Journal of Coastal Research	14	2	iii–vi	Royal Palm Beach, Florida	Spring 1998

EDITORIAL

Red Flags on the Beach

William F. Tanner

Geology Department
Florida State University
Tallahassee, FL 32306, U.S.A.

Seven problems in beach and coastal work are stated, and in each case common uncertainties or errors are pointed out. These problems ("red flags") are:

(1) The "River of Sand" concept explains sand transport.
(2) Major storms build beach ridges (several per year).
(3) Sea level is more-or-less fixed in position, unless mankind changes it.
(4) Modern transverse profiles illustrate dynamic equilibrium.
(5) Langmuir circulation is not important in near-shore water.
(6) About 100% of deep-water wave energy reaches the surf.
(7) Water wave length is not obtained readily, and is of little importance.

This is not an exhaustive list, but includes certain fundamental items which should have been corrected long ago, rather than matters for current debate.

INTRODUCTION

Scientific work in the coastal community spans so many different disciplines that it is exceedingly difficult to keep up with everything, and even to know all the basics that one needs in order to be able to juggle the different demands and claims that must be dealt with.

In the present note, seven different problems are presented, each of which is important to our understanding of what is taking place on and near the beach. Specific comments are made about each: the "received wisdom" is not always correct, and it is not necessarily true that the scientific literature will straighten us all out within a few years. These seven problems do not make up a complete list.

As an example of how long we continue to employ erroneous concepts, we are still limping along with the indefensible notion that major storms build beach ridges, almost 80 years after JOHNSON (1919) showed that this is not true.

For the reader who has already discovered all of this, it should be pointed out that he or she is in the minority.

THE RIVER OF SAND

One of the clearest and most attractive statements about sand transport in the near-shore zone has been known for many years under the heading "The River of Sand." In this model, the near-shore zone, along some rather large distance (such as 1,000 kilometers) can be thought of like a pipe, or a river. Sand is introduced at one end, and in due time is delivered to the other end.

If the prototype area is chosen carefully, perhaps there will be no gains or losses from one end to the other. If, however, this condition cannot be met, then small additions and/or subtractions can be made at pertinent points along the way. If the gains and losses are not equal, then the pipe (or river) can slide sideways. For a deficit of sand, the pipe can slide toward the continent, thus (presumably) acquiring enough new material to cover the deficit, or, alternatively, indicating erosion.

This model is easy to understand, almost poetic, and can be set to music. It has even been invoked to explain sand transport over very long distances, where such transport cannot have taken place. It is, however, badly in error.

In a survey of many actual coasts where the right questions can be asked and answered, it has been shown in detail (TANNER, 1987) that there is indeed no such "river of sand," except for rather small distances and rather small slices of time. For example, beach ridges, which are commonly thought to represent the workings of the river of sand, have been shown to have been built largely from offshore sources, with only minor amounts of sand derived from shore-parallel transport (TANNER, 1974, pp. 118*ff*).

In view of the fact that the "River of sand" is an erroneous idea, what might take its place? To maintain a level of simplicity roughly like that in the "River of Sand" concept, one might consider the Coastal Bookkeeping Equation (CBE):

$$E_1 + E_2 + E_3 - D_4 - D_5 - D_6 = 0.$$

The first three terms (E_i) are erosion terms, and indicate that sand has been acquired from somewhere else; the subscripts identify the upland (E_1), the up-drift (or obvious input area; E_2), and offshore (E_3). The next three terms (D_i) are deposition terms and show that sand can be deposited somewhere else; the subscripts identify the "land" (such as coastal dunes, overwash, and flood-tide deltas landward of inlets), the down-drift area, and offshore. If the terms on the left side do not sum to zero, then perhaps one can identify the problem areas, and therefore explain how it is, in a given case, that the expression does not balance.

In the "River of sand" concept, $E_2 = D_5$, and the other four terms are commonly omitted.

