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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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Dear editors, dear referee,

We thank the referee for his very detailed and constructive comments on the manuscript. The referee is an expert on the tectonics of the Alps and has carried out extensive work on the along-strike architecture and evolution of the orogen. We very much appreciate the suggestions and criticism advocated by the referee and are convinced that with the revisions done we can now present a strongly improved version of the manuscript.

The referee's major points of criticism address: (1) The use of the terms "vertical" and "horizontal" tectonics, and the inferred change from one tectonic regime to the other; (2) The interpretation of foreland shortening estimates with respect to ECM exhumation; (3) The inferred link between the Central Alpine slab as a lithospheric driver and late Miocene thrusting in the Subalpine Molasse.

Regarding point (1), we clarified the meaning of the terms "vertical" and "horizontal" tectonics in section 1 of the revised manuscript. This distinction is based on published work on ECM exhumation (Herwegh et al., 2017, 2020).

With respect to point (2), we realized that we did not adequately describe the pattern of foreland shortening. In the revised manuscript, we made an effort to describe more precisely how horizontal shortening changes along-strike. We also further elaborate on the influence of uncertainties in the estimation of horizontal shortening.

Taking up the third point of criticism by the referee (3) we adjusted sections 2.1, 5.2.2 and 5.2.3 of the revised manuscript in order to show more convincingly why late Miocene thrusting in the Subalpine Molasse is likely be attributed to the dynamics of the Central Alpine slab at depth.

Extensive replies (R) to these general and other detailed comments (C) are given below.

With kind regards on behalf of the authors, Samuel Mock

General comments

C: 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 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).

R: Please see section 1 of the revised manuscript for a clarification. Herwegh et al. (2017, 2020) use the term "vertical tectonics" to describe the early stage in the evolution of an ECM, i.e. a phase of buoyancy-driven extrusion of a European basement unit after its delamination from the lithospheric mantle. This occurs by reverse faulting along steeply dipping structures resulting in differential subvertical uplift of the Massif. Based on structural, thermochronological, and geophysical data, this has been suggested for the Aar Massif to have occurred between ca. 22-13 Ma. According to Herwegh et al. (2020), a similar scenario is probably applicable also to the Mont Blanc and Aiguilles Rouges Massifs. In the Aar Massif, this phase of "vertical tectonics" is then superseded by en-bloc exhumation related to the activation of NW-verging basal thrusts in combination with dextral strike-slip shearing in the south starting at ca. 13-12 Ma. Herwegh et al. (2020) use for this thrusting-dominated phase the term "horizontal

tectonics" and suggest that this concept might also be valuable for the Mont Blanc and Aiguilles Rouges Massifs. It is during this second phase of thrust-related en-bloc exhumation of the ECM, when deformation propagated into the Jura FTB. This is also the time frame during which we dated thrusting in the Subalpine Molasse. Hence, during this phase of "horizontal tectonics", shortening in the Aar Massif and probably also the Mont Blanc and Aiguilles Rouges Massifs is kinematically linked to shortening in the Jura FTB and the Subalpine Molasse. However, the variation in horizontal shortening in the foreland does not seem to reflect the different levels of exhumed basement blocks in the hinterland. This can be attributed to the fact that the ECMs were largely already in existence and that large parts (ca. 10 km for the central Aar Massif) of uplift is related to pre-12 Ma buoyancy-driven vertical reverse faulting (i.e. vertical tectonics) as reported by Herwegh et al. (2020). We incorporated this argumentation/clarification in the revised version.

With respect to the along-strike changes of shortening, it is important to note that shortening estimates for the Subalpine Molasse are subject to substantial uncertainties and likely represent minimum estimates due to: (i) the unconstrained large parts of proximal Subalpine Molasse which are hidden below the frontal thrusts of the Helvetic nappes and Penninic Klippen units, and (ii) the usual non-preservation of the hanging-wall cut-offs of individual thrust sheets. We adjusted the text in the revised manuscript accordingly in order to be clearer on this. Furthermore, Ortner et al. (2015) indicate that the decrease in shortening towards the east may to some extent also be due to the increase in the uncertainties of shortening estimates due to the lack of subsurface information.

