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Interactive comment on "The dynamics of laterally variable subductions: laboratory models applied to the Hellenides" *by* B. Guillaume et al.

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Reply to Referee #1 (Dr. Thibault Duretz)

We would like to thank Thibault Duretz for his comments and suggestions. These have been of considerable help in improving the quality of the manuscript and in preparing the revised version.

We will answer point by point the different comments raised by the Referee.

1. **p. 316 l. 7 - In the abstract you mention "two units", at this stage it is not**

clear what these two units are.

We agree with Referee #1 and modified this sentence to better emphasize what the two units are. It is now:

"Two subducting units, which correspond to a negatively buoyant oceanic plate and positively buoyant continental one, are juxtaposed via a trench-perpendicular interface (analogue to a tear fault) that is either fully-coupled or shear-stress free."

2. **p. 317 l.19 - Why would the subduction of continental material trigger transient effects ? Consequence of such process might be as dramatic as slab detachment and/or delamination. In the same sentence you seem to wanna confront perturbation of mantle flow to actual observations (upper plate strain, trench deformation). I would here use the term topography (instead of dynamic topography) since topography is an actual direct observation of surface deformation (i.e. not a model).**

   Concerning the first point raised by Referee #1, it is true that if a sufficient amount of continental lithosphere enters the subduction zone it may end with slab break-off or delamination. We therefore removed the term "transiently" that could be confusing and refer to the work by *Duretz and Gerya* (in press).

   Concerning the second point, we think it is more appropriate here to refer to dynamic topography, which is actually what is measured in our model.

3. **p. 318. l. 8 - You mention 'at least during the earlier stages', it is not clear which stages you refer to (oceanic, continental subduction, something else ?). I would also add a reference concerning the process of slab distortion during subduction.**

   We now specify that it occurs during *"the earlier stages of continental subduction"* and we added a reference to the work of *Spakman and Hall* (2010).

4. **p. 318 l. 21 - As a matter of clarification, I would clarify what the term "dynamically self-consistent" means. Do you mean that the flow is gravitationally driven ? Does it mean something in terms of boundary conditions ?**

   It means that the system is driven only by the slab pull force. No external kinematic boundary conditions, such as plate or trench velocity, are applied. This ensures that the experimental subduction process is a self-consistent response to the dynamic interaction between the slab and the mantle. It is now clarified in the manuscript:

   "We thus perform a set of dynamically self-consistent 3-D laboratory models, in which no external kinematic boundary conditions, such as plate or trench velocity, are applied, ..."

5. **p. 320 l. 9 - A continental crustal thickness of 16km seems rather thin. Is it specific to the natural prototype of interest ?**

   It is indeed rather thin but appropriate to the studied natural case since seismic data indicate that subducted continental crust beneath Northern Greece is $\sim$20 km-thick *Pearce et al.* (2012). We now explain it more precisely:

   "...(corresponding to a continental lithosphere with a crust thickness of $\sim$16 km and a density of 2.75, in the range of crustal thickness found for the subducting lithosphere beneath the Northern Hellenides (20km, Pearce et al. (2012))")

6. **p. 320 l. 24 - I am unsure that the term "thin viscous sheet approximation" is suitable, this is usually employed to describe the vertical integration of the 3D momentum equations. However, the laboratory models are fully 3D and do include vertical viscosity layering (at the LAB for example). I would rather tell that the lithosphere is modeled using one unstratified mechanical layer. Also, concerning the absence of "spontaneous" localization of tear**

faults, I'd rather think that this effect is related to the nature of the model's lithospheric layer (e.g. Newtonian viscous). Although, I am not sure that employing a (perfectly) stratified lithosphere would help triggering tearing (without introducing the geometry of a TOC).

   Ok. We now write:

   "Because the lithosphere in our experiments is simulated by the means of an unstratified mechanical layer with a Newtonian rheology, its deformation is not localized enough to spontaneously form a vertical tear fault during the subduction."

7. **p. 321 l. 9 - Adding a layer of vaseline probably leads to a huge viscosity contrast within the model's lithosphere (what is the viscosity of the employed vaseline ?). How does the introduction of this new parameter affects the scaling of the experiments ? Isn't it possible to employ the same material as you use to decouple the subduction interface ?**

   We already explained the mechanical implications of using petroleum jelly (generic term for vaseline) in the manuscript. We added some more information to make it more clear:

   "Mechanically, the viscosity of the employed petroleum jelly is so low that it implies shear stresses (but not the normal stresses) are negligible between the two plates."

8. **p. 321 l. 26 - For consistency and in relation to the above statement ("In the following, we directly express the quantities with their corresponding scaled values. . .") , you may want to scale this value.**

   It has now been changed to 1200 km.

9. **p. 322 l. 11 - Concerning the slight influence of the tracers on the fluid properties, adding a reference could help justifying this statement.**

We added a reference to *Funiciello et al.* (2006):

"We assume that tracers only slightly influence the density and viscosity of the mantle fluid, as in Funiciello et al. (2006)"

Reply to Anonymous Referee #2

We acknowledge Reviewer #2 for his/her constructive comments which helped us improving the clarity of our manuscript. He/She particularly insisted on three points (annotated 1 to 3) and made 9 specific comments (annotated 1 to 9).

