Black: Reviewer’s comments.
Blue: Our response to reviewer’s comments. Line numbers correspond to the updated version of the manuscript.

RC1: 'Comment on se-2021-137', Anonymous Referee #1, 20 Dec 2021

General comments

The article “The effect of sediments on the dynamics and accretionary style of subduction margins” presents 2-D compositional models of free subduction with a layering of the subducting plate including a sediment cover (of variable thickness and viscosity) over a stronger mantle layer. The authors calculated model diagnostics during the simulation to categorize the subduction simulations either as accretionary margins or as erosive (non-accumulative) margins.

I find the model results and comparison to natural subduction zones relevant to discuss how the interface properties feedback with convergence rate through plate viscous coupling, despite a concern regarding the simulations featuring very-low viscosity sediments (see point 5 below).

I suggest below major corrections regarding

(i) the “angle of attack” of the paper (points 1 and 2 below)

(ii) choice and representation of the diagnostic parameters (point 3)

(iii) additional reference to previous studies and interpretation of some results (points 4-5)

(iv) a new repartition of figures between main article and supplementary (point 6)

These corrections do not require any new simulation to be run.

Major comments

1- The title and abstract do not at present capture what in my opinion are the main results of the models. Indeed, only late in discussion (line 440) do the authors acknowledge that “it is unclear whether sediment influxes affect convergence rate, or convergence rate affect sediment accumulation at trench, or both”.

I suggest that this should be very clear from right from the introduction that the sediment layer properties will be both an input in the models (viscosity, thickness) and also a diagnostic output (width, angle, volume) because feedbacks are expected between interface properties, viscous coupling, convergence rate and sediment accumulation.

Hence the paper could not only bring insights on “how sediment fluxes influence subduction dynamics and plate coupling” but also on “how the margins form in response to subduction dynamics” and how these feedbacks lead to unstable, time-dependent subduction dynamics and sediment fate (see point 2).

The abstract should also contain more explanations on the processes, since it is at the moment too descriptive, listing the features of the different margins.

Title: we modified the title to indicate the focus of the study on the effect of sediment viscosity on subduction dynamics and margin style.

Abstract: we rephrased the abstract to include specific suggestions below. We tried to keep the abstract brief and clear. Since our experimental setup investigates the effect of low-viscosity sediments on subduction dynamics, we leave the effect of convergence rate on sediments for the discussion (because it remains an open scientific question).

2-Beyond the end-member classification between accretionary vs. erosive margins, I think that the model results could be used to highlight the temporal evolution of sediment fate in subduction zones. The gigantic error bars in Figure 4 (is a "mean" value relevant for e.g. radius of curvature or wedge width?) suggest the need for a more detailed post-processing of some simulations, to go beyond the mild sentence "In any given system, both processes may be occurring simultaneously, either in time and space or at the same time in different parts of the subduction zone" (lines 447-448).

We agree that the evolution of each model gives interesting insights and that there is a temporal variable that needs to be taken into account when characterizing the margin types. However, we purposefully wanted to focus on the outcome of margin type (accretionary vs erosion) using the diagnostic parameters, as this has not been done systematically before.

We consider that the evolution of diagnostics parameters shown in Figure 3 demonstrates that the means reflect the overall system behaviour (i.e., larger radii of curvature or faster convergence rates for accretionary wedges). Figure 3 also shows that for tectonic erosion the variability from the mean is small, such that the grey bars in Figs. 4-5 are small. On the other hand, variability of bars for accretionary margins are larger, which is indeed what we intended to highlight.

However, we provide additional evolution plots in Supplementary Material for reference models (Figs. S3-S6).

Lines 408-419: We also improved on the discussion of our model, explaining that in order to address the sentence that prompted the reviewer and the question "it is unclear whether sediment influxes affect convergence rate, or convergence rate affect sediment accumulation at trench, or both" require different experimental setups.

Therefore I strongly suggest that the "unstable" mode is given more attention. This mode, briefly described in lines 303-307, suggest that "the accretionary wedge quickly reaches a maximum size and critical angle and instead of moving laterally the wedge, material is being expelled down the subduction channel." The time-dependency of the sediment wedge geometry is in my opinion key to analyze for two reasons:

- it could explain why the models predict erosive margins at low convergence rates (Figure 4) whereas natural statistics show the opposite (Figure S2).

- the variation of convergence velocities through time because of "avalanche" effect of sediment wedge (or because of temporal variation in the sediment supply rate due to other factors) offers a new perspective on the spatio-temporal variations of plates velocities.

We agree that this model setup could provide an explanation to explain slow convergence rates for accretionary margins. However, our current model overestimates the density and initial thickness of sediments. Therefore, we prefer not to highlight the unstable model outcome and make unrealistic statements. A future study can investigate this question with a refined model setup.

We clarified in Lines 284-286: This margin style is a consequence of the density model chosen here, in which sediments have the same density as the rest of the lithosphere. Clearly, this is an overestimated effect.