Overall, we observe an eastward decrease in shortening in the Jura and complementary to that an eastward increase in shortening in the Subalpine Molasse until ca. 10° E. At least between ca. 7.5° and 10.5° E (a distance of ca. 250 km) cumulative shortening hovers between 24-18 km, which, given the expected large uncertainties described above, can be considered as being quite constant. Farther east, shortening in the Subalpine Molasse then decreases rapidly over just ca. 120 km. Hence, we would argue that shortening is not strictly progressive from west to east. Although, there seems to be a slight but poorly constrained decrease from the Jura to ca. 10.5° E, a rapid decrease to zero shortening is observed farther east until Salzburg.

C: 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).

R: The idea of a slab polarity change is currently intensely advocated for in the literature. However, we fully acknowledge that it is highly debated and that there are various studies which disagree with this hypothesis. We also acknowledge that our data & hypotheses do not necessarily require a subduction polarity reversal. All tomographic studies do, however, image an anomaly in the deep structure somewhere below the Tauern Window and describe an along-strike change in the slab geometry at depth, despite disagreeing on the exact nature of this change (Hetényi et al., 2018; Kästle et al., 2020; Lippitsch et al., 2003; Mitterbauer et al., 2011; Qorbani et al., 2015; Zhao et al., 2016). At no point in the manuscript do we link unequivocally the eastward end of late Miocene activity in the Subalpine Molasse with a slab polarity change at depth. We do, however, make the link to the unanimously described segmentation of the orogen at depth and infer from the various tomographic studies the area where this segmentation is most likely to occur to be between the Brenner Fault and ca. 13° E.

Linking this to the orogen, we see that the amount of late Miocene thrusting in the Subalpine Molasse decreases rapidly roughly north of the Brenner Fault and that late Miocene thrusting diminishes completely just before Salzburg (ca. 12.8° E). Hence, we argue that the fade out of late Miocene thrusting in the Subalpine Molasse fits well

with the conjectured transition in the slab geometries at depth. We are fully aware that there is also a Subalpine Molasse (or imbricated Molasse) farther east in Upper Austria. However, this part of the Subalpine Molasse was not active during late Miocene times as has been described by Beidinger and Decker (2014), Hinsch (2013), and Ortner et al. (2015).

C: 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 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.

R: We agree with the reviewer that based on the arguments given in the reviewer's comment this distinction is indeed not very useful. As the reviewer points out, the important question is whether we are able to identify the influence of a deep orogenic driver on upper crustal processes (in this case the tectonics of the Subalpine Molasse). We are fully aware that making this link is anything else than straightforward and, due to the lack of direct observations, prone to speculation. However, based on the observation of an agreement in (i) spatial location, (ii) wavelength, and (iii) temporality, we think it is valid to discuss the scenario of a possible link between the dynamics of the Central Alpine slab and Subalpine Molasse tectonics:

(i) The along-strike extent of late Miocene thrusting in the foreland correlates spatially remarkably well with the proposed extent of the steeply south-dipping Central Alpine European slab imaged by seismic tomography (Fig. 9; Kästle et al., 2020; Lippitsch et al., 2003; Zhao et al., 2016).

(ii) The large spatial wavelength of tectonically driven late Miocene exhumation of the Subalpine Molasse reflects the Central Alpine slab as lithospheric-scale tectonic driver acting at that wavelength.

(iii) The proposed segmentation of the deep structure of the Central and Eastern Alps (Handy et al., 2015; Hetényi et al., 2018b; Kästle et al., 2020; Kissling et al., 2006; Lippitsch et al., 2003; Mitterbauer et al., 2011; Schmid et al., 2004), which is expected to have induced a geodynamic and tectonic reorganization along the Alpine chain by the end of mid-Miocene times, could explain the subsequent late Miocene tectonism restricted to the foreland of the Central Alps until ca. Salzburg and thus decoupled from the Eastern Alps.

Please see our revised manuscript, where we clarified these points in section 5.2.2.

Detailed comments

Page2

C: line 2 : replace Ân ÌĄ . They also Âz ÌĞ, by Ân ÌĄ..., but they also Âz ÌĞ

R: I guess you want us to replace the full stop by a comma followed by a "but"? Done!

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

R: Fold-and-thrust belt development in the Jura and the Subalpine Molasse as well as the development of the ECMs does not occur in the early stages of continent-continent collision, but rather late in the evolution of the Alps, hence, the term "late-stage". We replaced the term with "ongoing", since we think this better suited here.

