14 August 2020
Wollongong, NSW, Australia

Review of

"Effects of basal drag on subduction dynamics from 2D numerical models" by Suchoy et al.

The authors have performed 2D numerical experiments of plate subduction in order to assess the role of basal drag on Earth's tectonic plate velocity-size correlation (if there is any), subduction behaviour (e.g. trench advance/retreat) and slab morphology. The numerical models are self-consistent, well tested for potential artefacts (e.g. box size) and well parameterized (e.g. rheological parameters). The text is well written. The research question addressed in the paper, which is understanding how mobile the plates are above the viscous asthenosphere during plate subduction and how this impacts the subduction system in general, is very appealing to geodynamics community, and the findings certainly have the potential to well contribute to geosciences in general only if the analysis of the numerical models is made more carefully and the arguments in the manuscript are widely revised based on the new analysis. Overall, the authors attempt to convey a number of interesting ideas in tandem, however, in its present form, the manuscript lacks a certain level of clarity and focus. To be more explicit, I provide my comments as below.

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I do find the arguments about linking slab strength to trench advance/retreat convincing and important. However, I do have concern that the analysis of the numerical models might have been done with partial ignorance of the forces (mostly resistive) acting on the subducting plate (Solomon and Sleep, 1974; Forsyth and Uyeda, 1975), henceforth limiting our understanding of how basal drag *solely* affects the whole plate subduction system. For example, the drag around the sinking slab (i.e. slab resistance) must also be acting along with basal drag and this has not been considered in the analysis so that the readers cannot separate the contribution of each. Therefore, it's not clear if it's the basal drag component itself driving the changes in plate velocity and slab morphology.

Similarly, once the slab dip is small and there is longer slab sinking into the mantle, analysis of *relative* significance of basal drag to the slab pull can be misleading because slab resistance is not taken into account with the increased slab area (i.e. slab interface in 2D) although it was considered for slab pull. The arguments of force balance cannot be reduced only to basal drag and slab pull as other forces also act and vary in time in the numerical model.

Equally important, some of the model results contradict with what the paper proposes throughout the text. For example, in the numerical experiments, a decrease in asthenosphere viscosity results in an increase in relative basal drag right after (for about 10 Myr) the plate has sunk to 660 km depth (comparing light red line with green line in Fig 6d). This is contrary to what is expected and needs clarification well before addressing other effects of basal drag (e.g. slab morphology).

The authors use a derived equation to estimate the slab pull and basal drag forces over time. These equations are (Eqn. 1, 2), most likely, valid only for iso-viscous plate and asthenosphere, and therefore may not be suitable to apply on the numerical modelling results. The authors also need to be cautious about necking of the subducting plate which results in significantly lower viscosities at the subducting plate as can be seen in Figure 3. This means, the slab pull force cannot act efficiently on the unsubducted part of the plate, hence an interpretation of the relative strength of basal drag and slab pull may become misinforming.

Abstract: It's worth briefly indicating why the force balance in subduction dynamics is incompletely understood (e.g. fails to explain plate velocity?) I think the force balance, or the method itself, is not to blame, but the contributions to the force balance by different forces are quite uncertain. This should be made clear.

[Lines: 140-150]: The lithosphere has a number of definitions and one of which is by viscosity profile (e.g. (Conrad and Molnar, 1997). In numerical models, the viscosities will vary and the effective lithospheric thickness that you have used in the estimation of basal drag will also change accordingly (Bodur and Rey, 2019).

[Lines 158-160]: Please justify why avoiding any slab detachment during subduction is favoured. If the model results are only applicable to plates not showing slab detachment, then this should be mentioned early in the text or in the abstract. It's important to acknowledge that slab detachment has been used to explain important features of the Earth (Göğüş and Psyklywec, 2008; Duretz and Gerya, 2013; Hacker and Gerya, 2013)

[Eqn1]: Please provide the derivation of the equation for basal drag and/or the page # of the citation you provided.

Section 4.3 [Lines 364-365]: The slab dip you consider here is quite higher than numerical models show. Why? It's also unclear what sort of data you have used to calculate the slab pull and basal drag estimates in Table 1. Please be more specific so that one can derive the same results individually for further reference. Also, for different plates, the asthenosphere viscosity ($\eta_{asth}=10^{19}$ Pa·s) is not necessarily the same, so you may need to consider different viscosities, at least mentioning about it.

[Line 391]: Although they can be correlated, Fig. 8b doesn't show subduction zone length, but the plate size vs. plate age at trench. It's better to be more specific.

[Lines: 399-401]: The correlation is weak already (based on error bars and scattered points in Fig. 8a), and the argument on explaining an already weak correlation "*at least in part*" is making this sentence more confusing for readers. I recommend restructuring those lines.

Additional Comments:

[Line 156]: 1024 Pa·s – 1025 Pa·s should be changed to 10^{24} Pa·s – 10^{25} Pa·s.

[Line 383]: Fig. B1 needs to be changed to Fig. C1.

References:

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