HIGH FREQUENCY SPECTRUM OF OCEAN WAVES[1]

By

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The subject "Motion in the Ocean," assigned to me by President Bronk, includes fish, Aqua-lung divers and density flows. For a very good reason I must confine myself to motion of the water itself. Further, I will limit myself by considering only that part of the motion of ocean water that can be associated with surface waves. It is nearly, but not quite, the irrotational component of the motion. This of course limits the subject, but not so much as you may think. It is by no means impossible that some processes previously explained by turbulence—the oceanographer's way of saying that he doesn't understand—are part of the surface wave problem[2] and that the Gulf Stream would be only a fraction of its present magnitude if the high frequency tail of the surface wave spectrum, the wavelets and the capillaries, were to be wiped out by an oceanwide slick resulting from a diatom bloom of impossible proportions.

The evidence for the latter statement was an experiment performed by Keulegan in a closed flume and repeated by Van Dorn in a 200 m long model-yacht basin. If Tide, Surf, or some other detergent with a suitable oceanographic name, is sprinkled on a water surface over which a wind is blowing, the capillaries are quickly damped. This in turn has two effects: (1) The momentum transfer from wind to water (stress) is significantly reduced at all but low winds, and (2) the larger waves present, though not directly affected by the detergent, fail to be regenerated.

Apparently the tiny waves play a disproportionately important rôle in the problem of wind stress as well as in the generation of the more prominent parts of the wave spectrum. This was the beginning of a three year study.

The Spectrum. Let me first review what we mean by spectrum. Suppose we record against time the elevation of the sea surface above

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[2] See Carl Eckart, Statistical Hydrodynamics, in this issue.

mean sea level, $z(t)$. If we use a tide gauge, we automatically discriminate against waves and swell in favor of the longer period (lower frequency) oscillation of tides. With a standard wave recorder we record waves and swell, but the instrument is rather unresponsive to capillaries, and perhaps to tides as well. Each instrument has its working range of frequency. Consider now an ideal instrument which is tuned with equal sensitivity to some narrow band of frequencies, from $f_0 - \frac{1}{2}\,\delta f$ to $f_0 + \frac{1}{2}\,\delta f$, but which does not respond at all to frequencies outside of this band. We now record the elevation of the sea surface (relative to mean sea level) through this spectral window. This gives us a wiggly record $z_{f_0}(t)$. The mean value of z_{f_0} is zero and is not interesting. The mean value of $(z_{f_0})^2$ is not zero and is interesting. It will be designated as $S(f_0)\,\delta f$, where $S(f_0)$ is the spectral density of the surface elevation (or simply, the spectrum) for the particular central frequency, f_0, to which our instrument was tuned. We now tune to a different frequency, f_1, and get a value of $S(f_1)$; by tuning to many such frequencies we generate the entire spectrum.

For our purposes it is more convenient to consider the spectrum $S(k)$ generated by an instrument tuned to a certain limited band of wave numbers k, where $k = 2\pi$/wave length. For example, the pitching motion of a ship is responsive to neither capillaries nor tides but only to wave lengths of the general order of the length of the ship. In actual practice the best way of determining $S(k)$ is to measure $S(f)$ and to make a theoretical transform. As a result of the work by Pierson and Neumann at New York University we have a fairly good idea of what $S(k)$ looks like (Fig. 1). It is peaked near