C: 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. . .

R: We adjusted the text accordingly and rearranged the sentences.

C: 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.

R: Has been changed accordingly.

C: Line 25: "In the same sense": ?

R: Has been changed to "Likewise".

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

R: This is a misunderstanding; we already defined the term ECM on page 2, line 15.

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

R: Has been changed accordingly.

Page 3

C: Line 6: "In regions": give some examples or references.

R: We added references in the revised version of the manuscript.

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

R: We deleted "In summary,".

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

R: We removed this in the revised version of the manuscript. We also rearranged the paragraphs in order to be more concise.

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

R: We added the missing references (Beidinger and Decker, 2014; Hinsch, 2013; Ortner et al., 2015)

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

R: At the time of submission, the Kästle et al. paper was not yet under review at Int J Earth Sci. We gladly update the reference, thank you for the notice.

C: Line 27: replace deriving by derived

R: Done.

C: Line 28: replace deriving by derived

R: Done.

C: 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. . .

R: We acknowledge that Rosenberg et al. (2018) did not base their location of the Central-Eastern Alps boundary on the deep structure and modified the text in the revised manuscript accordingly. However, since one aim of this manuscript is to show possible inferences of slab dynamics on the late Miocene tectonic evolution of the Northalpine foreland, we think it is appropriate to use a definition on the orogens segmentation based on its deep lithospheric structure. We follow here a similar definition, which has already been used in Kissling and Schlunegger (2018). With respect to the changing slab geometries at depth please see here our reply to the comment below

regarding page 14, line 1-12.

Page 4

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

R: In order to give the reader our reason on why we use the aforementioned definition on the boundary between the Central and the Eastern Alps, we think it is evident to put this sentence already here.

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

R: Done.

C: Line 6: "The": you never mentioned it was bivergent in the previous text.

R: This term is not so important here and since it seems confusing we deleted it in the revised manuscript.

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

R: See reply to comment above.

C: 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.

R: We adjusted the text accordingly

C: Line 10: "period of fast uplift": based on which evidence? Uplift or exhumation?

R: You are right. It is better to use here exhumation, since Hurford (1986) use cooling ages to determine cooling rates of the sampled rocks. Hence, what is effectively measured is exhumation. By combining the data with structural and tectonic analysis, Schmid et al. (1987) interpret the exhumation rates as uplift rates. We think that by giving the reader the appropriate references to follow up on the details, it is not necessary to go into much more detail here.

C: Lines 13-14: I would recommend that either the authors thoroughly discuss the matter of slab break off here with points in favor and against it or they drop this sentence with its citation.

R: We added this information at the request of a previous reviewer. However, since a full discussion on this matter is beyond the scope of this manuscript, we agree with the current reviewer and deleted this sentence in the revised manuscript.

C: Line 15: "over several tens of km ". . .of what?

R: We mean here "...over 'a distance of' several tens of kilometers...". We adjusted the text in order to avoid any misunderstandings.

C: 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.

R: We added here information about the Eastern Alps following the request of a reviewer's comment of the previous version of this manuscript. We added the information about upward folding, respectively.

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

R: We certainly did not intend to use this term as a clearly defined event, but rather to say that what we describe is mainly concerning the late stages of Alpine orogeny (i.e. happening in a late time window of orogeny). We adjusted the text in order to make it clearer.

C: Line 27: not "to the Southern Alps", they are themselves growing southward.

R: We adjusted the text accordingly.

C: 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".

R: We adjusted the text in the revised version of the manuscript and removed the sentence. We added information and a definition of "vertical" and "horizontal" tectonics in section 1 and 5.2.1 of the revised manuscript. Please see also our reply to the reviewer's general comment above and to his comments regarding page 11, line 28-29 and page 12, line 4.

C: 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.

R: We changed the text accordingly.

Page 5

C: Line 31: add Molasse after Subalpine

R: Done

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

R: We have deleted the second part of this sentence since it rather belongs into the discussion part of the manuscript.

Page 6

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

R: I think this is a misunderstanding . Herwegh et al. (2017, 2020) document thrust-related en-bloc exhumation of the Aar Massif starting at ca. 12 Ma, and not 20 Ma. This is in general agreement with the findings of thermochronometric studies (Glotzbach et al., 2010; Valla et al., 2012; Vernon et al., 2009; Weisenberger et al., 2012) and, in turn, occurs in the same time window as thrusting in the Subalpine Molasse as documented in this study and von Hagke et al. (2012, 2014) and Ortner et al. (2015) and main deformation in the Jura FTB (see Becker, 2000).

