# Review Report for

## Influences of structurally-controlled ridge asymmetry on drainage network and divide dynamics

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# Round 1 Review

## *Comments by reviewer 1 (anonymous)*

Completed: 2024-06-10 04:11 PM
Recommendation: Minor revisions

This study seeks to understand how relief asymmetry arising from geologic structure can control drainage divide dynamics. The paper is well written, easy to understand, and enjoyable to read. The authors focus on the chi metric, which is often interpreted to indicate disequilibrium when values are different across a divide. Using a DEM, the authors extract river networks and divides, then identify by eye the dip direction of homoclinal ridges. Their results show that overall, chi values are lower on the steep cliffs on the side of the divide opposite the direction of dip. They interpret this to represent a structurally-controlled delayed approach to landscape equilibrium. I think the main results of the paper are pretty clear and straightforward, and this new approach of looking at landscape scale asymmetry using the chi metric helps to improve our understanding of larger scale structural controls on landscapes.

**Main comments:**

Though the results shown in figures 4 and 5 are quite scattered, I buy the idea that chi values tend to be more negative. I generally tend to discourage authors from using p values and linear regression for noisy datasets, as it is not typically a meaningful analysis. To me, figures 7 and 8 are more than convincing to see differences in homoclinal ridges. However, these figures need their y axes labelled.

While the use of chi introduces a new way of looking at asymmetry in homoclinal ridges, I personally suspect that hillslope processes are by far the most dominant controls on these types of divides, not fluvial processes. I appreciate that the authors discuss this in section 5.2.3, but I think they could add a bit more of a substantial comment about the expectations for steady state in these types of landscapes in general. Due to the erosion style of resistant rock layers, boulders essentially must be calved off onto the slopes opposite the dip direction, resulting in down dip retreat of the divide and therefore an "aggressor" basin on the steep side (note that this may occur without any fluvial input, but is a result of the geometry of the system and the style of erosion of the resistant layer). Further, Glade and Anderson 2018 explore dip-parallel retreat of hillslopes resulting in a dynamic steady state, rather than a typical vertically oriented steady state assumed in "typical" landscapes. It is likely that the "aggressor" side of the divide is actually simply keeping up with the cliff retreat rate, leading to a dynamic lateral steady state. If the authors could clarify these points in the discussion, then I think this paper is worth of publication.

## *Comments by reviewer 2 (anonymous)*

Completed: 2024-06-28 10:13 PM
Recommendation: Reject with possibility to resubmit

**Summary**

The MS looks at the across-divide contrasts in chi, slope, and relief to detect landscape disequilibrium in asymmetric ridges in the French Southern Alps. The intent is to discuss how thin-skinned thrusts create and maintain these asymmetric ridges by comparing the delta-chi with Gilbert Metrics. The premise of this comparison is that delta-chi should tell if the ridges (divides) do not divide the stream network evenly (sensu Willett et al., 2014). In this scenario (delta-chi ≠ 0), the underlying structure of the ridges would set the network topology. In their study area, this is true more often than not due to a coincidence between low chi values and high slopes. The MS is well written. The figures look good and are of publication quality. Despite the qualities of a publishable paper, the paper has some shortcomings that I outline below.

**Overall recommendation**

I recommend that the authors work on the clarity of the arguments, premises, and assumptions. I think the paper suffers from an improperly framed null hypothesis. This issue needs to be addressed before publication. The paper can be better set based on recent literature that further contextualizes the use of the chi-metric (e.g. Whipple et al., 2017). The approach, while revealing interesting patterns, does not properly support the objective of the paper. I recommend that the authors revise the paper to clearly state the objectives, linking 'divide migration', 'long-wavelength' network geometry, and local topographic across-divide asymmetries in the introduction. I also recommend that the authors include a conceptual model to show how the different river network length scales are supposed to behave in each hypothetical case.

**My thoughts**

Clarity: The MS needs to improve the clarity of its arguments and scope. The title leads me to think that the paper is about how drainage divide migration is a function of the geometry of structures. However, as the manuscript progresses, the paper becomes about how the structures influence the drainage network geometry. These are two different views with different implications. Yet, the discussion of the paper focuses on local mechanisms such as lithological contrasts (which they have provided some data indirectly for), local contrasts in erosional efficiency due to tools-and-cover effects, and hillslope processes which include weathering and soil production dynamics. The latter three are local as opposed to the 'long-wavelength' focus of the paper. In the Conclusion section, the paper states that it is the slow response (but without quantifying it) to the tectonic/structural trigger that allows the asymmetries to persist over time. So, it feels like the paper is trying to create a dialogue with literature from different perspectives but falls short in justifying this need.

Hypotheses: The premise of the work is that delta-chi should equal zero if the divides are stable (the null hypothesis on the t-test applied to the data in Figures 4-6 is delta-chi = 0). However, this is only true for uniform uplift, climate, and geologic conditions. For non-uniform distributions, drainage divides are always positioned near the peak uplift or lowest erosion efficiency (i.e. low K) (e.g. Whipple et al., 2017). In this study, it seems like the divide is underlain by resistant massive limestones. In turn, the limestone is underlain and/or overlain by marly limestones and/or marls, both of which are richer in clay content and therefore less resistant to erosion. If, on the steep slopes, the marls are exposed under the limestone (i.e. at the base of the ridge), this configuration would exacerbate the topographic asymmetry. Added to the overall geometry of the thrusts, these non-uniform conditions, delta-chi can be high even without any divide migration (Whipple et al., 2017). Given that the underlying geology and tectonics are non-uniform, shouldn't the null hypothesis be that delta-chi is non-zero?

