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Discussions

Interactive comment on "Kinematics and extent of the Piemont-Liguria Basin – implications for subduction processes in the Alps" *by* Eline Le Breton et al.

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Dear editor, dear authors,

Hereby I provide my review of the paper by Le Breton et al entitled 'Kinematics and extentofthe Piemont-LiguriaBasin–implications for subduction processes in the Alps'.

Le Breton and colleagues make a kinematic model for the western and northern, and a bit of the northeastern Mediterranean region and display it as GPlates files. The Mediterranean region is complex and therefore an interesting challenge to reconstruct, and many people have attempted this before, and I understand the desire of the authors

to also give it a shot. I have recently done this as well, using a reconstruction protocol to restore paleogeography from kinematic constraints from orogens, and I therefore read this paper with interest to see what this team of authors, with a strong track record in plate reconstructions, would make of it.

I find it difficult, however, to see why this paper would be better than previous attempts to restore the Mediterranean region for the following reasons. 1. The authors provide no reason in their introduction why they found it necessary to make a new reconstruction and how their reconstruction systematically differs from previous attempts, or why they expect fundamentally different conclusions for the evolution of the Alpine Tethys with their approach. 2. The paper cites almost no geological data, but instead hinges almost entirely on interpretations and models. They don't describe the geological record of the Alpine Tethys, but the interpreted paleogeography. In my detailed comments below, I point this out for specific cases. For instance, they describe 'en echelon transtensional basins' in the Pyrenees. That is not what you see in the Pyrenees, you see a faulted, folded, intensely deformed, partly metamorphosed bunch of rocks that were interpreted to have been deposited in such basins. They describe a distal passive margin, or a hyperextended ocean floor, but the underlying observations or even the location where those are made are not specified. The model of Iberian motion is given as fact before alternatives and discussions are provided. This makes it very difficult for a reader to judge the validity of the final model, which to me rather seems a model based on selected models. 3. But my main problem lies in the inconsistency in argumentation. I believe this is best illustrated by the following example. The Valais Basin in the Alps is interpreted from an intensely deformed and metamorphosed series of deep-marine Mesozoic sediments and associated mafic and ultramafic rocks. These mafic rocks have crystallization ages estimated with geochronology at Jurassic to Early Cretaceous consistent with the sedimentary record, which is what the authors interpret as the age of hyperextension. ArAr ages of these same rocks nowhere give Jurassic or Cretaceous ages, but Cenozoic, which they (and everybody else) interpret as related to exhumation following burial. Regional HT metamorphism is then taken by the authors as evidence for slab break-off following collision. In the Pyrenees are also ultramafic and mafic rocks interpreted to reflect hyperextension. These are associated with Jurassic Sm/Nd ages and such Jurassic extension also follows from rifting recorded in the Bay of Biscay as well as in Pyrenean stratigraphy. Also here, ArAr ages are considerably younger, and follow upon HT metamorphism. In analogy to the Pyrenees, we have argued that the HT metamorphism is related to slab break-off following collision, pointed out where the slab is currently residing, and showed that that model is fully consistent with interpreted magnetic anomalies and paleomagnetic data. But the authors prefer for the Pyrenees to discard the evidence for Jurassic rifting, the magnetic anomalies, and the paleomagnetic data without explaining these data, and instead interpreting the HT metamorphism as hyperextension related. 4. As a result of the models they follow for the Pyrenees, they make a model for Iberia that devoid of any quantitative kinematic constraints (all constraints are discarded), from which it follows that also their reconstructed Piemonte-Ligurian ocean is does not have a quantitative basis.

The model in this paper is mostly a compilation of previous models, and in my view adds little to the discussion. I don't mind if it's published, I disagree with this model as much as with the previous ones for reasons I published before and repeat in my review. The authors do point out, however, that my model predicts shortening between Sardinia and Iberia during Iberian rotation, for which I must have become blind when I was working on my version: there is indeed no evidence for this, and it bothers me, so I'll research it and see how I can solve it. But I believe that solving that should not result in ignoring all paleomagnetic data and invoking a 230 km strike-slip fault through Adria for which there is no evidence (and which, if I may add, is not a simplification as you respond to Stefan, but rather a complication).

