

We thank the reviewers for the valuable comments and suggestions. The replies to each comment are given below (marked in blue).

Response to Referee #1's comments:

Patsa & Mandal present a paper to study return flow in accretionary wedges, a topic that has been widely studied in the recent decades and is still relevant today. This process has been modelled with theoretical solutions, numerical and analog models to explain field-based observations on pressure temperature conditions on metamorphic rocks along accretionary prisms. The authors provide a more generalized theoretical solution to include a non-parallel component on the slab to reproduce slab advance/rollback. In addition, they conducted analog models to enhance these results. Finally, an overview with natural observations is made.

The integration of analytical solutions with analog modelling represents a novel and valuable approach, with the potential to be further strengthened by incorporating existing numerical results. However, the current manuscript does not fully emphasize this novelty: the introductory section underplays the contribution, and the analytical and analog results are presented somewhat independently, without sufficient cross-comparison.

To improve the manuscript, I recommend (i) revising the introduction to more clearly articulate the novelty and significance of the combined approach, (ii) providing greater coverage and integration of the analog modelling, and (iii) addressing several technical issues within the analytical solution. If these major concerns are resolved, the manuscript would meet the standards for publication in *Solid Earth* and I look forward to see the revised version.

- We thank Referee #1 for all the insightful comments, providing an excellent synthesis of the scientific goals of this study. In the revised version, we carefully address the issues raised by the reviewer and incorporate all the suggestions outlined in this comment. The Introduction section has been modified to emphasize the novelty of the present approach that combines analytical solutions and laboratory modelling in evaluating the return flow kinematics in viscous accretionary wedges as a function of various parameters. Furthermore, the revised version includes an elaborate description of new sets of scaled laboratory model results to strengthen the integration between the theory and analogue modelling, as suggested by the reviewer. It is shown how the velocity fields within the wedge can evolve over time with respect to the instantaneous velocity patterns predicted from the analytical solutions. We also carefully address some of the technical issues and confusions that unfortunately occurred in the previous version of the manuscript.

Major Comment 1

The use of a non-dimensional parameter to quantify the strength or weakness of return flow is both effective and intuitive. This approach allows the balance of incoming and outgoing material along the top boundary to be measured, corresponding to sediment influx and exhumed units in an accretionary wedge. In the simplest case—rigid walls with parallel subduction—the value must equal 1, as dictated by wedge geometry and mass conservation.

However, when a non-parallel component is introduced to the slab, the theoretical models also incorporate material flux along the slab boundary. This introduces bias in the calculated rates of burial and exhumation, since part of the return flow ratio (FR) is influenced by this artificial slab-sourced material. In the case of slab advance, FR appears anomalously high due to two factors:

(1) the top-boundary influx of sediments, which the authors correctly identify, and (2) the additional, unaddressed influx of material along the slab boundary.

For the rollback scenario, the formulation permits material to exit through the slab boundary, which is physically unrealistic. With small rollback velocities, wedge geometry would still force material to return to the surface; however, in the present formulation, this is instead channelled out through the slab, leading to $F_R = 0$. Furthermore, at high rollback velocities, the assumption of a downward-tapered wedge may break down, opening the system to the upper mantle. To remain consistent with the wedge geometry assumptions, I suggest restricting the analysis to small perturbations of the non-parallel component.

Finally, the comparison of slab advance, rollback, and normal subduction must be conducted under consistent assumptions regarding material influx. One possible way forward would be to fix the total incoming volume and instead vary return velocities, though I acknowledge that implementing this within the analytical framework may be non-trivial.

- We sincerely thank the reviewer for raising these relevant points. The return flux (F_R) value should indeed be 1 for rigid walls with slab-parallel subduction, which has been rightly noted by the reviewer. This small departure from $F_R = 1$ results from discretization in the numerical approach used to calculate F_R from the analytical solutions. This issue has been addressed in the revised version. However, we have now performed a high-resolution calculation, which yields a F_R value of 0.997 for a wedge with rigid walls and a slab-parallel subduction, very close to the theoretically predicted value of 1 (included in the revised text in line 276).

The return flux value is indeed high for slab advance, as correctly commented by the reviewer. We would like to clarify that the boundary condition at the wedge-slab interface considered in this theoretical derivation does not introduce any material influx across the slab boundary. Inward-pointing arrows shown in the figures actually indicate the wedge-ward movement directions of the subducting slab, which is used as a boundary condition in the analytical formulation. Such slab advance kinematics gives rise to a bulk horizontal shortening of the wedge, and this shortening eventually forces wedge materials to flow upward at higher rates, leading to a condition of $F_R > 1$. In this upgraded version we clarify this point in lines 250-251.

