REPORT DOCUMENTATION PAGE

Form Approved
OMB NO. 0704-0188

1. AGENCY USE ONLY (Leave Blank)

2. REPORT DATE

3. REPORT TYPE AND DATES COVERED

4. TITLE AND SUBTITLE
Six Myths About Mathematical Modeling in Geomorphology

5. FUNDING NUMBERS
DAAD19-01-1-0615

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7. PERFORMING ORGANIZATION NAME(S) AND ADDRESS(ES)
University of Oxford
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8. PERFORMING ORGANIZATION REPORT NUMBER

9. SPONSORING / MONITORING AGENCY NAME(S) AND ADDRESS(ES)
U. S. Army Research Office
P.O. Box 12211
Research Triangle Park, NC 27709-2211

10. SPONSORING / MONITORING AGENCY REPORT NUMBER
41586.5-EV

11. SUPPLEMENTARY NOTES
The views, opinions and/or findings contained in this report are those of the author(s) and should not be construed as an official Department of the Army position, policy or decision, unless so designated by other documentation.

12 a. DISTRIBUTION / AVAILABILITY STATEMENT
Approved for public release; distribution unlimited.

12 b. DISTRIBUTION CODE

13. ABSTRACT (Maximum 200 words)

See attached report

20030723 049

14. SUBJECT TERMS

15. NUMBER OF PAGES

16. PRICE CODE

17. SECURITY CLASSIFICATION OR REPORT	18. SECURITY CLASSIFICATION ON THIS PAGE	19. SECURITY CLASSIFICATION OF ABSTRACT	20. LIMITATION OF ABSTRACT
UNCLASSIFIED	UNCLASSIFIED	UNCLASSIFIED	UL

DAAD19-01-1-0615

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Six Myths About Mathematical Modeling in Geomorphology

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Geomorphologists, geologists and hydrologists have always used models. Unfortunately an artificial schism between modelers and experimentalists (or "observationalists") commonly exists in our fields. This schism is founded on bias, misinterpretation, and myth. The schism is perpetuated by misuse and misrepresentation of data and models. In this paper we have tried to address six of those myths and illustrate, mostly with our experiences, why we think mathematical models are useful and necessary tools of the trade. First we argue for a broad definition of "physical" models. Mechanistic rigor is not always possible or the best approach to problems. Second, verification is impossible given that reality is imperfectly known. We can strive for some level of confirmation of model behavior and this confirmation must generally be of statistical, distributional, nature. Third we give examples of how even unconfirmed models can be useful tools. Fourth, examples are given of rejected models, in a sense "failures," that have advanced our knowledge and led us to discoveries. Fifth, models should become progressively more complex, but this complexity commonly results in simple outcomes. Finally, the best models are those with outputs that challenge preconceived ideas. Modeling, including mathematical modeling, is a necessary tool of field researchers and theorists alike.

INTRODUCTION

Are you a modeler? That question has been heard by many of us in geomorphology and other Earth sciences. More often than not it is asked with a pejorative slant. The translation is: are you one of those who play mathematical games that have little relationship to reality? Indeed, much has been written about the limitations of models [e.g., *Oreskes et al.*, 1994; *Beven*, 1996; *Haff*, 1996] but very little about their strengths and advantages. In a field dominated by a strongly empirical tradition [*Rhoads and Thorn*, 1996], there is a danger that the baby will be thrown out with the bathwater.

We believe that models in geomorphology (and we would venture to say in other fields) are indispensable tools of the trade. Models, conceptual or mental, are the basis of all interpretative sciences. All observations, and especially observations of products of past events, require interpretations that are founded on models. Measuring instruments and certainly data analyses require models to link observables to behavior and processes [*Brown*, 1996; *Rhoads and Thorn*, 1996]. A mathematical model is just a special kind of

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model that codifies its assumptions in symbolic language and logic that yields quantifiable and repeatable predictions.

First let us define "model." Webster's dictionary is a good beginning. Here are a few of the more applicable definitions it provides:

- a set of plans for a building
- a copy, an image
- a miniature representation of something
- a description or analogy used to help visualize some thing that cannot be directly observed
- a system of postulates, data, and inferences presented as a mathematical description of an entity or state of affairs.

We have left out from the definition the flesh and bone models who show off the latest high couture. But even they capture the essence of what a model is:

- an idealization of reality
- a representation
- a blue print of an idea
- an aid to visualization and understanding.

Geologists and geomorphologists observe outcomes of different processes that occur largely over decades, centuries and millennia. We never know the initial and boundary conditions with accuracy and hence we always need models to interpret the observations in terms of process understanding. Laboratory experiments are limited because of problems with scaling relationships and proper representation of heterogeneities. For similar reasons our field rarely can afford the luxury of full scale or laboratory (reduced scale) scale prototypes. "Afford" refers not only to the simple issue of cost but, more importantly, to the fact that we simply cannot repeat the past and control an arbitrary or created future. Once again, we need models that allow us to explore ideas, formulate initial hypotheses, and ultimately to predict behavior.

In this paper we will try to address a few of the myths that plague all mathematical modeling and modelers. Hopefully we will be able to convince a few that models, like the geologist's pick or geochemist's mass spectrometer, are tools that should be used or followed by all.

1. BEHIND EVERY GOOD MODEL THERE IS A SOLUTION TO PARTIAL DIFFERENTIAL EQUATIONS

The view of many geomorphologists and hydrologists is that in order for a model to be good it must be "physical" and in their definition that implies the following. There is a set of differential equations that describes the operating processes. The parameters used in the formulation are measurable. The parameters can be obtained from direct observation or experiment. The above criteria for a good, "physical" model are appealing but we would argue that a differential equation is not necessarily the key to geomorphic nirvana. The reality is that continuum mechanics is a model that only works well at certain scales. Newtonian physics breaks down at the elementary particle scale where quantum physics with its "probability functions" describing particle behavior takes over. Quantum physics breaks down at even smaller scales, giving way to string theory or yet to be invented theories. At certain large scales, continuum mechanics also breaks down when nonlinearity promotes localization and "shocks," as in breaking waves, hydraulic jumps, river channels, caves, etc. Continuum mechanics solutions are commonly based on idealizations that only hold at particular scales. For example, flow in porous media is commonly resolved using a hydraulic gradient dependent velocity called Darcy's law, which only holds over some integrated representative volume. Darcy's law certainly does not describe interstitial flow in soils, for example.

Let us think of the meaning of "measurable parameters." Can hydraulic conductivity be predicted from fundamental theory? No. Is "transmissivity" directly measurable? No. Is eddy viscosity a real property of the fluid? No, it is an idealization to allow us to deal with turbulent flow. What fundamental property does the Shields threshold in sediment transport represent? None. Is there a measurable Manning, Chezy or any other roughness coefficient? No.

We do advocate models that are founded on some understanding of behavior and processes. In our definition, though, a good physical model is one that (a) uses some principles that can be generalized, particularly conservation of mass and energy, (b) depends on a minimal set of parameters with real units that are normally rates or thresholds and (c) can be confirmed (see next section) with some observations. A broad range of models are good and "physical" under the above interpretation. Landscape evolution models like those of *Willgoose et al.* [1991a,b], *Howard* [1994], *Densmore et al.* [1998], *Beaumont et al.* [1992], and *Kirkby* [1987], *Tucker et al.* [2001a,b] are examples. They conserve mass. They represent processes in a prescriptive, general manner. They need to be calibrated, and although the parameters are for the most part not predictable from material and state properties, they do have intuitive physical meaning and appropriate values that are delimited by observations and rough arguments based on mechanics (one example is the analysis of bedrock stream erosion parameters by *Whipple et al.*, 2000). The advantage is that these models can be used as virtual realities in which experiments can be

