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Solid Earth Discussions

Interactive comment on "Insight into collision zone dynamics from topography: numerical modelling results and observations" *by* A. D. Bottrill et al.

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We thank the referee for these positive comments and address specific issues below.

Issues

1 *The models are dynamic models – and I note that the subduction phase has unrealistic time dependence – see figure 4D; there is no case of subduction increasing from 10 to 30cm/yr over a few million years and then slowing to <5cm/yr in less than another few million years. The question in one's mind is – is this model suitable? How does the answer differ with differing initial conditions? This could be addressed by trying to develop a more realistic initial condition behaviour – or at least compare a few different*

initial conditions to illustrate its importance.

It is correct that the initial phase of the subduction model show unrealistic subduction velocities. This is a well-known feature of almost all subduction models, and therefore these initial subduction rates are, indeed, often assumed to be not representative. Subduction rates settle down to a reasonable level after the slab starts interacting with the transition zone before collision. We have experimented with wider models that give a much greater period of subduction at more geologically reasonable subduction speeds. The results from these models showed the three main features discussed in this manuscript though with slightly difference magnitudes due to difference in the plate age. For clarification of this point in the manuscript we have now amended the explanation of the subduction velocity in the results section.

2 *Another linked aspect to this is the boundary condition of the model. The bottom boundary is no slip. As a result plates cannot penetrate and must move parallel to the boundary. In fact this leads to a flow structure which leads to the slab curling back on itself – and breaking off close to the base of the upper mantle (e.g. Figure 4C). Seismology does not identify such curling as a common structure, and in fact in most cases slabs do seem to descend through this boundary (though there are many examples of plates stalling). Again, this raises the question, is the model set-up suitable? This closed box also makes slab roll-back difficult – an important phenomenon in many subduction zones. These issues with the lower boundary could be tested by running models with an open lower boundary.* The no slip boundary is designed to simulate a slab interacting with the 660km discontinuity. This is a no-slip condition to simulate the higher viscosity of the lower mantle. Seismic tomography (e.g. Wortel and Spakman, 2000) show that slabs from the previously subducted Tethys ocean (which are the focus of this study) can mostly be found at the base of the upper mantle today, i.e. 10s of Myrs after their subduction. This suggests that these slabs encountered a significant resistance to penetration into the lower mantle. We use this to justify the choice of bottom boundary conditions. This model contains no mechanism for roll back. Although,

as the reviewer identified, roll back is probably an important process we exclude it here so as not to complicate the results with features moving in space. Improvements to the description of the boundary conditions in the Methodology section have been made to clarify these issues.

3 *There seem to be very rapid spatial variation in viscosity in these models. As a result one wonders whether the numerical results are 'resolved'. A single run at a higher resolution to demonstrate convergence would be helpful. What is the resolution in the region of interest? Is the grid uniform?*

There is a strong viscosity contrast in the region of the weak zone that de couples the plates. Grid is refined over the area of collision with the smallest elements being 4km in x- by 5km in z-direction. All the topography presented in figure 4 falls within this high resolution area. Results for a double resolution calculation are almost identical suggesting numerical resolution is sufficient. We have modified the method and results section to clarify the grid set up and that the result is unchanged at double presented resolution.

4 *While the elastic lithosphere response is a nice addition it is unclear whether it is 'correct'. Many would argue that the subduction contact means that there are two elastic plates, one on either side of the plate boundary; and not the single plate assumed in the current calculation. The likely net result, of imposing a free edge on both plates at the trench is that the local topography near the trench will have an increased magnitude. This filter (and the fact that its effect can depend upon how it is applied) can have a significant effect – for example – before applying the elastic filter – the CMDB would always stay below sea-level.*

During on-going subduction two independent elastic plates is probably a better model though once collision has happened one elastic plate is probably more suitable. It was decided to use the one elastic plate assumption as the majority of the feature of interest in this study are post collision and significantly into the overriding plate. We have added

a sentence into the method section to clarify our choice of one elastic plate.

5 *In looking at the viscosity in figure 3 – and comparing with the CMDB – one finds a correlation with the shallow viscosity at the basin. The overriding plate in this location seems to be weaker than its surroundings at the location of the basin. This deserves an explanation. What process is locally weakening the plate? Again, while the reader does not understand – there is room for doubt in the reader's mind.*

The surface plate weakening at this point is a manifestation of the topography generated. We use the same stress and temperature dependent rheology throughout the model so the downward stress responsible for formation of the collisional mantle dynamic basin (CMDB) also weakens the overriding plate. We have added a description of this effect in the results section.

