REPLY:Gravel Beach Profile Characterization and Discrimination

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J.D. Orford makes some interesting observations with regard to the process/response characteristics of gravel beaches with which we are in complete agreement. In criticising our recent paper in the *Journal of Coastal Research* (CALDWELL and WILLIAMS, 1985), he appears however to have interpreted certain of our results in a quite different way than was set out in the text.

It seems rather ironic to us for Orford to conclude that our procedure for discriminating between profile types is similar to that originally proposed by SONU and VAN BEEK (1971), as anyone reading the paper will see that our refined model is based fundamentally on their work. Our objective was to develop what appeared to us to be the promising interpretative value of their model (SONU and VAN BEEK, 1971) in terms of (1) its geometrical accuracy, and (2) its applicability to the gravel beach environment. Interestingly, this latter objective was also pursued by ORFORD (1978), with the limited results we cited.

Orford comments upon the characteristics of the beaches we studied, at Nash and Gileston, S. Wales, and their relation to his study beach at Llanrhystyd, W. Wales. Although his outline of the post-glacial genesis of gravel beach ridges is of relevance to all three beaches, his assumption that our beaches are characterized by the present economy of sediment scarcity observed at Llanrhystyd, is incorrect; as is also his contention that cliff-bound gravel beaches, such as is found at Nash, are unusual features.

We selected these two particular beaches because they represented examples of free-standing and cliff-bound types respectively, while at the same time sharing many key features (similar beach material, foreshore topography, climatic regime etc.). While we accept that littoral processes can differ both qualitatively and quantitatively on these two beach types, our profile classification (CALDWELL and WILLIAMS, 1985, Table 1) showed the fairly balanced spread of morphological types which we identified on both beaches, which should have effectively dispelled any doubt that our results were distorted by site variations.

At the heart of the matter, however, lies Orford's apparent misunderstanding of the way in which our method of configuration discrimination should be applied to a profile data set. By indicating that our profile standardization routine is redundant for the purposes of deriving the hypsometric integrals on which statistical testing is performed, the value and role of standardization has not been missed. It is not used to derive the integrals, but rather to facilitate visual classification of configuration types, which is often extremely difficult when comparing profiles with a wide range of h_{max} and S_{max} values. The quality of resulting populations of profiles in each configuration type is greatly enhanced by adopting this method of standardization, which does not in any way distort intrinsic profile configuration (CALDWELL and WILLIAMS, 1985, Figure 3).

Quite clearly, standardization makes no difference to the hypsometric integral values which can be derived for any profile, as was stated in our paper (CALDWELL and WILLIAMS, 1985, p.134), although we suggested that it can be slightly easier to obtain integrals for a population of profiles after standardization has been performed. The advantage of deriving integrals as one-dimensional descriptions of configuration type, is that Q/S space, as defined by SONU and VAN BEEK (1971), becomes redundant as a framework for morphological discrimination. Instead of comparing regression equations, visual interpretation is made possible in one plane, and data are much more amenable to statistical analysis (CALDWELL and WILLIAMS, 1985, Figure 4). As our results showed, justifiable non-parametric analysis enables statistically significant discrimination to be achieved at $P \leq 0.001$, which must be scientifically acceptable. Orford's reference to $P = 0.05$ and a X^2 test of integral frequency histograms bears no relation either to our results (CALDWELL and WILLIAMS, 1985) or those he presents.

We are of the opinion that the probable reason Orford's profile analysis showed a poor level of morphological discrimination was principally due to the fact that he did not carry out the graphical standardization routine on his data, which we

showed was so crucial to the quality of profile classification. If this had been done, we feel sure he would have obtained the statistically significant differences between (1) concave no berm, (2) concave with berm, (3) linear no berm, and (4) linear with berm which we found (CALDWELL and WILLIAMS, 1985, Figure 6). This is why we stressed the care with which our results should be interpreted.

Nontheless, we are sure that our presentation of a refined procedure for the discrimination of profile type is a valuable contribution to coastal science, and has relevance to both sand and gravel beach studies. We are dealing with a highly dynamic and complex system, which in relation to gravel beaches in particular is difficult to study at the most vital times of process/response interaction (*i.e.* during incident wave action, especially during storms!). It is not surprising, therefore, that our analytical tools are still fairly crude. The step/bar/composite model proposed by Orford is no more elegant than that we propose, but is just another valid conceptual view of the beach system. The advantages it presents in terms of the enhanced function of berm position must be balanced against its non-inclusion of processes which determine variance in macro-configuration. However, what our work on beach profiles does suggest is the occurrence of important long-beach morphological response variations, which may be related to longshore energy harmonics. This clearly needs to be incorporated into any improved model of beach profile genesis.