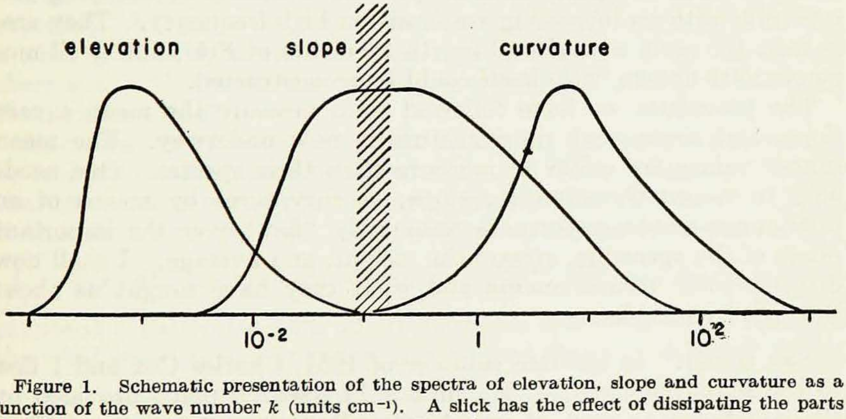

Figure 1. Schematic presentation of the spectra of elevation, slope and curvature as a function of the wave number k (units cm^{-1}). A slick has the effect of dissipating the parts of the spectra lying to the right of the vertical band.

$k = 5 \times 10^{-4}$ cm^{-1}, meaning that wave lengths of $2\pi/5 \times 10^{-4}$ cm, or about 100 m, contribute most to the spectrum. The contributions from waves longer than 500 m and shorter than 1 m are negligible. The total area under the curve is the contribution to the mean value of $z^2(t)$ from waves of all lengths, in other words, the mean square elevation, $\overline{z^2}$. The square root of this value, the rms elevation, comes close to what an observer would estimate to be the amplitude of the waves.

The two other curves in Fig. 1 are the spectra of slope and of curvature. The interpretation is as before. All we need to do is insert the word "slope" or "curvature" in the place of "elevation." It can be shown that the three spectra are given by

$$S(k), \qquad k^2S(k), \qquad k^4S(k), \tag{1}$$

respectively. Hence they are progressively weighted in favor of larger k's (i.e., short wave lengths, short periods, high frequencies) so that the study of slope and curvature provides a tool for learning about the high frequencies. The areas under the three curves are then interpreted as follows:

$$\begin{aligned} &\text{mean square elevation:} \int_0^\infty S(k)\, dk, \\ &\text{mean square slope:} \int_0^\infty k^2S(k)\, dk, \\ &\text{mean square curvature:} \int_0^\infty k^4S(k)\, dk. \end{aligned} \tag{2}$$

These three integrals likewise provide a means of studying the spectrum with an increasing emphasis on high frequency. They are, in fact, the zero, second and fourth moments of $S(k)$, and if all moments were known, $S(k)$ itself could be reconstructed.

The procedure we have followed is to measure the mean square slope, and some work on curvature is now underway. The mean square values are easier to measure than their spectra. One needs only to record elevation (or slope, or curvature) by means of an instrument whose response is reasonably "flat" over the important range of the spectrum, square the output, and average. I shall now describe some measurements and what they have taught us about the high frequencies.

Sun Glitter. In the late summer of 1951, Charles Cox and I flew to the island of Maui, T. H., in a B-17 research plane provided by the Air Force as part of our Air-Sea Boundary contract. Gifford

Ewing's 58′ schooner, the REVERIE, went out under sail to make the necessary surface measurements, particularly of wind speed and direction. The principal goal was to measure mean square slope from photographs of sun glitter. The idea is simple. If the sea surface were flat calm, the glitter would be small; in fact, it would consist of a single undistorted image of the sun itself. But when the water is roughened there are thousands of dancing highlights within an extended glitter pattern. Each is a tiny image of the sun due to a reflection of a ray from an appropriately inclined water facet. The principle is quite old, having in fact been mentioned by Spooner in 1822 in *Correspondence with Baron de Zach.*

We have related the statistics of the slopes to the distribution of radiance in the glitter area. Our results, which I believe are more quantitative than those previously obtained, have been reported in a paper in which Cox is senior author.[3] Let me review the results briefly.

Distribution of Slopes. One result concerns directionality. It is possible to build up the mean square slope from contributions by waves of all possible directions, just as we had previously considered contributions by waves of different frequencies. When this is done it is found that, of those relatively short waves that are prominent in the slope spectrum, a substantial portion travels up to 60° to either side of the wind. This is a surprisingly wide beam into which the winds generate waves, a fact that must be taken into account in any theory of wave generation. The corresponding beam of the elevation spectrum, which features the longer waves, appears to be a bit narrower.