In the meantime, Fig S6 shows model snapshots of a reference unstable mode. Evolutions of diagnostics parameters are shown in Fig S13-18.

3- The 2-D volume of the accretionary wedge (triangle ABC in Figure 1c) should be an additional diagnostic parameter since it allows a clearer distinction between "tectonic erosion" vs. "accretionary margin" that the current criteria described in lines 287-290.

We do not consider area of triangle ABC (or volume) a unique metric for either accretionary or erosive margins. That is because we also vary the thickness of the upper plate, which affects the length of the subduction interface. For example, the area of an accretionary margin with thin upper plate can be similar to the area calculated for an erosion margin with thick upper plate. Brizzi et al. 2021 calculated the area in the wedge, but they do not vary the geometry of upper plate, so in this case the area is a good metric for accumulating sediments.

We consider the angle is a better quantitative measure for differences between end-member models, as it is independent of thickness of upper plate. Actually, we consider the dynamics and geometry of plate interface should be formalized using dimensionless parameters (i.e., Gerardi et al, 2019, but they imposed a constant thickness of the interface, not a developing accretionary wedge). We chose the diagnostics parameters to be compatible to the ones in statistical analyses, but we agree that a formalized analysis using dimensionless parameters is a matter of future investigation.

An analysis of total volume of sediments subducted vs accreted is shown in Figure 6 instead.

For example, I do not see on Figure 3 that only “low-angle accretionary margins have increasing radii of curvature” (high-mangle margins do too) or that low-angle margins have “fairly constant” whereas high-angles ones have “irregular” convergence rates (this is too qualitative).

There is a clear difference between tectonic erosion and accretionary margins. The majority of diagnostics parameters in TE remain constant, and Rc and alpha have small values. In accretionary margins, Rc and alpha have higher values, increasing from initial conditions. The difference between low-angle and high-angle accretionary margins is indeed the evolution of convergence rate. In low-angle AW, the convergence rate does not vary, while in high-angle AW, the initial convergence rate is fast, and then it slows down when the wedge is large. We hope this is now clarified in Section 3.1.

Furthermore, tracking the evolution of this 2-D volume through time should highlight the temporal evolution of the sediment fate, for example by plotting:

- Figure 6 for unstable simulations

- co-evolution of 2-D volume (x-axis) and convergence rate (y-axis) as a function of time (marker color in the x-y diagram).

For reasons explained above, the results of the unstable mode are overestimated and should not be highlighted.

4- Please consider the following paper for the introduction dans discussion (first 3 are mandatory given the article topic):

Thank you for suggesting these publications! We complemented the literature review and discussion with the suggestions below.

- recent paper by Menant et al., Nature Comm, 2020 on the spatio-temporal evolution of forearc topography from transient stripping events of slab surface (basal accretion or erosion) https://doi.org/10.1038/s41467-020-15580-7

For example, their discussion on topography evolution (see their Figure 3) is quite complete with a higher resolution than the present study.

Added reference in introduction and discussion about detailed processes in accretionary margins.

- Cizkova & Bina, EPSL, 2019 - how plate interface viscosity plays a major role in controlling trench retreat https://doi.org/10.1016/j.epsl.2018.12.027

Added reference to Methods and Discussion.

- Arcay, Solid Earth 2007 – influence of rheological properties of the weak layer on viscous coupling https://doi.org/10.5194/se-3-467-2012

- models of mass fluxes (and temporal evolution) of sediment fluxes in a subduction system – Beaumont et al., 1999 https://doi.org/10.1029/1999JB900136

Added references on previous numerical models of accretionary margins

- review on subduction erosion – Straub et al. 2020 https://www.nature.com/articles/s43017-020-0095-1

Added reference to Introduction about erosion margins

- analogue sandbox experiments on largins, and importance of convergence rate vs. sediment rate - Lallemand et al., 1994 https://doi.org/10.1029/94JB00124 or Gutscher et al., 1998 https://doi.org/10.1016/S0191-8141(97)00096-5

Added references on analogue models of accretionary margins

- strong interface coupling for a thin sediment layer and implications for seismogenesis: Bangs et al., JGR, 2020 https://doi.org/10.1029/2020JB019861

Added reference to Introduction about influence on seismic coupling

- accretion at collision margins – Selzer et al., 2008 https://doi.org/10.1029/2007TC002169

Added references on previous numerical models of accretionary margins

- papers on how a weak crust may detach from the slab (in these papers in the transition zone, but perhaps similar to the "expulsion" of weak sediments down the subduction channel in unstable mode SubdSed04_100 on Figure S7), e.g. van Keken et al. GRL 1996 (https://doi.org/10.1029/96GL01594) and Yoshida et al. JGR 2012 (https://doi.org/10.1029/2011JB008989)

We think these two suggestions are beyond the scope of this paper, and are not immediately relevant to the discussion. Van Keken et al 1996 refers to the behaviour of crust (not sediments) at the mantle transition zone due to changes in mineral density.

