Reviewer comments in black, *authors' answer in blue italic*, authors' changes in blue.

We thank the two reviewers for their comments and suggestions, which we incorporated, in the revised version of our manuscript. We also clarified and corrected the manuscript where necessary and added a figure (new figure 12).

Reviewer 1: My compliments. This is a well written article. The authors took all possible scenarios, factors and variations into consideration and explained everything accordingly.

Technical corrections

Line 566= Banden = Banded

Line 733= palce= place

*We sincerely thank the reviewer for their kind words and positive evaluation of our work. We have corrected the two typographical errors :"*Banded*" line 566 (new line 576) and "*place*" line 733 (new line 759) as suggested.*

Reviewer 2: Review of *A numerical model for duricrust formation by laterisation* by Fenske et al.

This manuscript presents a new numerical model, developed to explore duricrust formation through in-situ laterization in concert with regolith formation and landscape evolution. The paper clearly outlines the details and the rationale behind the model and presents an extensive suite of modeling experiments to explore the model in detail. Overall, this is a well-written and well-illustrated manuscript.

This development of a new numerical tools to simulate duricrust formation under different assumptions and in concert with landscape evolution, represents a solid scientific contribution to our understanding of duricrust formation and their influence on topographic steady-state and long-term landscape evolution. Particularly in combination with the previous work of the authors (Fenske et al., 2025), I believe this paper will be of interest to the wider geomorphological community. Future work will allow the model to be applied to specific sites with observations.

We are pleased that the reviewer finds the manuscript well written, well illustrated, and a valuable scientific contribution to understanding duricrust formation and its implications for landscape evolution. We also appreciate the encouraging remarks regarding the connection with our previous work (Fenske et al., 2025).

In some cases, for instance in the transient model simulations, I would have found it useful with a more direct comparison with a scenario without hardening (e.g., fig. 11), to understand how the topographic evolution is different when introducing laterization and duricrust formation/hardening. This could simply be exemplified with curves of the transient evolution e.g. of the hilltop with and without the various components of the LAT model (e.g., excluding the hardening feedback on erosion and/or mass loss).

Comparison with non-hardening scenarios (e.g., Fig. 11)

Thank you for this suggestion. We agree that a direct comparison with a scenario excluding hardening would clarify the influence of duricrust formation on topographic evolution. A new figure has been added (new fig. 12), showcasing a scenario having the exact same conditions as in fig.11, without the hardening component, by increasing the order of magnitude of tau to simulate infinite formation time. The corresponding new paragraph has been added (new lines: 464 – 470):

The same simulation, using the percolation mode, was performed with a set tau = 1.2e8 yrs to generate a landscape without duricrust formation. In Figure 12, we compare the resulting topographies of both scenarios to illustrate the influence of duricrusts on the regolith column. During each uplift cycle, the duricrusted topography attains higher elevations than the regolith-only topography, although the difference in height remains minimal. The quiescent period induces more intense erosion on the regolith-only hill, while duricrust formation occurs on the other, and the resulting topography withstands erosion. We see that the topography difference between both scenarios is at its highest during that part of the simulation. These results show that duricrusts provide a degree of protection against erosion, although to a limited extent.

Although the approach is fitting, the introduction to the computation of ages in section 3.2 could be clearer (lines 484-486 could be moved to section 3.2). Perhaps the section could also benefit from an illustrative figure showcasing an example; illustrating also what a PDF mean age represents compared to the chosen threshold hardening value (i.e., when something is defined as duricrust in the model) that may be reached at a different time than the mean from the PDF of ages (which would be fine).

Clarification of age computation (Section 3.2)

We appreciate this insightful comment. We clarified the introduction to the computation of ages by moving lines 484–486 to Section 3.2, as suggested, and introduced the overall computation more effectively. Additionally, we rewrote the section to better illustrate how a mean PDF age compares to the threshold hardening value and how these definitions relate to when a material becomes classified as duricrust in the model. A new figure is not added to the section as figure S18 in the supplementary material alongside figure 16 (section 4.9) already illustrate the method, and adding a similar figure here would make it redundant.

