

SHORE PLATFORMS AND MASS-MOVEMENT: A REPLY

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Implicit in Mr Wright's note on shore platforms and mass-movement is a criticism of our findings on the role of mass-movement in shore platform development along the Gisborne coastline, New Zealand (McLean and Davidson, 1968). The lack of explicit criticism makes any reply difficult; we are not rebuked on our own evidence, nor is any fresh evidence presented from the same area to make it necessary for us to change or modify our original views.

Indeed, there appears little to comment on, except to restate the obvious — that we suggested a “positive” genetic relationship between mass-movement and shore platforms on the Gisborne coast, while Wright found no such relationship on the English channel coast. Such a difference is not at all surprising in view of the very different lithologies, structures, histories, topographies, rainfall intensities, wave environments etc., of the two areas. Similarly, Wright's comment that the “reverse situation” (a “negative relationship”?) — “Shore platforms frequently being absent from, or at least only poorly developed along coastal areas dominated by large-scale, discrete types of mass-movement . . .” — is not at all exceptional. Such comments may be equally applicable to many other areas, including for instance, much of the western U.S.A. coast where the activity of coastal landslides has been studied in considerable detail (Byrne, 1963, 1964; Dicken, 1961; Emery, 1960, 1967). None of these observers mention a direct association between mass-movement and shore platforms, presumably because it does not exist. One of us (R.F.M.) has travelled the United States west coast from San Diego, California, to Astoria, Oregon, observing shore platforms and coastal mass-movement, and at only a few sites e.g. Duxbury Reef and Point Arena, California, was a situation in any way comparable to that along the Gisborne coast apparent (incidentally, these areas have similar lithologies to the Gisborne area). It was the very lack of a general clear-cut visible relationship on that coast (as well as on parts of the New Zealand coast) which made the Gisborne coast worth investigating and writing about.

Our investigation was of a specific process element (mass-movement) and specific response element (shore platforms) and was carried out in a specific area (Gisborne coast). While allowing for the multivariate nature of shore planation processes we justified the selection of one independent and one dependent variable because “it has often proved valuable to isolate a single process element and consider its role with reference to platforms of a specific area” (McLean and Davidson, 1968, p. 16). Many precedents exist in the shore platform literature for this type of limited approach which highlights certain variables and excludes others. Our objectives were stated quite explicitly; the primary one being “to indicate that current shore platform development *on the Gisborne coastline* results primarily from the destruction of cliff-faces by mass-movement and removal of the resulting waste by wave action” (*op.cit.* p. 16). The phrase now italicized, in

this statement is important and it is unfortunate that Wright appears not to have taken it at face value. At no stage did we transgress our "case study" area and put forward the more general theory that wherever coastal mass-movement occurs shore platforms automatically result, or vice-versa. Wright infers that we made a generalization like this. For instance, he concludes "there is little or no evidence to support the view that mass-movement processes actually form shore platforms or assist in their development." Presumably *the view* here refers to our view, the implication being that we made a general statement rather than a statement restricted to a particular area. Our comments were limited to the area where we worked, just as we imagine, Wright's comment, quoted above, refers particularly to the area where he worked.

A further example of the danger of generalising from the particular, and the explicit particular at that, can be cited. In the opening sentence of Wright's paper he paraphrases our conclusions, and continues: "Work by the present author on the shore platforms of the coasts of Britain does not confirm this observation." *This observation* refers to the preceding sentence: "that *along the Gisborne coastline of New Zealand* there is a genetic connection between the distribution of mass movement phenomena and shore platforms" (our italics). Just how one, working on the coast of Britain can or cannot confirm anything along the Gisborne coast remains a mystery! Obviously, Wright has taken the last part of this sentence out of context, and in so doing has shifted the emphasis from the particular to the general case. This certainly was not our intention.

Such details however are minor, if somewhat annoying, and it seems worthless to continue this type of "word" or "phrase-picking."

Mr Wright has provided some valuable information for students of shore platforms (and coastal mass-movement). He has considered an hypothesis developed for one area and rejected it when attempting to apply this same hypothesis to another area. Such a sequence of events appears to be a feature of the history of many shore platform process topics — storm wave action, solution benching, water layer (level) weathering etc., have all been on centre stage at one time or another. Each has had its defenders and attackers. That mass-movement should momentarily hold the spotlight seems reasonable in view of the fact that in certain areas it is a major cause of cliff retreat and coastal change. In this context, two of the many recent contributions can be mentioned. Firstly, Emery (1960, p. 20) argues that "the amount of direct erosion during the cutting of a sea cliff is small compared with the effect of landsliding and sheet wash" on the Southern California coast, and secondly, Horikawa and Sunamura (1967, p. 68-69) show that on parts of the Japanese coast landsliding and rill erosion are important factors causing erosion of coastal cliffs. Both these studies include diagrams illustrating coastal erosion. (The sequential developments illustrated by Horikawa and Sunamura (1967, Fig. 2) and the more general model of Emery (1960, Fig. 21) are apt illustrations of our interpretation of what occurs on the Gisborne coast). While emphasis is placed on the vertical plane (cliff-face), the implications in the horizontal plane (shore platforms) are significant: with cliff retreat and removal of landslide talus a platform may be exposed. Often however, this last stage may not be reached — the sequence in effect being truncated before the "cycle" is completed. In some cases it may be a truism to say, using an analogy from "wood and trees", that literally one cannot see the platforms for the talus! It is quite clear that much depends on the rapidity of removal of the landslide talus. On the Gisborne coast removal is at a rapid rate, while on the Oregon coast for example where both advancing shorelines prograding by mass-movement and retreating shorelines bordered by mass-movement debris are found, it is at a much slower rate (Dicken, 1961, p. 16-17). All these examples point to the fact, rightly suggested by Wright, that the relationship between cliff retreat and the landward extension of shore platforms is not a simple one.

Finally, the studies referred to in this reply and our own specific contribution make it imperative that mass-movement should still be considered in any checklist of possible shore planation processes. As with other processes, mass-movement can be rejected where the evidence warrants it (e.g. along the English Channel coast). There is however, little doubt that in some areas mass-movement is an *actual* process in shore platform development (e.g. Gisborne coast) and in other areas an *actual* cause of coastal cliff erosion (e.g. Southern California, Oregon, Japan) — and by inference a *potential* cause of shore platforms.

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