REVIEWS ON THEME 4

Impacts and Management of Steeplands Erosion

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Our knowledge of geomorphological systems is very fuzzy and because of this there is a real lack of geomorphological theory based on physical principles rather than simple log/log regressions of the variables that we happen to measure. I have been somewhat concerned this week about the qualitative way in which much of the material has been presented, even where the understanding demonstrated in the papers has been based on quantitative evidence. We have seen lots of gut feelings of how things work, and we have seen lots of land management techniques based on those gut feelings of how things work. We have seen very little of the structuring of knowledge into predictive models based on physical principles.

Why is this important? In any geomorphological investigation we have to make gross assumptions about the operation of the system. If geomorphology is to progress as a science it is important that those assumptions be stated and understood in a clear and rigorous way. Even in some highly mathematical papers this is rarely done, the assumptions are often implicit in the analysis. However, in more qualitative analysis techniques, such as the evaluation of landslide hazard classes, there are many assumptions that will be necessarily implicit because of the subjective judgements involved. This has a number of consequences. We have already been told how such evaluations can be expected to be operator dependent, and of the difficulties of weighting the importance of various classification indices. Obviously, if we get that weighting wrong or neglect some factors (perhaps regolith geology or changes in root strength following tree harvesting) we may be very wrong in such evaluations. More importantly, in my view, without some underlying physically-based structure on which to base this type of analysis, we may be very wrong in interpreting the success or failure of our evaluations. Without this understanding, progress in our science will be slow.

The major contribution of models in this context is to force us to think about assumptions, about interactions in the system, and to recognize those places where we have least understanding. There is a useful feedback loop in this process between models and measurements. A model or theory will often suggest additional measurements that might be made to evaluate one or more critical assumptions.

Whenever possible, we should base our models on well-defined physical principles, rather than on empirical relationships (such as the Universal Soil Loss Equation) that to a large extent obscure cause and effect relationships. The most fundamental physical principle that we work with is the continuity equation. The basic model that we work with is the sediment budget, as demonstrated by Kelsey *et al.* in this session. It has taken us a long time to reach this level of sophistication at the catchment scale. However, I think that we should be able to do better than that now and start to look at the operation of the system processes in response to external influences. I think we can for single processes, and in principle for interacting processes. The difficulty is, as always, getting enough data to calibrate and validate a model. We know that any parameter required by a process model is going to be subject to considerable uncertainty. This is, if you like, the characterization in model terms of the statistical variance of gut feelings.

What we can do with a physically-based model, and this is very important, is to try to *quantify* the variance in the predictions due to parameter uncertainty in the model. An example of this is the paper by Ward *et al.* in session 1. If parameter uncertainty is taken into account, then the model has a stochastic component and the predictions will be probabilistic.

Let us look now at the specific problem of evaluating the impact of steeplands erosion on human activities. In this case two predictions are required: the expected value of erosion without land use change; and the expected effects of the proposed human activities. These are both stochastic problems because the sequence of natural forces over time is essentially a stochastic process. Thus even with a qualitative predictive tool, or a purely deterministic model, we can only at best predict the probabilities of possible impacts. Uncertainty in the model will modify those probabilities but will reflect our lack of knowledge of the system directly. This is what we should be aiming at.

However, models are not going to solve all our problems. An incorrect model is not going to give us correct results. Even if we are satisfied that we have a reasonable model of, say, landslide occurrence or bedload transport rates, the span of probable outcomes, given likely sequences of rainfall/runoff events and estimates of ranges in parameter values, may be so wide that the results are useless to the land manager. If we are scientists, we should not be discouraged by this but should develop ways of improving those predictions. The important point is that we should recognize that uncertainty, and develop ways of passing on that knowledge to the manager, who at present certainly doesn't want to hear about probabilities.

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Erosion control is an essential part of a land manager's job. Many government agencies and private landowners are involved in erosion control activities, ranging from soil conservation to land reclamation. In many cases, however, land managers apply erosion control techniques without fully understanding the erosion processes responsible for the problem, or without monitoring the impact of the management treatment.

