Discrete differential geometry of fluvial landscapes

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We thank both reviewers for their time and thoughtful feedback.

One commonality between the reviews was confusion around our statement of the landscape evolution equation (former Eq. 1), which set the expectation that we would elaborate on applications of our surface theory approach to landscape evolution modeling. In response, we have removed several paragraphs from the introduction to reduce this distraction and keep the manuscript focused on surface classification, the intended focus.

In addition, both reviewers found the section comparing intrinsic quantities to other common approaches poorly motivated and awkwardly placed. We have moved this section earlier in the manuscript (now Section 4.3), presenting it alongside other results. The introduction to this section has been revised to sharpen focus on why we consider it important — specifically, that it situates our approach quantitatively within the geomorphology literature. We have also made an effort to identify cases where the intrinsic approach is beneficial, while acknowledging that it is not always necessary or appropriate.

Overall, we have shortened the manuscript by removing unnecessary and redundant content. We find the manuscript greatly improved through both stages of the review process.

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

First, I appreciate the major reorganization of the paper. I realize this wasn't easy, and it makes the manuscript much more pleasant to read.

We sincerely thank the reviewer for the thoughtful and detailed comments that greatly improved the manuscript.

Unfortunately, many of my initial comments have not been addressed, and the material has instead simply been rearranged. The manuscript is unnecessarily repetitive and should be more concise in both content and presentation. Consider whether each sentence is necessary or merely repeats a point made elsewhere, and whether it can be stated more clearly in fewer words.

We have provided a detailed point-by-point response to the original review comments, with specific manuscript changes noted for each. Where suggestions were not implemented, justifications are provided in our prior response. In the current revision we have made an additional pass to reduce redundancy, shorten the manuscript, and improve readability.

Some of the analysis underlying Section 5 is not accurate, which I did not notice during the first round of reviews. The overall writing quality is still insufficient. The differential geometry section remains scattered and contains numerous errors, making it difficult to trust even where it is correct. I've tried to flag these sections in the line comments (though near the end of the paper I stopped).

We have carefully examined the line comments and addressed each flagged item directly below. The concerns raised fall into three categories: objections to our chosen terminological conventions (e.g., our use of 'reference frame'), notational choices, and mathematical claims we respectfully dispute. For the latter, we provide justifications and standard references below demonstrating that the results are correct as stated. In regards to non-flagged errors, we are confident in the ability of our approach to calculate accurate surface geometries as is demonstrated in section 4.3 (previously section 6).

I still see a path to publication, but the authors need to revise substantially, cutting the large portions of the manuscript that read as unfoun

We have made a pass through the manuscript with these concerns in mind, and have addressed specific cases in the line comments.

My second major comment is that the authors have not adequately addressed one of my original comments, perhaps due to misinterpretation. The authors dismissed my comment by stating: "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 increasing slope." This interpretation is not accurate. There was also a typo in my review, and the correct expansion is

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

The linear diffusive/Laplacian term is leading order only when $|\nabla z|/Sc \ll 1$. In the OCR this condition holds only in a small neighborhood near hilltops, and not throughout the hilltop to hillslope transition. For slopes comparable to the critical slope, the nonlinear term is of the same order as the linear diffusive term, so the expansion is not asymptotically ordered. Therefore the Laplacian/linear diffusive flux does not control this steep transition region (and presumably nor does the DL-SP erosion). Instead, this region is governed in large part by the nonlinear (but more realistically nonlocal) slope-dependent fluxes.

We apologize for not adequately addressing your concerns, and are grateful for the opportunity to do this now. First, we have tried to rearrange the manuscript to make it clear that we are not trying to model hillslope transport physics in this work, and are rather focused on geometric classification that may be applied to mechanistic transport models the future. However this is not the goal of this paper, and we are sorry if this did not come across in earlier versions of the manuscript.

We understand the reviewer's frustration with our prior response, as they are correct the expansion is not asymptotically ordered in all cases.

That said, we would like to clarify the definition of the Laplacian, as it is relevant to this and later comments by the reviewer. The mathematical definition of the Laplacian on a surface is the divergence of the gradient of a scalar field (elevation z in this case), which reduces to $\nabla^2 z$ for a flat metric. If the surface is non-euclidean the same information is given by the Laplace-Beltrami operator, which returns a scalar quantity exactly equal to double the mean curvature (O'Neill, 2006; Needham, 2021; Struik, 1950). In cartesian coordinates this value can be returned by retaining all terms in a Maclauren expansion similar to what is given here (but without the threshold slope factor). Retaining all terms recovers a value equal to $2K_M$, consistent with the Laplace-Beltrami operator applied to the surface. Near hilltops the frame is basically flat, so the Laplacian is approximately $\nabla^2 z$, a value the reviewer treats as equivalent to the Laplacian itself. Calculating this quantity in an intrinsic frame incorporates all terms exactly, without requiring an asymptotic expansion. The ordering issue raised by the reviewer is therefore a limitation of the projected Maclaurin expansion specifically, not of the geometric quantity itself.

