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DISCUSSION

Rejoinder to: Cowell, P.J. and Thom, B.G., 2006. Reply to: Pilkey, O.H. and Cooper, A.G., 2006. Discussion of: Cowell *et al.*, 2006. Management of Uncertainty in Predicting Climate-Change Impacts on Beaches. *Journal of Coastal Research*, 22(1), 232–245; *Journal of Coastal Research*, 22(6), 1577–1579; *Journal of Coastal Research*, 22(6), 1580–1584

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We are pleased to have the opportunity to continue the discussion of mathematical modeling of coastal processes with COWELL and THOM (2006). Our respective views can be succinctly summarized as follows: Our view is that quantitative modeling of coastal processes with sufficient accuracy for most engineering and planning purposes is impossible. We do believe that qualitative modeling—answering the how, why, and what-if questions—can be useful. Cowell and Thom on the other hand believe that accurate predictive modeling of coastal evolution is feasible on a scale useful to engineers and coastal managers and that coastal managers require such predictions to do their work.

To make their point, Cowell and Thom cite a number of "successful" modeling efforts in Australia and elsewhere. We argue that their examples do not prove their point for two reasons.

- Their cited "predictions" are actually hindcasts not forecasts.
- Their modeling relates to the millennial-scale morphostratigraphic record of coastal change as preserved in nearshore stratigraphy and not to the annual or decadal scales needed by coastal managers.

Hindcasting is not forecasting. Their evidence of model success is therefore less than convincing. Hindcasting is always subject to tweaking or picking of "good" numbers for model parameters. Because most model parameters (*e.g.,* wave characteristics, storm processes, longshore sand transport volumes) are never known with accuracy, ample room for tweaking with a clear conscience is always available. Furthermore, as a number of workers (*e.g.,* ORESKES, SHRADER-FRECHETTE, and BELITZ, 1994; THIELER *et al.,* 2000) have pointed out, successful model application in one instance has little bearing on successful application of the model in the next instance. According to ORESKES (2000, p. 27), ". . . a match between model results and present observations is no guarantee that future conditions will be similar because natural systems are dynamic and may change in unanticipated ways."

We find it particularly frustrating that Cowell and Thom think that "confirmation" of their predictive modeling results gives them confidence to understand response of shorefaces to sea level rise. We know of not a single situation on eroding shorelines in which the effect of sea level rise has been plucked out of the huge number of factors and events that affect shorefaces. Scientists simply do not know how a gradual sea level rise will affect erosion rates on an annual or decadal scale. Certainly the Bruun Rule is not the answer. We suspect that, as a rule, the sea level signature on mobile sandy shorelines does not exist at timescales of less than a century or even a millennium—extreme storms wipe out evidence at any shorter timescale.

Cowell and Thom maintain that that the models are based on sound data. The examples cited, however, are typical coastal geology studies in which sand supply is inferred (at best qualitatively) but never known. The same is true for wave climate history and the effect of sea level rise or fall, which can never be differentiated from the many other factors that control shoreline evolution.

Earth surface processes are controlled by many parameters, but the models used to describe them have only a few such parameters. The assumption in quantitative predictive modeling must always be that the few parameters used in the model are the only important ones. Clearly this is not ever the case. COOPER and PILKEY (2004a, 2004b) have presented partial lists of parameters that control both longshore

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