Response to Review #2 by Andrea Argnani

General comments

The paper of Le Breton and co-authors aims at presenting an up-dated kinematic reconstructions of the Alpine-Mediterranean area, with a focus on Corsica-Sardinia-Adria, implemented within a recent global plate model. Kinematic scenarios are tested for geodynamic consistency using thermo-mechanical modelling of the rifting phase and compared with geological records from the Alpine region s.l. Some interpretative choices strongly mark the paper. I am not commenting the adopted motion of Iberia, a debated issue with various models present in the literature, but the choice of having the Corsica-Sardinia block attached to Iberia is not the most popular. Besides the palaeomagnetic evidence which is against it, it seems that this choice causes some inconsistencies in the presented kinematic reconstructions (Fig. 4). Some are described below. but the most relevant is the necessity of displaced northern Adria along a lithospheric fault for which there is no geological evidence. The reconstructions by Le Breton et al. present significant critical points that perhaps should be better discussed and compared with previous reconstructions, but above all they should be better supported by geological evidence. For this, I think the paper requires substantial revision work. Some of the critical issues are in the specific comments below.

We thank Andrea Argnani for the constructive comments and suggestions. We know that the model we choose for Sardinia-Corsica (and Iberia) is debated. We have made significant changes to the manuscript to address this issue, present alternative scenarios and arguments to support our kinematic model. The main argument is that other models based on paleo-magnetic data such as the recent one of Van Hinsbergen et al. (2020) would predict more than 500 km convergence between Iberia and Sardinia, of which there is absolutely no geological evidence. To avoid this, we rather overestimate the amount of strike-slip along existing strike-slip faults. Following your review, we changed the location of the fault within Adria to the Mattinata Fault, to follow strictly the proposed model of Schettino & Turco (2011; see answer to your specific comment on that point below). We acknowledge that the 230 km of strike-slip fault is very likely an overestimation. Future work should look at more diffuse intraplate deformation within Adria, as well as Iberia, which may help solve the debate on the paleo-magnetic data.

Despite this limitation in our kinematic reconstructions, we would like to stress that this motion of Iberia (post-145 Ma, opening of Bay of Biscay; Tugend et al. 2014) and within Adria (100-40 Ma; Schettino and Turco, 2011) does not influence the opening of the PL Basin which is earlier (200-145 Ma) and therefore does not change our main results and interpretations.

Specific comments

Section 2. Geological setting **line 80**: The microplate nature of Adria is debated. It not certain whether, when and for how long it was a microplate. Perhaps using just the term Adria avoids any questions.

Changed “microplate” to “plate”, when referring to present-day setting (line 90). When going back in time, we only refer to Adria.

lines 127-128: It is not clear whether the Brianconnaise is considered a microcontinent (as CSB) or as an extensional allochthon. It should be clarified.

The exact nature/former thickness of the Brianconnais is poorly constrained and discussed, we would rather keep the following formulation: Briançonnais continental "unit", as recorded today in the Alps (lines 146, 148).

lines 131-132: I believe that alternative interpretation are equally possible, without kinematically linking the Bay of Biscay and the Valais.

Alternative scenarios for the opening of the Valais have been added in section 3.1 and illustrated in the new version of Figure 3.

lines 153-156: from the reconstructions in Fig. 4 it seems that the Valais ocean opened at 130 Ma; the Jurassic opening is not obvious. Moreover, the location of the Valais to the north of the CSB looks a bit strange. This domain is still there at 83 and 67 Ma and even at 35 Ma, always between Eu and the CSB. I am not aware of evidence of remains of an ocean in Provence. Moreover in the 35 Ma frame a N-ward subduction is present north of the CSB; that subduction disappears in the subsequent 20 Ma frame (are there remains of it somewhere?), where subduction jumps to the south of the CSB.

Extension during rifting is not shown on Figure 4 (see text and figure caption: "Note these maps do not show intraplate deformation and rifting along the continental margins but the divergence between plates when considered rigid"). We don't consider the Valais Basin as a mature ocean but an extended continental/transitional crust (section 2.1.2). We checked and corrected the map to show only divergence between Europe and Iberia-Sardinia-Corsica. However, please note that the exact location of divergence/extension depends on where and how we draw the plate boundaries. This is not the scope of our study and is represented only in a simplified manner on Figure 4.

The thrust zone indicates the Pyrenean compressional deformation that affects also Provence until the Eocene. There are numerous remain of this contractional deformation in Provence with E-W trending thrusts and folds and syn-tectonics foreland deposition (e.g. Andreani et al. 2010; Espurt et al. 2012; Bestani et al., 2015, 2016; references added to the text).

line 162: why subduction initiation is intra-oceanic? Most authors consider that subduction initiated at the southern margin.