The detailed comments below may help the authors to clarify their paper and bring their new message across better. I don't expect the authors to agree with me, but I do ask them to explain the basis for discussions, rather than to just choose models based

on other models.

Douwe van Hinsbergen Utrecht, November 3, 2020

l. 16: Should a kinematic model be geodynamically 'consistent'? Suppose that data show a kinematic pattern, but you're not able to model it because we currently lack the conceptual understanding of the process, should we then adjust the kinematic model? Or learn something about the process? I believe that kinematic models should be boundary conditions for dynamic models, not the other way around, don't you?

l. 22: What is a bit confusing is that you use a paleogeographic rather than a tectonic definition of the Piemonte-Ligurian Ocean. Paleogeographically, it is an oceanic basin that bounds Adria to the west and north. But plate tectonically, it contains three plate boundaries: Adria-Iberia, Adria-Europe, and Europe-Iberia. So the spreading has different rates and timing and direction between Iberia and Adria, and between Europe and Adria, and it may be a good idea to treat those separately. Iberia is for instance irrelevant for the opening of the Valais-Magura ocean.

l. 25: How do you subdivide between subduction and collision? There is only subduction, of oceanic and continental lithosphere, and then convergence stops, or flips to Adria subduction/underthrusting. Collision is a rather vague term, since accreted oceanic and continental units behave similar and together form one nappe stack stripped from its lower crustal and mantle lithospheric underpinnings.

l. 25: How did you measure these distances? Net Adria-Europe convergence between 84 and 0 Ma is about 550 km or so, so are your distances measured along the motion path? Or do you invoke extension between Adria and Africa in the Late Cretaceous-Cenozoic? And if you measure along the motion path, then why do you start measuring at 84 Ma and not before? There is extensive evidence in the eastern Alps for nappe stacking and subduction since the Early Cretaceous already.

l. 26: Does extended continental material subduct or collide in your terminology? And where do you put the difference? In addition: much of the oceanic portion subducted without accretion, so of the accreted material probably much less than 60% is derived from hyperextended crust I presume?

l. 27: What is your definition of an ophiolite? Ophiolites in the sense of Oman (or hyperextended versions with much thinner crust like in the Alps or southern Tibet) are upper plate-derived, and hence do not have to be exhumed. They're the uppermost structural unit. Accreted ocean plate stratigraphy, like the Valais sediments or the HP-LT metamorphic units derived from the PL Ocean, may exhume but are offscrapings from subducted crust. Do you call all these units ophiolites, or do you make a subdivision?

l. 28: Can you indicate what this importance is? Those margins just subduct and accrete their sediments to orogens right?

l. 34: If it subducted, there's no record of it. If there's a record, it accreted.

l. 39: I think the work of Geoff Mohn is also relevant here.

l. 40: But it didn't, did it? The highest structural unit of the western Alps are ophiolites, and the highest accreted units are PL ocean-derived, so subduction in the western Alps formed within the PL ocean, perhaps at the previous slow-spreading ridge or so, not on its hyperextended margins. In addition, plate motion went from extension to transform to subduction didn't it? So the 2D concepts of rifting and inversion don't really apply here.

(l. 44: I'd drop 'very' twice, text becomes more convincing if you don't use such emphasis.)

l. 45: Are there multiple episodes of slab break-off? I think there's only one, with one slab right? Davies and von Blanckenburg are relevant here.

l. 46: Why is it difficult (don't use the emphasis) to interpret? I find it very easy to interpret (although that interpretation may be wrong). And why is the curvature or the lack of a magmatic arc important to interpret tomography? Without that interpretation,

how do you know how many phases of slab break-off there were?

l. 47: Is it? Numerical modeling is a key tool in understanding the drivers of processes that of which we documented the result. Numerical models in no way constrain the structure or evolution of the Alps, nor how we should interpret seismic tomography. Numerical models are only helpful in interpreting what may have caused features that we documented. The papers you cite are not about the Alps, but about processes in which some of these papers use a highly simplified setup that they interpret to be somewhat relevant for the Alps.

l. 50: Which models need that, and for what purpose? If the direction of plate motion should be taken into account, then all numerical models for the Alps or Pyrenees that do 2D experiments go out the window since neither orogen resulted from 2D motion. The input parameters required for a numerical experiment depend highly on what that experiment tries to resolve.