BEACH RIDGES

JOHNSON (1919) explained in detail how it is that we know that beach ridges are not built by individual storms. Many coastal specialists do not know about this venerable treatment, or have forgotten having seen it, and therefore continue to present and defend the storm hypothesis.

However, the storm hypothesis is still not tenable, and nothing has happened since Johnson's extensive treatment to change his conclusions. TANNER (1995) reviewed this situation, and stressed the fact that most beach ridges were built at intervals of 30–70 years, much longer than the intervals implied in popular statements such as "six or eight beach ridges are formed each year, one during each storm." This last statement has no support in actual beach ridge data, as he showed in detail, but has been spun from inferences drawn strictly from weather reports ("There are six to eight major storms per year, and therefore they must have built six to eight beach ridges").

Although JOHNSON (1919) did not present a suitable replacement for the storm idea, such a replacement is now available (TANNER, 1995). Each ridge-and-swale pair represents a couplet of small sea-level changes: one up, and one down (about 5–20 cm each). In the original report, as well as in a further discussion (TANNER, 1996), various papers were cited from the recent literature, showing that such changes are common on the world ocean, and especially in the coastal zone.

IS SEA LEVEL FIXED?

The recent debate about whether or not mankind is now busily altering the climate, and hence sea-level, is predicated in part on the assumption that sea level is fixed, unless it is altered anthropogenically. This conclusion may have arisen from ignorance of the geological past. Geological history, from the beginning of Cambrian time some 600,000,000 years ago when the details first become reasonably clear, has been a matter—in good part—of global or regional sea level changes, some of them exceeding 100 meters.

In the last 18,000–20,000 years, where the information is plentiful, sea level has risen roughly 140 meters (HOPLEY, 1982), and this rise was made up of many reversals, despite the net upward trend. When a few reversals are included in the calculation, the rate of rise becomes, or exceeds, 1 cm/yr. There is nothing in Pleistocene history to indicate that changes in sea level have been any smaller, or any less rapid, over the last two-to-three million years. In other words, the record shows that the current long-term pattern of sea level change is not far from 1 cm/yr, or 1 m/century.

But modern man has moved into the coastal zone in large numbers, and has created a great deal of expensive infrastructure. As he has done so, he has brought with him the lemma that sea level does not change: it is taken for granted that this is one of the immutable facts of life on our planet. The automatic corollary is that, if we observe a change in sea level, it must be due to the activities of mankind. Unfortunately, this is a very large error.

TANNER (1993) has showed, at least as far back as 7,800 BP, that beach ridge patterns indicate many sea level changes, some as small as a few centimeters, and some as large as 2–4 meters, at rates not far from 1 cm/yr.

Sea level has been rising for the last two or three centuries. If the recent past repeats itself, it will soon (geologically) be going down again (unless human activities finally over-ride natural changes).

"NOW" IS IN DYNAMIC EQUILIBRIUM

One of the interesting questions about the coastal zone is, "Can we formulate a more-or-less simple algebraic scheme which will summarize how incoming waves shape the shallow sandy sea floor, and, if so, will this relationship work both ways?" The intuitive answer, for many scientists, might be "Yes, and yes." But a very difficult problem arises when we try to carry it out.

The concept of dynamic equilibrium on the transverse nearshore submarine profile, when put into words, is simple enough. Per BRUUN (1954) undertook to measure real-world transverse profiles, in order to find out what form the algebraic equivalent should have. He came up with the expression

$$\text{Depth} = a * \text{distance}^{2/3}, \quad \text{or,}$$

$$h = a * y^{2/3},$$

where "a" is a coefficient to be determined. Robert DEAN (1977), likewise working with real-world profiles, provided a numerical value for "a" (which varies with the measure system that is used, although it is not always possible from the pertinent literature to tell which system this was; it also varies in some other ways, which makes matters even more complicated).

The actual value of "a" is not important at this point, because the basic concept is flawed. How did Bruun and Dean determine that actual (modern) transverse near-shore submarine coastal profiles are in dynamic equilibrium? This was done by assuming (whether consciously or not) that if such profiles *exist,* then they *must* be in dynamic equilibrium.

