

Interactive comment on “Folding and necking across the scales: a review of theoretical and experimental results and their applications” by Stefan Markus Schmalholz and Neil Mancktelow

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Received and published: 26 July 2016

This is a very extensive and nice review on folding and necking instabilities, which brings the reader up-to-speed with the available literature on the topic and I appreciate the effort that the authors have put into it. I believe it is well suited for publication in SE; yet I feel that a few topics could and should be discussed a bit more extensively, as it will make the review more complete and as I suspect that we will have to wait quite a few years for the next review on this topic.

1) Effect of brittle/plastic or Mohr-Coulomb rheology This review mainly focusses on the mechanics of elastic or (nonlinear) viscous folding & necking instabilities. If the number of citations is a measure of the scientific interest in a topic, a quick (and unrepresenta-

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tive) google scholar seems to at least suggests that the papers that get most citations deal with lithospheric-scale folding theories. As you already state at several places in the introduction, the upper crust (and potentially the mantle lithosphere) predominantly deforms in a brittle manner, rather than viscous or elastic. Most in-situ stress measurements suggests that Byerlees law represent the state of the stress of the crust pretty well and that the (upper) crust is close to failure, which can be reasonably well mimicked by Mohr-Coulomb plasticity. Most papers (from the 70ies & 80ies) that deal with lithospheric deformation take this into account by approximating the brittle layers as powerlaw layers with a high powerlaw exponent (in some cases with a depth dependent pre-factor).

Yet, given the linearization involved, it is not all that clear to which extend those results are actually correct in the nonlinear regime. In my experience, the analytical models are inconsistent with those obtained in numerical models in which the overburden has a pressure-dependent rheology (which you can also see by inserting $n=\infty$ in your equation for powerlaw detachment folding in table 2, which results in a dominant wavelength of zero). In Yamato et al. (2011, geology) we looked at this and found that numerically computed growthrate diagrams are capable of predicting the folding/faulting boundary (and the wavelength developing in finite strain numerical models). The growthrate diagrams are quite similar to the growthrate diagrams you discuss here for viscous folding (with a single maximum). Yet, it remains unclear how to best reproduce this with purely mechanical/analytical models. There are a number of papers on this subject (e.g. Johnson, 1980 Tectonophysics; Erickson 1996 JSG; Simpson 2009 JSG), and you do discuss some of the experimental studies. Yet, a separate and more extensive discussion of this topic would be very helpful.

2) Crustal-scale folds Given that the title of your manuscript has “across the scales”, I feel you move from outcrop-scale folds to lithospheric-scale folding a bit too quickly, while missing many of the spectacular and well documented 3D examples of crustal scale folding (such as in the Zagros). I believe there has been quite a bit of progress in

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recent years in understanding why there are folds (and not faults) in some areas, how this is related to the crustal scale structure and geometry, and how the 3D evolution of folds fits with constraints from uplift and river network deflection. I believe this fits well to the topic of your review and it would be great if you can include both figures of crustal-scale folds & a discussion on (mechanical) research on this.

3) LAS Dani Schmid & coworkers have already pointed it out in their review that the large amplitude solution should and can be discussed more completely here. To me, the main breakthrough in folding literature over the last say 15 years or so is that we can now model (ductile) folding instabilities with approximate analytical solutions in 2D and 3D up to large amplitudes. The importance of that cannot be emphasized enough as it really gives us a deeper understanding of physics and as this can serve as a starting point to start looking at some of the complications that play a role on a lithospheric scale (temperature and depth-dependency of viscosity & brittle effects). I would suggest to include the figures of Schmid & co.

4) Open questions Do you consider research on folding a finished topic or is there work to be done for young students? If yes, what are some of those open questions? Can you include a section on that as potential encouragement for young readers?

Minor points:

I. 17: including ON the lithospheric

I. 44: “competent” ->this is a very vague term. Do you mean a layer with a higher viscosity, a layer that has larger stresses if deformed? It would be good to explain how you define competent

I. 182: remove “easily” -> what is easy is relative for different readers.. Fig. 12: Mention in the figure caption that it was computed with equation 12.

I.262: From looking at figure 12, is is not so clear to me that the thin plate solution does not go to zero as you don't plot things up to zero. Maybe add a blowup in the figure?

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I. 275/fig. 13c: Same argument here; I don't see the oscillating behavior on the plots. Can you add a zoom-in of that in the figure ? Also, is this oscillating behavior reproduced with numerical models or is it an artifact of the analytics? A small discussion on the underlying causes would be appreciated.

I. 310/fig. 15: Explain better in the figure caption what the colored lines represent.

I. 664: “A reasonable value” -> I suppose that you think it is reasonable as it is similar in magnitude as ridge push forces; please explain add that here.

I. 997: How do you know that the analytical solution provides “reasonable accurate results”? Numerics?

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Additional note:

I published quite a few papers with the first author, including recent ones, which is something that I pointed out to the editors of SE before accepting to do this review. In SE there are currently no official guidelines regarding this, and as the review process is open anyone can comment on it. I do not feel that this has biased my review, but do want to point it out.

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-80, 2016.

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