Similarly, during slab rollback, the slab is migrating away from the wedge, and there is no outfluxing of wedge materials across the slab boundary (outward-pointing arrows in the figures). This is also evident in the analogue models (Fig. 8c, 9c), which allow us to observe the modified wedge geometry with progressive slab rollback movement. The reviewer has correctly noted that, for small rollback velocities, the wedge would still produce return flow. It actually occurs in our model for the case of very slow rollback velocity ($\phi < \sim \theta_1/3$ for rigid wall). This is mentioned in lines 284 of the revised version. Also, it can be observed in the return flux (F_R) vs. subduction obliquity (ϕ) plot (Fig. 10 of the revised version), where F_R is positive for a range of low positive ϕ values (i.e., slab rollback). We agree with the reviewer that the assumption of wedge geometry would break down in a relatively short time duration if the slab undergoes rollback at high velocities. Based on the reviewer's suggestion, we present our model results for relatively lower rollback velocities in the revised version. We thank the reviewer for these valuable suggestions.

We would like to clarify that our theoretical model is subjected to a velocity boundary condition at the base, excluding any addition or removal of new materials in the wedge (i.e., the mass conservation condition is satisfied at any instant in the system). In all the analytical models, the subducting slab is moving at a velocity of 3 cm/yr. For slab advance and rollback settings, the

wedge itself is squeezing and widening, respectively, due to the horizontal velocity component of the subducting slab. Both the situations satisfy the mass conservation condition. This theoretical consideration is mentioned in the Basic Premises section of this article (line 147). We greatly appreciate these discussions by the reviewer.

Major Comment 2:

I strongly recommend undertaking a systematic comparison between the analytical solution and the analog modelling, as this would greatly strengthen the validation of the theoretical framework. At present, the two sets of results are described independently, which makes the paper look unbalanced. Establishing a one-to-one correspondence between the analytical predictions and the analog experiments for identical setups would provide a more rigorous test of the model and highlight the novelty of the combined approach.

That said, some modifications and additional discussion will be required to enable such a comparison. As noted in lines 415–416, the analog experiments allow material to exit through the bottom boundary, a feature not incorporated into the analytical solution. This discrepancy must be explicitly acknowledged and its implications discussed, since it directly affects the comparability of the results. One option is to consider modified boundary conditions in the analytical framework, or alternatively to constrain the analog results so that they are evaluated under conditions more consistent with the theoretical assumptions.

In addition, I recommend expanding the analog modelling section. At present, the manuscript dedicates considerably more space to the analytical solution, leaving the analog results underdeveloped. A more balanced treatment would not only give greater weight to the experiments but also allow for meaningful side-by-side comparisons. Importantly, you could draw inspiration from the approach of Moulas et al. (2021), who validated their analytical solution against numerical models with a similar setup. Extending your study to include a three-way comparison of analytical, analog, and numerical would significantly increase the robustness and originality of the manuscript.

To facilitate these improvements, some reorganization of the manuscript structure is advised. For example, moving Section 4 earlier in the text, immediately after the presentation of the analytical results would allow for more direct comparisons between the different methods. This restructuring would make the narrative more cohesive and highlight the integrative character of the study, which is currently one of its main strengths but not fully emphasized.

- We are grateful to Referee 1 for providing us with these valuable suggestions. We have thoroughly revised the analog modelling section, incorporating results from sets of fresh experiments performed with appropriate model scaling, which is described in this version as per the reviewer's suggestion (section 4). The revised version presents the analogue experimental results, giving a one-to-one comparison with the velocity patterns predicted from analytical solutions (Fig. 8, 9). Additionally, the model velocity fields are shown for two different time snaps of the experimental run, aiming to show whether the wedge follows the analytically predicted flow kinematics after significant amounts of slab movements.

We would like to clarify that the base of our experimental model is essentially rigid, as shown in the model setup (Fig. 7 of the revised version). For rollback, the base of the overriding plate is pulled by a very thin (~3 mm) buttress, which is underlain by the rigid model base. Our model setup thus did not allow any material to exit through the bottom boundary. In lines 415-416 of

previous version of the manuscript, we meant that the widening of the wedge facilitated the downward material flows within the wedge. We acknowledge that our statements in lines 415-416 were somewhat confusing, which is modified in this version (section 4.3.3). We thank the Referee #1 for raising this issue.

We greatly appreciate the reviewer's suggestion to expand and modify the analog modelling section. The revised version largely expands the descriptions of the experimental setup, scaling, and the model results (section 4). Additionally, we present a one-to-one comparison of the laboratory model results with the analytical results for identical setup (Fig. 8, 9). These suggestions have greatly helped us in improving the manuscript.

Considering the suggestions of the reviewer, we have reorganized the structure of the manuscript. In the revised version, the analog modelling section has been placed immediately after the analytical results. We thank the reviewer for these constructive suggestions.

Line to Line comments:

Line 20: Delete "the" or simply state "facilitate subduction."

- The sentence is modified in the revised version.

Line 21: References are missing for geophysical observations; see Abers (2005).

- We have added the reference of geophysical observations (line 23). We thank the referee for the suggestion.