Are the thrusts inactive? If not, then the asymmetry would arise from a combination of the thrust dip and its influence on particle motion and mass flux (i.e. horizontal vs vertical advection) (e.g. Willett et al., 2002; Miller and Slingerland, 2006; Whipple and Meade, 2004). In this case, it reinforces my previous question about the null hypothesis.

Approach: Why cut off confluences that are <10km away from the divide? What do those look like in the delta-chi plots? If non-zero delta-chi is the null hypothesis, getting closer and closer to the individual structures should improve the correlation between delta-chi and delta-slopes. For example, setting the base level to the trace of the thrust fault on the steep side and the change in slope on the gentle side should exacerbate the delta-chi and more clearly reveal the structural influence. I know that in lines 172-174 you mention you do that to 'avoid small-scale interactions' and to capture 'long-wavelength' patterns, but the paper's focus is on divide migration associated with local structural control, thus you need to demonstrate that. If the local fluvial system was still influenced by the geometry of the thrust ramps, you should expect a strong relationship between delta-chi and delta-slope. Moving away from the structure only tells you how far away from it that structure might have influenced the drainage network and therefore is a different line of argument with different implications.

Discussion: Discussing the results from a lithologic, geometric, and topographic perspective is necessary and warranted by the types of data used in this paper. However, the discussions involving tools-and-cover and hillslope/weathering processes are disconnected from what the paper can deliver. The paper does not provide data nor discusses other papers from the study area that would favor or oppose across-divide variations in tools-and-cover and hillslope/weathering processes. I understand how those processes can influence the divide asymmetry from a theoretical standpoint, but discussing them from a purely hypothetical perspective without pointing in any particular direction does not seem very productive. My suggestion would be to provide more convincing arguments that these processes are likely to operate in these landscapes – then the discussion would be warranted.

The previous comment also stems from the simplistic conceptual model in which the authors imply that steep slopes with high sediment fluxes would have low weathering fluxes due to a thin soil layer. Knowing that this argument is (1) opposite to what global datasets show – a positive correlation between erosion and weathering (e.g. Anderson et al., 2007) and (2) that the relationship between weathering and physical erosion can take complex patterns (e.g. Ferrier et al., 2017), and further noting that this manuscript presents no data that allows it to speak with this literature or advance the current understanding begs the question if this topic has a place in the discussion.

Conclusion: the authors conclude that, because delta-chi is non-zero, divide migration must be slow and the landscape has not responded to the deformation episodes that formed the asymmetric ridges. Can the authors provide/discuss data (i.e. erosion rates?) to support this claim? Again, I would have thought that due to non-uniform geology and tectonics, this would be the null hypothesis. If you can prove an improvement on the delta-chi, delta-slope relationship if the base levels are set to the limits of each structure, discussing the timescales of landscape response since the ridge-forming episodes would be more appropriate.

Citations – There are many statements made throughout the paper that lack a proper citation. I referred to some in the line-by-line comments, but the authors should revise the entire manuscript.

Long-wavelength organization of river networks – the MS uses this term to describe the signal recorded by the chi-parameter. However, this term is used very vaguely. How long is long-wavelength? The MS limits the chi-analysis to confluences >10km away from the headwaters of the streams, but also tests >20 km. Is that the long-wavelength? How this term is used needs clarification.

Methods – How was slope computed? Did you measure hillslope gradients within a window? Is it the same as the relief? Or, is it the fluvial slope? This was not clear from the description and needs to be in the methods.

**Line by line comments:**
Line 47-48: Incomplete list of references on landscape dynamics and fish evolution.

Line 50: The literature has certainly focused on tectonic and climatic causes, but there is no study that allows the definitive affirmation that they are "the main factors" and that anything else is secondary. No doubt they are the most straightforward ones to mention, but there's more to it than just climate or tectonics.

Line 67: repeating the statement in Line 62. Also, can you be more specific than "structurally controlled"? What do you mean by that? From your statement in line 63, it seems like you are referring to the ramp subparallel to strata as the structural control and also an underlying necessity of the asymmetric feature. However, the asymmetry arises because there is horizontal advection of rock coeval with vertical advection independently of the strata dip, with the resulting particle motion being diagonal to the Earth's surface. This is well-illustrated in modeling papers (Willett et al., 2001; Miller and Slingerland, 2006; Miller et al., 2007).

Line 75: I am not sure that Braun (2018) is an appropriate citation for this statement. Whipple et al., (2017) would be better suited here.

Lines 95-102: Please cite the relevant references about the geology of the area.

Line 114-116: Is this different from Fig 1B?

Line 119: replace “strong” with “first-order”?