- papers discussing the order of magnitude of sediment viscosity since it appears critical an input in the models. For example, discussing the effect of sediment hydration/fluids on viscosity, the expected strength of detritic (continental) sediments vs. oceanic sediments.

We explained in the methods section (Lines 136-143) our choice of sediment thickness and viscosity using the results from Behr and Becker 2018.

The method section needs to be clearer on the choice of viscosity parameterization for the weak crust. At present, the sentence "The crustal thickness represents a parameterization of the strength weakening of the lithosphere with depth due to hydration and weak sediment cover" (line 155) is confusing and needs to be clarified: why is the depth-dependent parameter related to the sediment cover? Does hydration increased with depth?

The comment "high viscosity sediments are representative of a stronger crust and mantle component at the slab surface" on line 392 is too light.

Lines 136-143: We addressed individual suggestions from intermediate/minor comments, and we hope the methods section is now clearer on the viscosity parameterization.

5- I am worried that the sediment body labelled "high-angle accretionary wedge" (in Figure 2) is not a good analogue for the natural object. Indeed the sediments seem to accumulate only downward: does the sticky-air layer(viscosity > 10^18 Pa.s?) prevents the upward growth when the sediment viscosity is too low (10^18 Pa.s)?

The sticky-air does not cause the effect described by the reviewer, which is due to the higher density of sediments used in this set of experiments. The sediments have the same density as the slab and tend to be dragged down in the subduction channel. Future experiments should further investigate the effect of sediment density.

The authors should comment in section 3 upon the thickness of the accretionary wedge, reaching 100 km in Figure 2 !!! The wedges simulated in Menant et al. 2020 (https://doi.org/10.1038/s41467-020-15580-7) are much thinner and appear closer to the observed geometries on Earth.

We caution that our experiments overestimate wedge geometries for thick sediment layers in results and model limitations (Line 459).

Thus, the “flat slab subduction” of the high-angle accretionary wedge result from the irrealistic accumulation of low-viscosity sediments downward, with the wedge forcing the slab to flatten at very shallow depths.

I therefore strongly suggest that the authors put the emphasis on “small-volume wedge” (analogue to erosive margins) vs. “large-volume wedge” (accretionary) rather than over-analyzing the differences between high- and low-angle accretionary margins.

We agree that the emphasis should be on small-angle accretionary wedges. We adapted the text accordingly in the Results and Discussion sections. We also noted that the exaggerated effect of the wedge on slab curvature comes from the relatively high sediment density.

6- Along with the change of focus of the article main results (see points 1-2-3 with suggestion of new figures), I suggest that:

- Figure S2 (and maybe S1) is moved to the main article to be directly compared with Figure 4. Beware to use the same scale and units for model (Figure4) and nature (Figure S2) graphics.

There are two main reasons why we prefer to keep Fig S1 and S2 in the Supplementary material: 1) Fig. S1, S2 show replotted data - already published -, and we do not perform a new analysis on this data to warrant inclusion in the main text, 2) the model shown here is highly simplified to allow us for a direct comparison with the data.

- addition of a figure (and movie?) showing an unstable simulation through time (SubSed04_100?)

Added in Supplementary material Figure S6.

- Figure S7 is moved to the main article

The methodology and analysis presented in this manuscript is intended to characterize all model results using a limited number of diagnostics parameters. We added supplementary figures including full results in order to be transparent and to show that our diagnostics work. We consider Figure S7 (currently Fig S11) to be beyond the scope of the main text and would require description of all panels.

- Figure 5 (topography) is moved to the supplementary

We agree that the topography analysis in Fig 5 provides a less clear picture on the correlation between the effect of sediments and topography. However, we consider this an important model result because topography can be a direct observable and be used in future analyses. We also expect a stronger correlation in ocean-continent subduction simulations or in simulations with imposed plate convergence. Pusok and Kaus (2015) show that both convergence rate and upper plate parameters control topography build-up.

Intermediate comments

- the authors need to better describe the concept of tectonic erosion at non-accretionary margins, to discuss both the variation of volume of the arc crust and the removal of upper-plate material by basal erosion. The current figures do not show any erosion: on Figure 2, the velocity field is unreadable because too small. I suggest that the authors provide in the supplementary material a zoom of the interface/upper plate base to more clearly see whether 'the sediment layer is an integrated part of the subducting slab and is eroding the upper plate.' (line 275).

We re-wrote the entire subsection on tectonic erosion. We also modified Fig 2 to include strain rate maps and velocity arrows in an enlarged area at the subduction interface.

- Same thing about the viscous coupling that need to be better defined and better tracked in models outputs. Ex on lines 297-300: the localization of the decoupling motion between lower and upper plates is not visible from the too small velocity field vectors in Figure 2. The authors could either plot zoomed-in view of the velocity field at the interface, of plot a velocity profile along a transect through the interface to visualize the velocity transition.