We rephrased this paragraph and focus on the first record of convergence between Adria and Europe between ca. 130-84 Ma (Eo-Alpine Orogeny), within the Eastern Alps (Austroalpine units) that record and intracontinental subduction zone, with sinistral component linked to subduction of Neo-Tethys to the east (see references in the text, lines 173-179).

We agree, the "Alpine" south-directed subduction initiated later at about 84 Ma on the southern margin (Sesia zone).

Lines 196-197: the extension in the Strait of Sicily may not reflect the motion of Adria, but it could be related to the dynamics of the subducted African slab (e.g., Argnani, 2009 SP Geol. Soc. London).

We agree and added a sentence and reference to this paragraph (now in section 3.4, lines 340-341).

Section 3.3 MATF: There is some confusion when describing the tectonics of the Adriatic Sea. The boundary inferred by DAgostino et al. (ca. E-W trending) is just sketched and is not related to a specific structure or set of structures. It refers to the present tectonic activity, and that's why a proper boundary is not yet developed... if there will ever be a plate boundary. The Mid-Adriatic Ridge of Scisciani and Calamita is NNW- SSE trending geological feature. It was previously named Central Adriatic Deformation Belt (Argnani and Frugoni, 1997) and is a belt of foreland inversion structures where the Adriatic seismicity tends to concentrate. It has also been considered as the in- land continuation of a major transform fault (Argnani, 2009, Bull Soc Geol It).

There is no evidence, however, of a major strike-slip displacement, and Scisciani and Calamita describe inversion not strike-slip structures. The age of this deformation ranges from Quaternary to possibly Eocene, whereas the activity of the large strike-slip fault (MATF) used in the reconstruction ranges from Late Cretaceous to Eocene. In addition, the authors use the poles of Northern Adria from Schettino et al. which considered Adria decoupled along the E-W-trending Mattinata Fault, located in the southern part of the Gargano promontory. This fault shows evidence of Paleogene activity (Argnani et al., 2009, GSA) but the amount of displacement is unlikely to be on the order of 100s km.

We corrected the location of the strike-slip fault to the Mattinata Fault across the Gargano Promontory as proposed by Schettino and Turco (2011). We follow their reconstructions for the motion of Adria (modified only for the post-20 Ma time period; Le Breton et al., 2017) as this avoids the overlap problem of Adria with Corsica-Sardinia. Indeed, our model for Sardinia is not the one recently proposed by Advokaat et al. (2014) and Van Hinsbergen et al. (2020), which uses paleo-magnetic data on Sardinia. However, their model implies more than 500 km convergence between Iberia and Sardinia (please see new version of Figure 3), for which there is no geological evidence of. We thus prefer to have a significant strike-slip motion within Adria and between Europe and Iberia-Sardinia-Corsica. We agree that the amount of strike-slip motion (230 km) implied by the Schettino & Turco (2011)'s model between 100-40 Ma is most likely over-estimated and was very probably distributed along several fault zones. However, this is poorly constrained and requires future work as mentioned in the text. However, we would like to emphasize that this strike-slip motion does not influence the kinematics of opening of the PL Basin which is much earlier, in the Jurassic, and which is the main scope of this paper. We also discuss this assumption in section 5.3 where we compare our plate convergence estimates with other kinematic models that do not have this split of Adria, and we show that our estimates can be considered as a maximum.

We improved the text accordingly in section 3 (lines 387-409) and 5.3, added geological description and the suggested references to describe the Mattinata Fault and Mid-Adriatic Ridge (or Central Adriatic Deformation Belt).

Section 3.4 From Corsica to the west there is no evidence of an Alpine subduction followed by an Apennine-Maghrebian subduction. The two-subduction model adopted is one of the possible interpretations and other authors, more or less explicitly, prefer two opposite-vergence subductions since the beginning of convergence, handling in different ways the interpretation of Alpine Corsica (e.g., Jolivet et al., 1998, Argnani 2012 Tectonoph.) Critical in the two-subduction model is the presence of the AlCaPeCa (micro) continent, that is represented at 67 and 35 Ma, with a size of about twice the Corsica-Sardinia Block. I found intriguing that the CSB is still almost intact within the Mediterranean, whereas the larger AlCaPeCa micro continent has been completely dismantled.

