Black: Reviewer's comments.
Blue: Our response to comments from reviewers. Line numbers correspond to the updated version of the manuscript.

Comments to the author:
Dear authors,

Your revised manuscript has now been seen by two reviewers. One has a number of minor suggestions (please note that there are in the form of a pdf supplement), whereas the other is more critical regarding the classification as tectonic erosive margin. I would ask you to take the suggestions of both reviewers into account in your revision. I recommend a 'major' revision as i think the revision is somewhat more than a 'minor', but it is mainly a matter of presentation (that is, no new models for example are suggested) and i hope therefore that the efforts on your side will be reasonable.

With best wishes,
Susanne Buiter

Referee #3 (comments in PDF)

Thank you for the comments! We have addressed all the minor changes suggested in the annotated PDF. In addition, there are two comments we'd like to address in more detail.

Line 69: The reviewer suggested providing more details on the 'dominant mode' from Clift and Vannucchi (2004). The authors of that study did a binary classification of margin type (accretionary or erosive) based on which mode was more dominant in the last 10 Myr. We consider that providing more details on the assumptions and analysis that went into that study is beyond the scope of this paper. The reader may refer back to Clift and Vannucchi (2004) for these details.

Line 360, 386: We deleted 'convergence rate' from Line 360 because the paragraph only refers to margin style and radius of curvature. We also revised reference to Brizzi et al. 2021 in discussion about convergence rate in accretionary and erosive margins. While there is a parallel between our study and that of Brizzi et al., 2021, the analysis of results differs in major aspects:

- Analysis in Brizzi et al. 2021 takes model results at a snapshot in model evolution, at ~4 Myr, which is not necessarily indicative of the same stage of evolution for all simulations. As seen for high-angle AW models here, results may differ significantly in early stages of subduction compared to late stages of subduction.
- There is no classification of margin type in Brizzi et al. 2021. Therefore, the statement 'low convergence rates for accretionary margins' (as opposed to erosive margins) is not necessarily true, because the models in Brizzi et al. 2021 suffer from similar model limitations for erosive margins as ours.

We consider that understanding the physics governing erosive margins is a future challenge for numerical modelling.

Referee #1

I have reviewed the revised version of the manuscript "The effect of low-viscosity sediments on the dynamics and accretionary style of subduction margins" presenting a suite of numerical subuction models investigating the influence of input sediment layer thickness and viscosity on subduction dynamics and wedge/interface properties.

While the authors have made revisions or fair rebuttal to certain comments of the initial two reviews (e.g. title, abstract, discussion, wedge angle vs. volume, new Figure 2, additional figures in the supplementary material), I find that the issue regarding designation and classification of "tectonic erosion" vs. "accretionary wedge" margins (details hereafter) still makes major revisions necessary before publication.

Thank you for appreciating the previous revision of the manuscript! We agree that our model results are far from resolving processes happening in tectonic erosion margins. Therefore, we changed the names of end-member models into "tectonic coupling" and "accretionary wedge". We adapted the figures and manuscript text to have a clear separation between model results and nature.

- Indeed, no actual tectonic erosion is observed in the presented models. The authors state in the results section that "Entrainment of sediments within upper plate material at the interface is indicative of some erosion of the upper plate" (line 253), but incorporating sediments to the upper plate is the opposite to upper plate erosion.The authors choice of labelling small accretionary wedge as a "tectonic erosive" margin thus seems impromper, and misleading comparibg to nature, where the inverse relationship between convergence rate and sediment thickness is observed (Supp. Fig. S2 vs. Fig. 4).

We agree with the reviewer that we do not model "tectonic erosion". While the difference between accretionary and erosive margins constituted the motivation of this study, erosive margins are governed by processes not fully understood, including hydrofracturing, erosion, melting, or deformation of multi-phase material. No numerical model can do that at the moment.

Following the reviewer's suggestion, we tried to find a good terminology for our results. We renamed our results previously-labelled 'tectonic erosion' as 'tectonic coupling. We made changes throughout the manuscript, and we added an explanation in Paragraphs 351, 390 and Conclusion, to link this regime end-member with tectonic erosion in the limit of viscous rheology. The pdf with highlighted changes shows all relevant changes.

- The distinction between "accretionary" vs. "tectonic erosion" models only relies on diagnostics quantifying the degree of sediment accretion (wedge width, wedge angle), subduction dynamics (convergence and trench rate) and slab morphology (radius of curvature). For these diagnostics, there is no clear dichotomy in the models (cf Supp. Fig. S9) but rather a continuity from small wedges (low convergence velocity, small wedge angle and width, small radius of curvature associated to steeper slabs) to large wedges (vice versa).

We agree that model results represent a continuum between ‘accretionary’ and ‘tectonic coupling’ end-member models. However, we still need to define end-member models, and we make the binary classification for analysis purposes. But as seen in Figures 4-5, there is a gradual transition between them. We added an explanation in the text on this point (Line 471).

- Hence I strongly advise against labelling the models as “tectonic erosion” in the results section. I suggest that the authors discuss how low-accretion margins could related to tectonic-erosive margins in the discussion, but present their results (Fig. 3-6) as a function of input model parameters (e.g. sediment thickness and viscosity). End-members simulations would thus become “low viscous coupling” (large thickness, low viscosity) vs. “high viscous coupling” (small thickness, large viscosity).

