Discrete differential geometry of fluvial landscapes

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We would like to thank both Benjamin Kargère and the anonymous reviewer for providing valuable feedback that has greatly improved the quality of this work. Both reviewers found the organization of the manuscript confusing, which we have tried to address by consolidating and shortening the theoretical treatment of curvature and by saving comparison to standard surface geometry metrics for later in the paper. This, we hope, makes it easier for readers to focus on the landscape partitioning framework that is a central motivation, without being distracted by technical comparisons that we see as secondary. We have also removed historical context that both reviewers found superfluous and have tried to remove passing references to potential implications (e.g., connections to minimal surface theory) that the reviewers felt made the article lack focus.

Both reviewers were concerned about our treatment of errors in common topographic metrics, and so, in addition to moving that section to later in the paper, we have dramatically changed the language to lessen the implication that common geomorphology workflows are inherently flawed. This was never our intention, and so we have tried to better frame this discussion as a comparison between metrics, each of which may be appropriate in different applications. We have not followed the suggestion by both reviewers to remove this section, as we believe it is the responsibility of scientists to always pursue methods with the greatest accuracy. In this case, intrinsic approaches provide the most accurate means of surface quantification and so we find it important to quantify discrepancies, while acknowledging that simpler approaches are appropriate in many cases.

In addition, we made the choice to define the percent composition of landscape regions intrinsically, which changed the values slightly, and we noticed a typo in Fig. 13 of the original manuscript, which would have said 0.7% rather than 7%.

In this document reviewer comments are in bold, responses are in red, and direct quotes from the revised manuscript are in blue.

Reviewer 1: Benjamin Kargère

Overview

Klema et al. present a new interpretation of topographic "curvature" in the context of coupled fluvial hillslope landscape evolution. Using the Oregon Coast Range as a case study, they show that, in steep terrain, mean curvature is not equivalent to the Laplacian (despite frequent conflation in the literature) and that mean curvature, together with Gaussian curvature, helps delineate process domains between hillslopes and river networks. This is an interesting and useful paper, and I think it fits well within the scope of ESurf. I am supportive of it, and the length of this review reflects how much I enjoyed reading it. That said, the foundational claims and their presentation require substantial revision.

We are very grateful for the thoughtful review, and overall support of the work. We have made an effort to restructure several of our arguments according to your comments, with specific responses to your concerns below.

Major Comments

As stated in the abstract, the major goals of this paper are to introduce mathematical notions of curvature, develop a workflow for measuring the curvature of landscapes, and use this workflow and theory to understand hillslope-fluvial landscapes. Overall, the workflow-related material is clearly presented, including the filtering procedure and figures illustrating the application of curvature-based landscape classification. Given the concerns below, I won't elaborate on these strengths, and I hope this doesn't make the review seem overly negative.

We thank you for your positive comments, and overall enthusiasm regarding the work.

I found this paper less than compelling in its presentation of the mathematical framework, its analysis of curvature in the context of geomorphology, and its selection and framing of some of the main findings. The authors begin by linking curvature to geomorphology through the Laplacian "curvature" (i.e., the divergence of a linear diffusive soil-creep flux) in the stream-power plus linear diffusion model.

A major claim of this paper is that the Laplacian is not an accurate representation of curvature in steep terrain. However, the authors fail to note that linear flux laws themselves (and thus the Laplacian) do not accurately represent geomorphic processes in steep terrain. Take, for example, the Andrew-Bucknam/Roering nonlinear flux law:

$$\mathbf{q}_s = \frac{-D\nabla z}{1 - \left(\frac{|\nabla z|}{S_c}\right)^2}$$

Performing a Maclaurin expansion in $|\nabla z|$,

$$\mathbf{q}_s \approx -D\nabla z - \frac{D}{S_c^2}|\nabla z|^2\nabla z..., \qquad -\nabla\cdot\mathbf{q}_s = D\nabla^2 z + \nabla\cdot\left(D\frac{|\nabla^2 z|}{S_c^2}\nabla z\right) + ...,$$

showing that, for shallow slopes, the leading-order term is a linear flux law, the divergence of which returns the Laplacian of z. This linear, local flux law approximation holds for shallow slopes but breaks down at large slopes. This is why hilltop curvature equals the Laplacian and can be used to approximate uplift.

It is clear from both reviews that the flow of the paper put unintentional emphasis on the comparison between intrinsic and extrinsic geometric quantities. This was not meant to be the central focus of this work, but was included as a demonstration of why, in addition to extracting underutilized surface information, intrinsic reference frames could help geomorphologists avoid known sources of inaccuracy. Instead, a major motivation of this work is a relative lack of geometry metrics that can be applied with consistent accuracy across process transitions. This is highlighted by your argument above. While it is true that curvature is only equal to the erosion rate at shallow slopes, it is still the leading-order term in the expansion. Thus, the dynamics of the hilltop to hillslope transition are defined by the rate at which the magnitude of curvature decays with building slope. Comparison between the area-space evolution of slope and curvature with building drainage area in the hilltop domain (now Fig. 8), and the dependence of projection error on slope (now Fig. 15), suggests this transition is not well constrained by extrinsic geometry in 2-D. As it is beyond both the goals and scope of this work to do rigorous comparison to other technical approaches, we have moved this section to the discussion, and changed the language considerably in an effort to make this a point of consideration, rather than suggested action. We have also changed $\%$-error to $\%$-difference in hopes of making this section feel more observational than critical.