But this is the trap of assuming that sea level is fixed in position, and in fact that various other things, connected with this topic, are constant also. The geologist should know that sea level has changed a great deal in the last 18,000–20,000 years (perhaps 140 m; HOPLEY, 1982), and continues to change by modest amounts (a centimeter or so per year).

What is magic about 1980, or 1990, to provide that all of a sudden the transverse near-shore profile is in dynamic equilibrium? Why not peg this statement to a moment 20,000 years ago, when sea level was roughly 140 meters lower than it is now? Or to any other moment in between? Considering the form of the Bruun-Dean equation, it should be obvious that there is, *at the most,* only one moment in late Pleistocene and Holocene time when it might be correct (and we do not really know that there *is* such a moment).

"Now" is *not* a suitable time to scour the world's near-shore bathymetric charts for numerical constraints on any algebraic expression of the dynamic equilibrium concept (whether Bruun-Dean, or some other). Whatever the modern average profile may be, we are not permitted to place sea level at *any* convenient point (such as now) on that profile and to claim that we now have dynamic equilibrium. (Perhaps computer simulation will turn out to be helpful, in the light of our inability to investigate the ideal moment in geological history, if there was such, and hence our present inability to identify the "real" equilibrium profile.)

It should be pointed out that the Bruun-Dean equation is not even a dynamic statement at all (ignoring the "equilibrium" part, for the moment), for the simple reason that there is nothing "dynamic" about it: it is a geometric scheme (depth vs distance). A dynamic statement should have at least one wave parameter in it. Computer simulation may provide a practical means of attacking this difficult problem.

LANGMUIR CIRCULATION

The Langmuir three-dimensional circulation model was first presented by I. LANGMUIR (1938). He deduced from observation of wind-rows on the sea surface that there must be an overturn in the upper layer of the ocean, other than what we see in wave activity. This model can be visualized in terms of a system of parallel, horizontal cylinders (aligned more or less with the wind), in which even-numbered cylinders turn like the advance of a right-handed screw, and odd-numbered cylinders turn like a left-handed screw.

This model produces, at the ocean surface, alternate strips of rising water (slicks; divergence; little or no wave motion) and sinking water (convergence; trash, or foam, lines). These two kinds of lines are more-or-less parallel with each other, and typically are spaced some tens, or a few hundreds, of meters apart.

Because the overturn is really three-dimensional, this circulation commonly is stated to require water depths of roughly half of the spacing. Therefore, according to this line of thought, Langmuir circulation cannot penetrate into truly shallow water, and hence is unimportant among coastal processes: it is supposed to be limited to deep water, in the open sea, far from shore.

Lee ENTSMINGER (1978) presented data and photographs to show that Langmuir cells do operate, in some cases, right up to the beach. And field observations by TANNER and SOCCI (1980), at other times and places, led to the same conclusion. This work showed that the ellipticity of Langmuir cells can exceed the widely-quoted values of 1:1 or 1:2. The result should be visible at the beach, at least on a few occasions, as Entsminger reported, and it provides a hitherto little-used mechanism for explaining the origin of beach cusps, a fact that was not lost on TANNER and SOCCI (1980).

ENERGY PARTITIONING

One of the basic assumptions made by some coastal specialists is that 100% of the deep-water wave energy density is delivered to the surf zone, without any significant wave attenuation prior to breaking. If the experienced diver, who has felt the strong back-and-forth wave motion that can be experienced a short distance above the sandy shallow inner shelf, doubts this statement, he needs only examine the literature (FREDSOE and DEIGAARD, 1992, p. 88, for example, wrote that "the shore-normal energy flux at deep water . . . is therefore equal to the energy flux at wave-breaking . . .").

A quite different way of looking at the problem built into this assumption is to examine the horizontal distance between the point where the waves first touch bottom, and the outer edge of the surf zone. If this distance is extremely short, then it will be an easy matter to visualize essentially 100% of deep-water wave energy density being delivered to the surf.