We mentioned this debate in the text and added the suggested references (lines 433-434).

line 380: explaining the Eo-Alpine orogeny using an intra-continental subduction that is part of strike-slip system linking the PL to the Vardar seems an ad hoc solution not based on evidence. (also for **lines 407-408**)

This was proposed based on field evidence from the Eastern Alps (Schuster and Frank, 1999; Neubauer et al. 1999; Frank and Schlager, 2006; Schmid et al. 2008; Stüwe and Schuster, 2010; these references are in the manuscript).

Section 4.3 The authors take a geological section across the southern Alps to constrain the initial with of the rift, which is estimated to be ca. 300 km. This rift system has a Beta < 1.5, which is typical of continental rifts that do not evolve to oceanization. As described by many authors, including Froitzheim and Eberli, the rifting leading to oceanization is located further to the west, in present coordinate. Assuming that also this sector was affected by the first stage of rifting, the initial width of rifting was larger than 300 km. Otherwise, the western sector was affected only by the second stage of rifting, but this does not fit the inferred evolution which is at the base of the numerical modelling. This is actually a minor point for the paper, though the mechanical behaviour that controls the location of oceanization is an interesting issue.

We agree, the rift localized more to the west as shown on figure 6 and later in our modelling (section 4.4, figure 7). We used this section through the Southern Alps to estimate approximately the area affected by extension between Corsica and northern Adria. We agree that this is an absolute minimum, as this section represents only the preserved proximal part of (one side) the rift. We added this statement to the text (lines 535-536). We note that this minimalistic approach is completely independent from the geodynamic modelling done in Section 4.4. The geodynamic modelling is based on the amount of net divergence between Corsica and northern Adria in our kinematic reconstructions.

Section 4.4 The portions of exhumed mantle that crop out in the Western Alps are considered of subcontinental origin (e.g., Piccardo and Guarnieri 2010, Int. Geol. Rev.), as in type 1 margins of Huismans and Beaumont, 2011. Does that fit with the result of the numerical modelling? In the model of Fig. 7 it seems that it is a newly formed lithospheric mantle to be exhumed.

Modelling subcontinental mantle exhumation is still an open challenge for the modelling community. We added a paragraph and references to address this point (lines 622-628):
"A subcontinental affinity of the exhumed mantle has been inferred from petrological analysis in the Western Alps suggesting diffuse porous flow of asthenospheric melts through the continental lithospheric mantle as a key process (Piccardo and Guarnieri 2010, Int. Geol. Rev.). Similar to other recent modelling efforts (Hart et al., 2017; Jammes & Lavier, 2019; Andrés-Martínez et al. 2019), our numerical models do not capture porous flow processes and are therefore not comparable with this type of observation. However they show that the asthenospheric mantle resided close to continental mantle lithosphere prior to exhumation, which might be indicative of continental mantle affinity."

Section 5.2

lines 600-603: the opening of the Ionian basin in a NW-SE direction, with Malta and Apulia escarpments acting as transform faults, contrasts with using the Malta and Apulia as conjugate margins.

Yes, we agree that the Malta and Apulian escarpments were not conjugate (e.g. Frizon de Lamotte et al. 2011 and Tugend et al. 2019), but those escarpments were nevertheless formed during transtensional opening of the Ionian Basin. What we meant is that we used the width of the basin between these two escarpments to reconstruct the opening of the Ionian Basin. We changed this sentence accordingly (line 674).

Section 5.3 The max. width of the oceanic domain in PL is taken as 250 km, based on the results of numerical modelling that also sets a length of 120 km for the hyperextended domain (80 + 40 km) and a length of 110 km for the necking domain (65 + 45 km). This gives a width of 480 km for the PL basin. Plate kinematics describe 680 km of convergence, that is subdivided in 420 km subduction and 260 km collision, based on the geologically inferred age of subduction and collision. With subduction initiating at the SE PL margin, and assuming that subduction occurs at the NW tip of the necking zone, after 370 km the conjugate necking zone is entering subduction: would that be considered onset of collision?