Line 138-141: only under uniform lithology and uniform, pure-vertical uplift rates (e.g. Whipple et al., 2017; Forte and Whipple, 2018; Habousha et al., 2023).

Lines 142-150: “These metrics are directly related to the local erosion processes acting near the divide, and are not likely to be influenced by the long-wavelength landscape organization.”

As written, the focus on the chi-metric and the absence of an equivalent explanation of the Gilbert metrics seem to be at odds with the purpose set in the introduction. In line 76, you say: “This study investigates the implications of relief asymmetry, induced by structures and stratigraphy, on drainage divide dynamics.” However, in line 139-150 you outline the reasons why chi gives you long-wavelength patterns and why it is different from the local across-divide relief contrast. So, as written, it seems like you either chose the wrong metric/proxy or made the wrong argument for the proxy you chose. I know you evaluate chi, slope, and relief, so perhaps providing some explanation of the Gilbert Metrics (i.e. why are they useful as predictors of divide mobility) would take care of the imbalance. In addition, it is not clear in the introduction (though it becomes clear later in the MS) that the purpose is to seek evidence for longer-wavelength dynamics. My suggestion is to clearly state what the purposes of the paper are, what each proxy will reveal, and how it connects to the main goal of the paper.

Lines 202-204: This isn't very clear. Tectonically controlled topographic asymmetry arises from spatial differences in the uplift rates, i.e. either an uplift gradient (e.g. Goren et al., 2014; Habousha et al., 2023) or a combination of vertical and horizontal rock advection (e.g. Miller et al., 2007). In both cases, the steeper side develops because of greater mass flux through that side which consequently yields across-divide differences in erosion rates (e.g. Willett, 1999; Willett et al., 2001; Whipple and Meade, 2004; Miller and Slingerland, 2007; Goren et al., 2014).

Line 213: “drains” = drainages?

Line 254-256: but it is random, right? Looking at plots 4-6, there is no statistically significant relationship between the delta-slope and delta-chi. The moving median shows a weak trend at best. What is the slope of a linear regression?

Line 262-263: But you do not show the data for channel pairs with <10 km to confluence. If you test those, what does it look like?

Line 316: Citation?

Lines 318-321: Weathering rates tend to scale with physical erosion rates (see review by Anderson et al, 2007), but can also show more complex relationships (e.g. Ferrier et al., 2017) so this interpretation is highly simplistic and overall lacks theoretical standing. See also Carretier et al (2014).

Lines 352-354: are there blocks in the steep sides of the studied divides?

**Figure comments:**

Figure 1 – please add a global map as an inset in panel A.

Figure 2 – add a box showing where Figure 1 is located. Red lines in Panel A should appear as a legend on the figure instead of a statement in the figure caption.

**References:**

Anderson, S. P., von Blanckenburg, F., & White, A. F. (2007). Physical and chemical controls on the critical zone. Elements, 3(5), 315-319.

Carretier, S., Goddéris, Y., Delannoy, T., & Rouby, D. (2014). Mean bedrock-to-saprolite conversion and erosion rates during mountain growth and decline. Geomorphology, 209, 39-52.

Ferrier, K. L., & West, N. (2017). Responses of chemical erosion rates to transient perturbations in physical erosion rates, and implications for relationships between chemical and physical erosion rates in regolith-mantled hillslopes. Earth and Planetary Science Letters, 474, 447-456.

Forte, A. M., & Whipple, K. X. (2018). Criteria and tools for determining drainage divide stability. Earth and Planetary Science Letters, 493, 102-117.

Goren, L., Willett, S. D., Herman, F., & Braun, J. (2014). Coupled numerical–analytical approach to landscape evolution modeling. Earth Surface Processes and Landforms, 39(4), 522-545.

Habousha, K., Goren, L., Nativ, R., & Gruber, C. (2023). Plan-form evolution of drainage basins in response to tectonic changes: Insights from experimental and numerical landscapes. Journal of Geophysical Research: Earth Surface, 128(3), e2022JF006876.

Miller, S. R., & Slingerland, R. L. (2006). Topographic advection on fault-bend folds: Inheritance of valley positions and the formation of wind gaps. Geology, 34(9), 769-772.

Miller, S. R., Slingerland, R. L., & Kirby, E. (2007). Characteristics of steady state fluvial topography above fault-bend folds. Journal of Geophysical Research: Earth Surface, 112(F4).

Whipple, K. X., Forte, A. M., DiBiase, R. A., Gasparini, N. M., & Ouimet, W. B. (2017). Timescales of landscape response to divide migration and drainage capture: Implications for the role of divide mobility in landscape evolution. Journal of Geophysical Research: Earth Surface, 122(1), 248-273.

Whipple, K. X., & Meade, B. J. (2004). Controls on the strength of coupling among climate, erosion, and deformation in two-sided, frictional orogenic wedges at steady state. Journal of Geophysical Research: Earth Surface, 109(F1).

Willett, S. D., McCoy, S. W., Perron, J. T., Goren, L., & Chen, C. Y. (2014). Dynamic reorganization of river basins. Science, 343(6175), 1248765.

Willett, S. D., Slingerland, R., & Hovius, N. (2001). Uplift, shortening, and steady state topography in active mountain belts. American journal of Science, 301(4-5), 455-485.

