

Interactive comment on “Large-wavelength late Miocene thrusting in the North Alpine foreland: Implications for late orogenic processes” by Samuel Mock et al.

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Review Mock et al.

GENERAL COMMENTS

The paper presents some new AHe thermo-chronologic data and discusses them in the frame of geodynamic models of Alpine collision. The paper is well written and well illustrated. The data provide consistent results that allow one to constrain the age of thrusting of the Alpine front. The results show that the Alpine front in the western Central Alps is consistent with its age further east and this is a very interesting

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result. However the authors discuss these data in the context of lithospheric-scale kinematic models and the discussion becomes very speculative and mostly based on other inferred conceptual models rather than on the data of the paper itself. Parts of the Discussion section as that on the Bavarian Molasse are very far from the results presented by this manuscript and the Discussion section in general is rather far from the actual data of the paper.

I disagree with some of the interpretations in the Discussion section. One concerns the inferred general transition from vertical to horizontal tectonics in the Central Alps: I don't think that the tectonics of the Alps were ever vertical, and I don't think that a significant change of style before and after 12Ma took place. In addition the term "vertical tectonics" needs to be clarified and the evidence for such a change needs to be presented more convincingly in the introduction part. The authors state that there is no correlation between along-strike changes of shortening in the northern part of the Alps and along-strike changes of uplift (exhumation?) in the ECM. I think that the along-strike change of shortening goes together well with the one of exhumation in the ECM (see detailed comments).

A simple distinction between shortening along the Alpine front in the west and no shortening in the east is made and the transition is suggested to coincide with the inferred change of slab polarity. However, the Subalpine Molasse does not generally disappear east of Salzburg, it just disappears along a 100 km long segment, before re-appearing again further east. Shortening is still significant there (Beidinger and Decker, 2014). In addition, the area of the inferred transition is difficult to define precisely based on tomography (see detailed comments).

The authors interpret their age data as the result of a deep (slab) driver, instead of a more "local, upper crustal one". I don't think that this distinction is very useful. The paper dates (very nicely!) an upper crustal thrust that is an expression of syn-collisional upper crustal shortening. Collision affected the entire lithosphere, hence there is no doubt that shortening in the Subalpine Molasse is related to a deep (slab) driver. But

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this is not a special case related to the Subalpine Molasse...it is true for any other shortening structure of the Chain. The question is not if there is such a surface-to depth relationship, but rather if we are able to identify it. Trying to link these different levels of the orogen in the Discussion is a worthy effort, but it makes the Discussion speculative and largely unrelated to original data of the Results sections.

DETAILED COMMENTS

Page2

line 2 : replace $\hat{A}n$. They also $\hat{A}z$, by $\hat{A}n$... , but they also $\hat{A}z$

line 19: late stage? What do you mean by that?

Line 21: “However...”: this sentence suggests that classical propagation of thrusts towards the foreland should be continuous and that this is in contrast to the Miocene Alps. Sequence thrusting is discontinuous per definition and in the Miocene Alps still rather continuous I would say...the question is only what the absolute time interval between distinct thrusts is, in order to infer HOW discontinuous it really is... .

Lines 24-25: “In addition...”: in addition to what? The previous sentence states a difference between Alps and classical wedge tectonics, this one states that something is not resolved yet.

Line 25: “In the same sense”: ?

Line 27: abbreviation ECM is not mentioned and explained in text yet, just in the abstract.

Line 32: These mechanisms? You have not mentioned any mechanisms yet. Page 3

Line 6: “In regions”: give some examples or references.

Lines 14-15: not really a summary, it is the 1st time that the new LT geochron data are mentioned.

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Line 18: “late-orogenic large-scale change...”: this has not been mentioned before and it is written as if it was rather obvious. . .

Lines 21-23: this statement on the along-strike change needs a reference. Ortner et al. 2015?

Line 25: the reference is Kästle et al., 2020 (Int J Earth Sci), not 2019 (SE).

Line 27: replace deriving by derived

Line 28: replace deriving by derived

Line 31: I fully agree with placing the limit Central/Eastern Alps further East, but not with justifying this by the changing geometry of slabs at depth, which remains highly speculative, highly debated and controversially interpreted. By contrast, the limit based on the tectonic “transverse” system Giudicarie Fault- Brenner Fault is clearly mapped and its position will not change in the next years. . . Page 4

Lines 1 and 2: this sentence would be more appropriate at the end of section I.

Line 5: delete Schmid et al. 1996 if the reference is about the relationship of Central Alps to the slabs.

