REVIEWS OF THEME 1

Methods for Assessing Slope Erosion and Non-channel Sediment Sources in Upland Regions

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Colin L. O'Loughlin

Forest Research Institute, Christchurch, New Zealand

INTRODUCTION

The current interest in slope erosion assessment in Pacific rim steeplands centres principally on mass wasting processes and features. In a broad context the assessment of slope movements encompasses:

1. identifying existing and potential landslides;
2. generally evaluating the physical characters of the landslide terrain;
3. classifying landslides into recognisable types;
4. estimating landslide size, rates of movement and distribution;
5. identifying probable causes of failure;
6. evaluating the economic importance of landslides, i.e. their potential to damage structures, people, streamwater quality, forests and farmland;
7. determining whether or not instabilities are preventable or controllable.

The symposium papers in Theme 1 are primarily concerned with the first five aspects listed above.

Despite considerable advances in assessment methodology and knowledge of regolith properties, the complexity and heterogeneity of most natural slopes prevent accurate determination of the stability conditions and a complete understanding of the causes of failures. The great variability in strength properties, in the stress fields operating, and in pore water pressures in natural slope situations, suggest that probabilistic approaches to stability analyses of slopes will be required in the future.

The Pacific rim steeplands have a number of attributes which further complicate the assessment of slope erosion. First, large tracts of steeplands are composed of converging plate margin sediments (geosynclinal sediments), commonly fractured, folded and weakened by intensive tectonism and often rich in montmorillinite and related clays. Such features lead to high natural rates of erosion and sedimentation. Second, the circum-Pacific region is subjected to periodic seismic and volcanic activity, often of great intensity which leads to localised very high erosion rates. Third, man's influences on the natural ecosystem of the Pacific rim steeplands have been widespread and concentrated mainly over the last 150 years. Accelerated erosion and sedimentation responses to man's influence are superimposed on already high natural denudation rates. Under such circumstances of great instability, erosion assessment techniques which may be very suitable for tranquil environments are often totally unsuitable.

REVIEW COMMENTS

Seven papers are reviewed, four of which are concerned with western US forested steepland environments, two with NZ steeplands and one with the New Guinea highlands.

Ward *et al.* employ an infinite slope model and a series of landslide potential and landslide probability equations, based on realistic estimates of variables such as soil strength parameters, to analyse slope stability in steepland forests. This innovative approach permits soil heterogeneity and lack of knowledge about regolith conditions to be taken into account. Segmentation of an area under investigation into discrete cells, each characterised by known or estimated input data, enables computer-based landslide hazard evaluation and mapping over large areas. The methodology offers much to forest management planning as it overcomes many of the problems involved in making hazard evaluations based on physical stability models over large areas. However, two problems could be inherent in the technique. Landslide initiation zones are commonly located in discrete slope hollows and depressions less than say one hectare in area. If cell sizes are too large the variation in landslide potential on sub-areas within cells could be greater than that between cells. Furthermore the infinite slope model may be irrelevant to slope movements such as deep slumping or creep.

Blong's descriptions and analyses of mudslides in New Guinea provide an excellent example of the difficulties involved in applying limit equilibrium analysis to complex failures. Uncertainties about magnitudes of pore water pressure, regolith strength along failure planes, the nature of the stresses operating within the shale regolith (due to the movement of adjacent shale slabs), and the likelihood that movement and deformation are not restricted to one clearly-defined failure plane, make the application of an infinite slope model tenuous to say the least.

Swanson *et al.* adopt a low-cost field inventory approach aided by aerial photograph analysis to study debris avalanche erosion in managed forest steeplands in Oregon. The technique successfully indicated a 4-fold and 120-fold increase in the rate of soil removal by debris avalanching on clear-felled slopes and road right-of-ways respectively. Similar techniques have been used in other studies of forest-land erosion in the US and in New Zealand. Temporal variations in management techniques, in the proportions of land under forest or various treatments, and in slope erosion rates on the same site, have often complicated the interpretation of the results and reduced the reliability of longer-term estimates of slope denudation rates. Swanson *et al.* use an accounting system in their analysis which incorporates units of cumulative area per unit time (such as hectare-years for, say, clearcuttings of different ages) to at least partly overcome these problems. This analysis technique should be of value to others engaged in broad scale slope erosion surveys where the forest management history is well known.

