SUMMARY CONTENTS

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Several of the previous summary speakers have made pleas for more work on fundamental processes and less on qualitative description and superficial correlations. I endorse the first of these pleas but hold reservations about attempts to be quantitative. It is my view that a good judgement is better than a bad measurement. Judgements are generally improved by knowledge of what the measurements are, but the quantities being examined are almost all so variable, in time and in space, that these measurements can only be taken as background information, to be processed by our shoulder-top computers during the formation of judgements.

I suggest that we might order our studies by asking of each situation questions such as:

1. Is this sediment *transport* situation controlled by the availability of sediment (of suitable grain size) or by the ability of the river to transport it? The answer will seldom be "by both", it will be controlled by one or the other.
2. Is this sediment *generation* situation controlled by
   - (a) reduction of the strength of the regolith, i.e. shear failure, or
   - (b) dispersion within the regolith (generally $Na+$ rich soils) or
   - (c) mechanical weathering (including slaking) or
   - (d) tensile failure (usually resulting from seasonal shrinkage and cracking), or
   - (e) scour?

For each of these processes different treatments are appropriate.

I was disturbed to hear people talking about slope stability calculations and 'factors of safety'. Such calculations (carried out for situation 2a above) are based on values of soil cohesion (c) and internal friction (ϕ). I do not believe that meaningful values of c and ϕ — or even ranges of these quantities — can be assigned to the (regolith) material near the surface of slopes in natural ground. In the upper metre or two of regolith the variability in c and ϕ values with depth and with location (say a few metres away) is such that a prohibitively large number of samples would have to be tested before sensible averages or minimum values could be expressed. Added to this are the difficulties of measuring pore pressures in the field and the latitude which the investigator has when drawing the envelope to the Mohr's circles. Furthermore there is the problem that values of c and ϕ may change with time (seasonally) as a result of changes in dry density due to dessication during dry spells and shearing-induced dilatancy during wet weather. The values for c and ϕ may therefore be dependent on when in the season the testing is done.

As if this were not enough, there is the further difficulty with regard to the implied assumption that slopes with a lower factor of safety are more likely to fail than those with a higher factor of safety. This is to assume that the different slopes will all experience strength reduction at about the same *rate* during storms, and this is to assume similar infiltration rates and permeabilities on the different slopes.

My advice is — forget about calculations of factors of safety: the only thing that the concepts of c and ϕ have to offer us is the *understanding* that *at* failure the strength will always be due in part to cohesion and in part to internal friction. This understanding will help us to make good professional judgements, but until such time as we can apply mathematical models of the pre-failure behaviour — the development of failure conditions within the soil mass — in terms of density, hydraulic conductivity, stress and strain, we will not be able to base probabilities of failure on measured soil properties.

Before leaving the subject of slope stability calculations I should add that I accept that they do have some validity for the design (by civil engineers) of two-dimensional recompacted earth dams and embankments, and for major cuttings in geologically homogeneous materials — particularly the former. For recompacted structures values of c and ϕ may be measured in carefully controlled laboratory tests on samples compacted to the same density as is planned for the full scale structure (and checked on during construction). Even in this situation the exercise is one of only marginal validity owing to the almost inevitable development of "deflection induced" cracks and other factors.

More meaningful than studies of regolith strengths are studies of why, and at what rate, the strengths *change*. Processes of strength change include shearing-induced dilatancy, reduction of soil water tensions, tensile cracking, chemical weathering, mechanical weathering and biological weathering (including the rotting of tree roots).

For these class 2a (shear strength reduction) problems there is a seasonal (weather dependent) cycle of strength gain and reduction. This pattern will take some patches of the slope to failure in a 'one year' rainfall event and more in a 'one hundred year' rainfall event, while all the time, weathering processes are moving each and every piece of the slope into a higher frequency category.

There has been some comment from the floor on how little (one paper) we have heard of erosion in areas other than standing and cleared forests. One answer to this is that it would have taken us another week to do justice to pastoral hill slope erosion. As things are we have had a good go at erosion and sediment transport in the Pacific Rim *forested* steeplands.

Finally, I wish to raise the question of 'long-term' implications. From many of the contributors of this symposium we have heard that the incidence of landsliding has increased very dramatically as a result of man's intervention. We have been left with the question implied but not expressed, let alone discussed — "what are the implications in the 50 year and 100 year term?" Have the Pacific Rim steeplands been taken a few steps along that same way which took the Adriatic Steeplands in Yugoslavia (for example) from forest to barren rock in only a few centuries? This is a question on which I would like to hear some professional opinions at our next symposium.