Responses to Review Comments

In the following, review comments are in ***blue italic*** font, while responses are in **black normal** font.

--

Reviewer #1 (Adam Daniel McArthur)

(1) I enjoyed reading this manuscript, which is a rare example of trying to model submarine sedimentation across an active fault scarp. The methodology of combining a sand-box model with flume tank experiments is novel and for this alone is worth publication, especially the supplementary videos, which would be great to include in the main publication if the journal has this capacity. It's mostly clearly written (with some noted exceptions in the Results and Discussion sections), with a logical flow and supported by good quality figures and explanation of the data and results. However, there are limitations in applying the modelling fine grained, diffuse sedimentation to the typically coarse-grained deposits associated with canyons and hangingwall fans, which are typically dominated by mass-wasting. Particularly, if this work is to be used to "predict the morphological evolution and sedimentary processes of submarine canyon-fan systems in active fault settings", which unfortunately, as written it does not. I outline my main concerns below; with some careful re-framing this could be published after major revisions.

Reply: We are grateful and thank the reviewer for thorough assessment of our manuscript and for providing us constructive comments and suggestions. In the revised version, all the comments and suggestions have been considered and changes have been made to improve the presentation. We now add point-by-point reply (in black normal font) to the comments and suggestions of the reviewer (blue italic font) and make clear where and what changes have been made in the revised version of the manuscript.

(2) I have annotated a PDF with a number of comments and minor corrections that will improve the final paper. The main points to remedy, which will improve the readers confidence in the work are: The study needs to be better framed, in terms of 1) modern work on source to sink systems (e.g., Sømme et al., 2009; Nyberg et al., 2018), 2) recent work on canyon systems (e.g., Bernhardt and Schwanghart, 2021; Soutter et al., 2021) and 3) the wealthof work on active margin systems (e.g., Bührig et al., 2022; McArthur et al., 2022). As it stands many of the motivations for doing this work seem dated and ignorant of the wealth of work on active margins over the past twenty or more years.

Reply: We appreciate receiving the reviewer's annotated PDF, which is very helpful. We have followed the recommendations of the reviewer and reframed the entire article, making significant revisions to the Introduction and Discussion sections. Please refer to our revised manuscript.

(3) This leads to some confusion on the actual features being modelled, which are really hangingwall fans (sensu Leeder and Gawthorpe, 1987), rather than classical submarine fans. This is an important distinction and the study needs reframing in this light. This has the added complication that most hangingwall fans are constructed by a mixing of turbidity currents and mass-wasting (e.g., McArthur et al., 2013; Barrett et al., 2021). This at least needs to be considered and discussed, otherwise the whole premise of the study seems flawed.

Reply: We agree with the reviewer's suggestion and will change all instances of "submarine fans" to "submarine hangingwall fans". In discussion 4.1, we discussed the formation of submarine canyons and hangingwall fans, which involves a combination of turbidity currents and mass-wasting processes, as described by McArthur et al. (2013) and Barrett et al. (2021).

(4) Furthermore, the basic assumption that flows through canyons and that form said hangingwall fans are fine grained, here modelled as saturated brine to represent mud-rich turbidity currents, using a diffusion model really limits the application of this to understanding how natural systems evolve. We know that most active margin canyons are conduits for- and hangingwall fans build stratigraphy that is a mix of high concentration turbidity currents, debris flows and mass transport deposits. Furthermore, recent work has shown how varying grain size and sorting of flows strongly influences their ability to erode, bypass or deposit (Crisóstomo-Figueroa et al. 2021; Amy and Dorrell 2022). Therefore, the limitation of modelling fine grained particles with diffusion vs. the complicated nature of reality should at least be stated up front and discussed if the authors truly believe this work will help us "understand the initiation and evolution of fault-controlled submarine canyon-fan systems driven by downslope gravity flows".

Reply: Thank you for the reviewer's suggestions. We have made modifications to the wording of "mud-rich turbidity currents". We acknowledge that the current one-dimensional geometric relationship (Eq. 1 and Eq. 2) and morphodynamic model (Eq.3 and Eq. 4) are simplified models that cannot accurately describe the details of grain size and sorting.

