaches from the northern Alpine foreland across the Alpine orogenic front to beneath the Western and Central Alps, as well as the westernmost part of the Eastern Alps (Fig. 2, solid red contours). In three profiles crossing these parts (profiles B, 1 of Figs. 3B, 3C, 4A), $+V_p$ and $-V_p$ anomalies in the 100-220 depth interval form coherent, inclined layers and together outline a package that dips beneath the Alpine front to below the center of the orogen. The base of this layer is interpreted to be the base of the lithosphere, or lithosphere-asthenosphere boundary, LAB. The layered structure making up the European lithosphere (see next chapter below) continues down-dip to the SE and beneath the core of the orogen, where it is interrupted, marking a slab tear (Figs. 3B, 4A)."
- Many parts of the paper are similar, so difficult to read. Dear Laurent, review comments like this are unspecific and therefore of no help.
- Figure 3 and all similar figures: please enlarge the numbers associated with the profiles on the map inset. Have done.
- Line 312: how do you recognize a tectosphere on tomographic images. You see velocity anomalies, you do not see rheology nor lithologies. As stated above, the term lithosphere (which we now use instead of "tectosphere") is defined strictly kinematically, not rheologically and also not lithologically. If you do not explain what makes you decide to draw the base of the tectosphere here or there, the reader cannot understand your point. Yes, see criteria and new line references above.
- Figure 4: same. Why do you decide to draw the base of the European tectosphere there? What is so special at that depth? On section 2, the line crosses the red patch below the blue patch, why there? We add an explanantion that in regions where the base of the lithosphere is unclear, it is interpolated between regions where it is clearly imaged aand has the same pre-Alpine history (**new lines 961-968**).
- In general, this part is too difficult to read. OK, you were tired and pressed for time.
- Figure 5: please add the number of the profile (16) in the caption of panel C. Done
- Figure 6: the base of the European tectosphere seems just to go through anything, this is annoying. The base is shown as a dashed line, meaning that it is unconstrained by seismological observations and drawn to be compatible with the along-strike structure of the Variscan-age Alpine foreland (**opening paragraph of section 5**).
- Line 420: why are "classical" sections of the Alps only those published by Schmid et al. ? Same for seismological interpretations of the deep structure those of Pfiffner ? This is a misunderstanding: we are NOT saying that the interpretations of Schmid and Pfiffner are classic, rather that the section across the Central Alps itself IS classic, meaning that it has been studied many times in the course of Alpine research (e.g., Argand, Staub, etc.). Without citing all these old studies, we have referred to Schmid and Pfiffner as examples

(e.g.,…) because they authored the most recent studies along this section across the Central Alps. This meaning is clear for native speakers and those who read carefully.

- Lines 423-463: this part is highly speculative and the reader is not given any hints about why it should be as you say. I do not mind speculations, but they should be presented in the discussion, not here, and you should better explain how you see anything about composition on these images. The lateral continuity of the velocity layering and the dipping domains of positive anomalies are observables (see old lines 282-287, **new lines 62-64, 553-555 and 753-755**, Fig. 7A which shows just the observations and is uninterpreted), rather than interpretations. Interpretations are presented later in this same section in connection with Figs. 7B and 7C. A discussion about the effects of anisotropy and composition is now expanded in section 6.3.
- Line 462: what do you call collision in the Alps? 40-32 Ma is an important period but continental units have been subducted earlier. Yes, for example, in the Eastern Alps (Koralpe-Wölz unit, 90-110 Ma) which is really a separate orogen because is related to the closure of the Neo-Tethys, or in the Sesia Zone (70-85 Ma), which is an anomaly because it was subducted and exhumed just before the onset of subduction of Piemont-Liguria (e.g., Babist et al., 2006; Agard & Handy, 2021). To be clearer, we have substituted "collision in the Alps" (a spatial term) instead of "Alpine collision" (a temporal term), **new line 1101**.
- Figure 7: see my point in the general considerations in the first part of this review. The interpretation of the European vs Adriatic nature of the slab does not say anything about tectosphere vs lithosphere. Agreed. We have already commented on this above.
- Line 526: no, this is certainly not "evident" on the images. Agreed. We now specify "evident" to pertain to the Central Alps (**new line 974**).

Here I gave up and jumped directly to the discussion

- Line 838: what do you mean by "equilibration of the slab" ? This is unclear. Agreed. We have amended the text to read more simply: …during or after northward Adriatic indentation and slab detachment in Neogene time (e.g., Ratschbacher et al., 1991; Favaro et al., 2017), **new lines 1366-1368**.
- Lines 880-890: what are the precise arguments here to say that water content rather than temperature is more likely to influence seismic velocities. I have nothing against it, by what is the point. We have clarified the sentence to read: "In view of the fact that water content **in addition** to temperature influences seismic wave velocities in the mantle (Karato and Jung, 1998; Shito et al., 2006)..." (**final paragraph of section 6.3, new lines 1474-1480**)
- Lines 894-895: here, this is circular reasoning. The sink rate of van der Meer et al. can only be taken as average values, it certainly cannot be used for a given region without a long discussion. In the upper mantle, flow rates of the mantle have to be highly variable, and they can be much higher than 1.2 cm/year. And flow is certainly not only vertical. We state at the outset of this sentence (**new line 1483-1484**) that the estimates of slab sink rates are "rough" (means very approximate), and go on to discuss why they are rough and how the estimates yield minimum times since detachment of the sinking slabs (section 6.4, **new lines 1486-1489**). This is not circular reasoning.
- Section 6.4 in general: you discuss the influence of slab detachment on the tectonic evolution of the Alps. Fine. But you do not mention slab retreat (or delamination, which

is equivalent) that may have partly very similar consequences. Focusing of slab detachment only is misleading, I think. And this becomes very important when you discuss the ages of magmatism. Retreat and detachment can also go together, the discussion should be somehow more subtle here. Agreed, good point. We have added a sentence emphasizing a point already made in the text that the European lithosphere, especially beneath the Eastern Alps, delaminated before dropping off (**new lines 1487-1489**). The amount of delamination is probably greater in the Carpathians where roll-back subduction is well-documented. In the Conclusions section, we already referred to delamination leading up to detachment.

- Line 943: please avoid using words like “exciting”. OK

Mark Handy on behalf of all co-authors