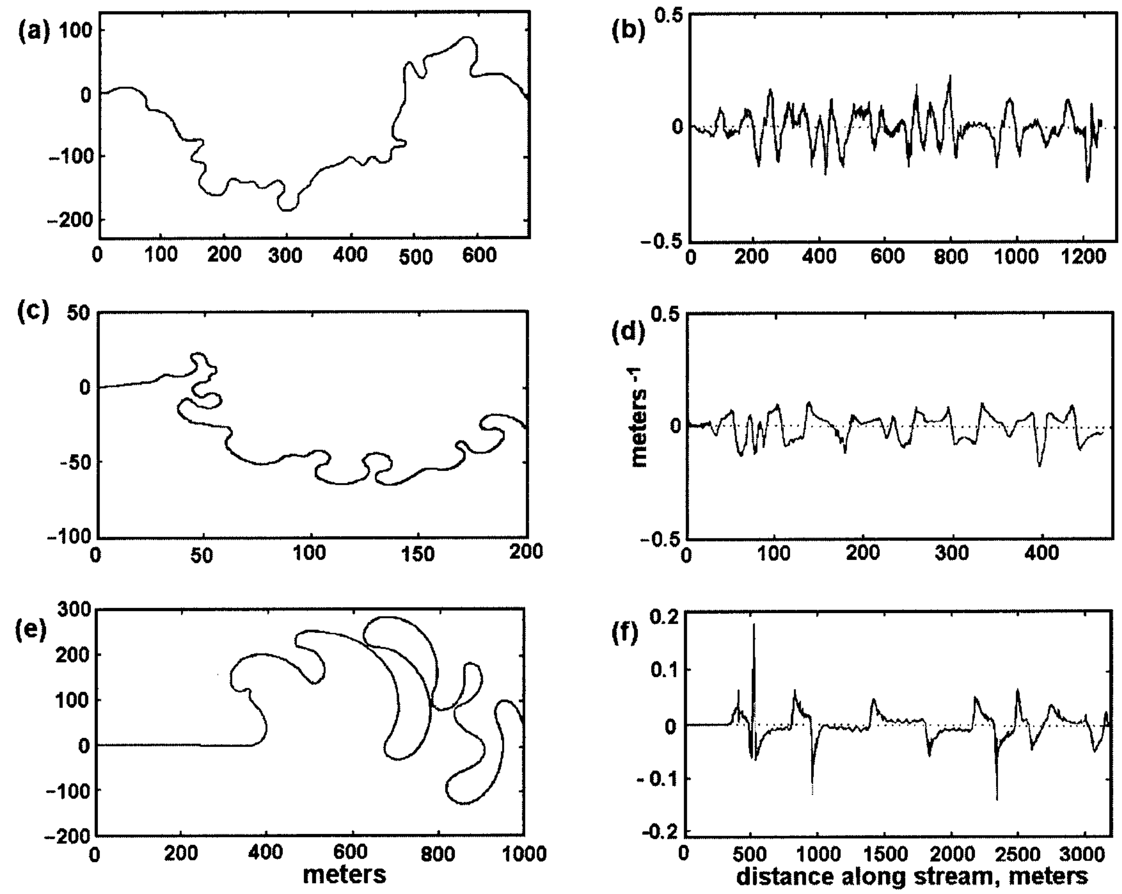

Figure 1. Comparison of Muddy Creek River plan with results from the *Johannesson and Parker* [1989] and *Lancaster and Bras* [2002] models run with Muddy Creek parameters [*Lancaster,* 1998]. On the left, (a), (c), (e) show planform of the river, the Lancaster model, and the Parker model results respectively, on the right, (b), (d), (f) the corresponding curvature. Curvature is defined in terms of the rate of change in direction in the planform. Note that the scales are different in all figures. The observed planform (a) exhibits fairly fast fluctuating directions (curvature) The distance along the stream visible on (b) is twice the straight-line length of the stream segment. The Lancaster and Bras model (middle plots) also doubles the distance along the stream as the loops are traversed. The magnitude of the curvature seems slightly less than observed. The Johannesson and Parker model (bottom plots) produces very large meandering loops, tripling the distance along the stream and exhibits much larger curvature magnitude.

performed beyond the range of space-time scales of the experiences that help formulate them. In contrast, a black box or system input-output model is far more limited by the data used to infer it.

Meandering rivers have been studied as a highly non-linear phenomenon from a fluid mechanics perspective. Meandering has been attributed to unstable behavior in fundamental equations or to complicated interactions of primary and secondary flows [*Kitanidis and Kennedy*, 1984; *Seminara and Tubino*, 1992; *Johannesson and Parker*, 1989; *Begin*, 1981, *Howard and Knutson*, 1984; *Sun et al.*, 2001 a, b]. These analysis tools are useful in elucidating behavior, but not terribly convenient or practical to use in the simulation of the drainage system in which meandering occurs. A contrasting approach is the work of *Lancaster and Bras* [2002] which uses observed behavior to argue for dominance of certain fluid flow phenomena and captures those phenomena with simple approximations that although derivable from basic principles are basically heuristic formulations. The parameters of the *Lancaster* [1998] model are similar to those of *Johannesson and Parker* [1989] and have the same interpretation. Nevertheless, the two models give different results. Figure 1 shows these results of the two models as in an attempt to reproduce the planform of an actual river. The

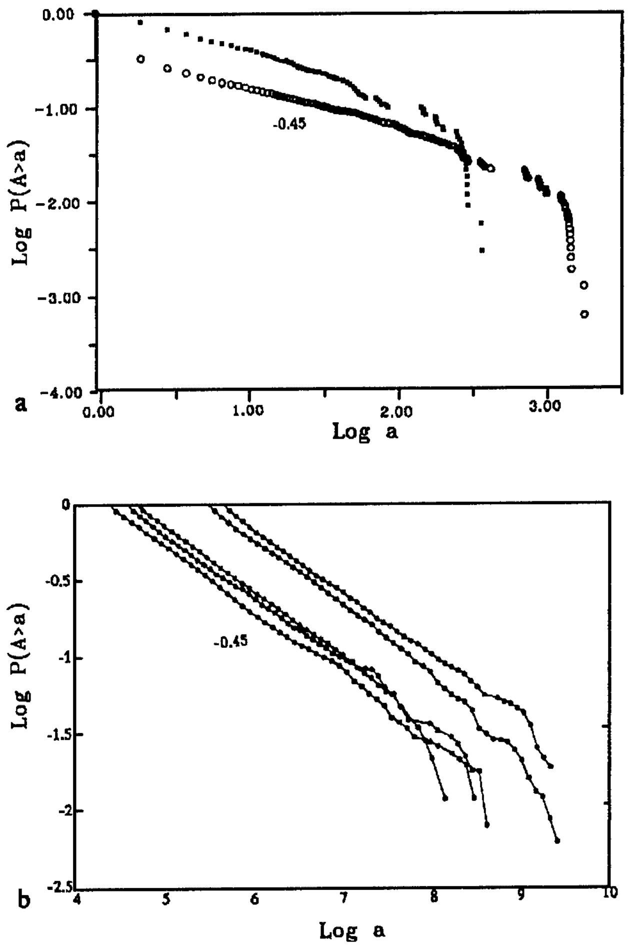

Figure 2. Plots of the probabilities of exceedance of contributing areas for networks resulting from (a) OCNs and (b) for real basins, derived from Digital Elevation Models (DEMs) [*Rinaldo et al.* 1992]. Contributing area is defined as the area draining through a point in the stream network, so it increases downstream as a function of the pattern of aggregation. The top panel shows two OCN's results. The slope of the log-log plots is -0.45, implying a power relationship between the probability of exceedance of area and area. The bottom panel shows the same relationship (power law with exponent = -0.45) for five basins derived from DEMs. This type of result, consistent in nature, is reproduced by the OCNs.

community can debate the differences and argue which model is best but we suspect that before the debate is over another "physical" formulation will come to the forefront. At the end of the day, the value of each model rests on the context in which it is proposed and intended to be used, not on its adherence to arbitrary mechanical purity.

In the other extreme, there are also "toy models" that propose simple empirical rules that control the evolution of rivers and erosion phenomena. Cellular automata [*Chase*, 1992; *Murray and Paola*, 1994; *Segre and Deangeli*, 1995; *Coulthard et al.*, 1997] largely, though not necessarily always, fall in this camp. The value of these models rests on their ability to represent complex interactions in very simple ways, hence allowing the exploration of possible solution spaces very quickly. The difficulty lies in choosing the rules to accommodate situations for which there are no observations or experience.