As per request we have provided a zoomed-in view of the velocity field for these cases in Fig 2. Supplementary figures S3-S6 also contain zoomed-in views of interfaces.

- the authors need to clarify when they talk about correlations or when they refer to causal links (a process has been identified).

For example on lines 35-37: the very assertive sentence "On a regional scale, sediments influence patterns of deformation by controlling the morphologies of subduction interfaces, accretionary prisms and forearc basins" refers to three observational papiers and only one modelling paper (Simpson, 2010).

Line 33: The statement is written intentionally as a general statement, and is supported by the evidence contained within the references provided which are well-cited papers indicating such knowledge is known widely amongst researchers in this area.

Example on lines 5-6 and 440: it is not clear whether the sentence refers to a descriptive correlation ("Accretionary margins are dominated by accretion of thick piles of sediments") or to a causality ("tectonic erosion is favored in regions where the sedimentary cover is <1 km").

Within the context of an abstract, the statement in lines 5-6 is clearly descriptive, which is made obvious because the prior sentence, which states that there are two classes that can be described by considering the sediments, alludes to further elaboration. The statement on 440 (current Line 408) is a statement made in a different context of the discussion section, which is made to address the ambiguity between cause and correlation.

- section 2: clarify whether "weak crust" (throughout the text) refers to both green layers on Fig.1 ("magmatic" crust and sediments) or only to the dark green layer ("magmatic" crust).

The weak crust is the dark green lithology and sediments are the light green lithology. We added an explanation in Fig 1 caption.

The sentence "15 km combined weak crust and sediments" is not clear: does the thickness of the magmatic crust vary opposite to the sediment thickness variation? (the sentence on lines 158-160 should appear earlier)

Yes, the thickness of the weak crust varies in order to keep the combined thickness constant. By keeping the combined thickness constant, we keep the buoyancy of the plate constant, and we can isolate the rheological effect.

Did you run model with the same sediment thickness but different magmatic crust thickness to check that this did not have also an influence on the model dynamics? Indeed, when the thickness of the sediment layer is varied is the different simulations, the thickness of the magmatic weak crust does not remain constant.

No, this is left for a future investigation. As explained above, the aim was to isolate the rheology of weak crust and sediments while keeping slab pull constant. If we keep the crustal thickness constant and vary the sediment thickness, we would vary the initial buoyancy force too.

- Figure 3: I see the point of the authors' concern about variable timestep magnitude (section 3.1) however I am much concerned about plotting diagnostics parameters' evolution as a function of (time-dependent) timestep and not of time or dimensionless time. It is very hard to understand subduction dynamics if the time-scale is dilated (in the current version, the spacing between different timestep may not be equal to the same absolute duration since we do not know the dispersion of timesteps around the mean value). Hence, I demand that Figure 3 plots show the x-axis with dimensionless time (e.g. time divided by the mean timestep shown in Fig. S3a).

We replotted data in Fig 3 and Fig 6 such that the x-axis now represents characteristic time (tchar). tchar was calculated in the following way: 1) identify tfinal as the time taken from beginning of simulation needed to subduct the entire slab ~2500 km (i.e, Marker 1 reaches the trench), 2) normalize the time variable such that tfinal becomes tchar = 1 (i.e., tchar = t/tfinal). tchar now varies between 0 (initial time) to 1 (full consumption of slab).

Minor comments

-line 1: I would not refer to sediments as "continental material" which refers first to silicic magmatic rocks (e.g. granite). I suggest you keep the term "sediments", or refer directly to the chemical elements.

-lines 2-3: if the "recycling of volatiles" links well with the subduction of sediments (full of water and carbon), the "petrogenesis of continental crust" and the recycling of "continental material" are not obvious to relate to the paper topic during the first read of the abstract > I suggest you rephrase into "… is important to the understanding of both recycling of water and carbon back into the mantle, and the petrogenesis of continental crust through water-induced partial melting."

These introductory sentences were removed in the updated version of the abstract.

- line 4: perhaps rephrase the unclear sentence "When sediments are considered, convergent margins appear to fall into one of two classes"

Following this suggestion, we have clarified the statement: "Observations of sediments at subduction margins appear to divide them into two classes: accretionary and erosive."

-lines 6-7: what is the "geometry of the global subduction system"? It is not clear whether you refer so the accretionary wedge or to the slab.

We removed the confusing sentence.

- line 9: "how sediment fluxes influence subduction dynamics and plate coupling" could be more accurately rephrased as "how sediment fluxes influence subduction dynamics through plate coupling"

Corrected.

-line 11: you refer to "modes" of the subduction interface, however you only categorize them depending on the thickness of the sediment cover (erosion vs. accretion) and on the angle (high vs. low). Hence I suggest you refer to them as "geometries of the subduction interface", that you can later relate to the system dynamics (how did these different geometries form over time?)

Adapted to 'geometry modes'.

-line 13: I would be more precise regarding "the extent of viscous coupling" with "the lateral extent and the depth of viscous coupling".

