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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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The authors would like to thank Philippe Agard for his constructive review that has helped us improve the manuscript. We have addressed specific points below as well as editing the manuscript to reflect these changes.

Obvious weak points are:

1. *There aren't that many sensitivity tests...The ones performed and shown in the Appendix are not sufficiently discussed, and should be inserted in the main text in some way. The models are very sensitive to your weakness zone. Can you comment on this? Similarly, the insensitivity to the mantle wedge viscosity and width, for example, shows*

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that it is so weak in the model that the other parameters are more influential and rate limiting. How if you were to vary within reasonable bounds? Note that whatever the parameters chosen, the amplitude of the topography for the sag basin is very high (> 4000m; see below) and you only show the lowest values in figure

We will include more sensitivity tests in the revised manuscript and discuss both these new sensitivity tests and the originals further. Having discussed with co-authors we would prefer to not include them in the main text as we feel they rather distract from the take home message. Though agree that they are important to show the validity of our model and so will discuss them and their implications in the main text. The model result is sensitive to the weak zone viscosity. This is because the weak zone governs a lot of the dynamics of the model. Including the subduction speed and coupling between the plates. The sensitivity test only shows the maximum depth that the CMDB (collisional mantle dynamic basin) reached and not when this occurred. What we did find was that with high viscosity weak zone slows subduction substantially so the deepest point is much later in time. The lack of sensitivity of the CMDB to the mantle wedge is due to the basin being due to slab steepening. This means that, assuming the viscosity of the mantle wedge is not so high that it prevents slab steepening, a stress will be generated that will deflect the surface plate. This stress will be proportional to the size of the slab not the material connecting it to the overriding plate. We will further discuss the sensitivity testing in the Appendix.

Sensitivity to the initial geometry is not explored, and in particular to the size of the continental block (not explored either in van Huenen and Allen, 2011, by the way). What if we depart from a 700 km long block? Why chose a block rather than of a fully continental plate to the left of the oceanic domain, as was the case for the Arabian plate? What is the justification for it?

The size of the continental block is not explored here. Although this would be good to include for completeness we would predict that results wouldn't be greatly affected, assuming the block is large enough to cause subduction to cease. This is due to the to-

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pography presented being for the overriding plate and the subducting block not having any significant momentum due to its slow speed (momentum is actually neglected in the numerical calculations due to being so small). This lack of momentum means that a larger continental block would not “hit harder”. A fully continental block would change the model as the mid ocean ridge at the edge of the model provides a far field ridge push force as well as decoupling the continental block to allow it to travel towards the subduction zone. We would argue that the inclusion of the ridge push force and decoupling of the subducting plate is applicable to all collision situations including the Arabia Eurasia collision. We will include a sentence in the method section on the importance of the mid ocean ridge at the left edge of the model.

It would be good/interesting to see what happens at other subduction/convergence velocities (eg Himalayan ones). I am curious to see what altitudes you reach...

We do have results from the changing the weak zone viscosity (Appendix A) which has the effect of changing the subduction velocity by changing the coupling between the plates. These results show a broad trend of a slower subduction velocity producing a similar pattern of topography over a longer period of time. Although it would be nice to present topography time maps for different subduction speeds, we feel this would more easily sit in future work discussing applications of this generic model to multiple collision zones.

2. *The tracking of topography is problematic and claims are too high at present. What plays the trick in your model? OK, as they authors briefly mention this is due to the "elastic" filtering of the normal stresses by flexure equations... but that sounds a little magic at present (or allusive, to say the least) and should be explained in more detail. Please comment your topography time maps (Fig. B.1 BTW, I would insert this one too in the manuscript), since they are in fact a number of changes (for example, there is no sag basin for $T_e=40$ or 50 km). The topographic evolution of the back arc is very little discussed! Furthermore, what are the precision and accuracy on the tracking of topography: this is not even mentioned!... The reader needs to evaluate how exactly*

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topography is reproduced in your models and simple tests (i.e., for simpler geodynamic configurations) should be provided here.

We will improve the explanation of how topography is calculated from the model. As a brief explanation the normal stresses at the surface of the model are taken at every surface model node. These stresses are assumed to deform a continuous elastic beam with elastic thickness of 30km (though results are shown for other T_e values in the appendix). The deformation of this beam is the elastically filtered topography. We again feel that insertion of figure B.1 into the main manuscript would cause confusion for the reader. The sag basin appearing to disappear for T_e 40km and 50km is unfortunately just due to all of the topography time maps being plotted on the same colour scale. We will improve the clarity of this figure to make this more obvious. The topographic evolution of the back arc basin is not discussed as this was not the main focus of this study. We have tested our model without the continental block and found that once subduction velocities settle to a sensible level we produce similar results to (Husson 2006). We shall try to add some explanation for the back arc basin features seen. Unfortunately the most dramatic change in the back arc basin is when the slab interacts with the 660km discontinuity which could be argued is a model artefact. For testing the topography calculation method we did a number of simple point load calculations which were compared to published solutions.

