Solid Earth Discuss., 4, C339–C344, 2012
www.solid-earth-discuss.net/4/C339/2012/

Solid Earth
Discussions

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

Anonymous Referee #1

Received and published: 20 August 2012

Insight into collision zone dynamics from topography: numerical modelling results and observations

The manuscript reports numerical models of subduction and continental collision. The models are dynamic and focus on predictions of dynamic topography changes on the overriding plate. The predictions are compared to observations of stratigraphy observed on the overriding plate of the Arabia-Eurasia collision zone in Iran and Turkey. The models suggest that subduction generates a mantle dynamic basin on the overriding plate, which uplifts as collision proceeds and then subsides following slab break-off. It is claimed that marine carbonates are deposited between terrestrial clastic sedimen-

tary rocks in Iran/Turkey and that these rocks do not record major compressional deformation. They claim this fits well with the modelled topography changes. This work is within the scope of SE.

The manuscript is significant since it tries to tie together observations of evidence for dynamic topography on the overriding plate of a collision zone with plausible models. Another novel aspect of the work is that it has applied a filter mimicking elastic lithosphere response. This reduces some of the extreme and probably unrealistic estimates of simple isostatic estimates of topography. Another strength of the work is that it uses 'dynamic' models.

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.

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.

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?

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.

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.

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?

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.

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.

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?

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.

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?

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.

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)?

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

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?).

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

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.

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

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

Presentational issues

The title reflects the nature of the paper, and the abstract is a good summary. The manuscript is well written – a few slips are mentioned below. It is well structured and generally clear. The supplementary material is appropriate, and possibly could be increased slightly to address some of the issues raised above.

If the authors/editors were looking for material to reduce then the discussion of Italy could be removed without weakening the manuscript. On the other hand this discussion does relate to the modelling – so I would not require it be removed.

The scale needs to be given for the velocity arrows in the figures.

Slips

P892, l 5 (line 5) – causes -> cause

P892, l 7 – delete - the

P893, l 17 – descent

P895, l 12 – delete – it

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 reference – der?) 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.

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