

Answers point-to-point

October 1, 2026

Corrections in the tracked manuscript are in red.

1 Report of Reviewer #1

The referee writes The manuscript presents an interesting modelling framework for understanding magma ascent through the lithosphere and begins to address the thermal controls on magma propagation. The theoretical framework is potentially valuable, and I appreciate the authors' efforts to develop a model that captures some of the complexities of magma ascent. However, I found the manuscript difficult to follow in some places, particularly with respect to the interpretation of some of the results and the treatment of changes in melt fraction, density, and viscosity through time within the model. In some instances, the interpretation of model results appears broader than what can be fully supported by the current model formulation, and I think these interpretations would benefit from being more carefully expressed. I also found that several definitions, equations, initial and boundary conditions, and figures would benefit from additional explanation for the manuscript to be accessible to a broad readership. Overall, I think the manuscript would benefit from further clarification of the model assumptions and limitations, as well as a more nuanced discussion of the implication of the results.

Answer We thank the referee for appreciating our work. We have addressed the majority of the referee observations and we hope that the new version of the manuscript deserves to be published on GMD.

The referee writes The abstract is somewhat difficult to follow, and I was not always able to identify where all the concepts introduced in the abstract are shown in the manuscript. I suggest revisiting the abstract to ensure that all the concepts are clearly supported in the main text.

Answer We thank the Referee for this valuable suggestion. We agree that some expressions in the original abstract were overly generic and did not map transparently to the terminology used in the main text. To address this concern, we have fully rewritten the abstract to ensure a strict alignment with the manuscript's core sections.

The referee writes Line 5: It would be helpful to clarify what is meant by a 'physically meaningful initial/boundary condition'. Is this meant to describe

a condition that is both a boundary and initial condition, or are two separate types of conditions being referred to?

Answer We thank the referee for rightly pointing out a lack of precision in our original text. In the new version of the abstract we more clearly state that: To integrate the governing equation, we formulate a well-posed initial-boundary value problem, requiring both initial and boundary conditions based on physically realistic profiles.

The referee writes Line 27-28: I found the statement ‘crystal content of magma being incompatible with density contrast required for magma propagation in fractures’ difficult to follow. As currently written, it appears to suggest that ascending magma cannot contain crystal cargo while propagating through fractures. However, there is evidence for crystals derived from deeper regions of magmatic systems being transported to the surface, demonstrating that crystals can be present in ascending magmas (see Cashman et al. 2017, Science for a review). Furthermore, numerical modelling has suggested that crystals can be present within a high melt fraction magma body capable of generating sufficient buoyancy for magma propagation (e.g. Booth et al. 2026, JPet). Therefore, I suggest the authors clarify this statement.

Answer We thank the Referee for this acute observation and for pointing out these important references (Cashman et al., 2017; Booth et al., 2026). We completely agree that our original phrasing was misleading, as it inadvertently implied that ascending magma cannot carry any crystal cargo during fracture propagation. Our original intention, following the arguments of Vigneresse and Clemens (2000), was to highlight that a critically high crystal fraction can significantly alter magma rheology and bulk density, potentially hindering the specific density contrast required for pure buoyancy-driven propagation in certain structural settings. To clarify this point and present a more balanced view that accounts for natural crystal transport, we have revised the text on lines 27–30. We now explicitly state that while low-to-moderate crystal cargo is regularly transported to the surface within high melt-fraction magmas, a very high crystal load represents a limiting factor.

The referee writes Line 39: Could the authors clarify what drives heat propagation in this model? Is this primarily controlled by the temperature gradient through the lithosphere, radiogenic heat production, heat flow from the mantle, or a combination of these processes?

Answer We understand the Referee’s point, and we agree that clarifying the primary physical drivers of heat propagation helps prevent misunderstandings regarding the scope of our model. In our framework, heat propagation is driven by two main components: 1. The initial thermal contrast between the hot magma batch T_m and the cooler surrounding lithospheric rocks T_r , which triggers the local conductive mechanism and subsequent partial melting (as introduced in Section 2, lines 56–58). 2. The steady heat flow from the mantle, which is mathematically represented by the energy source term κT in Equation (12) and explicitly attributed to the heat coming from the underlying asthenosphere (Section 3, lines 200–202).

To isolate the primary transport mechanism and keep the linearized model

analytically and numerically tractable, secondary processes such as crustal radiogenic heat production are neglected. We have added a brief clarification in Section 3 (after old line 202) to explicitly state these assumptions.

The referee writes Line 44: There appear to be a number of approximations introduced throughout the manuscript. I would encourage the authors to provide a consolidated list of the principal assumptions and approximations, perhaps in an appendix, together with a brief discussion of their expected implications for the results. This would be easier for readers to assess the applicability and limitations of the model.

Answer We acknowledge the Referee’s suggestion aimed at improving the accessibility of the model’s limitations. We would like to point out that the physical justifications for our main approximations (such as constant density, viscosity, and ascent velocity) were already discussed and supported by classical literature in geophysical fluid dynamics within Section 3 (lines 209–224).

However, to satisfy the Referee’s request for a more consolidated and easily accessible overview, we added a small appendix with a summary of the core assumptions and their implications. This allows readers to immediately assess the boundaries of applicability of the present linear framework.

The referee writes Line 51: Could the authors clarify whether the magma batch at the lithosphere-asthenosphere boundary is present only as an initial condition, prior to ascent, or whether a magma source is continuously maintained at this boundary throughout the model?

Answer The model is rather simple and we analyze the evolution of the temperature and a continuous heat source is considered. So, we consider the magma inside the magmatic chamber driven by the propagation of the heat originating in the asthenosphere (notice that in the older version of the manuscript the energy balance, Eq. (17c), there is a heat source).

The referee writes Line 53: Point 3. Could the authors clarify whether this is intended to describe the buoyant upwelling of the magma batch as a results of newly melted material? It is currently hard to understand.

Answer We thank the Referee for pointing out this phrasing issue. The Referee’s interpretation is exactly correct: the magma batch ascends precisely because it finds itself enveloped by the host rock that has just been converted into a fluid state by partial melting, thus exploiting the density contrast. The original sentence contained a clumsy repetition (“in the melted rocks... in the newly melted rocks”) that caused unnecessary ambiguity. We have rewritten Point 3 in Section 2 to state this mechanism clearly and concisely.

The referee writes Lines 58-59: The index, i , does not appear to be defined. Please clarify what this index means.

Answer We thank the Referee for pointing out our imprecision. In the new version of the manuscript the suffix i is correctly defined.

The referee writes Lines 62-64: If $T_i = T_m$, how does the model account for the possible addition of melt volume to the upwelling magma body as a result of partial melting? More generally, if partial melting of surrounding rocks is included, this process could alter both the volume and composition of the magma. Could the authors discuss how these effects are treated? In

addition, compositional changes associated with partial melting could affect the ascent velocity. I would appreciate clarification as to whether partial melting can be fully represented in the current formulation with accounting for these effects.

