# REVIEWS OF THEME 3

## Human Impact on Erosion and Sediment Yield in Steeplands

A. J. Pearce

*Forest Research Institute, Christchurch, New Zealand*

### INTRODUCTION

Five of the papers in this theme addressed the effects of natural and man-induced changes in vegetative cover on erosion and sediment yield from forest or brush-covered areas of the western USA and southeastern Australia. Two papers present predictions of soil loss and sediment yield using the universal soil loss equation (USLE) at the farm scale and at the drainage-basin scale. One paper outlines a perspective of natural time and space variations of erosion and sedimentation in North Island, New Zealand. Lastly, Pickup *et al.* describe the natural erosion and sediment yield behaviour of the Fly River, Papua New Guinea, as a first step in assessing the likely impact of rock waste dumping from large-scale open-pit mining.

### EFFECTS OF VEGETATIVE AND LAND-MANAGMENT CHANGES

Wells' study reproduces results found throughout the chaparral county of California, that wildfires dramatically increase both storm runoff and sediment yield. The well-known post-burning increase in dry gravel rates is hypothesised to depend partly on changes in soil particle size and clay mineralogy induced by the 500°C to 800°C soil temperatures at shallow depths during fires. Wells proposes an attractive theory of post-burning rill formation which depends on the development of water repellency at shallow depths in the soil during burning. Rills are proposed to develop from miniature debris flows in the saturated soil above the water-repellent layer, but rill down-cutting ceases once the water-repellent layer has been cut through.

Ziemer documents changes in root biomass and estimated soil strength over a synthesised 24-year post-logging period using data from intensive sampling of six logged areas and one unlogged area in the Klamath Mountains. The relative reinforcement of the soil at varying ages by both live roots and dead roots is estimated from dead and live root biomasses and measured relations between root diameter and root strength for a number of species. The contribution of live roots to slope reinforcement seems to be satisfactorily estimated by this procedure. Ziemer makes the conservative assumption that the strengthening effect of unit dead-root biomass and that of unit live-root biomass are the same. This assumption does not account for previously demonstrated exponential reductions in root strength with time after death, at least in the first five years after root death. Thus although 25% of root biomass remains after 8 years, root strength per unit biomass may be between 10% and 50% of that for recently-killed roots. The procedure could easily be modified to take into account the time-dependent behaviour of dead-root strength and I would like to see such a study done.

Rice and Datzmann assess the utility of an erosion hazard rating in predicting post-logging erosion in northwestern California, using simple and multiple regression techniques. Since the (erosion potential) hazard rating is used to modify logging methods and erosion prevention practices in specific logging operations, one would expect that the hazard rating would not be a very good predictor of *actual* erosion. If the hazard rating were perfect and operational controls based on it were uniformly and perfectly applied, there should be no correlation between the hazard rating and observed post-logging erosion. Rice and Datzmann find essentially this result but they conclude that operator performance is as great a source of variability as the site characteristics, and even with a prediction equation based on the sample data, are able to explain only about 40% of the variation in post-logging erosion. An important question to be faced in future studies of a similar kind is whether the success or utility of a particular hazard rating system can be assessed with data for only one (perhaps poorly controlled) treatment level. The differential success of several rating systems, and operator controls based on them, could, however, be compared with an experimental design similar to that used by Rice and Datzmann.

Burgess *et al.* compare the effects of chipwood logging and natural wildfire in dry schlerophyll forest on suspended and dissolved loads of streams. Suspended loads of logged streams were double those of unlogged streams for several months after logging, but little effect on dissolved load was found except in the first post-logging storm. Following the wildfire, both suspended and dissolved loads increased markedly although the post-fire rainfall events were small compared to the rainfall events immediately following logging. Because of the low frequency of the rainfall events following logging, Burgess *et al.* concluded that their results are likely maximum effects of similar logging. The small rainfall events following the fire suggest to the authors that the observed increases in suspended and dissolved loads are the likely minimum effects of burning. More detailed analysis of data from a longer post-treatment period for these study catchments should make a valuable contribution to forest land management in southeastern Australia.

