

We thank Referee #2 for the valuable comments and suggestions. The replies to each comment are given below (marked in blue).

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 duration up to 10 Ma. 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). This finding stands qualitatively unchanged even after significant finite movements in the models. These new experimental model results are included in the revised version. 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). 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. This phenomenon is similar to that observed in the classical lubrication theory, where the convergence angle 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, in case of extremely low taper angles ($\theta_1 < \sim 10^\circ$), or very low viscosity ratio ($\mu_r < \sim 10$), the circumferential shearing can be large enough to distort the wedge-overriding plate interface for longer distances, which in turn results in a considerable change in the velocity pattern with time. We acknowledge this limitation and include an explicit discussion on this issue in the revised version. 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. 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.

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. 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).

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, 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. 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.

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. 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, 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-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. 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 regime diagram of Moulas et al. (2021) in our revised manuscript. 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. Thanks for the suggestion.