However, our main goal is to construct a simple mathematical model to describe the long-term geomorphic evolution of the continental slope and submarine canyon-hangingwall fans, and we hope to explore the existence of self-similarity in the system (which indeed exists, as shown in Fig. 10 in the revised manuscript). In our model, we have decoupled complex phenomena into two primary mechanisms: (1) breaching processes driven by gravity; (2) submarine canyons and hangingwall fans formed by saline underflow. The comparison between experiments and models is presented in Fig. 8, Fig. 9, and Fig. 10 in the revised manuscript.

At the laboratory scale, the breaching process itself includes debris flows and mass wasting processes. The morphological response is that the slope will maintain the angle of repose, which can be described by a simple geometric relationship and the comparison is acceptable (Fig. 8). This angle of repose slope is consistent with the experiments in Lai et al. (2016), i.e., the final steady state of laboratory-scale breaching processes is a featureless slope, and the angle will be maintained at the material's angle of repose.

In addition, we describe the long-term geomorphic evolution of the submarine canyon-hangingwall fan subjected to saline underflow using the diffusion equation. This idea was inspired by the theoretical foundation of our previous studies on hyperpycnal deltas (Lai and Capart, 2007; 2009; Lai et al., 2017; 2019; Lai and Wu, 2021). Although not perfect, the comparison between experiments and models has yielded acceptable results (Fig. 9 in the revised manuscript). Although a 1D model cannot capture the details of grain size or sediment sorting (e.g., Crisóstomo-Figueroa et al., 2021;

Amy and Dorrell, 2022), the diffusion equation provides a transparent mathematical relationship that can serve as a scientific basis for validating complex phenomena. There are also many successful cases of using the diffusion equation to describe long-term geomorphic evolution (Paola et al., 2009). For instance, Spinewine et al. (2011) applied a nonlinear 1D diffusion model to the Amazon channel of the Amazon Submarine Fan. Likewise, Lai and Wu (2021) applied a 1D diffusion model to describe the foreset evolution of the field-scale Po River delta, which can depict over 200 years of long-term geomorphic evolution. We appreciate the reviewer's comment, which enabled us to enhance the comparison between our model and reality, highlighting both the strengths and limitations of the approach we are proposing. This content has been incorporated into the new section titled "4.1 Why do geomorphic experiments work?" within the discussion.

(5) Much is made of varying the effects of varying inflow discharge. However, upon seeing the supplementary videos, it appears that most of the sedimentation through your canyons to build the fans is actually via footwall erosion and reworking – this is actually closer to reality that your described methods and results. Much more description of this process, perhaps accompanied by greater investigation of this erosion would help address some of the concerns of how appropriate it is to use these experiments to understand natural systems. Further information on the composition of the footwall is required in the methods and this process needs to be incorporated into your Morphodynamic model.

Reply: Please refer to our previous response. We discussed the relevant mechanism in Discussion 4.1.

(6) However, the flows shown in the supplementary videos do not appear to resemble the "long-lived hyperpycnal flows or mud-rich turbidity currents" that are claimed to be modelled. Particularly, the flows moving sediment and building your hangingwall fans appear as simple sediment gravity flows, i.e., grain flows/avalanches, which have very different flow properties to turbidity currents. This fundamentally questions the applicability of this study to natural systems dominated by turbidity currents.

Reply: Indeed, saline underflow and turbidity current are fundamentally different, but these two types of density currents may exhibit similar changes in morphology in response to bed load transport (e.g., Sequeiros et al., 2010). At the reduced scale of our experiments, unfortunately, gravity currents cannot be sustained by turbulent sediment suspension, hence the need for salinity to provide a sustained density contrast. We have decoupled the complex phenomenon into the breaching process and saline underflow transport process. These two mechanisms show significant differences in laboratory experiments. Please refer to our previous response. The field conditions are, of course, much more complex, and the results of various processes need to be thoughtfully interpreted when comparing to our research findings.

(7) The Discussion needs to be just that. I.e., you should compare your results to those of other studies to discuss what works and what doesn't. At the moment it's just a rather long winded attempt to further explain the results of your models. Breaking up the discussion into distinct sub-sections to address specific points will also help build a narrative.