The slope distribution itself is nearly Gaussian, and, as is to be expected, the mean square values increase with wind speed. For a wind speed of 14 m sec^{-1} the rms slope is tan 16°. This means that there is a 10% chance that the slope exceeds 22° and a 1% chance that it exceeds 30°.

Since the distribution is nearly Gaussian, it can be represented by a single number, such as its mean square value. Cox and I found that the mean square slope increases *linearly* with wind speed. Now, if the elevation spectrum $S(k)$ is known, the mean square slope, $\int_0^\infty k^2 S(k)\, dk$, can be derived theoretically. Gerhard Neumann has proposed a particular form of an elevation spectrum which involves the wind speed as a parameter, and we find that it yields just such a

[3] Cox, Charles and W. H. Munk, 1954, statistics of the sea surface derived from sun glitter, J. Mar. Res., *13* (2): 198–227.

linear relation as we have observed. This is a satisfying result. It means that Neumann's lengthy observation of wave heights from the bridge of a German freighter plowing the Atlantic and our photographs of the sun's glitter in the Alenuihaha Channel yield consistent results. If I seem overjoyed by a consistency that is no different from that which should be expected and which is in fact found in other fields of physics, it is because most oceanographers have not been spoiled in this regard.

The linear relation is only part of the story. What about the proportionality constant? Here it is found that the rms slopes computed from Neumann's spectrum equal 60% of the values we observed. There are two ways of remedying the discrepancy, but I am not sure whether either of them, or the discrepancy itself, is significant. One is to fit Darbyshire's recorded data,[4] which are in some aspects superior to Neumann's observations, to Neumann's spectral function. The other is to compare Neumann's original coefficient with our observations in the presence of slicks.

The last remark needs some amplification. One of the duties of the REVERIE was to pump a mixture of diesel oil, used crankcase oil and sardine oil onto the water to make a coherent slick about one-quarter square mile in area. It was found that the mean square slope was reduced in this way by a factor of two or three, apparently by the elimination from the spectrum of waves shorter than a foot (Fig. 1). We have considered at some length why this should be so. We know, by the amount of oil added and by the presence of interference colors, that the slick was perhaps 1000 molecules thick. Reynolds' classical theory for monomolecular layers does not apply. The reason for the damping remains obscure, but the question *whether* oil dampens waves can be answered in a fashion. By removing only the high frequency components, the mean square elevation is nearly unaffected, the mean square slope is reduced by a factor of two or three, and the mean square curvature is virtually eliminated. The optical properties of the surface are greatly altered, giving the appearance of smoothness seen otherwise on calm days only. I wonder whether this is not part of the reason why oil has been poured on troubled waters since time immemorial.

Curvature. The success achieved by the Neumann spectrum in predicting the distribution of slopes provides a temptation to extend

[4] Darbyshire's own fit, defective in the high frequencies, yields a *decrease* in mean square slope with increasing wind speed. It has been suggested by Deacon at the UGGI meeting in Rome that the discrepancy may be due in part to generation in shallow water.

it even further to inquire what it has to say about curvature. In doing so we realize that this means extending it far beyond the range intended by the author.

The Neumann spectrum predicts a mean square curvature which corresponds to a 12 cm radius of curvature and which is nearly independent of wind speed. Cox is now making measurements of curvature by sweeping a photomultiplier tube across a small area of sea surface and counting glitters. It can be demonstrated that the values so obtained can be used to compute the statistics of curvature. It is really too early to be certain, but it now appears as if the Neumann curvature is too low and that curvature does increase with increasing wind speed. Thus it may turn out that the Neumann spectrum is deficient in the capillaries and that we may find here a separate band of high activity.