A very different type of model with no process dynamics is called Optimal Channel Networks (OCN) [*Rodriguez-Iturbe et al.*, 1992a, b; *Rigon et al.*, 1993]. This model postulates that nature will transport water and sediment in the most efficient manner, given some constraints. The model is static in that it resolves the drainage network that will result after a long-term evolution to equilibrium conditions. The final equilibrium does not depend on the trajectory to that final condition. Is that model physical? Yes, according to the criterion defined above, because it is based on three general principles of energy expenditure in the drainage network:

1. the principle of minimum energy expenditure in any link of the network
2. the principle of equal energy expenditure per unit area of channel anywhere in the network
3. the principle of minimum energy expenditure in the network as a whole.

The above are well defined from concepts of open channel flow dealing with velocity and shear stress. Are OCNs a good model? Natural channel networks exhibit a scaling invariance of the probability distribution of some of their variables, such as the probability of stream length exceedance or the cumulative probability of contributing area. Those variables have a power law distribution over a wide range of spatial scales. Figure 2 shows the distribution of contributing area from Digital Elevation Models (DEMs) and outputs of the OCN model. For nearly 4 log scales, the measured and computed distributions follow the same scaling relationship than that found in river networks. The model also predicts most other scaling behavior and self-organizing properties of river basins.

In summary, physicality, like beauty, is in the eye of the beholder. Any model that obeys principles that can be generalized, maintains continuity of mass or energy, and uses parameters that can (potentially) be estimated, should be considered physical. Although there is clear appeal to building models based on fundamental equations of fluid and sediment flow, there are also disadvantages. In particular, these mechanically rigorous models often come at the price of a highly restricted range of space and time scales over which they can be implemented. Heuristic rule-based models can overcome scale limitations and allow us to ask whether cer-

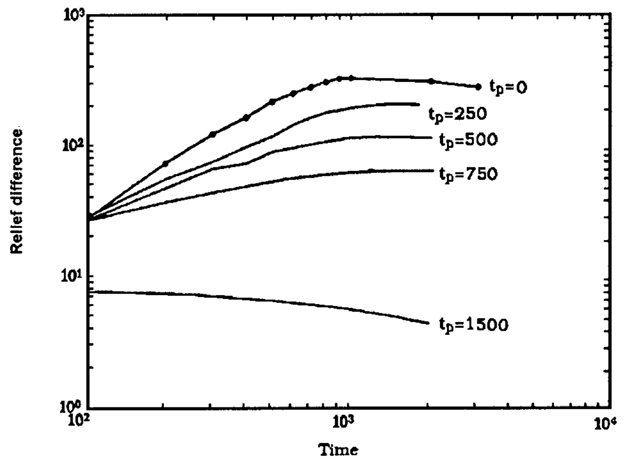

Figure 3. Growth of relief difference between a base unperturbed simulation and perturbed simulations. *Ijjasz-Vasquez et al.* [1992] used a landscape evolution model [*Willgoose et al.*, 1991a, b] to study the sensitivity of the development of a basin and its topography to perturbations. A basin is perturbed at different times (tp) in its evolutionary history. The curves have been displaced horizontally so that they all start from the vertical axis. The perturbations are arbitrary small changes in elevations at one or more points. The relief difference is defined as the sum of the difference in elevations between the perturbed basin and the unperturbed control. A perturbation in the initial condition (curve with tp=0) grows as a power law, until it stabilizes after the drainage channels are well defined. Perturbations at later times (tp= 250, 500 and 750) in the evolution, i.e. when the drainage network is more defined, have a lesser impact in the growth of relief difference. If the basin is perturbed after the drainage system is well established (tp=1500), the relief differences do not grow and are dampened with time.

tain patterns can emerge from a system regardless of the details of its internal constitutive laws.

2. A MODEL IS VERIFIED WHEN IT PREDICTS OBSERVED FEATURES OF LANDSCAPES

The use of the word "verify" is strongly criticized by *Oreskes et al.* [1994] on the grounds that verification is an impossible goal in Earth science. Their point is that geomorphology and Earth sciences in general deal with complex open systems. In open systems the "truth" is never known. The analysis of open systems requires boundary conditions, initial conditions, time and space scales and assumptions that are sources of uncertainty and largely unverifiable. Furthermore, field characterization is always incomplete and that introduces uncertainties in the models. By the strictest definition of "verify," therefore, no model in the Earth sciences—whether conceptual, mathematical, or otherwise—is verifiable.

Oreskes et al. [1994] suggest the use of the term "confirmation" instead of "verification." Models could be confirmed to various degrees as they reproduce empirical observations. In our mind confirmation implies the ability to predict behavior beyond that used for calibration. Regressions are normally confirmed on split samples, data, which were not used for parameter estimation. "Verification" is often used in this looser sense, closer to *Oreskes et al.*'s [1994] definition of "confirmation." Most mathematical models in geomorphology are confirmable (or "verifiable" by the definition of many) according to the definition given above, but there are exceptions. A model will remain un-confirmable if it predicts phenomena that simply cannot be detected or measured, either directly or indirectly. Does this mean that such a model is useless? No, because a phenomenon that is undetectable today will not necessarily be so tomorrow. In fact, a model may stimulate the development of the new measurement techniques required to confirm or refute it. Consider the case of the theory of black holes, which could not be confirmed for years until suitable detection techniques were developed. Alternatively, a model may describe a phenomenon that will never be measurable, like the interaction of early mountain building and climate, but still of interest.

Ijjasz-Vasquez et al. [1992] show that the initial conditions and the history of boundary conditions and forcings *are* important in the development of a landscape. They studied the sensitivity of a landscape evolution model to disturbances during the period of transition while the drainage system is being established. For that, they perturbed the elevation field at different time during the simulations. Figure 3 shows the growth of relief difference between the perturbed simulations and a base simulation, which is not perturbed during the whole simulation. The model output is particularly sensitive to these perturbations. The difference grows as a power law until the channel network develops sufficiently to drain the whole area. At that time, a certain level of stability and robustness is achieved once the drainage system is created. The implication is that it is not possible to predict the exact state and location, say, of a river in the future or in the present from assumed initial conditions and forcings. As the system is increasingly constrained, for example looking at the evolution of an existing stream reach over short time, the ability to deterministically predict improves but it will never be perfect. Deterministic prediction of detailed geomorphic expression is as impossible as predicting the instantaneous energy state of a quark or the position of a photon. Only the probabilistic distribution of energy and position can be described. Hence confirmation in geomorphology must focus on testing the statistical or probabilistic expressions of models.

What does the above imply for mathematical models in geomorphology? Figure 2 already shows one such example where OCNs are shown to reproduce observed behavior of nature. In the case of landscape evolution models, the only hope is to reproduce properties and distributions that are not "wired" in the model. For example, there is no hope to predict the exact location of streams, but it should be possible to reproduce properties like the drainage density (a mean quantity), the width function, the link concentration function (the number of links at a given elevation in the basin, *Gupta and Mesa*, 1988), the hypsometric curve, the distribution of contributing areas, the distribution of stream lengths, the slope area relationship, and the roughness characteristics [*Bras*, 1990; *Moglen and Bras*, 1995a,b]. For meandering models, the significant property can be sinuosity, its variance and characteristic length scales [*Lancaster and Bras*, 2002]. It should be clear that certain statistics have more power of discrimination than others. Horton numbers are not able to distinguish between different models. It is also possible to confirm or "verify", say, planar measures (width function) while altogether missing elevation properties like the link concentration function, or the slope – area relationship.

This brings up the issue of equifinality. Can truly different models lead to the same outcome? We believe that would be very rare, if outcome is defined as the complete description of the physical entity being predicted. The difficulty lies in finding and using the appropriate discriminating statistics to distinguish between outcomes of different models [cf. *Beven*, 1996]. For the most part, because of data limitations, tests focus on a few low order moments of states or outputs and don't explore the full distributional characteristics. That leads to the ambivalence that is called equifinality. Science must always seek better and better "microscopes" or analysis tools that will be able to "see" the difference between the predictions that may lead to the confirmation of different models.

To summarize, full confirmation of geomorphic models, or a loose interpretation of verification, could be impossible or very difficult to achieve. For the most part "verification" needs to be based on the distributional properties of the outcomes of models. "Determinism" (in the sense of single valued predictions of features) is an impossible dream in most geomorphic settings. Difficulties with confirmation or verification should not deter us. Indeed, unconfirmed models can lead us to better and different observations that will then result in confirmation (or refutation) of model behavior. Unconfirmed models can also be useful in a variety of other ways, as discussed in the next section.