## *Recommendation by Associate Editor 2 (anonymous)*

Completed: 2024-07-15 06:55 PM
Recommendation: Major revisions

The manuscript analyses how lithology and geological structures influence catchment asymmetry, using chi metrics to understand divide dynamics based on specific variabilities. It is well written and easy to follow. It is relevant to the journal and, with the suggested modifications, could be considered for publication.

In particular, there are some issues regarding the general hypothesis, the aim and how these are addressed in both the introduction and the discussion. This is an observation made by both Reviewers.

Although the title and abstract clearly state that drainage divide migration is the main focus of the study, the following sections do not reflect this. It is mainly a study of how the geological structures of a homoclinal ridge define the catchment asymmetry. There is no clear link between the objective of the study and the final remarks/conclusions, and how this asymmetry translates into divide migration.

In addition, there are several limitations of the methodology, which are mentioned (section 5.1), but are mostly not taken into account: such as the influence of river incision, glacial erosion (excessive fluvio-glacial undercutting), mass wasting, slope orientation, etc.

I suggest to follow Reviewer 2 in detail and resubmit for another round of review.

## *Authors' responses to review comments*

Completed: 2024-11-20

**Associate Editor**

The manuscript analyses how lithology and geological structures influence catchment asymmetry, using chi metrics to understand divide dynamics based on specific variabilities. It is well written and easy to follow. It is relevant to the journal and, with the suggested modifications, could be considered for publication.

In particular, there are some issues regarding the general hypothesis, the aim and how these are addressed in both the introduction and the discussion. This is an observation made by both Reviewers.

Although the title and abstract clearly state that drainage divide migration is the main focus of the study, the following sections do not reflect this. It is mainly a study of how the geological structures of a homoclinal ridge define the catchment asymmetry. There is no clear link between the objective of the study and the final remarks/conclusions, and how this asymmetry translates into divide migration.

*We modified the title, abstract, introduction and conclusion to better represent the content of our manuscript and our final conclusions, where we propose that structurally-asymmetric ridges impact local drainage dynamics and from there have an imprint on the larger-scale drainage planform organization.*

In addition, there are several limitations of the methodology, which are mentioned (section 5.1), but are mostly not taken into account: such as the influence of river incision, glacial erosion (excessive fluvio-glacial undercutting), mass wasting, slope orientation, etc.

As now better explained in section 5.1, the usual methodological limitations in the computation of the $\chi$ metrics apply, but these do not impact our conclusions. Indeed, these limitations are expected to generate a certain variability in the calculated metrics, but cannot explain the systematic observed negative values in the case of structurally-controlled ridges.

I suggest to follow Reviewer 2 in detail and resubmit for another round of review.

**Reviewer #1**

This study seeks to understand how relief asymmetry arising from geologic structure can control drainage divide dynamics. The paper is well written, easy to understand, and enjoyable to read. The authors focus on the chi metric, which is often interpreted to indicate

disequilibrium when values are different across a divide. Using a DEM, the authors extract river networks and divides, then identify by eye the dip direction of homoclinal ridges. Their results show that overall, chi values are lower on the steep cliffs on the side of the divide opposite the direction of dip. They interpret this to represent a structurally-controlled delayed approach to landscape equilibrium. I think the main results of the paper are pretty clear and straightforward, and this new approach of looking at landscape scale asymmetry using the chi metric helps to improve our understanding of larger scale structural controls on landscapes.
Main comments
Though the results shown in figures 4 and 5 are quite scattered, I buy the idea that chi values tend to be more negative. I generally tend to discourage authors from using p values and linear regression for noisy datasets, as it is not typically a meaningful analysis. To me, figures 7 and 8 are more than convincing to see differences in homoclinal ridges. However, these figures need their y axes labelled.
*On figures 7 and 8, Y-axes are the density estimates, the value of which has no particular signification. A common practice, in order to simplify the plots, is to avoid displaying these axes, which is what we do here. This is now stated in the caption of these figures*

While the use of chi introduces a new way of looking at asymmetry in homoclinal ridges, I personally suspect that hillslope processes are by far the most dominant controls on these types of divides, not fluvial processes. I appreciate that the authors discuss this in section 5.2.3, but I think they could add a bit more of a substantial comment about the expectations for steady state in these types of landscapes in general.
*Indeed, we use the chi metrics to describe and characterize imbalance at the structurally-controlled ridges, in addition to Gilbert metrics. We do agree that hillslope processes are expected to exert the first order control on the morphology of the investigated asymmetric ridges, at least locally, as discussed in the various sub-sections 5.2. The point here is to observe that these local morphological asymmetries, documented by the local Gilbert metrics, inherited from a structurally-controlled configuration, and maintained by local hillslope processes, also imprint the fluvial landscape at a larger scale, as denoted by the contrast in the associated chi metrics. The idea of equilibrium for a fluvial system is a theoretical concept, especially concerning its planform organization. Events of transient mobility of ridgelines are frequent, even in the absence of changes in long-wavelength forcing, and correspond to the internal dynamics of the system and there is forcibly a certain natural variability. From this perspective, it is unsurprising to still observe local differences in $\chi$ metric across ridges in relaxing landscapes such as the one investigated in this study. The key point of our study is that these differences are systematic and show a preferential relationship with local geological structures. All these points are now clarified in section 5.2*