Erosion control treatments to date are largely based on years of field experience and empirical data. Methods that work in one area are applied to different sites and may or may not be effective. Such an approach is expensive, time-consuming, and not entirely successful, and it does not expand our knowledge of the processes involved. A better understanding of both the mechanics of a process and the links between processes would advance erosion control technology. We could then evaluate the conditions at a specific site, predict the extent of erosion at the time, and design an erosion control program to treat those specific problems. An approach based on theoretical considerations would be an improvement over the present "hit-or-miss" method of erosion control.

To treat fluvial erosion, for example, we must define links between processes, such as the weathering of a particle at a site, its initiation of movement, and its transport off that site. Before we can effectively control rill and gully erosion, we must first understand under what physical conditions rill formation will occur. Critical velocity, shear stress and particle size must be quantified for this analysis. Unfortunately, at the present time the mechanics of rill formation is poorly described, and consequently treatments for rilling slopes are based on empirical judgments.

On a larger scale, human life and property are crucially affected by the occurrence of debris flows, and so a predictive model of debris flows is socially as well as scientifically desirable. Such a model should consider the relationships of debris flow occurrence with climatic events and hillslope processes.

Knowledge of the mechanics of debris flows has been expanded by Dr Takahashi and his colleagues, and his work forms an excellent basis for further investigations. He described the conditions under which the initiation of debris flow movement will occur. In addition, to advance any predictive model of debris flows, several other processes must be described, such as the transfer of material from hillslopes to channels before a debris flow occurs. The rate of supply of debris to a channel is controlled by the rate of weathering of the regolith, hillslope processes, and climatic conditions. The ordering of climatic events will affect the volume of debris accumulated in channels, and thus influence the volume of material transported by and deposited from a debris flow.

Many hillslope processes are commonly thought to be directly modified by vegetation manipulation, and hillslope erosion problems in many countries are treated by revegetation. In reality, a bare hillslope may appear to be in better condition once it is vegetated, but the transfer of material from hillslopes to channels may not be significantly influenced by the revegetation. For example, severe gullying may still occur on logged sites even though restocking of trees has taken place. Revegetation is useful in controlling sheet erosion, and in that way decrease the supply of fine sediment to a channel. Nevertheless, many sedimentation problems are caused by mass erosion and gullying, both of which supply coarse material to stream channels. In many cases revegetation does not directly affect the amount of bedload-sized sediment entering a stream. Before massive revegetation programs are implemented, specific sources of sediment production should be evaluated to see if revegetation will actually ameliorate the erosion problems in question.

A related aspect of understanding erosion processes is understanding how our actions affect the processes themselves. When we alter one aspect of the sediment routing system, we must be aware of the implications to the rest of the system. In many cases a feedback mechanism exists, whereby human modification will lead to process modification. A close monitoring program should be an integral part of any erosion control effort in order to document how the erosion control methods affect the hillslope-stream relationship, as well as to quantify the effectiveness of the techniques.

Vegetation manipulation, for example, may indirectly affect sediment transfer processes. Changes in vegetation may result in changes in soil properties, and in soil erodibility. An increase in root strength due to reforestation of hillslopes may increase slope stability on marginally stable sites. Evapotranspiration rates from a basin may change following vegetation modifications, thus altering the hydrologic regime of the basin. A change in water yield will in turn change the rate of sediment delivery to a channel and the rate of sediment transport downchannel. Thus human intervention can change not only the supply of sediment to a channel, but also the timing of the sediment transport. An effective monitoring program should address all effects resulting from intervention.

In a natural system, sediment moves down a channel in a series of discontinuous transport events. Between events it is stored temporarily in the channel bed, behind log jams, in gravel bars, and in terraces. Some erosion control treatments alter sediment routing by creating new temporary storage units.