We prefer to keep it simple because it is not just the spatial extent of coupling between plates, but the strength of coupling too (weak versus strong coupling).

-line 14-15: you should clarify how the properties of the sediment layer modulate viscous coupling, ie. “When the viscous coupling is increased” or “when the viscous coupling is reduced” = how is the thickness/viscosity of the incoming sediment layer different? You need to make the link with the previous sentences categorizing the styles of interfaces (high viscosity, low viscosity).

Corrected as suggested.

- lines 16-17: the sentence “Diagnostic parameters are extracted automatically from numerical simulations to analyze the dynamics and differentiate between these modes of subduction margin.” should go higher up in the asbtract, right after the description of the models (before “Our results...”).

Modified as suggested.

- line 18: the authors should specify at which depth is the “radii of curvature” considered, and they should expand on which “observations of present day SZs” are the model predictions confronted.

This level of detail is not appropriate for an abstract, and these details are discussed in the appropriate sections where Wu et al. 2008 is provided repeatedly as the reference for such observations.

- lines 25-26: the point “ii) whether large volumes of existing continental crust are ever recycled back into the mantle over long periods of geologic time” is less obvious than the others two (“petrogenesis of continental crust” and “cycling of volatiles”). I suggest that the authors eithr drop it or clarify it (I personnaly think of continental subduction – generating HP eclogitic igneous rocks - when I read this sentence, not of sediment subduction).

Because terrigenous sediments have long been associated with geochemical reservoirs in the mantle (specifically EM2), we would not make such a statement as to the relative importance of this process compared to the other two.

- line 34: “The lubricating effect of sediments […] is critical for the mechanism of plate tectonics” is a bit an overstatement > maybe mention only the importance a decoupling interface.

It is a possibility in our minds that this lubricating effect is indeed critical to the operation of modern subduction (and by extension modern plate tectonics), and it is sediments that provide this decoupling interface. Recent study by Sobolev and Brown, 2019 attest to the fact that availability of sediments has been important for Earth’s tectonic evolution. https://www.nature.com/articles/s41586-019-1258-4

- line 36: what is the “morphology” of a subduction interface = dip, thickness, lateral extent, maximal extent? Same for the accretionary wedges and forearc basins, please specify which properties you are referring to.

Line 32: We agree with the comment, but we try to keep the body of literature brief and refer to several citations for a more in depth look into morphology.

- line 43-44: it is a bit unfair to state that “Most of the effort in subduction dynamics studies focused on quantifying dissipation due to slab bending” given the diversity of focus of subduction models, including the ones co-authored by the authors themselves. You should replace by “a lot of effort”...

Line 39: Replaced with ‘considerable effort’.

- line 47-48: the papers cited do not refer to “analytical models” but to “laboratory and numerical models”.

Line 43: Corrected to ‘Laboratory models’.

- the introduction is quite lengthy and somewhat repetitive and could be more concise, especially for lines 63-97.

We shortened the text in this section (63-97), without compromising what would be essential background information needed to understand subsequent sections of the manuscript.

- line 63: it is an overstatement to write “in reality, subduction interfaces are […]” since there is a poor direct access to these deep features. The paper by Agard et al. refers to either indirect geophysical monitoring or to fossil SZ exhumed rocks, which are proxies/models but not “real” samples.

Line 58: This was not our point, but rather that the entity of a “subduction interface” should not be considered as a discrete layer or surface separating two plates, but rather a messy zone that incorporates material from both sides. We have replaced the phrase “in reality” with “Indirect observations suggest” and adjusted the statement with more emphasis on this aspect.

- l. 65-68: this statement is oversimplified and the assertion should be less bimodal (it is not one type of margin or the other). The authors cite for example earlier the paper of Simpson (2010) who states that “rather than viewing compressive plate margins as accretionary versus erosive, the dominant mode may repeatedly switch back and forth through time”.

We agree that some margins may switch back and forth between accretionary type and erosion type. Clift and Vannucchi 2004 have a detailed discussion on this in their attempt to classify subduction zones bimodally. However, for the purpose of the investigation, it is useful to define end-member models.

- the definition of accretion and tectonic erosion should occur earlier in the introduction (presently on line 69 and 80), i.e. before discussing the controlling factor

Agreed. These definitions are now introduced earlier and precede any description of controlling factors.

- when mentioning the influence of convergence rate, it would be relevant to mention that trench retreat (a diagnostic of subduction models), part of convergence rate, is likely to play a role in the margin accretion or erosion.

Yes, trench retreat plays an important role, but referenced studies in the introduction relate only convergence rate to accretionary and erosion margins. We add this point in the discussion, where we expect a stronger effect on topography with less trench retreat (Lines 385-387). We also note that convergence rate is independent of global reference frame while values of observed trench retreat rates vary depending on global reference frame.