REJOINDER: Gravel Beach Profile Characterization and Discrimination

J.D. Orford

It is rare to be allowed a second bite at the 'discussion apple,' but CALDWELL and WILLIAMS (1986) make a number of points concerning my discussion of their paper on gravel beach profile analysis (CALDWELL and WILLIAMS, 1986, ORFORD, 1986) which require further development. These comments are expressed in the order in which they are raised by CALDWELL and WILLIAMS (1986).

The question of sediment scarcity on any beach depends on some form of definition and possible cause of the problem. To deny without evidence, that sediment scarcity exists on a specific beach is insufficient! I suggest that most gravel beaches in west/south Wales show degrees of sediment scarcity as witnessed, for example, by the failure of such beaches to show long term major regressive develoment associated with the contemporary sea-level stationarity. Some reasons for this occurrence were given in my original response. Clearly definition of sediment scarcity depends ultimately on the bugetary notion of sediment input/output, regardless of whatever mechanisms are operating between these two states. In gross terms the lack of regional macro-scale gravel beach growth indicates beach equilibrium or stability at best, and sediment scarcity at worst. The latter state is reflected in the lack of convex gravel profiles and the high elevation (>3 m) above MHWS) of gravel beach crests when only one beach ridge is present: two features that are characteristic of many fringing gravel beaches along the Welsh coast.

Neil Caldwell and Alan Williams 'feel sure' that if I had used their graphical standardization procedure the results of the analysis would have been different. I can't say definitely that they are right or wrong on this point! I accept (indeed I made the point myself) that *a priori* nominal profile categorization could alter the discrimination outcome. In response to their point, I suggest that the lack of any significant step/bar profile discrimination using the integral method (ORFORD, 1986, Figure 3) is not caused by the lack of graphical profile transformation. I do not believe that a transformation is required to differentiate between step/bar profiles when (a) the wave and swash conditions prior to the profile measurement were available as grouping criteria, and (b) the effective range of active beach width and beach height had already been partially controlled by the use of tidal split and the standardization of beach volume (ORFORD, 1986, Figure 4). Lack of integral based discrimination of Llanrhystyd profiles is related to an indifferent index which does not adequately specify the variation in beach profile relief.

I see no evidence to suggest that the volume of the gravel beach berm is anything but proportionately small to the volume of the beach sediment prism as defined by the H x W method of SONU and

VAN BEEK (1971). Clearly the only way to make the berm volume larger in terms of variance source is to consider the beach profile in terms of deviation from some mean/median/characteristic profile. In a sense this is what the eigen value/vector method of profile analysis attempts, with the added bonus that distinctive development of upper and lower beach berms would probably show up as differing components of profile variation. I would accept with this technique that variable beach profile widths may need to be numerically standardized prior to eigen value/vector analysis. The integral and Q_s methods apportion the affect of volume change by berm accretion across the whole beach profile. Thus berm presence is desensitized by integral and Q_s analysis, despite the clear diagnostic indication of macro-beach process that berm position contains.

I would not contest the point that Caldwell and Williams make concerning the elegance of either the step/bar/composite profile model, or the integral model. However to say that the former model has 'non-inclusion of processes' in incorrect. The step/bar/composite profile model has implicit process assumptions concerning the balance of beachface fluid force asymmetry as a function of breaker type and storm severity, a point which CALDWELL and WILLIAMS (1985, p.135) themselves recognize.

Clearly a number of problems and moot points concerning the optimum way to characterize gravel beach profiles remain. However as CALDWELL and WILLIAMS (1986) note, new approaches to analysis are always needed. If they work they open up log-jams, if they don't work they at least have an heuristic benefit. Either way our understanding of gravel beach dynamics and sedimentation are in sore need.

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INTERNATIONAL GEOLOGICAL CORRELATION PROGRAMME (IGCP)

Project No. 200: Sea-Level Correlations and Applications

1985 Annual Report on Scientific Progress

Late Quaternary Sea-Level Changes: Measurement, Correlation & Future Applications

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