Lines 25–28: I recommend mentioning the P–T–t path for consistency with the rest of the sentence, and introducing the concept of recycling here.

- We have modified the sentence (line 25-29).

Line 33: Retain only geochronological, since this sentence refers exclusively to exhumation rates and not geochemical constraints.

- Sentence modified.

Line 39: Verify the reference "?", for review."

- Referencing corrected.

Line 45: A reference is required. The corner flow model also accounts for both prograde and retrograde metamorphism; please mention this. Additionally, note that the model supports the possibility of sediments reaching mantle depths where partial melting may occur.

- The sentence is modified, and references are added (lines 46-51). We also mention about the possibility of partial melting of sediments, if it reaches mantle depths (lines 51-52).

Lines 48–49: Add one or two sentences on the thermal regime of subduction zones, as this strongly influences eclogite formation.

- The thermal regime indeed is an important factor for eclogite formation. This is addressed in the revised version (lines 49-51).

Lines 56–57: Clarify the rationale of this sentence, or consider removing it.

-The sentence is removed.

Line 59: Quantify exhumation rates, providing values from numerical models and natural estimates. Restrict the discussion to the specific tectonic setting under study (wedge geometry)

- Exhumation rates in the numerical models, and those reported from natural accretionary wedges are provided in the revised version (lines 65-69).

Line 61: At present, the introduction does not clearly define the scientific gap. While the questions posed are valid, they appear abruptly. The gap would be clearer if you outlined: (i) the discrepancies between modelled and observed exhumation rates, (ii) the wide variability in return-flow models, and (iii) the influence of parameters such as channel width, rheology, and boundary conditions. Emphasize the mismatch between observations and models.

- We sincerely thank the referee for giving us an excellent guideline to show the scientific gap in this work. The Introduction section has been substantially upgraded taking into account these points given by the reviewer. We must acknowledge that the modifications along these directions clearly bring out the novelty of this study.

Lines 62–63: Clarify whether questions (1) and (2) are essentially identical.

- Thanks for raising this point. The sentence has been modified.

Line 64: Revise to “theoretical and analog study.”

- Modified.

Line 65: Specify the depth range of both the theoretical and analog models. Although this is mentioned later, it should also appear here. State the main assumptions explicitly—for example, that the accretionary wedge is closed and material cannot enter the mantle—since this represents a special-case scenario.

- The depth range for the theoretical and analog models are mentioned in the revised version (lines 73-76). The main assumptions and limitations of the models are described briefly in this section, and discussed later in details in later parts of the article.

Line 70: Integrate content from later lines: prior work has already tested different boundary conditions in numerical models (e.g., Gerya et al., 2002). The novelty here lies in the analytical treatment of non-parallel slab boundary conditions, which allows replication of slab rollback and advance. When combined with analog modelling, this provides a unique contribution.

- The sentence has been rephrased in the revised version (lines 90-92). We thank the referee for these constructive suggestions.

Line 76: Clarify the phrase “oblique to the slab.” If it refers to the trench, rephrase as “a non-parallel component of slab velocity.”

- By the phrase “oblique to the slab”, we meant that the instantaneous subduction velocity vector is oriented oblique to the slab boundary on a vertical section perpendicular to the trench line (lines 86-89, lines 133-135). The sentence has been rephrased in the revised version.

Lines 86–88: This is the first mention of analog experiments, which are central to the manuscript. Introduce them earlier in the introduction and highlight the novelty of combining analytical solutions with analog modelling.

- We thank the reviewer for this nice suggestion. In the revised version of the manuscript, we have mentioned about the analog modelling approach of our study much earlier in the introduction section (lines 73-76).

Line 96: Since some material may be dragged down, this is an important limitation—discuss explicitly.

- The reviewer has correctly pointed out that the assumption of no material outflux through the bottom of the wedge is an important simplification, which is clearly mentioned in the revised version (lines 77-78, 105-107).

Line 107: Even if non-linearity is not first-order, note that complexity may also arise from contrasting lithologies within the accretionary wedge.

- We completely agree with the reviewer. Non-linear rheology or contrasting lithology in the accretionary wedge can result in strain localization, folding at large scales, which can perturb the velocity field predicted from simple continuum approximation with linear viscous rheology. This limitation is discussed in the revised version (lines 673-674).

Line 117: Clarify whether this component is oblique to the trench or simply non-parallel.

- In this article, we only consider the slab-oblique kinematics in a vertical section perpendicular to the trench line. So, the slab-velocity vector is non-parallel to its boundary with the wedge. This is clarified in the revised version (lines 86-89, 126-127, 133-135).

Lines 120–121: If the trench-oblique component is merely a reduction of subduction velocity, avoid presenting it as a trench-oblique term, since this implies a 3D model.

