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Review for **Struble and Roering, Hilltop curvature as a proxy for erosion rate: Wavelets enable rapid computation and reveal systematic underestimation**.

The authors present an application of wavelets to calculate the ridgetop curvature. Quantifying erosion rates is one of the key elements and biggest challenges of geomorphology. Recent work suggests that calculating ridgetop curvature is one of only a handful of topographic measurements that can do so. The increasing availability of high resolution lidar datasets makes this a particularly appealing method for obtaining erosion rates. However, topography is noisy and complicates accurate measurements of ridgetop curvature that reflect that background erosion rates. Wavelets serve as a relatively fast way to simultaneously perform a low-pass filter that removes topographic noise and calculate curvature for that smoothed surface.

The authors present a nice development of ridgetop curvature as a metric for erosion rates, introduction to wavelets, application of the method, and exploration of various scenarios with noisy surfaces. Wavelets have previously been used to calculate curvature across landscapes (Lashermes et al., 2007). However, this is the first application and consideration of the method to ridgetop curvature. The distinction of ridges is key because the smoothing scale of wavelets changes the curvature values - particularly when the ridge is sharp. The authors identify that a smoothing scale of roughly 15 meters is appropriate for the Oregon Coast Range and add to a body of work that demonstrates general agreement between long term erosion rates and hilltop curvature. The authors also explore and suggest an explanation for a recently observed relationship between measurements of ridgetop curvature and erosion rates that deviate from the expected linear relationship.

I believe that this work is quite publishable following a few changes. Most of my comments are relatively minor, but I have a few points (mostly concerning the presentation of the wavelet) that I think might add clarity.

General Comments:

Organization: I think that the paper might read more smoothly if there's a bit of rearrangement. I suggest that the authors first describe all methods of calculating curvature then move on to field demonstration. It currently jumps from methods, to field setting, and back to methods. I suggest that the authors place 3.1 directly after the introduction and move the description of the field site to directly before the application to watersheds. I also wonder if the authors might combine the two sections on computational efficiency in to one section - probably section 4.1. Section 3.2 is just rather short and straight-forward.

Torrence and Compo: To me, the Torrence and Compo wavelet is a distraction throughout the paper. The wavelet that is introduced by Lashermes et al (2007) is explicitly for calculating the second derivative, whereas the wavelet in Torrence and Compo is not. The relationship between λ and s in L07 is much more intuitive for this purpose then. Further, I am not convinced the results between the wavelet definitions for a single value of λ are comparable.

DoG wavelets: I think it would be clearer if the wavelets were presented as the second derivative of a specific Gaussian. The Gaussian can be thought of as the kernel for the low-pass filter, which I find to be a much more intuitive thing. Further, because it is a low-pass filter, the Gaussian must sum to unity and so the parameter, s, has a very specific meaning. I suggest starting with the Gaussian and then presenting the second derivative written out with s in the expression.

I have several line comments throughout - some of which are specific examples of the comments suggested above.

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line comments:
line 33: Aren't landscapes composed entirely of rivers and hillslopes?

line 36: Boundary conditions? It's not clear that these are boundary conditions

line 51: Why the quotes around 'nonlinear'? I understand they are getting to a nonlinear flux formulation, which drives a nonlinear response of hillslopes. I suggest rearranging and stating that nonlinear flux relationships with slope drive hillslope responses that become insensitive to further increases in erosion rate.

line 56: "recorder" seems unusual to me. "soil mantled hillslopes are an effective record of" would work.

lines 73-85: The simplest way to calculate or estimate the curvature is to use the central difference. I recognize that this is sensitive to topographic roughness - but it seems like it should be addressed before moving on to more involved methods. Further, even without smoothing, I wonder how much curvature calculations would differ between the central difference and wavelet methods if one were to perform the central difference on elevations that were, say, 10 meters apart as opposed to the one meter that lidar gets you.

line 74: How is curvature defined multiple ways? it is always the second derivative of topography right?

line 80: Change to something like: "To reduce to error introduced by topographic roughness..."

line 80: "... stochastic sediment perturbations ..." I suspect that the authors meant to say "stochastic sediment transport perturbations". But even so, I think that they should remove the reference to sediment transport because there are other sources of topographic noise - boulders, lithology, hollows, etc.

line 157: I think that this statement needs a citation or a mathematical justification. Maybe just state the property of derivatives of convolutions as done in Lashermes et al., 2007?

line 157: Based on the rules for derivatives of convolutions it seems like curvature is calculated with the positive second derivative. Why do you use the negative here?

Eqs. 7 and 8: I find these definitions a bit confusing. It seems contradictory that λ is a physical thing but has two different definitions with regard to s. Since s is the parameter for the wavelet, I imagine setting this equal to a single value and plugging it in to (6), which would produce one result but would have been applied over two different scales according to these definitions? I think that some clarity might come from writing out the function of the original Gaussian that forms the basis for the DoG, because then the meaning of s becomes clear. The original Gaussian must sum to unity because it is the kernel in a low-pass filter and therefore, $s = 2\sigma^2$, where σ is the characteristic length scale (or standard deviation) of a Gaussian. This is consistent with Lashermes et al., 2007 and clearly leads to definitions of the second derivative of topography. The definition of wavelets from Torrence and Compo is perhaps more general and is capable of relating the energy contained in certain wavelengths at certain positions in the landscape, but it is not clear that it directly computes curvature. I think that it would be simpler to acknowledge Torrence and Compo and other applications of wavelets or definitions, but just consider the case that is more clearly linked to the second derivative.

Section 3.3 How is the calculation performed? with Fourier transforms? Isn't that relevant for the computational speed?

Figure 3: This seems like two different ways to display the same information. I would suggest using c and d because the raw time - i suspect depends more on the users computational power whereas I would expect the ratio of the run times to remain similar for different users (so long as the domains are the same size).

Figure 4: Is this full page figure necessary? I'm not sure that it adds much more to the manuscript.
section 4.1: How do the run times scale linearly with domain size? The real value in this seems to be when one would apply it over a much larger domain.

line 290: See comment of equations 7 and 8. I think that this continues to add unnecessary details and confusion. For a single value s, we get two different smoothing scales, which implies that you are not using the same wavelet and I think that the two are not directly comparable. In one, the length-scale of the original Gaussian is multiplied by a factor of $\sqrt{5/2}$ of the other.

Figure 5C: The mode of curvature is zero. Which suggests erosion rate of zero on most of the watershed. Is this an issue?

Figure 6: Great results!

line 328: standard deviation of 50 cm seems quite large. Roth et al., median roughness values approach 10 cm.

Figure 7: add "Note the z-axis on each plot may differ".
line 410: How does the result change by calculating curvature via wavelets with $\lambda = 15$ m on a 1-m resolution lidar dataset versus, say, a 10 meter resolution DEM which are available for the contiguous U.S? I understand that a wavelet with a smoothing scale of 15 meters is taking a weighted average over 15 meters of topography and calculating the second derivative at 1 meter. But is the result appreciably different from just using a 10 meter DEM to begin with?

line 427: This is the first time that the authors have brought up linear diffusion and the steady state profile of topography. I think the message would still come across if they were to remove the reference and reword to say that slow eroding landscapes have broader ridges where the curvature is finite and represents background geomorphic conditions.

line 530: power law and square root relationship?