Due to the erosion style of resistant rock layers, boulders essentially must be calved off onto the slopes opposite the dip direction, resulting in down dip retreat of the divide and therefore an "aggressor" basin on the steep side (note that this may occur without any fluvial input, but is a result of the geometry of the system and the style of erosion of the resistant layer). Further, Glade and Anderson 2018 explore dip-parallel retreat of hillslopes resulting in a dynamic steady state, rather than a typical vertically oriented steady state assumed in "typical" landscapes. It is likely that the "aggressor" side of the divide is actually simply keeping up with the cliff retreat rate, leading to a dynamic lateral steady state. If the authors could clarify these points in the discussion, then I think this paper is worth of publication.

*The key point here, as now better explained in Section 5.2.1 of the discussion is that locally delayed and slowed erosion on the steeper updip hillslopes of structurally-controlled asymmetric ridges are needed to explain the persistence of the imbalance long after deformation has ceased, and that these local processes along the ridges are expected to impact the associated larger fluvial network and its planform organization. We have also expanded the discussion to make a more explicit reference to the dynamic equilibrium model of Glade and Anderson and its implication for the persistence of asymmetric morphologies. However, such local dynamic equilibrium, if present, is only local, at the scale of the ridges, and should be distinguished from the larger-scale imbalance in the planform organization of the river network that we point out.*

*There is therefore the need for slower erosion on the steeper side of the ridge. There are several situations where this is possible, as reported in the various sub-sections of section 5.2, in particular with reference to the work by Glade and Anderson. As we also illustrate with the model we explore for hillslope dynamics, it ultimately depends on the relationship between slope and erosion rate. While a monotonously increasing relationship between the two is widely admitted, we show that a surprising prediction of the humped weathering model is that, under certain conditions, weathering rate can decrease with slope. In the absence of massive mass movement, this process could limit the capacity of the aggressor basins to move the divide toward the opposite side. We hope that the new developments provided in the discussion allow clarifying these points.*

**Reviewer #2**

*Summary*

The MS looks at the across-divide contrasts in chi, slope, and relief to detect landscape disequilibrium in asymmetric ridges in the French Southern Alps. The intent is to discuss how thin-skinned thrusts create and maintain these asymmetric ridges by comparing the delta-chi with Gilbert Metrics. The premise of this comparison is that delta-chi should tell if the ridges (divides) do not divide the stream network evenly (sensu Willett et al., 2014). In this scenario (delta-chi̸ = 0), the underlying structure of the ridges would set the network topology. In their study area, this is true more often than not due to a coincidence between low chi values and high slopes. The MS is well written. The figures look good and are of publication quality. Despite the qualities of a publishable paper, the paper has some shortcomings that I outline below.

*Overall recommendation*

I recommend that the authors work on the clarity of the arguments, premises, and assumptions. I think the paper suffers from an improperly framed null hypothesis. This issue needs to be addressed before publication. The paper can be better set based on recent literature that further contextualizes the use of the chi-metric (e.g. Whipple et al., 2017). The approach, while revealing interesting patterns, does not properly support the objective of the paper. I recommend that the authors revise the paper to clearly state the objectives, linking 'divide migration', 'long-wavelength' network geometry, and local topographic across-divide asymmetries in the introduction. I also recommend that the authors include a conceptual model to show how the different river network length scales are supposed to behave in each hypothetical case.

*We have substantially modified the introduction and methodology section of our manuscript and believe that these various points are now clarified. We address all related more detailed questions hereafter*

*My thoughts*

*Clarity* The MS needs to improve the clarity of its arguments and scope. The title leads me to think that the paper is about how drainage divide migration is a function of the geometry of structures. However, as the manuscript progresses, the paper becomes about how the structures influence the drainage network geometry. These are two different views with different implications. Yet, the discussion of the paper focuses on local mechanisms such as lithological contrasts (which they have provided some data indirectly for), local contrasts in erosional efficiency due to tools-and-cover effects, and hillslope processes which include weathering and soil production dynamics. The latter three are local as opposed to the 'long-wavelength' focus of the paper. In the Conclusion section, the paper states that it is the slow response (but without quantifying it) to the tectonic/structural trigger that allows the asymmetries to persist over time. So, it feels like the paper is trying to create a dialogue with literature from different perspectives but falls short in justifying this need.

*We have modified the title of the article and substantially revised the introduction, and we hope these revisions help to clarify the scope and objectives of the paper. In particular, we attempted to clarify the various length scales at play, and their interactions in relation with the problem we want to address, from the local structurally-controlled asymmetric ridges to the larger scale drainage topology. The new model for hillslope erosion which is now included in the discussion explores now in a more robust and quantitative way one of the possible mechanisms for slow erosion of the steeper updip hillslopes we were invoking in the previous version. The term long-wavelength has been replaced with network scale in many instances.*

Hypotheses The premise of the work is that delta-chi should equal zero if the divides are stable (the null hypothesis on the t-test applied to the data in Figures 4-6 is delta-chi = 0). However, this is only true for uniform uplift, climate, and geologic conditions. For non-uniform distributions, drainage divides are always positioned near the peak uplift or lowest erosion efficiency (i.e. low K) (e.g. Whipple et al., 2017).