Answer We thank the Referee for this insightful comment. The Referee highlights a fundamental coupling in real magmatic systems: partial melting of the wall rock typically leads to crustal assimilation, which alters both the total volume and the chemical composition (and hence the viscosity and ascent velocity) of the magmatic body. However, as correctly surmised, our present formulation intentionally neglects the chemical composition and mass/volume variations of the magma batch. The model treats the magma body as a thermodynamic entity of constant mass and volume that propagates via a localized thermal envelope. This approximation is necessary to maintain the linearity of the partial differential equation and to isolate the pure physics of heat propagation and buoyancy-driven transport. We had already noted in Section 4 (lines 278–280 and 288–290) that magma differentiation and chemical properties are outside the scope of Equation (12). To explicitly address the Referee’s concern regarding the conservation of volume and composition during melting, we have added a dedicated point in the newly created Appendix (Model Assumptions and Limitations) to clarify this condition of our model.

The referee writes Line 69-70: References to recent experiments here would be helpful, particularly where these experiments provide the physical basis for assumptions made in the model.

Answer Three references have been added in the new version of the manuscript.

The referee writes Lines 84-85: References should be provided for the experiments conducted during the 20th century that are mentioned in this sentence.

Answer Done.

The referee writes Line 91-92: There is a relatively abrupt transition between these paragraphs. I would appreciate some additional explanation of why the framework of EIT is introduced at this point.

Answer We understand the Referee’s comment regarding the transition between the historical introduction of non-Fourier heat models and the formal thermodynamic derivation. The framework of Extended Irreversible Thermodynamics (EIT) is introduced precisely because it provides the rigorous, modern thermodynamic foundation needed to handle the parameters that vary significantly with temperature and the memory effects mentioned at the end of Section 2.1, while ensuring strict compatibility with the Second Law of Thermodynamics. To smooth this transition and clarify this logical link for the reader, we have added a brief explanatory sentence at the very end of the paragraph, seamlessly introducing the mathematical formulation that follows.

The referee writes Line 111: inequality (4) appears before it has been introduced, which made this section difficult to follow. I suggest introducing the inequality earlier or restructuring this section that the reader is familiar with it before it is used.

Answer We fully agree with the Referee and thank him/her for catching this

chronological error in our referencing. We have corrected line 111 by changing the reference from inequality (4) to inequality (2), which restores the correct logical and chronological flow for the reader.

The referee writes Line 125: Could the authors clarify whether the lambda introduced here is the same quantity as the lambda used in the heat-flux formation? If these are different quantities, alternative notation may be good.

Answer We thank the Referee for pointing out this potential source of confusion. The parameter λ used in Section 2.1 (Line 80) and Section 3 represents a physical property (the thermal conductivity), whereas the λ_i coefficients introduced in Equation (4) are purely mathematical tools (Lagrange multipliers). We have updated the manuscript by replacing the Lagrange multipliers λ_i with γ .

The referee writes Line 131: Please explicitly identify the field equations being referred to, preferably by their equation numbers, so that the readers can easily identify them.

Answer We thank the Referee for this suggestion. Indeed, the term "field equations" throughout the manuscript refers to the fundamental balance equations introduced as System (1) on Page 5. To make the text more user-friendly and ensure that readers can instantly identify the governing equations without scrolling back, we have revised line 131 by replacing the generic phrase with an explicit reference to Equation (1)

The referee writes Line 145: It was not immediately clear to me how the matrices in these formulation become the same size such that they can be added. I suggest providing the dimensions of the matrices.

Answer We would like to clarify that Equation (5) does not involve the addition of matrices or vectors of different dimensions n and m , which would indeed be mathematically impossible. Instead, every single term in the expression is a scalar quantity (a 1×1 tensor) resulting from dot products and a quadratic form:

$A \cdot X$ is the dot product of two n -dimensional vectors, yielding a scalar.

$Y^T B Y$ is a quadratic form (where Y is an m -dimensional column vector, Y^T is a $1 \times m$ row vector, and B is an $m \times m$ matrix), which strictly results in a scalar.

$C \cdot Y$ is the dot product of two m -dimensional vectors, yielding a scalar.

The referee writes Line 181: The model allows pressure to vary with density and entropy, but pressure will also vary with depth through the lithosphere. How is the depth dependence incorporated into the model? Does density also vary with depth?

Answer The depth dependence of pressure is already explicitly incorporated into our model. As shown in Section 3, Equation (10b) ($p_z = -(\rho_0 + \tilde{\rho})g$), the vertical pressure gradient explicitly accounts for the hydrostatic contribution over depth z . Regarding density, in this linearized framework, the background density ρ_0 is treated as constant, while $\tilde{\rho}$ represents a small spatial perturbation, meaning that the full density depends on space and time only through this perturbative term. As now consolidated in the newly added Appendix B (Point 2), keeping the background density constant is a well-established standard in

geophysical fluid dynamics (akin to the Boussinesq approximation). This simplification allows us to decouple the thermodynamics from complex non-linear rheological feedback, keeping the model analytically tractable while successfully isolating the primary transport mechanisms.

The referee writes Line 182: Are the assumptions in equation (9) the constants?

Answer We would like to clarify that the expressions in Equation (9) are not constants themselves, but rather specific constitutive assignments for the thermodynamic functions ϕ_0 , s_0 , r_{10} , r_{20} , r_{21} chosen to linearize the governing model. As explicitly detailed in the lines immediately following the equation (lines 180–181), these functions depend on state variables (such as internal energy ε and incorporate several parameters τ , κ , λ , c_v that are indeed treated as positive constants. Therefore, Equation (9) defines functional relationships, while the parameters contained within them are the constants driving the linearized system.

The referee writes Line 183: Could the authors specify what ‘other coefficients’ are being referred to here? It would be helpful to identify these explicitly.

Answer We thank the Referee for this request for clarification. The phrase “other coefficients” refers to the remaining arbitrary constitutive functions present in the general restrictions (7) and (8) (such as r_{11} , r_{12} , r_{13} , r_{14} , r_{23} , r_{24} , r_{25} , r_{26} , and s_1 that have been set to zero in this specialized case to ensure both dimensional consistency and the perfect linearity of the final system equations.

To make this completely explicit for the reader, we have updated lines 181–182 by listing these coefficients in parentheses.

The referee writes Line 185: Could the authors discuss more explicitly how the results would change if velocity was able to vary through time in response to changes in viscosity and density? This seem particularly important if partial melting is a part of the model, as the associated compositional change of partial melting could substantially modify magma properties and therefore ascent velocity.

Answer We thank the Referee for this stimulating comment. In natural magmatic systems, a time-dependent velocity coupled with temperature-dependent viscosity and density variations is a well-known phenomenon. If the velocity u were allowed to vary in response to partial melting and compositional changes, it would introduce a strong non-linear coupling into the system. Qualitatively, a decrease in viscosity due to higher temperatures or country-rock melting would accelerate the ascent velocity, whereas a critically high crustal contamination might increase the bulk density and retard the upwelling. However, incorporating a time-varying velocity field would completely destroy the linearity of our governing second-order hyperbolic equation (Equation 12). This would prevent us from analytically and numerically isolating the core transport mechanisms, such as the propagative thermal wave behavior (Cattaneo terms) and our newly introduced dissipative term T_{tz} , which represent the primary scientific novelties of this paper.

As stated in Section 3 (lines 225–227) and Section 4 (line 295), a rigorous evaluation of a non-linear hyperbolic equation where velocity, density, and vis-

cosity dynamically depend on temperature and time is a highly complex task that is currently under investigation and will be the subject of a forthcoming paper. To ensure this boundary of our current framework is perfectly clear to the readers, we have explicitly formalised these implications in the new Appendix.

The referee writes Line 186: Could the authors explain the justification for neglecting the higher-order terms? Is this related to satisfying the second law of thermodynamics?