Added text:

[…] "Examples of predicted PDFs or age distributions are shown in Supplementary material Figure S18. […] *(new lines: 304-305)*

[…] "Our choice of defining the hardening or laterisation age as the mean of a distribution of ages is driven by the need to compare our results to observational constraints that, as we explained above, consist, for the most, in age distributions. Another option would be to compute the time at which the critical hardening value for duricrust formation, kappa_c, has been reached. Although simpler to compute, such a hardening age would be more difficult to compare to observations." […] *(new lines 311-314)*

More specific comments:

- Is section 1.6 really needed, or could it be included in section 1.2 or elsewhere? *Section 1.6 has been moved to 1.2 (new lines: 84 to 86).*
- Figure 2. I am confused about the red arrows in *b* linked to 'mineral transport'. There is no transport of minerals within the bedrock as I understand, but advection of fresh material with the uplift. This could simply be mentioned in the text (as it already is). If keeping the arrows, they should have the same length/velocity as the uplift arrows. *Corrected. We deleted the red arrows in figure 2 to avoid confusion as the explanation is in the text as noted by the reviewer.*
- Line 144: I would suggest '*in* the subsurface'. *Corrected*
- Figure 3 caption. I assume the brick red line represents the surface topography and not the rate. *Clarified the caption.*

"[…] the water table geometry (in blue), and the topography (in brick red), after 10 […]"

- Lines 202-206: perhaps you want to motivate more explicitly here already (or earlier) why you explain the details of this other model (you do it later, lines 212-214). *Lines 202 – 2026 moved to new lines 207 – 211 to clarify.*
- Line 215. 'introducing a second hardening equation'; this may be misunderstood as if the first one is also in the model implementation. Consider using 'an alternative hardening equation'. *Corrected.*
- Equation 7 vs. 8; what is the reason for introducing the alternative nomenclature dy' ? *Thank you for pointing out this inconsistency, we fixed equations 6 and 7. In all 3 equations, dy' refers to the variable of integration, whereas y on top of the integration symbol refers to the integral upper bound. We cannot use the same symbol for both.*
- Line 300: 'the ages are produces', perhaps consider formulating this as the production of the elements that can be dated. *Corrected formulation (new line: 299).*
- Line 367 and elsewhere, would it be more appropriate to use K_{D0} instead of K_D, according to equation 9? Also in line 390, you use K_d (small d). *K_d is corrected. For the use of K_D instead of K_{D0}, we describe in line 367 and others the whole evolution of the K_D parameter through time, i.e. all K_D which sprouted from the initial K_{D0}. Thus, only mentioning K_{D0} would be detrimental to understanding the continuous evolution of the whole K_D paramater.*

- Line 395. This is the first time surface sedimentation is mentioned. I would recommend mentioning this already when the model is presented, that your model includes sedimentation in addition to surface erosion. This was not clear until this point. *The model's topography mainly evolves through the component K_D, the surface transport coefficient (see section 2.1). This means that we mainly implement erosion, while sedimentation is not explicitely included in the model. We mention sedimentation in a broader way, as to acknowledge that sedimentation might modify aspects of the regolith column even though this is not something our model can show. We clarified this (new lines 402 – 404):*

[…] above the water table. In natural settings, strong mass loss could eventually lead to burial of the duricrust by surface sedimentation, although this process is not represented in our model. […]

- Lines 408-410: repetition, consider leaving out. *Deleted*.
- Equation 17, should the precipitation fraction not be the other way around, to be consistent with equations 15 and 16? *We agree, we thank the reviewer for this. It is corrected.*
- Figure, 15, 16, and 17, etc. The gray regions as well as the uplift rate dashed line in the right two panels are difficult to read, in terms of the relation to depth/distance axes. Perhaps add more explanation or additional axis on the panels. In addition, the white lines on the left panels are not mentioned in the caption. *The figures 15, 16, 17, 22 and 24 were modified for clarity. The dashed lines for the uplift and precipitation rates are now more prominent, while the shaded area for the distribution is now beige to add contrast. New axis for the uplift and precipitation rates have also been added (new figures 16, 17, 18, 23 and 25).*
- Line 567: using 'utilizing the duricrust formation model in concert with data' instead of 'implementing […]' may be more appropriate here. *Corrected (new line: 577).*
- Line 660-661: what is relatively well constrained? *Clafiried the text. "Water table fluctuation ranges are relatively well constrained" (new lines: 670-671)*
- Given the points in the discussion related to steady state (or not), I find it would be useful if it was more explicitly mentioned whether a certain presented model is in steady state or not. *Added a note at Fig.6. Steady state in our models cannot be achieved when duricrusts keep forming (line 661). All equations assume steady-state but can be used to show evolution in transient cases (line 587). When a scenario is considered at steady state or quasi-steady state, it is mentioned in the caption of the figure (e.g. fig. 7, 8, 9, in the text for fig 10.) The other scenarios are all derived from the same reference scenarios, thus considered attaining (quasi-)steady-state too.*