- We agree with the reviewer that the trench-oblique velocity component implies a three-dimensional framework. In our article, we clearly mention that by subduction obliquity, we mean a velocity component of subduction at an angle to the slab-wedge boundary, taken on a vertical plane perpendicular to the trench line (lines 126-127, 133-135).

Lines 127–128: Replace “trench-perpendicular vertical plane” with “non-parallel component of slab velocity.”

-The sentence is modified in the revised version (lines 133-134).

Line 130: In Fig. 2 it seems only the oceanic plate's fixed wall is constrained. Confirm whether the top boundary is also fixed.

- For the rigid overriding plate model, we considered the upper boundary of the wedge to be fixed (lines 130-131). However, for a deformable overriding plate (OP), the upper boundary was free to deform. As Fig. 2 shows the model setup for both the cases, the constraint of a fixed upper boundary for the rigid OP case was not shown exclusively in the figure.

Line 181: This derivation follows Moulas et al. (2021). Add a phrase such as "Following the approach of Moulas et al. (2021)..." You may shorten this section and direct readers to that reference until the non-parallel extension is introduced.

-Reference of Moulas et al. (2021) is added (line 188). However, for a more comprehensive presentation of the mathematical derivation for readers, we keep this part in the main text.

Line 214: Same as line 181.

-Reference is added (line 223).

Lines 229–230: Indicate that these models assume either a rigid overriding plate or very strong subduction channels, which generate extremely high overpressures.

- The texts of section 3 is modified in the revised version. We thank the reviewer for this suggestion.

Line 245: Clarify whether this is the discretization used to evaluate equations. If so, specify resolution and grid type.

- For measuring the return flux, we discretize the wedge into 1000 equal small arc elements at $r = 10$ km, where r is the radial distance from the wedge. Each arc covers an angle of $\theta_1/1000$. Since the velocity field in the corner flow solution is independent of r , F_R does not depend on the choice of r . The method of determining F_R is mentioned in the revised version of the manuscript (lines 252-254). Thanks for this suggestion.

Lines 261–263: Add references or case studies linking models to natural observations.

-References added (lines 269-273).

Line 267: In this simple case, the flow ratio (F_R) should equal 1 due to mass conservation and wedge geometry. Clarify whether this depends on discretization.

-We thank the reviewer for pointing this out. Numerically determined F_R value indeed depends on the discretization. However, a high-resolution analysis gives the numerically determined F_R close to the exact theoretically predicted value. In the revised version, we have done a high-resolution analysis of F_R by taking 1000 arc elements across the wedge. This gives F_R of 0.997, which is nearly 1 (line 278). F_R values are modified in the revised version.

Line 271: Is this because you are adding material through the slab or because of the "squeezing" of the wedge?

-Many thanks for this point. According to our model considerations, this occurs essentially due to squeezing of the wedge. Slab advance causes bulk horizontal shortening of the wedge (as discussed above), leading to exhumation of the wedge materials at higher rates. This issue is mentioned in lines 251-252.

Line 286: Is this difference with the rigid case because of the viscosity ratio only? what if μ_r is even higher (i.e., 10^5 or 10^7), do you reach the rigid wall solution? Also, see later comments to define high/low F_R .

-Yes, a lower value of F_R occurs as the walls are deformable ($\mu_r = 10^3$). For increasing μ_r to 10^5 or 10^7 , F_R approaches values, 0.993 and 0.996, respectively, which are close to the numerically determined value for the equivalent rigid case ($F_R = 0.997$). We appreciate the reviewer's comment.

Lines 290–291: Figures 4–5 show material still returning to the surface but further from the trench. Confirm whether F_R is calculated only for the accretionary wedge (if so, specify at line 245).

- In our calculation, F_R is calculated only for the accretionary wedge. This has been mentioned clearly in the revised version (lines 245-248).

Lines 292–294: Consider adding a figure similar to F_R vs. obliquity (Fig. 7) to illustrate this result, and extend the same approach to other variables.

- We thank the reviewer for this nice suggestion. Based on the reviewer's suggestion, we have now included an additional panel to independently show the effects of viscosity ratio (μ_r) on the return flux. However, both the plots of Fig. 10 (of revised version) individually show the effects of subduction obliquity, viscosity ratio, and taper angle on the F_R value. Hence, to maintain the number of figures in the manuscript, we do not provide an additional plot of F_R as a function of taper angle. Its effect, however, can be directly observed in both of Fig. 10 (a) & (b).

Line 323: Since deformation is not described, either remove the vorticity figures or move them to supplementary material with an explanation.

-We appreciate the reviewer's suggestion. We have provided the vorticity figures for showing the rotational behaviour of the flow field, which plays an important role in the tectonics of the wedge (as discussed in section 5.3). The role of several parameters on the vorticity field of the wedge is described in the Parametrical Analysis section. The vorticity figures are also cited later in the discussion section. Hence, we keep these figures in the main text to keep the coherence in the presentation of our model findings.