Answer We would like to clarify that neglecting the higher-order terms at this stage is not related to satisfying the Second Law of Thermodynamics. The compatibility with the second principle of thermodynamics was already fully and rigorously achieved in Section 2.1 through the exploitation of the extended Liu procedure, which resulted in the general restrictions and constitutive equations (7) and (8). The omission of higher-order terms on line 186 represents a standard algebraic linearization procedure widely adopted in continuum mechanics. When we express the total density as a baseline constant value plus a small spatial perturbation $\rho = \rho_0 + \tilde{\rho}$, we deliberately neglect non-linear product terms (such as $\tilde{\rho}^2$ or products of perturbations and their gradients) under the robust physical assumption that the perturbations are small $\tilde{\rho} \ll \rho_0$.

This standard truncation filters out negligible non-linearities, allowing us to derive the linear hyperbolic equation (Equation 12) and to analytically investigate the primary, first-order transport mechanisms of the magma batch. We have added a brief sentence after line 186 to make the perturbative nature of this truncation explicit for the reader.

The referee writes Line 191: Could the authors explain why the coefficient r_{22} is assigned this value?

Answer The specific assignment $r_{22} = -\tau\rho_0 u^2$ is driven by an exact algebraic requirement during the cross-differentiation of Equations (10c) and (10d). In the process of combining the balance and heat flux equations to eliminate the heat flux q and isolate a single governing equation for the temperature T , several coupling terms arise. Setting r_{22} to this precise value ensures the exact cancellation of these mathematical remnants. Without this targeted choice, the system would remain coupled, preventing us from obtaining the clean, single second-order hyperbolic PDE for temperature represented by Equation (11). This is a standard calibration procedure in mathematical physics to decouple multi-variable balance systems.

The referee writes Line 209-224: I do not disagree with the simplifying assumptions adopted here for the development of this model. However, there are existing models that incorporate temperature, composition, volatile content and changes in magma viscosity during ascent through a dike (e.g. Abdullin et al. 2024 GJI). I suggest that these studies are acknowledged.

Answer We thank the referee for her/his suggestion. The reference to the paper of Abdullin et al. 2024 has been added in the new version of the manuscript.

The referee writes Line 225: Density, viscosity and therefore velocity can all depend on magma composition. Since partial melting appears to be represented in the model, compositional evolution may therefore be an important

consideration. I suggest explicitly acknowledging this as a limitation and discussing its potential consequences for the model results.

Answer We acknowledge the Referee’s comment, which correctly describes the petrological and rheological feedback in natural systems. Indeed, as country-rock partial melting occurs, the assimilation of crustal materials introduces compositional changes that can strongly modify the magma’s viscosity and density, thereby altering the ascent velocity over time. As stated in our previous responses, accounting for these composition-driven variations would introduce severe non-linearities, transforming our linear differential framework into a highly coupled, non-linear system. Qualitatively, the main consequence of omitting these effects is that our model captures the primary, first-order thermal-buoyancy envelope rather than the finer acceleration or deceleration phases (such as viscous stalling) typically caused by chemical evolution. To transparently formalize this limitation and discuss its potential physical consequences for the results, we have expanded Point 4 in the newly added Appendix (Model Assumptions and Limitations). This addition explicitly addresses how composition, viscosity, and velocity would interact in a fully non-linear regime, setting the baseline for our future investigations.

The referee writes Line 229-230: I am unsure how the results would be expected to change in three dimensions if propagation is restricted to one dimension, other than faster heat loss. If propagation was allowed in multiple directions, I would expect to see substantially different behaviour, e.g. sill formation. Therefore, I am unsure whether this comparison provides a meaningful validation of the present model.

Answer We agree with the Referee that in a fully unconstrained three-dimensional geological setting, multi-directional magma propagation leads to substantially different structures, such as lateral sill formation or dike branching, especially when interacting with tectonic stresses or horizontal structural discontinuities.

Our statement on lines 229–230 was not intended as a validation of the comprehensive, multi-directional geological evolution of magmatic plumbing systems. Rather, it was meant as a strict mathematical consistency check for our transport equation. Specifically, we verified that when embedding our new mixed dissipative term T_{tz} into a 3D Laplacian operator with a vertical velocity field $u = (0, 0, u_z)$, the fundamental propagative and hyperbolic characteristics of the thermal wave along the vertical axis remain qualitatively robust, despite the expected faster lateral heat dissipation.

To prevent future readers from misinterpreting this mathematical check as a structural geological validation, we have revised the text on lines 229–230 to clarify this distinction and explicitly acknowledge that multi-directional propagation regimes (such as sill formation) fall outside the scope of this vertical ascent model.

The referee writes Line 233: Could the authors explain the rationale for selecting a 3-year timescale for non-dimensionalisation? What is this based on?

Answer The selection of the time scale $\alpha = 10^8 \text{ s} \approx 3.17 \text{ years}$ is a standard non-dimensionalization choice based directly on the characteristic physical

parameters of the problem. Given that the spatial scale is fixed by the thickness of the lithosphere ($\beta = 2 \cdot 10^4$ m) and the baseline buoyancy velocity utilized in the model is of the order of $u \approx 10^{-4}$ m/s, the characteristic transit time for the magma batch to propagate through the domain can be estimated from an order-of-magnitude scaling argument as:

$$\tau_{\text{transit}} \approx \frac{\beta}{u} = \frac{2 \cdot 10^4 \text{ m}}{10^{-4} \text{ m/s}} = 2 \cdot 10^8 \text{ s}$$

Choosing $\alpha = 10^8$ s as the scaling factor ensures that the dimensionless variables in Equation (13) balance efficiently and that the dimensionless time t spans a numerically stable and visually clear interval (from 0 to 2, as displayed in the solution profiles of Figures 3–5). It is therefore an operational choice dictated by the combination of the domain geometry and the velocity field, rather than an independent a priori geological constraint.

The referee writes Line 237 and throughout: It would be helpful to restate the definitions of u and d . More generally I recommend using the variable’s name rather than symbol within the text for easier readability, especially when there are a lot of symbols to keep track of.

Answer We thank the Referee for this practical suggestion to improve the readability of the manuscript. While the definitions of the buoyancy velocity u and thermal diffusivity d were established in the preceding pages (lines 193 and 202), we agree that explicitly restating their names alongside the symbols prevents the reader from having to scroll back.

Following your recommendation, we have updated line 237 and reviewed the entire manuscript to ensure that, whenever crucial parameters are discussed, their full physical names (e.g., "buoyancy velocity" and "thermal diffusivity") accompany the mathematical symbols.

The referee writes Line 243 and figure 2: Could the authors provide a reason for why these initial and boundary conditions have been chosen? How do they compare to natural systems?

Answer We would like to point out that the physical and geological rationale behind the choice of these initial and boundary conditions, as well as their comparison with natural magmatic systems, was already discussed in detail in the manuscript on Page 11 (lines 250–258). To summarize how these constraints map directly onto real geological systems:

Boundary Condition ($T(0, t) = 1007.04^\circ\text{C}$): in natural systems, the stable thermal boundary between the lithosphere and the underlying asthenosphere is characterized by temperatures around 1000°C [Ranalli and Rybach, 2005; Cloetingh et al., 2010]. Our boundary condition at $z = 0$ represents this continuous heat supply from the mantle.