- Lines 674-678: this does not seem to help to distinguish between the two formation mechanisms as said in the beginning of the paragraph. Perhaps elaborate further or clarify. The wording 'strongly suggest that the duricrusts formed by WTF' is in strong contrast with the following sentence. *Pragraphs from line 674 to 683 have been rewritten to clarify our claims. (new lines: 684 to 699)*

"Indeed, comparing the ages predicted by the two models, two main aspects can be considered. The first concerns the relationship between the age of the duricrust and that of the surrounding regolith. If older regolith is observed above the duricrust, then this configuration is more consistent with duricrust formation by WTF (as shown in Figure~\ref{models}a). WTF duricrusts typically form in the subsurface, along the water table fluctuation range (Fenske et al. 2025), thus the regolith below and above is unaffected by duricrust formation and evolves independently provided there is no shift in base level. This reasoning has a similar approach to the cross cutting principle where where features that form or cut through an already existing layer is younger than the layer itself.

Conversely, if the duricrust is older than the overlying regolith, it implies that the regolith may consist of transported soils deposited on the duricrust, which formed by LAT (Figure2b). However, it should be noted that removal of the overlying regolith by erosion, might change the age profile of the column, potentially making it more difficult to differentiate between both models. In situations where there is no regolith above the duricrust or erosion reworked the material, age distributions can provide clues. Thus, the second aspect concerns the age distributions. An asymmetrically broadening aging age distribution from the lower layers to the surface of the weathering profile indicates formation by LAT, as it is typically found in nature for comparable weathering profiles (Shuster et al. 2012). In contrast, duricrusts formed by WTF may exhibit age distributions that reflect characteristics of external sources, lacking any trends. Additionally, if there are multiple generations of duricrusts present in a single hill, it is most likely indicative of WTF formation. In contrast, duricrusts formed by LAT tend to develop as a single continuous entity from the underlying weathering profile.

- Line 702: the use of burial here is confusing, if there is no sedimentation. Is it simply lowering relative to base level? If there is mass loss throughout the column, I assume a given part of a vertical profile is getting closer to the surface while the surface is lowered? *Clarified.*

*"*In the LAT/saturated case, the duricrusts experience downward displacement in the regolith column due to the mass loss associated with compaction during duricrust formation. Despite this, sedimentation does not occur due to the large topography that is created during the uplift phases.*" (new lines: 717 – 719)*

- Line 706-707: consider adding a few additional sentences on how isotopic composition could be used to distinguish between processes of formation. *Added (new lines: 723 – 728).*

*"*Isotopic data are particularly revealing as duricrusts formed by hydrological processes do not present any genetic link to the regolith and bedrock below, as they formed from external input of material (Ollier and Galloway 1990, Taylor and Eggleton 2001). In contrast, duricrusts formed by laterisation directly result from bedrock and regolith weathering and are therefore genetically linked (Tardy and Roquin 1992). By investigating the isotopic compositions across the profile, it is possible to assess whether a genetic link between the regolith and duricrusts exists, thus identifying which mechanism was at play.*"*

- Lines 750-754: consider moving these lines to section 5.6. *Moved to section 5.6 (new lines: 745 – 749)*

We also decided to change the Appendix figures to Supplementary material as the manuscript is already very long.

We thank the reviewers once again for their insightful and well-considered comments, which have helped us improve the clarity and quality of the manuscript.