C: Line 6: delete “the” before eroded

R: Done.

Page 7

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

R: Done.

C: Line 24: replace technology with method

R: Done.

Page 8

C: Line 13: “max degree of freedom”: explain

R: By using only a few well constrained constraints, we can give the model a high degree of freedom and do not force it beyond given constraints. We changed the text to make it clearer.

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

R: We mean that the single grain ages of one sample are all within the error bars of each other. Text has been adjusted to clarify.

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

R: Done

Page 9:

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

R: has been changed to “. . .group within their margins of error.”

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

R: Not necessarily. Unfortunately, we have no age data from the front-most thrust (cutting the Burdigalian deposits), which would allow us to define the exact sequence of thrusting in this part. However, the timing of activity correlates well with out-of-sequence thrusting east of the Aare valley at ca. 6Ma. As we discuss later in the manuscript, the site where thrusting occurs seems to be very variable along-strike the Subalpine Molasse and is mainly controlled by the mechanical stratigraphy.

C: Line 9: what is a balanced map?

R: In order to explain the kinematic evolution of the Jura, Philippe et al. (1996) use an approach where they balance palinspastic maps by restoring structural map units, which are delimited by tectonic boundaries. We adjusted the text to make this clearer.

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

R: We added additional information in the supplementary material. Furthermore, we also moved the appendix of the manuscript, which describes the apatite separation and picking methods to the supplementary material.

C: 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 concides 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.

R: With respect to along-strike changes in shortening estimates please see our detailed reply to your comment regarding page 13, line 20. Yes, there is a slight decrease in shortening from west to east, however, the variation in horizontal shortening does not seem to reflect the different levels of exhumed basement blocks in the hinterland. As a simple observation this holds true. We reformulated the sentence. We further elaborate on this observation in section 5.2.1 of the revised manuscript. Studies from the ECMs show that the massifs were already in existence when deformation propagated into the foreland. Hence, late Miocene thrusting and shortening of the foreland is kinematically linked to northward thrusting along the northern front of the ECMs (referred to as “horizontal” tectonics by Herwegh et al., 2017, 2020). This, however, seems to occur regardless of the maturity of an ECM or whether an ECM is present in the hinterland or not.

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

R: We removed this in the revised version of the manuscript.

Page 10

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

R: Done.

C: Line 13: replace “released” by “accommodated” and replace “much more distributed” by: “in a much more distributed fashion”

R: Done.

C: Lines 17-18 redundant. Delete.

R: Done.

C: Line 19: replace release

R: Done.

C: Line 20: sampled area, or better: study area

R: Done.

C: Line 21: replace release

R: Done.

Page 11

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

R: We are not aware of any reported cooling ages from the Subalpine Molasse east of Lake Constance. However, from tectono-sedimentological data (e.g. Hinsch, 2013; Kempf et al., 1999; Ortner et al., 2015; Schlunegger et al., 1997), it is known that the Subalpine Molasse was of course already tectonically active before 12 Ma and became subject to thrusting already in the early Miocene or even earlier. In this case, however, we focus on the late Miocene tectonism in the Northalpine foreland, i.e. the Subalpine Molasse and the Jura FTB, and correlate this with the documented late Miocene en-bloc exhumation along shallow-dipping thrusts below and in the ECM.

C: Line 9: coevally

R: Done.

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

R: We moved this into the geological setting and just shortly refer here to it.

C: Line 24: as stated above I disagree!

R: Please see our response to the reviewer’s comment above regarding page 9, line

20-21. In order to clarify, we adjusted the whole paragraph in the revised version of the manuscript.

C: 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.

R: We agree with the reviewer and deleted this sentence. We adjusted the whole paragraph in the revised version of the manuscript.

C: 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.

R: We acknowledge the comment of the reviewer and see the need to clarify this point. Please see also our reply to the reviewer's general comment above and to his comment regarding page 12, line 4. We adjusted the whole paragraph in the revised version of the manuscript. We have realized that we need to re-dimension this observation / statement to the northern Central Alps (i.e. the Jura FTB – Molasse – ECM system). We base this statement on the following arguments.