As the authors note on line 85, over the horizontal length scales of entire mountain ranges, the Laplacian (linear-diffusion) approximation provides a reasonable estimate of tectonic uplift rates. This holds because the horizontal length scales greatly exceed the vertical length scales, making the shallow-slope approximation appropriate. The shallow-slope approximation is also inherent in the derivation of the stream-power model (Stark and Stark, 2022; Prancevic et al., 2014). I found that the relationship between vertical and horizontal scales and the related shortcomings of the shallow-slope approximation in steep terrain are central to the paper's motivation, yet the authors do not clearly identify these issues.

As noted above, the first submission of this article did not accurately portray motivations. Rather than focusing on short-comings of the extrinsic approach, the goal was to point out advantages of invariant metrics. These quantities are

accurate on all continuous surfaces, thus making shallow-slope approximation arguments somewhat irrelevant from a processing perspective (though not from a theoretical perspective as you have noted). In addition to de-emphasizing comparison to Laplacian curvature in the flow of the paper, we have added the paragraph below as in introduction to the metric-comparison section (section 6) acknowledging that our approach is not universally advantageous or appropriate.

In this work, we have proposed a self-consistent means of defining several geometrical aspects of topography through connection to formal surface theory. We will now draw quantitative comparisons to a subset of more common metrics. While we do this in an effort to highlight potential advantages of the invariant approach, we want to acknowledge that there are many cases where simpler methods are entirely appropriate and our formulation would be more complicated than necessary. In addition, this is a far-from-comprehensive analysis in the early stages of development. Our goal is not to supplant other approaches, but rather to point out potential avenues of theoretical improvement through the use of intrinsic reference frames.

In Sections 3 and 5 the authors present an extensive review and abstract mathematical derivation of curvature. Although interesting, the discussion of the mathematical history of curvature (Section 3) extends beyond the paper's scope and could be streamlined.

We have removed much of this section, and streamlined elements that we thought added meaningfully to the story.

Considering the mathematical derivation in Section 5, I would describe the authors' approach as "tell-don't-show," in that the treatment remains abstract with few explicit connections to the landscape evolution equations. For example, explicit mathematical details describing the (u, v) coordinates that follow the surface are not presented. The abstract curvature derivation also contains many inaccuracies. As written, this section adds little practical value beyond the cited references and, without explicit equations, leaves it unclear how to compute curvature for real landscapes. I have provided several suggestions in the line comments to help streamline the mathematical presentation.

We agree that the presentation of the mathematical basis was overly abstract, and not well structured. In response, we have combined all technical background on curvature (the former Sections 3 and 5) into a single section (now Section 3). Throughout, we have tried to make more explicit connection to Figure 2 in hopes that motivated readers will reference the figure to better understand the geometric arguments. We have changed all equations to Leibniz notation, which may be more familiar to the audience, and tried to leave out mathematical elements that are non-essential, noting that a more detailed version of our derivation is readily available (Struik, 1950). To help readers understand our intrinsic coordinate definition we have added the following sentence:

In this case the uv-coordinates follow the lines on the map-view grid, but we do not assume orthogonality on the surface.

Some of the paper's conclusions are vague and may be misleading. A major example is that the authors label the discrepancy between metrics for the leading-order shallow-slope approximation (Laplacian and drainage area) and metrics for the full three-dimensional (non-shallow) surface (mean curvature and surface area) as "error." It is not "error" so much as that these are not the same quantities. Calling this discrepancy an error also implies that the three-dimensional metrics are more appropriate for this application, yet the authors provide no physical justification for why three-dimensional curvature or surface area are preferable in geomorphology. This is not to deny that the three-dimensional geometric properties are interesting, but their physical utility for landscapes remains unclear. I do not expect the authors to fully resolve this question, but given the lack of evidence, the tone and presentation should reflect this.

This is mostly a semantic issue, but one that we ultimately agree on due to the noted implication of superiority between one approach and the other in all cases. Our language used in this section came from more of a mathematical framing than a geomorphological one. We will clarify our meaning using the Laplacian example. From a mathematical perspective, both the Laplacian and mean curvatures are meant to portray bending of the surface, something that should not depend on orientation. The Laplacian emerges in many fields of theoretical physics (ie. electricity and magnetism, gravitation, heat transfer, elasticity) and is accurately calculated on complex manifolds through use of integration variables that accommodate geometry. In such contexts, failure to account for orientation would be considered "error", something implied in geomorphology when shallow-slope curvatures are stated as "approximations". If we define "error" as systematic and predictable differences between a metric of geometry, and the quality of the surface it is meant to represent, then deviations from true curvature and slope values in ways that are clearly tied to treatment of coordinates qualify as "error". The distinction with area is more nuanced. We will address this in more detail in a comment below.

All of this said, we realize that our tone implies widespread error in geomorphology studies, which is neither our intent nor our opinion. To address this, we have moved the section on metric comparison to a different point in the narrative, changed the wording from "error" to "distance", and tried to state more explicitly that, while we present a set of metrics that are extremely utilitarian, they are not necessary or appropriate in all applications. We find this better aligns with the goal of this section, which is to point out that systematic differences do exist, and it is important to consider on a case-by-case basis when certain approaches are appropriate.