The importance of regolith creep and other slow mass-wasting processes in the total denudation of slopes, both as direct mechanisms for transferring soil downslope and as precursors to more rapid failures, is usually overlooked because they are difficult to measure. In what must be one of the most comprehensive studies of creep and earthflow erosion completed to date, Swanston used 35 bore-hole inclinometer tubes installed on a range of terrain types to measure slope movements on western US forest lands. His data analysis technique determines a plane of maximum movement which enables construction of a vertical profile of movement. The method provides not only accurate estimates of the rates of regolith movements and their changes with time but also information on the mechanisms of movement. The measurements are sufficiently sensitive to detect movement of only a few mm per year and the method is eminently suitable for detecting accelerations in movement in response to various forest activities or to seasonal changes in soil moisture. However, inclinometer tube methods can pose many problems. The success of the study under review is at least partly due to the great experience and expertise in slope stability research that Swanston and his co-workers have built up over the last two decades. Even so, on some sites, movement rates appear to be near the limits of detection of the equipment. The method can be expensive, drilling holes in steep terrain can be very difficult, and inclinometers are notorious for malfunctioning.

The structurally and lithologically complex Cretaceous and Tertiary terrain in the eastern Raukumara Range, NZ, displays a diverse range of slope forms and mass-wasting features which are closely related to the underlying rock characters. Pearce *et al.'s* excellent attempt to unravel the details of these relationships is based on an investigation of the whole rock and clay mineral composition of the Mangatu and Tikihore Formation rocks and a very good knowledge of the region's broad geology and erosion processes gained from previous surveys. Slope forms and processes are shown to be closely linked to the clay mineralogy and strength of the regolith. The paper provides a useful explanation to account for the occurrence of complex mass movement features in Mangatu Formation rocks which possess relatively high peak strength due to calcite cementation. Acid sulphate weathering of pyrite leading to leaching of the calcite cement produces a shallow montmorillonite-rich regolith of low remoulded strength susceptible to mass wasting. The real role of tephra mantles in the possible retardation or prevention of these regolith processes requires further investigation.

Lehre's sediment budget study of a small Californian basin establishes a general model of process relations and identifies the major sediment sources, sediment storage sites, erosion processes, and the rates of sediment transfer from one storage site to another or to the mouth of the catchment. Particular attention is paid to the swale refilling processes and rates which enable Lehre to estimate that landslide frequency has increased 10-fold in the past 50–150 years. It is unfortunate that complete sediment budget studies such as Lehre's and an earlier study by Dietrich and Dunne (1978) are relatively rare, as this approach provides a comprehensive set of data needed for a full understanding of landscape development and for assessing or predicting impacts of management.

Owen's attempt to explain why landslides in Wairarapa hill country, New Zealand, are aspect-related, is based on analysis of soil and turf-mat strength and Atterberg limit data. The data show that the lower liquid limits, plastic limits and plasticity index, and the lower strength on sunny compared to shady aspects account for the preferential fluidisation of soil on sunny aspects. It appears that the results of this investigation pose a number of unanswered questions and further geotechnical work on the regolith may elucidate the underlying causes of the regolith strength differences on different aspects.

CONCLUDING REMARKS

Several of the papers are concerned with regolith strength at time of failure. There is growing recognition that it is not only the failure strengths that require investigation but also the development of failure conditions within the soil mass where lack of strength is the prime cause of failure (Hawley and Luckman, 1980). This is particularly relevant where management activities have induced changes in soil pore water pressures, in the soil loading conditions, and in the density and strength of vegetation root networks. The further development of field and laboratory techniques to assess the magnitudes and rates of these changes is required to accurately predict short term and long term changes in landslide hazards. Quantification of the real significance of tree root networks in preventing regolith failures and assessment of the rates at which root reinforcement declines after tree removal are aspects of special significance in Pacific rim steeplands where many of the erosion problems are associated with historical deforestation or present-day forest harvesting.

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R. J. Blong

School of Earth Sciences, Macquarie University, Australia

After Lumb (1966) demonstrated that soil material properties were quite variable, it became clear that slope stability analyses needed to be considered within a probabilistic framework. Others have developed such an approach (e.g. Alonso, 1976) but the contribution by Ward and his co-authors represents the first attempt to map slope stabilities in probabilistic terms.

Although the authors have chosen a simple method of estimating the means and variances of soil cohesion, root cohesion and the angle of internal friction, the assumption that these variables have uniformly distributed random values might not be realistic. We might also question the use of the mean of uniformly distributed random values rather than the lowest values. Perhaps the choice depends on the purpose of the survey?