Main comments:

1. **Against the background of a "discussion paper", I believe that the manuscript will gain weight and credibility when incorporating a couple of publications, which are worthwhile to compare the results with. This can be done within the model description and discussion part. These papers are highly relevant to the investigated problem as the underlying process (subduction) is the same. On the topic of mantle flow: Schellart (2004); Schellart et al. (2011); on the topic of plate coupling and overriding plate deformation: Gerya et al. (2008), Faccenda et al. (2008, 2009), de Franco et.**

al. (2008), Iaffaldano et al. (2012), Luth et al. (2010, 2013); on the topic of trench depth and trench morphology Yanez and Cembrano (2004); on the topic of tear (STEP) faults: Baes et al (2011).

   When necessary we incorporated the suggested references. For example, concerning mantle flow, we now refer to *Schellart* (2004) in section 3.1.1. and also added a reference to the paper by *Funiciello et al.* (2004), which also described the phenomenon. We do not refer to *Schellart et al.* (2011) since we do not discuss here the effect of lateral slab edge distance.

   Concerning plate coupling and trench depth, reference to the work by *De Franco et al.* (2008) is now given in section 2 and to the study by *Yáñez and Cembrano* (2004) in section 4.2.

   On the topic of tear faults, a reference to the article by *Baes et al.* (2011) is made in the Introduction.

2. **In the manuscript strong emphasis is put on the Kefalonia fault as an equivalent to the tear-fault in models 5 and 11. Scaled to nature, the Kefalonia fault is a first order structure, which should have a clear expression in the geophysical data, which seems not to be the case. Given the importance of this structure for the manuscript I suggest to explain in more detail the geologic/geophysical evidence concerning the Kefalonia fault as a lithosphere-scale feature and its proposed link to the North Anatolian fault, which is not that obvious when for example reading Brun and Sokoutis (2010). Furthermore the Kefalonia fault is displayed ad strike slip fault in fig. 10 and 11, but as thrust fault in figs. 1 and 9, which is somewhat confusing. Please explain and/or correct.**

   While the nature and origin of the Kefalonia Fault are already discussed in the third and fourth paragraph of section 4.1, we now also add the results from P- and S-wave velocity perturbations around the Kefalonia fault *Pearce et al.* (2012).

They show that based on the identification of crustal offset of subducted lithosphere between two sections north and south of the Kefalonia Fault, the amount of slab retreat must be 70-85 km larger for the southern unit than for the northern unit, consistent with the proposed 100 km offset along the right-lateral Kefalonia transform fault *Royden and Papanikolaou* (2011).

Symbols for the right-lateral Kefalonia transform fault were indeed missing in Figs. 1 and 9. They have been added.

3. **All of the issues addressed in this study are described and discussed using models 1,5,8 and 11. There is almost no comparison to the results of the other 7 models listed in Table 1. For example, upper plate deformation (section 3.4) is described on the basis of model 1 and model 11 (with no upper plate at all) although there is according to table 1 also models, which posses an upper plate (models 2 and 7). I would be very eager to learn how the different choice of parameters of model 1 with respect to models 2 and 7 influences the result, particularly because the deformation of the upper plate in the Aegean region is well known and is thus important for the validation of the modeling results. In summary, it would be good to incorporate the main results of the other 7 models in a concise manner to get a feeling for the diversity of modeling results as a function of different parameter combinations.**

   The additional 7 models served as support for our analysis since they do not differ significantly from the four described ones. When necessary, we now point out the differences that arise from the slight differences between models.

   Concerning reviewer #2 comment on upper plate deformation, if we use model 11 in section 3.4 to confront mantle flow to upper plate deformation it is because due to technical limitations, horizontal mantle flow at the top of the glucose syrup is only measurable without overriding plate. In this sense, models 2 and 7 are of

no help here.

Model 7 was more specifically designed to study the overriding plate topography (which proved unsuccessful, see below), and as such no grid was designed on top of the silicone plate preventing any attempt of internal horizontal deformation analysis. In addition, unlike models 1 and 2, this model is 40-cm large (instead of 20 cm), which influences along-trench kinematics. Model 7 should instead be compared to model 5 to discuss the role of the upper plate on trench kinematics, as now done in section 3.1.1.

Model 2 instead better compare with model 1, only marginal differences in terms of viscosity being introduced. Indeed, analysis of trench kinematics and overriding plate deformation reveal a similar evolution, which support results from model 1. It is now stated in section 3.4.

Model 3 has the same geometry as model 1 but no overriding plate. It only results in an increase of the absolute value of trench retreat, with values twice as high as in model 1, but changes in trench kinematics following continental subduction occur on the same timescale, as now indicated in section 3.4.

Models 4 and 6 provide similar results as model 5, as now indicated in section 3.1.1.

Model 10 is strictly similar to model 11.

Model 9 is an intermediate case in terms of slab width between model 5 and model 11. As such, trench kinematics are in-between those of these end-members, subduction following a 4 stage evolution as in the other two models. We now indicate it in section 3.1.3 but consider useless to describe this model into more details as the paper is already long.

