Review of:

The Direction of Erosion

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General Comments

I am supportive of this interesting paper. It definitely should be added to the conversation regarding how we conceptualize and formally describe the motions of eroding surfaces/lines. Please know that, whereas I am mostly familiar with the concepts and some of the mathematics presented, parts of the presentation are unfamiliar to me. I have attempted to work through the logical progressions to my satisfaction at numerous points in the text (there are many moving parts in the analyses), but I must acknowledge that I have not carefully checked all elements of the work. That said, my experience suggests that the authors are careful in their work, and things seem to be in order.

The authors are asking the readers to absorb the essentials of level sets, the idea of a Hamiltonian, Hugyens's principle, geodesics, etc. Those who have a background in these topics might be fine with the presentation. But I suspect some of this material will be quite unfamiliar to many *ESD* readers. This is a deep read, and it likely will require more than one sitting for many readers (as it did in my case). For this reason, my recommendations starting with the next paragraph are aimed at helping make the material more accessible to a broader audience — assuming this is the intention rather than just being aimed at a restricted group of readers.

The Abstract reasonably describes the key elements of the paper, whereas the information content of the Introduction (Section 1) is sparse. Following this, I suspect that some if not many will stop reading, with eyes glazed over, somewhere within Sections 2.4 through 2.14. The material in these sections comes fast, and although the authors attempt to make connections with descriptions of Earth surfaces/lines, my reading suggests that these sections risk confusing readers without offering a clear idea of why it is important for readers to grasp these relatively unfamiliar concepts and techniques. As one who repeatedly struggles with the question of how to best present (oftentimes) unfamiliar mathematical material in papers, I suggest the following possibility.

Offer an example (or examples), with clear diagram(s), right up front in the Introduction. Show the elements of what the analysis is describing about the motion of a surface/line and what the motion implies. Describe what is happening with reference to qualitative descriptions of the level set(s), normal motion, asymmetry, etc. Such diagrams could be simplified versions of material contained in diagrams that appear later. The relatively familiar problem presented in Figure 8 might be a good candidate, notable given that the rays computed from the Hamiltonian description are directly compared and explained relative to what is obtained from the conventional approach (Section 4.3). Then — and importantly — state the implications of the analysis relative to what is normally envisioned/modeled, including what the analysis reveals that otherwise is not accessible from conventional analyses, including key material selected from the Discussion section. In other words, explain at the outset what the specific merits of a mostly unfamiliar style of analysis are. I suspect that such a "preview" might well help readers with the subsequent primer that unfolds the technical material. I further suggest that this sort of introductory material deserves to be reasonably thorough if it is to be effective. That said, please know that I will not be offended if the authors prefer to reject these recommendations, as I do not imagine that my role as a reviewer involves telling authors what they ought to present (and how) in lieu of the presentation they wish to make. (Note that I recently added a section of this sort, for similar reasons, to one of my own papers in response a reviewer suggestion. I think it helped a lot.)

The authors are clear about the idea that the "rate of change of surface geometry is solely a function of surface geometry... [thus imposing a] geometric self-constraint" that leads to the Hamiltonian description (Section 3.4 and elsewhere). They then use the familiar erosion model embodied in Eq. (24) to highlight the description and its implications. Meanwhile, the science is moving beyond this simplistic formulation of erosion, except perhaps as a rough indication of large scale landscape behavior. I therefore suggest that the value of this example mostly resides in its familiarity, whereas I would like to imagine that the analyses in the paper are aimed more generally at providing a different perspective on describing motions of eroding surfaces/lines. That said, I also am thinking that the class of such formulations of erosion (satisfying the geometric self-constraint condition) is a small set. Partly for these reasons, I pose a problem concerning river meandering at the end of this review.

Specific Comments

Line 34: Is there a compelling reason to "unify" them if they are described with respect to the physics involved? Unified in what sense? There may be merit in pulling key items offered in the discussion (Section 6.1) to clarify what is intended.

Lines 64 and 249, and in reference to Line 515: To "drive" evolution implies physics, whereas a "least time" argument is a geometrical outcome of the variational analysis, as elaborated in the vicinity of Line 950. (The attention given in Section 6.3 to the issue of uncritically appealing to an energy interpretation is appreciated, as summarized in the Abstract.)

Line 70: $h(x, y, t)$

Line 75: Such problems are numerical rather than mathematical, however? The overhang idea, of course, is a restriction.

Lines 108–109: Changes in attributes(?) or effects(?) of vegetation and precipitation are easy to incorporate? Hmm...

Lines 850–855: As the authors describe here, in the Abstract, and elsewhere, the idea of "geometric self-constraint" suggests that the analysis is in fact restricted to those situations in which the formulation of erosion specifically satisfies this constraint, setting aside the added complexities of what the authors are calling "non-locality" with changes in contributing area/length. Given that this is the core premise of the work (Line 906), it probably merits description in the introductory example(s) described above (if the authors decide to offer such a "preview" at the outset). Regarding the final sentences: "...this is the variational principle that governs geomorphic surface erosion. It appears that energy dissipation need not be invoked, and that instead all that matters is geometry," the word 'governs' is strong. After all, physics does the erosion whereas the described outcome arises from the geometric self-constraint — which is only presumed to adequately represent the physics.

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Now I'm going to gently throw a wrench into the works, not because I want the wrench to stop the spinning of the machinery, but rather, because I am curious to see whether the machinery absorbs the wrench and continues to whir along.

The paper focuses on examples in which the surfaces are embedded within a Cartesian coordinate system, where ray trajectories are well defined starting from a set of points on the initial surface/line. Now consider, instead, the problem of a freely meandering river over long time scales. It is now conventional to choose a curvilinear coordinate system in which the primary (intrinsic) coordinate coincides with the sinuous channel centerline. The principle reasons for this choice are to ensure that local channel attributes (direction, curvature, etc.) are single-valued functions for high-amplitude bends, and because the equations of fluid motion can be readily, naturally adapted to this system. In turn, the local rate of channel (centerline) migration is taken to be normal to the centerline. Typically this local rate is expressed as a convolution of upstream (and sometimes downstream) channel geometrical states (i.e., curvature), thus satisfying the geometric self-constraint condition. However, this description of the system means that, with reference to a suitable origin or to two selected initial points on the centerline, the arc length of the deforming coordinate system continuously changes such that the centerline coordinate distance of a local position defined at time t changes at time $t + \mathrm{d}t$, whether the "point" at this position remains fixed, or changes, with respect to a global Cartesian system.

So... setting aside the severe complication arising from meander cutoffs, how might one approach this problem using the variational methods/techniques? Is this even possible? Might it reveal information that we otherwise would not discern from conventional analyses (including numerical simulations of meandering)?

Please know that these questions merely reflect my curiosity. I am not suggesting that they need to be addressed in the paper. Rather, it would be interesting to hear what the authors might have to say about this problem.

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