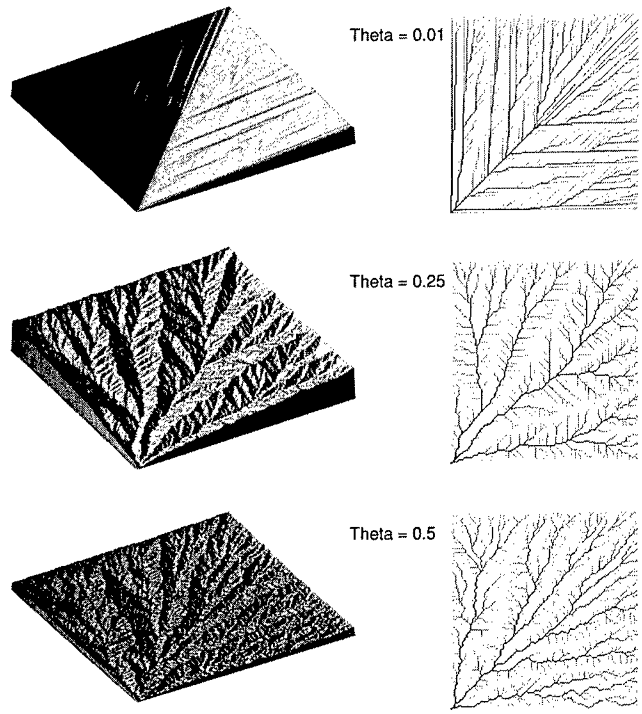

Figure 4. Simulated topographies at equilibrium and corresponding drainage network generated by *Veneziano and Niemann* [2000] using the slope-area relationship with $\theta = 0.01$, 0.25, and 0.5. The most realistic looking topographies are obtained with theta in the range 0.25 - 0.75. A θ value of around 0.5 is the most common in nature. Values approaching or greater than 1 also give very unrealistic results. OCNs predict a value of 0.5. The value of θ is also related to fractal and multifractal properties of basins. This figure also illustrates a very important point, the planar and relief properties of basins are very much related. Note that the slope-area model used is a statement of relief (slope) and organization yet it clearly impacts the planar expression of the drainage system. The bottom line is that even simple models clearly point out when their parameterization, or formulation, is wrong.

3. THE FUNCTION OF A MODEL IS TO MAKE QUANTITATIVE PREDICTIONS FOR COMPARISON WITH NATURE

It has already been argued that numerical models are largely unverifiable, if verification implies demonstrating the truth. Simply put, we never have enough information to establish that we have the truth. We have also argued, like *Oreskes et al.* [1994], that models should be confirmed. Nevertheless, is a model of any use if its empirical foundations are shallow? Or if its predictions have not been backed up by demonstrated consistency with observations? We believe so. "Unverified" models are useful in many different ways.

First, mathematical models make predictions, and their predictions may or may not be immediately testable (and thus confirmable or refutable). Far from being useless, "unverified" models can, or should, stimulate the collection of data required for testing them. This deductive mode of operation—with models making predictions that are then tested by experiment or observation—is standard in physics

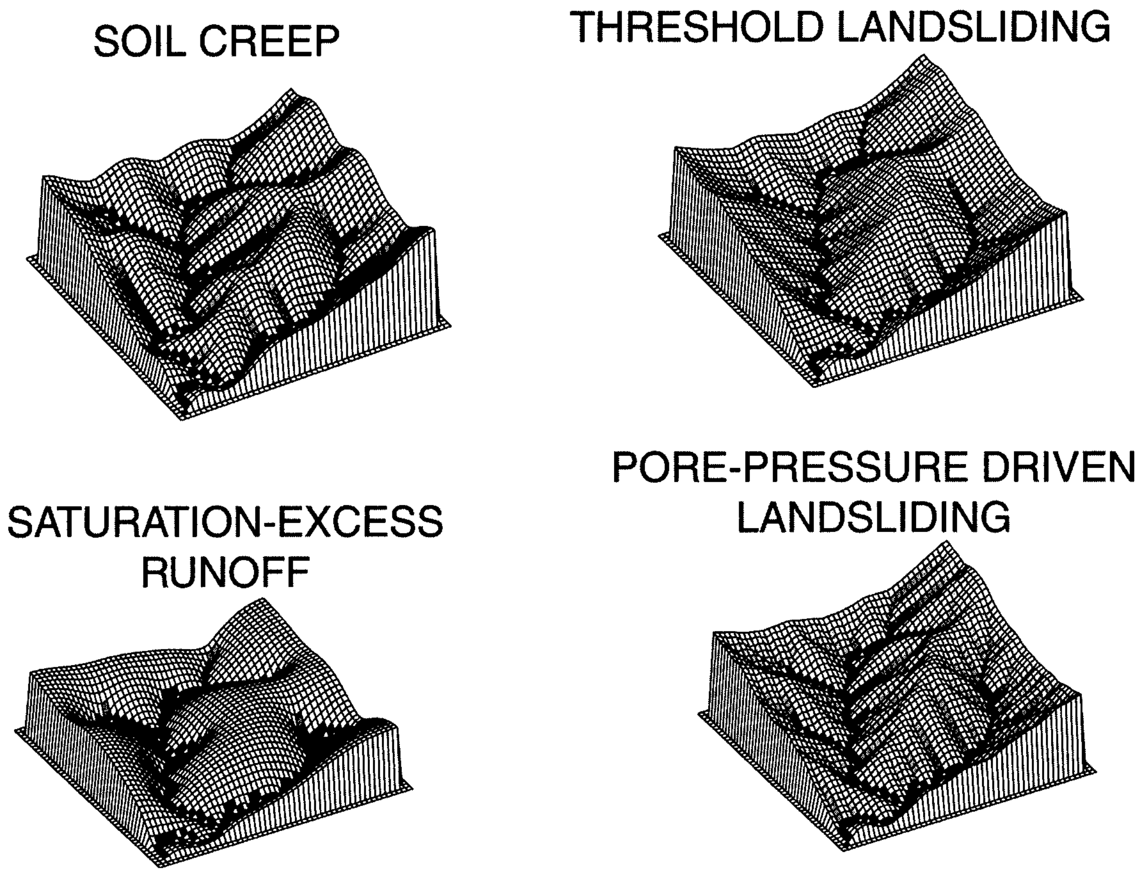

Figure 5. Simulated landscapes [*Tucker and Bras*, 1998] with varying hillslope processes: (a) simple competition between creep and runoff erosion, (b) hillslopes dominated by simple threshold landsliding, (c) runoff production by saturation overland flow, and (d) hillslopes dominated by pore pressure driven landslides. The "signature" of dominant processes is clear. Most observers would recognize the nature of the landscapes shown. For example, the pressure driven landslides are reminiscent of the Apennines in Italy, the saturation excess landscape reminds us of New England. That they are robust results of underlying processes is a valuable insight of an "unconfirmed", uncalibrated model that is not trying to reproduce a particular landscape.

but a relative newcomer in most fields of the Earth sciences, which has a long tradition of theory following observations rather than the reverse. The theory of black holes, noted above, is one example of the usefulness of an (initially) unverifiable model.

Second, many have argued that models are foremost a tool for organizing scientific thought [*Konikow and Bredehoeft*, 1992]. In geomorphology models are generally collections of process representations. The validity or level of confirmation of each process may be variable and the way they interact even more uncertain. Formulating the model forces the conscious choice of process representation and of interaction. They serve as frameworks, templates, that require explicit decisions and choices, and as a result, they force rigor in our hypotheses. For example, interpretations of climatic control on erosion rates are often phrased in terms of whether the climate was "wetter" or "drier" in the past [e.g., *Kiefer et al.*, 1997]. Building a mathematical model to describe climate-geomorphic connections forces one to specify precisely what "wet" means—is it a higher mean annual rainfall or more intense storms? [e.g., *Rinaldo et al.*, 1995; *Tucker and Slingerland*, 1997; *Moglen et al.*,1998].