Reply: We have rewritten the Discussion section, dividing it into sections 4.1, 4.2, and 4.3, to better address and organize the issues raised during the reviewing process.

(8) As it stands, this main conclusion and other broad statements about how this modelling can help us understand the geological evolution of submarine canyons and "submarine" (hangingwall) fans is not supported by your data or results. That is before even considering the multitude of natural complications that may arise e.g. other processes (i.e., canyon tides and internal waves, contourites etc.), variability of natural systems (e.g., seasonal, Milankovitch cycles etc.), variation in catchment area (size and composition), structural complexities (e.g., faults are rarely just one fault, but normally a fault zone of many structures active at different times with different effects; effects of earthquakes on landslides and sediment remobilisation, etc.) and many more besides. I appreciate these natural variabilities cannot be addressed by simple modelling, but they should at least be acknowledged if you want to suggest these results can be applicable to natural systems. One of the key things to consider in an attempt to reframe the impact of this work, is that your models are free from many of those complexities, e.g. climate change, variation in sediment supply. So with all things being equal it appears tectonics is the overriding control; this would be a very useful main conclusion and agree with work on natural systems (e.g., Soutter et al., 2021). References above are all listed in the annotated PDF at the appropriate points.

Reply: Thank you for the suggestions. We have revised the Conclusions. Our research findings align with the conclusions of Soutter et al. (2021), highlighting tectonic as the overriding control. We have also addressed the limitations of our research findings and reminded readers to consider the various complex conditions in interpreting field phenomena.

In the revised manuscript, we have added Fig. 12 where we compare the experimental data with the field data (over 10,000 data points spanning from underwater to above water). We have also included the modern data from Bührig et al. (2022). Finally, we propose a scaling relationship from laboratory-scale to field-scale.

In addition, we have also made modifications to Fig. 13 in the revised manuscript. We have individually labeled all 26 field cases (covering passive margin, active margin, and mixed margin) and added estimated data from Bührig et al. (2022) to demonstrate how our proposed empirical formula can assist in estimating hard-to-obtain fan volume information.

References cited in our reply:

Amy, L. and Dorrell, R.: Equilibrium sediment transport by dilute turbidity currents: Comparison of competence-based and capacity-based models, Sedimentology, 69, 624-650, https://doi.org/10.1111/sed.12921, 2022.

Barrett, B. J., Hodgson, D. M., Jackson, C. A. L., Lloyd, C., Casagrande, J., and Collier, R. E. L.: Quantitative analysis of a footwall-scarp degradation complex and syn-rift stratigraphic architecture, Exmouth Plateau, NW Shelf, offshore Australia, Basin Res., 33, 1135-1169, https://doi.org/10.1111/bre.12508, 2021.

Bührig, L. H., Colombera, L., Patacci, M., Mountney, N. P., and McCaffrey, W. D.: Tectonic influence on the geomorphology of submarine canyons: implications for deep-water sedimentary systems, Source or Sink? Erosional and Depositional Signatures of Tectonic Activity in Deep-Sea Sedimentary Systems, https://doi.org/10.3389/feart.2022.836823, 2022a.

Bührig, L. H., Colombera, L., Patacci, M., Mountney, N. P., and McCaffrey, W. D.: A global analysis of controls on submarine-canyon geomorphology, Earth-Sci. Rev., 104150, https://doi.org/10.1016/j.earscirev.2022.104150, 2022b.

Crisóstomo-Figueroa, A., McArthur, A. D., Dorrell, R. M., Amy, L., and McCaffrey, W. D.: A new modelling approach to sediment bypass prediction applied to the East Coast Basin, New Zealand, Geol. Soc. Am. Bull., 133, 1734-1748, https://doi.org/10.1130/B35687.1, 2021.

Lai, S. Y. J. and Capart, H.: Two-diffusion description of hyperpycnal deltas, J. Geophys. Res.-Earth, 112, https://doi.org/10.1029/2006JF000617, 2007.

Lai, S. Y. J. and Capart, H.: Reservoir infill by hyperpycnal deltas over bedrock, Geophys. Res. Lett., 36, https://doi.org/10.1029/2008GL037139, 2009.