- Based on tectonic, thermochronological, and geophysical arguments, studies from the Aar Massif have suggested that the Massif uplifts differentially during an early and mid-Miocene buoyancy-driven stage ("vertical tectonics"), which is then followed by a thrusting-dominated stage ("horizontal tectonics"), when the whole Massif is exhumed en-bloc above a series of basal thrust ramps (Herwegh et al., 2017, 2020). Although this has been showed exemplarily for the Aar Massif, a similar evolution is probably also applicable for the Mont Blanc and the Aiguilles Rouges Massif (see Herwegh et al., 2020).

- During the buoyancy-driven uplift of the Aar Massif, the northern deformation front re-

mained stationary in the Subalpine Molasse (triangle zone; Burkhard and Sommaruga, 1998; von Hagke et al., 2014; Ortner et al., 2015). It was not until 12 Ma when deformation propagated into the Jura FTB (e.g. Becker, 2000) and widespread activity of thrusting in the Subalpine Molasse occurred (von Hagke et al., 2012, 2014; Ortner et al., 2015; this study). This occurred coevally and is kinematically linked with low-angle thrusting in and below the Aar and Aiguilles Rouges Massif.

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

R: We agree with the reviewer that we cannot generalize this for the other ECMs. However, in their review article, Herwegh et al. (2020) have proposed a similar scenario also for the Mont Blanc and Aiguilles Rouges Massifs. We adjusted the text accordingly and distinguish more carefully between the different ECMs.

C: Line 31: why "reactivation"? Were they ever disactivated?

R: changed to "activation"

Page 12

C: 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 or 16 Ma.

R: Done.

C: 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.

R: Please see here also our reply to the reviewer's general comment above and to his comment regarding page 11, line 28-29. We refer here to recent publications (Herwegh et al., 2017, 2020) where the authors describe a phase of buoyancy-driven uplift of the Aar Massif between ca. 23-13 Ma based on a geodynamic scenario of slab roll-back (Fry et al., 2010; Kissling, 2008; Kissling and Schlunegger, 2018; Schlunegger and Kissling, 2015; Singer et al., 2014). Due to the sub-vertical direction of movement along steeply dipping reverse faults, they simplify this as "vertical tectonics". Starting at 13 Ma, they see evidence for low-angle northward thrusting along a series of thrusts in and below the Aar Massif, which leads to en-bloc exhumation of the entire Massif. It is the latter event, which they are referring to as "horizontal tectonics" and which is kinematically linked to thrusting in the Subalpine Molasse and the Jura FTB. Based on thermochronometric arguments, they also suggest a similar style of en-bloc exhumation above basal thrusts of the Aiguilles Rouges Massif, which similarly is also kinematically linked to foreland deformation. Hence, with our new age data from the Subalpine Molasse, we can further corroborate the timing of this inferred transition, from buoyancy-driven "vertical tectonics" (ca. 23-13) to "horizontal tectonics" represented by basal thrusting of the Aar and Aiguilles Rouges Massifs and thrusting in the Subalpine Molasse and the Jura FTB.

In summary, the term vertical tectonics describes the buoyancy-driven differential uplift of delaminated (supposedly within the lower crust) European basement units along steeply dipping shear zones. This has been suggested for the Aar Massif based on structural, thermochronometric, and geophysical arguments, but may also have played a role for the evolution of the Mont Blanc / Aiguilles Rouges Massif. The term "horizontal tectonics" describes the subsequent phase of en-bloc exhumation of the Aar and Aiguilles Rouges Massifs due to foreland verging thrusting along low-angle thrust domains. This is kinematically linked with thrusting in the Subalpine Molasse and the Jura FTB.

Please see our adjustments to sections 5.2.1 and 5.2.2 of the revised manuscript.

C: Lines 6-7: these statements cannot be applied so simply to "the Alps" in general.

R: We agree with the reviewer and adjusted this whole paragraph in the revised version of the manuscript.

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

R: we deleted the term here and specified it a few lines later in revised version of the manuscript.

C: 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?

R: Since, this seems to be quite speculative and not so important for the purpose of this manuscript, we decided to remove this sentence from the revised version of the manuscript.

C: 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?

