Solid Earth Discuss.,
doi:10.5194/se-2016-56-RC2, 2016

Interactive comment on "The Subduction Dichotomy of Strong Plates and Weak Slabs" *by* Robert I. Petersen et al.

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Received and published: 26 May 2016

The manuscript discusses the effect of having slabs that are weak at depth but strong close to the surface on the dynamics of slab bending and subduction. Even though there are a few interesting aspects to the work (such as comparing various methods to compute the radius of curvature, and the temporal evolution of these parameters), I have a number of more serious concerns that should be addressed:

1) One of the main points of the MS seems to be to argue that slabs are strong at the surface and become very weak at depth. This depth-weakening is achieved by lowering the maximum cutoff viscosity at depth. The authors vaguely argue that this:

"is a computational efficient way of parameterizing the effective viscosity that may result due to any number of mechanisms such as Peierls creep, damage rheology or other

Non-Newtonian rheologies" [p. 2]

I disagree; what you are doing is rather arbitrary and has little to no physical justification. If you lower the maximum cutoff viscosity, you reduce the plate strength everywhere, and importantly the viscosity contrast between the slab & surrounding mantle (which causes the buckling effects you are after). This is in my opinion very different than the potential mechanisms you put forward:

a) Peierls plasticity has a slight depth dependence which lowers the plate strength only in locations where the stresses are reaching the yield stress (which is on the order of several 100 MPa for most Peierls plasticity models), In my experience this is something that occurs throughout the bending zone, but certainly not at much larger depths such as in the mid upper mantle or close to the 660 (even though exceptions can occur in the bending zone around the 660).

b) Non-Newtonian viscosities depend on the local strainrate, and this strainrate is quite similar outside and inside the slab (at least in most models I performed sofar). The result of that is that you weaken the asthenosphere in pretty much the same manner as the slab itself and therefore there is very little difference in the effective viscosity contrast between the slab and the surrounding mantle (and the slab is thus not weakened in the manner that you describe).

c) There might ofcourse be a damage rheology that does exactly what you simulate, but I am not familiar with what this mechanism should be (it would be good to have experimental evidence for it as well).

Given how crucial this assumption is for your manuscript, this should be thoroughly and convincingly discussed (and requires certainly more text). Alternatively, you instead focus on the effects of slab curvature and it's temporal evolution, and change the title and text accordingly (which seems to be the more natural thing to do).

2) Overall, the MS seems to be more on how slab strength controls the bending radius

and how this changes with time, rather than on the dichotomy itself or on why this is the preferred model for Earth. As far as I understand, whether a slab really buckles at the 660 or not is poorly constrained from tomographic models, which can at max. see a pile of slabs but not how those are internally deformed. Earlier work (e.g. Ribe et al. 2007 EPSL) has shown similar folding (among many other papers) and discussed that the thicker high velocity zones above the 660 might be explained by such a folding mechanism. Yet, the emphasis is on "might", and it is far from clear whether this is indeed the case. Using it as a fact, as you do in the conclusions, and taking this as proof that slabs must therefore be weakened at depth by "some mechanism [conclusions]" is not a very strong argument in my opinion.

More minor issues:

- figure 1: you write that the sticky air layer is 200 km thick, but in the text & table it is 100 km. - page 4, line 13/14: maybe rephrase to ".. lower mantle boundary, after which it buckles and piles" - page 4, line 20: You prescribe a slab with a radius of curvature of 400 km, and none of your methods is capable of recovering this radius of curvature? To me it seems that there is something iffy here. Have you tested that your methods are implemented correctly? It certainly deserves more discussion. - page 6, line 30: "have the advantage that the" - section 4.4: I find it encouraging that there is actually very little difference between models with a no slip lower BC and a highly viscous lower mantle at least during the initial model stages. As the viscosity jump effectively acts as a no-slip or a difficult-to-slip boundary this is not entirely surprisingly. The speedup of the slab that you discuss in the text is not apparent from figure 5, so maybe you can add insets of how the slab morphology looks like after a predefined time? - figure 3: Radius of curvAture

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Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-56, 2016.