l. 55: This introduction is a bit unfocused: how do overpressures change kinematic models? I have never used the inferred depth of burial in a kinematic reconstruction, but only structural overlap between nappes, and the stratigraphically constrained time interval of its subduction combined with constraints on plate convergence. Those are always enough to explain documented pressures, with or without overpressure. Do you use these pressures for your reconstructions, and if so, how do you date climax pressure and how do you correct for slab dip, etc?

l. 60: So is this paper about hyperextension, or the role of thinned crust in subduction, or overpressure? Or just kinematic reconstructions?

l. 64: During the Alpine orogeny, or during orogeny in the Alps? The Alpine orogeny is much larger than just the western Alps (you seem not to include the eastern Alps?)

l. 66: See earlier comment: I think you should check whether interpretations of geodynamic models are consistent with kinematics, not the other way around.

l. 76: This is not really a geological setting, but rather an overview of current interpretations on plate tectonic history. In addition, it is important here to identify the difference between Iberia-Adria and Europe-Adria motion. You seem particularly focused on the Alps part (Adria-Europe), but you also mention Alpine Tethys units from the Apennines-Maghrebides-Alboran region (Adria-Iberia).

l. 80: Adria is arguably part of a separate microplate today (but with a diffuse southern plate boundary), but for most of its history it is not a separate plate, also not in your reconstruction. So what do you refer to here, the continental realm of Adria?

l. 81: In the Alps, Adria is presently also downgoing plate, with the Southern Alps as offscraped crustal relics, isn't it? E.g. Ustaszewski et al 2008?

l. 81: it is not entirely surrounded by orogens today, the Adria continent has a passive margin in the south, bordering the ocean basin underlying the Ionian Sea.

l. 87: The upper plate is also Adriatic continental crust, so from this it follows that you consider Early Cretaceous subduction to occur within the Adriatic continent? I think that's inevitable, but it's not often explicitly mentioned this way.

l. 90: The closure of the Neotethys started in the Jurassic (170 Ma metamorphic soles and SSZ ophiolites in the Dinarides and Hellenides), but its closure didn't finish until the late Cretaceous.

l. 101-109: What are the underlying observations of this interpretation, and where were they made?

l. 110: where are these cherts found? What is the observation that shows that the cherts were deposited on a distal margin? What does the 166$\pm$1 Ma age represent, cherts in a coherent sedimentary sequence from clastics interpreted as syn-rift to open marine chert sedimentation? Or are these simply the oldest reported cherts?

l. 112: Vissers et al J Geol Soc 2017 reported ArAr ages from shear zones in Cap de Creus that they interpreted as extension-related normal faults in the Iberian margin.

These ages are 175 and 159 Ma, consistent with ages Etheve et al 2016 for extension in the Gulf of Valencia. So the end of rifting may not everywhere by 166$\pm$1 Ma.

l. 119: See also the ages in this range in the Betics (e.g., Puga et al 2011 and refs therein).

l. 128: the Jura mountains are not the Alps right? The Brianconnais became separated from Europe by the Valais ocean basin, so is not the European margin.

l. 131: No, this is not commonly accepted, this is commonly speculated. But this can be tested, through paleomagnetism, and those data are grossly inconsistent with that speculation. Why do you present this as a fact, ignoring our arguments for an alternative? It's fine if you disagree, but it's not that we just threw out some nonsensical armwaving that is best shoved under the rug I believe.

l. 136: Sorry, but please provide the geological documentation instead of the armwaving. The Organya basin is bounded to the south by what is now the Montsec thrust, and which originally was a listric normal fault if you restore the stratigraphy. This is not a pull-apart basin. The North Pyrenean fault is a post-84 Ma structure along which oceanic/hyperextended units are exposed, and it is far from certain what its structural evolution is. The 'en echelon pull-apart basins' is very much an interpretation of currently highly deformed relics within the Pyrenean fold-thrust belt. To give the reader an opportunity to make up his or her own mind, I would like to ask you give the geological facts alongside the major interpretations. I also don't understand why you present this model as a fact, and ignore the more recent (data-based) discussions on Iberia as well as its connection (or absence thereof) to Sardinia-Corsica, and hence Brianconnais.

l. 140: But this is not required by that stratigraphic age. The opening of the Valais ocean is easily explained without a connection to Iberia, in a way that is consistent with paleomagnetic data.

l. 165: The Austro-Alpine units are not intra-oceanic. They're entirely intracontinental.