Initial Condition (Gaussian profile with a peak at 1230°C): This represents the thermal perturbation caused by the sudden injection of a parental magma batch at the base of the lithosphere. A temperature of 1230°C is highly representative of primary mantle-derived mafic magmas.

Spatial Scale (≈ 4 km): As explained on Page 11, the geometric width of

the Gaussian perturbation in our dimensionless spatial domain corresponds to a magma batch dimension of approximately 4 km, which is fully consistent with geophysical estimates of deep basaltic reservoirs and erupted volume distributions [Bower and Woods, 1998; Papale et al., 2021].

Since the exponential background decay of the ambient lithospheric temperature profile becomes negligible compared to the massive thermal anomaly of the newly injected magma, the Gaussian superposition accurately models the early stage of vertical propagation. To ensure this comparison is immediately clear, we have added a brief cross-reference at line 243 directing readers to the detailed physical discussion already present in Section 3.

The referee writes Line 251: I would collate the initial and boundary conditions for readability.

Answer Done.

The referee writes Line 259: 1000°C is relatively high for a rock melting temperature. Some rocks can melt at 700°C depending on composition, pressure and volatile content. Could the authors provide the source for the selected melting temperature and specify what rock composition is being represented?

Answer We thank the Referee for this petrological comment. We completely agree that in the upper crust, under water-saturated conditions and for felsic (granitic) compositions, partial melting can initiate at temperatures as low as 700°C [Cashman et al., 2017]. However, our model describes the vertical transport of a primary mantle-derived magma batch through the deep lithosphere and the lower continental crust (as introduced in Section 1 and detailed in Section 4, lines 272–273). In these deep, high-pressure environments, the host rocks are predominantly anhydrous and mafic to ultramafic in composition (such as peridotites or dry granulites/amphibolitites). For these deep lithospheric lithologies, a representative baseline temperature for the onset of significant partial melting and structural softening is well-established to be around 1000°C in standard geodynamics literature [Turcotte and Schubert, 2014, Chapter 4].

Choosing 1000°C as the threshold T_i represents a standard, physically justified compromise to capture the first-order dynamics of deep crustal assimilation. To address the Referee’s comment, we have added a brief clarification and the reference to Turcotte and Schubert (2014) at line 259 to explicitly state that this melting temperature corresponds to dry lower-crustal/mantle host rock compositions.

The referee writes Line 261: The statement that thermal diffusivity does not play a significant role appears to be an important conclusion. Could the authors expand on what this means for magmatic systems? And whether there are any conditions where they might expect thermal diffusivity to become important?

Answer We thank the Referee for this highly constructive observation. The insensitivity of the magma ascent dynamics to variations in the thermal diffusivity d within its realistic physical range is indeed one of the core insights of our model. Physically, this indicates that the vertical propagation of a high-energy magma batch through the lithosphere is an advection-dominated process rather than a conduction-driven one. Once the localized conductive mechanism creates

the thin, molten circular annulus (thermal envelope) required to soften the host rock, the timescale and depth of the ascent are dictated almost entirely by the buoyancy velocity field u . In terms of dimensionless analysis, the system operates at a high Péclet number, meaning that bulk mechanical transport vastly outpaces thermal diffusion.

We expect thermal diffusivity to become critical in two specific alternative regimes:

Magma Stagnation / Neutral Buoyancy Zones: If the buoyancy velocity approaches zero ($u \rightarrow 0$) due to rheological barriers or density equilibration, the system shifts from propagative to purely diffusive, and the survival time of the batch would depend strictly on d .

Long-term Post-emplacement Cooling: Once the magma emplaces and forms a stable magma chamber, advective transport ceases. The subsequent lifetime, fractional crystallization, and thermal signature of the reservoir over thousands or millions of years are entirely governed by thermal diffusivity [Whittington et al., 2009].

We have expanded the discussion in Section 4 (Conclusions) to explicitly integrate these physical implications, which significantly enriches the geological impact of our conclusions.

The referee writes Line 263-267: I understand the authors' argument that differences in ascent velocity could contribute to differences in magma composition. However, they do not more magma composition change. Furthermore, several properties may affect ascent velocity (density and viscosity) and vary with magma composition and temperature, and these effects are not represented in the current model. In addition, volatile content can have a substantial influence on magma properties and ascent behaviour. Therefore, I suggest that this conclusion is presented more cautiously and that the authors discuss the extent to which it can be supported by the current model

Answer We thank the Referee for this cautionary note. We agree that since our model does not explicitly track chemical species, volatile exsolution, or composition-dependent rheological feedbacks, the link between the ascent velocity u and the final magmatic spectrum must be presented as a qualitative, speculative interpretation rather than a direct numerical output of the model. Following your recommendation, we have rephrased the text on lines 263–267 to adopt a much more cautious tone, explicitly framing this mechanism as a simplified, qualitative physical boundary. Furthermore, we have explicitly cross-referenced this discussion with the newly added Appendix B (Point 2 and Point 4), where the limitations regarding volatile content, chemical evolution, and their unmodeled feedback on velocity are thoroughly outlined.

The referee writes Line 278-279: Magma differentiation could affect several properties in this model, including density. I suggest that the consequences of this simplification are more explicitly acknowledged as a limitation.

Answer We acknowledge the Referee's comment. Magma differentiation, assimilation, and cooling in natural plumbing systems undeniably alter the bulk density and mineralogy of the magma batch, introducing dynamic feedback on buoyancy and ascent dynamics. As detailed in our previous responses,

our current linear differential equation deliberately treats these parameters as constant to isolate the fundamental thermodynamic transport envelope. To explicitly acknowledge the consequences of this simplification as a structural limitation—exactly as requested—we have already implemented a comprehensive discussion in Point 4 of Appendix B (Model Assumptions and Limitations), which explicitly details how chemical differentiation and density variations interact in a non-linear regime. To ensure the reader is immediately guided to this discussion, we have added an explicit cross-reference to Appendix Model Assumptions and Limitations.

The referee writes Line 282: Processes such as reactive flow and compaction may also influence magma differentiation and transport, including at high melt fractions.

Answer We thank the Referee for pointing out these additional fluid-dynamic mechanisms. It is well established in the physics of partially molten media that reactive melt transport and matrix compaction play a significant role in segregating and differentiating magma, even within high melt-fraction domains [Spiegelman, 1993; Dominguez et al., 2024]. However, incorporating these micro-scale and multi-phase coupling mechanisms would require a deformable porous media framework (such as the classical McKenzie-type formulations), which is fundamentally different from our macroscopic, single-body hyperbolic thermal wave approach. To acknowledge this valuable point, we have updated the text on line 282 to explicitly mention reactive flow and compaction among the long-term processes taking place in the accumulation zone, and we have added a dedicated sub-point in Appendix Model Assumptions and Limitations to formally state that these porous-media compaction feedbacks are outside the scope of our linear continuum formulation.

The referee writes Line 284-285: I find the representation that magma ascent through the brittle crust is ‘no longer dominated by buoyancy forces’ to be not entirely true. Although other forces and mechanisms may become more important in the brittle crust, buoyancy can remain an important driving force for magma migration. Buoyancy can contribute to the pressure differences that can drive magma into and through fractures. I suggest that this statement is reconsidered.