Although Ward *et al.* use the infinite slope model to determine factors of safety, they provide a significant improvement on the usual analysis in that their model takes account of both root cohesion and vegetation and soil loading. However, the model still ignores many environmental factors known to influence landslide location. Table 1, summarised from a pile of reprints cluttering up my office, sets out the frequency with which a number of factors were judged significant in promoting landsliding. No precise significance can be attached to the data in this table, but they do remind us that the infinite slope model takes no account of several important factors.

TABLE 1—Spatial and temporal associations with landslide occurrence.

Attribute	No. of studies
Slope	16
Rainfall	15
Ancient landslides	4
Man's intervention	7
Time	5
Rock type	11
Soil type and property	5
Altitude	1
Aspect	4
Earthquakes	3
Tides	1
Vegetation/clearing	4
Groundwater position	4
Topographic position	7
Total	87

One could agree that the mapping approach proposed by Ward *et al.* takes many of these additional factors into account, but this is so only if the cell size is small and the field input for each cell appropriately detailed.

Choice of cell size would seem to be a crucial issue. If cell size is too large, within-cell variability in soil material properties might be as great as between-cell variation. How does one choose cell size? Should each cell be smaller than the expected landslides? As with the factor overlay or environmental approach to landslide hazard, mapping costs increase dramatically with increasing scale or decreasing cell size. It would be interesting to compare the costs and benefits of the probabilistic mapping approach with those of the environmental factor approach. The initial study in Oregon by Ward *et al.* resulted in a "better than 80 percent match of estimated hazardous cells to actual cells with landslides". How would an environmental approach have compared? What would the relative costs of the two studies have been?

Certainly, as indicated, the approach of Ward *et al.* scores both because of its probabilistic approach and its extreme flexibility which allows the modelling of the effects of possible futures. However, these aspects are not necessarily beyond the environmental approach. Table 2 indicates the basis of a probabilistic approach to landslide potential mapping, an approach which could also be used for scenarios. As almost everyone who has attempted an environmental approach has devised their own scheme, we can be sure that there would be plenty of argument about the relative weightings of the variables considered. None the less, such an approach, with modifications, would seem to be viable. Finally, we could note that the advantage of a probabilistic approach in urban landslide mapping is that slopes are no longer delineated as 'stable' or 'unstable'. Thus, litigation is avoided, or, at worst, made more expensive as the arguments about the meaning of relative probabilities becomes interminable.

TABLE 2—Basis of a probabilistic approach to landslide potential mapping (after Ingles, 1974).

DEMERIT PROBABILITIES (all for on or next to the site)		MERIT ALLOWANCES (all for on site only)	
Impeded drainage on or above a slope greater than 1 in 8	0.40	Good deep drainage	—0.40
Slope greater than 1 in 4	0.20	Slope less than 1 in 8	—0.20
Cleared land	0.15	Good tree cover	—0.15
Subsurface clay layer	0.05	Artificial toe loading (or head cuts)	—0.05
Inclined rock bedding	0.05	Horizontal rock bedding	—0.05
Artificial head loading (or toe cuts)	0.05		
Ground water (springs)	0.05		
Surface cracking	0.03		
Seismic region	0.02		
(TOTAL)	1.00		

NOTES:

(1) Evidence of movements greater than 0.1 mm per year in any given probability zone automatically increase the risk in that zone by 0.2.

(2) If any of the above conditions are not known, allow maximum demerit.

(3) Mapping units may be pooled, but not subdivided.

The thoughtful and thought-provoking paper by Fred Swanson and his co-workers produced, for me, two contrasted feelings. Firstly, the feeling that we now know it all. Their Table 1 summarises results from a number of studies around the Pacific Rim and demonstrates that the effects of clearcutting and forest road network construction on landslide frequency and downslope soil transfer rate are everywhere comparable. These results suggest that there is little profit in further studies of this type, but the authors indicate that more data are needed on management history and that an improved accounting system using units of cumulative area per unit time (e.g. hectare-years for clearcuttings and roads of different age classes) should be instituted.