It should be mentioned that the group velocity of waves reaches a minimum value of 17.8 cm sec^{-1} for a wave length of 4.4 cm. The minimum represents a balance, so to say, between surface tension and gravity. Shorter waves are principally affected by surface tension, longer waves by gravity. It is not unusual in geophysical problems to find prominent features in records to be associated with such a minimum in group velocity.

Dissipation. Another feature may make the study of slope and curvature rewarding. It can be shown that the energy dissipated due to molecular viscosity by wave numbers in the range $k \pm \frac{1}{2}\,\delta k$ equals

$$4\mu g\, k^2\, S(k)\, \delta k + 4\mu\gamma\, k^4\, S(k)\, \delta k \quad , \tag{3}$$

where μ is the molecular viscosity and γ the specific surface tension. Comparison with (1) shows that the two terms are proportional to the slope and curvature spectra, respectively. The total dissipation, due to all wave numbers, is therefore

$$4\mu g\sigma^2 + 4\mu\gamma\kappa^2 \quad ,$$

where σ^2 and κ^2 are mean square slope and curvature (eq. 2).

For a 10 m sec^{-1} wind we find $\sigma^2 = .06$. Blowing wind over Washington tap water, Schooley found $\kappa^2 = 1$ cm^{-2}. With these values, each of the two above terms equals approximately 2.5 ergs $cm^{-2}sec^{-1}$. If it should turn out that there is a separate band of high activity in the capillaries, then the second term would be correspondingly larger.

We have here an optical method for studying an energy sink of the ocean wave spectrum. Perhaps the energy is passed on by some nonlinear process (white-capping?) from the longer to the shorter waves until it is finally dissipated by the high-frequency components. This would be analogous to conditions as found in the turbulence spectrum. The point of view is opposite to the one taken at the start of this discussion, when we pointed out that the growth of the larger waves requires the existence of the very short ones. Could it be that the capillaries provide for both construction and destruction of ordinary waves?

It should be pointed out that dissipation by capillaries is not the only way in which wave energy can ultimately be dissipated. Surface wave energy may also go directly into turbulent energy, by breaking or otherwise, and be dissipated finally by the small elements of the turbulence spectrum. Perhaps one can gain some insight into this aspect of dissipation by the following approach. It was pointed out to me by Norman Barber that the statistics of *acceleration* are closely related to the statistics of slope. Thus, for a wave profile $z = A \cos (kx - \omega t)$, the slope $\partial z/\partial x$ has an amplitude kA, the vertical acceleration $\partial^2 z/\partial t^2$ has an amplitude $\omega^2 A$, and, for the linear theory, $\omega^2 = gk$. Thus the accelerations at the surface, measured in units of g, equal the tilt in radians. For a 14 m sec^{-1} wind we found an rms slope of tan 16°; accordingly the rms acceleration is about $\frac{1}{4}\, g$. There is a 1% chance that it exceeds $\frac{1}{2}\, g$. If one dares to extrapolate this distribution even further, one can compute the probability that the acceleration exceeds g. Spray would certainly be formed if the acceleration exceeded g. There are obvious difficulties with this approach, but in some ways I find it more attractive than a discussion based on conventional breakers with 120° cusps. As an aside, what is the effect on micro-organisms near the ocean's surface when they are being tossed around with an rms acceleration of $\frac{1}{4}\, g$?

Wind Stress. We now return briefly to a problem mentioned at the beginning of this paper, namely the transfer of momentum from wind to water. At the Rome meeting of UGGI, Charnock gave a critical summary of reliable observations and concluded that this transfer increased with wind speed U faster than U^2 but slower than U^3. I think our results are not inconsistent with this conclusion.