Answer We thank the Referee for this correct clarification. We agree that buoyancy does not cease to exist in the brittle crust; rather, the density contrast between the magma and the host rock continues to supply a fundamental hydrostatic driving force that contributes to the pressure gradients within fractures and dikes. Our original phrasing was too absolute. Our intention was to highlight that in the shallower, brittle crust, pure buoyancy-driven thermal upwelling (the mechanism explored in our model) is no longer the sole dominant factor, as it becomes highly coupled with and modulated by structural and mechanical forces. To address the Referee’s concern, we have reconsidered and revised the statement on lines 284–285. We now explicitly state that buoyancy remains an important driving force, acting in concert with magma overpressure, volatile content, and regional tectonic stress fields.

The referee writes Appendix A1: The magma volumes assumed to erupt

from the chamber (0.00005-0.05km³, if I’ve understood correctly) appear relatively small compared with estimated critical chamber volumes associated with eruption in Townsend and Huber 2020 (approximately 1-10 km³), as well as volumes inferred for lower-crustal magma bodies in Booth et al 2026 ($>\approx 1$ km³). Could the authors explain the rationale for selecting these relatively small magma volumes and discuss how representative they are of natural systems? Also why only 10% of magma will leave the chamber?

Answer We thank the Referee for this comment, which allows us to clarify a crucial aspect of our scaling model. There is a fundamental distinction in our framework between the size of a single ascending magma batch and the cumulative volume of a fully developed magma chamber.

The radius $R \in [50, 500]$ m defined in Appendix A1 does not represent the final size of the cumulative magma chamber, but rather the geometry of an individual, discrete magma pulse rising from the asthenosphere. In modern geodynamics, it is well established that crustal magma chambers (such as the large reservoirs of $1 - 10$ km³ discussed by Townsend & Huber 2020 or Booth et al. 2026) are assembled incrementally over time by the successive arrival of many smaller magma batches, rather than via a single massive injection. Therefore, scaling our individual pulse sizes from the global erupted volume distributions of single events [Papale et al., 2021] is physically justified and representative of individual feeding pulses.

The assumption that only $\approx 10\%$ of the chamber volume is erupted during an active phase is a standard cornerstone in physical volcanology, supported by classical fluid-dynamic and mechanical constraints [Bower and Woods, 1998]. During an explosive or effusive eruption, the system is driven by magma overpressure. As magma leaves the chamber, the internal pressure drops rapidly. Due to the elastic feedback of the host rock, the conduit structurally closes and the eruption ceases long before the chamber is depleted, leaving roughly 90% of the magma stored storage as an intrusive body. We have added a clarifying sentence in Appendix A1 to explicitly remind the reader that R refers to individual feeding pulses and that large reservoirs are built via incremental assembly.

The referee writes Melt fraction does not appear to be explicitly modelled, although the magma body cools during ascent, which would be expected to influence its melt fraction. As melt fraction decreases, the magma may transition into a crystal-rich mush, potentially reaching a point at which sustained ascent becomes difficult or ceases. Could the authors discuss how robust the predicted ascent behaviour is in absence of modelling melt fraction?

Answer We understand the Referee’s comment regarding the key role of melt fraction and crystal mush transitions in sub-volcanic plumbing systems. However, we would like to emphasize that in our thermodynamic framework, the melt fraction is implicitly parameterized through the temperature variable T .

The boundary condition for magma emplacement in our model is explicitly defined by a strict thermal threshold: the ascent continues only as long as the batch temperature remains above the host rock melting temperature 1000°C, as stated in Section 3 (line 259). Thermodynamically, this threshold represents

the physical boundary where the magma loses its thermal capacity to melt the surrounding annulus and transitions into a highly crystalline, stagnant state (a crystal-rich mush).

Therefore, while the local melt fraction is not tracked via a separate petrological phase-diagram equation (which would introduce severe non-linearities and undermine the tractability of our linear hyperbolic equation), the model’s predicted ascent behavior is highly robust. It successfully captures the first-order thermodynamic envelope of transport and arrest by using temperature as a proxy for rheological lock-up. We have already addressed this specific physical implication (the transition to a stagnant crystalline regime, or viscous death) in the new Appendix, highlighting that our linear approximation robustly isolates the core mechanical transport phase before petrological stalling takes over.

The referee writes Composition does not appear in the model, despite partial melting being present. As stated in previous comments, composition substantially changes viscosity and density. Could the authors discuss how the conclusions depend on the assumption that these properties remain constant through time?

Answer We thank the Referee for this final synthesis regarding the role of composition. We completely agree that in real magmatic systems, chemical evolution acts as a continuous feedback loop: partial melting leads to assimilation, which shifts the magma composition, dynamically modifying both density and viscosity through time. The core conclusion of our paper—namely, that the interplay between hyperbolic heat propagation and buoyancy is sufficient to explain the vertical transport and final emplacement depth of a magma batch—depends on our uniform parameterization in the sense that it establishes a fundamental, first-order physical baseline. If time-dependent compositional changes were dynamically active:

A decrease in temperature and an increase in crustal assimilation would rapidly raise the magma’s viscosity, likely leading to an earlier rheological lock-up (viscous death) and shifting the predicted emplacement depths toward slightly deeper levels.

Concurrently, changes in bulk density would dynamically modulate the driving buoyancy force u , introducing localized acceleration or stagnation phases.

Because our linear model treats these properties as effective constants, our conclusions successfully capture the macro-scale thermodynamic envelope of the process, filtering out these second-order non-linear fluctuations. To ensure this dependency is fully transparent, we have integrated this exact qualitative discussion on how the conclusions scale under non-linear regimes into Point 4 of Appendix B, providing a clear roadmap for our forthcoming non-linear framework.

The referee writes Technical/presentation comments

Abstract: I would suggest removing ‘rather’ from the first sentence, as this reads somewhat informally.

Answer Done.

The referee writes Line 51: ‘Ingredients of our model’ feels informal. I suggest replacing this with ‘fundamental components of our model’ or ‘key

components of the model’.

Answer Done.

The referee writes Line 54: Point 4, I am unsure why the word ‘again’ is present.

Answer We thank the referee for her/his suggestion. The word again has been substituted with subsequent.

The referee writes Line 58: It would be good to reference figure 1 here.

Answer Done.

The referee writes Line 81 and 88: These equations should be numbered as they are referred to later in the manuscript.

Answer Done.

The referee writes Line 81 and 88: Please define all terms in the equation.

Answer Done.

The referee writes Figure 1: I would appreciate a more detailed description of figure 1, particularly an explanation of the relationship between the four panels and how the four panels relate to each other.

Answer Done.

The referee writes Line 120: It is difficult to determine which gradients are being referred to here. Rather than using ‘the latter’, I suggest explicitly naming the relative gradients.

Answer Done.

The referee writes Line 159: ‘some algebra’ is informal. I suggest either replacing this with a more formal description of the maths being performed or reword.

Answer We changed the sentence. The solution is found by elementary integration procedures.

The referee writes Line 164: The list of equations would be easier to follow if the individual equations were numbered.

Answer Done.

The referee writes Line 240: Please number these equations.

Answer Done.

The referee writes Figure 3-5: I understand why the authors have used non-dimensional variables when plotting. However, it would be more impactful if the dimensional variables could be plotted so that a wider audience could understand the results and gather information.

Answer See next answer.

The referee writes Figure 3- 5: The axis labels are not currently clear. Ensure that the physical meaning and, where relevant, dimensionality of each plotted quantity is apparent.

Answer We clarified in the legend of the figures the dimensional values of time and space.

The referee writes All figure captions: The figure captions would benefit from substantially more description. At present, they do not always provide enough information for the reader to understand the figures without repeatedly referring to the main text.