- line 103: the question “Why some margins accrete sediments while others do not?” is also related to the issue of sediment flux from continents to the margin -depending on topography, climate, river system, oceanic currents – hence the question could be rephrased as “for the same sediment flux at the margin, why do some margin accrete sediments and other not?”

These are overview questions about the effect of sediments at subduction zones, which we hope will open a new direction for research. We prefer to leave the text as it is, because in the next paragraph, we described the goal of our focused investigation - ‘understand what causes convergent margins to either accrete material delivered by the subducting plate or, alternatively, to subduct the trench sediment pile and even erode the basement of the overriding plate’.

- line 104: the question “How do sediment fluxes influence subduction dynamics and back?”, especially the “and back?” should be better phrased.

Line 87: Rephrased to ‘What is the feedback between sediment fluxes and subduction dynamics?’

- line 104-105: the last question “How should the subduction interface be treated in numerical models, while relaxing the assumption of an interface with constant thickness?” is not really discussed later on in the paper. Please remove.

While we do address this question in the discussion (Section 4.4), we agree that this question is more technical and does not fit with the other scientific questions. We removed the question.

- line 114: for clarity, the authors should mention some of the “dependent and independent variables” compared between present-day SZs and model outputs.

These are described in detail in the Methods section. Added reference to the section.

- line 120: it is confusing that the authors specify that thermal diffusion is neglected when solving the equations… whereas there is no equation of energy conservation (the models are isothermal).

- section 2:specify that the subduction models are mechanical (or compositional) with not temperature variations.

For both comments above, we added a sentence at the beginning of the Methods section ‘Numerical models presented below are purely mechanical.’ We hope this will make it obvious that we neglect the energy balance.

- section 2.1: what are the properties of the upper plate? The upper plate models an oceanic plate, so why is its layering and rheological structure so different from the one of the subducting plate?

The lithosphere and core of upper plate are the same as the subducting plate. In the absence of temperature profile, the strength profile is dictated by the thickness of these layers. We also assume that the subducting plate has the incoming sediments, which are weaker, while the upper plate has no sediments.

Fig.1: it makes no sense to refer to the upper plate mantle material as “slab”, please use a different wording.

Corrected to ‘Lithosphere’ in Fig 1.

- for clarity and conciseness, the introduction of section 2.2 should be directly placed into the subsections “input parameters” and “diagnostic parameters” except lines 180-183.

We revised and clarified the section. We hope the current structure is now improved.

- line 152: please rephrase “mimicking mid-ocean ridge margins at the tails of the slab” since formation of an oceanic plate take plate at the ridge (whereas “tail” suggest the end), since “slab” only refers to the sunken plate portion and since “margin” often refers to an ocean-continent transition rather than to an oceanic ridge. Suggestion: “mimicking mid-ocean ridges at the trailing edge of the plate away from the trench”

Adapted as suggested.

- line 189: specify that the variable thickness mimicks variable ages/thermal structure of the upper plate.

Line 165: Added sentence: The variation in upper plate thickness mimics variable plate ages.

- line 189-191: the sentence “Since the sediment construction is a parameterization of the strength of the oceanic lithosphere, a no-sediment case is still considered when the sediment viscosity is high (higher proportion of crust at the interface).” is incomprehensible, please reformulate.

Line 167-168. Rephrased to: A no-sediment case is represented by high-sediment viscosity (i.e., higher proportion of crust at the interface).

- line 191-193: the two sentence describing the effect of sediments on mechanical coupling do not belong to the method section but rather to the results or discussion section.

We removed the sentence.

- Line 200-203: the sentence “These variables are compatible with parameters derived from statistical analyses of present-day subduction zones (i.e., Clift and Vannucchi (2004); Lallemand et al. (2005);Wu et al. (2008); De Franco et al. (2008); Heuret et al. (2012)).” is not relevant in the methods section but belongs to the discussion.

We think assumptions and motivations for parameter choice should be in the Methods section. Our choice of diagnostics parameters was motivated by these statistical analyses.

-line 207: you mean “Marker 2” instead of “Marker 3”

Corrected.

- line 216: I do not understand the assertion “However, considering that slabs bend elastically, radius of curvature is more appropriate to describe slab deformation with depth, while slab dip represents only the tangent to curvature close to the surface.”, please explain in more details the link with elastic deformation. The real issue seems to me the depth interval up to which the radius of curvature is calculated or over which a mean dip is calculated.

Deleted ‘considering that slabs bend elastically’

- lines 232-235: it is puzzling that the authors chose to characterize similar bent geometries by two different geometrical diagnostics: a radius of curvature in the case of the slab and a dip in the case if the wedge. I am not sure I understand since slab dip seems a diagnostic also available from SZ data.

We consider that the radius of curvature describes the geometry of slab, while the wedge angle describes the geometry of accretionary wedge. The wedge angle is not the same as slab dip (that the reviewer refers to). The curvature is obtained on a larger scale, while the wedge is limited to 10s of km extent.

- line 482 belong to the methods section 2.1 to justify the range of sediment thickness tested.