We realize that we have been imprecise with language in previous manuscript versions, and have referred to $\nabla^2 z$ as the Laplacian (a convention common in the geomorphology literature that may have contributed to the reviewers confusion). We have made an effort to explicitly reference this as a euclidean approximation of the Laplacian in section 4.3 to help avoid such confusion. We have also changed labeling in Fig. 6 to make this distinction more clear.

More generally, steep regions can't be well described by erosion laws whose physical basis is rooted in a shallow-slope approximation. As noted in my prior review, this fact is intimately connected to the results presented here.

We agree entirely. The dynamics on steeper slopes are more complicated than the shallow-slope approximation admits. We note that nonlinear hillslope transport remains geometrically driven, which motivates our calculation of invariant geometry in this region.

As written, placing Eq. (1) immediately before describing the OCR study site suggests that the equation adequately represents the OCR, but (at the minimum) its limitations should be clearly noted.

We have removed the presentation of Eq 1 in an effort to sharpen the manuscript's focus toward geometric classification.

Major Comments

1, 3: "to understand" is used in both of the first two sentences.

We have changed this language to avoid repetition. It now reads

Geomorphology as a discipline is defined by the use of topographic form to understand surface process rates on Earth and other planets. In practice this requires drawing quantitative connections between measures of surface geometry and rates of exhumation.

8: "at a point" is unnecessary.

This has been removed.

11: "systematic errors" is still misleading.

This has been changed to

We develop a workflow, including careful spectral filtering to isolate wavelengths of interest, that provides a nuanced view of landscape geometry and avoids projection distortions inherent to map-view geometric calculations.

20: "with relief generally increasing with the horizontal scale of measurement " is not clear.

We find this to be an intuitive description of the red-noise character of topography, in which power increases with wavelength. We are happy to clarify this in the text if the editor feels it is necessary.

36: Use active language. Start with "Several parallel" and end with "DEM processing."

We have incorporated this suggestion. This now reads

In several parallel earth science disciplines, the potential of differential geometry for DEM processing has already been established.

Paragraph starting on line 36: The details of how curvature is used in geomorphology are discussed in the next section, and the final sentence here is vague. I think the last two sentences of this paragraph could be cut and the paragraph merged with the following one.

This sentence is intentionally speculative, as it proposes an explanation for why formal geometric methods have not been widely adopted in geomorphology despite prior efforts. We believe this framing is useful context for motivating the paper, but are happy to revise the phrasing if the editor feels it is too vague.

36: Either use "models" for all of the items or only the first: "models of stresses, sheet joint development, and bedrock fold structure."

We have reworded this sentence. It now reads

For example, similar methods have been used in modeling of topographic stresses relevant to critical zone processes (Moon et al., 2017), of sheet joint development on bedrock surfaces (Martel, 2011), and the structure of bedrock folds (Mynatt et al., 2007; Pearce et al., 2006).

45: Ending the sentence with "with rich potential" is awkward. "Extracts valuable surface curvature information(?)"

We have rearranged this entire paragraph. It now reads

With this in mind, here we develop a landscape classification workflow based on invariants of the curvature tensor. This extracts underutilized geometric information from topographic surfaces, and provides a fully self-consistent means of calculating all common topographic metrics on discretely sampled DEMs that is robust against distortions that arise from derivative calculations on steep, complex surfaces (Bergbauer and Pollard, 2003; Minár et al., 2020). We apply our method to topography of the Oregon Coast Range, long taken to be a type setting for near-steady-state landscape dynamics.

47: Is "continuous" the right word? It seems you may mean "consistent," but "continuous" suggests something different.

We have rearranged this paragraph and it no longer includes this language (see response above)

48: Unnecessary and awkward. Cut after dynamics?

See responses for two previous comments

54: "has"

This has been corrected.

62: If Cayley and Maxwell reached essentially the same conclusion, their discussions should be merged and the emphasis placed on the mathematical results rather than attribution.

have reworded this section slightly to try and draw more focus to their mathematical results. It is now

Arthur Cayley (1859) used topographic contours to show that watershed bounding ridges are composed of "summits" (we will term these structures "domes") connected by "knots" (we will call these "saddles") such that each ridge line contains one more "summit" than "knot". He argued that "immits" (we will call these "basins") would be similarly connected by bridging saddle structures such that there is one more "immit" than connecting saddle. James Clerk Maxwell (1870) similarly argued that the Earth's surface could be sorted into four shape classes; "hills" (domes), "dales" (basins), "passes" connecting hills (antiformal saddles), and "bars" connecting dales (synformal saddles), such there is always be one more dale than bar, and one more hill than pass, thus reaching the same conclusion as Cayley regarding the distribution of topographic curvatures.