A proposed major finding of this paper is an equipartition between positive and negative mean curvature. For this to be "remarkable," as stated in the abstract, there should be some kind of null hypothesis for comparison. However, many statistically random fields (e.g., white noise) would exhibit a similar equipartition. This result would also be unlikely to hold for other ratios of hillslope length to boundary length (the channelization index) (Bonetti et al., 2020; Litwin et al., 2022a; Anand et al., 2023), since, if the domain were zoomed in on a single hillslope, the distribution would change. The authors interpret the equipartition between positive and negative mean curvatures as evidence that landscapes

might minimize surface area. I do not find this suggestion to be persuasive. Minimal surfaces have zero mean curvature everywhere on the surface (pointwise), not in an averaged sense. More generally, optimization or variational principles in physics derive from forces and energies, typically involving the minimization of a potential energy. Minimal surfaces arise due to surface tension, a tangential stress (force per unit length along the interface). Surface tension is absent in landscape evolution, and the relevant forces are gravitational, which are not naturally tangent to the surface but instead point downward. This comment also understates the extensive body of literature on optimization principles in landscape evolution (Rodr iguez-Iturbe et al., 1992; Hooshyar et al., 2020; Birnir and Rowlett, 2013; Smith, 2021; Kleidon et al., 2013; Stark and Stark, 2022), which, to my knowledge, makes no mention of surface area. If there were the beginnings of a physical explanation for why landscapes might minimize surface area, I would find this very interesting. In its current form, however, which lacks any physical mechanism, I find the suggestion misleading.

While we stand by the statement that this feature of the landscape is "remarkable" (meaning worthy of focused thought and attention), we agree that positing an explanation of landscape organization through minimal surface theory are both poorly developed and premature. In instances where minimal surface theory has been successful in describing self-organized structures (Andersson et al., 1988), it requires a definition of surface geometry that quantifies energy density along an interface (not necessarily surface tension/tangential stress). We have stopped well short of defining such a geometry and thus have removed this potential implication from the manuscript.

In Section 6.3, the conclusion that surface area may be more correct than drainage area in the sense of the water continuity equation ("potential implications for the interpretation of continuity equations") is misleading. Some of the assumptions in the drainage area formalism include (up to constants of proportionality) that rainfall is constant and is applied via the horizontal x-y domain, that horizontal depth-averaged water velocity is constant, that water is routed via the steepest descent of the topography (normal-flow), and that infiltration is neglected (Bonetti et al., 2018; Smith, 2010; Fowler et al., 2007). Given these assumptions, surface area is irrelevant to the continuity equation for (specific) drainage area. In order for surface area to have an effect, one or more of these assumptions needs to be relaxed, which may include Manning velocity parameterization (Gailleton et al., 2024; Smith et al., 1997; Prescott et al., 2025), infiltration/groundwater effects (Litwin et al., 2022b), or possibly orogenic effects.

As stated above, the implication of superiority in intrinsic metrics in all cases was not intended, and this section was mostly included as a springboard for discussion around potential source of inaccuracy in geomorphology models. We agree that projected area is appropriate in many cases, and under many assumptions. That said, it is clear that some of the assumptions stated above are limiting and undercut our ability to understand some processes and landscapes. By definition, assumptions are simplifications that ignore processes known to be relevant in some cases, so we find it valuable to consider what happens under relaxation of some of these assumptions and have provided a tool to do so. We agree this line of thought was underdeveloped, and so added a conclusion to this section that summarizes cases where quantification of surface-area may be beneficial. The specific passage is as follows:

It should be noted that there is an extensive literature on drainage area calculation, and catchment values are sensitive to grid resolution (Bernard et al., 2022), filtering scale (Erdbrügger et al., 2021), the choice of flow routing algorithm (Tarboton, 1997), and reference frame (this work). It is beyond the scope of this study to put our intrinsic approach in full context with this work, but we find it useful to point out that true land surface area is derivable from DEMs and could be beneficial in some applications. As examples, efforts to define catchment-scale hydrologic responses to snow melt in mountain basins depend on estimated snow water equivalent values interpolated over topography (Chen et al., 2022; Acharki et al., 2025); models that consider groundwater infiltration in addition to overland flow contain parameters that depend on land surface area (Taherian and Ameli, 2026); and certain definitions of characteristic topographic length scales depend on measures of area accumulation defined on the surface (Gallant and Hutchinson, 2011; Grieve et al., 2016; Kargère et al., 2024). While not related to flow routing specifically, connections between topographic form and rates of carbon sequestration in weathered soils rely on mass densities defined relative to the topographic surface (Hunter et al., 2024). Differences between projected and intrinsic surface area could amount to meaningful differences in the resulting interpretation of organic carbon pools at the landscape scale. In each of these cases, the ability to accurately define surface area from map-view DEMs could be beneficial for accurate and efficient landscape-scale analysis.

A final, minor suggestion is to consider noting that drainage area and specific drainage area depend on grid resolution on hillslopes. As a simple example, the drainage area assigned to a topographic maximum equals the grid cell area, and along an idealized planar hillslope it scales linearly with grid spacing. This is not a consequence of filtering, but rather follows from dimensionality. Drainage area has the dimension length squared, whereas the horizontal projection of the contributing region for a point on a hillslope is not necessarily a well-defined area (Kargère et al., 2025; Bernard et al., 2022). Given the widespread use of drainage area to delineate process domains in the literature, it is reasonable to use this metric, but you should note that its value on hillslopes is resolution dependent.

We have taken this suggestion, and added a sentence about sources of ambiguity in catchment area measurements in the above paragraph.