That's indeed a good point. Here, we distinguish between continental subduction and continental collision and define this clearer in section 2 (lines 191-204):
"The exact timing of onset of collision in the Alps differs depending on the criteria used. For instance, continental units, such as the above mentioned Briançonnais, entered the trench and were subducted prior to 35 Ma. However, here we distinguish continental subduction, in which rifted and thinned continental lithosphere behaves similarly to oceanic lithosphere, from continental collision, where slab pull is out-weighted by the positive buoyancy of the (less rifted) continental lithosphere following slab break-off and detachment of the subducted lithosphere. Timing of slab break-off in the Alps is inferred from timing of magmatism along the Periadriatic Line, mainly between 34-28 Ma (Rosenberg, 2004). Indeed, the geochemistry of these magmatic rocks indicates melting of lithospheric mantle, best explained by a slab break-off event (von Blanckenburg and Davies, 1995). Moreover, this time period (35-30 Ma) is also marked by a change in sedimentation from "Flysch" to "Molasse" (Rupelian Lower Marine Molasse, 33.9-28.1 Ma) in the Alpine Foreland Basin (Matter et al., 1980; Sinclair, 1997), and onset of medium temperature – medium pressure Barrovian-type metamorphism within the orogen (Lepontine dome, Tauern Dome; e.g. Bousquet et al., 2008) attributed to thickening of the European crust (Venediger Duplex formation in the Tauern Window; Scharf et al., 2013b). Thus, 35 Ma appears to be a reasonable time for onset of collision in the Alps, as defined above."

The numerical modelling is reproducing the various elements of the PL margins, and continental material was certainly subducted. However, I suspect that mixing geological dates of subduction and collision, amount of convergence from plate kinematics and time frames from numerical modelling may lead to quantitative results that are not really representative. How representative is the estimate of the amount of subducted continental material?

The amount of plate convergence we provide is based on the kinematic reconstruction and thus is quantitatively constrained. We only split the amount of plate convergence and discuss its variation in two time periods that we think represent different modes of convergence: 1) subduction with slab pull and subduction to high-pressure metamorphism which mostly occurred between 84-35 Ma in the Alps, to 2) collision after slab break-off event at about 35 Ma (see previous response above).

We clarified this in the text (section 5.3, lines 685-689).

Figure 4. Some of the reconstructions are puzzling, and many features are not really supported or described in Section 3. Sudden shifts in plate boundaries among frames seem not always justified.

The frames from 130 to 35 Ma, in particular, present some intriguing aspects. From 130 to 83 Ma a major plate boundary rearrangement is depicted, with a system of large strike-slip lithospheric faults in the Adria region. I don't see much geological evidence for these features, and the connection between Alpine Tethys and Vardar looks a bit forced.

130—84 Ma is the most difficult period to reconstruct, the motion of Iberia and the onset of convergence between Africa and Europe must be accommodated somewhere. There is evidence for intracontinental subduction and sinistral strike-slip deformation within Austroalpine units (please see references above and in our revised manuscript). To link it with sinistral motion along the NPF makes kinematically sense, as previously proposed (e.g. Handy et al. 2010, and other references quoted in the manuscript), and avoid major problem with convergence between Iberia and Sardinia (as implied in model based on paleo-magnetic data; see new version of Figure 3).

The Valais ocean is positioned between CSB and Europe; such a reconstruction is difficult to support as described above.

The MATF that is represented in the frames 83 and 67 Ma has also some problems. as commented above.

Please see responses above, corrections to Figure 4 have been made accordingly.

Minor corrections

Strike slip motion along various fault systems is often mentioned throughout the text; the sense of motion, however, is almost always not indicated. Where possible this is a useful indication.

The two main strike-slip fault systems mentioned in the text are between Europe and Iberia-Sardinia-Corsica during the Cretaceous and within Adria during Cretaceous and Paleogene times, both are sinistral, which is now clearly indicated in the text and figure (lines 62, 402-405, Figure 3.1, Figure 4).

line 23: does the 250 km width refer to the truely oceanic part or does it include the exhumed mantle and hyperextended margin sectors too? The term ocean can be ambiguous. It looks it refers to the truly oceanic part, but this point should be clarified.

Yes, the 250 km represents the width of the mature ("true") ocean (not transitional), we clarified the text accordingly (line 24; lines 71-72; line 746).

line 86: Gawlick and Missoni is not in the References list.

We added the reference to the list

line 90: Channell and Kozur 1997 should be cited as an early paper describing the oceanic branches.

We added this reference.

line 371: Handy et al is 2015

line 382: Handy et al is 2015

line 878: Handy et al is 2015

This paper of Handy et al. was first published online in 2014, then in the Issue of January 2015, which brought confusion in our referencing system. We updated the citation as indicated on the website of

Springer (2015).

line 460: "154-145 Ma and... 145-130 Ma". Ma and not km

corrected

line 470: indicate which is the Brune et al 2017 cited

2017a, added

Fig. 5: slight increase in velocity: "light to dark blue" instead of "dark to light blue"

We corrected this sentence (increase from NE to SW, light to dark)

Sincerely

Eline Le Breton, on behalf of all co-authors