62: Remove "prominent physicist" and everything but their last name. This applies throughout the paper (Gauss and Euler), since the 19th century academics being cited are well known for obvious reasons and mentioning it doesn't add substance.

"prominent physicist" has been removed. His full name is retained as it now begins a sentance as shown in the above response.

71: Comma after models.

This has been corrected.

73-74: "models model"

This sentence now reads

For example, at the scale of orogenic provinces, simple models of landscape response to uplift use a heat equation where erosion rates are taken to be the product of long-wavelength surface curvature and an empirical diffusivity constant (Watts, 2001; Ruh, 2020).

75: "Channels themselves are defined on the basis of curvature" was already noted on line 65. This distracts from the goal of arriving at Eq. (1).

This sentence has been shortened to remove this statement. It now reads

At finer spatial scales, curvature-driven diffusion of ridges (Roering et al., 1999; O'Hara et al., 2019) is overtaken by advective transport as drainage area increases, and sediment is transported by concentrated overland flow within the fluvial network (Whipple and Tucker, 1999).

81: Use z(x, y, t) to show that it is a PDE. You could also write A(x, y, z(x, y, t)) and remove "at a given point," though that's a matter of preference.

We have removed this section

83: Note that this is the detachment-limited stream-power model.

We have removed this section

84: ∂z/∂t

We have removed this section

85: Eq. (1)

We have removed this section

84-92: Eq. (2) is unnecessary. I suggest making a new paragraph starting with steady state.

We have removed this section

87: It's obvious that the parameters refer to those in the equation since they were just defined.

We have removed this section

93: Awkward wording. “directly from the curvature tensor, providing” (?)

This suggestions has been incorporated

94: Awkward wording. “At a point,” “assumed to,” and “alike” are unnecessary.

These words have been removed.

107: “Identified” and “documented” are unnecessary. Remove the comma after topography.

These words have been removed.

110: with little.

This suggestion has been incorporated.

Section 3: This section still contains many inaccuracies. I have noted some of them, but the list is far from exhaustive.

We have addressed specific areas of concern below, and are confident in the accuracy of our formulation.

116: “The term” is unnecessary. Comma after or.

"The term" has been removed and the comma added.

117: “The tools of” is unnecessary. Switch “developed” and “in part.”

These suggestions have been incorporated.

118: Awkward and indirect wording. “Curvature is either ‘intrinsic’ or ‘extrinsic.’” (?)

This sentence has been removed

121: I don’t think ‘reference frame’ is technically correct. “How the surface sits in space?”

We disagree with this reviewer statement. Extrinsic curvature depends on the embedding of the surface within an ambient space, and thus on the choice of that ambient space and its associated reference frame. While the choice of reference frame is arbitrary, the geometry and orientation of the surface are not. We recognize this confusion likely stems from our lack of clarity in defining ’reference frame’ here — we use it to mean a coordinate system defined within the embedding space. We have tried to reword this sentence to make this more clear.

Extrinsic curvatures are defined with respect to the ambient space in which the surface is embedded, and thus depend on the choice of external reference frame (Struik, 1950; O’Neill, 2006).

122: “Discretely sampled” is beside the point here. The geometric argument in this section would hold even at infinite resolution.

“Discretely sampled” has been removed. The sentence now reads

On DEMs, 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.

Paragraph starting on line 122. The mathematical notation remains inconsistent. The hats could reasonably be confused with unit vectors, so primes would be better here. Points should be scalars (as on line 127). dx, dy, du, and dv should not be described as displacement vectors and should not be bolded. The displacement vectors should be ds and ds$'$, not ds.

We have broken our response to address the separate critiques.

dx, dy, du, and dv are tangent vectors, and should be written in bold accordingly. Treating them as vectors ensures that the metric tensor is a well-defined inner product, which is necessary for distances computed from it to be independent of the choice of coordinates (O’Neill, 2006). We maintain this notation.

We appreciate the concern of confusing the hats with unit vectors and have incorporated your suggested prime notation.

Points should absolutely be scalars, and we have made this correction in the erroneous definition of q'.

134: Not a reference frame.

This is a semantic distinction that likely reflects differences in how differential geometry is conceptualized by pure mathematicians and physicists. While mathematicians use the terminology of ’frame fields’ — purely geometric objects carrying no notion of an observer (Struik, 1950) — physicists tie local coordinate systems to observers through the concept of a local reference frame (Goldstein et al., 2002). We favor the physics terminology, as it is more consistent with past work incorporating differential geometry in the geological sciences (Bergbauer and Pollard, 2003; Stewart and Podolski, 1998; Martel, 2011). That said, we find this sentence could be more focused and have reworded it as follows

To accurately define a surface, distances and angles between grid cells are not treated as uniform quantities — they are computed locally within a reference frame defined at each point, reflecting the variation in surface geometry across the domain.

135, 607: Refer to Euler and Gauss by last name only.

This suggestions has been incorporated.

141: As who? Colloquial.

