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

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Referee #2 enumerates 2 major points to address.

The first point is regarding the method used to weaken slabs at depth. As described in the text we weaken slabs using a parameterization that limits the maximum viscosity at depth. The rheological laws, and parameters of the same, have been determined empirically from laboratory experiments on Earth materials using sample sizes millimeters or centimeters in size and at strain rates several order of magnitude larger than deformation in tectonic settings. A common practice in computational modeling is to adopt specific values for parameters from these laboratory-derived empirical relationships and employ them in models of regional or global extent, but formally applied at length-scales and timescales determined by the resolution of the computational mesh in the

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numerical model. This process requires some level of arbitrary decision making, and is reinforced by the modeler's experience and understanding of how these parameters change the overall behaviour of the system. In much the same tradition the weakening mechanism employed in our models achieves the overall effect of diminishing the mesoscale strength of the plate, on the order of tens of kilometers and on timescales of thousands of years and larger, and this scale is not represented by the empirical relationships derived from laboratory experiments. The deformation at the mesoscale includes subgrid physics and phenomena including an undetermined number of unresolved faults that allow for deformation and fluid transport, both of which will enhance the weakening present in typical flow laws developed from cm size samples.

Furthermore, in models that use a yield stress for modeling pseudoplasticity, it is common practice to assume that the material strength is instantaneously renewed back to its full strength, as determined by the empirically-derived flow laws, as soon as it is no longer subjected to stresses that exceed the yield stress. Although this is typically done, this represents an implicit assumption being made in such models. However, materials that have undergone significant yielding have likely developed other weaknesses (e.g. damage) at both the grain scale as well as the mesoscale including tectonic fabrics and generation of subgrid faults. Without labeling the decisions as whether to include, or not include, a post-yielded reduction in material strength, as arbitrary, our models represent an alternative to the defacto assumptions that are implicitly made. The interpretation of rheological relationships in numerical modeling outside of the experimental conditions that they are based upon is an important scientific discussion that should be held in the community, and at this point there are several alternatives that justify further exploration.

The referee has also raised the point that the mechanisms suggested in the manuscript, Peierls creep and Non-Newtonian creep, may not be present where we apply weakening. In a study by Garel et al, 2014, the authors developed models which used a composite viscosity calculated as the harmonic mean of several rheological

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laws, Peierls and Non-Newtonian flow among them. In that study the authors find the primary deformation mechanism of Peierls creep to be widely active inside the slab to depths of 660. Further, they show that dislocation creep to be the primary deformation mechanism within the slab at the edges while there continues to be an overall viscosity contrast between slab and background mantle. We have updated the Methods section to explicitly call out these results.

The second major point questions the interpretation of slab piles. The referee suggests that our interpretation of slab piles at the upper mantle/lower mantle boundary is stronger than the literature would allow. The referee cites Ribe 2007 stating that in that manuscript the interpretation is that the piles "might" be folded slabs. That manuscript addresses the "principal alternative" to folded slabs of shear thickening and citing Gurnis and Hager 1986 and Gaherty and Hager 1994, and concludes that the alternative mechanism is insufficient to reproduce what is seen in tomography: "The principal alternative, sometimes called 'advective' or 'pure shear' thickening, is uniform widening of the slab in response to the downdip compressional stress due to an increasing viscous resistance with depth. However, numerical models [24] and [9] show that this mechanism can thicken the slab by at most a factor of two, which is not sufficient to account for the tomographic observations." The reviewer states that, in our conclusions, we are taking the interpretation of piles as folded slabs as "a fact." Our conclusion is that in these models a weakening mechanism allows for folded slabs and, as compared to models without weakening, best reproduces those types images seen in tomography. Our discussion section has been updated to acknowledge that the interior structure of the observed piles is unknown and to cite Ribe's discussion of the likelihood of alternatives.

Referee 2 minor issues:

"figure 1: you write that the sticky air layer is 200 km thick, but in the text & table it is 100 km." The figure has been updated to reflect a 100km thick layer of sticky air

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“page 4, line 13/14: maybe rephrase to ’.. lower mantle boundary, after which it buckles and piles”’ Updated text accordingly.

“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.”

The fact that no method returns exactly the prescribed radius of curvature is a result of three factors: first, the 400km radius of curvature used in the initial condition is at the surface of the plate while the fit circles are mid-crust, second, the least squares fit to a cloud of randomly placed points is a statistical solution subject to noise, third, and most significant, no method selects all of the points from the trench to the tip of the slab. This “angle” method, in the case of a perfect circle will select points down to a depth of 117 km. The “depth” method is prescribed to select points no deeper than 150km. The “spline” method is also a statistical method, subject to an arbitrary smoothing parameter and the radius of curvature at any given point is only sensitive to nearby particles. Both of the circle methods only use an arclength that is a fraction of the total circle, which leads to the underestimation of the radius of curvature, and using a longer arclength recovers the correct radius of curvature. However, for this study we use an arclength that is approximately equal to length from the trench to the depth of the seismogenic zone.

The above explanation has been added to the text in the Radius of Curvature subsection of the Results Section. We have also added text in the initial condition section that makes it clear the radius of curvature is from the surface of the slab. Given a set of points that lie exactly on a circle each of the circle methods does return precisely the correct answer. The spline method is nonetheless subject to the smoothing parameter.

- page 6, line 30: “have the advantage that the” Updated text accordingly.

- section 4.4: I find it encouraging that there is actually very little difference between

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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? An inset showing both models at 4.4Ma is included in that figure and the caption has been updated to reflect this.

- figure 3: Radius of curvAture Figure title fixed accordingly.

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