R: Yes, we are talking about the same process here. Kissling (2008) proposed that the crust delaminates (i.e. separates) from the mantle somewhere within the lower crust, enabling the mantle slab to sink while the less dense and buoyant crust becomes accreted to the orogen. Later this concept has been refined further and put into a slab rollback model for the Alps (Fry et al., 2010; Kissling, 2008; Kissling and Schlunegger, 2018; Schlunegger and Kissling, 2015; Singer et al., 2014). We clarified the usage of the term "delamination" in the revised version of the manuscript.

C: 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. . .

R: We adjusted the text accordingly.

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

R: We follow here an argumentation of a published roll-back subduction model. However, we adjusted the wording and used the subjunctive where appropriate.

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

R: Rosenberg et al. (2018) did however observe a "...gradient of crustal thickening, increasing westward, associated to an eastward decrease of average elevation." We reformulated the sentence accordingly.

C: 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.

R: Our statement of a decoupling is based on two observations: (i) while west of Salzburg, late Miocene thrusting occurred in the Subalpine Molasse, there is no such evidence for the Subalpine Molasse east of Salzburg (Beidinger and Decker, 2014; Hinsch, 2013; Ortner et al., 2015), (ii) Current estimates on cumulative horizontal shortening, certainly when considering the presumably large uncertainties with respect to shortening within the Subalpine Molasse, do not call for a progressive decrease from west to east. However, what we observe is a rather rapid decrease in late Miocene horizontal shortening east of ca. 10.5° E to zero shortening east of Salzburg. With respect to the shortening estimates please see our detailed reply to your comment regarding page 13, line 20. We also adjusted the text to make this last argument clearer.

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

R: The use of the subjunctive wording should already indicate that this is a possibility

and not at all an unambiguously describable fact, which we would like to discuss. With respect to the shortening estimates please see our reply to your comment regarding page 13, line 20.

C: 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 Lippitsch et al. (2003), and the one of Mitterbauer et al. 2011.

R: It is true that there is no uniform opinion on the slab geometry at depth and especially about their origins (Adria vs. Europe) and possible mechanisms (e.g. slab break-off, subduction polarity change, overturning slabs, etc.). However, nobody doubts (i) the existence of an attached and steeply south-dipping slab of European origin below the Central Alps, and (ii) the existence of an anomaly somewhere below the Tauern Window and a segmentation of the slab geometry along-strike, although the nature of this segmentation is very much debated. See also our reply to your comment regarding page 13, line 25.

C: 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.

R: It is important to note that shortening estimates for the Subalpine Molasse are subject to substantial uncertainties and likely represent minimum estimates due to: (i) the unconstrained large parts of proximal Subalpine Molasse which are hidden below the frontal thrusts of the Helvetic nappes and Penninic Klippen units, and (ii) the usual non-preservation of the hanging-wall cut-offs of individual thrust sheets. We adjusted the text in the revised manuscript accordingly in order to be clearer Furthermore, Ortner et al. (2015) indicate that the decrease in shortening towards the east may to some extent also be due to the increase in the uncertainties of shortening estimates due to the lack of subsurface information.

In summary, we observe an eastward decrease in shortening in the Jura and complementary to that an eastward increase in shortening in the Subalpine Molasse until ca.

10° E. At least between ca. 7.5° and 10.5° E (a distance of ca. 250 km) cumulative shortening hovers between 24-18 km, which, given the expected large uncertainties described above, can be considered as being quite constant. Farther east, shortening then decreases rapidly over just ca. 120 km. Hence, we would argue that shortening is not strictly progressive from west to east. Although, there seems to be a slight but poorly constrained decrease from the Jura to ca. 10.5° E, a rapid decrease to zero shortening is observed farther east until Salzburg.

C: 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.

R: We added the missing references. They all describe the lateral termination of a steeply S-dipping Central Alpine slab somewhere in the area of the Giudicarie Fault.

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

R: We adjusted the text accordingly.

C: 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.

R: We realized that it is probably better to use the term ‘exhumation’, since this is the signal we can measure by using thermochronometry. We very much appreciate your comment regarding the possibility to infer a link between upper crustal processes

and deep-seated processes. Please see also our response to the reviewer’s general comment above. We adjusted section 5.2.2 of the revised manuscript accordingly.

C: 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?

R: We very much appreciate the reviewer’s comment and adjusted the text accordingly.