*This point requires clarification, as we do not formulate the hypotheses this way, and we think the reviewer is misunderstanding the statistical tests performed on figures 4 to 6. The idea is not to check if $\Delta\chi$ should equal zero, but rather if the mean of $\Delta\chi$ distributions is or is not significantly different from zero for specific $\Delta S$ values and with respect to ridge configurations. We obviously expect a certain variability in $\Delta\chi$ distributions in such a complex landscape. The point we specifically look at is rather if these $\Delta\chi$ have no relationship with the geometry of the ridges (aggressor basins are equally likely to be located on either side of the ridges) or if there is a significant "polarity" (aggressor basins more likely to be located on one specific side of these asymmetric ridges). We have attempted to clarify this point in the introduction while setting the premises of the questions addressed in this manuscript, and in the presentation of the results. Additionally, we now further emphasize and recall in the introduction that active tectonics (and uplift) is here to be neglected, the only key parameter is here the initial geometry of this relaxing landscape, which is controlled by inherited structural geometries*

In this study, it seems like the divide is underlain by resistant massive limestones. In turn, the limestone is underlain and/or overlain by marly limestones and/or marls, both of which are richer in clay content and therefore less resistant to erosion. If, on the steep slopes, the marls are exposed under the limestone (i.e. at the base of the ridge), this configuration would exacerbate the topographic asymmetry. Added to the overall geometry of the thrusts, these non-uniform conditions, delta-chi can be high even without any divide migration (Whipple et al., 2017). Given that the underlying geology and tectonics are non-uniform, shouldn't the null hypothesis be that delta-chi is non-zero?

*The area threshold used for the definition of the river networks allows avoiding such near field effects in the computation of $\chi$. Concerning the definition of the null hypothesis, see our response above (Hypotheses). We also recall here that active tectonics are here to be neglected, only the underlying layered geology is to be considered as a local key parameter*

Are the thrusts inactive? If not, then the asymmetry would arise from a combination of the thrust dip and its influence on particle motion and mass flux (i.e. horizontal vs vertical advection) (e.g. Willett et al., 2002; Miller and Slingerland, 2006; Whipple and Meade, 2004). In this case, it reinforces my previous question about the null hypothesis.

*Current deformation is very slow and unresolvable with geodesy. Recent deformation* *is similarly considered to be very slow, and possibly only local. Therefore, one of the key points in studying the specific landscape of the French Southern Alps is that tectonics can be neglected. We consider that the observed asymmetric configurations are inherited from the Eocene and Miocene deformation phases, and are to be considered as the initial structural conditions of a relaxing landscape. We now further clarify this point in the introduction.*

Approach Why cut off confluences that are <10km away from the divide? What do those look like in the delta-chi plots? If non-zero delta-chi is the null hypothesis, getting closer and closer to the individual structures should improve the correlation between delta-chi and delta-slopes. For example, setting the base level to the trace of the thrust fault on the steep side and the change in slope on the gentle side should exacerbate the delta-chi and more clearly reveal the structural influence. I know that in lines 172-174 you mention you do that to 'avoid small-scale interactions' and to capture 'long-wavelength' patterns, but the paper's focus is on divide migration associated with local structural control, thus you need to demonstrate that. If the local fluvial system was still influenced by the geometry of the thrust ramps, you should expect a strong relationship between delta-chi and delta-slope. Moving away from the structure only tells you how far away from it that structure might have influenced the drainage network and therefore is a different line of argument with different implications.
*We select for distance to confluence very early in the process. Not doing so would result in a huge amount of divide segments between adjacent first-order basins dominating the data set. The corresponding basins are draining the same flanks of the ranges, and their divides are often developing perpendicular to the main structures and range crests. These divides are therefore not relevant to our analysis of the cross divide dynamics at the scale of the different ranges. These points would result in a biased tight cluster around zero in the* $\Delta\chi$ *vs* $\Delta S$ *plot We have made that clearer in the method section. In any case, as now better explained in the introduction, we are indeed precisely interested in documenting whether/how inherited and structurally controlled asymmetric ridges - as locally characterized by contrasts in the Gilbert metrics on either side and by the underlying geology - impact and imprint the larger scale fluvial network - as derived through the chi metric*

Discussion : Discussing the results from a lithologic, geometric, and topographic perspective is necessary and warranted by the types of data used in this paper. However, the discussions involving tools-and-cover and hillslope/weathering processes are disconnected from what the paper can deliver. The paper does not provide data nor discusses other papers from the study area that would favor or oppose across-divide variations in tools-and-cover and hillslope/weathering processes. I understand how those processes can influence the divide asymmetry from a theoretical standpoint, but discussing them from a purely hypothetical perspective without pointing in any particular direction does not seem very productive. My suggestion would be to provide more convincing arguments that these processes are likely to

operate in these landscapes – then the discussion would be warranted.
*We think that providing a discussion of the possible processes responsible for the signals we observe is necessary and the precise purpose of such a discussion section, even if some of them might be perceived as more speculative. We try to provide a few more contextual elements and physical analysis that could support the viability and applicability of these processes.*

