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).

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. 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. The reference frame used is the Norhtern 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 originally 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. It is true

that this criterion cannot be applied clearly in cross sections across the Eastern Alpine foreland (e.g., Figs. 4B, 6A), which we also discuss. 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 the reviewer means by "…either European or continental" (presumably he means "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.

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 and discuss these consequences in the following section. They should also clearly say the differences between tectosphere and lithosphere. In my understanding there is no difference whatsoever. As stated above, the difference lies in the heterogeneity expressed by the occurrence of both +Vp and –Vp anomalies in coherently moving pieces of mantle. I do not see why you need this "new" concept (which is not new, Please, 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" as proposed by 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 "tecto" 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 in passing to Jordan's idea of chemical/mineralogical gradients to explain the longevity of thermal anomalies in the mantle.

- 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. Good point. This applies primarily to profiles across the Eastern Alpine foreland. 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 limits that define the base of our plates are not arbitrary, but based on considerations of coherence with respect to the Northern Alpine Front, plausible thickness of down-going pre-Alpine continental lithosphere (certainly more than the observed 80 km in our images) and the thermal significance of -Vp anomalies within coherent layers of mantle (revised section 6.4).
- 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: "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. See criteria and line reference 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 now add that the base of the lithosphere is extended along strike between regions where it is clearly imaged.
- In general, this part is too difficult to read. No comment. It's unclear what you mean here.
- 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 classical, rather that the section across the Central Alps itself IS classical, 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 (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.

- 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 barely related to Alpine Tethys, or in the Sesia Zone (70-85 Ma), but the Sesia Zone 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).
- 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.

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 simply: …during or after northward Adriatic indentation and slab detachment in Neogene time (e.g., Ratschbacher et al., 1991; Favaro et al., 2017)..
- 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 (final paragraph of section 6.3): "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)..."
- 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 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). 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. 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