Check dams are a common erosion and flood control device that store sediment and form a new base level for the channel. Channel bed scour downstream of check dams is a common problem, and is indicative of a change in the transport regime of the stream. Sediment supply downstream of the dam is diminished, and the channel scours to replace the sediment load in the stream. A new design of check dams, suggested by Li *et al.* may circumvent this problem. Alternatively, if a check dam fails under a high-magnitude flood, a large supply of sediment is suddenly mobilized, and the temporary base level of the channel is destroyed.

Sandpockets, as described by Smart, also form a new sediment storage unit. In this case they control the location of deposition of volcanic debris on an alluvial fan. Sediment may be deposited in sandpockets at a rate different from the formation of natural alluvial fans. If the dikes fail under high magnitude events, there may be a sediment release problem. In any case, the natural sediment routing scheme is somewhat disrupted. Sandpockets perform a socially desirable function, but they should be monitored to assure no undesirable side effects result.

Widespread landscape alterations, such as those resulting from the US Redwood National Park land rehabilitation program, change the sediment source areas and sediment transport system of disturbed watersheds. New drainage networks, channels and hillslopes are formed during rehabilitation activities. This new landscape will adapt to conditions imposed upon it. These adaptations must be closely monitored to assure no new erosion problems result.

In some cases, changes in a watershed that occur in response to erosion control techniques do have undesirable side effects which require further modifications. For example, scour below consolidation dams can be corrected by the construction of another dam downstream, but the second dam will eventually require the protection of a third dam, and so on. The creation of sedimentation basins on floodplains may, on a short-term basis, fulfil a desirable function (filling in a swampy area and preventing sediment from entering a channel), but on a longer term, it may prove undesirable (decreasing floodplain storage capacity and increasing the flood potential downstream).

In conclusion, to evaluate erosion control structures in a broad perspective, we need to place them in a sediment routing scheme. Several factors should be quantified before deciding what erosion control treatment is best suited for a problem: the location of sediment source areas, the volume of sediment eroded, transported, and delivered to the channel under present conditions, the magnitude and frequency of sediment transporting events, the present rate of sediment deposition or scour, an estimate of sediment delivery and transport after treatment, the projected rate of sedimentation, and the projected useful lifespan of erosion control structures. In some cases, it may be most effective to treat sediment source areas directly to decrease sediment production; at other sites, controlling erosion of temporary sediment storage areas in order to decrease the sediment supply downstream may be more beneficial. Monitoring the effects of erosion control treatments will not only assure that we are addressing an erosion problem correctly, but, in addition, will improve our knowledge of erosion processes.

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My first comment concerns the papers in the final session, Impacts and Management of Steeplands Erosion, and then I have a more general comment on the nature of papers in the entire symposium and on the tenor of discussion during this splendid meeting and the associated field excursions. We have had a valuable, stimulating opportunity to assess our knowledge of erosion and sediment transport in the Pacific Rim. This assessment suggests to me some directions in which I, and perhaps others, should channel research efforts. It also suggests the need for a fundamental alteration of our style of doing research.

BIOMECHANICS OF EROSION AND EROSION CONTROL

Papers on the role of vegetation in stabilizing hillslopes and those on programs of erosion control reminded me of how little we know about the biomechanics of erosion and erosion control. The state of the art in analyzing root strength as a stabilizer of hillslopes against mass wasting is represented in this room by the presence of O'Loughlin and

Ziemer, whose work is both interesting from a mechanical point of view and of immediate relevance for land management. Ziemer's paper in this symposium was welcome not only because of its value in analyzing forest management problems but because it could be used in theoretical geomorphology for modelling the effects of climatic change, vegetative succession, and other time-dependent factors in erosion and hillslope form.

However, much remains to be done in the development of a sound physical theory about root strength, so that measurements of tensile strength can be converted into a form that can be incorporated into slope stability equations. This will require analysis of the stress field in a root-laden soil. Such analysis would also aid the interpretation of results from *in situ* field measurements of root strength. The present approach of slow, expensive field measurements cannot be conducted quickly enough to answer land management questions that might arise, for example, from the introduction of rapidly growing trees for fuelwood production or industrial materials. The work of O'Loughlin and Ziemer has prepared the way for an exciting avenue of theoretical and applied research.