The previous comment also stems from the simplistic conceptual model in which the authors imply that steep slopes with high sediment fluxes would have low weathering fluxes due to a thin soil layer. Knowing that this argument is (1) opposite to what global datasets show – a positive correlation between erosion and weathering (e.g. Anderson et al., 2007) and (2) that the relationship between weathering and physical erosion can take complex patterns (e.g. Ferrier et al., 2017), and further noting that this manuscript presents no data that allows it to speak with this literature or advance the current understanding begs the question if this topic has a place in the discussion.
*We have significantly developed and further justified this point in the discussion, with the exploration of a simple physical model and a new dedicated figure. These modeling results highlight the existence of the complex hillslope behavior when considering a non-monotonous soil production function, resulting in a decrease of weathering and erosion with increasing gradients, as we were proposing in the initial ms*

Conclusion the authors conclude that, because delta-chi is non-zero, divide migration must be slow and the landscape has not responded to the deformation episodes that formed the asymmetric ridges. Can the authors provide/discuss data (i.e. erosion rates?) to support this claim? Again, I would have thought that due to non-uniform geology and tectonics, this would be the null hypothesis. If you can prove an improvement on the delta-chi, delta-slope relationship if the base levels are set to the limits of each structure, discussing the timescales of landscape response since the ridge-forming episodes would be more appropriate.
*There are a few erosion rates in this setting, and we have added a paragraph discussing their implication in terms of landscape evolution. Again, even if we do not frame it in terms of null hypothesis, we are aware that $\Delta\chi$ is expected to be different from zero in this type of complex setting. The key observation of our study is not that $\Delta\chi$ is different from 0, but that these $\chi$ differences are significantly and systematically oriented. As indicated in our previous answers to the reviewers' comments, We have re-framed various parts of the manuscript to make this fact clearer, including the conclusion and introduction, and we believe that this should be much clearer now*

Citations There are many statements made throughout the paper that lack a proper citation. I referred to some in the line-by-line comments, but the authors should revise the entire manuscript. *Thanks for noting this and the various suggested complementary citations.*

Long-wavelength organization of river networks the MS uses this term to describe the signal recorded by the chi-parameter. However, this term is used very vaguely. How long is long-wavelength? The MS limits the chi-analysis to confluences >10km away from the headwaters of the streams, but also tests >20 km. Is that the long-wavelength? How this term is used needs clarification.
*As now better stated in the introduction and in the methods sections, long-wavelength reports to the large-scale river network as characterized by the chi metrics - by opposition to local parameters at the scale of the investigated ridges. The term network scale is now used preferentially.*

Methods How was slope computed? Did you measure hillslope gradients within a window? Is it the same as the relief? Or, is it the fluvial slope? This was not clear from the description and needs to be in the methods.
*We use the steepest direction slope calculation. This is now indicated in the methods.*

Line by line comments
• Line 47-48: Incomplete list of references on landscape dynamics and fish evolution. *We have added a couple of additional references, but note that the objective here is not to provide an exhaustive review on this topic.*
• Line 50: The literature has certainly focused on tectonic and climatic causes, but there is no study that allows the definitive affirmation that they are “the main factors” and that anything else is secondary. No doubt they are the most straightforward ones to mention, but there’s more to it than just climate or tectonics. *The sentence has been modified to reflect this point. We still consider that climate, tectonics and lithology (which was mentioned in the following sentence) are the key drivers and controls on landscape evolution, but we are not sure what are the others the reviewer seems to be alluding to. Similar to the prior point on Line-47 to 48, our purpose is not to provide a complete review of these topics, and in general we consider the statement that climate and tectonics are key drivers to be not controversial.*
• Line 67: repeating the statement in Line 62. Also, can you be more specific than “structurally controlled”? What do you mean by that? From your statement in line 63, it seems like you are referring to the ramp subparallel to strata as the structural control and also an underlying necessity of the asymmetric feature. However, the asymmetry arises because there is horizontal advection of rock coeval with vertical advection independently of the strata dip, with the resulting particle motion being diagonal to the Earth’s surface. This is well-illustrated in modeling papers (Willett et al., 2001; Miller and Slingerland, 2006; Miller et al., 2007). *We refer here to the (structural) geometry*

*of the layered bedrock, which is to be distinguished from the motion of particles driven by active tectonics. The revised introduction should now read clearer. In the case of the landscape investigated ahere, tectonics can be neglected and only the inherited geometry of the layered sedimentary series is to be considered.*

• Line 75: I am not sure that Braun (2018) is an appropriate citation for this statement. Whipple et al., (2017) would be better suited here. *Reference to Whipple et al. (2017) added, Braun (2018) is still present as we consider it to be a relevant reference, in particular because of the derivation of an elegant formalism for divide migration rate based on parameters characterizing processes efficiency.*

• Lines 95-102: Please cite the relevant references about the geology of the area. *References added.*

