Review for **Solid Earth**
Manuscript by Bottrill et al.
« Insights into collision zone dynamics from topography: numerical modelling results and observations...»

This paper is well written and develops around one major result: the evolution of the topography during incipient collision (i.e., after the critical stages of continental subduction driven slab breakoff) is nice and simple.
It is marked by short wavelength uplift of the overriding (continental) plate and short-lived back-arc subsidence until breakoff, that is before uplift (and shortening) starts affecting a much larger portion of the overriding plate. The Qom formation is thought to mark the locus of this topographic back-arc topographic low (or sag basin).
The work is well set against recent advances in modeling convergence and/or slab detachment (including the work of the authors), but less so with respect to topography. The figures are clear and both the results and the take-home message are clear.
The points below should nevertheless be addressed before publication :

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 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 3.

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?

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...

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

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! Let us remind that there were no significant slopes, no turbidites, no debris-flows in and around the Qom formation... Actually, the depth infered 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!

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).

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).

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.
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.

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

"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 and references therein)!

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

Overall I am still skeptical about the magnitude of topographic variations, but the relative trends and the explanations given sound reasonable. I therefore recommend the paper to be published pending moderate revisions.

Philippe Agard