This sentence has been changed to

At any point on a twice-differentiable surface, there exist two perpendicular directions along which the minimum and maximum normal curvatures occur (O'Neill, 2006).

141: Continuity of the surface is not enough for the curvature to vary smoothly. Curvature depends on second derivatives, so it should be twice continuously differentiable.

This has been incorporated into the rewording of the sentence above.

145: As noted in my last review, cut the parenthetical note. The equation is already provided, and it is clear that it is unrelated to number theory or complex analysis.

The parenthetical note has been removed.

147: Direction, not path (as on line 151). Not a reference frame either.

The first sentence now reads

Equation 1, known as Euler's theorem, shows that the principal curvatures can be used to calculate the normal curvature of the surface for any tangent direction.

147: Coordinate system, not reference frame. This definition would fit better near line 120, alongside the definition of intrinsic versus extrinsic.

This sentence felt redundant, and has been removed.

150: an extrinsic quality

We disagree with this suggestion. A “quality” is a descriptive characteristic, while a “quantity” is something with a measurable, numerical value. Mean curvature has a measurable, numerical value and is thus a “quantity”.

156: cut “both,” the comma after transformations, and the second “that it.”

These suggestions have been incorporated.

Figure 2: Use 3-D and 2-D. b. is repeated.

Both have been corrected. The caption now reads

Difference between distances and angles measured on a map projection versus on the surface. **a.** Map projection of DEM including map grid defined by E-W and N-S lines with grid spacing dx and dy. The red line corresponds to the rectangular outline in the adjacent panel. **b.** DEM viewed as a 2-D manifold embedded in a 3-D space. Dashed lines show a locally defined uv coordinate system that follows x and y curves on the map projection, but which are not orthogonal or of equal length due to surface distortion. E and G are coefficients of the first fundamental form, and ds is the displacement vector that results from moving one grid space along each of these coordinate vectors

Paragraph starting on line 165: This disrupts the flow. It should be removed or, at a minimum, substantially shortened and not presented as a separate paragraph.

We have shortened this, and combined it with the preceding paragraph. This paragraph now reads

The mean and Gaussian curvatures together determine the geometry about a point uniquely as one of eight distinct shape classes (Bergbauer and Pollard (2003); Fig. 3). Since the Gaussian curvature is the product of the two principal curvatures, it will only be positive in instances where k_1 and k_2 have the same sign. Positive K_G thus correlates to either domes or basins, though we cannot discern which from K_G alone. If K_G is negative, then k_1 and k_2 have opposing signs and the surface is locally a saddle. Again, the orientation in space cannot be determined from this intrinsic quality. In cases where either k_1 or k_2 is equal to zero, K_G is also zero. Such shapes comprise a class of ‘developable surfaces’, which are intrinsically flat and can be formed from a plane without altering surface area. Curvature thresholding to extract developable forms (Mynatt et al., 2007) is a promising approach for classifying landforms. However, we do not explore this further here

Paragraph starting on line 170. I do not think this is accurate. Consider flipping the landscape upside down. Does this still hold?

Here there are two points we would like to clarify.

First, for any given embedding space, the sign of the mean curvature depends on an arbitrary choice of positive direction for the normal vector, which in turn determines the sign of the principal curvatures. This choice varies between studies. We have chosen to define concave-down structures as positive. We could choose the opposite sign convention without changing interpretation. We thank the reviewer for pointing out that we do not address our convention choice explicitly here, and have added a sentence to this paragraph, which is now

The orientation of a shape is an extrinsic quality that can be determined from the mean curvature, allowing us to put geometric classifications based on K_G into a landscape reference frame. This requires an arbitrary choice of positive curvature direction, which we choose to be positive for downward concavity consistent with differential geometry implementations in structural geology (Bergbauer and Pollard, 2003). K_M is positive in two cases: when both k_1 and k_2 are positive, or when the higher-magnitude curvature (k_1) is positive. This means that points in the landscape with $K_M > 0$ are concave down and are locally either domes or antiformal saddles. Similarly, if K_M is negative, then the surface must be mostly concave up and is either a basin or synformal saddle. More generally, the sign of the mean curvature allows us to differentiate between the divergence and convergence of surface gradient vectors.

Second, we see two possible interpretations of the reviewers thought experiment and would like to address both. If the reviewer is suggesting calculating curvatures on the underside of the surface the mean curvature will remain unchanged. This is because the curvature is calculates as the projection of the change in a tangent vector onto the surface normal. As long as the normal vector convention is the same it does not matter which side of the surface is used for calculation (Struik, 1950).

Another possibility is that the reviewer is considering somehow reflecting the topographic surface, and is concerned about resultant changes in mean curvature. Since this reflection would be a change in the embedding, we would expect changes in extrinsic quantities (including K_M). So, the reviewer is correct that our connections between mean curvature and topographic structures would change under such a reflection, however that is completely expected and consistent with the goal of this paragraph, which is to put the invariant Gaussian curvatures in a specific landscape reference.