We moved it to the methods section in line 166.

- Figure 3e: please specify whether a positive trench rate means advance or retreat.

Added explanation in Figure 3 caption.

Citation: https://doi.org/10.5194/se-2021-137-RC1

RC2: 'Comment on se-2021-137', Anonymous Referee #2, 17 Jan 2022

This manuscript describes a series of 2D geodynamic models that focus on the role of sediment thickness and viscosity on subduction dynamics and accretionary/erosive style of the margin. The study is well-organized and a systematic modeling approach is adopted. The manuscript is well-written and outcomes are relevant to better understand how interface properties affect subduction dynamics. However, I think a more detailed explanation on the physical processes controlling the models behavior is missing.

Major comments:

1. Abstract should be more detailed on the results of the study and include an explanation on the processes

We revised the entire abstract. We hope it is clearer now.

2. Since viscosity of sediments is critical, the method section should include more details on the choice of the orders of magnitude of sediment viscosity. Where does the choice of using 15 km-thick weak crust

come from? More importantly, since the thickness of the weak crust remains the same in all the simulations and only sediment thickness is varied, have you checked that the thickness of the magmatic weak crust (dark green layer) has no influence on the results?

We explain in the response to intermediate comments from reviewer #1 that the thickness of the weak crust also varies in this set of experiments in order to keep the combined thickness (crust and sediments) of 15 km constant. By doing this, we keep the initial buoyancy of the plate constant and isolate the rheological effect of sediments. We leave the effect of buoyancy of sediments (changing thickness of crust and sediments, density of sediments) for another investigation.

Lines 136-143: We also modified relevant text in the Methods section to clarify our parameterization of sediment viscosity.

3. Lines 279-280: I am not sure that this is a good way to classify a margin as erosive. A probably better way would be to measure the area of triangle ABC and check if this increase or decrease through time. This could be also useful to understand the behavior of unstable accretionary wedge (see following point)

In response to Reviewer #1 comment's above, we note that the area of the triangle ABC is not a unique measure for either accretionary or erosive margins because we also vary the thickness of the upper plate, which affects the length of the subduction interface. For example, the area of an accretionary margin with thin upper plate can be similar to the area calculated for an erosion margin with thick upper plate. We consider the angle (and its evolution) is a better quantitative measure for differences between end-member models.

4. The behavior of the model with highly unstable accretionary wedge (SubSed04_100) is really interesting and probably deserves more attention. For example, figure S7 shows the temporal evolution of the convergence rate. It would be interesting to understand if the decrease in convergence rate corresponds to an increase of the sediment subducted and/or variations of subduction geometry (cf. Brizzi et al. 2021)

We explained above in response to Reviewer #1 comment's, that, indeed, the unstable accretionary wedge model could provide an explanation for slow convergence rates in accretionary margins. However, our current model overestimates the density and initial thickness of sediments. This makes it more likely for sediments to be subducted in the model. Therefore, we prefer not to highlight the unstable model outcome and make unrealistic statements.

5. I think the manuscript should include a figure more informative on the role of sediment thickness and viscosity (and upper plate thickness) on convergence velocity, radius of curvature, etc. At present, figure 4 shows correlations among diagnostic parameters color coded by margin style (erosive or accretionary), but I am not fully convinced of the classification criteria used to distinguish them (see point 3). As the authors acknowledge, the thickness of the sediments is less relevant compared to their viscosity for the margin style (table 2 shows that an erosive margin can form also when the sediment thickness is 10 km, given that viscosity is high). Similarly, the upper plate thickness, which controls the downdip extent of the viscous coupling, seems to be important for e.g. plate velocity (see also Brizzi et al 2021). For example, the high-angle accretionary wedge case makes me think to the influence of the plate interface length, which influences the integrated shear stress. When the wedge is growing, the plate interface is smaller, resistance to subduction is lower and convergence rate is higher. Perhaps plotting the diagnostic parameters as a function of sediment and upper plate thickness, and sediment viscosity could help defining a hierarchy.

We hope we clarified the classification criteria, as requested also by reviewer #1. The analysis in Brizzi et al. 2021 follows the approach that the reviewer #2 is suggesting: plotting the diagnostic parameters as a function of sediment and upper plate thickness, and sediment viscosity (or individual input parameters).

We consider that plotting individual parameters is one approach, but that will create an incomplete picture. For example, in Brizzi et al. 2021 we found this statement:

"Low sediment density makes sediment subduction more difficult, so that slab pull is higher if sediments are relatively light (Figure 3b). This causes higher slab velocities for such lower sediment densities (Figure 3c)."

However, Fig 3c and 3d in Brizzi et al 2021 show a large variation in convergence velocities (5-10cm/yr) for low sediment densities (triangle markers). The statement is correct only if all other parameters are kept constant, while density is varied. But that is not the case in Brizzi et al. 2021 or in this study - we do not vary a single parameter.