In light of these major recommendations and the length of the manuscript, I suggest streamlining the paper to emphasize what it does best. At its core, this paper presents a substantial amount of interesting and useful material (sufficient for publication), including the workflow, figures, mapping of curvature domains, and the more straightforward connections to between shape classes and geomorphic process domains. The clarification of the distinction between the Laplacian and the mean curvature in steep terrain is particularly useful. Interpreting the more nuanced and exploratory results concerning the physical processes underlying the shape classes is challenging, and in-depth explanations are clearly outside the scope of the paper. This is not a drawback. It makes the paper more interesting. For the interpretations that are offered, I recommend focusing on those that are well supported by the evidence and by physical reasoning. In this respect, less is more.

We appreciate this feedback and have made an effort to streamline the manuscript, remove unnecessary content, and devise a flow that will hopefully keep the reader more focused on direct contributions of this work.

Line Comments

Before turning to the detailed notes, I want to clarify that many of the following comments (the longer ones) are suggestions and should be taken as such. My goal is not to impose my viewpoint, but offer constructive feedback. For stylistic issues, which occur throughout, I recommend a careful read-through to check punctuation (missing commas), ensure that references to sections, equations, and figures follow the Copernicus style guide, and verify consistent capitalization. I have flagged a few examples, but given the paper's length, it is beyond the scope of this review to note every instance.

We are grateful for your thoughtful and thorough review. We have gone through to try and identify stylistic errors, as-well-as punctuation and grammar mistakes.

5, 9: As noted in the major comments, "systematic error" seems overstated. In the small-slope limit, the Laplacian approximates twice the mean curvature, but this breaks down as slopes increase. Similarly, the difference between upstream surface area and horizontal drainage area reflects distinct quantities, not errors in map-view approaches.

As elaborated on in responses to the major comments, we have changed this syntax here and throughout.

10: The pointwise curvature tensor is a tensor field.

We changed this to "defined at every point" to make it clear that we evaluate one element of this tensor field at each DEM pixel

10: Gaussian should be capitalized, "mean" should not be.

We have fixed this throughout

12: Complement.

This has been fixed.

12: As noted in the major comments, I don't think this is an abstract-worthy finding.

While we agree this alone does not provide a compelling link to minimal surface theory, we think the identification of a landscape-scale geometric pattern that seems persistent across scales is interesting in itself and worth pointing out. We have left this passage.

17-18: isostasy (not "isostacy").

This has been fixed.

21: Though it's often described this way in the literature (and I've made this mistake myself) the streampower model is technically not advection, but a sink term (Bonetti et al., 2020). True advection would take the form $\nabla \cdot (zu)$, where u is a velocity field.

This seems open to interpretation. In the context of Bonetti et al. (2020), this makes sense, as it is a term that effectively removes material from the system (sink), but there are several examples where this term is invoked to make a statement about bulk velocity and rates of material transport (advection). In addition, this term has been used to invoke knickpoint celerity arguments (Berlin and Anderson, 2007), which couch this term as an advection term in a kinematic wave equation. Since we are here connecting it to a notion material transported by a fluid we find advection to be an appropriate descriptor and maintain this language.

22-25: "Can be" is used twice.

This sentence now reads:

The rates of these erosion processes determine topographic form, and so landscape geometry can be used quantitatively to understand spatial and temporal patterns in erosion-modulating factors, such as bedrock uplift (Kirby and Whipple, 2012; Klema et al., 2023), lithology (Stock and Montgomery, 1999), climate (Ferrier et al., 2013), and ecology (Amundson et al., 2015).

28: Be more precise. The issue is less about oversimplification and more about overlapping process domains.

Here we are trying to make the argument that not accounting for the dynamics of overlapping process domains is an oversimplification, which limits our abilty to fully understand landscape dynamic. We have reworded this sentence to try and make this more clear.

There are many established approaches to partitioning the landscape into process domains (Montgomery and Foufoula-Georgiou, 1993; Shary, 1995; Jasiewicz and Stepinski, 2013); however, the compartmentalization of processes comes at the risk of oversimplification, as it seems plausible that landscapes are partially defined by interactions between processes.

33: "Development" is used twice.

This sentence now reads:

As digital elevation models (DEMs) become increasingly high resolution in space and multi-temporal (Crosby et al., 2020), there is growing opportunity to understand landscapes holistically using quantitative tools that are accurate, and informative, across all regions of the landscape.

76: DEM was already defined.

We removed the full phrase here and left "DEM".

Eq. (9): The cross product produces the zero vector, which should be typeset in bold.

This equation no longer appears in our derivation.

90: Eq. (1) defines a partial differential equation in $z(x, y, t)$. Therefore, use $\partial z/\partial t$.

This has been corrected.

132: Per the Copernicus style guide, use Sect., followed by the number in the running text, except when at the beginning of a sentence.

This has been corrected throughout.

132: As noted in the major comments, the Laplacian of z is proportional to the mean curvature only to leading order when z is $O(\epsilon)$ relative to the horizontal length scales.

We have removed direct reference to the Laplacian from this section, and instead focus of the differences between intrinsic and extrinsic curvatures generally. This paragraph is now:

On landscapes, the accurate calculation of either intrinsic or extrinsic curvatures requires careful consideration of coordinates to avoid distortions that come from projection of topography onto a map grid. The effects of projection can be seen in Fig. **??**, which compares the distances and angles of a map projection (Fig. **??**.a) to those of the same grid lines overlaying the 3-D surface (Fig. **??**.b). In the map-view representation, the E-W and N-S grid lines are perpendicular and evenly spaced. If one were to define displacement vectors $\mathbf{dx}$ and $\mathbf{dy}$ emanating from point p along these grid lines, their combination would create a resultant displacement $\mathbf{d\hat{s}}$ ending at point $\hat{\mathbf{q}}$. In Fig. **??**.b, however, displacement vectors $\mathbf{du}$ and $\mathbf{dv}$, which connect p to the same points on the surface as $\mathbf{dx}$ and $\mathbf{dy}$ respectively, are not perpendicular and their combination results in a displacement ($\mathbf{ds}$) that maps to a different point (q). It can be seen that neighboring grid cells do not have uniform dimensions, and that the angle between the grid lines is not consistent.