C: 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. . .

R: Yes, this is true. Mitterbauer et al. (2011) argue against a subduction polarity change. However, they too report an along-strike change in the slab configuration between ca. 12°-14° E, with the change from a shallow slab to a deep, possibly detached slab configuration. They also image the along-strike change from a south-dipping to a north-dipping shallow slab. Hence, although not agreeing with the polarity change theory, they also clearly report an important along-strike change in the slab configuration. Kästle et al. (2020) show very nicely that the European slab extends eastward until 12.5°, which correlates with the extent of the late Miocene Subalpine Molasse. For the purpose of this manuscript it is not the aim to give a definitive solution on this problem; and we dare not to do so, since it is not our field of expertise. In addition, the variety in the tomographies and their interpretations certainly do not allow for a concise solution at this point. Hence, we deleted the proposed mechanisms here in the text.

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

R: We agree that the amount of shortening does decrease towards the east within the Subalpine Molasse. However, it is important to note that shortening estimates for the

Subalpine Molasse are subject to uncertainties and likely represent minimum estimates due to: (i) the unconstrained large parts of proximal Subalpine Molasse which are hidden below the frontal thrusts of the Helvetic nappes and Penninic Klippen units, and (ii) the usual non-preservation of the hanging-wall cut-offs of individual thrust sheets. We adjusted the text in the revised manuscript accordingly in order to be clearer. With respect to shortening in the Subalpine Molasse of Upper Austria please see our reply to the comment below (p. 14, l. 1-12).

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

R: We adjusted the text in order to specify that we describe this transition for the northern Central Alps (i.e. the Jura FTB – Molasse – ECM system). Please see our reply to the reviewer's general comment above and to his comments regarding page 11, line 28-29 and page 12, line 4.

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C: 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.

R: Yes indeed, there exists an imbricated Molasse, or Subalpine Molasse, east of Salzburg, a fact which we also mention in the geological setting. However, there clearly seems to be no evidence of late Miocene shortening in the Subalpine Molasse east of

Salzburg as several publications indicate (Beidinger and Decker, 2014; Hinsch, 2013; Ortner et al., 2015). This is an argument, which we state very clearly in the manuscript (see page 5, line 30-32 of the unrevised manuscript). It is true that the transition in the slab geometries at depth is subject to large uncertainties and we discuss this adequately in the manuscript by citing the many tomographic studies and interpretations (Hetényi et al., 2018; Kästle et al., 2020; Lippitsch et al., 2003; Mitterbauer et al., 2011; Qorbani et al., 2015; Zhao et al., 2016). Please note that in the revised manuscript, we added a missing reference from the AlpArray tomographic study (Hetényi et al., 2018). We can infer that a segmentation in the deep structure of the Alps occurs somewhere below the Tauern Window; an observation on which all of the publications imaging and interpreting the Alpine slab structure in this area concur on. Furthermore, the various tomographic studies allow us to infer the area where this segmentation is most likely to occur, i.e. between the Brenner Fault and ca. 13° E. When we compare that to the Subalpine Molasse, we see that the amount of late Miocene thrusting decreases rapidly roughly north of the Brenner Fault and late Miocene thrusting diminishes completely just before Salzburg and does not occur farther east. Hence, we argue that the fade out of late Miocene thrusting in the Subalpine Molasse fits well with the conjectured transition in the slab geometries at depth.

C: 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.

R: We added this paragraph at the request of both reviewers of a previously submitted version of this manuscript. Since, there is literature, which specifically discusses the influence of lateral extrusion on the tectonics of the Subalpine Molasse (see Ortner et al., 2015), we think it is appropriate to discuss this and include it in our considerations about the late Miocene tectonic evolution of the Subalpine Molasse.

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C: Line 7: replace release

R: Done

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

R: We changed the text accordingly.

C: Line 14: what is a “tectonic signal”?

R: See reply to comment below.

C: Line 15: so the “signal” is shortening?

R: Yes, the signal is thrust-related shortening. We adjusted the text accordingly.

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

R: Done.

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

R: Indeed both. Exhumation of the Subalpine Molasse has been constrained by cooling ages between Lake Thun and Lake Constance and has been attributed to wide-spread thrusting, which, for the same time frame, has also been observed for the Subalpine Molasse farther east.

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