We consider the model outcomes a consequence of multiple factors. We agree that a formalized analysis using dimensionless parameters would be ideal, but that is beyond the scope of this study. Color coding our analysis with margin type allows us to characterize the same outcome using different model inputs. Moreover, our goal here was to link the same diagnostics that are used in statistical analysis of subductions. Those analyses lack initial 'model' parameters.

Minor comments:

- Lines 6-7: sentence is unclear. What is the geometry of the global subduction system?

We removed the sentence and revised the abstract.

- Lines 14-15: This could be more specific. How does the viscous coupling relates to the thickness and viscosity of sediments?

We added short explanations in the abstract text and hope it is clearer now.

- Line 16: this sentence should be probably moved up, before writing about the results. Also, I would suggest to specify what these diagnostic parameters are.

Sentence moved up; however, details on the diagnostics parameters are left out to keep the abstract short.

- Line 36: what is the morphology of the subduction interface? Please clarify

Line 32: We use 'morphology' as a generic term to describe whether the interface includes frontal/basal accretion of sediments, sediment subduction or erosional features.

- Line 47-48: Funciello et al 2008 is based on analog experiments.

Line 43: Corrected to 'laboratory experiments'.

- Line 105: This doesn't seem to be addressed in the paper. I would remove.

The sentence was removed.

- Line 151: Why did you choose to simulate ocean-ocean subduction instead of ocean-continent? What are the properties of the upper plate and why are they different from the subducting plate?

We consider ocean-ocean subduction as a first set of experiments, where we wanted to isolate the effect of sediment rheology from buoyancy of plates and sediments. We expect buoyancy of both sediments and upper plate (oceanic or continental upper plate) to influence our results as seen in previous studies (i.e. Currie et al. 2007). This should be the purpose of a future investigation.

The upper plate is not completely different from the subducting plate. Thickness is a proxy for the age of the lithosphere. A subducting plate and upper plate with the same thickness, also have the same buoyancy. The only difference in this case is assuming that a hydrated/altered crust and a thin layer of sediments on the subducting slab lubricates the subduction interface, while the upper plate (with higher topography) lacks the weak crust.

- Line 155: This sentence is confusing. Please clarify

'The crustal thickness represents a parameterization of the strength weakening of the lithosphere with depth due to hydration and weak sediment cover.'

Line 141. Corrected to: 'The viscosity structure of the combined weak crust and sediments represents a parameterization of the strength weakening with depth due to hydration and weak sediment cover.'

- Line 163-164: I would add the tested sediment thicknesses and viscosities here. Also, what is the density of sediments and crust?

We explain in line 145 that the plates are 85 kg/m3 denser than the asthenosphere (reference \rho0). We do not vary the density structure in simulations.

- Line 169: What are the subducting plate parameters? Please specify

Line 154. Previous studies investigated the effect of many parameters, mostly related to thickness and strength of plates. We modified the sentence accordingly.

- Line 188: In my opinion, a sediment thickness of 5 km as the lowest end member is a bit too high. Natural subduction zones can have a (trench) sediment thickness < 1km. Is this choice related to the model resolution?

While global compilations show that sediment thickness goes from 0-12 km (e.g., Laske et al. (2013);Dutkiewicz et al. (2015)), we agree that we test some upper range values for the thickness, indeed due to the choice of model resolution. We expect some of the details to change if higher resolution is used, but the overall pattern of our model results to be reproduced and leave this for further investigation.

We clarified this aspect in lines 166-168.

- Line 189-191. The sentence is unclear. Please clarify

Line 165. We rephrased the sentence to: ‘The variation in upper plate thickness mimics variable plate ages. A no-sediment case is represented by high-sediment viscosity (i.e., higher proportion of crust at the interface).’

- Line 207: Marker 2 instead of marker 3

Corrected.

- line 249: are you sure that slab buoyancy is constant? Brizzi et al. 2021 shows that the amount of sediments subducted influences slab buoyancy.

It is correct that subsequent slab buoyancy is affected by sediment subduction or accumulation. We corrected to: ‘By not changing the density of the sediments or the slab geometry, the magnitude of initial slab-pull force is the same among simulations.’ in line 151.

- Line 291: lubricates and promotes

Corrected.

- Line 297-300: it is really difficult to see what the authors state about interface dynamics from the velocity field in figure 2. The colorbar for velocity is also quite impossible to read. I suggest to use a different colorbar and include a zoomed version of the interface in figure 2

We updated Fig 2 to include strain rate maps and velocity arrows of an enlarged area of the subduction interface. We also adapted the text.

- Line 321: Parentheses for the reference are missing

Corrected.

- Lines 394-395: the finding that a bigger accretionary wedge promotes high radius of curvature is consistent with results from Brizzi et al 2021

We referenced our results with those from Brizzi et al 2021 throughout our manuscript. Brizzi et al 2021 was published after we submitted our manuscript.

Citation: https://doi.org/10.5194/se-2021-137-RC2