Poor appreciation of the role of vegetation in erosion control was particularly apparent after the recent eruption of Mt St Helens. In the devastated zone, the forest cover was removed and an ash cover varying in surface texture from silt to sand dramatically lowered the infiltration capacity. Rapid sheetwash and rill erosion followed. A program of aerial seeding with grasses was instituted, but there was no theory, or even relevant experience, upon which to predict whether the seeding program would have any effect on sediment yields, which were supplied mainly through undercutting and collapse of rill walls in the ash cover and of large canyon walls on the debris avalanche in the North Fork Toutle valley.

Another example of our limited understanding of the biomechanics of erosion control is apparent in the literature on the effects of sparse vegetation cover on runoff and erosion in semi-arid rangelands or disturbed land surfaces elsewhere. Land management text-books assert that increasing ground cover density promotes infiltration and reduces runoff. In dry rangelands, such an article of faith is not well-proven and, in fact, several studies have been unable to document any such relationship when the ground cover is sparse. Why is there such confusion and lack of predictive capability? This seems to be another subject for which a fuller understanding of the physical mechanisms involved would pull together the disparate observations on various covers, soil types, and rainfall intensities. The results would indicate the range of conditions under which a sparse vegetation cover can promote infiltration, and where it has no significant effect.

GENERAL CHARACTERISTICS OF SEDIMENTATION RESEARCH IN THE PACIFIC RIM

Throughout this meeting and the associated field excursions, both formal presentations and informal discussions have reinforced a perception that I have long held about research into erosion and sediment transport in the Pacific Rim steep lands of North America. Most of us are pursuing descriptive investigations dominated by the short-term needs of land managers, and we are ignoring the more fundamental, analytical studies which, combined with mathematical modelling, would lead to a richer, more productive science and would also serve the land manager better if attention were placed on putting research results into a usable form.

One symptom of the emphasis on description is an obsession with world records. We keep telling one another that the lands around the Pacific are taller, steeper, wetter, more unstable, more variable, eroding faster, and generally less tractable for comprehension or management than anywhere else in the world. Even if this were true, it is surely of only passing interest as we focus on understanding erosion and sediment transport. Yet, so often in this immature science, we congratulate one another for having discovered these records and for proceeding only to the vaguest and most qualitative explanations.

A surprising number of scientists in this field revel in complexity. During this symposium, we have observed gleeful responses and satisfied, nodding smiles when speakers made statements such as "The Pacific steep lands are more complex than cornfields in Iowa; the Universal Soil-Loss Equation doesn't work out here!" or "This is just another example of a theory concocted by a . . . academic with no data that doesn't work out here!" I am sure that the offending academic and the devisers of the Soil-Loss Equation would encourage us to develop our own analytical tools for the study of erosion and sediment transport in a manner relevant to active plate margins, but to do so in a way that will lead to *useful generalizations* that broaden our understanding and our capacity for prediction.

Science progresses through the making of generalizations in the face of the complexity of nature. But if Isaac Newton had reported his reaction to a falling apple in the manner that we commonly use around the Pacific Rim, he would have described the gauging station by which he was sitting, the uniqueness of weather patterns during the preceding three years, the particular apple, and his plans to spend the next three years sitting there to observe other apples, in the hope that at the end of his data collection program, he or someone else would be able to decide what it all meant!

We can learn a great deal from the papers presented at this symposium by colleagues from Japan, which is no less complex than other parts of the Pacific Rim. Drs Ashida, Takahashi, and Ikeya have demonstrated that through the use of some simple mechanical principles one can gain powerful insights into geomorphic processes and can make useful predictive statements. Instead of complaining about the scatter, hysteresis, instability, and other annoyances in his suspended-sediment data, Professor Ashida analyzed some of the channel processes affecting variations in sediment transport. He developed a predictive model which can be examined objectively, which suggests some critical field observations, and which will be modified in other regions. It focuses analysis and field measurements on critical issues. Rather than showing the excruciating details of yet another debris flow that destroyed his instrumentation, Dr Takahashi used the results of two theoreticians (Isaac Newton and Ralph A. Bagnold) to develop and test a method for predicting the temporal and spatial occurrence of debris flows and their deposits. Dr Ikeya demonstrated how a few simple measurements of variables suggested by theory can lead directly to useful predictions of deposition on alluvial fans.