KIRBY (1997) has made the pertinent statements here. This distance, he wrote, "is often no more than several wavelengths" (p. 56); and the evolution of waves "occurs over length scales which are not terribly long compared to a wavelength" (also p. 56). What does "several" mean? If only "a few," then the shoaling zone, prior to breaking, is very narrow: for 15-sec waves, perhaps about 1000 meters, for 10-sec waves, about 470 meters, and for 5-sec waves, about 120 meters. In the eastern Gulf of Mexico, where 5–7 second waves are common, this denies active wave-formed ripple marks on the sandy sea floor more than about 150 meters offshore, despite many fair-weather observations of these features out several kilometers from shore.

Computer simulation, using a well-verified algorithm (WAVENRG, May, 1974), shows that in fact large amounts of deep-water wave energy density are expended prior to reaching the surf zone. Although no method is available for knowing precisely what the partitioning *should* be, it seems fairly clear that the percentage (P) of initial wave energy density reaching the surf zone, after crossing the inner shelf, may be somewhere in the range of 30–70%, perhaps 40–60%. This figure must be variable, depending on wave period, initial wave height, and geometry (and roughness) of the shallow sea floor, but the results to date do not allow an allocation of 100%, or even 90% or 80%, to the surf zone.

It should be noted that on relatively small ocean water bodies, such as the Gulf of Mexico, the breaker-zone wave energy densities vary greatly in rather short distances parallel with the coast, indicating that losses on the ramp are highly variable, just as the computer simulation shows. A single wave that produces a breaker 1.0 m high, at one point, cannot also make a breaker only 0.30 m high not too far away, if the energy delivery to the surf is highly efficient (such as 90–100%).

Wave heights in the open Gulf of Mexico (as reported from wave-rider buoys) may be less than 2.0 to about 4.0 times the near-shore wave heights just prior to breaking on the pertinent beach. This suggests that in many cases the deep-water wave energy density may be four or more times the surf zone wave energy density, hence P is somewhere about 25%, or perhaps less, but not more than 50%.

From computer simulation, we learn that some profiles produce the same P at essentially all wave periods, whereas on other profiles, large wave periods are associated with large values of P. A close relationship between P and wave period (T), does not seem to be compatible with the idea of transverse dynamic equilibrium. But regardless of that, the delivery of wave energy density to the surf is typically far below 100%, probably less than 50% or 60%.

WAVE LENGTH

The wave period (T) is the primary parameter for identifying individual wave trains. It is quite clear that $T = 12$ (sec) represents a wave quite different from $T = 3$ (sec).

The second most important parameter is the wave height; this is the *only* variable component in E, the expression for wave energy density. Although we expect storm waves (such as $T = 12$ or 14 sec) to have a high energy content, it is not necessarily this way; after travel across a very wide ocean, 12-sec waves may have a greatly reduced height, and the energy content may be relatively small.

The third parameter is the wave length. In deep water, this is a rigorously defined function of the period ($L = 1.56 \times T \times T$), and therefore does not need to be reported separately, as long as we are limited to deep water.

In shoaling depths, things are quite different, and local wave length can be obtained only by iteration. This is because H and L are not diminished at the same rate, and the second derivatives do not always trend in the same direction. However, iteration can be carried out by computer methods, with an accuracy that depends largely on the detail with which we know the actual, or hypothetical, profile with which we are working.

It has been stated that wave length really cannot be determined very well, but that it does not matter, because it has little or no usefulness. It is true that obtaining reliable measurements of inshore wave lengths is difficult-to-almost-impossible, at most locations, and that in any case these wave lengths cannot be extrapolated to other depths, especially back to deep water. However, real-time wave data from various wave-rider buoys, out in deep water, permit the recording of wave heights and wave periods, and from the latter one can calculate—readily—wave lengths. Computer methods then permit a step-by-step assessment of various wave parameters, including wave length, as the wave approaches shore.