Third, mathematical models are also quite unforgiving when those choices are obviously wrong. In other words, wrong choices normally reflect themselves in an abysmal lack of confirmation. As an illustration, Figure 4 shows topographies and drainage networks generated by *Veneziano and Niemann* [2000]. They use the "slope-area" relationship used by *Ijjasz-Vazquez et al.*, [1993]. This model, a close cousin of self-organized critical analogies [*Rigon et al.*, 1994] and OCNs, imposes a power relationship between slope and area draining through a point. This leads to flow aggregation and network organization. The key parameter is the exponent, θ, in the relationship $S=\alpha A^{-\theta}$, where S is slope, A is area, and θ is another parameter. It is clear from Figure 4 that a value of 0.01 leads to unrealistic river networks, in contrast to values in the range of 0.25 to 0.75 that result in networks that visually cannot be easily eliminated as unreasonable [*Tucker and Whipple*, 2002]. With parameters outside of the range 0.25 to 0.75, the model is suspicious at best, and this tells us a lot about our assumptions of the behavior of nature.

A similar experience, in which the model points to gaps of understanding is given by *Gasparini et al.* [1999]. Landscape evolution models that assume soils of uniform grain size do not always seem to be able to reproduce the level of concavity of the longitudinal profile of channels that is observed in nature, indicating a serious deficiency in behavior. This experience has led to a multiple grain size representation, which does preserve the observed concavity of channel profiles. The next section will also show that multiple grain sizes in the sediment also lead to regularity and simplicity in the organization of the river network and its sediments. As *Oreskes et al.* [1994] write, the models at the very least guide us to further study and even challenge existing formulations and, we would argue, existing hypotheses. This last point will be expanded on later.

A fourth role of models, even for partially confirmed models, is to compare relative behavior against other models. Note that we are not arguing to calibrate model against model or to seek confirmation via such comparisons (although we would argue that model self-consistency can be tested in this way). Comparison of different models can help us to diagnose and elucidate what elements of the various models lead to the observed differences. It should be clear that in models of geomorphic systems the interaction of elements is such that it is often impossible to anticipate what the behavior of the integrated system will be. In fact, the most useful mathematical models in geomorphology are the ones where outcomes cannot be anticipated. For example the meandering model of *Johannesson and Parker* [1989] and that of *Lancaster and Bras* [2002] lead to different behaviors, such as in the formation of compound bends (Figure 1). A related use of unconfirmed models is sensitivity analysis. Well designed mathematical models allow us to test the relative importance of elements, links, parameters and processes.

Finally, the value of any modeling exercise is foremost as a virtual laboratory. The introduction to this paper argued that laboratory and field studies in geomorphology are limited by issues of unknown scaling rules, heterogeneity of materials and processes, and lack of knowledge of initial and boundary conditions. Mathematical models, when used properly and carefully, serve as virtual bench-tops wherein experiments can be controlled and repeated, albeit subject to model uncertainty. The models can be simple or complicated, so long as they are understood and controllable. *Bak and Chen* [1991] and *Bak et al* [1987, 1988] built many of their ideas of self organized critical behavior on a very simple model of a sand box which allowed a variety of experiments to explore parameter space or more subtle issues like the role of local interactions. *Tucker and Bras* [1998] used a landscape evolution model to selectively test the topographic imprint of various hillslope processes, i.e. Hortonian versus saturation from below runoff, landsliding, etc. As Figure 5 illustrates, these processes lead to very well defined landscape signatures, all of which are familiar to a careful observer. In this example the value of the model is not in reproducing any particular landscape or for that matter the distributional characteristics of a site. The value is in helping us visualize, in a controlled manner, what reasonable process representations do to a landscape and then set us off to confirm, in an objective manner, that "prediction" in nature.

In summary, the idea that a model is only useful to quantitatively predict the behavior of nature is a myth for several reasons. First, unconfirmed (unverified) models serve as deductive tools that help guide the search for new observations. Second, the process of model-building itself is a valuable exercise that forces rigor in our hypotheses and interpretations. Third, models can highlight errors of understanding and concept. Fourth, models may lead us to unanticipated insights, and fifth, they serve as virtual laboratories.

4. A REJECTED MODEL IS A FAILED EXPERIMENT

If this myth were true, science would be littered with bodies of "has been" colleagues and their theories. Accepting the premise that "truth" is never perfectly known leads to the expectation that today's model and theory will sooner or later be superseded by other models that exhibit a greater degree of confirmation. Science is by nature incremental. We owe everything to those that came before us and led us, consciously or unconsciously, to the issues and inspirations of today. Geomorphology is not an exception. There are vast numbers of rejected but useful models. The well-known Horton's characterization [*Bras*, 1990] of the river basin in terms of bifurcation ratios, length ratios and area ratios is weak and non-discriminating [e.g., *Kirchner*, 1993].

Many networks which otherwise make unrealistic river basins have reasonable Horton numbers. Yet Horton's thinking opened the way to the characterization of fluvial networks by revealing the regularity of drainage patterns, challenging scientists to explain the physics behind the regularity. Until 20 years ago Horton's numbers were the only way to look at these regularities. His "failed model" was a great teacher and probe of deep questions.

Shreve's [1966, 1967] topologically random network is another wonderful "failure". It is hard to imagine a more elegant construct to capture the origins and properties of drainage networks. It explained Horton's observations and more. Many will now argue that the topologically random model is intrinsically flawed because it ignores the inseparable nature of the planar expression of the basin from its third dimension [*Gupta et al.*, 1986] and because the dynamics of basin evolution are not random in the sense assumed by Shreve.

Willgoose et al. [1991a, b] argued that the value of modern landscape evolution models resides in recognizing that the channels and hillslopes are part of an integrated system. Studying channels alone or hillslopes alone is severely limiting because they result from competing processes that cannot be viewed in isolation. Geomorphologic literature is nevertheless full of attempts, past and present, to model hillslopes or channels alone. We argue that there was value in those efforts. They were needed before increased knowledge and computational power allowed us to tackle the integrated problem and, not insignificantly, they were also useful in engineering applications.

We would argue that it is healthy to have competing models, even though chances are that sooner or later one of them will fall out of favor because it fails to represent a yet to be specified observation as well as some other model. This schizophrenia of models and theories can occur even within research groups and individuals and certainly exists among the authors of this paper. For example, do channels begin when certain well defined process thresholds occur [*Montgomery and Dietrich*, 1994] and are processes in channels and hillslopes correspondingly different [*Willgoose et al.*, 1991a, b]? Or is there a continuum, modulated by heterogeneities and "randomness," between channels and hillslopes where the landform shaping processes are essentially the same [*Tucker and Bras*, 1998; *Tucker et al.*, 2001a,b]? It is hard to tell. The two hypotheses might not be mutually exclusive or they may indeed be mutually exclusive. The authors are unable to be definitive about one hypotheses or the other. In the meantime we are happy schizophrenics and will continue to explore both ideas until the dust settles.

5. COMPLEX MODELS MUST YIELD COMPLEX RESULTS

Most Earth systems are complex. Here we define complexity in the sense that Earth systems generally involve many different processes. These processes are typically connected via intricate feedbacks and operate across a wide range of space and time scales. Consider the example of weathering and erosion in a mountain drainage basin. Bedrock may be broken down through an array of processes including stress release fracturing, chemical attack by water circulating through fractures, stresses imposed by the growth of root systems, and breakage during episodes of mass movement near the surface. The breakdown products might be transported downslope by any combination of processes, such as gravitational mass movement (fast or slow), overland flow during storms, and soil dislocation through plant growth and decay or animal activity, to name a few. Most of these processes interact: for example, formation of stress-release fractures increases permeability, which may increase rock-dissolution rates leading to further stress release. Some processes involve many frequent, small events—such as a rodent excavating soil from a burrow—while others involve large but rare events—such as a deep-seated landslide that carries most soil and near-surface rock to the valley below.

Given this bewildering array of processes and space-time scales, most of which are laundry-listed in any geomorphology textbook, can one really hope to be able to understand mountain hillslopes to the level of being able to quantitatively model their behavior as sediment-producers or as evolving landforms? We believe most practitioners would agree that we can and should do so. Many or most practicing geomorphologists, whether or not they acknowledge it, proceed with the tacit assumption that beneath the apparent complexity of geomorphic systems we can find simple principles and behaviors that can be deciphered if only we ask the right questions and apply the proper tools. In other words, most of us seem to proceed with the faith that the highly complex systems we study ultimately produce simple patterns, at least when those systems are considered on space or time scales that are larger than the characteristic scales of the processes concerned. The fact that many nonlinear, highly dissipative systems display regularity and robust behavior on such scales lends support to this idea.