They're continent-derived nappes shoved underneath Adria. The oldest HP metamorphism that I am aware of in PL-derived units are 83 Ma or so on Corsica, and the Sesia fragment (also continental) gives an 85 Ma or so age from HP units.

l. 170: Handy et al 2010 is relevant here.

l. 176: Why is the underthrusting of the European margin 'collision' and of the Brianconnais continent 'subduction' if both are thrusted below an upper plate, make nappes, and cause metamorphism?

l. 178: Is collision depending on whether a basin is under- or overfilled? When is a basin overfilled? Because there is Paratethys waters in the Alpine foreland into the Miocene, is that basin that underfilled? Why does HT metamorphism signal 'collision'? There is HT metamorphism in Japan, so is there collision there? Do you need slab break-off for a collision? There was slab break-off in California, but there is no collision. Sorry to fuss about details, but there is a large basket of apples and oranges here to call something by a term of which the implication is unclear.

l. 180: Also: where else are you referring to?

l. 181: No, there is upper plate extension in the Miocene. The 'roll-back' subduction started well before, like in the Alps, but was at the same rate as upper plate advance.

l. 181: What is indentation and how does it differ from collision?

l. 184: Can you explain what you mean here? It is kinematically decoupled because of the opening of the Pannonian Basin, or because of a plate boundary in the Dinarides?

l. 23: All of the previous in this paper were kinematic scenarios (without the debates). I suggest you present the kinematic scenarios first, and then provide the facts, or the other way around, but in the previous 6 pages you have presented models and scenarios as geological facts, so what is a reader supposed to get from the rest of the paper? I have a hunch what your conclusions are going to be, regardless of the debates that you are going to outline. You have already described your model above.

l. 191: For which time interval? Adria is at present moving relative to Africa, has moved relative to Africa during Ionian Basin opening. And at other times it did not.

l. 194: This is a strange argumentation. There is little deformation in that basin because data show that there is little deformation in that basin, not because it is oceanic. If it is oceanic and there is still deformation, then there is still deformation.

l. 195: There is a rich debate on the opening of the Alpine Tethys. I followed Speranza, but he is far from the only one who has made an interpretation of this.

l. 197: How limited?

l. 203: 118 Ma according to which timescale?

l. 204: replace 'Spain' by 'Iberia'. They also come from Portugal.

l. 206: Again, indicate what the basis for this interpretation is. The hyper-extension interpretation is first and foremost based on the presence of subcontinental mantle rocks in the NPZ. And it makes a lot of sense to interpret those as hyperextended. Gabbros in those rocks contain Sm/Nd ages of 170 Ma, and there are Jurassic marine sediments that suggest rifting of that time, but also ArAr ages of $\sim$105-100 Ma HT metamorphism and associated volcanism that affects the mafic rocks and overlying sedimentary rocks. The papers you cite interpret the 105 Ma HT metamorphism as related to hyperextension-related (no such HT records in any other hyperextended margins or ophiolites that I'm aware of), and thus discard the paleomagnetic data (the largest paleomagnetic dataset of any continent, ever) because those are not possible to reconcile with extension in the Pyrenees. None of the authors you cite explain those data. However, those data are explained as HT metamorphism following slab break-off (you know, the argumentation you cite for the Alps as dating collision). I admit that in my reconstruction of Iberia I predict convergence in the PL ocean during rotation – I shamefully acknowledge that I had not realized this and it invites reinterpretation because I am not aware of evidence for this. But this does not mean that the paleomagnetic data are wrong. Data are data. And you seem to throw them overboard without explaining them.

l. 209: Dated how? And how is this relevant for the Alps? The Alps form at a different plate boundary.