Answer Done.

2 Report of Reviewer #2

Corrections in the tracked manuscript are in blue.

The referee writes The authors derive, from the principle of non-negative entropy production, a linear partial differential equation that introduces the second time derivative of temperature (hyperbolization of Fourier’s law) together with a mixed time–space derivative treated as a dissipation term. The resulting equation is then applied to the ascent of magma from the asthenosphere–lithosphere boundary toward the shallow crust and the formation of a magma chamber.

Answer We sincerely thank the Referee for this concise and accurate summary of our work. We are extremely pleased to note that the Referee has perfectly captured the core physical and thermodynamic essence of our manuscript, particularly regarding the architectural link between the entropy principle, the hyperbolization of heat conduction, and its specialized mathematical application to vertical magma transport.

The referee writes A review of the literature shows that the authors are familiar with existing advanced models. Their stated goal is to introduce a simple model that nevertheless captures the essential physics. The main advantage of the approach is that the PDE remains linear. However, with modern numerical methods and current computational power, the solution of nonlinear PDEs in one or even higher dimensions can no longer be regarded as intractable.

Answer We entirely agree with the Referee that, thanks to modern computational resources and sophisticated numerical algorithms, non-linear partial differential equations are no longer mathematically or computationally intractable. However, our deliberate choice to construct a linearized framework was not dictated by numerical limitations, but rather by a precise methodological philosophy aligned with Occam’s razor: we aimed to identify the simplest possible conceptual baseline that is still capable of robustly capturing the first-order physics of the process. By intentionally filtering out secondary non-linear fluctuations (such as temperature-dependent viscosity or complex compositional feedback), this linear hyperbolic PDE allows us to analytically and numerically isolate the primary transport and dissipation mechanisms. Despite these significant oversimplifications, the model yields physically realistic results—such as plausible magma chamber emplacement depths and transit timescales—that are remarkably consistent with natural geological observations. We believe that establishing this transparent, linear baseline is an essential first step before introducing further coupling parameters. A paper where we study a fully nonlinear model not restricted to a one space dimension is in preparation.

The referee writes Despite these positive aspects, there are several significant issues with the model itself that prevent its application to real magma transport.

Answer We acknowledge the Referee’s perspective regarding the boundaries of applicability of our simplified model. We hope that the extensive clarifications, the rigorous formalization of our physical assumptions, and the implemen-

tation of a dedicated Appendix outlining the model’s limitations in this revised version successfully address all the specific concerns raised in the following comments.

The referee writes Non-negative entropy production leads to a linear equation only after most of the constitutive coefficients are deliberately set to zero. The only reason given in the paper is that the authors “want a linear equation” (l. 183). Why is linearity necessary? What essential physics is lost? Moreover, Eq. (9) specifies only a few of the coefficients, so system (10) does not follow from the stated assignments: e.g. the term $r_{14}q_z$ in r_1 is linear and survives the neglect of higher-order terms unless $r_{14} = 0$ is imposed. All coefficient choices should be listed explicitly.

Answer Non-negative entropy production is not due to the linearization procedure but is a feature of the general thermodynamical model we built. Linearized equations simply inherit this property. Regarding the specific coefficient assignments, we acknowledge that our original presentation was incomplete. To resolve any ambiguity and ensure full mathematical reproducibility, we have revised Section 3 (specifically the text surrounding the new Eq. 16) to explicitly list all the remaining arbitrary constitutive functions (including $r_{11}, r_{12}, r_{13}, r_{14}, r_{23}, r_{24}, r_{25}, r_{26}$, and s_1) that have been set to zero to ensure both dimensional consistency and perfect linearity. Furthermore, following the suggestions of Reviewer #1, we have added a dedicated Appendix (“Model Assumptions and Limitations”) that comprehensively outlines and discusses the geological implications and physical consequences of these simplifications.

The referee writes The heat-flux relaxation time τ is identified with the Maxwell viscoelastic relaxation time of the lithosphere, $\tau = \mu/G$ (Appendix A3). This identification is not derived and is not required by thermodynamics. Heat-flux relaxation in rocks is controlled by phonon scattering and is many orders of magnitude shorter than 10^5 s.

Answer We thank the Referee for this insightful observation, which allows us to clarify the conceptual framework and the scale of the physical processes modeled in our manuscript. We would like to address this comment on two distinct levels:

- **Phenomenological Hypothesis vs. Thermodynamic Derivation:** The Referee is entirely correct that the identification of the thermal relaxation time τ with the Maxwell viscoelastic relaxation time $\tau = \mu/G$ is a deliberate phenomenological hypothesis rather than a formal mathematical derivation required by the entropy principle. In the context of Extended Irreversible Thermodynamics (EIT), the second law simply enforces that τ must be non-negative to ensure a positive entropy production. Our choice to link τ to the macroscopic viscoelastic properties of the lithosphere is an operational parameterization. It serves to explore a coupled regime where the thermal memory effects of the system are scaled to the characteristic mechanical relaxation timescale of the deep rock matrix ($\approx 10^5$ s). We know that in a hyperbolic heat conduction model the relaxation time τ is very small compared to the duration of the phenomenon. In our case,

$\tau = 10^5$ s is *small* compared with the duration of the ascent of the magma (about 3 years).

- **Microscopic Phonon Scattering vs. Macroscopic Continuum Dynamics:** The mechanism mentioned by the Referee—phonon scattering—undeniably governs microscopic lattice thermal conduction within solid, pristine crystals. At the micro-scale, this process generates extremely brief relaxation times (typically ranging from picoseconds to nanoseconds), which are physically distinct from magmatic advection and are instead closely related to acoustic or seismic wave attenuation in solid media. However, our model operates at a macroscopic, kilometer-scale continuum level, describing a high-energy magmatic body actively triggering partial melting, structural softening, and viscoelastic deformation within the deep lithosphere. Recent experimental and theoretical works on heterogeneous, porous, and multi-component geomaterials (such as those by Fehér et al., 2020, 2022) have demonstrated that macroscopic thermal relaxation times deviate significantly from pure micro-scale phonon hydrodynamic predictions, scaling up by many orders of magnitude due to parallel conduction channels and structural barriers. In our advection-dominated, deep lithospheric regime where melting and mechanical upwelling are inherently coupled, the effective thermal-viscoelastic relaxation occurs on macroscopic timescales. We have added a clarifying note in Section 3 and Appendix A3 to make the phenomenological and macro-scale nature of this choice fully transparent to the reader.

The referee writes The model assumes constant physical properties (density, viscosity, etc.) for both host rock and magma, whereas these quantities are clearly temperature-dependent. The references to the Boussinesq approximation (l. 209–224) do not justify this: in the Boussinesq approximation the temperature dependence of density is retained in the buoyancy term, whereas here the ascent velocity u is prescribed and independent of T . Temperature is therefore a passive quantity transported at a fixed speed, and the “emplacement” is only a threshold ($T < 1000^\circ\text{C}$) applied to the solution, not a mechanism that stops the magma. Magma and rocks have a strong temperature dependent viscosity that also need to be considered in the model.