That complex systems can yield simple and, many times, highly organized solutions has strong intellectual roots. Such behavior is to be expected in highly dissipative, non linear, systems. In geomorphology the prime motivator is the observed regularity of landforms. There are numerous examples of such regularity. River networks, despite their variability in pattern, possess many common properties worldwide [*Rodriguez-Iturbe and Rinaldo*, 1997] regardless of variations in geologic setting. Glacial erosion produces U-shaped valley forms that are so characteristic that the existence of Quaternary alpine glaciation can be deduced simply from the shapes of valleys. In semi-arid parts of at

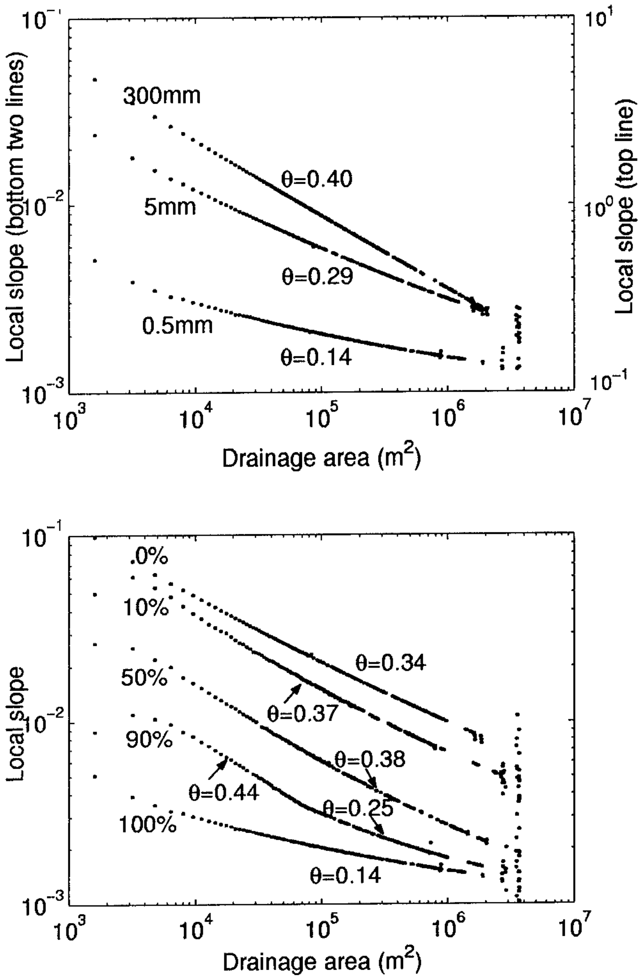

Figure 6. Slope-area relationships resulting from a landscape evolution model using homogeneous or heterogeneous (mixed sand and gravel) sediment [*Gasparini*, 1998; *Gasparini et al.*, 1999]. The top panel shows how the θ value changes with different homogeneous sediments. Finer sediments produce less concave (small θ) profiles than coarser sediments. Natural rivers exhibit more consistent concavity with values generally fluctuating between 0.3 and 0.5 implying significant concavity in the profiles. The bottom panel shows slope-area relationship from basins developing in heterogeneous sediment substrates. Results are shown for substrates that have 10%, 50% or 90% sand of the same median size (balance is gravel), and for the end members, all gravel (coarse, 0%) or all sand (100%). It is interesting to note that any heterogeneous substrate results in a slope area relationship with values close to 0.4. In the case of the 90% sand, there is a downstream region of sand dominated surface layer resulting in a less concave (θ = 0.25) region. This is observed in nature. The model with heterogeneous substrate adjusts its surface (erodable) layer and slope to remove the supplied (via uplift) substrate sediment and to achieve this equilibrium. The slope changes with contributing area so that the concavity corresponds to that observed in nature, with little variability.

least three different continents, arroyos and arroyo-like gullies with rectilinear cross-sections have formed in historic time, despite many differences in the details of vegetation, soils, and climate. These are just a few of many examples in which a complex array of processes produces landforms that are in certain respects distinctive and independent of many of the governing details. This regularity of form, and the belief that it is explicable, is one of the fundamental guiding assumptions in geomorphology.

Given that we expect complex natural geomorphic systems to yield simple forms, it would be natural to expect that models of these systems would behave the same way. There is a common tendency in model-building to begin with a highly simplified description of a system, and then iteratively add components in search of greater realism.

Is this trend toward increasing model complexity warranted? We believe that it is, so long as the level of complexity in a model does not exceed our ability to understand it. The process of model-building inevitably involves a trade-off between fidelity (to nature) and parsimony (low parameter space and ease of understanding). By adding successive layers of detail, one is effectively hypothesizing that there will come a point at which no further details are needed. In other words, building models of complex geomorphic systems is one way to test our faith in the ultimate simplicity of these systems. If this hypothesis is correct, we should be able to reach a point where adding extra processes and corresponding parameters to a given model either no longer has a significant influence on the behavior in which we are interested, or actually simplifies a model's behavior. One could also proceed by successively pruning away from a model those aspects that have little or no influence on the outcomes. In either case, for any given model and any given behavior of interest, there should exist a point of "optimum complexity." Optimum complexity would correspond to the minimum set of equations and parameters for which the addition of extra feedbacks, processes, or parameters produces negligible influence on the behavior of interest. Note that there is no guarantee that for any particular geomorphic system the optimal model will involve a small number of components or parameters, though given the nonlinear, dissipative nature of geomorphic systems this is a likely outcome for many. Once identified, an optimally complex model should explain the origins of regularity in the system of interest.

There are at least two ways in which a natural complex geomorphic system can yield simplicity and/or regularity. The first and most obvious is the case in which the system's behavior is dominated by just a handful of its components. This is the assumption, for example, behind the concept of a transport-limited hillslope [*Carson and Kirkby*, 1972]. If the rate of regolith production on a hillslope is at least as fast as its removal, then the removal of regolith should (at least according to this theory) no longer depend on the processes responsible for generating that material, however complex such processes and their feedback may be. In such cases, the necessary and sufficient conditions for a given outcome (e.g., the shape of a hillslope) encompass only a limited subset of the conditions that actually exist in a particular situation. Mathematical models in geomorphology can play an important role illuminating physically plausible necessary and sufficient conditions for a given phenomenon (see, for example, the study of meandering river avulsion by *Slingerland and Smith*, 1998).

The second, and less intuitive, possibility is that simple outcomes of a geomorphic system arise not in despite of, but because of a large number of interacting processes. This concept has been widely explored in the context of nonlinear systems analysis and to some extent in geomorphology [e.g., *Phillips*, 1996; *Slingerland*, 1990; *Rodriguez-Iturbe and Rinaldo*, 1997; *Favis-Mortlock et al.*, 2000]. There is no point in reviewing that literature here. Instead, we simply wish to draw attention to a related concept that has potentially important implications for how and why we use models in geomorphic research.

Recent research in nonlinear systems tells us that there are potential cases in which increasing the number of processes in a system can, paradoxically, lead to a reduction in the range of potential outcomes. One example of this in a geomorphic system is the analysis by *Gasparini* [1998] of the role of river sediment sorting in drainage basin evolution. Until the late 1990's, most mathematical models of river basin evolution assumed, for the sake of simplicity, uniform sediment size. These models were formulated with the reasonable but untested working assumption that sediment size variations are a detail that exerts only a minor influence on large-scale drainage basin morphology and dynamics.