Nolan and Janda analyse a large set of specific discharge and specific suspended-sediment discharge data for tractor-yarded, clearfelled areas and nearly unlogged redwood forest, in a region with naturally high erosion rates because of the underlying rock types, their tectonic history, and the present climate. The combined effects of increased streamflow and increased sediment supply from logged areas cause specific suspended-sediment loads to be about ten times greater than those from unlogged areas. Increased water and suspended-sediment yield effects persist for about a decade following timber harvesting. The authors provide a useful methodology for separating the effects of land management from the naturally high erosion and sediment transport rates, which should find wide application in other areas of high erosion rate where man-induced increases in erosion and sediment transport are suspected.

## PREDICTION OF SOIL LOSS USING THE USLE

Taneda reports plot-scale studies in Japan to obtain local values of the rainfall, soil erodibility, slope steepness, crop, and conservation practice factors of the USLE. The author provides a useful local calibration of the USLE, but further comparison of predicted losses and observed losses at the plot and field scale are needed to fully assess the usefulness of the USLE in predicting soil losses for Japanese agricultural practices.

De Vera applies the USLE, with standard tables and monographs from the US Department of Agriculture, to predicting soil loss from drainage basins ranging from 50–1200 km$^2$ in the Philippines. Soil losses have about a four-fold range and account for 80% of the variation in observed sediment yields which have about a 25-fold range. The USLE prediction gives a high statistical explanation of the observed yields but 12 of the 18 basins studied have predicted soil losses less than observed yields, and the mean observed yield is 1.5 times the mean predicted soil loss. The standard values of the USLE factors underestimate the true soil loss in the Philippines and local calibration will be necessary for the equation to be usefully applied to predicting soil loss itself. Nevertheless, De Vera demonstrates that the equation can probably be usefully applied as an empirical predictor of sediment yields from ungauged basins for the range of drainage basin sizes he has studied. The USLE factors, however, have little physical significance in this context.

## NATURAL EROSION AND SEDIMENT YIELD OF STEEPLANDS AS BACKGROUND TO MAN-INDUCED CHANGES

Grant describes a series of episodes of increased erosion and sedimentation in the axial ranges of North Island, New Zealand over the last 700 years. Alluvial stratigraphy, paleosols, tree-ring ages, and $C^{14}$ dates permit construction of a chronology for these episodes. The relative magnitudes of erosion periods are assessed using cross-sectional areas of alluvial deposits in one large drainage basin as an index of depositional volumes. Evidence for these events has been gathered and refined by Grant and other workers over about a 15-year period in which several revisions of the number and relative magnitudes of events have been made. Two important questions are not yet fully resolved in my view. First, important erosion periods may be unrecognised because their deposits have either been eroded or have been completely masked by more recent deposits. Second is the key inference that major alluvial terraces consistently represent the upper surfaces of deposition from periods of aggradation caused by increased erosion upstream. The widespread occurrence of the main surfaces that Grant interprets in this way, and the consistency of their dating, support Grant's interpretation but the evidence is by no means conclusive. Grant's work provides a valuable perspective of natural erosion and sedimentation fluctuations in which human impact on erosion and sedimentation should be viewed.

Pickup *et al.* present sediment-yield data for headwaters of the Fly River, Papua New Guinea. Sediment yields in the Ok Tedi headwaters are very high ($10^3 - 10^5$ t km$^{-2}$ yr$^{-1}$), in keeping with the steep terrain, very high rainfalls (perhaps up to 11000 mm/yr), and frequent seismic activity. Landslides, particularly very large slides which occur perhaps once in ten years somewhere in the headwaters, dominate sediment inputs to the headwaters, and sediment inputs further downstream are much smaller. Some large landslides ($>10^6$ tonnes) produce debris flows causing bed level changes of up to 3 m as far downstream as 15 km from their source. This study provides valuable background data for assessing the likely impact of the mining project in the region, but also adds to the very limited sediment yield data available from very high rainfall regions.

