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Interactive comment on "Control of 3D tectonic inheritance on fold-and-thrust belts: insights from 3D numerical models and application to the Helvetic nappe system" *by* Richard Spitz et al.

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Author's response to

Interactive comment on "Control of 3D tectonic inheritance on fold-and-thrust belts: insights from 3D numerical models and application to the Helvetic nappe system" by Richard Spitz et al.

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Control of 3D tectonic inheritance on fold-and-thrust belts: insights from 3D numerical models and application to the Helvetic nappe system by Spitz et al. Submitted for publication to Solid Earth

General comments The paper uses 3D numerical thermo-mechanical modelling of a heterogenous rifted margin with variable basement/sediments and structures assuming viscous-plastic laws. Results are then compared to deformation observations along 3 sections of the external Swiss-French Alps that have reached temperature conditions of 250-380âŮęC. The model setup is intended to reproduce the initial architecture of grabens of the European margin (proximal part) with different sediment thickness and geometry of basins varying along-strike. A V-shaped North-Helvetic basin is assumed. One main implications of such modelling approach is that for sedimentary units to be detached above the basement and form thrust nappes (little internal deformation) no mechanical softening is required. Strain localization results from the geometry and strength vari-ations, which conditions are likely met in many mountain belts. The overall modelling presentation and results are well written and quite easy to follow. I am only concern about erosion that is not modelled (see below). Results are sounding and the application to the Alps relevant, but please consider say some words about the choice of having discarded the role of erosion. I only suggest to consider reorganising/rewriting the Introduction and the Discussion (Section 5.3).

Specific comments Introduction Lines#44-50: This paragraph is very specific (i.e. only viscous mechanism are addressed) relative to the rest of the introduction. I suggest to move them after lines#55-61 where the authors present older studies with more gen-eral mechanical behavior.

We changed the order as suggested.

In addition, in classical model of FTBs (like later in the intro) a major decoupling level (low friction or linearly viscous like salt) if often assumed. The high contrast between basal and internal strength allows thrust nappe formation. This best applies to external zones when pressure and temperature (well below 300âŮęC) does not modify the original rheological layering. In the case of shear zones the au-thors study here (intermediate domains of tectonic burial) this is very different because such weak layers where deformation localizes is not prescribed so a "self-localization" process is required. The introduction should better emphasize the differences between the end-member approaches. One "cold" frictional classical approach of Coulomb-type thrust wedges vs one "hot" viscous-plastic approach of ductile nappe stacking. They are both valid and should co-exist, depending which part of the orogen you are dealing with. Here we are clearly more interested in ductile-type fold-thrust belts. Methods. The authors should indicate why erosion is not relevant or not taken into account in their modelling approach. Because erosion is not considered here rocks do not cool during deformation, thrusting/folding and the crust is thickening. Therefore they are always in the ductile field. This may be valid but should be clearly presented especially the authors are dealing with the most recent Miocene sequence of shortening in Alps and erosion is a major factor in orogens during these late stages.

There are three reasons why we do not model erosion: (1) due to simplicity. If we would consider erosion, we should ideally test the impact of different erosion models (diffusion-type, slope dependent etc.) on our results, which is beyond the scope of the study. (2) the topography in our model does not show significant lateral variation and with the exceptions of minor undulations the topography remains more or less straight. (3) the main phase of uplift and associated exhumation started at ca. 20 Ma, which is towards the end of the main phase of nappe formation. Therefore, erosion, exhumation and associated cooling might have only affected the late stage of nappe formation. We added some of this information in the revised text.

Discussion The authors are able to reproduce thrust nappes (little internal deformation) of sedimentary units detached above the basement without mechanical softening. This is important. “Kinematic” strain localization is the result of the geometry and internal strength variations.

Lines#453-456: Section 5.1. This part raises a very interesting point. The impact of this result is that the initial rheological layering and configuration of these layers suffice to produce thrust nappes. I am wondering how this could extend to the application of "static" models of brittle thrust wedges (e.g. Dahlen et al., 1990) with no mechanical softening or more dynamic ones including friction (Ruh et al., 2012). These models indeed assume that mechanical properties do not change with time and incremental displacement.

Indeed, the results of Ruh et al. (2012) and ours (and several other studies) essentially show that material heterogeneities can generate localized deformation and when such heterogeneities are considered, it is not necessary to apply prominent strain softening to the model units. In our case, we argue that the particular geometry of the nappe system, and in particular it’s lateral variation, are to first order determined by the pre-Alpine geometry, or tectonic inheritance. This seems maybe obvious and has been suggested by many geologists, however, no one has, to the best of our knowledge, supported this with 3D thermo-mechanical numerical simulations.

Lines#463-470: Thickness does matter to explain salients and recesses in FTBs but these peculiar structure may also reflect the occurrence of laterally discon-tinuous decollement levels (changes in lithology and thickness - for viscous décollement - also play an important role); in other words not only changes in the overburden thickness are relevant.

We clarified the statement and now write: ‘This observation is in accordance with our study which shows that lateral changes in the lithology, such as thickness and rheology, produce different salients (\ref{fig:fig4}).’

The Section 5.3 presents a discussion on our attempts to compare geological sections with numerical models. I had hard time to follow the reasoning here. First the authors introduce the balanced cross-section approach which can only scarcely be applied here because of the dominant ductile behavior of the HN; thickening/thinning of layers (and lengths variations) during deformation and out-of-plane deformation preclude the use of 2D balancing techniques. 3D balancing techniques with volume balancing could do it. I think the value of this section is not the comparison with balanced cross-sections. I would suggest to focus on the 3D aspects of their numerical approach and explain how the structural complexities seen in 3D modelling could be simply explained by lateral variations in the original structure rather than by mechanical complexities mechanics. This is important when searching for mechanical interpretation of lateral changes in cross-sections (balanced or not !). Maybe is what the authors intended to do here but could not clearly see it from my reading.

We reformulated this section to make our aims clearer.

Other technical corrections are in the pdf attached.

Please also note the supplement to this comment:

https://www.solid-earth-discuss.net/se-2019-173/se-2019-173-RC2-supplement.pdf

We considered all corrections and modified the manuscript accordingly.

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