• Line 114-116: Is this different from Fig 1B? *Reference to figure 1B added.*

• Line 119: replace “strong” with “first-order”? *Done.*

• Line 138-141: only under uniform lithology and uniform, pure-vertical uplift rates (e.g. Whipple et al., 2017; Forte and Whipple, 2018; Habousha et al., 2023). *OK, the text has been modified here and references added.*

• Lines 142-150: “These metrics are directly related to the local erosion processes acting near the divide, and are not likely to be influenced by the long-wavelength landscape organization.” *The corresponding paragraph has been rewritten.*

As written, the focus on the chi-metric and the absence of an equivalent explanation of the Gilbert metrics seem to be at odds with the purpose set in the introduction. In line 76, you say: “This study investigates the implications of relief asymmetry, induced by structures and stratigraphy, on drainage divide dynamics.” However, in line 139-150 you outline the reasons why chi gives you long-wavelength patterns and why it is different from the local across-divide relief contrast. So, as written, it seems like you either chose the wrong metric/proxy or made the wrong argument for the proxy you chose. I know you evaluate chi, slope, and relief, so perhaps providing some explanation of the Gilbert Metrics (i.e. why are they useful as predictors of divide mobility) would take care of the imbalance. In addition, it is not clear in the introduction (though it becomes clear later in the MS) that the purpose is to seek evidence for longer-wavelength dynamics. My suggestion is to clearly state what the purposes of the paper are, what each proxy will reveal, and how it connects to the main goal of the paper.

*We have expanded the presentation of Gilbert metrics in the Methods section. These metrics are more straightforward to compute and understand than the $\chi$ calculation, which was the reason for the shorter initial presentation. As already stated, the introduction has been significantly revised to address the various questions on how the various spatial scales may interact.*

• Lines 202-204: This isn’t very clear. Tectonically controlled topographic asymmetry arises from spatial differences in the uplift rates, i.e. either an uplift gradient (e.g.

Goren et al., 2014; Habousha et al., 2023) or a combination of vertical and horizontalrock advection (e.g. Miller et al., 2007). In both cases, the steeper side develops
because of greater mass flux through that side which consequently yields across-divide differences in erosion rates (e.g. Willett, 1999; Willett et al., 2001; Whipple and Meade, 2004; Miller and Slingerland, 2007; Goren et al., 2014). *The corresponding text has been modified to clarify the distinction between cross divides morphological contrasts acquired through the initial tectonic deformation or from the response to differential erosion propagating in the adjacent catchments. We also insisted more on the fact that we are analyzing a relaxing landscape where tectonics are no more active. This specificity of the French Southern Alps is highlighted in the revised introduction.*

• Line 213: "drains" = drainages? *Done.*

• Line 254-256: but it is random, right? Looking at plots 4-6, there is no statistically significant relationship between the delta-slope and delta-chi. The moving median shows a weak trend at best. What is the slope of a linear regression? *We have modified the sentence to clarify that we refer to the fact that $\Delta\chi$ values do not display a symmetric distribution around 0 for the various $\Delta S$ bins. There is no statistically significant linear fit of $\Delta\chi$ against $\Delta S$, the key observation from this dataset is that, for large $\Delta S$ values, the $\Delta\chi$ distribution means of the corresponding bins are significantly different from 0. This is now better explained in the text*

• Line 262-263: But you do not show the data for channel pairs with <10 km to confluence. If you test those, what does it look like? *See response to comment above (approach). These divides are separating adjacent basins on the same side of a ridge and are not relevant to our analysis of the cross divide dynamics at the scale of the different ranges.*

• Line 316: Citation? *Citation added.*

• Lines 318-321: Weathering rates tend to scale with physical erosion rates (see review by Anderson et al, 2007), but can also show more complex relationships (e.g. Ferrier et al., 2017) so this interpretation is highly simplistic and overall lacks theoretical standing. See also Carretier et al (2014). *The simple model that we now explore (new figure 9) allows a more robust discussion of this possible counter-intuitive effect.*

• Lines 352-354: are there blocks in the steep sides of the studied divides? *Situations are very variable according to lithologies and vegetation at the bottom of the cliffs, and this does not allow for making a systematic inventory. Scree is abundant on the slopes and large blocks are a common occurrence in the lower part, but probably not as dense as in the type of situation corresponding to the Glade et al. model. We have modified the corresponding paragraph to reflect this.*

• Figure 1 – please add a global map as an inset in panel A. *As indicated in the legend figure 1 is already located on the broader context of figure 2B.*

• Figure 2 – add a box showing where Figure 1 is located. Red lines in Panel A should appear as a legend on the figure instead of a statement in the figure caption. *The* *location of figure 1 is indicated by a marker on figure 2B, and this is specified in the caption. Legend added.*

# Round 2 Review

## *Comments by reviewer 1 (anonymous)*

Completed: 2025-01-20 05:01 PM
Recommendation: Accept

All concerns from the reviewers and AE2 were addressed. There are only some minor editorial changes, such as punctuation errors.

## *Comments by reviewer 2 (anonymous)*

Completed: 2024-12-03 05:29 PM
Recommendation: Accept

## *Recommendation by Associate Editor 2 (anonymous)*

Completed: 2025-01-18
Recommendation: Accept