134: "is must"

This has been corrected.

141: Add a comma after result.

This has been corrected.

150: Local, not locale.

This has been corrected.

Figure 2: Use Δ rather than d to denote a finite, non-infinitesimal change.

While Δ would be more mathematically accurate, we choose to keep the infinitesimal notation in hopes that it more effectively connects this figure to our mathematical workflow.

163: Citation style

This paragraph has been removed.

Section 4: This section gets is sandwiched between two sections about curvature, which distracts from the flow. Consider putting it around Sect. 6 or so.

We have rearranged the manuscript such that this section comes after our full definition of curvature invariants

170: Coarser

This has been corrected

Figure 2: "dashed lines how." "show"

This has been corrected

219: It might be clearer to define the surface in terms of the endpoints of the position vector, rather than defining points on an undefined surface. This definition would be along the lines of: "The surface is the set of endpoints of the position vector, parametrized by the coordinates (u, v), forming a subset of R3."

This sentence now reads:

First, a position on the surface is taken to be the endpoint of a position vector parameterized by uv-coordinates such as those shown in Fig. **??**.b, but referenced to a Cartesian basis via as

$$\mathbf{r}(u,v) = r_1(u,v)\hat{\mathbf{e_1}} + r_2(u,v)\hat{\mathbf{e_2}} + r_3(u,v)\hat{\mathbf{e_3}} \tag{1}$$

where the $\hat{\mathbf{e_i}}$ are unit vectors corresponding to easting, northing, and elevation, and u and v are any two intersecting curves on the surface.

220: It would be useful to clarify that each coefficient depends on the (u, v) coordinates, i.e., $r1(u,v)$**,** $r2(u,v)$**,** $\mathbf{r_3(u,v)}$**.**

Agreed. We have incorporated this suggestion.

224: "the the"

This has been corrected.

224-225: u **and** v **are scalar coordinates (a chart). The E-W and N-S curves are the level sets where either** u **or** v **are constant.**

We have defined them in reference to the map-grid as follows:

In this case the uv-coordinates follow the lines on the map-view grid, but we do not assume orthogonality on the surface.

225: Add a comma after "displacement."

This sentence is no longer there.

225: ds **is not a small displacement. Displacement is a vector quantity, whereas** ds **is a scalar quantity. Refer to the magnitude of the displacement as infinitesimal rather than merely "small." By definition** $\mathrm{ds} = ||\mathrm{dr}||$ **is the length of an infinitesimal displacement. Therefore t = dr/ds is a unit tangent vector, as noted in line 242.**

This sentence is no longer in the manuscript.

229: Replace the phrase "resultant of" with "resulting from."

This sentence is no longer in the manuscript.

230: Add a comma before "with."

This sentence is no longer in the manuscript.

232: ds^2 is the square of the infinitesimal displacement magnitude (infinitesimal arc length) along the curve.

This sentence now reads:

The square of the infinitesimal arc-length between points is then given by

$$I = ds^2 = d\mathbf{r} \cdot d\mathbf{r} = Edu^2 + 2Fdudv + Gdv^2 \tag{2}$$

where...

233: The authors also use I to denote the metric tensor itself (Eq. 33). If this is the chosen convention, be more precise and write $I(dr, dr)$ to denote the bilinear form I acting on the tangent vectors dr, returning a scalar value.

Where we state the metric and curvature tensors in Sect. 3.4 we now use a more standard tensor notation rather that the I and II notation.

236: Capitalize "Cartesian."

This has been corrected.

236-239: The definition of α here distracts from the flow of the explanation.

We have removed this definition from this section opting to define it later.

240: ds is not "any surface curve." It is the infinitesimal increment (line element) along the a curve on the surface.

We have removed this sentence.

246: Use 'Eq. (10)' rather than 'equation 10.'

This has been corrected

247: As in line 233, refer to the fundamental forms as bilinear operators acting on the infinitesimal displacement vectors, and use parentheses.

We have chosen to avoid this language and instead focus on descriptive language. We want readers to understand the basic information stored in the fundamental forms (curvatures and lengths defined intrinsically) and are worried much of our audience would be lost if we couch this in terms of bilinear forms.

255: N-S grid lines, respectively,

This sentence is no longer in the manuscript.

