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Interactive comment on "A lattice grain model of hillslope evolution" *by* Gregory E. Tucker et al.

J. Roering (Referee)

jroering@uoregon.edu

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This manuscript describes a new method to simulate hillslopes based on a lattice-based cellular automaton framework. This work is highly innovative, clever, and wonderfully presented and I recommend publication given some minor considerations and clarifications that I'll detail below.

First, though, I'll note that this manuscript is situated on the cusp of invigorated efforts to revisit how our community does the accounting of mass on hillslopes. The continuum perspective that pervades so much of geophysical theory and simulation is being revisited to account for stochastic, nonlocal, and other effects and the approach adopted here complements forward-looking work by Furbish, Ganti, and others. While cellular automaton schemes have been around for some time, I am not aware of studies that are so carefully crafted as this one as to allow for comparison and calibration

with commonly used continuum parameters. This manuscript offers a clear roadmap for applying CA schemes to well-trod field sites and characteristic landforms.

Second, this manuscript is innovative not only for the algorithmic advances but the interpretations that emerge are also highly compelling. In particular, the ability of the model to account for disturbance processes as the source of both soil transport and production is conceptually appealing and a long-overdue advance. The continuum requirement of breaking up these processes has always been unsatisfying and this effort is to my knowledge the first to provide a unifying treatment. Also, the model's ability to account for retreating cliff-rampart and rocky landforms is highly compelling. The authors neatly applied their toolkit to two field sites as a means to independently confirm their scaling arguments for the model parameters.

Third, (I'm still in gushing mode), I'll note that the lattice grain model presented here is built on parsimony from the parameterization standpoint, which enables the authors to efficiently study a wide range of model outcomes.

Below, I detail several comments for the authors to consider.

1) While the authors claim to invoke minimal parameters in defining their simulations, it seems that the choice of hexagon elements (which conveniently pack purely) as well as the myriad rules for different geometries constitute choices that impact the model outputs. For example, the hexagon packing results in a 30 degree angle of repose slope due to the geometry of the hexagon contacts as well as the gravity rules. This is an arbitrary choice that's convenient for geometry and computational purposes. It turns out that 30 degrees is a fairly common friction angle for smooth cohesion grain piles, so this is appealing. That said, what prevents the model from being performed with octagons? In that case, the packing isn't as efficient and voids are required but it seems defensible nonetheless. Such a geometry would result in a 45 deg friction angle if the same rules were applied. Other element shapes and packing schemes are possible, in fact, a mind-boggling array could be tested. How does the choice

of hexagons affect the results? Beyond setting the angle of repose, how do different packing schemes affect the rules for state changes (see below)?

2) The rules of state changes are nicely laid out with an example orientation in figure 4, although it seems that some possible states are not represented and it would be nice if the figure was comprehensive given that no standards are offered as to why certain motion orientations result in motion and others do not. Can the authors write criteria for the outcomes of the inelastic collisions shown in Figure 4? In particular, the two particles that come together (far right) generate split 50/50 outcomes, why doesn't the same type of outcome emerge with the far left shear-like interaction? I suspect that some of these details don't matter terribly much but it'd be interesting to better understand which do matter and thus account for the behavior observed in the simulations. Is it possible to do an accounting of the state transformations and where they occur in order to determine which interactions dominate in the physical space of the model? Such an exercise is probably beyond the scope of this paper but it'd be very interesting to better understand the impact of the interaction choices listed here.

3) I'm unclear on how the state transition occurs during a given time step. Given that state rules are only defined pair-wise, how are the transitions generated given that each element has 6 neighbors? How is precedence or primacy established? I'm certainly missing some aspect of how these calculations are performed, so perhaps a bit more explanation on this point is possible? Do the pair-state calculations actually occur in sequence? Or does the state transition happen simultaneously? If so, how are the particular pairs chosen? Or is every combination assessed?

4) While the text addresses previous some work on soil-mantled slopes and transport models, the description of rocky slopes and cliff-rampart settings is lacking in scholarship and context. There's a rich literature about cliff retreat in which conceptual models posed are likely consistent with the results generated here. Many workers, including Davis, King, Oberlander, Twidale, Ollier, and others have contemplated these landforms with varying process models in mind, including and notably overland flow. Only

a very recent paper is cited here. The fact that the lattice grain model can generate iconic landforms without directly invoking overland flow is compelling and worth mention and/or context.

5) The emergence of steady state rocky landforms for baselevel lowering rates that exceed the maximum soil production rate is perhaps the most attention-grabbing result of this model. The authors nicely describe how this results from slope and roughness, which is intuitive upon reflection. Given the importance of surface area for the total weathering flux, do any characteristic wavelengths emerge along the interface? is the roughness isolated to the element-scale or is it superimposed on broader wavelength fluctuations? It would be interesting to see if complexity (1/f noise or otherwise) of the surface emerges due to the need for weathering rates to keep pace. The possibility for feedbacks seems like fertile territory.

6) The connection between disturbance and weathering through exposure is very appealing and nicely stated. In this sense, the creation of porosity and the evolution of that porosity in the regolith is a major conceptual advance. That said, it isn't not clear to me in the text that porosity can be essentially advected to depth given a particular sequence of phase state changes. Is that the case? I'm not sure how else porosity goes downward, but however it does, it's worth clarification b/c this sets the weathering rate along the bedrock interface. This goes back to the choices about pair-wise interactions. Would other choices for the interaction rules result in more/less porosity advection?

minor comments: 1) abstract: line 6: the concept of disturbances is part/parcel of this contribution and may merit mention in the abstract. Otherwise, the large block comment is unclear and is probably secondary in importance. 2) pg 2, line 38ish: is it worth mentioning that the lattices don't actually move but rather than the state evolves? it took me awhile to come around to this realization and it could be stated earlier in my opinion. 3) pg 4, line 20-25: the concept of state is employed here before it's explained, I recommend moving some of the text from below (30-33 or so) upwards to clarify. 4) pg 7, line 22: given the porosity, would it be possible to include water and a weathering function in the future? 5) pg 10, line 16: given comments above about hexagon and friction angle, isn't there really more than four parameters? I'm not sure how the counting works, but my sense is that the hillslope relief should depend on the angle of repose which results from hexagon worldview. 6) pg 13, figure 8: would it be possible to fit the standard paraboblic (and or nonlinear) analytical curves to these experimental profiles? that would be another way to determine the parameter scaling. 7) pg 14, eqn 13: I'm dim, but why is 3 on the denominator rather than 2? My apologies but whenever I've done this integration I end up with 2 on the bottom...please help. 8) pg 19, figure 13: it's difficult to see the various symbols here. 9) pg 20, line 4: isn't the creation of porosity and it's downward advection akin to a depth-dependent process? the text here seems restrictive. 10) 7) The diffusivity values for Gabilan Mesa seem quite high and I wonder if this results from K calibration using low-curvature hilltops that are not reflective of integrated erosion in that site.

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