My second general feeling provoked by the paper of Swanson *et al.*

is that, as yet, we know nothing. They make the suggestion that changes in soil-transfer rate under management may be only short-term, the long-term rate remaining unchanged. I can think of two studies that perhaps support this thought. Scott's (1975) study of debris avalanching in Hawaii over a period of 3–5 years suggests that soil-transfer rates under forest and fern are little different despite differences in landslide frequency and landslide volume (Table 3). Scott believed that the contrasting rooting systems allowed distinct depths of soil to develop before failure occurred. A rather similar view was put forward in Schweinfurth's (1966) study of landslides under forest in the Milford Sound area, New Zealand. Schweinfurth believed that, once landsliding had occurred, further landsliding at the same site was inhibited until mature forest again stood on the slope. Both authors suggest, then, that the rate of downslope transfer by landsliding is weathering-limited. Are the slope movements described by Swanson *et al.* also weathering-limited in the long term? Although we now have results from around the Pacific Rim which confirm the not insignificant effects of management on soil transfer rates in the short term, until we can place these results in a longer-term context, we know little.

TABLE 3—Landslides under forest and fern (after Scott, 1975).

Vegetation	Forest	Fern
% of area	60	40
Depth of root penetration (m)	0.50	0.225
No. of landslides	53	66
Area of typical landslide (m x m)	7 x 16	6.5 x 13
Depth of landslide (m)	0.57	0.42
Slide volume* (m^3)	31.9	17.7
Total volume of slide (m^3)	1692	1171
Slide volume/unit area (m^3/unit area)	28.2	29.3

* Calculated as wedge-shaped.

Another question which Swanson's data raise concerns long-term sediment production from landslides. As I understand it, the results presented in their Table 1 are based on once-only measurements of landslide volumes, presumably within a few years of landslide occurrence. But, how much sediment is produced by these landslides in ensuing years? Andre Lehre provides measurements of landslide scarp erosion rates from a small basin northwest of San Francisco of 60 mm/yr. We do not know how long such rates are maintained, but Table 4 indicates that the volume of post-slide erosion can more than equal the volume of the original landslide. This Japanese example is probably extreme, but both sets of data demonstrate that we need to know much more about post-slide erosion.

TABLE 4—Growth of landslide scars with time (after Tanaka, 1976).

	1923 Volume (m^3)	1970 Volume (m^3)
Landslide — Scar 1	~1420	3580
Landslide — Scar 2	~780	1720

Andre Lehre's paper, which owes much to the methodology of Dietrich and Dunn (1978), demonstrates that the construction of sediment budgets may help us with answers to some of the questions raised above. I have only two brief comments on this paper. Firstly, we should use this sediment-budget approach much more often. Secondly, I am surprised by the absence of data in the budget on faunal activity. My observations in alpine meadows on Mt Rainier, Washington, USA, indicate the complete reworking of a 2 cm+ layer of 450-year-old Mt St Helens W* tephra by northern pocket gophers *(Thomomys talpoides).* On the sandstone country around Sydney, Australia, unpublished studies by Geoff Humphreys and Peter Mitchell of Macquarie University indicate the following mounding rates: by earthworms, 1.33 t ha^{-1} yr^{-1}; by ants, 8.41 t ha^{-1} yr^{-1}; and by lyre birds, 44.0 t ha^{-1} yr^{-1}. In southern Queensland Parker *et al.* (1976) showed marked concentrations of rabbit burrows in particular soil types (and with specific vegetation associations). In these three examples the depth of reworking, and the proportions of mounded material moving downslope, will certainly be variable. The volume of material may also be more substantial than the volume moved by soil creep; or is this contribution from soil fauna and ground-dwelling mammals and birds a major component of soil creep? Before we can answer this question for Australia we need more conventional measurements of soil creep and an examination of the role in downslope soil transfer of such wondrous animals as wombats, bandicoots, numbats, potoroos, dibblers, noolbengers, dunnarts and wuhl-wuhls.

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D. N. Swanston

US Forest Service, Juneau, Alaska, 99802, USA

REVIEW COMMENTS

Ward *et al.* describe a practical, computer-based technique, usable in the field, for both storage and retrieval of basic watershed data and the analysis of potential landslide hazards. The technique provides analyses based on classic soil mechanics theory from both a deterministic and a probabilistic approach which should be of considerable value for forest-land management decision making.

Depending on the level of data input at cell node points and the size of the cells segmenting a watershed the technique has potential usefulness at least down to the project level of analysis, although at the present level of application it appears to be most useful for broad area planning. Its limitation at this level is primarily the high cost and difficulty in obtaining quantitative data for higher orders of analysis.