Comments on Section 5

This section is abstract, and only minimally applied to the problem at hand. I wonder how many in the geomorphology community will have the patience to work through this abstraction. Figure 2 does a good job, but some mathematical 'show-don't tell' could be useful. Below are some mathematical identities that may help connect this derivation to the form of Eq. (1). A natural place to start might be the x-y 'projected' coordinate system, given that this is the coordinate system for Eq. (1). The (land) surface is defined by the set of endpoints of the position vector

$$\mathbf{r}(x,y) = (x, y, z(x,y))^{\mathbf{T}}.$$

This is what you have in Eq. (8), where x, y, z(x, y) are your r1, r2, r3. This also appears to be the form of Bergbauer and Pollard (2003). In terms of (u, v):

$$\mathbf{r}(u,v) = \big(x(u,v),\, y(u,v),\, z\big(x(u,v), y(u,v)\big)\big)^{\mathbf{T}}.$$

These coordinate systems are related by a Jacobian:

$$\begin{pmatrix} \mathbf{d}u \\ \mathbf{d}v \end{pmatrix} = \begin{pmatrix} \dfrac{\partial \mathbf{u}}{\partial \mathbf{x}} & \dfrac{\partial \mathbf{u}}{\partial \mathbf{y}} \\ \dfrac{\partial \mathbf{v}}{\partial \mathbf{x}} & \dfrac{\partial \mathbf{v}}{\partial \mathbf{y}} \end{pmatrix} \begin{pmatrix} \mathbf{d}x \\ \mathbf{d}y \end{pmatrix} = \begin{pmatrix} \dfrac{\partial \mathbf{u}}{\partial \mathbf{x}} & \mathbf{0} \\ \mathbf{0} & \dfrac{\partial \mathbf{v}}{\partial \mathbf{y}} \end{pmatrix} \begin{pmatrix} \mathbf{d}x \\ \mathbf{d}y \end{pmatrix}.$$

This Jacobian matrix is invertible, allowing for changes between the intrinsic and projected coordinates. By Eq. (1), the surface is assumed to be differentiable (no cliffs or overhangs) and therefore ∂z/∂x and ∂z/∂y are well-defined. As in Stark and Stark (2022), you may also want to rename the land-surface height to something other than z for clarity. It follows from the assumptions in the paper that:

$$\frac{\partial \mathbf{u}}{\partial \mathbf{x}} = \sqrt{1 + \left(\frac{\partial \mathbf{z}}{\partial \mathbf{x}}\right)^2}, \qquad \frac{\partial \mathbf{v}}{\partial \mathbf{y}} = \sqrt{1 + \left(\frac{\partial \mathbf{z}}{\partial \mathbf{y}}\right)^2}.$$

The metric tensor (gxy) in the (x,y) coordinates is:

$$\frac{\partial \mathbf{r}(x,y)}{\partial \mathbf{x}} = \left(1, 0, \frac{\partial \mathbf{z}(x,y)}{\partial \mathbf{x}}\right)^{\mathbf{T}}, \qquad \frac{\partial \mathbf{r}(x,y)}{\partial \mathbf{y}} = \left(0, 1, \frac{\partial \mathbf{z}(x,y)}{\partial \mathbf{y}}\right)^{\mathbf{T}},$$

$$\mathbf{g}_{xy} = \begin{pmatrix} 1 + \left(\dfrac{\partial \mathbf{z}}{\partial \mathbf{x}}\right)^2 & \dfrac{\partial \mathbf{z}}{\partial \mathbf{x}} \dfrac{\partial \mathbf{z}}{\partial \mathbf{y}} \\ \dfrac{\partial \mathbf{z}}{\partial \mathbf{x}} \dfrac{\partial \mathbf{z}}{\partial \mathbf{y}} & 1 + \left(\dfrac{\partial \mathbf{z}}{\partial \mathbf{y}}\right)^2 \end{pmatrix}.$$

The gxy metric tensor is mapped to guv (first fundamental form) using the inverse of the Jacobian matrix. If slopes are shallow, then the gxy metric tensor is approximately the identity matrix, since the partial derivatives of z in x and y enter as higher-order terms. To make this explicit, assume z = $O(\epsilon)$. In other words, the surface height is small compared with the horizontal domain size. Then ∂z ∂x and ∂z ∂y are $O(\epsilon)$, so Fxy = ∂z ∂x ∂z ∂y are $O(\epsilon_2) \approx \mathbf{0}$. The deviations of Exy and Gxy from 1 are also $O(\epsilon_2)$. In steep landscapes such as the OCN, the vertical height scale is not negligible compared to the horizontal scale, so the slopes are not negligible, and this expansion breaks down. As a result, the metric tensor here is not approximately equal to the identity matrix.

We thank the reviewer for taking the time to do such a thorough analysis of the mathematical basis, and for their recommendations in making this technical portion more accessible. As outlined above, we have taken several steps to remove abstraction including grouping the curvature derivations, transitioning to Leibniz notation, and focusing on elements of the theory that are most tangibly connected to Fig. 2. We also enjoyed your approach, but have chosen not to adopt it for the following reason. While tensor approaches to differential geometry are certainly preferred from a standpoint of mathematical simplicity, we have found that matrix-based approaches (eg. Mynatt et al. (2007); Pearce et al. (2006)) are more computationally expensive (Sect. 3.4) when compared to our more classical solution. While we agree that not all readers will take the time to understand our derivation, we want to maintain a presentation that most closely follows the accompanying code for those who may wish to use, and improve upon, it. We hope that our newly structured Sect. 3 adequately addresses your concerns.

