Solid Earth Discussions

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Comment on se-2021-135

Guillaume Duclaux (Referee)

Referee comment on "The topographic signature of temperature-controlled rheological transitions in an accretionary prism" by Sepideh Pajang et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-135-RC2, 2021

Review of "The topographic signature of temperature controlled rheological transitions in accretionary prism", by Sepideh Pajang, Laetitia Le Pourhiet, and Nadaya Cubas.

This preprint presents an innovative contribution focusing on understanding the surface morphology and tectonic evolution of fore-arc wedges through a series of two-dimensional numerical models. The authors investigate the dependence of effective basal friction on temperature due to metamorphic reaction or brittle-ductile transition. Accretionary systems in general are of considerable interest to the research community studying subduction megathrusts, the interlinks between tectonic and sedimentation, and are also of economic significance for the oil industry. The authors present here a set of 15 two-dimensional models based on state-of-the-art finite elements methods. The modelling approach is pretty well designed and a section detailing some of the limitations of the current implementation are explicitly presented, which keeps this contribution honest.

The paper briefly reviews published literature on fore-arc wedges morphology and tectonics in the light of the classical critical taper theory (CTT), as well as a state of the modelling work published to date. Then, they explore the effect of basal heat flow, shear heating, thermal blanketing by sediments, and the thickness of incoming sediments on the dynamics of an accretionary prism. The authors found that the seaward limit of the brittle-ductile transition in the prism enhances topographic slope, and that ductile deformation could play a primary role in the prism dynamics. There findings imply that viscous deformation in the prism could have a direct consequence for the mechanics of subduction megathrusts.

The manuscript is well written and organised, and a series of models' snapshots combined in 6 key figures illustrate the different models' dynamics through space and time. I believe this contribution is solid and seems well suited for Solid Earth journal. I recommend accepting this preprint for publication after minor revision.

I present below a few questions that I have about this work followed with a list of minor comments which I hope will help the authors improve their contribution.

1) My main concern is related to the model flat base and its impact on the dynamics of the prism itself. In the classical CTT approach the prism internally evolves to maintain a balance between a plunging basal décollement (plunging landward in natural cases) and the surface topography. Because here the basal decollement is flat (parallel to the base of the model box), the simulated surface topography should be overestimated, and not in direct agreement with observations. I understand this is an actual limitation of the model, but it should be further discussed as it might impact the whole topographic slope analysis presented by the authors. Still, I remain fairly convinced with the authors study, as although the absolute topographic slopes predicted in their models might be wrong, the relative change in the slopes linked to brittle-ductile transition or metamorphic reactions should still exist.

2) Shear heating is a very important factor in this study as it is the sole heat production term accounted for in the energy equation. Now, the unit for the heat production by shear heating doesn't make sense to me... according to Eq. 4 it is in Pa.s?? How? Some additional explanations are necessary. Could the authors please clarify this in the methodology section?

3) A vertical scale must be added to each figure presenting the model results (Fig. 2 to 7, and 9). There is a very important vertical exaggeration in these figures. Understanding this is critical to compare the models with natural cases and make this work directly usable to others.

4) Boundary conditions (BCs) are of prime importance in numerical models. The fixed left wall certainly has a strong influence on the development of the normal fault described by the authors, and the exhumation pattern of the dome visible in various models. Have you considered, or tested, alternative BCs? To be clear I'm not asking the authors to run additional models here, but the importance of the fixed left wall on the dynamics of the ductile region of the prism should be pointed out in the discussion, if not conceptually explored.

Minor comments:
+ l. 31: replace "along" with "within"
+ l. 50: reach instead of rich
+ l. 68: missing "of" --> the introduction of temperature
+ l. 91 and 95: to be consistent with the rest of the manuscript please use an upper case "E" for Eq. and Eqs
+ l. 91: shear heating, see comment above.
+ l. 127-128: please rephrase the sentence about mesh elements as it isn't very clear. It reads as if all models had 2 independent meshes, one for the shale unit, and one for the sediments above... But, as I understand it some experiments have 16 elements vertically, others have 24 (ny in Table 2).
+ l. 139: remove the "/" before (

+ l. 165: "k" font should be in italic
+ l. 191: I would start a new sentence at the start of this line with "The mature [...]"
+ l. 193: "topographic slope which corresponds to the brittle-ductile transition" --> ok, but could you please be more specific? Indeed, the brittle-ductile transition is present in M1 everywhere there is viscous deformation, on the left side of the model, at median depth between the surface and the bottom of the box.
+ l. 219: The vertical partitioning between simple and pure shear is not so clear to me. The base is indeed dominated by simple shear, but as far as I can see the top is barely deforming at that stage, except near the dome that forms next to the backstop. Could you provide some additional arguments/evidences for the pure shear deformation please across the model? Or is it limited to the region near the backstop?
+ l. 320: I would suggest adding 'only' in "slope than the brittle-only part".
+ l. 323-333: I understand the D2 metamorphic foliation should be subvertical, is that correct? Please describe the superimposed fabric orientation in the context of the model. Is this in agreement with observations in forearc wedges?
+ l. 355: The 7s are supposedly TWT (two-way time)? Please precise this here.
+ l. 356: references formatting is incorrect. Please fix that.
+ l. 367: I would recommend the authors look into Vérati et al (2018 --> https://doi.org/10.1016/j.lithos.2018.08.005) paper about the development of low grade metamorphic foliation in the volcanic pile of the Lesser Antilles arc. Although it is not directly the sedimentary prism, this work seems very relevant to this study.
+ l 376, 378 and 382: It's probably me, but I'm not familiar with the term "geodetic coupling" or "geodetic deformation". Could you please explain those terms?

Figure Captions:
Fig 4: for consistency with the main text please replace high sedimentation with surface diffusion.
Fig 8 b) and c): Do not use "cross-sections". A cross-section from a 2d model would be a 1D line... These are the "models".

Guillaume Duclaux
Hobart, 18/12/2021