If spray and rain can be neglected, the transfer of momentum is accomplished by two processes. First there is a transfer by *skin drag* as it is found whenever fluid flows over polished plates. It equals $\mu\, (dv/dz)_{z=0}$ either in the water or the air, with the shear dv/dz evaluated at the very surface. There have been no systematic attempts to measure dv/dz close to the surface. I am impressed with a casual observation by Kittredge. Working from a skiff, he put some dye on the water and then sprinkled a bit of talcum on the dye. He found that the talcum drifted apart from the dye mark at a rate of something like 10 cm sec^{-1}. If we set the thickness of the dye as 1 mm, then the shear must have been of the order

$$\frac{10 \text{ cm sec}^{-1}}{0.1 \text{ cm}} = 100 \text{ sec}^{-1} ,$$

and μ times this quantity equals 1 dyne cm^{-2}. This is of the expected magnitude for the shear stress.

In analogy with other experimental evidence one can expect the skin friction to be proportional to U^2. In addition there is a transfer due to *form drag* resulting from systematic differences in pressure on the windward and lee sides of waves. Air impinging onto a vertical surface exerts a pressure ρU^2. If the slope is zero, the pressure is zero. The simplest interpolation between these values is then $\rho U^2 m$, where $m = \sin \theta$ and θ is the tilt in the direction of the wind. So far this is equivalent to Jeffreys' sheltering theory. The horizontal component of this pressure is $\rho U^2 m^2$. If the tilted surfaces move downwind with a velocity C, the appropriate expression is $\rho\ (U - C)^2\ m^2$. Over land, $C = O$ and m^2 is constant. The form drag accordingly varies as U^2. Over water, if $C \ll U$, then the average value of the above expression is $\rho\ U^2 <m^2>$, where $<m^2>$ is the mean square slope component. Our results show that $<m^2>$ increases linearly with U, and hence the form drag varies as U^3. To allow for C, one is tempted to write the above expression in the spectral form

$$\rho \int_0^\infty [U - C(k)]^2\, k^2\, S(k)\, dk\ .$$

The integrand represents the contribution to the total form drag by wave numbers in the interval $k \pm \frac{1}{2}\, \delta k$. The integration can be carried out for the Neumann spectrum in closed form. One finds that the drag again varies as U^3 and that the resulting formula can be made to fit Van Dorn's and Charnock's results.

I won't make too much of this circumstance because of the many uncertainties involved. At what elevation is the wind to be measured? What is the effect of the lapse rate? Can the momentum transfer (and pressure pattern) for each wave number be assumed independent of other wave numbers, as implied in the foregoing integral? Can skin drag be added to surface drag? Is $C(k)$ the phase velocity, or does it involve also the orbital velocity of the larger waves present?

But, regardless of these difficulties, I am convinced that the form drag is much more closely related to the slope statistics than to the elevation statistics. I believe this conclusion is a departure from the usually accepted view. There one deals with Reynolds numbers, or with roughness coefficients; and in either case, a length parameter is involved which is supposed to be mysteriously related to wave height. If the elevation statistics were essential, then the effect of a six foot sea on form drag would be equivalent to that of a six foot tide! If this were so, one would expect a critical dependence of wind stress on fetch and no measurable effect due to a detergent spread on the water surface. On the other hand, if the slope statistics, hence the high frequency spectrum, is the essential element of surface roughness, then the dependence of wind stress on fetch should be slight, as observed; and the reduction by a slick should be considerable, again as observed.

Before leaving this subject I should like to bring up one more possibility. The glitter measurements have shown that the wave spectrum is symmetrical with respect to wind direction, as expected. But under special conditions this directional spectrum might be skewed in the sense that waves travelling to the left of the wind would be more prominent than those to the right, or vice versa. This would give rise to a crosswind component in form drag. Henry Stommel has made the interesting suggestion that attenuation of high-frequency waves by slick bands lined up with the wind would result in just such a directional skewness and that this would give a component of drag into the slick from both sides of the slick. There would then be a convergence in flow which might serve to maintain the slick in the compressed state prerequisite to wave attenuation.

Comments on the Statistical Approach. By this time some violent objections to our statistical point of view will have developed in the mind of the listener. If he is a plant taxonomist he is likely to ask where we would stand today if our knowledge of diatoms and their wondrous diversity consisted of statistical descriptions of their geometry. Are we obscuring observable quantities behind statistical curtains? Certainly "mean square slope" and "mean square curvature" are not readily identified with anything we can see on the sea surface. And even if it is admitted that some statistical treatment is required, why choose probability distributions and spectra among the many possible statistical descriptions?