6 *While the manuscript looks at the influence of the viscosity in the channel on the depth of the CMDB – does the thickness and down-dip length of the channel play a role? Would a fault lead to a different result than a channel? Is the channel maintained for the whole simulation (if so, this would discourage trench migration) – and if so, how? Are particles with a weak rheology placed on top of the downgoing plate to generate the weak zone self consistently?*

The channel is maintained for the whole simulation by creating a weak zone between the two plates. This ensures that the plates are always decoupled. Thickening this would increase the decoupling in a similar way to reducing the viscosity of the weak zone. The effect of the model setup has on the eventual results is one of the intrinsic problems in subduction modelling. However here we have chosen the decoupling parameters so that, once the plate interacts with the 660 km discontinuity, subduction velocities are maintained at a geological reasonable rate. The mechanical evolution of the trench/suture after collision is unknown and therefore we chose the simplest approach of keeping things constant. We have edited the manuscript to further elaborate on the coupling between the two plates.

7 *The embedding of the continental lithosphere in the oceanic lithosphere does not satisfy plate tectonics. The two boundaries of the continental lithosphere are passive. Therefore the age should decrease away from the continent. This is not true on the trench side of the soon to be subducted continent. I suspect that this might not make a big difference in this case since it is all fairly old with limited changes in properties with distance.*

We would agree that this is not the most realistic situation, though as an approximation for a numerical model that helps us understand the process it is probably adequate. We have tried different plate ages and found that, although having effects on the magnitude of topography displayed, they don't alter the pattern produced. Therefore we can indeed confirm that this doesn't have a great effect on our results. We have elaborated in the manuscript on model setup so it is clear what simplifications have been made.

8 *The continent-ocean boundary is infinitely sharp in these models. In many cases the boundaries are known to be very sharp – but even boundaries of 10's to 100 km width could make some difference in these models – since the variations in topography are happening laterally on relatively short length-scales also.*

Again this is a simplification made during the model set-up. We would agree that a more geologically realistic model would involve a tapered lithosphere and crust. We have tried models with a thinned portion of continental crust on the colliding edge of the plateau. These produce very similar results also the lithosphere structure approaches a tapered structure after a number of time steps. We would therefore conclude that These assumptions don't have an effect on the board feature examined here.

9 *The ignoring of oceanic crust was understood – but it is only self-consistent if the deeply subducted continental crust is converted to eclogite facies. Was this done? Maybe continental crust never reaches deep enough in these models?*

We would argue that our ignoring of the oceanic crust is only correct if continental crust does not transform into eclogite, since then its buoyancy effect remains present.

Since oceanic crust does transform to eclogite, its buoyancy effect disappears, and we can ignore this buoyancy. So therefore we make the assumption in our models that continental crust does not transform readily to higher-density phases when it subducts.

10 *Is the lack of shear heating important? In regions of high strain-rate one might have imagined that it could lead to strong feedback and change the time-scales of processes.*

Shear heating is not included in these models since we make the Boussinesq approximations. In a way, it can be argued that shear heating is implicit in our choice of the weak zone viscosity (it forms one of the mechanisms to make the weak zone weak). Investigating the effect of shear heating on subduction viability and vigour is investigated by other authors, and not the focus of this work.

11 *Where is the basin located relative to the arc? Would a constructional arc have any influence on the topography; e.g. locally, but also by flexure loading into the back arc basin?*

With the slab dip angle used in this model, arc volcanism would be expected at around 100km from the basin. This would put the arc at around 200km in front of the CMDB. We shall include this in our description of our results. The additional loading of a constructional arc would likely have a small effect due to the assumption that the overriding plat has a thick and therefore strong lithosphere. This is again something we felt difficult to quantify for inclusion into our numerical model but accept would have an effect on observed topography.

Clarifications

Page (p) 895, line (l) 22 – *How is the oceanic lithosphere thickness set? Thermally? What is the logic of 'it (thickness) is proportional to its position relative to the left edge of the model'? This suggests that different initial conditions were used – as I mention above understanding the influence of IC would be useful.*

The oceanic lithosphere is set thermally using the half space cooling model. We model a mid ocean ridge at the left edge of the model and assume that, to define the initial thermal state, the plate ages at a rate of 3cm/yr to the end of the subducted slab. We have included this extra description into the methodology section of the manuscript.