l. 213: It doesn't help the debate if you armwave at some possible explanations. Could you explain (because Neres does not) how you can remagnetize rocks across Iberia in such a way that those data give a larger rotation than Iberia ever underwent? And what does 'incompatibility with the GAPWAP' mean? The fact that the Iberian APWP is not the same as the GAPWAP in Eurasia, African, or North American coordinates is the argumentation that it was a separate plate that rotated relative to all three. Nothing of what you write here provides an explanation for the data and does not explain to the reader what the discussion is about. You're just choosing one model over the other without explaining why.

l. 215: The data from Iberia come from all over Iberia. How do you explain a rotation in the Aptian documented in Central Iberia, in SW Portugal, and in the South Pyrenean basins by intraplate deformation possibly along the Ebro Block? How do local rotations in the Ebro block explain that data that are collected all along the 100's of km long Messejena dyke from the CAMP align perfectly with data from dykes in the Moroccan Atlas and from Canada in the Vissers and Meijer fit, but misalign with the Jammes fit (Ruiz-Martinez et al., EPSL 2012). I am entirely open to alternative explanations, but sorry, we're not bringing science forward if we only throw some interpretations on the table and pick a few that we like better.

In addition: if you throw out marine magnetic anomalies from the North Atlantic, as well as paleomagnetic data, you have no quantitative data left to reconstruct Iberia. Jammes etc don't use any quantitative data for Iberia, those reconstrutcions are entirely qualitative. How are you going to make a reconstruction of the Alpine Tethys then, if you have no data to reconstruct Iberia?

l. 220: But how did these authors estimate those widths? I think what you are comparing here are estimates of the amount of extension (Vissers et al) with estimates of the width of the basin (Handy) and those are different things in the first place.

l. 224: Can you indicate what the kinematic constraints were of Vissers and Handy?

l. 225: Nowhere have you indicated that there is a long-lasting controversy on the width of the PL ocean. I don't really think there is one, everyone agrees it's a few hundred km, there's no space for more.

l. 234: Why do you need shortening in the Dinarides to reconstruct the Alps or the PL Ocean?

l. 235: You make it sound like you're the first to use the Atlantic to reconstruct the PL ocean, but everyone has done this so far. Frisch, Stampfli, Vissers, Dercourt, Rosenbaum, Dewey. Why do you need to do this again?

l. 245: Fine, but I believe I have done exactly this in my 2020 paper. And I may be entirely wrong, but it may be informative for a reader to indicate in the introduction what the rationale is to do this again.

l. 270: and violates all constraints from paleomagnetism except for the 5% outliers that Barnett-Moore does find reliable.

l. 273: none of these FT belts constrain Corsica or Sardinia.

l. 277: How does the debate on Iberia affect Adria reconstructions?

l. 287: Well, that entirely depends on your model for Iberia, and since you don't use quantitative constraints for Iberia, you can pretty much choose what you want. Besides, none of this argumentation provides an explanation for the paleomagnetic data from Sardinia.

l. 301: But this creates one hell of a Cretaceous subduction zone between Sardinia/Corsica in the early Cretaceous, as shown by Schettino and Turco. Do you have evidence for that?

l. 315: In your response to Stefan Schmid's comment you call this 230 km of strike-slip in Adria a 'simplification'. But that is not a simplification. It's a complication. It would be a simplification if there are 5 faults with a cumulative displacement of 230 km that you summarize by one fault. This overlap is an artifact of the speculation that Corsica and Sardinia are Iberia.

l. 268: Why do you reconstruct Tisza-Datca in this paper? It's off-topic, and you have provided no information on this region in the paper so far.

l. 369: How can this possibly be derived from the Adriatic plate? The Adriatic 'plate', which is not a plate in most of the time you reconstruct it, is the upper plate below which all these units accreted. So it was during accretion part of the Eurasian plate. It may have been part of the Adriatic continent, but not of the Adriatic plate.

l. 377: And then in 2020 I did not speculate about it like Schmid and Vissers did and made a reconstruction using structural geological and paleomagnetic data and showed that this interpretation is impossible. I'd say either make an analysis of this region and properly restore it, or leave it out.

The rest of the paper contains a description of the model, and if the model is correct, the discussion is logical. I don't quite buy the reconstruction choices, so I refrain from discussing the implications of the model, I don't object to the first-order conclusions of slowspreading in the PL Ocean.

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