I really don't know of a good answer to these objections, but at least a few things can be said in favor of our choice. Suppose you were to ask me about wave conditions off La Jolla in June and I were to reply by presenting you with file cabinets filled with records of wiggly lines. I don't think this would be quite satisfactory. Apparently some kind of statistical generalizations are in order. But which are the most pertinent? This depends entirely on the problem to be solved, and for a great variety of problems the spectral description is convenient. Pierson and St. Denis have made remarkable progress in studying the motion of ships in a rough sea by first decomposing the sea surface into a spectrum of elementary sine waves of all possible lengths and directions and then considering the response of the ship to each of these. The essence here is that the response of the vessel be *linear*, i.e., that the total response should be the sum of the responses to the individual trains; this is pretty much the case so long as there is no green water over the bow. An example for high frequency waves is the scatter of radar and sound waves from a rough sea surface. This can be estimated from a knowledge of the wave spectrum. The only components of the wave spectrum that are important here are those whose wave lengths are of the same order as the wave lengths of the radar or sound waves. For present equipment this means the high frequency tail of the spectrum.

However, our principal interest is not to compute the scatter of radar waves, even though this is a matter of some importance, but to work toward a respectable theory of the generation and decay of ocean waves and of the transfer of momentum across the air-sea boundary. How does the spectrum help here? In other fields of physics a knowledge of the spectrum has helped a lot in pointing the way toward a successful theory. If at some future time a theory of wave generation should critically confirm the Neumann spectral function, such a theory should be considered a major achievement.

On the other hand, it is certainly dangerous to select one's statistical tools without having first had a good look at observations uncontaminated by analysis. Fig. 2 shows a remarkable photograph taken by von Arx off the Woods Hole dock. In spite of considerable effort I have never been able to improve on this picture. It shows a series of tiny wavelets on the lee side of larger waves, with the shortest wavelet farthest out in front (downwind). On days with light winds, wavelets can be seen by the naked eye. Wavelets are eliminated by a slick, and I suspect, therefore, that they represent a major contribution to the statistics of curvature. However, what may be the essential features of wavelets, their dispersive appearance and their location relative to the crests of the longer waves, could be buried by routine measurements of curvature. To put these features into an appropriate statistical language requires more elaborate statistics. I think this photograph indicates how important it is to look at the raw data before deciding on pertinent statistical parameters.