175: Misleading, since $KM \neq 2\nabla \cdot (\nabla z)$ for non-shallow slopes, as noted in my prior review.

This statement by the reviewer is incorrect and conflates the shallow-slope approximation of the Laplacian with the Laplacian definition itself. The Laplacian is defined as the output of the Laplace-Beltrami operator, which reduces to $\nabla^2 z$ when the metric is flat (O'Neill, 2006; Needham, 2021). In the map-view frame, the full Maclaurin expansion of the projected expression is exact when all terms are retained. The shallow-slope approximation enters only upon truncation at leading order. The reviewer's critique implicitly treats this truncated form as the definition of the Laplacian, which is inconsistent with both standard usage and the fundamental mathematical definition.

176: Add a comma after "opposite" or revise the sentence structure.

We have removed the paragraph on perfect saddles in response to reviewer feedback

177: Put the comma inside the quote or simply remove the ’ around minimal surfaces. Also, “which have arisen” is awkward.

We have removed the paragraph on perfect saddles in response to reviewer feedback

179: The comment about perfect saddles doesn’t add much.

We have removed the paragraph on perfect saddles in response to reviewer feedback

180-182: It’s clear that perfect saddles are not discussed here, so trim and merge the sentences to resolve the awkward wording in the second. The connection to process regimes and drainage area is already clear.

We have removed the paragraph on perfect saddles in response to reviewer feedback

184-192: Move this section to join the paragraph starting on line 122 to rigorously (and correctly) explain Figure 2. This section should also define the tangent vectors ∂r ∂u and ∂r ∂v , using them to get ds.

We have chosen to keep the mathematical derivation consolidated in section 3 for clarity, while the discussion connected to Figure 2 is intentionally conceptual to provide geometric intuition before the formal development. We note that the tangent vectors $\partial \mathbf{r}/\partial u$ and $\partial \mathbf{r}/\partial v$ are already defined explicitly in section 3 in the context of the first fundamental form coefficients, where they arise naturally in the derivation of ds.

187: “via as”

“via” has been removed

190: r1, r2, and r3 serve no clear purpose. You already use x, y, and z in a Cartesian coordinate system so you may as well keep that notation. As noted in my previous review, you may also want to use something other than z here for the multivalued case (cliffs), which can’t occur for a single-valued z(x, y, t) as in Eq. (1) (Stark and Stark, 2022).

We appreciate the reviewers consideration of most efficient notation. However, we have retained the notation r1, r2, r3 rather than x, y, z deliberately, as this allows the theoretical framework to remain general and applicable to non-Cartesian coordinate systems. Restricting notation to x, y, z would imply a Cartesian limitation on the generality of the approach. Furthermore, while our derivation does not employ tensor index notation, the position vector formulation r1, r2, r3 connects more naturally to the Einstein summation convention that underpins modern differential geometry.

Regarding our elevation coordinate, we do not use z in this section, and note that our current workflow is restricted to raster datasets, which cannot accommodate multivalued cases.

192: Note ds = |ds|

We find this unnecessary. Since ds is positive definite by construction, absolute value symbols would be both redundant and non-standard.

207: "Resultant curvature values reference" is an awkward wording.

This sentence has been removed from the latest manuscript version.

216: Use $\kappa(\lambda)$ to distinguish it from Eq. (3).

This suggestion has been incorporated.

217: Check all equation reference notation, as noted in my prior review.

We have checked this notation and are confident is consistent with our chosen formalism (Struik, 1950)

218: "The result is given by" is unnecessary. Equation 12 and 13 should be combined.

We have removed Eq 13 and restructured the sentence. It is now

Since the principal curvatures correspond to extrema where $d\kappa/d\lambda = 0$ we differentiate eq. 11 with respect to λ and set the result equal to zero giving

$$\frac{d\kappa}{d\lambda} = (E + 2F\lambda + G\lambda^2)(f + g\lambda) - (e + 2f\lambda + g\lambda^2)(F + G\lambda) = 0, \tag{10}$$

a quadratic equation in λ whose roots correspond to the principal curvature directions.

225: Use present tense: "are found" or "are obtained." This applies throughout this section.

This suggestion has been implemented throughout this section.

245: It appears that this convention was also used earlier in lines 170-175.

This convention is used throughout, but is now explicitly noted in section 3.2 in response to the previous comment. For this reason this sentence has been removed.

Section 3.4/Line 249: The distinction drawn here is misleading. It's the same approach, just expressed in different notation.

This is correct, as is the following claim that it is likely possible to find a more efficient matrix-based computational approach than we did. We have removed this section.

260: I am skeptical of this claim. Fundamentally it's the same math, so this appears to reflect the computational implementation rather than the method itself. Given this, Section 3.4 seems unnecessary and could be moved to an appendix or removed.

See response to previous comment.

268-270: Combining these sentences would be cleaner.