Comments on Section 6.1

The set-up of (Bergbauer and Pollard, 2003) provides an analytical expression for the mean curvature:

$$\mathbf{K}_M = \frac{\left(1+\left(\frac{\partial \mathbf{z}}{\partial \mathbf{y}}\right)^2\right)\frac{\partial^2 \mathbf{z}}{\partial \mathbf{x}^2} - 2\frac{\partial \mathbf{z}}{\partial \mathbf{x}}\frac{\partial \mathbf{z}}{\partial \mathbf{y}}\frac{\partial^2 \mathbf{z}}{\partial \mathbf{x}\partial \mathbf{y}} + \left(1+\left(\frac{\partial \mathbf{z}}{\partial \mathbf{x}}\right)^2\right)\frac{\partial^2 \mathbf{z}}{\partial \mathbf{y}^2}}{2\left(1+\left(\frac{\partial \mathbf{z}}{\partial \mathbf{x}}\right)^2+\left(\frac{\partial \mathbf{z}}{\partial \mathbf{y}}\right)^2\right)^{3/2}} = \epsilon\,\frac{\mathbf{1}}{\mathbf{2}}\nabla^2\mathbf{z} + O(\epsilon^3).$$

This form can be compared analytically compared the half Laplacian, which is the leading order term for shallow slope. Given this analytical solution, I wonder whether this section is necessary. I also don't find the inclusion of the $D8$ algorithm particularly useful. If you prefer to keep these sections, consider moving some of this material to the appendix.

We acknowledge that the information contained in this section can be defined analytically, and that it has been presented elsewhere (Bergbauer and Pollard, 2003; Minár et al., 2020). We also note that it is rarely explicitly considered, despite a rich body of literature leveraging the Laplacian approach and well-known analytic solutions. Thus we think it is valuable to find ways of presenting slope effects in new ways. In this section we also seek to validate our approach by showing that our numerically calculated metrics are accurate on both an analytically defined surface and topography. Since we dont know what to expect topographic curvatures to be, we leverage the differences between mean curvature and Laplacian to show that the same relationship holds.

With regards to the $D8$ algorithm, its inclusion is motivated by the primary goal of this study, which is to find ways of consistently defining topographic geometry so that subtle geometric expressions of geomorphic process can be resolved. From this perspective it is worth acknowledging that gradient values measured with the $D8$ algorithm have systematic aspect dependence.

For these reasons, we have opted to keep this section in the main text, though have moved it to near the and, and have made considerable edits, as outlined in our responses to the major comments.

270: The parenthetical note does not aid clarity.

While its inclusion does not add clarity in our logical flow, we think it could be useful for those trying to learn the material. If one searches for 'Euler's Theorem' it brings up a range of results. This formula tends to be slightly buried.

271: Write "Figure" at the beginning of sentences, but Eqn. (19) in the middle.

This has been corrected.

273: Eqn. (17)

This has been corrected.

319, 335, 365: Determinant.

This has been corrected.

383: "the the"

This has been corrected.

396: "As outlined in Section 3,"

This sentence is no longer in the manuscript.

400: It is not clear what is meant by the "x" and "y" coordinate vectors. I believe you mean the vectors $\partial r/\partial x$ and $\partial r/\partial y$, but these have not been defined.

This sentence has been removed.

474: No citation.

This sentence has been removed.

476: "the the"

This has been corrected.

564: Citation style.

This has been corrected.

650: You could mention channel widening (as on line 610) as well, which to me seems just as important as step-pool morphology, if not more (Bernard et al., 2022; Gailleton et al., 2024).

This is a great suggestion, and we have incorporated the following sentence to the end of this paragraph.

In addition, the discernible valley widening signal discussed in Sect. **??** could aid understanding of known correlations between valley width and other landscape parameters (Bernard et al., 2022; Turowski et al., 2024).

503, 666: In the current form, the proposed partitioning scheme isn't so much physically justified as geometrically justified (486).

It is physically justified in that surface geometry records relative rates of diffusion processes, and whether overland flow of either sediment or water is divergent vs. convergent.

675-679: Given my major comment, it seems unfounded to classify the discrepancy between upstream drainage area and upstream surface area as "error."

AS previously mentioned, we have changed this wording here and throughout.

Reviewer 2: Anonymous

For this review, I am going to focus on the big concerns, not point-by-point minor comments. I do this because there are major concerns from my end. I will be direct here: this is a difficult paper to read because several claims and concepts are introduced somewhat abruptly, and the sections do not connect well conceptually. I welcome the authors' explanations, but my understanding is that the manuscript needs a major rewrite; it should not be published in its current form.

We would like to thank you for your direct and honest feedback. In reflecting upon both reviewers' comments we have come to agree. In response, we have dramatically restructured the manuscript to bring more clarity and direct focus to elements of this work that are both most novel and directly supported. We hope you will find this to be an improvement and we welcome further feedback.

My first and biggest concern is the way the manuscript discusses the "error" in the Laplacian. As written, the text implies that the Laplacian is incorrect because it does not match the true 3D curvature. In my view, this is a conceptual misunderstanding. The Laplacian in Figure 7b and the percent error in Equation 39 are measuring exactly what the Laplacian is supposed to measure. It is not intended to represent the geometric curvature of a 3D surface, so the discrepancy shown is not an "error" in the operator itself. It is merely an expected difference. The manuscript repeatedly frames this difference as if geomorphologists expect $\nabla^2 z$ to equal true surface curvature, but I do not think this is accurate. Principal curvature is a geometric property of the surface and is not part of the hillslope diffusion equation; it becomes relevant only when computing intrinsic surface curvature, not when solving diffusion of z(x,y). Yes, geomorphologists sometimes use the shallow slope approximation where Laplacian curvature approximates surface curvature near a hilltop, and the paper rightly shows that this breaks down in steep or complex terrain. However, no quantitative geomorphologist would claim that the Laplacian is exactly equal to 3D mean curvature, and framing the deviation as "error" is misleading. The Laplacian and the surface curvature simply describe different things.