We followed reviewer’s suggestion to rename model end-members - as ‘tectonic coupling’ and related results with tectonic erosion in Discussion. However, we did not choose the terminology suggested, “low-viscous coupling” vs “high-viscous coupling”, because our motivation was to understand why some margins accrete sediments while others do not. We wanted to keep some resemblance to the motivation of occurrence of accretionary wedges.

Other comments :

- Despite the zoom, velocity arrows are still illegible on the left and middle panel of Figure 2. I suggest that this Figure is rotated (landscape format) to enlarge the velocity field, referred to on lines 250 (“”the motion between subducting and upper plate in tectonic erosion margin is accommodated in the middle of the sediment layer, a region of high strain-rates) and 275 (“internal counter-clockwise flow inside accretionary wedges”). Another possibilité is to reduced the number of arrows and increase their scaled size.
We rotated the figure in landscape format as suggested. We think the velocity magnitude (colorscale) is more important than the actual arrows, and we hope that is clearer in a magnified figure.

- the criteria of “transient vs. steady-state” (line 227, 246, 306) cannot be used to discriminate between margins types, cf Supp. Fig. S9. The authors should directly refer to the continuous spread of wedge angle and width between high and low end-members (Fig. 4).

The distinction of steady-state (constant) vs transient (changing) is important in defining the end-member models. We agree that all simulations then occur as a spread between these end-member models. But we first have to define the end-members. One way to understand this binary classification is: 1) geometry of the wedge stays constant (steady-state) in low-accretion models, 2) geometry of wedge changes (transient) in high-accretion models. Therefore, our distinction between transient or steady-state relates to changes in diagnostics over model evolution. Diagnostics in tectonic coupling margins remain constant over time. For example, $dR_c/dt = 0$ (i.e., steady-state).

- An interesting result is the partitioning of weak interface material between subduction and wedge (Fig. 6). Even though the “sediment” layer in the models is not buoyant, I find the models relevant at first-order given that subduction interface is a melange of (lighter) sediments and (denser) metamorphic oceanic crust. However, Fig. 6 presents the relative volume fraction (in %) as a function of a dimensionless time, which makes it hard to directly compare absolute subducted volumetric flux rates (in m3/yr) between simulations (as noted by the authors lines 430-434). Maybe the authors could estimated an absolute value rather than a relative fraction (since initial sedimental thickness vary between simulations)

We appreciate the comment and agree that estimates of absolute sediment fluxes to depth are important. However, our model setup overestimates sediment density (here, sediments have a tendency to subduct in the mantle) and thicknesses, so absolute values of sediment fluxes would represent an upper bound. We prefer to refine our model setup further before we make estimates of absolute volumetric fluxes, which could have major implications for water/volatile cycles.

- I am not sure I follow the authors when they say that "The definition of subduction interface in numerical models could also be relaxed" (line 574). Indeed, "the tendency for the subduction interface to develop spontaneous thickness variation as the models evolve" (line 578) is already present in the models (see e.g. Fig. 2 and S4) hence it is not a "discrete isosurface" (line 58).

Line 456: This statement is intended for the numerical-modelling audience. For a long time, one validation of numerical models of subduction was to ensure that the thickness of the interface stays constant (see discussion in Sandiford and Moresi, 2019). We rephrased the sentence for clarity.

- some informations are missing in the section on model set-up : sediment layer and basaltic crust densities (same as lithospheric mantle?), thickness of lithospheric layer below plate core (refer to "slab" on line 146) – 45 km ?

Line 141: We wrote 'plates are 85 kg/m3 denser' relative to reference mantle density. This includes sediments, crust, lithospheric mantle. The plates have the following structure: 80 km thick plate, with 20 km thick core (centered, as shown in Fig 1), and 15 km weak crust+sediments.

To answer the reviewer, the thickness of lithospheric mantle below core is 30 km.
In line 142, we replaced 'slab' with 'lithospheric mantle'.

- change the sentence "All other parameters are kept the same among simulations" (line 151) since the weak crust thickness varies from 10 km (when hsed=5 km) to 5 km (hsed=10 km).
Line 151: We rephrased and moved the sentence in the next subsection. We hope the distinction between varied and fixed input parameters is now clearer.

- the sentence "We also consider constant sediment fluxes at the trench" (line 152) is not clear. The authors maybe refer to constant sediment flux through time, but this flux differs between simulations (since sediment thickness is varied, which changes convergence velocity).
Sentence was removed.

- the sentence “a no-sediment case is represented by high-sediment viscosity (i.e., higher proportion of crust at the interface)” (line 168) is confusing. I suggest instead: “the highest sediment viscosity (eta_0) is equal to the weak crust viscosity, hence analogue to a no-sediment case”.
Line 163: Thank you for the suggestion! We rephrased the sentence.

- the sentence “the trench is retreating, specific to ocean-ocean subduction” (line 183) is uncorrect: some intraoceanic subductions exhibit trench advance in nature (e.g. Izu-Bonin).
Line 177: Replaced ‘specific to’ with ‘common to’. Majority of intraoceanic subduction zones are retreating (Schellart et al. 2007).

- please remove the 4-digit precision in Table 1 which makes the read more difficult. A round number suffices for Rc, alpha_wedge, W_wedge, and a 1-digit number is enough for u0, uT, h_trench, hmax and hmin.
Table 1: We removed the 4-digit precision, and rounded off to 1 or 2 digits to retain the variability of results.