This sentence now reads

To calculate DEM curvatures, it is necessary to do some degree of smoothing to remove artifacts of the gridding process (Reuter et al., 2009; Bui and Glennie, 2023; Bater and Coops, 2009).

277: "have been extensively applied in geomorphology, with applications including" is clunky.

We agree and have changed the sentence to

Fourier methods have been extensively applied in geomorphology toward the identification of characteristic process scales (Perron et al., 2008), landform classification (Booth et al., 2009), and the assessment of topographic controls on mass transport mechanics (Richardson and Karlstrom, 2019; Black et al., 2017; Crozier et al., 2018).

278: "such that the resulting topography is equal to its actual value only in center of the grid, and is elsewhere damped towards the margins" reads clunky.

This sentence now reads

It is common to accomplish this by convolving the DEM grid with a 2-d raised cosine (aka Hanning window), such that the resulting topography is equal to its actual value only at center of the grid, and approaches zero at the margins (Perron et al., 2008).

287: Citation style.

This has been corrected.

290: This sentence is unnecessary.

This sentence has been removed.

316: "Curves are pulled" is unclear. Add a comma after wavelengths.

Since we do not develop this idea further in the manuscript this sentence has been removed.

325: Drainage area on hillslopes is especially dependent on grid resolution, as noted in my previous review. This should be made clear here.

We do not feel this is an appropriate place in the manuscript for this to be discussed. In response to reviewers previous comments this discussion was incorporated into section 6 (now section 4.3). Here the method of observing curvatures is being developed, and we would find the discussion of drainage area dependencies distracting. We have defined our method of calculating drainage area in the section above, so interested readers can recreate this result, and consider the effects of grid resolution in the discussion section. We note that area binning is ubiquitous in the geomorph literature, and is very rarely accompanied by a discussion of grid-scale dependence. Thus we feel justified in not treating this issue in detail here.

341: This assumption is not well supported and the reasoning is incorrect. KM is not generally proportional to $\nabla \cdot (\nabla z)$. Stability and instability are concepts from dynamical systems theory (phase plane), and the suggested interpretation of KG as indicating of the dynamical stability of the surface is not appropriate. The cited references do not support this interpretation.

As summarized in our response above, the Laplacian is exactly equal to 2KM by definition; the reviewer's claim to the contrary conflates the shallow slope approximation with the actual definition, which is incorrect.

The reviewer is correct that stability is a concept of dynamical systems theory. However, that field of physics is deeply tied to differential geometry. The phase portrait representation relies on vector fields defined on a phase space that is itself a differentiable manifold. Stability at critical points is determined by the local geometry of that manifold. Elliptic critical points, associated with positive Gaussian curvature, correspond to stable equilibria, while hyperbolic critical points, associated with negative Gaussian curvature, are unstable. This phase space classification is fundamentally geometric. While we do not find it appropriate here to go into that level of detail, this paragraph basically says that topographic hollows are more gravitationally stable than topographic convexities, which does not seem controversial or overly speculative.

Regarding references, Matsumoto (2001) provides several geometric definitions of stability on manifolds while Bonetti et al. (2018) posits a very similar idea connecting geometry to surface stability of topographic surfaces. We have swapped Goldstein et al. (2002) for Calkin (1996), as it addresses connections between manifold geometry and stability more directly than the former reference.

344: comma after geometry.

This comma has been added.

347: This sentence doesn't add information and should be removed.

This sentence has been removed per reviewer recommendation.

360: This sentence should be removed.

We have opted to keep this sentence. Since the primary goal of this paper is landscape partitioning it seems relevant to quantify the implied landscape composition.

376: remove after transition.

We have removed the section of this sentence after 'transition'.

381: The convergence/divergence interpretation is not clear.

This is outlined in section 3.2.

383: This sentence is unclear.

We interpret this to mean the reviewer is confused by the reference to the local maximum in KG. This is clear in Figure 10.b. We have added a reference to this figure to help make this more obvious.

385: This sentence is broken and unclear.

This was due to a crucial ‘as’ missing from the sentence, which has been added. The sentence is now

This same trend is apparent in the shape class distributions in Fig. 10.c, where basins trade off with synformal saddles as surface gradient vectors converge.

387: The end of this sentence is redundant, since it is obvious that $25\% > 18\%$.

We have removed this part of the sentence. The sentence is now

This region makes up 25% of the study area.

388: It’s not clear what this sentence adds.

We have removed this sentence.

392: This sentence is obvious and adds nothing of substance.

While we agree this sentence is obvious to people well versed in the literature, we disagree that means it adds “nothing of substance”. Particularly, it shows that the Gaussian curvature landscape partitioning approach is consistent with ubiquitous slope-area methods. We have shortened this sentence to

The growing influence of channels in defining landscape curvature is consistent with area-space fluvial transitions inferred elsewhere in the literature (Montgomery and Foufoula-Georgiou, 1993).

399: Remove the note after the dash.