Line 344 and Fig. 7: $F_R = 0.5$ is presented as a threshold between significant and negligible return flow. Explain how this value was determined, or move section 3.2 to the discussion. Kerswell et al. (2023) may provide guidance.

- We appreciate the reviewer for raising this point. An ideal wedge setting (with rigid walls and wall-parallel subduction) produces return flows of buried materials with $F_R = 1$. A wedge with no return flow yields $F_R = 0$. Hence, we choose a value of 0.5 (middle between the two extremities) as a reference to express the return flow strength in wedges (mentioned in lines 249-251 of the revised version). Following the suggestion of the reviewer, we move section 3.2 to the 'Discussion' section in the revised version of the manuscript. We also thank the reviewer for providing the

reference of the work of Kerswell et al. (2023), which provides valuable insights on the recovery of HP rocks in subduction zones. This paper is appropriately cited in this version.

Line 351: Revise “wedge” to “downward-tapered wedge.”

- Modified (line 477).

Lines 356–357: Move this sentence to the discussion section.

-The sentence has been moved to the discussion section.

Lines 392–395: Provide scaling for analog experiments. Do they correspond with analytical models or plate tectonic velocities? Indicate scaling parameters (e.g., Schellart & Strak, 2016) to demonstrate consistency with natural systems and analytical calculations.

- Yes, the subduction velocities of the analog experiments are scaled with respect to natural plate tectonic velocities (and analytical models). Scaling of the laboratory experiments are discussed in details in the revised version of the manuscript (section 4.2). We thank the reviewer for this suggestion.

Lines 411, 413: Replace “30%” with “0.3U” and “one-sixth” with “U/6” to align with line 415 (“0.6U”).

- Replaced.

Line 413: Clarify whether oblique shortening with slab advance and oblique extension correspond to slab rollback. If so, use consistent terminology.

- Yes, by oblique shortening, and oblique extension we refer to slab advance, and slab retreat, respectively. However, we have changed these terms in the revised version of the manuscript for consistency in terminology.

Line 419: Remove “grossly.” Add a one-to-one comparison with the analytical solution.

-The term has been removed. In the revised version of the manuscript, we show one-to-one comparison of the velocity pattern of experimental results with analytical results in an identical setup (Fig. 8, 9).

Lines 422–423: Gravity influences the analog model if the bottom boundary is open. Add a brief discussion of this effect.

- We would like to clarify that the bottom boundary was not left open in our experimental model setup. The rigid bottom boundary (Fig. 7) restricted wedge materials from flowing out of the system.

Line 425: If slab rollback or oblique extension precludes return flow, clarify how comparisons were made, since the analog and analytical models differ in bottom boundary conditions.

-In both the analog and analytical models, the wedge is underlain by rigid material, representative of the upper mantle.

Line 429: Change “crusts” to “crust.”

-changed.

Lines 449–450: Clarify how comparisons between theoretical models and natural examples are made. Estimate taper angles and subduction dynamics (advance vs. rollback) for each exhumation case, then compare calculated F_R values with reviews (e.g., Agard et al., 2009) or case studies (e.g., Franciscan Complex; Ring, 2008).

-We thank the reviewer for this valuable suggestion. The exact values of the parameters, such as wedge taper angle, subduction obliquity, viscosity ratio, from natural settings are presently unavailable. However, to demonstrate the applicability of our model findings, we adopt a qualitative approach using observed signatures (e.g., relatively higher exhumed: subducted rock volumes ratio) in some of the natural accretionary wedges (Agard et al, 2009), and suggest that the subducting slab motion (e.g., advance) and rheological conditions (high μ_r) might be possible factors to facilitate the exhumation of deep crustal materials in convergent belts. For example, the high exhumation velocities (3-3.5 cm/yr) in the Ampelos/Dilek nappe during advance of the Anatolian microcontinent with Eurasia (Ring et al., 2007) can be correlated with high exhumation velocity (and F_R) obtained from our model results during slab advance (lines 549-553).

We also acknowledge that a one-to-one comparison between the model predicted F_R and natural observation is difficult as this ratio can be controlled by other factors, such as erosion, which are not considered in the present study (lines 506-508). These limitations are more elaborately discussed in the revised version.

Line 453: Are there documented cases of absent accretionary wedges in modern rollback settings?

-We could not find any case studies reporting the absence of accretionary wedges in rollback settings. In line 452-453 (of previous version), we only suggest the absence of return flows in accretionary wedges as a consequence of slab rollback settings, as observed in our theoretical and analogue models.

Line 454: Revise: note that HP units are present in Chile (Willner, 2005), with localized pressures of 2–2.5 GPa (González-Jiménez et al., 2017).

-The sentence has been removed in the revised version.

Lines 503–504: Mention the thermal regime, as it controls the brittle–ductile transition depth and influences viscosity.

-We have modified the sentence in the revised version (lines 562-563). Thanks for the suggestion.