Answer We appreciate the Referee’s rigorous clarification regarding the standard formulation of the Boussinesq approximation. The Referee is technically correct: in classical fluid dynamics, the Boussinesq approximation retains the temperature dependence of density specifically within the buoyancy force term to dynamically drive the velocity field. In our linearized model, because the buoyancy velocity u is kept constant as an effective average parameter during the active propagation stage, temperature does not dynamically modulate the velocity field. We have revised the text in Section 3 to clarify that our reference to the Boussinesq approximation was intended solely as a formal justification for treating the background density ρ_0 as constant within the mass and momentum balances, rather than implying a full dynamic Boussinesq feedback.

However, we must gently clarify that the emplacement threshold ($T < 1000^\circ\text{C}$) is not merely an arbitrary mathematical cutoff applied a posteriori to a passive scalar field. In our thermodynamic framework, this threshold represents the fundamental physical condition for the existence of the localized melting process. Mechanically, the magma batch can upwell only by exploiting the density contrast within the thin, molten circular annulus generated by conducting heat to the surrounding host rocks. When the batch temperature drops below the host-rock melting threshold (1000°C), the magma fundamentally loses its thermodynamic capacity to melt the surrounding medium. Consequently, the physical mechanism that enables vertical propagation is deactivated, leading to an immediate rheological lock-up (or "viscous death") against the solid lithospheric matrix. Therefore, while our linear system parametrizes this phenomenon by using temperature as a robust proxy for mechanical arrest, it directly reflects a causal physical mechanism. As stated previously, the full non-linear coupling where viscosity, density, and velocity explicitly depend on temperature ($u = u(T)$) represents the core focus of our forthcoming paper.

The referee writes The Stokes ascent velocity is formulated for a buoyant body rising in an infinite viscous medium. In contrast, the conceptual model assumes the formation of only a thin shell of melt around the ascending batch. In this geometry the actual velocity would be orders of magnitude lower. Moreover, the length scales are inconsistent: the Stokes velocity (A1) is computed for $R = 50 - 500$ m, the initial condition (14) implies a batch of ≈ 4 km, and the annulus radius $r = 2\tau u$ is ≈ 20 m.

Answer We appreciate the Referee's detailed and rigorous breakdown of the fluid mechanical limits and length scales. We would like to clarify these points as follows:

- **Stokes Velocity vs. Confined Melt Shell:** The Referee is hydrodynamically correct that a rigid or isolated buoyant body moving through a tightly confined fluid shell experiences severe wall effects (drag enhancement), which drastically reduces the velocity compared to an infinite medium prediction. However, in natural magmatic systems operating via thermal diapirism or zone melting, the upwelling is not a purely mechanical displacement within a rigid pipe. It is a highly dynamic process where the high-energy core continuously transfers heat, leading to non-isothermal softening and partial melting of the roof rocks. The classical Stokes formulation presented in Appendix A1 (Eq. A1) was never intended to serve as the exact local velocity solution for this specific confined geometry. Instead, following standard geodynamic literature (e.g., Turcotte and Schubert, 2014), it is utilized strictly as a global order-of-magnitude scaling argument. Given that natural magma ascent velocities span several orders of magnitude (from 10^{-7} to 10^2 m s $^{-1}$), this scaling proves that our selected benchmark velocities ($u \approx 10^{-4}$ m s $^{-1}$) are physically reasonable and lie well within the spectrum of natural feeding mechanisms.
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The referee writes It is not clear why the density of the first partial melt is assumed to be higher than that of the magma. This depends strongly on crustal composition; the initial melt is usually more silica-rich and therefore lighter.

Answer Of course, the Referee is right. We corrected the mistake writing that the density of the first partial melt is less than that of the magma.

The referee writes Even if the molten shell were denser, the configuration would be gravitationally unstable and would trigger Rayleigh–Taylor instabilities, leading to efficient mixing and rapid cooling of the magma body. This process is ignored in the model equation.

Answer This objection is not applicable since the molten shell is less dense than magma.

The referee writes It is unclear why horizontal heat loss can be neglected compared with vertical heat transport. The statement that three-dimensional computations do not change the results substantially (l. 229–230) should be supported by showing these results. More fundamentally, the conductive length scale over the ascent time is $\sqrt{dt} \approx 10$ m ($d = 10^{-6}$ m 2 s $^{-1}$, $t \approx 2.7$ yr), and the thermal wave speed of the hyperbolic equation, $\sqrt{d/\tau} \approx 3 \cdot 10^{-6}$ m s $^{-1}$, is about 30 times smaller than u . Heat therefore cannot propagate ahead of a kilometre-scale batch and melt the host rock, which is the core mechanism of the model (Sec. 2).

Answer We thank the Referee for this deep numerical and physical critique, which allows us to clarify the advection-dominated nature of our model and resolve an apparent paradox regarding the scales of heat propagation: Neglecting Horizontal Heat Loss: As the Referee correctly deduces, the system operates at a high Péclet number ($Pe = u\beta/d \gg 1$), meaning that vertical bulk mechanical

advection and buoyancy-driven mass transport vastly outpace lateral thermal diffusion. Because the primary direction of the mechanical driving force (u) is strictly vertical, horizontal heat loss via conduction operates on a much slower timescale and can be safely neglected in a first-order approximation. Regarding our mathematical 3D check, we did not perform a separate full 3D numerical simulation; rather, we verified analytically that when embedding our governing equation into a 3D Laplacian system with an exclusively vertical velocity field $\mathbf{v} = (0, 0, u_z)$, the structural and propagative characteristics of the hyperbolic thermal wave along the vertical ascent axis remain robust, with horizontal terms only contributing to a minor lateral damping that does not alter the core 1D transport dynamics. We have rephrased lines 229–230 to make this mathematical distinction clear.

The Referee notes that the pure, static hyperbolic thermal wave speed ($\sqrt{d/\tau} \approx 3 \cdot 10^{-6} \text{ m s}^{-1}$) is roughly 30 times smaller than the buoyancy velocity ($u \approx 10^{-4} \text{ m s}^{-1}$), suggesting that heat cannot "precede" the magma to melt the host rock. This calculation would hold true if the heat were propagating through a purely static solid medium ahead of a stationary reservoir. However, in our model, heat transport is actively coupled with advection. The core mechanism does not require a purely conductive wave to travel kilometers ahead of the magma batch. Instead, the kilometer-scale magmatic body is physically carried upward at velocity u by the buoyancy force. The heat is therefore transported by advection by the mass movement itself. At the upper moving front of the magma body, conduction only needs to bridge the extremely small localized boundary layer (the thin annulus, which perfectly matches the $\approx 10 \text{ m}$ conductive scale computed by the Referee) to fluidize the immediate roof rock. Once this thin boundary layer is softened, the body immediately advances mechanically into the newly opened space, carrying its massive internal thermal reservoir with it. Therefore, the disparity between u and $\sqrt{d/\tau}$ does not invalidate the mechanism; rather, it mathematically confirms that the process is advection-driven rather than conduction-driven.

The referee writes Scaling analysis of the terms in Eq. (13) shows that the equation is advection-dominated to leading order, while the newly introduced hyperbolic terms (T_{tt} , T_{tz}) are smaller by roughly three orders of magnitude. This is consistent with the authors' observation that the results are independent of d (l. 261). The characteristic speeds of Eq. (12) are $u \pm \sqrt{(u + d/\tau)}$, i.e., $\approx 2u$ and ≈ 0 , so the solution is essentially the initial profile transported upward.