Lai, S. Y. J., Gerber, T. P., and Amblas, D.: An experimental approach to submarine canyon evolution, Geophys. Res. Lett., 43, 2741-2747, https://doi.org/10.1002/2015GL067376, 2016.

Lai, S. Y., Hung, S. S., Foreman, B. Z., Limaye, A. B., Grimaud, J. L., and Paola, C.: Stream power controls the braiding intensity of submarine channels similarly to rivers, Geophys. Res. Lett., 44, 5062-5070, https://doi.org/10.1002/2017GL072964, 2017.

Lai, S. Y. J., Chiu, Y. J., and Wu, F. C.: Self-similar morphodynamics of Gilbert and hyperpycnal deltas over segmented two-slope bedrock channels, Water Resour. Res., https://doi.org/10.1029/2018WR023824, 2019.

Lai, S. Y. J. and Wu, F. C.: Two-stage transition from Gilbert to hyperpycnal delta in reservoir, Geophys. Res. Lett., 48, https://doi.org/10.1029/2021GL093661, 2021.

McArthur, A. D., Hartley, A. J., and Jolley, D. W.: Stratigraphic development of an Upper Jurassic deep marine syn-rift succession, Inner Moray Firth Basin, Scotland, Basin Res., 25, 285-309, https://doi.org/10.1111/j.1365-2117.2012.00557.x, 2013.

Paola, C., Straub, K., Mohrig, D., and Reinhardt, L.: The "unreasonable effectiveness" of stratigraphic and geomorphic experiments, Earth-Sci. Rev., 97, 1-43, https://doi.org/10.1016/j.earscirev.2009.05.003, 2009.

Sequeiros, O. E., Spinewine, B., Beaubouef, R. T., Sun, T., Garcia, M. H., and Parker, G.: Characteristics of velocity and excess density profiles of saline underflows and turbidity currents flowing over a mobile bed, J. Hydraul. Eng., 136, 412-433, https://doi.org/10.1061/(ASCE)HY.1943-7900.0000200, 2010.

Soutter, E. L., Kane, I. A., Hodgson, D. M., and Flint, S.: The concavity of submarine canyon longitudinal profiles, J. Geophys. Res.-Earth, 126, e2021JF006185, https://doi.org/10.1029/2021JF006185, 2021.

Spinewine, B., Sun, T., Babonneau, N., and Parker, G.: Self-similar long profiles of aggrading submarine leveed channels: Analytical solution and its application to the Amazon channel, J. Geophys. Res.-Earth, 116, F03004., https://doi.org/10.1029/2010JF001937, 2011.

Reviewer #2 (Morelia Urlaub)

This manuscript seeks to understand how submarine canyon systems and fans develop in a fault-controlled setting. The authors use laboratory analogue models of a continental slope, the bottom of which is modified by a simulated normal fault. The parameters tested here are fault slip rate and sediment inflow discharge. The authors find that the fault slip rate controls the number and spacing of canyon-fan systems (higher slip rate = more systems at closer spacing). Changes in sediment input do not result in different morphology. Length, area and volume of canyon and fans are proportional to sediment input. While this is an interesting study and the manuscript is well-written, I have several comments that need clarification. I must say, though, that I am not an expert in geomorphological modelling.

Reply: We thank the reviewer's positive feedback and constructive comments. Our responses to each question are listed below.

1. For the field analogue the Queen Charlotte Fault is mentioned. This is a strike slip fault, which is also correctly stated in the manuscript. Yet, the model set-up includes a normal fault at the bottom of the slope. I do not see the connection. In general, I would like to see more field examples for fault-controlled submarine canyon systems, ideally with distinct morphologies and, if known, different fault slip rates. This would strengthen the relevance of this study, and could serve as validation for the laboratory work.

Reply: We acknowledge that Queen Charlotte Fault, as a strike fault, may not be the most suitable choice to illustrate the manuscript content. We have removed all references to the Queen Charlotte Fault and added several other field examples for fault-controlled submarine canyon systems in the revised Introduction. We also discuss the influence of fault slip rate and inflow discharge on canyon morphology in Discussion section 4.2 to strengthen the connection between experiments and field observations.