Specific comments:

1. **P. 319, line 3: . . ."mantle flow and its motion is resisted by viscous dissipation in the mantle, in the slab and in the overriding plate". One would argue that also the resistance at the plate interface as important. Please comment.**

   Reviewer #2 is right. Dissipation at the plates interface has been added.

2. **P. 323, line 24: . . ."retrograde" motion. . .., perhaps better to use different terminology here not to confuse the reader with petrologic terminology.**

   Ok. "Retrograde" has been replaced by "retreat" or "roll-back" here and elsewhere in the text.

3. **P. 327, section 3.3 "Mantle flow". Along the lines of one of the "general comments": Mantle flow is described on the basis of model 11 only. It would be interesting to learn how the tear fault (model 5) influences the flow pattern in the mantle, if at all??? Please comment.**

   We would have also been curious to observe mantle flow after tear fault opening but unfortunately technical reasons prevented us to obtain those images.

4. **P. 328, 3rd paragraph: In this paragraph you suggest that differential shear stresses at the base of the lithosphere are responsible for the trench parallel stretching. It is not clear to me what the source of the "differential shear stresses is". Please explain.**

   We refer to basal shear tractions resulting from heterogeneous mantle flow below the overriding plate, as now written in the manuscript:

   "Stretching is not uniform, owing to differential basal shear tractions at the lithosphere–asthenosphere boundary resulting from heterogeneous mantle flow below the overriding plate."

5. **P. 329, line15: I think you should avoid mixing "indentation" into the game as the**

   Reviewer #2 comment is incomplete but to avoid any confusion, "indentation" has been removed.

6. **P. 329, section "Dynamic Topography": How do you distinguish the tectonic from the non-tectonic component of topography and how big are to your opinion the error bars related to that?**

   This section presents topographic results from model 11 in which no overriding plate is involved. Therefore the tracked topographic signal at the top of the glucose syrup (top of the asthenosphere) only results from vertical stresses produced by mantle flow. The potential filtering effect of the lithosphere on dynamic topography at the surface and the tectonic component of topography are not accounted for. It is now more clearly stated:

   "Because there is no overriding plate in this model, all the topography has a dynamic origin."

   Error bars in measurement of dynamic topography at the lithosphere-asthenosphere boundary in our models directly depend on the scanner vertical precision and the scaling of the models. As indicated in the section 2.2, the precision of the device is 0.05 mm, which given the scaling used here correspond to 300 m as indicated by the error bars in the Fig. 8.

7. **P. 330, 2nd paragraph: How do the vertical motions of model 11 compare to the vertical motions of models with an upper plate?**

   Unfortunately, in models with an upper plate (1, 2 and 7), we had optical issues with the scanning device and the used silicone, which resulted in a noisy unexploitable topographic signal.

8. **P. 334, last paragraph: In this paragraph you place a critical assumption, namely that the viscous strain of your models is representative of brittle deformation in the crust in nature. To my opinion you should be a bit more cautious here, because the Aegean domain is considered as a fairly weak domain and there is no guarantee that the strain is not significantly partitioned between the layers of different strength.**

   We agree with Reviewer #2 that there may be some partitioning between layers of different strength, not accounted for in our models, as now indicated in the manuscript:

   "In addition, strength layering that may produce significant deformation partitioning over the lithosphere thickness is not accounted for."

   We are also now more cautious by indicating that *"the **first-order** comparison between the continuous deformation of the models to the discrete deformation observed on Earth holds. Indeed, our models show comparable patterns of deformation and **may** explain the temporal evolution of Aegean tectonics ..."*.

9. **P. 3y 39, line 11: This first sentence of section 4.5 is highly speculative, as the efficiency of the transmission of the mantle drag into the lithosphere is a function of the rheology at the base of the lithosphere, which we do not know. Be more modest in your formulation.**

   This unnecessary and confusing sentence has been removed.

<u>Technical corrections</u>

10. **P. 330, line 11: substitute “of” with “by”.**

    Ok.

11. **P. 332, lines 11/12: Here you introduce the “South and North Hellenic subduction zones”. Would be good to label these subduction zones in the pertinent Figure (Fig. 9).**

    Done.

12. **P. 337, line 27: Subsitute the second “for” with “from”.**

    Ok.

13. **P. 338, line 3: . . .”teared” should be “torn”.**

    Ok.

14. **P. 338, line 23: “Our models show”. . .instead of “shows”.**

    Ok.

15. **Throughout the manuscript, you often use the phrase “thanks to” (3 times on p. 342). Replace with scientific writing!**

    Done.

16. **Fig. 2: Would be good to add the N-direction to the figure as you describe your results in terms of the geographic coordinate system. Also provide the figure on the depth of the tank (in cm).**

    North and vertical scale are now indicated in Fig. 2.

17. **Fig. 6 gives the impression that the models are not fixed to the backwall, which is different to drawing of the setup in Fig. 2.**

    The plate are indeed attached and we modified Fig.6 to show it.

References

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