We have removed the note after the dash.

401: The tone here feels overly promotional.

We have removed this paragraph

Section 5.2: At this point in the paper, it feels unnecessary to go through all of this again, even if the results are new. It may be more concise to combine the Gaussian and mean curvature interpretations and treat the landscape together. This would need to be written more concisely to avoid becoming convoluted.

We disagree that these results are unnecessary. Mean and Gaussian curvatures measure fundamentally different qualities of topography. While Gaussian curvature is useful for identifying process transitions, mean curvature measures the partitioning of broadly convergent and divergent structures. The symmetry in this curvature field represents a previously unrecognized landscape organization signal, and we believe it warrants the 14 lines of text devoted to it.

424: This note is not useful and should be removed.

We disagree and choose to leave this note. As this is primarily a methods paper it is important to point out potential applications of the method, and explicitly state potentially fruitful avenues that we are choosing not to pursue.

Section 6, Fig. 15: These sections come very late in the paper. These ideas have not appeared earlier in the draft, so it is hard to treat them seriously. Remove them or move them to an appendix. If kept and moved earlier (as they were in the earlier manuscript), they will need a much clearer and more concise explanation.

We have moved this section earlier, so that it is now part of our initial results. We have also restructured the Laplacian comparison in response to the reviewers previous comments, and to hopefully avoid future confusion around the definition of Laplacian curvature. It now reads

Formally, the Laplacian of a single-valued curved surface $z(x,y)$ is given by the Laplace-Beltrami operator, which yields

$$2K_M = \nabla \cdot \left(\frac{\nabla z}{\sqrt{1+|\nabla z|^2}} \right) \tag{24}$$

regardless of slope (O'Neill, 2006). Expanding Eq. 24 in a Taylor series about the point $|\nabla z|^2 = 0$ gives

$$2K_M = \nabla^2 z - \frac{1}{2}\nabla \cdot \left(|\nabla z|^2 \nabla z\right) + \frac{3}{8}\nabla \cdot \left(|\nabla z|^4 \nabla z\right) - \cdots, \tag{25}$$

which reduces to the Laplacian for a Euclidean metric ($\nabla^2 z$), as $|\nabla z| \to 0$. Equation 25 is the basis for the small-slope approximation in geomorphology, wherein soil diffusion is taken as proportional to $\nabla^2 z$ on hilltops, and higher-order terms are associated with non-linear behavior on steep hillslopes (Andrews and Bucknam, 1987). The mean curvature, however, is valid regardless of slope and thus captures the geometry of both linear and non-linear hillslope regions. Figure 6 compares $\frac{1}{2}\nabla^2 z$ calculated on a coordinate grid to the invariant mean curvature K_M. We first compare on a unit hemisphere (Figure 6.a-b) and then on binned topographic data in the Oregon Coast Range study site (Fig. 6.c).

461: “And yet” is an awkward construction.

We have replaced this with ‘however’

However, the average value of the stream power model curvature (1.1×10^{-5} m^{-1}) is close to the average value of k_1 (8.3×10^{-5} m^{-1}) extracted from the DEM (we expect an even closer match if tributaries are included in the stream power model, e.g., Willett (2010)).

506: It’s not clear what’s meant by this comment.

We agree this idea is incompletely developed and have removed this sentence.

556-560: This is not accurate, as noted earlier.

While not rigorously developed here, we believe this is a defensible hypothesis for the reasons outlined in our previous response.

The paper should conclude sooner, and the discussion needs to be severely shortened.

While we have made an effort to make the discussion more focused, we respectfully disagree with this assessment as a general statement. The discussion section serves to contextualize our methods against existing approaches, identify limitations, and outline directions for future work. This is standard in a methods paper of this scope, and some length is required to put technical ideas in new context within an established field.

Section 6 should not appear where it is. Remove it or move it to an appendix (provided it doesn’t make inaccurate claims).

We have moved this section earlier in the paper (now section 4.3) in response to your previous comment, and are not aware of any inaccurate claims.

Reviewer 2: Shashank Kumar Anand

I appreciate the effort the authors have made to reorganize the manuscript in response to earlier comments. The paper is much clearer now. I have a few concerns and clarification questions that the authors may wish to consider before finalizing. At a high level, the manuscript analyzes distributions of mean and Gaussian curvature across landscape scales and relates them to geomorphic process domains. This is interesting and potentially useful.

We thank the reviewer for the encouraging words, and for their previous comments that greatly improved the manuscript.

However, I think the motivation in the abstract and introduction could be more focused and better aligned with what is actually done in the paper. In particular, I am still not convinced about the role of the stream power/landscape evolution equation introduced in Section 1.1. It is not used later, and Equation (1) is not referenced again. If it is meant to provide context, that connection should be made explicit; otherwise, this section could be shortened or removed.