## SUMMARY

The papers presented in this theme add considerably to our understanding of the site-specific effects of major land management changes in various parts of the Pacific Rim. Our ability to predict impacts based on these and other case studies is, however, rather limited, and the papers presented place rather less emphasis on prediction than on documenting changes that have already occurred. It seems that one of the major future needs is for studies that attempt to synthesise results from many case studies into more general predictive guidelines or models. An interesting example of such a synthesis is Caine's (1980) analysis of rainfall intensity-duration combinations which produce shallow slope failures. Caine identifies, from some 70 studies, a threshold of rainfall intensity and duration above which slope failure will generally occur on undisturbed slopes. Such a threshold, in conjunction with rainfall intensity-duration-frequency data, could be used to identify the likely return period of slope failures on undisturbed areas. Where human impacts are suspected to have influenced failure, checks of rainfall intensity-duration data against this threshold may help to assess whether failure would have occurred irrespective of human impact. Syntheses of empirical observations should also help towards identifying the underlying physical bases of the effects of different land use. In conjunction with such attempts at synthesis and the development of theory, more studies of natural time and space variations in erosion and sedimentation are needed so that human impacts on these processes can be viewed in their proper perspective.

## REFERENCES

Caine, N. 1980: The rainfall intensity-duration control of shallow landslides and debris flows. *Geografiska Annaler 62A:* p 23-27.

# SEDIMENT BUDGETS AS A VEHICLE FOR UNDERSTANDING SEDIMENT TRANSPORT AND AIDING IN LAND MANAGEMENT

Harvey M. Kelsey

*Redwood National Park, Arcata, CA 95521, USA*

Using sediment budgets as a research technique and as a practical tool in catchment management has advantages and limitations. Sediment budgets by their nature compartmentalize processes in order to avoid accounting for the same volume of sediment transport two or more times. Such compartmentalization often obscures intermediary transport processes and can oversimplify complicated processes that involve a linear series of events or a train of events that feedback to increase erosion at the initial site. Nevertheless, as a quantitative summary of sediment movement by different processes, they are a valuable research technique.

Some of the aspects of budgets that can be most easily measured are mass hillslope and streambank transfer processes, such as translational slides, earthflows, mudflows and alluvial bank erosion; and sediment yields at a gauging station. There are flaws in the analytical techniques that generate these measurements, but measurement precision relative to that for other geomorphic processes is high. The difficulty of quantifying the effects of land use on transfer processes (even if we can accurately measure the total volume of transferred sediment) is a major problem in using budgets as land management guidelines. For example, Rice and Datzmann in this volume were only able to account for 40% of the variability in erosion from study plots when attempting to find the optimum relationship among variables such as logging technique, geology, aspect, and slope, which are basic determinants of erosion susceptibility. The unpredictable nature of operator judgement during land use makes land-use induced erosion an elusive quantity, at best, in sediment budgets.

A difficult aspect to quantify in sediment budgets is the volume of sediment in storage in third-order or greater channels and the residence time of this sediment in different bedforms in the channel. An ability to quantify storage and predict when stored sediment moves, how far it moves, and how much will move, is one of the basic needs of land managers concerned with development on flood-prone alluvial bottoms. It is also a prime concern of biologists studying fisheries habitat.

Since the details of alluvial storage are so important to management, as well as being one of the least understood geomorphic processes in large catchments, constructing sediment budgets over geologic time frames, where storage is most often considered to be steady state, has limited practical use. However, budgets that define transport over geologic time are essential if we ultimately wish to portray the complex interaction and cycling of inorganic and mineral constituents through a catchment. For the shorter term, though, sediment storage is far from a steady-state condition in catchments, and storage variation over specific time intervals is exactly the type of information that is most needed.

Finally, the most serious limitation of sediment budgets is that some processes can be quantified much more accurately than others. Because of the awesome job of constructing a detailed budget that accurately quantifies both storage and transfer of sediment through the range of processes that operate on the hillslope and in the channel, most budgets can quantify, to varying degrees of accuracy, a portion of the processes. Processes that are too complex to measure, or that are poorly understood, provide the unknowns into which one lumps the error from other quantities in order to balance the budget. Frequently, one unknown aspect of the budget accumulates the errors, and this quantity has limited credibility.