2. How is the scaling problem addressed? I assume that grain size, overall size of model, slope angle all need to be balanced to reproduce realistic settings. I was surprised by the slope angle of 38°. Where in nature do you find such slope angles in submarine settings? Also in line with this: How are boundary effects accounted for?

Reply: We address the scaling issue in revised Discussion section 4.1. Below, we provide a brief explanation.

In the study, we decouple complex phenomena into two main mechanisms: (1) breaching processes driven by gravity, and (2) submarine canyons and hangingwall fans formed by saline underflow. The comparison between experiments and models shows that the long profiles themselves exhibit a high degree of self-similarity (Fig. 10), indicating that laboratory-scale canyon-hangingwall fan long profiles are scale versions of each other. In the morphometric analysis, we discovered Hack's scaling relationship spanning 22 orders of magnitude (Fig. 12), which connects from underwater to above water, across experimental and field scales. Additionally, our proposed empirical formula (Fig. 13), when compared with 26 representative canyon systems worldwide and using the latest modern data to predict fan volumes, yields estimations within a reasonable range, indicating the absence of scale issues.

The 38-degree angle of repose refers to the natural angle at which materials in a laboratory reach a stable slope. It indicates that the phenomenon is simply controlled by gravity and naturally collapses. It is not suitable to directly analogize it to the 38-degree angle in the field. On the other hand, in areas affected by saline underflows, the depicted slopes are much smaller than the angle of repose. The difference between the two is evident.

Our current experimental approach is to set up an active fault at the position x = 400 mm. The upstream and downstream boundaries will not affect the evolution of submarine canyons and hanging wall fans in the middle. Although there may be sand leakage caused by the uplift and subsidence on the left and right sides, we have confirmed that the displacement on both sides will not have any impact on our observation area.

3. To be able to put results into a general context it would be helpful to also show a reference model without fault movement. What sort of morphology is produced and how does it compare to those with fault slip?

Reply: In our experiment, all canyon morphology starts with the fault slip generated by increasing relief. However, a closer look can be had at the continental shelf in our experiment, i.e., the footwall region (Fig. 4). This area is where there is no fault movement. When saline underflows flow through this area, there will be no changes in morphology. The underflows will bypass the continental shelf until they encounter the shelf-slope break. This is one of the key elements in our experimental approach. Our new results fully agree with the conclusion of Lai et al. (2016) that in order to generate developing submarine canyons in the laboratory, both increasing relief and density underflows are necessary.

4. In section 3.3 it is stated "A similar trend was also established for submarine fans (Fig. 10)". How was this trend determined for submarine fans? On field data? From where - only Haida Gwaii? This comment relates to comment 1.

Reply: We rephrased our statement.

5. I am not familiar with Hack's law. Maybe explain briefly so that non-specialists understand.

Reply: We have added Fig. 12 and explained the relationship and application of Hack's Law.

References cited in our reply:

Lai, S. Y. J., Gerber, T. P., and Amblas, D.: An experimental approach to submarine canyon evolution, Geophys. Res. Lett., 43, 2741-2747, https://doi.org/10.1002/2015GL067376, 2016.

Reviewer #3 (Laura Henrika Bührig)

Dear Authors,

I have read your manuscript "Evolution of submarine canyon-fan systems in fault-controlled margins: Insights from physical experiments" with great interest, given the limited number of experimental studies that have been published on the evolution of deep-water systems, the detailed documentation of your experiments, and the focus on the interplay of gravity flow processes and fault activity. From this, your research contributes valuable new insights which complementthose of other experimental, numerical, field, and metastudies of deep-water systems. The manuscript is overall well written, and the scientific content is clearly delivered in both text and figures. However, there are several major issues I find with the manuscript, in particular: (i) a shortcoming regarding the critical discussion of the experimental setup, the results and their interpretation, and (ii) the very limited consideration of recent modelling and metastudies of deep-water sedimentary systems both in providing a framework for your research and for the discussion of your findings. Please find in the following a summary of my main points of critique.

Reply: We are grateful and thank the reviewer for thorough assessment of our manuscript and for providing us constructive comments and suggestions that specially enhanced the contextualization of our study from a field research perspective. In the revised version, all the comments and suggestions have been considered and changes have been made to improve the manuscript. We added a point-by-point reply in the following.