Although I have no problem with explaining the Qom basin as a fairly passive (i.e. driven by far field forces, since there is no known tectonics in that region at that age) I am very doubtful about the amplitude of the topography: 4000m! This looks more like a trough than like an epicontinental basin with continental and carbonate rocks only!

The depth of the basin is largely around 1km deep (Fig 4). The comment may refer to the 4000m elevation contrast with the nearby uplifted region. However this is probably an overestimate as it relates to whole scale subduction of the continental crust with no imbrication as would be realistically occur. We will highlight the lack of imbrication in the discussion of the results *Let us remind that there were no significant slopes, no*

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turbidites, no debris flows in and around the Qom formation Actually, the depth inferred for the Qom basin is not even mentioned! It is just argued: "...fits well,... fits well"... well I am fully convinced this is not the case!

We try argue that the position in time and space of the basin fits well with the Qom formation. We do accept that our general model probably over estimates the magnitude and underestimates the spatial extent of these basins.

We don't made reference to the actual depth of subsidence as this is difficult to estimate due to not knowing the exact elevation of the area pre subsidence. If we had to be drawn on an inferred depth of subsidence from the Qom formation we would suggest the probably 500m – 1km which is less than predicted by our model with a 30km effective elastic thickness.

We do acknowledge that you would expect turbidites and debris flows from the evaluation contrast predicted in our model. However our models are also likely to overestimate the amount of uplift due to no buoyant crustal material being removed from the subducting plate.

Similarly, I am a little concerned about the huge (transient) altitudes predicted for the overriding plate before breakoff (Fig.3, 7 Myrs) for which there is no evidence at that age (ie, much before the Plio Quaternary coarse clastic deposits of the Bakhtiari formation).

This very high topography directly after collision is caused by the subduction of continental material. This area is probably much higher than would be likely in reality as no imbrication of buoyant continental crust happens in the model. There is also some suggestions that the Arabia Eurasia collision was a “soft” collision (Ballato et al. 2010) where a thinned portion continental crust separated the oceanic and continental crust. This could also limit the initial uplift seen as less buoyant continental crust would be subducted. We will highlight the reasons for this initial high topography in the results section of the manuscript.

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Why are your topographic signals so narrow too? (obviously, again, a matter of elastic filtering...) Is this reasonable? What are the respective contributions to the topography of the crust and mantle? To which extent is this controlled by the chosen rheology? (for which there is very little information provided, incidentally).

The topographic signal is narrow due to the stress responsible for the topography act on the overriding plate in a relatively narrow area. We attempt to counter this affect by applying an elastic filter but acknowledge that the elastic thickness of 30km does not reducing the magnitude of some of the features enough. The choice of elastic thickness used in our model has been of great debate between co – authors. There still seems to be some debate as to the elastic thickness for the region from direct observation with 10km (Mckenzie and Priestly) to 40km (Watts etal). With these wide estimate we chose 30km to show we could reproduce the first order pattern without entering the debate over the effective elastic thickness of the region. We will highlight this debate and the inherited weakness in our results in the results section of the manuscript.

3. The authors correctly point out one major shortcoming of the modeling: the inability to reproduce thickening and tectonic slicing. This should be better emphasized, however.

We will better emphasise the short comings of the models

Similarly, one should mention from the start that tectonic slicing (and friction) acts as an important parameter controlling topography not just buoyancy, flexure of the lithosphere and mantle flow beneath as mentioned in the introduction.

This will be added to the Introduction and Discussion noting that both the strength of lithosphere and crust are important to topography.

Minor points

It would be fair to cite Agard et al. 2005 when refering to the timing of the Arabia – Eurasia collision (i.e., 35 Ma). The work of François et al.(at EGU 2011,2012) should

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also be mentioned since all this work looks very similar...

We will add these references (Agard et al. 2005; François et al. 2012)

"Oceanic crustal buoyancy ignored..." Why is that? By the way, the blueschist to eclogite transition is certainly not effective at 30 – 40 km depth (see Agard et al., 2009 an reference therein)!

This is an assumption regularly made in numerical subduction modelling. The assumption is that although blueschist are formed from crust transported to greater depth. Relatively little oceanic crustal material makes it to this depth and so for model simplification it can be ignored. A note on this can be added to the method section.

Check for a few minor misspellings: "in it the lithosphere;bouyancy; etc..."

We will correct these misspellings in the revised manuscript.

References

Agard, P. et al., 2005. Convergence history across Zagros (Iran): constraints from collisional and earlier deformation. *International Journal of Earth Sciences*, 94(3), pp.401–419.

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