P 896, l 4 – *How is the zero set for topography? Is the dynamic pressure given an absolute zero? Is zero normal stress assumed to correspond to sea-level? What density is assumed for the crustal column (or is only a density difference used)?*

The topography is all scaled so as to set the left edge of the model tot -2700km to simulate a mid ocean ridge. The dynamic pressure has no absolute maximum or minimum. The crust is given a density of 27000 kg/m3 and the mantle material 3300kg/m3 at 0°C. We have added this information to the manuscript.

P896, l 15 – *is this sufficient information to solve the equation? What is assumed for the gradients of w at the boundaries?*

It is assumed the left edge is at -2700m to simulate a mid ocean ridge. The right edge is at its isostatic value corrected for moving the left edge to -2700m. It is also assumed that there is no change in gradient at the boundary e.g. $\frac{d^2w}{dx^2} = 0$

P896, l 15 – *describe the terms – or at least mention that they are defined in Table 1. (Would it be better to have 2 tables – so that they could be near the appropriate equations?).*

We feel that one table is clearer but have made more reference to them.

P900, l 13 – *Is the variation in the coupling during the subduction process likely to change the prediction of the topography.*

Variations in coupling would produce different topography results. We have chosen to keep the coupling constant so as not to introduce further complication into the model, and since very little is known about such variation. We have added further explanation of this and the reasoning to the Methodology section.

P900, l 26 – *can you make clear whether it is the form, or just the presence, of the carbonates that indicate little compressional deformation; I presume the form.*

The presence of the carbonates indicates sinking of the region. Dating and the stratigraphic context of various faults have shown that these mainly postdate the deposition of these carbonates. This suggests that the basin the carbonates formed in was not fault controlled. We shall highlight the evidence for little compressional deformation in the discussion section of the manuscript.

P902, l 15 – *so is the match of the modelled basin with observations slightly fortuitous?*

There is a slightly fortuitous aspect to the Eurasian plate being at the right height to record the topography change by sinking below sea-level. Other collisional setting may not record the topographic change so effectively due to the overriding plate not being taken to below sea level during the basin formation. This could mean that any change in sedimentation was not so dramatic.

P903 – l 26; *'transmission of stresses through the weak zone', seems counter-intuitive.*

This is a combination of a typo and us not explained clearly enough in the manuscript the process. The explanation for the depth of the mantle wedge being insensitive to the mantle wedge viscosity is that the flow generated by slab steepening is generated over a much larger area that the mantle wedge. This means that even with stronger or weaker mantle wedges the stress is still transmitted to the overriding plate creating the topography. This section has been amended to reflect this.

Presentational issues

We will deal with all the points raised below in the manuscript.

P892, l 5 (line 5) – causes -> cause P892, l 7 – delete - the Interactive Discussion P893, l 17 – descent Discussion Paper P895, l 12 – delete – it C343 P895, l 17 – due TO their P896, l 20 – Middle P896, l 24 – s/he uses – P897, l 13 – need to describe more precisely what the numbers relate to P898 l 21 - time slice -> time slices P898,

l 24 – reduction IN magnitude P899, l 21 – through P899, l 22 – Earth P903, l 26 - delete – the P905 onwards; some references do not have correct names of authors; e.g. MJR Wortel, JH Davies, F. von Blanckenburg, JX Mitrovica, AM Forte, SP Grand, NA Simmons,(van der Meulen referenceder?) etc. P908 – l 8, space between of and Gondwana P909 – is pressure deviatoric? Is Hydrodynamic, or Dynamic pressure better? P909 – Poisson's ratio

Summary The manuscript presents a sophisticated numerical model which provides one plausible explanation of the Qom formation in Iran. The schematic figure (figure 5) also gives the reader a good pictorial summary of what the authors feel controls the changes in dynamic topography in their model. While I have some doubts as to whether the model is robust to variations in parameters (see above), and we have only a limited sense of its sensitivity it is still a very interesting model. Hopefully a more comprehensive investigation will follow in future work.

Again we would like to thank the reviewer and acknowledge the contribution they have made to the improvement of this manuscript.

Interactive comment on Solid Earth Discuss., 4, 889, 2012.