Based on the above observations, I feel there are some useful directions to pursue in sediment budget studies if such studies are both to aid land management decisions and to improve our basic understanding of sediment dynamics in mountainous, Pacific-rim catchments.

First, to confront the problem of changes in quantities of stored alluvial sediment and its potential for movement, budgets should be constructed for a specified time period. A series of short-term budgets showing sequential changes is best. The smaller the catchment, the easier such a task would be.

Secondly, if it is necessary in developing a sediment budget to accumulate errors in the least understood quantity of the budget, then the study should be designed so that the least understood variable is not one of the crucial items for basin management. For instance, the error should not accumulate in the alluvial storage term, even though alluvial storage may be difficult to measure accurately.

Thirdly, to be useful for management, sediment budget studies must necessarily be site-specific because differences in basic geomorphic processes, climatic history, underlying geologic parent material, and especially land use history, vary greatly between basins. With more basic research, it may be possible at a future date to develop a budget for a specific basin using limited basic data for the basin and then applying appropriate equations, but at the present time, I feel useful sediment budgets invariably demand considerable on-site data collection to be accurate.

Finally, though sediment budgets for catchments are powerful tools to use in making land management decisions, budget studies are not always the best use of limited funds available to study management problems. Many geomorphic processes that may be affected by land use are still poorly understood, and more detailed, basic research into the operation of one process or one land-use process interaction may yield more pertinent data than trying to use the same funds to broadly define a catchment sediment budget.

This approach to developing and using sediment budgets is largely empirical, in that past field work and research in catchments gradually add to the credibility of sediment budgets and determine the value and direction of sediment budget work in the future. I recognize that an equally desirable approach is to start with the basic physical theories that underlie rock weathering, fluvial transport, mass movement, and other processes, and from this foundation, develop a probabilistic model for a basin sediment budget, in which geologic, tectonic, and climatic variables unique to a basin are incorporated into basic equations applicable in all field situations. However, in the short term, this latter approach is not a useful alternative for the land manager. The two approaches are presently being pursued simultaneously. I hope developments in both approaches will be conveyed informally between workers, and more formally through conferences such as the present one to which I direct this commentary.

---

R. M. Rice

*Redwood Sciences Laboratory, USDA Forest Service, Arcata, CA 95521, USA*

In this commentary I will be discussing the acquisition and transfer of scientific information. As I perceive it, those tasks are the principal business of all of us attending this symposium. I have come to the conclusion based on my review of Theme 3 papers, that many of us are not fulfilling our obligations as scientists as adequately as we ought to when we set about to acquire new information and report it to our peers.

In his keynote address for Theme 1 Painter summarized the interest of land managers as being able to assess, understand, and control erosional processes. Certainly, any investigation includes some assessment and attempts at understanding. Most investigations, further, have the ability to control as their ultimate objective. Having said that, I would claim that Theme 3 papers and the types of investigations hey represent, are aimed primarily at assessment. That being the case, the authors in this section have a ma'or responsibility for providing ground truth for those attempting to understand and control erosional processes.

The importance of the assessment role was highlighted by both Pickup (Theme 2) and Swanson (Theme 3) in their keynote addresses. Pickup said that we need more case studies. Taking a slightly different tack, Swanson expressed the need for a "natural history" approach to our investigations. He asserted that we need to de-emphasize narrowly focused investigations and emphasize those which, at a minimum, completely treat all of the various aspects of a major component of the sediment budget of the area under investigation. Collectively, their admonitions suggest that we don't understand the phenomena we are studying very well and must, therefore, conduct extensive investigations if we are to acquire some understanding of the movement of sediment in natural and man-disturbed systems.