Wave length decreases markedly in the outer part of the shoaling zone, where changes in wave height are relatively small. As the change in wave height increases, the change in wave length decreases (but one is not the inverse of the other). Therefore, in the inner part of the shoaling zone, changes in wave height may be large, relatively, and changes in wave length may be small.

This can be visualized in terms of the vertical distribution of the wave energy density: the wave height identifies the wave energy content, but the wave length shows how that energy is distributed vertically (in deep water, it is concentrated near the water surface, and tapers off downward in non-linear fashion, but in shallow water, it is spread out more-or-less evenly over the vertical water column).

The wave length is rarely reported from wave-shoaling depths, yet it is the key to thinking about water celerities close to the bottom, where ripple marks are formed and maintained, and where sand grains are put into motion.

CONCLUSIONS

Seven specific problems are identified here, and each is stated to be a matter of considerable concern, because in each case various members of the coastal community continue to use outdated or counter-productive versions, despite the fact that at least some of these versions have been discussed, correctly, in the literature.

However, this is not a complete list, and it certainly does not include many matters which are suitable for further debate, such as the value of the coefficient "a" in the Bruun-Dean equation, or the question of whether or not the Bruun-Dean formulation is valid outside of a very narrow strip parallel with the beach.

LITERATURE CITED

BRUUN, P., 1954. Coastal erosion and the development of beach profiles. U.S. Army, Beach Erosion Board, *Technical Memorandum No. 44* 79p.

DEAN, R., 1977. Equilibrium beach profiles; U.S. Atlantic and Gulf coasts. *Ocean Engineering Report 12,* Department of Civil Engineering, University of Delaware, Newark, Delaware.

ENTSMINGER, L., 1978. Beach pads and beach cusps, St. Joseph Peninsula, Florida. Unpublished M.S. thesis, Florida State University, Tallahassee, 151p.

FREDSOE, J. and DEIGAARD, R., 1992. *Mechanics of Coastal Sediment Transport.* Singapore: World Scientific, 369p.

HOPLEY, D., 1982. *The geomorphology of the Great Barrier Reef.* New York: Wiley, 453p.

JOHNSON, D.W., 1919 (4th prntg., 1938). *Shore Processes and Shoreline Development.* New York: Wiley, 584p.

KIRBY, J.T., 1997. Non-linear dispersive long waves in water of variable depth. *In: Gravity waves in water of finite depth,* HUNT, J.N., (ed.), Southhampton, U.K.: Computational Mechanics Publications, Pp. 55–125.

LANGMUIR, I., 1938. Surface motion of water induced by wind. *Science,* 87, (2250), 119–123.

MAY, J.P., 1974. WAVENRG: A computer program to determine the distribution of energy dissipation in shoaling water waves, with examples from coastal Florida. *In: Proceedings First Symposium on Coastal Sedimentology,* TANNER, W.F., (ed.), Tallahassee: Geology Department, Florida State University, pp. 12–80.

TANNER, W.F., 1974. Applications of the model. *In: Proceedings First Symposium on Coastal Sedimentology,* TANNER, W.F., (ed.), Tallahassee: Geology Department, Florida State University, pp. 104–141.

TANNER, W.F., 1987. The beach: Where is the "River of Sand"? *Journal of Coastal Research,* 3, 377–386.

TANNER, W.F., 1993. An 8000-year record of sea-level change from grain-size parameters: Data from beach ridges in Denmark. *The Holocene,* 3, 220–231.

TANNER, W.F., 1995. Origin of beach ridges and swales. *Marine Geology,* 129, 149–161.

TANNER, W.F., 1996. Comment on Tanner, W.F., Origin of beach ridges and swales. *Marine Geology,* 136, 126-ff.

TANNER, W.F. and SOCCI, A., 1980. The coastal Langmuir cell project: Preliminary Report. *In: Proceedings 5th Symposium of Coastal Sedimentology,* TANNER, W.F., (ed.), Tallahassee: Geology Department, Florida State University, pp. 167–182.