Line 510: Replace “subduction” with “subduction zones.”

-Replaced.

Line 519: Expand to “Multiple structural fabrics and fluid-assisted deformation (e.g., Muñoz-Montecinos & Behr, 2023).”

-The sentence has been modified in the revised version (lines 591-592).

Lines 530–545: This section would benefit greatly from plotting the geological cases in Figure 8, enabling direct comparison between tectonic settings, model outputs, and natural data. Analog model results could also be added for completeness.

- We appreciate these suggestions by the reviewer. However, we could not find the exact estimates of the parameters (e.g., taper angle, subduction obliquity, wedge-OP viscosity ratio) for natural orogenic belts in existing literature. It has not been possible to show natural examples at any specific position in the graphical plot of Fig. 8 (of the previous version). However, in the discussion section, we provide examples of natural occurrences of shear reversal from several natural accretionary wedges well reported in literature. Considering the suggestion of the reviewer, analog model results, showing deflection of initial marker lines after finite deformation, are included in the revised version of the figure.

Line 590: Include rock strength in the brittle regime. In favorable conditions, rocks can sustain tens to hundreds of MPa before failure (Platt, 2019).

-We thank the referee for this suggestion. In the revised version, we have included the possibility of tectonic overpressures in brittle regime (lines 631-633).

Line 625: Remove μ_r here, as it denotes viscosity ratio.

-Removed.

Response to Referee #2's comments:

In the manuscript "On the criticality of return flows ...", the authors investigate scenarios of rock burial and exhumation during subduction and orogeny. They present analytical solutions for corner flow involving two adjacent deformable wedges: one representing the overriding plate and the other the accretionary wedge (or, in some cases, the subduction channel), where burial and exhumation may occur. The presented analytical model extends the model of Moulas et al. (GJI, 2021). While Moulas et al. considered only subduction velocities parallel to the wedge base, the present study also examines non-parallel velocities. The authors systematically analyze conditions under which significant return flow arises. They further compare the analytical velocity fields with velocity fields from analogue laboratory experiments for similar configurations and show first-order agreement between experimental and analytical velocity fields. Research on burial and exhumation in subduction zones remains highly relevant, as the controlling mechanisms are still debated. Comparing analytical predictions with analogue experiments is also very relevant. However, I have major concerns about the applicability of the presented analytical solution to natural subduction and orogenic burial-exhumation cycles.

- We are grateful to Referee #2 for thoroughly reviewing the manuscript, and raising critical questions on the analytical model considerations. In the revised version, we carefully address these issues. The comments have greatly helped us improve the quality of our theoretical and experimental results presentations.

Major comments

1) A key assumption of the analytical solution is that wedge geometry remains constant throughout the burial-exhumation cycle. Both the analytical model and analogue experiments yield essentially instantaneous velocity fields. However, the boundary conditions in the analytical model imply that wedge geometry must change over time, as observed in the experiments. If the velocity at the wedge base is not parallel to its base, the wedge will be squeezed or extended. Assuming average burial and exhumation rates of 10 mm/yr, a 50 km burial followed by 50 km exhumation would last ~10 Myr. In the scenarios presented (slab advance and rollback), non-parallel velocity components of just a few mm/yr would displace the wedge base by several tens of kilometers over this timescale. Such large geometric changes would strongly alter the internal corner flow field. Moreover, the issue of corner "stability" was examined by Moulas et al. (2021), but their findings are not fully considered here. For example, Fig. 4 includes results for viscosity ratios ≤ 1000 , yet at such values the internal wedge boundary would deform significantly during the burial-exhumation cycle, effectively destroying the corner geometry. For these reasons, I am not convinced that the presented velocity fields for slab advance and rollback can reliably be integrated to predict a complete burial-exhumation path.

- We greatly appreciate the reviewer's comments, raising these valid questions regarding the applicability of the model results where the wedge geometry evolves on million-year timescales. Our analytical solutions primarily aim to show the instantaneous kinematic behavior of a wedge for a given geometry and under a specific set of boundary conditions. We completely agree with the reviewer that a non-parallel velocity component of subduction in the boundary condition would lead to a progressive change in wedge geometry (squeezing or extension). In such cases, after a finite time of million years, the wedge geometry will largely differ from the initial geometry condition considered in deriving the analytical solution. To evaluate this limitation of our theoretical models, we present results from new sets of scaled laboratory experiments, run for a finite timescale (section 4 of the revised manuscript). The experiments allow us to track the evolution

of the velocity field within the wedge with time. Experimental results show that the first-order velocity pattern does not deviate much from the instantaneous velocity field predicted from the analytical solution (i.e., slab advance and/or high μ_r promote return flows, and slab rollback and/or low μ_r hinder them) (Fig. 8, 9). This finding stands qualitatively unchanged even after significant finite movements in the models, until gravity-driven flows begin to capture the wedge kinematics at shallow depths. These new experimental model results are included in the revised version (section 4.3, S4, S5 in supplement S1). We sincerely thank the reviewer for bringing out this very fundamental issue on our theoretical modelling.