My feeling that all was not well with how we're going about acquiring new information was seconded by several speakers earlier in our program. Beschta, commenting on Theme 2 papers, said that we need to be more careful in defining the objectives of our investigations. It is hard to know what you have found out if you don't know what you are trying to find out. Pickup urged us to be sure that we make our measurements at the relevant place. I would further urge that we also be sure that our measurements, themselves, are relevant. Too often we use surrogates for the measurements that we ought to be making and then blithely assume into nonexistence the problems that may have been created by the noncongruence of the surrogates with relevant measurement taken at the relevant place. Dissatisfaction with the current state of affairs was seconded by Sutherland when he asked: "Have we really learned much from all the field investigations?" I guess I would have to disagree with the implied answer to Sutherland's query. I think we have learned quite a lot, but I think we could have learned a lot more had we heeded the Pickups and Painters among our peers.

It is not fair for me, or others, to merely assert that we're not doing very well and leave it at that. We ought to attempt to identify the source of our difficulties and at least suggest some remedial actions which we might take. Fundamentally, I believe that many of our problems stem from the fact that watershed management is coming of age around the Pacific Rim. Consequently, land managers are now asking for the answers to hydrologic and sedimentologic questions. This condition might be quite favorable if it were not for the fact that most managers have an inflated view of the state of our science and art. That deficiency would not be too serious were it not for the fact that many of us lack the courage to correct this misconception. We appreciate the attention that managers are currently giving to our views after so many years of indifference. We can't bring ourselves to inform them that we cannot walk on water — even at 500,000 milligrams per litre. As a result, we find ourselves conducting ill-defined investigations which arrive at weakly supported conclusions in an unrealistically short period of time.

As a remedy, I would suggest that we start being more hardnosed about tasks we will undertake and the price we are willing to accept. Our clients should be made to understand the relationship between the time and resources devoted to an investigation and the quality of the information produced. We need to be very exact when we explain to them what we can and cannot do. Part of that explanation ought to be some definition of a "successful" completion of the task and an estimate of the probability of success. Due to the vagaries of nature, especially climatic variability, many of the investigations we undertake carry a low probability of reaching a completely successful conclusion. This fact ought to be driven home before studies are undertaken on behalf of others.

Until now I've been discussing the acquisition of scientific knowledge. Such knowledge, however, obtains most of its value when it is transmitted to others. What are the problems and possible solutions to them? This question is perhaps more difficult to answer. Often it is hard to distinguish poor science from poor communication. I will assume that I am dealing with poor communication.

One source of the difficulty lies, I believe, with our publishers. All of us chafed under the page limitations applied to the proceedings of this Symposium. I would maintain that much of what we do is ill-suited to reporting by means of short articles. Certainly, it would be unwise for Fred Swanson to attempt to describe one of his "natural history" studies in a brief article. In this regard, some of the Theme 3 papers sounded better when presented here. The authors filled in the gaps and explained away many of the ambiguities in the Proceedings. For those of us here, that was an adequate solution to the problem — what about all of those who could not come to Christchurch and will only gain their impressions of the research reported here by reading the Proceedings?

Page limitations are not the sole explanation. We must share the blame. In our desire to appear omniscient we find it difficult, especially in print, to resist the temptation to put the best possible face on what we have done. We are reluctant to own up to the random perturbations that degrade most of our research. In Theme 3, only Nolan and Janda acknowledged the political pressures which led to some of the compromises which blemished their investigation. I believe I can perceive similar skeletons in many of our closets. And they went unnoticed in our papers.

There are several steps that we can take to remedy our communication difficulties. Many of us play roles in various professional societies. Let us attempt to get them to review the appropriateness of their publication policies. There will, however, always be some limits. The world is never going to be enthralled with *all* we have to say. Therefore, it is incumbent on us to adjust our report to the medium which we are using to transmit the information. In this way, what we have done can be adequately understood by our readers. Let us be willing to rewrite our articles or postpone a publication if we cannot accomplish this.

Both in the planning and in the reporting of our research, let us define both precision and accuracy to the best of our abilities. Lastly, let us clearly distinguish between scientific inferences and our professional opinions. If we will take these steps and others which I have urged earlier, I am confident we will become members of a more vigorous and productive scientific community.