The technique has one major shortcoming in its present form. This is the use of the infinite slope method for stability analysis which limits its effectiveness and accuracy in estimating the factor of safety against failure on slopes underlain by earth materials subject primarily to creep, slumping and earthflow processes.

Blong describes some interesting and useful aspects of mudslide behaviour in clay shales and the problems inherent in the application of the simple infinite slope model to such complicated quasi-viscous flow processes.

The author has clearly recognised and stated the major areas of error in terms of data measurement and interpretation when applying this method to such a complex process and has provided a reasonable although largely unquantified assessment of the causes of uncertainty in analysis results.

The author makes an important point which should be carefully considered by practicing geotechnical personnel when attempting to analyse similar unstable terrain. For most values of slide thickness and slope inclination, residual strength values and hydrostatic pore-pressure values can provide a reasonable estimate of conditions at failure. Local variations in slide geometry, composition, and structure of the sliding mass, however, may require the application of peak strength values and a careful consideration of failure-surface inclination, and the possibility of geostatic or confined pore-water pressures, which may greatly alter stability analysis results.

Swanson *et al.* describe an inventory technique and the results of an inventory analysis of one of the most unstable forest terrains along the Pacific Coast of North America. Such inventories and subsequent analyses are being applied with increasing frequency on forest lands in the Pacific Northwest of North America to develop knowledge of long-term soil mass-movement erosion trends and cumulative impacts of forest harvest activities on accelerated soil mass-movement activity. While the approach is clearly at a very broad level, such inventories can be accomplished at a very low cost compared to other forest resource inventories and can provide a rapid means of assessing the long-term impacts of various harvest activities on slope erosion and the modifying or ameliorating influence of changing land management practices.

The authors' analysis of landslide activity in the Mapleton area shows substantial acceleration in rates of occurrence of soil mass movements from clearfelling and road construction, with roads accounting for the greatest increase. These rates of acceleration are similar to those reported from elsewhere in the Pacific Northwest. However, the authors have added an important and, I believe, significant adjustment in the way rates from clearfelled areas and roads are reported. Thus, while road construction remains the single most damaging forest activity in terms of numbers of landslides generated, the effects of timber removal alone are more clearly expressed and are shown to be a much more significant initiator of accelerated landslide activity.

Pearce *et al.* provide an extremely interesting and edifying analysis of the effects of localised weathering, cementation, and the masking or mantling of potentially unstable material, on soil mass movement activity. The authors have carefully defined and characterised soil mass movement activity over a broad area of the eastern Raukumara Range, New Zealand. They related it to the underlying bedrock and successfully account for variations in soil mass movement activity and general slope configuration on each rock type through differences in clay mineralogy, degree and type of weathering, the presence or absence of carbonate cement, and the presence or absence of a mantling tephra layer which apparently reduces the amount of water reaching the underlying rock.

Such an analysis provides considerable insight into the causes of variability in soil mass movement activity within a specific terrain or within or between geologic provinces. I believe it has tremendous potential for interpreting this variability elsewhere on unstable ground around the Pacific Rim.

Lehre presents an excellent description of the routing path of sediment through a small drainage basin in unstable terrain. The routing processes are clearly defined and the hypothesised sequences of events are reasonable and correspond closely to sediment origin and paths of movement observed elsewhere along the North American Pacific Coast.

The apparent long-term acceleration of soil mass movement rates reported is interesting and reasonable since there is some evidence for this elsewhere along the Pacific Coast. The causes, however, may be more directly related to regional climatic changes although this needs to be more extensively researched. The sediment routing model the author describes may be applicable to many areas of unstable terrain around the Pacific Rim.

Owen gives an interesting account of the apparent direct influence of aspect on soil strength and response to moisture levels in the soil. Three independent methods of measurement clearly show differences in strength characteristics although differences do not appear great enough to materially alter failure conditions in the presence of high intensity rainfall and a near-surface water table. The author describes prolonged rainfall conditions but says little about temporary water table development so I must assume little active pore-water pressure at the sites. It would be nice to know this.

I am as much intrigued by the additional questions generated as by the results of the study. Why is there little difference in penetration resistance between turf mat and subsoil on shady aspects? Why does the subsoil on shady aspects offer greater resistance despite substantially higher water content? Why are strength parameters at high water content reduced more rapidly on sunny aspects? Could it be a greater degree of weathering on sunny slopes? I hope these questions can be answered in the future.