Line 6: “The”: you never mentioned it was bivergent in the previous text.

Line 6: I am not sure that Cretaceous subduction is related to the bivergent structure, but a paper showing this is :

Line 9: 1. nobody really knows if there was a slab breakoff, hence it should be written as an “inferred” process. 2. Nobody knows the age of this event in case it took place, but 32Ma is the age of the plutons inferred to derive from slab breakoff. Hence breakoff must be older.

Line 10: “period of fast uplift”: based on which evidence? Uplift or exhumation?

Lines 13-14: I would recommend that either the authors thoroughly discuss the matter

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of slab break off here with points in favor and against it or they drop this sentence with its citation.

Line 15: “over several tens of km “. . .of what?”

Lines 20-24: I am not sure if it is necessary to describe the Eastern Alps in this section. However if you do so, please do not forget that lateral extrusion goes together with one of the most spectacular upward folding of the nappe pile.

Line 25: “The late stage”: it sounds as a clearly defined event, but it is not the case.

Line 27: not “to the Southern Alps”, they are themselves growing southward.

Line 28: this change from dominantly vertical to dominantly horizontal growth is very critical to this paper, but it cannot be assumed so simply. I see no reason for a general change from vertical to horizontal tectonics during this “Late stage”.

Line 30: FTB in the S-Alps starts in the Eocene (pre-Adamello thrusts) and the post-15 Ma shortening is about 50% based on Schönborn (1992), it is not the largest part.

Line 31: add Molasse after Subalpine

Line 32: not very clear: you mean “where the deep slab is present? Attached? What is this “configuration”?”

Page 6:

Line 1: So there is a phase of uplift at 20 Ma (Herwegh et al.), one at 10 Ma (Glottbach et al.) and one at 5 Ma? Can we really distinguish these three events within 15 Ma? I doubt it. . .

Line 6: delete “the” before eroded

Page 7:

Line 22: replace “estimates when” with “estimates about the time. . .” or something similar. . .

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Line 24: replace technology with method

Page 8:

Line 13: “max degree of freedom”: explain

Line 21: what do you mean by “reproducing”?

Line 30: Add “compared” before to? Or reformulate otherwise. Not very clear.

Page 9:

Line 2: I still think that reproducing is not the appropriate term

Line 6: yes, but is it then an out of sequence thrust?

Line 9: what is a balanced map?

Line 9: if there are own data in the figure they should be described.

Lines 20-21: I fully disagree with this statement, both with its content and its formulation. A “peak of high uplift domain” is a very vaguely defined term that should be avoided here. Max. shortening in the west coincides with the position of the Mt Blanc Aiguilles Rouges ECM: the “peak of highest uplift”. Shortening decreases in front of the Aar ECM, which is less exhumed than the Mt Blanc, and it decreases even less in front of the Engadine Window, which is a sort of proto-ECM. Finally there is nearly no shortening east of Munich, but no ECM exists there.

Line 23: After reading 5.1. I don't really understand why the section should have “Downscaling as a title”.

Page 10:

Line 11: replace “strain release (i.e. thrusting pattern)” simply by “the pattern of thrusting”.

Line 13: replace “released” by “accommodated” and replace “much more distributed”

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by: “in a much more distributed fashion”

Lines 17-18 redundant. Delete.

Line 19: replace release

Line 20: sampled area, or better: study area

Line 21: replace release

Page 11:

Line 7: Post 12 Ma is ok, but there must be some pre-12 Ma thrusting too to justify cooling at 20 Ma.

Line 9: coevally

Lines 12-14: This should rather be in the geological setting.

Line 24: as stated above I disagree!

Lines 25-26: But why should they be a CONSEQUENCE of the exhumation of the ECM? I wouldn't know which process could possibly explain this. But as you write they are kinematically LINKED, which means that the Subalpine thrusts root in the ECM, hence the Subalpine Molasse is shortened together with the ECM.

Lines 28-29: This statement is very strong and clear, but where is a sound evidence to support it? I don't think that the Alps ever went through this change in Miocene times.

Line 29: this has been described in the Aar Massif, but not in the other ones. The plural and generalization are not appropriate here.

Line 31: why “reactivation”? Were they ever deactivated?

Page 12

Line 3: Schmid et al. 1996 quote Schönborn, 1992, which is really THE reference. Schönborn writes about Mid Miocene. So I would suggest either to say Mid-Miocene

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or 16 Ma.