I have a simple hypothesis as to why these dispersive wavelets are

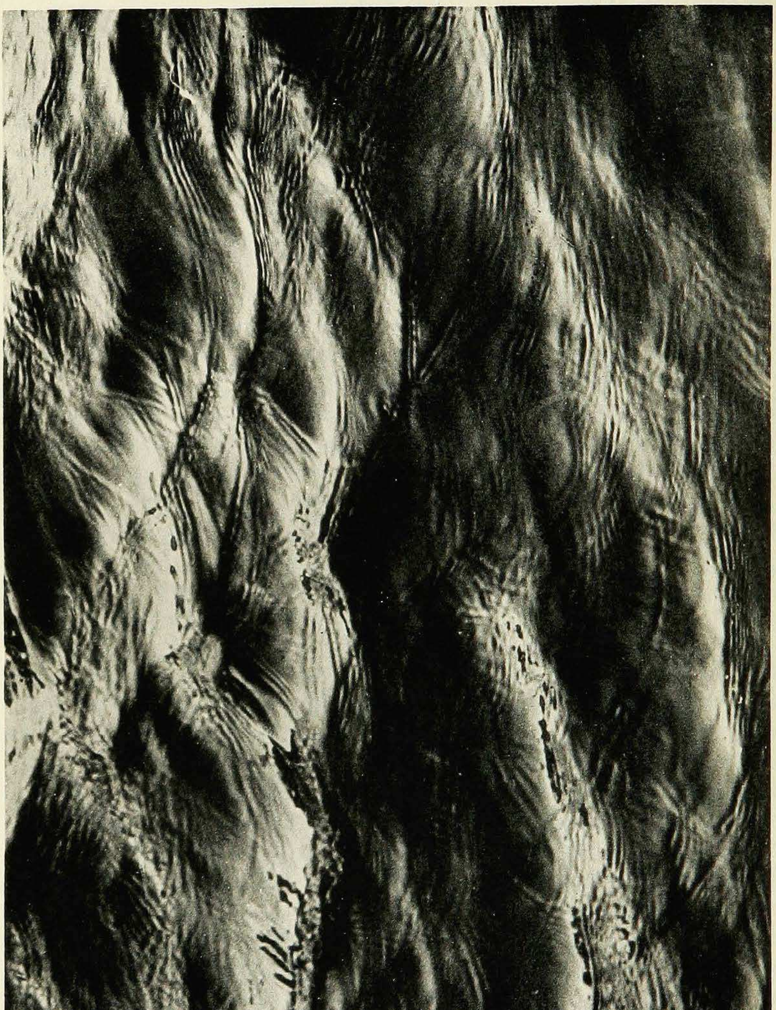

Figure 2. The wind blows from right to left.

found only forward of the wave crest and why the short waves lead the long waves. Consider (Fig. 3) an underlying long-crested wave with height H, phase velocity C, and orbital velocity U (less than C). Capillaries with phase velocity c ride this wave. Their total rate of advance is $c + U$, and they will remain stationary with respect to the wave crests provided

$$C = c + U(x) \ . \tag{4}$$

Consider first the region (1) to (2) forward (to the right) of the crest. If $c + U$ were to exceed C slightly, the capillary would advance to a region of lesser U until eq. (4) is again satisfied. If $c + U$ is slightly less that C, then the capillary falls back toward the crest where the orbital motion is faster, and equilibrium is again achieved. However, in the region (2) to (3) behind the crest, a slight variation from the stationary condition leads to conditions for the permanent

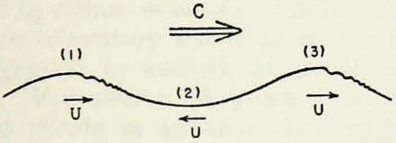

Figure 3.

removal of the capillary from this region. Stationary conditions exist only forward of the crest, and to the rear of the trough. This is in fact where the capillaries are found.

We now wish to find the capillary wave lengths. The simplest case is one for which $L \gg L_m$ and $l \ll L_m$, where L and l are the lengths of the waves and capillaries, respectively, and where $L_m = 1.7$ cm, the wave length for minimum phase velocity. It can then be shown that

$$l = \frac{L_m^2}{L}\left(1 - \pi \frac{H}{L} \cos \frac{2\pi x}{L}\right)^{-2} ,$$

where x is the distance forward of the crest. Since $H \ll L$, this means that l is largest near the crest, then diminishes forward. Qualitatively this agrees with what is observed.

There is an alternative, and perhaps an even simpler hypothesis. Suppose one takes a straight edge (thin compared to L_m) and brushes it across water at a velocity exceeding the minimum phase velocity (23 cm sec^{-1}). Then one eventually finds a series of capillaries preceding the straight edge and gravity waves following it, with each crest remaining parallel and at a fixed distance from the ruler. The short waves are in front because their energy is propagated faster than the wave crests. The reverse holds for the relatively long waves to the rear. This is the one-dimensional equivalent to the "fish-line problem," and it is fully discussed by Lamb (p. 270–271, 6th ed.).

It is not unreasonable to assume that, at the very crests of waves, where instability first manifests itself, such a travelling stress is applied. If this happens over only a short period, then there will be noticeable dispersion, with the shortest waves farthest in front. In this sense, the capillaries on the photograph may represent a shock front in a dispersive medium.