The reviewer has also correctly pointed out the unstable nature of the corner geometry of a wedge for low viscosity ratios. The distortion of wedge-corner geometry occurs due to circumferential shearing at the wedge-overriding plate interface, as discussed by Moulas et al. (2021). However, this shearing decreases linearly with increasing distance from the wedge tip. Thus, a large part of the wedge-overriding plate interface remains almost unaffected even for moderate viscosity ratios ($\mu_r \sim 100$), and moderate taper angles ($\theta_1 \sim 30^\circ$) of the wedge, which is shown from new sets of analogue model experiments (detailed results included in this revised version section S4). In such a scenario, the two wedge walls grossly converge at angles close to initial θ_1 for the most part of the wedge, and their converging configuration plays the dominant role in governing the first-order velocity pattern in the ~ 20 km long wedge. In case of low taper angles ($\theta_1 < \sim 30^\circ$), or low viscosity ratio ($\mu_r \sim 10$), the circumferential shearing can be significant enough to distort the wedge-overriding plate interface on a longer distance. This results in a considerable change in the geometry of the wedge (as also observed in experimental models, Fig. 9, S5 in supplement S1). Experimental results, however, suggest that, in absence of gravity-driven flows, even the distorted wedge gives rise to flow patterns grossly similar to that observed in the initial stage of the experimental run (Fig. S5 in supplement S1). This phenomenon is similar to that observed in the classical lubrication theory, where the convergence of the bounding walls controls the velocity pattern, even when the wedge has a finite width at its tapering end (Batchelor, 1967; Mancktelow, 1995). However, for extremely low taper angles ($\theta_1 < \sim 10^\circ$), or very low viscosity ratios ($\mu_r < \sim 10$), the circumferential shearing can be large enough to significantly distort the whole wedge-overriding plate interface, which in turn will result in a considerable change in the velocity pattern with time. We thank the reviewer for providing us with these scientifically sound points.

2) Analytical corner flow models can be useful for certain geodynamic scenarios. However, regarding (U)HP rock exhumation in subduction zones, most 2D thermo-mechanical simulations do not generate wedges with forced corner flow when the subduction zone develops self-consistently (i.e., without a pre-imposed weak zone or wedge). In such models, (U)HP exhumation is typically driven by buoyancy or plate divergence/extension, rather than by forced return flow. Thus, the presented corner flow model may be applicable to burial and exhumation at crustal depths, but for rocks buried deeper than ~ 35 km, exhumation is more likely controlled by buoyancy. Another limitation is the assumption of constant linear viscosity across a wedge spanning the entire crust or even deeper. In reality, significant temperature variations produce large variations in effective rock viscosity. Also, deeper ductile regions may localize strain into shear zones, allowing the subducting plate to slide beneath the wedge without initiating a distributed corner flow. Hence, the authors should clearer discuss the range of applicability of their model. In particular, I find the reference to both an accretionary wedge and a subduction channel in Fig. 1a problematic. An application to shallow accretionary wedges seems far more realistic than to subduction channels in sub-crustal depths.

-We agree with the reviewer that factors such as buoyancy and extension can play important roles in the exhumation of (U)HP rocks from mantle depths. However, our model is strictly limited to

the viscous part of the accretionary wedge at crustal depths between ~ 20 km and ~ 40 km. This has been clearly mentioned in several places in the revised text (e.g., lines 73-76, 268). Our model does not account for the effect of buoyancy, extension, or erosion, as our primary objective is to exclusively evaluate the contribution of corner flow mechanics to the exhumation process of HP rocks under different geometric, kinematic, and rheological conditions. This limitation in our study is discussed in the revised version (lines 113-115, 678-680).

The assumption of constant linear viscosity across the wedge is indeed a simplification of the natural settings, where temperature-dependent, non-linear rheology may become effective in the lower crust. The reviewer has also correctly pointed out that the wedge deformation may be localized into shear zones, which is not considered in our continuum approximation of the accretionary wedge. These are indeed limitations of our present model and have been addressed in the revised manuscript (lines 675-676). We like to note here that we treat the wedge as a single continuum, considering that small-scale (with respect to the bulk wedge) perturbations, such as shear zones, are averaged out, and analyze the first-order velocity field using such a continuum approximation.

Minor comments

The introduction is a bit confusing because the authors do not use a consistent terminology. They mention accretionary wedges in places but seem to refer to subduction channels. In Fig. 1a the corner flow seems to be representative for both accretionary wedges and subduction channels. Buoyancy may be ignored in a shallow accretionary wedge but is likely important for subduction channels at sub-crustal depths.