This is a valuable comment, and something that was common throughout our reviews. We have realized the statement of the landscape evolution equation, which was initially included for context, distracted from the goals of the paper. As we are not conducting a landscape evolution study, but are rather proposing a method for surface classification, we have removed this section so that we can better focus the manuscript.

Related to this, there are still a few places where the distinction between Laplacian-based "curvature" and geometric surface curvature is not as sharp as it could be. The point that Laplacian curvature is an approximation to surface curvature under small slopes is valid and well known. What is not clear, however, is what new insight is gained from emphasizing this distinction in the context of this study. As it stands, the manuscript shows that the two measures can differ, but it is not clear how this difference changes geomorphic interpretation or process understanding.

This was a common concern between the reviewers, which we have addressed by more explicitly stating the relationship between the Laplacian and the Mean curvature in the beginning of section 4.3 (section 6 in the previous manuscript version). This better motivates our comparison of Laplacian and mean curvature, and the discussion of implications in geomorphology. This section now reads

Formally, the Laplacian of a single-valued curved surface $z(x,y)$ is given by the Laplace-Beltrami operator, which yields

$$2K_M = \nabla \cdot \left(\frac{\nabla z}{\sqrt{1+|\nabla z|^2}} \right) \tag{24}$$

regardless of slope (O'Neill, 2006). Expanding Eq. 24 in a Taylor series about the point $|\nabla z|^2 = 0$ gives

$$2K_M = \nabla^2 z - \frac{1}{2}\nabla \cdot \left(|\nabla z|^2 \nabla z\right) + \frac{3}{8}\nabla \cdot \left(|\nabla z|^4 \nabla z\right) - \cdots, \quad (25)$$

which reduces to the Laplacian for a Euclidean metric ($\nabla^2 z$), as $|\nabla z| \to 0$. Equation 25 is the basis for the small-slope approximation in geomorphology, wherein soil diffusion is taken as proportional to $\nabla^2 z$ on hilltops, and higher-order terms are associated with non-linear behavior on steep hillslopes (Andrews and Bucknam, 1987). The mean curvature, however, is valid regardless of slope and thus captures the geometry of both linear and non-linear hillslope regions. Figure 6 compares $\frac{1}{2}\nabla^2 z$ calculated on a coordinate grid to the invariant mean curvature K_M. We first compare on a unit hemisphere (Figure 6.a-b) and then on binned topographic data in the Oregon Coast Range study site (Fig. 6.c).

Deviation of $\frac{1}{2}\nabla^2$ from the mean curvature is dramatic in end-member cases, but is negligible in many applications. It can be strategically avoided by focusing on low-slope regions (Hurst et al., 2012), or evaluating curvature along 1-D hillslope profiles in which it is easier to account for slope effects (Roering et al., 2007). A formal approach, however, has potential to strengthen such studies. In reality, there are few points in the landscape with zero slope. For example, the hilltop region identified in this study makes up 18% of the landscape (Sect. 5.1.1; Fig. 9). Roughly half of this subset is along steep ridge lines with slopes above 0.4, where slope distortion in the Laplacian is around 20% (Fig. 6b-c). Selecting lower slope thresholds increases accuracy, but at the cost of data volume, a tradeoff that does not need to be considered with intrinsic approaches. Quantifying the difference in these values also measures the degree to which non-linear processes increase with slope, as $\nabla^2 z$ represents the linear term (Roering et al., 2001).

Relatedly, the role of Figure 15 is unclear. If the goal is to demonstrate differences between Laplacian and geometric curvature, this is already expected. If there is more to it, the authors should clarify what additional insight this figure provides beyond that known distinction.

The goal of this section, and of this figure (now Fig. 6) , is to provide quantitative values for these differences. While the existence of such differences is well known, as noted by the reviewer, their magnitude and systematic variation with surface geometry and orientation have not, to our knowledge, been explicitly documented in a geomorphic context. We show that differences between Laplacian-based and intrinsic curvature are not uniform across the landscape but vary systematically with slope and azimuth, reaching magnitudes that are significant in steep terrain. This has direct implications for studies that use curvature or slope as proxies for erosion rate, particularly in the high-relief regions. We have tried in this iteration to better focus this analysis, and better ustify the focus on these differences.

I also have a clarification question regarding Sections 5.1.1–5.1.4: how are the drainage area thresholds chosen? Are these based on identifiable inflection points in the data, or selected empirically? A brief explanation would help.

These area thresholds are defined based on area-space inflection points in the respective curvatures. This is stated in section 5.1 with the sentence

As our partitioning approach is rooted in geometry, we choose a labeling scheme Σ_i^j based solely on curvature invariants. Subscripts indicate the curvature used ($i = G$ for K_G and $i = M$ for K_M), while superscripts (j) correspond to the number of previous zero crossings in area space ($j = 0$ corresponds to zero drainage area).

Overall, I think the main contribution is sound, but tightening the conceptual framing and clarifying these points would improve the manuscript.

We thank you for your thoughtful reviews, which have greatly improved the manuscript.

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