We fully agree that our comparison between mean and Laplacian curvatures was done poorly. We welcome the opportunity to explain our initial approach, and how this has changed in our latest submission. First, we want to clarify that the Laplacian operator measures second-derivative imbalance between orthogonal coordinates. When applied to surfaces, this is a metric of true surface curvature that is connected to diffusion processes through statistical mechanical frameworks. This is why the mean and half-Laplacian curvature take on the same value where there is zero slope. You are correct that it is a 2-D scalar quantity, but we wish to clarify that the mean curvature is also a 2-D scalar quantity, as are all surface geometry metrics used in this study.

The Laplacian is used in many fields of physics where non-trivial geometries have to be accommodated (ie. electricity and magnetism, gravitation, heat transfer, elasticity). In these applications, people have developed mathematical approaches to deal with changing surface orientation without sacrificing accuracy in defining curvature. Projection distortion is often correlated with incorrect treatment of coordinates and is referred to as “error” in these contexts. We adopted this convention in the initial manuscript.

We also agree that quantitative geomorphologists are aware of the limitations of the Laplacian. However, we think this has more to do with broad awareness of projection issues than it does with a notion of it not representing surface geometry. This is evidenced by the ubiquitous reference to the Laplacian as “curvature” (Struble and Roering, 2021; Hurst et al., 2012), and reference to the “shallow-slope approximation” of curvature-driven diffusion processes. We note that curvature drives the leading-order term in hillslope diffusion models (Roering et al., 1999), and so can be expected to play a role in mass transport well outside the region where the shallow-slope approximation is appropriate.

All this said, our goal was never to poke holes in current approaches, but rather to point out that there exist well developed tools for minimizing known distortions, thus making it easier to build process models that are continuous across geometrically complex transitions. For this reason we have de-emphasized the section comparing extrinsic and intrinsic metrics by moving to the end of the manuscript (now Sect. 8). We have also characterized discrepancies as “differences” rather than “errors”. As you correctly point out, there are good reasons to use one measurement technique over others, and characterizing these differences as errors implies superiority of one approach over the other. Finally we have expanded that section to try and make it more clear that our goal is simply to point out measurable distortions, but not to make rigorous comparisons between the many ways problems of topography geometry can be approached. We hope our restructuring of this argument, and the associated language, will help alleviate your concerns.

A second major concern is that the manuscript sometimes conflates 2D planform and 3D surface quantities. For example, the comparison between the horizontally-projected catchment area and true surface area is presented in a way that suggests one is correct and the other is not. These are simply different definitions used for different purposes. The tone, as written, comes across stronger than needed. A concrete example is the discussion of the specific drainage area as defined in Bonetti 2018 and related work: this is explicitly a horizontally projected quantity (see figure one in that paper). That does not make it wrong, just different. There is also some confusion between the specific drainage area, which is a pointwise quantity, and the total drainage area, which is defined for a contour width, and the manuscript occasionally cites papers on one while discussing the other. For a paper that aims to clarify differential geometry in fluvial landscapes, this distinction needs to be handled carefully.

First, there seems to be a fundamental misunderstanding in this review regarding the dimensionality of surface quantities. In this work all surface quantities describe a 2-D surface. That said, the reviewers concern about our treatment of area metrics is well justified. We completely agree that these describe different measurements, each of which may be more appropriate in certain contexts. We have added a paragraph to the end on Sect. 8, where we both acknowledge the large body of literature on drainage area calculation, and point out specific examples of instances where true surface area may be appropriate.

Regarding structure: Section 5 is essentially textbook differential geometry, but it appears after Section 4 on spectral filtering and before earlier conceptual sections are fully settled. This makes the manuscript feel meandering and difficult to follow.

We have combined sections 3 and 5 (now collectively Sect. 3) and shortened the mathematical derivation considerably, focusing on what we consider to be the essential elements. We hope collecting this information has made the manuscript easier to follow.

The historical context in the early part also feels excessive; a condensed version in the discussion might be more effective.

The majority of this has been removed.

Sections 6 and 7, which are the main contribution (application to real topography), come way later.

While there is still considerable buildup, we hope the new structure of the manuscript will make it easier to focus on these sections, without the distracting interludes.

Another example where the writing overreaches is line approximately 570: "This observation could be interpreted as reflecting a Minimal Surface condition... in which total Mean curvature is minimized." If taken literally, this would imply that steady state landscapes tend toward zero mean curvature everywhere, which is clearly not the case. A plane would be the solution. This statement exemplifies a broader tendency in the manuscript to make speculative claims without engaging fully with the existing literature on landscape organization.

It is false that a minimal surface is synonymous with a plane. There are many examples of minimal surfaces with complex surface geometry (Andersson et al., 1988). However, we agree that this idea is underdeveloped for inclusion in this manuscript and have removed the suggestion.

My recommendation: focus the paper on what is genuinely novel, applying well-established differential geometry tools to compute principal curvatures on DEMs and demonstrating what we can learn about landscape segmentation from doing this rigorously. That contribution could be valuable on its own. I suggest removing or significantly toning down

the claims about Laplacian "error" and landscape self-organization, as these distract from the real strengths of the paper. As it stands, the main message is difficult to discern.

We appreciate this feedback, and have taken it seriously in our second submission. We have changed the way in which we discuss the differences between extrinsic and intrinsic curvatures, and have moved this section to the discussion so it does not distract from the primary goals of the paper.

I hope these comments help in improving the manuscript.

They definitely have, and we appreciate you perspective.

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