-We would like to clarify that our work solely focuses on the accretionary wedge tectonics. The term subduction channel was used in a few places because of the usage of this terminology even for crustal depths, as an alternative to accretionary wedge in some of the previous works (e.g., Moulas et al., 2021). Corner flow kinematics has also been described previously for tapering deep subduction channels (Marques et al., 2018). However, the reviewer has correctly pointed out the importance of buoyancy for exhumation within deep subduction channels. In the revised version, we exclude using the term 'subduction channel' for our model.

Line 79-90: In section 2.1. the authors state that they generalize the corner-flow model of Moulas et al. (2021), which is based on coupling two corner flow solutions to consider the deformation of the overriding plate. Also, dynamic pressure fields, velocity fields and a velocity profile showing burial and return velocities have all been shown and discussed in Moulas et al. (2021). Also, Fig. 2 is very similar to figures 2 and 3 in Moulas et al. (2021). Maybe the authors could clearer state in the Introduction that their model is a modification of the model of Moulas et al. (2021). In lines 79-90 the study of Moulas et al. (2021) is not mentioned.

-Thanks for providing this suggestion. In the introduction section of the revised version, we clearly state that our analytical model is a modification of the corner-flow model of Moulas et al. (2021) (lines 90-91).

Line 103: Buoyancy is excluded but may play the dominant role for rock exhumation from sub-crustal depths in many orogenies.

- We agree with the reviewer that buoyancy can play a dominant role in the exhumation of deep crustal rocks in many orogens. This limitation has been acknowledged in the revised version (lines 113-115), as discussed above.

Line 236-237: I would argue that even more studies consider the positive buoyancy of subducted rocks as a more important mechanism for the exhumation of (U)HP rocks that have been subducted to depth larger than the average crustal thickness.

- Positive buoyancy of the subducted rocks does lead to upward movements of crustal blocks at high velocities (in the order of few cm/year). This has been considered to be one of the most effective mechanism for exhumation of crustal blocks. However, at the same time, several workers also stressed upon the importance of corner flow circulation in accretionary wedges (or even subduction channels) for the exhumation of crustal rocks (e.g., Yamato et al., 2007; Van Dinther et al., 2012, Marques et al., 2018). Such kinematics is likely to play the dominant role for uplift of rock from depth in the cases with low or no positive buoyancy. This buoyancy effect is clearly highlighted in the revised version (lines 37, 113-115, 485-487, 555-556, 678-680).

Line 270: If I understood correctly, a value of $F > 1$ implies that more material is exhumed by return flow than is subducted. How can this be applied to natural scenarios? It means that more material is exhumed than buried. How is this in agreement with mass balance and where is the additional material that is exhumed generated? $F > 1$ also implies that the wedge disappears after some time. Likely I missed something, but the authors should better explain the meaning, implications and applications for models with $F > 1$.

- We appreciate the reviewer's concern for bringing this critical issue regarding mass balance. We would like to point out that there is no additional material influx in our model. The mass conservation condition is always satisfied to obtain our analytical solutions (lines 149). During slab advance, bulk horizontal shortening leads to squeezing of the wedge, which results in an additional extrusion (exhumation) of materials in the wedge, leading to the condition $F_R > 1$. In the revised version of the manuscript, we clarify the points (lines 251-252), as suggested by the reviewer.

Line 274: The lower boundary of the wedge in Fig. 3 indicates the top of the subducting plate (see also Fig. 2a). When the velocity arrows are not parallel to this wedge boundary and point towards the subducting plate, then this implies that material from the wedge flows into the subducting plate, because the lower wedge boundary does not move with the velocity. This makes no sense to me. Keeping the wedge geometry constant but applying boundary velocities that would change the wedge geometry is not consistent.

- The velocity vectors used as the basal boundary condition indicate the subducting plate movement direction against the wedge. They do not imply the flow of wedge materials into the slab, but indicates the movement of the wedge-subducting plate interface. Evidently, the wedge under this condition is extending by the moving slab, and such movement would change the wedge geometry with time, as correctly noted by the reviewer. We would like to mention here that our analytical solution essentially aims to deal with the instantaneous velocity field for a given wedge geometry, and kinematic boundary conditions. To study its evolution, we have performed analog model experiments. Their results are presented in the revised manuscript, showing the temporal variation of the velocity field in a wedge with progressively changing geometry (Fig. 8, 9). We also compare them with the analytical results. We sincerely thank the referee for raising these excellent points.

Line 315-316: The mentioned interface migration has been studied by Moulas et al. (2021) with a 2D dimensionless “regime diagram” distinguishing a “stable corner” (with negligible interface migration) and an “unstable corner” (with significant interface migration).

-We mention the discussion by Moulas et al. (2021) in our revised manuscript (lines 328-329). Thanks for the suggestion.

Section 4.1: Please provide a scaling analysis for the experiment so that it is clear how the laboratory experiments can be scaled to the natural situation.

- In the revised version of the manuscript, we have added the scaling analysis of our analog experiments (section 4.2). Thanks for the suggestion.