Gasparini [1998] examined the implications of this assumption by incorporating *Wilcock*'s [1997, 1998] sand-gravel bedload transport formula within a model of drainage basin evolution (the model is described by *Gasparini et al.*, 1999). For cases in which the substrate is homogeneous (either all sand or all gravel), the analysis predicted that the resultant longitudinal river profile concavity should depend strongly on relative grain size (Figure 6a). (Note that this result is not unique to the Wilcock transport model but applies generally for any transport relationship in which total transport rate varies as a near-linear function of slope and discharge over and above a threshold for entrainment; for discussion and derivations, see *Howard*, 1980, 1994; *Tucker and Bras*, 1998). In cases with heterogeneous sediment, however, the model predicted a much narrower range of possible morphologic outcomes (Figure 6b). The addition of complexity—the potential for adjustment in bed texture in addition to gradient at each point—led to a *reduction* in the dynamic range of predicted channel concavity values, compared with either the uniform case or with models in which texture change was simply imposed as a boundary condition [e.g., *Snow and Slingerland*, 1987; *Sinha and Parker*, 1996]. The physical explanation lies in a trade-off between entrainment of the gravel fraction (favoring high concavity) and equal transport of the sand fraction (favoring lower concavity), and the fact that gradient is only one of two (model) variables that can adjust. At each point in the network, gradient and grain-size composition adjust to provide the correct (imposed) transport rate of both size-fractions [*Gasparini et al.*, 1999]. In headwaters, where shear stress is lower, the gravel fraction must be larger to provide sufficient rates of gravel transport. Further downstream, under higher discharges, higher shear stresses lead to a lower transport capacity differential between the two sizes, and therefore the gravel fraction in bed sediment decreases. The reduction in mean grain size downstream results in a moderate degree of longitudinal profile concavity [*Snow and Slingerland*, 1987; *Sinha and Parker*, 1996]. This behavior holds regardless of the relative fractions of the two sizes in transport.

No doubt one could criticize the details of this particular study, but the lesson remains that there may well be many cases in geomorphology in which mutual adjustments among multiple variables lead to simple, "emergent" outcomes [e.g., *Haff*, 1996; *Favis-Mortlock et al.*, 2000]. Such "emergent simplicity" may in fact be partly or even largely responsible for the apparent regularity of many landforms.

One could argue that there is a risk in this process of ending up with "optimally complex" models that are at once too complex to understand (too many parameters) yet are still too simple to adequately explain nature—in other words (to paraphrase a colleague), we risk ending up with two things, rather than one thing (nature), that we do not understand. Indeed this is a risk, but we contend that nonetheless the trend toward increasing sophistication in geomorphic models is not only valuable but necessary to progress. Obviously, to construct a model that defies understanding is an empty exercise. But to go to the limits of model complexity—to build models that are as sophisticated as we can hope to deal with and still make sense of their behavior—is the one of the only pathways through which we can rigorously challenge our preconceptions about which details are likely to matter, through which we can hunt for instances of emergent, counter-intuitive behavior that would have been difficult or impossible to predict a priori, and through which we can identify new testable predictions.

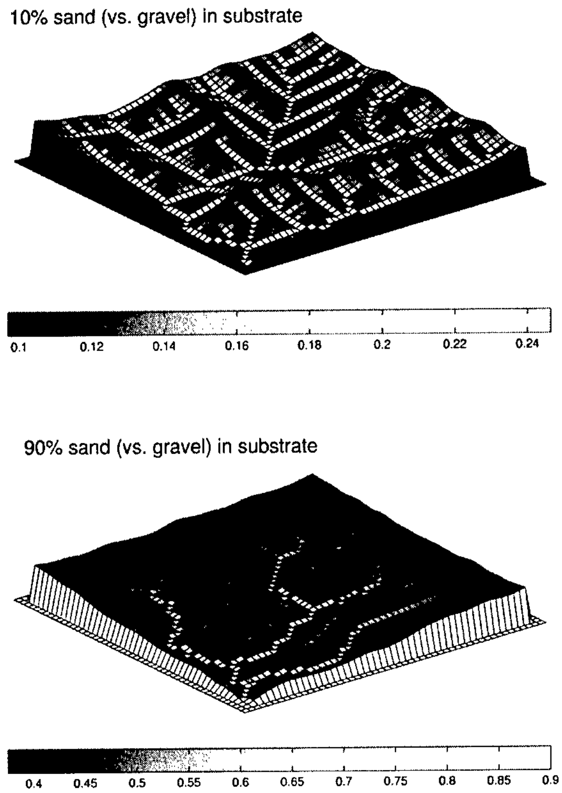

Figure 7. Two drainage basins colored by the proportion of sand (vs. gravel) in the surface layer. Light colors indicate more sand. The 10% and 90% labels in the two figures indicate the proportion of sand in the substrate material and hence the initial surface content of sand [*Gasparini et al.*, 1999]. Both basins, regardless of substrate mix result in downstream fining. Upper reaches of the basins are coarser than downstream reaches. In the case with a substrate which has 10% sand (top panel), the majority of the surface layer is finer (has a higher sand fraction than the supplied substrate) but still the downstream is finer than the upstream (24% vs. 10% sand). In the case of a sandy substrate (bottom panel), the surface layer coarsens overall but still fines downstream with the bottom at 90% sand and the top at 40% sand. This downstream fining occurs in an experiment where the basin is in equilibrium; there is no abrasion and no net deposition contrary to previously held beliefs about the causes of downstream fining.

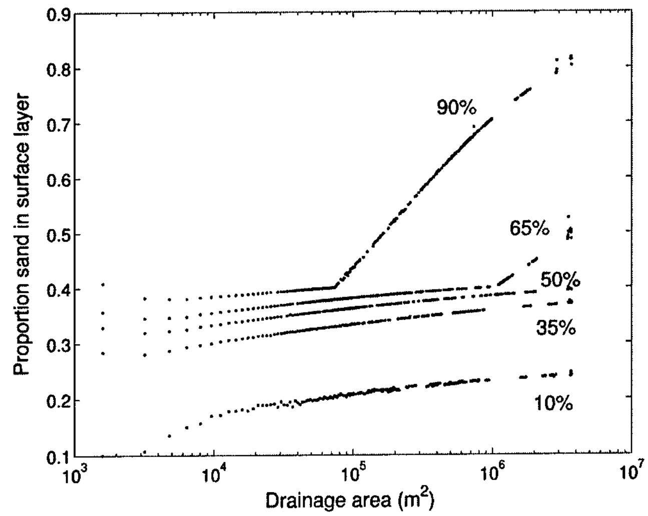

Figure 8. Relationship between proportion of sand in the surface layer and drainage area in five basins with different substrate textures (labels indicating 10% to 90%) [*Gasparini et al.* 1999]. This figure reinforces the results discussed in figure 7. Downstream fining always occurs in the surface layer of a computer simulated equilibrium erosional environment, no matter what the mixture of sand and gravel is. The figure also shows that for large enough basins relative to the proportion of sand in the substrate there is a point where the reach becomes sand dominated and the fining occurs at fast rate. A corresponding break in the slope area relationship will be observed, as was shown in Figure 6. The results shown in Figure 6 and 8 can be predicted analytically.

6. COMPLEX MATHEMATICAL MODELS RESULTS SHOULD AGREE WITH GUIDING PRINCIPLES OF BEHAVIOR

Most research in geomorphology, as in other disciplines, is underpinned by what *Brown* [1996] calls guiding assumptions. These are essentially models (either conceptual or quantitative) that underlie the types of questions we ask, and the way we interpret the results. To take a geomorphic example, Davis' geographical cycle provided the guiding model for a lot of geomorphologic research in the first half of the 20th century. Such guiding assumptions are crucial to progress, providing the framework within which we pose questions, design research strategies, and interpret observations [*Rhoads and Thorn*, 1996]. In the context of modelling, one often expects mathematical models to be consistent with their guiding assumptions. Indeed, one of the most common uses of models in geomorphology and geophysics, quite reasonably, is to provide numbers to support a given argument (e.g., the use of a lithosphere flexure model by *Watts et al.* [2000] to make a case for erosion-driven isostatic uplift in southern Britain, to name just one example). Models used in this way serve in a sense as quantitative expressions of our guiding assumptions (e.g., that flexural isostasy is an important source of epeirogenic movements), and their behavior is therefore consistent with those assumptions. At the same time, however, clearly one of the most important modes of scientific progress consists of refining or undermining guiding assumptions [*Brown*, 1996; *Kuhn*, 1962], and mathematical models can play a valuable role in this process as well.

Guiding assumptions in geomorphology are often conceptual and qualitative in nature. Sometimes they are well-known and carefully articulated theories (like the geographical cycle example); in other cases, they are unspoken but widely held ideas that influence the way in which most of us interpret evidence and the questions we choose to ask. Mathematical models in geomorphology can and should play a vital role in challenging both types of guiding assumptions.