I suggest that the majority of us working on sedimentation problems around the Pacific Rim abandon our nihilistic attitude to theory and make a stronger effort to develop useful generalizations from our fieldwork. Of course, such a suggestion conjures up the dreaded word "modelling" — a task which we usually think can be left to young people who use computers and wear ties. In references to "modellers" there is commonly the implication that they form a separate group, and also that they do not genuinely understand what the field is like. Modellers, on the other hand, frequently refer to field studies as uselessly descriptive and unilluminating.

The split and its associated prejudices reminds me of episodes in "Gulliver's Travels". In the Brobingnag region of the profession, there are perceived to be clumsy, ingenious, rural giants, proud of their ability to withstand rain, snow, steep slopes, and raging rivers to collect data about "the real world". In a separate Laputian region there are absentminded, impractical, mono-maniacal theoreticians with one eye turned inward to observe their own thoughts, and the other pointed upward to the heavens. The Laputians encountered by Gulliver employed servants to strike them on the mouth and ears to remind them to communicate with one another.

Of course, it is easy to obtain agreement that such a dichotomy (however satirized) exists, and that some day things should be different. However, this symposium, in spite of being immensely stimulating and informative, has indicated that we are not bridging the division very quickly. In particular, the proceedings volume contains very little theory. Even with a liberal definition of theoretical papers, only ten qualify.

To alter this stimulation and to make more fruitful progress to change the way we conduct most of our investigations, we need to plan our next research projects with the express intention of developing some useful generalizations that will expand the theoretical framework of the science. More emphasis needs to be placed on planning field measurement programs that will generate the critical data required for modelling rather than just the data that are easy to obtain. Such planning requires that from the outset the study should be designed either by someone skilled in both theory and fieldwork or by a partnership of such interests.

The main stimulus which this symposium has provided for me is the encouragement to conduct field studies with more general significance and a firmer foundation in physical theory and experimental design than is characteristic of most of our studies on erosion and sediment transport in Pacific Rim steep lands.

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The topic of part one of theme four of this symposium is "Impact of Steepland Erosion on Human Activity". I found all five papers included in this part to be stimulating and certainly germane to the overall theme of the symposium. However, only the keynote address of Ashida and Takahashi explicitly discussed erosion impacts on human activity. In part, this anomaly probably reflects little more than the difficulty in organizing a large diverse group of technical papers into a manageable number of discussion themes. Nonetheless, I have to wonder if this might also reflect a tendency for hydrologists and geomorphologists to mention the practical aspects of their work in project proposals, but to ignore these issues in published reports. During times of economic contraction the immediate social benefits of research expenditures are closely scrutinized by funding agencies and taxpayers. Thus it becomes increasingly important to demonstrate how our work is directly relevant to hazard abatement and other social problems. An increased focus on applied research should be looked upon as a challenge and not as a nuisance or threat; the papers presented by the Japanese delegation to this symposium eloquently demonstrate that applied research can lead to new insights in fundamental hydrological and geological processes.

Ashida and Takahashi reviewed in some detail the issues that are central to this specific topic, and also provided a summary of the physical attributes of the Pacific Rim steeplands that make them particularly susceptible to erosion and sedimentation problems. They pointed out how erosion can have adverse impacts upon the productivity of upland sites as well as downstream resources. They mentioned the need for long term data sets to assess the "intermittency" of some processes, but suggested that highest priority for future research should be given to stochastic modelling with a physical basis. Embedded within their discussion was the idea that the adverse impacts from some processes are more easily mitigated than others.