Line 4: I do not agree with a general statement assessing the existence of a phase of vertical tectonics in the Alps before 12 Ma in the core of the Alps. And what is the core of the Alps? The Lepontine? Was the Lepontine affected by vertical tectonics before 12 Ma? In this case the term “vertical tectonics” should be defined precisely.

Lines 6-7: these statement cannot be applied so simply to “the Alps” in general.

Line 15: I am not sure that I understand the term “delamination” in this context. It should be specified.

Line 18: The relationship between slab unloading and backthrusting along the Insubric Line is extremely speculative, and anyway not explained by these sentences. Note that backthrusting is very significant in the western Central Alps and disappears in the eastern Central Alps. How does this match with such a large-scale interpretation, where the cause of these structures is seen in the deep slab?

Lines 19-21: on line 19 it is stated that the EU slab is delaminated, on line 21 “crustal delamination” is mentioned. Are you talking about the same process here?

Lines 21-22: “as evidenced”: the Helvetic nappes are the evidence of shortening in the cover during collision, not the evidence that crustal material was entering the subduction system during rollback. . .

Lines 21-23: Is it really necessary to propose these speculative interpretations?

Page 13

Line 4 : Rosenberg et al. did not state this.

Line 9-12: The very progressive eastward decrease in the amount of shortening as illustrated in fig. 6 is not satisfyingly explained by a process of decoupling in one specific area.

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Lines 15-17: a bit speculative, but again, the shortening data would call for a progressive change, not an abrupt one.

Line 18: this interpretation is also made difficult by the fact that there is no unanimous agreement on the deep structure of the slabs. See the different interpretations of Lip-pitsch et al. (2003), and the one of Mitterbauer et al. 2011.

Line 20: I agree that there is less shortening eastward, but again, the evidence is that of a PROGRESSIVE decrease that starts already in the Jura Mts.

Line 21: No references are given for the seismic tomography here. This is a problem, because several, but not all of the published tomographies support this last statement.

Line 22: Why the term “individual tectonic pulses”? They are just thrust sheets.

Lines 22-23: I frankly disagree with this attempt of categorizing the possible driver of shortening in the Molasse into two classes: deep (slab) driver, and crustal, local driver. If the authors argue that the driver of shortening is the displacement of the slab I certainly agree, but this generally true in convergent systems! The question here is not if the driver is somewhere outside the slab “(upper crustal phenomena)”. All upper crustal phenomena are supposed to be related to very deep ones. The question is whether we are able to make this link from the outcrop-scale to the orogen-scale to finally arrive at the lithospheric one. In this paper the authors are very keen on linking their relatively small observations to the very large one with the risk of being speculative.

Line 23: “bigger players such as plate tectonics”: I don’t understand the problem. Did anybody ever (since 50 years) doubt that the frontal Alpine thrusts are related to Plate Tectonic processes?

Line 25: The references are not all consistent with the statement of a polarity change. Mitterbauer is not, and Kästle (2020, not 2019) isn’t really. . .

Line 27-28: Not only the amount of shortening progressively drops even WITHIN the

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Central Alps, but in addition it increases again in the eastern Eastern Alps (Beidinger and Decker, 2014). Therefore there is a complex pattern that I cannot reconcile with the simple one of one slab in the Central Alps terminating at its eastern boundary.

Line 29: Again, I see no reason to infer such a macro-tectonic regime in general for the Central Alps. Page 14:

Lines 1-12: this paragraph discusses the spatial coincidence between the eastern termination of the subalpine Molasse close to Salzburg and the inferred change in subduction polarity in the Eastern Alps. The first is clearly defined in space, the second is not. Its position varies as a function of depth and its resolution in space is in the order of +/- 100 km. Thus stating that the two coincide is a vague statement. The other problem I see is that the Subalpine Molasse does not really end in the area of Salzburg, it is just absent for ca; 100 km and starts again further east. So the Alps are not so simply separated into a western part with a Subalpine Molasse and an Eastern one without it. They are rather a chain with a Subalpine Molasse that is missing in a small segment between Salzburg and Linz.

Lines 13-31: These paragraphs are about the tectonics of the Eastern Alps and their relationship to the Molasse Basin, which is however speculative and very far from the data of this paper. I wonder if they are really necessary.

Page 15:

Line 7: replace release

Line 11: all the Discussion emphasizes the along-strike tectonic change. . .why is it remarkably constant here?

Line 14: what is a “tectonic signal”?

Line 15: so the “signal” is shortening?

Line 15-16: structure of the sentence needs to be readjusted

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Line 17: so the thrusting is the large-wave length structure? Not the exhumation of the Molasse in general?

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