How can a mathematical model challenge guiding assumptions? Consider the study by *Slingerland et al.* [1996] of paleo-ocean circulation in the Cretaceous western interior seaway. This shallow seaway extended northward into the interior of North America from the Gulf of Mexico, reaching the Boreal Ocean during the highest sea level stands. Before their study, the prevailing view was that circulation within the seaway must have been clockwise, just as it is in the Atlantic and Pacific today. The apparently sensible assumption of clockwise circulation functioned, in effect, as a guiding assumption behind interpretations of sedimentary strata deposited in the seaway. Yet when *Slingerland et al.* [1996] ran a series of simulations with an ocean circulation model using the reconstructed seaway paleogeography, they found that the model suggested counterclockwise circulation. In retrospect, the differences between the seaway and modern oceans were easy to understand: the seaway was about 40 times shallower and 3 times narrower than the modern northern hemisphere ocean basins, and, most importantly, the freshwater influx would have been a much greater proportion of the total ocean volume. The numerical modelling led to a new and radically different guiding assumption about seaway circulation. In the process, like any good model, it provided an explanation for certain observations—paleo-current orientations and the distribution of tropical foraminifera—for which the earlier model could not account.

The aforementioned study of *Gasparini et al.* [1999] on the origins of downstream fining provides a second example. Downstream fining refers to the common tendency for the mean size of river-bed sediment to decrease systematically downstream [*Sternberg*, 1875; *Yatsu*, 1955]. During the 1990's the origins of downstream fining attracted widespread research interest, partly because of its importance to interpreting ancient sedimentary rocks [*Paola et al.*, 1992; *Hoey and Ferguson*, 1997; *Seal and Paola*, 1995; *Robinson and Slingerland*, 1998a, b]. One of us (GT) first became acquainted with the problem in discussions with two colleagues who were working on the subject in the early 1990's. At the time, it was believed that the one case where downstream fining of bed sediment would not occur was for a steady flow of sediment through a river system in which the relative proportions of size-fractions in transport remained uniform downstream (e.g., the proportion of sand-sized material in transport was constant along a channel), assuming clast abrasion was negligible. In the mid-1990's, when together with N. Gasparini we incorporated multiple sediment size-fractions within an existing model of drainage basin evolution, the research aims had nothing to do with downstream fining. The goal was simply to add a potentially important component, not addressed in prior models, in support of a study of climate change impacts on watershed geomorphology. One of the first exercises with the new model was to run a test case in which a substrate composed of a spatially uniform mix of sand and gravel fractions was subjected to a steady rate of baselevel fall at the outlet point of a simulated drainage basin—very much like the test case that we thought we already understood (Figure 7). After a period of time under these conditions, a model of this sort will reach a state of equilibrium in which the rate of erosion at each point exactly balances the rate of baselevel fall. This condition logically implies two other conditions: first, that the size-distribution of the sediment flux is equal at all points in the drainage network, and second, that sand and gravel are entrained and eroded in proportions equal to their relative proportions in the underlying substrate at all points in the basin. To our surprise, the model still produced clear, systematic downstream fining in the active transport layer, even though both conditions were met (Figure 8).

This simple "numerical thought experiment" revealed flaws in our original reasoning. We had implicitly assumed that uniformity of size-fractions in the sediment flux implied uniformity in the bed-sediment composition. But there is no logical reason why this should necessarily be true. Where size-dependent entrainment thresholds exist, bedload-transport theory (in this case, *Wilcock*'s 1997, 1998 model) implies that relative entrainment rates among grains of different sizes should depend on two things: bed shear stress and bed-sediment composition. If bed shear stress varies systematically downstream, then bed-sediment composition must also vary if relative entrainment and transport rates are to remain uniform. The earlier thought experiment had ignored downstream changes in bed shear stress due to increasing discharge. The model alerted us to the fact that the null hypothesis – that downstream fining of bed sediment can occur independently of any of controls that are widely believed necessary for it (abrasion, selective entrainment, and selective deposition)—could not be rejected as easily as many had assumed.

These examples illustrate how the process of formulating and "playing with" a mathematical model can generate unanticipated surprises that reveal errors in conceptual reasoning. By predicting new types of behavior, such "surprises" help to enhance insight and generate new hypotheses.

Should model results always "make sense?" In retrospect, of course they should. Should we expect them to support our initial ideas, and be disappointed when they fail to do so? We definitely should not react that way. One of the best things a mathematical model can do is to surprise us and challenge our ideas.

CONCLUSIONS

Scientists have always relied on models. Observations, data interpretation and analysis depend on a context, a series of references, and a body of knowledge that are models of some type or another. Science cannot occur in the absolute, its progress is always relative to existing ideas and concepts. Conceptual and mental models are quite common, many times implicit in our actions, sometime explicit. Mathematical models are just another expression of the need to idealize, represent and visualize reality.

Geomorphologists, geologists and hydrologists have always used models. They must use models to make progress, and that includes mathematical models. Unfortunately an artificial schism between modelers and experimentalists (or "observationalists") exists in our fields and in many other scientific endeavors that are founded on data interpretation and observation. This schism is founded on bias, misinterpretation, and myth. The schism is perpetuated by misuse and misrepresentation of data and models. In this paper we have tried to address six of those myths and illustrate, mostly with our experiences, why we think mathematical models are useful and necessary tools of the trade. For the time being, geomorphologists may not have to use mathematical models directly but they cannot afford to ignore them and their users.

We predict that in the not too distant future all geomorphologists will be users of mathematical models at some level. Modeling will be a necessary tool of field researchers and theorists alike.

We always want to use "physically based" models. We have tried to argue for a broad definition of physical models, away from sometimes misleading mechanistic rigor. The argument is that few physical principles are immutable and absolute. We do argue for models based on principles and processes that can be generalized and that depend on parameters that have an observable interpretation.

It could be argued that verification is impossible given that reality is imperfectly known. Certainly models accept more or less confirmation with existing data, limited by our ability to observe. Beyond that problem with semantics, we argue that our field is such that deterministic verification of any model outcome is nearly impossible. We can hope to confirm the behavior of the constituent processes of models and to confirm model output in a distributional, statistical, sense. We can preserve behavior and general features expressed as moments of distributions, but it is almost impossible to exactly reproduce a feature of nature that is the outcome a highly non linear system having poorly known initial and boundary conditions.

Even unconfirmed or partially confirmed or "verified" models can be useful. First, unverified models serve as deductive tools that help guide the search for new observations. Second, the process of model-building itself is a valuable exercise that forces rigor in our hypotheses and interpretations. Third, models can highlight errors of understanding and concept. Fourth, models may lead us to unpredictable insights, and fifth, they serve as virtual laboratories.

Rejected models are not necessarily a waste of time or a failure. In fact the nature of science progress is such that all models will, hopefully, be proven less than ideal as our knowledge increases. The literature is full of "failures" that have been invaluable in terms of guiding our thinking and framing our search for new knowledge. In a sense rejected models are stages of maturation for our science.

We believe that models must progressively become more complex as we codify increased knowledge and observations. But complexity of construct does not necessarily imply complexity of output. In fact, we subscribe to the idea that nature's expression is simple, although the result of many complex interactions between processes at all time and space scales. The complexity of our models should be modulated by our ability to understand their behavior.

Finally, the best models are those with outputs that challenge our preconceived ideas. Models should be didactic tools. Their output should not be constrained to reproduce existing ideas. If their output challenges existing ideas, then it behooves us to look deeper into the results before rejecting the model as nonsense. We should always be ready to admit we were wrong.

In summary, in response to the loaded question: are you a modeler? We answer a resounding "yes" and argue that all should answer similarly.

Acknowledgements. The work reported in this paper has been funded through a variety of grants. First we will like to acknowledge the support of the Army Research Office, Terrestrial Sciences, and its director Dr. Russell S. Harmon (DAAH 04-95-0181, 33959-EV, DAAD19-01-1-0513, and DAAD19-01-1-0615). Additional help was received from the Army through the Construction Research Engineering Laboratories (DACA 88-95-R-0020, DACA42-00-P-0175, DACA-88-95-C-0017). Dr. Bras also acknowledges the support of the Italian National Research Council of Italy, through its cooperative agreement with MIT. The views, opinions and/or findings contained in this paper are those of the authors and should not be construed as an official position, policy, or decision of the sponsors unless so designated by other documentation. The authors would like to thank the reviewers and the editors for excellent input. In particular, we want to acknowledge the extraordinarily useful input of Dr. Brad Werner.

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