Kelsey and his colleagues then expanded upon this idea and went on to point out that often the erosion processes that are most difficult to control are precisely the ones that you most want to control in order to minimize downstream sedimentation.

Pearce pointed out that one of the interesting implications in the paper by Tonkin and his colleagues is that past erosional activity has created hillslopes that are hostile environments for grazing animals and that grazing had not initiated alpine and subalpine scree formation. If time permits, I should like to discuss the merits of the strategic techniques employed in this paper in some detail. I believe that these techniques may be the only practicable means of obtaining the long time series of data that are needed to assess the geomorphic importance of some periodic (intermittent) geomorphic processes. Admittedly, the precision of stratigraphic methods is less than what process-oriented scientists might desire, but the time scale with which we must deal is such that these methods deserve at least as much attention as direct measurement.

The paper by Bergstrom and Schumm showed that the downstream progression of sedimentation events in some badlands mimics closely the downstream progression of events along some steepland rivers. Schumm's oral presentation indicated that the same progression was also discernible in physical modelling experiments at Colorado State University. This paper demonstrated that the complex manner in which individual events and processes are linked in space and time often makes it difficult to relate specific downstream impacts to specific upland events and processes.

Beven clearly describes how the manner in which events and processes are linked in time can cause further interpretative problems. But unlike some who merely beat their breasts in frustration, Beven went on to suggest an imaginative way of statistically manipulating available measurements to generate synthetic data sets that are of long enough duration to investigate how the geomorphic effectiveness of individual events can be affected by changing the order of events. In his oral presentation, Beven also encouraged the use of various modelling approaches, but points out that such models will probably always have large stochastic components embedded within them.

The papers as a group made me particularly aware of the fact that although all geomorphic processes operate at widely varying rates, it is often quite useful to distinguish between geomorphic processes that are *persistently* active and those that are active only *episodically*. As the timescale of concern lengthens, this distinction becomes less important. However, for most practical problems, episodically active processes are those that display brief periods of activity (seconds to days) separated by many years of inactivity. Bergstrom and Schumm use a more encompassing definition of episodic behaviour. Debris torrents and shallow debris slides are prominent examples of what I consider episodically active processes. Movement of many complex earth flows in the California Coast Ranges and Oregon Cascade occurs only during the wet season and displays dramatic storm-related surges of movement late in the season. Nonetheless, I consider this form of mass movement an example of a persistently active process. Similarly, despite strong seasonal influences and threshold conditions, I would consider the fill-and-flush badlands system described by Bergstrom and Schumm as another example of a persistently active suite of processes.

The distinction between persistent and episodic processes is useful in attempting to identify the linkages between upland and lowland sedimentation phenomena. This distinction is also important in designing experimental methods to document the behaviour of specific processes. For example, episodic debris slides influence stream sediment transport quite differently than persistently active earth flows, and one should probably use different techniques for studying these two different forms of mass movement. Persistently active processes often can be successfully studied by direct measurements and the duration of observation need only to be long enough to encompass a wide range of hydrologic conditions. On the other hand, episodic processes can rarely be successfully measured while movement is occurring, so one may want to utilize various physical and mathematical modelling schemes or indirect forms of observation. If the periods between episodes of activity are short (years to decades), much useful information can be gleaned from study of time sequential aerial photographs. If the periods between episodes of activity are longer (centuries or millenia), one must rely on stratigraphic approaches similar to those utilized by Tonkin and his colleagues.

A final point is although I strongly agree with the need to make greater use of both stochastic and deterministic models to understand geomorphic systems, I also believe that we must make greater efforts to assure collection of the long term data sets needed for calibration and verification of those models. Long term data sets are jeopardized on two fronts. The first is the obvious high cost of such programs. The second is the tendency for scientists to want to push on into new subject areas. We rush in to study the immediate impacts of floods, earthquakes, and other disasters, but rarely stick around to study the persistence of those impacts and recovery. If we are to get any long term data we will have to assign priorities to our needs.