Major points:

(1) My first major concern with the manuscript in its present form is that it is missing both in the introduction and in the discussion sections the acknowledgement and integration of findings from previous studies that have contributed new insights into the evolution of deep-water sedimentary systems in a source-to-sink context and are relevant to your research.

For example, there are several recent studies which have quantitatively investigated aspects of submarine-canyon geomorphology as a function of environmental and physiographic variables on a global scale based on large datasets (e.g. Nyberg et al., 2018; Bernhardt & Schwanghart, 2021; Soutter et al., 2021; Bührig et al., 2022a&b) in addition to the study by Sømme et al. (2009) of which some of the findings have been utilised by Lai et al. All of the above studies have investigated the tectonic setting as a controlling factor on canyon evolution; hence, the statement in line 29 that "most of our knowledge about the evolution of submarine canyons come from case studies along passive margins" is not true.

Reply: We have extensively revised the Introduction and Discussion sections.

(2) In addition to the very limited view on quantitative metastudies, the introduction solely focuses on insights gained from experimental studies and omits findings from numerical/stratigraphic forward modelling studies that have explored the evolution of deep-water systems (e.g. Wan et al., 2021; 2022).

Reply: We have added modeling studies in the Introduction to enhance the completeness of literature reviews.

(3) The underrepresentation of findings from recently published studies relevant to the conducted research in the manuscript in the introduction is carried into the discussion section. A discussion of the findings from your study in context of those by prior studies is desirable to assess the applicability of the findings to real-world systems, and vice versa.

Reply: We have extensively revised the Discussion section.

(4) Generally, I have missed in the discussion section a critical discussion of the results and their interpretations. It is stated in line 340 that the experimental approach is [likely] "universal for all margin types", but this ignores the complex and variable influence of different tectonic settings on the morphometric characteristics of subenvironments in deep-water systems (e.g. submarine canyons; Nyberg et al., 2018; Bernhardt & Schwanghart, 2021; Soutter et al., 2021; Bührig et al., 2022a&b), as well as the importance of the physiographic setting of a submarine canyon for the dispersal of sediment to deep-water environments (e.g. Soutter et al., 2020; 2021; Bernhardt & Schwanghart, 2021; Bührig et al., 2022a&b). Moreover, it should be highlighted that other environmental controls (e.g. contour currents, Rodrigues et al., 2022) and processes (e.g. canyon flushing, Mountjoy et al., 2018) –just two examples amongst many – which have been inferred to exert control on the morphologies of deep-water systems might overprint such scaling relationships. As a consequence, some continental margins might be more suitable for the attempt at reconstructing deep-water system morphologies from scaling relationships compared to others. These are caveats to the approach which need to be mentioned. That being said, it is equally important to highlight the strength of the study setup, which is that it allows to investigate the influence of isolated controls and processes on the geomorphologic evolution of deep-water systems, which is difficult, if not impossible, in natural systems.

Reply: In the Discussion section, we have revised our statement, reminding readers to consider more local complex environmental conditions when using the findings of this study. In addition, we have included the modern data from Bührig et al. (2022) in Fig. 12 and Fig. 13, aiming to assist readers in connecting experiments and field cases, especially in deep-water sedimentary systems.

(5) My second major concern with the manuscript is that the authors state in the last paragraph of the conclusions section "we claim and demonstrate that at least we are able to predict the first-order general morphological and sedimentological patterns at basin scale".

In my opinion it is not possible to make these deductions based on the results presented in the study and that this is also not shown by the results. Due to the inherent limitations given under experimental conditions the experiment setup cannot reproduce natural conditions. This needs to be stated more clearly and also needs to be addressed in the discussion and conclusion sections. Moreover, the Haida Gwaii margin does not represent a classical transform margin but also has a convergent component (see Harris et al., 2014), which has to be considered regarding the applicability of the findings to other active margin settings.

Reply: We have rephrased our statement in the revised Conclusions and removed references to Haida Gwaii. We have instead used the modern data from Bührig et al. (2022a) and Bührig et al. (2022b) to help readers establish links between experiments and the field.