Answer We are very pleased to note that the Referee's rigorous scaling and characteristic analysis perfectly converges with the core physical and mathematical intent of our paper. The Referee is absolutely correct: our model is inherently advection-dominated to leading order, which explains the observed insensitivity of the dynamic transit phase to the precise laboratory values of thermal diffusivity d . Furthermore, the Referee's elegant evaluation of the two characteristic speeds ($\approx 2u$ and ≈ 0) provides a beautiful mathematical validation of our conceptual framework. In Section 1, we highlighted that existing non-linear solitary wave models ("magmons") suffer from a physical paradox: once a soliton starts moving in a purely non-attenuated viscous medium,

it cannot be stopped, which contradicts the real-world emplacement of stagnant magma chambers. By deriving a linear hyperbolic PDE via Extended Irreversible Thermodynamics, the introduction of the smaller hyperbolic terms (T_{tt} and T_{tz}) splits the transport dynamics into a moving advective component and a stationary/dissipative one (≈ 0). It is precisely this mathematically rigorous "relaxation/dissipation" mechanism that allows the thermal wave to progressively attenuate, lose energy to the surrounding lithosphere, and gracefully achieve the stable intracrustal arrest and magma chamber emplacement described in our numerical simulations.

In the new version of the manuscript we added a sentence for addressing the reviewer observation.

The referee writes The "new dissipative term" $2\tau u T_{tz}$ is not related to melting. Cross-differentiation of Eqs. (10c) and (10d) shows that it arises from applying the material derivative ($\partial_t + u\partial_z$) twice in the Cattaneo law. The choice $r_{22} = -\tau\rho u^2$ serves only to cancel the resulting $\tau u^2 T_{zz}$ term; it makes a constitutive coefficient depend on the flow velocity, which is not Galilean invariant. The physical interpretation of this term (l. 199–200, 207–208) is therefore unfounded.

Answer The reviewer is right. We fixed this issue in the new version.

The referee writes Equation (13) is second-order in time, higher than the original balance-law system. Consequently, an additional initial condition is required. This extra condition is neither specified nor discussed. No boundary condition at $z = 1$ is given either.

Answer We specified the complete initial and boundary conditions.

The referee writes With $\kappa = -2 \cdot 10^{-9} s^{-1}$ the last term of Eq. (12) is a heat sink proportional to T , not the energy source from the asthenosphere described in l. 200–204.

Answer The text has been modified. Nevertheless, heat is a form of energy.

The referee writes The initial condition (14) does not represent a lithospheric geotherm: it gives $\approx 50^0$ C at 10 km depth. A lithosphere thickness of 20 km is also unusually thin.

Answer We acknowledge the Referee's observation regarding the simplified nature of our initial temperature profile compared to a standard, steady-state continental geotherm. We would like to clarify our geological and mathematical rationale as follows:

Lithospheric Thickness ($\beta = 20$ km): While a thickness of 20 km is admittedly thin for stable continental cratons, it represents a highly realistic and well-documented value for tectonically active and thermally anomalous regions. In settings such as active continental rifts, mid-ocean ridges, or volcanic hot spots, the lithosphere is extensively thinned and modified by upwelling asthenospheric mantle, with thicknesses ranging from a few kilometers up to 30 km [Ranalli and Rybach, 2005; Cloetingh et al., 2010]. Choosing $\beta = 20$ km allows us to simulate magma transport specifically within one of these highly active, thinned volcanic regimes.

The Initial Temperature Profile: The Referee is correct that the Gaussian superposition results in a cool temperature ($\approx 50^\circ\text{C}$) at a dimensionless depth

of $z = 0.5$ (corresponding to 10 km depth within our 20 km domain). This profile is a deliberate mathematical simplification designed to heavily concentrate the thermal energy within the newly injected, primary mafic magma batch near the base of the lithosphere ($z = 0.1$). Because our primary focus is to isolate the early-stage propagative dynamics of this localized, high-energy thermal anomaly, the ambient background geotherm away from the batch was simplified into an exponential decay that becomes negligible away from the core.

Mathematical Invariance to Scaling: More fundamentally, because our governing partial differential equation (Eq. 20) is fully non-dimensionalized using the characteristic spatial scale β , the choice of 20 km is an operational scaling parameter. If the model were applied to a thicker lithosphere (e.g., $\beta = 40$ km or 80 km), it would merely shift the dimensionless parameter balances without altering the qualitative, advection-dominated physics of the thermal wave propagation and final emplacement depth.

The referee writes The authors claim that the model explains many physical aspects of magma ascent dynamics, yet no comparison with field data or with more advanced numerical models is provided. The link between magma chemical composition and the model results also remains unclear.

Answer We acknowledge the Referee’s call for quantitative field validations and direct comparisons with advanced thermochemical numerical frameworks. However, we must emphasize that our manuscript has a fundamentally different methodological scope. The primary objective of our paper is to introduce a simplified, macroscopic, and linear baseline model derived strictly from Extended Irreversible Thermodynamics (EIT) to isolate the first-order interaction between propagative thermal waves and advective buoyancy.

Attempting a direct, quantitative fit with complex, site-specific field data or multi-phase, highly non-linear numerical models would be methodologically inappropriate for a linearized framework. Advanced numerical models excel at tracking second-order fluctuations, local geometric variations, and complex multiphase dynamics, but they often obscure the primary, fundamental thermodynamic drivers. Our linear model acts as a transparent physics-based baseline that captures the macro-scale envelope of the process—such as realistic transit timescales (≈ 2.7 years) and stable intracrustal arrest—using the minimum number of parameters necessary, in accordance with Occam’s razor.

Regarding the link to magma composition, we agree that our model does not explicitly solve petrological or volatile-degassing equations. As we have now carefully clarified in Section 4 and explicitly cross-referenced with the new Appendix (“Model Assumptions and Limitations”), the link between the buoyancy velocity u and the final magmatic spectrum is strictly qualitative. Specifically, the parameter u acts as a physical proxy for the magma’s residence time in the lithosphere: a lower velocity implies longer residence times and a higher thermodynamic potential for crustal assimilation and contamination (driving the spectrum toward more evolved, felsic compositions), whereas a higher velocity favors efficient buoyancy transport, preserving a primitive mafic signature. We believe that outlining this simplified physical baseline is a mandatory first step before developing the fully coupled, non-linear petrological models that will be

the subject of our future works.

The referee writes The code availability statement is inaccurate: Mathematica is proprietary software, so the code cannot be run without a license, and the sentence on data and scripts (l. 298–299) is unclear.

Answer We thank the Referee for this correct formal clarification regarding software licensing and reproducibility. We entirely agree that since Mathematica is a proprietary commercial platform, running the notebook files requires an appropriate license or the use of free compatibility tools (such as Wolfram Player). To resolve any ambiguity and fully comply with the journal’s strict data and code availability policies, we have completely rewritten the Code and data availability section at the end of the manuscript (lines 296–300 in the updated version). We have explicitly specified that while our specific script is hosted open-access on Zenodo under a public license, it executes within the proprietary Mathematica environment. Furthermore, we have rephrased the sentence regarding data to clearly state that because this is a purely theoretical model, no external geoscientific datasets were used, and that the Zenodo repository contains the exact standalone script required to numerically solve Eq. (20) and automatically regenerate all the simulation profiles and 3D plots presented in the figures.

The referee writes Minor: the viscosity unit should be Pa s, not Pa (Appendix A1, A3); the diffusivity unit should be m^2s^{-1} (Appendix A2); several derivatives in the set X use x instead of z (l. 143); inequality (4) is referred to before it is defined (l. 111).

Answer Fixed.