(6) It is stated in lines 30 & 31 that the occurrence of downslope gravity currents is more frequent along active margins compared to passive ones, but this is only supported by two references which focus on Taiwan as a study site and hence do not support this claim. Moreover, as emphasized in the Milliman & Kao (2005) paper the triggering of these flows along the margins of Taiwan do not necessarily have to be related to seismic activity but to climate phenomena (e.g. typhoons).
Reply: We have removed these sentences since they were supplementary to the manuscript and lacked the necessary precision.

(7) It is stated in line 33 that "Haida Gwaii [...] provides good field examples that illustrate the evolution of canyon-fan systems in an active margin". It is not clear from the text why this margin was specifically chosen and why it constitutes a "good" example. It is also unclear to which extent the examples of submarine canyons hosted along active margins cited in lines 37-38 are "similar" to the Haida Gwaii canyons. I suggest expanding on the topic of different active-margin settings in the introduction section a little bit more to provide the framework for the later discussion.
Reply: We have removed all references and comparisons to Haida Gwaii, and extensively rewritten the Introduction and Discussion.

Minor points:
(1) I found it confusing that in section 2.2 and Figure 2a "Morphometric definitions" canyon width (Wc) and fan width (Wf) are introduced as study parameters but are later not referred to.
Reply: In the analysis of morphometric analysis, we did obtain data on canyon width and fan width. However, we have not yet discovered any interesting trends that can be compared to published data or help us predict hard-to-obtain volume information. Future research can continue to analyze these width data and perhaps establish more valuable relationships. This clarification has been included in the revised Conclusion.

(2) Section 3.2 contains text that is interpretative (lines 204-206 & 220-224) and should be moved to the discussion section.
Reply: We have deleted the text.

(3) The first key finding of the study listed under Conclusion No. 1 is that the results support the existence of Hack's scaling relationships "in both laboratory-scale and field scale submarine canyon-fan systems". I suggest rephrasing since the results of the study only support the former.
Reply: We added Fig. 12 in the revised manuscript. This figure illustrates that Hack's scaling relationship is applicable to laboratory-scale and field-scale, even with the new data from Bührig et al. (2022a) and Bührig et al. (2022b).

(4) The way conclusion No. 7 is presently phrased inadvertently reads as if the prediction of fan volumes based on canyon length is a finding that has newly emerged from the outcome of this study.

I suggest rephrasing the sentence more clearly to highlight the new formula developed based on the results of the study and to explicitly credit Sømme et al. (2009) who identified this scaling relationship prior to the present study.

Reply: We have modified the figure legend of Fig. 13 and added experimental data on washovers (Lazarus, 2016) as well as modern data from active margin and passive margin (Bührig et al., 2022). For deep-water sedimentary systems, we propose an empirical formula for estimating fan volume based on canyon length. This formula is in good agreement with the comparison results of 26 representative source-to-sink (S2S) systems worldwide, including passive margins, active margins, and mixed margins. The estimated fan volumes using the modern data from Bührig et al. (2022) fall within a reasonable range compared to the globally representative S2S fan volumes.

(5) Correction of the spelling of "Somme et al." in Figure 12b to "Sømme et al." and adding of the reference to the reference list.

Reply: We have corrected the spelling.

(6) Grammar-checking of the manuscript.

Reply: We have checked the grammar of the article.

References cited in our reply:

Bührig, L. H., Colombera, L., Patacci, M., Mountney, N. P., and McCaffrey, W. D.: Tectonic influence on the geomorphology of submarine canyons: implications for deep-water sedimentary systems, Source or Sink? Erosional and Depositional Signatures of Tectonic Activity in Deep-Sea Sedimentary Systems, https://doi.org/10.3389/feart.2022.836823, 2022a.

Bührig, L. H., Colombera, L., Patacci, M., Mountney, N. P., and McCaffrey, W. D.: A global analysis of controls on submarine-canyon geomorphology, Earth-Sci. Rev., 104150, https://doi.org/10.1016/j.earscirev.2022.104150, 2022b.

Lazarus, E. D.: Scaling laws for coastal overwash morphology, Geophys. Res. Lett., 43, 12,113-112,119, https://doi.org/10.1002/2016GL071213, 2016.