THE AIR-SEA MOMENTUM EXCHANGE

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Abstract. Rather simple general arguments lead one to conclude that at least a very large fraction of the momentum transfer from air into water must be in the form of wave generation. This is in agreement with some recent measurements. The theory of wave generation remains incomplete and it seems probable that more attention should be paid to three-dimensional motions.

The nature of the mechanism for the transport of momentum between the atmosphere and the surface of water attracts scientific attention for two reasons: first, parameterization of this process is extremely important to the understanding of the circulation of the atmosphere and in particular of the ocean; and second, the nature of the process, intimately connected as it is with that of wave generation, is scientifically fascinating. It may also be the case that a fairly complete understanding of the process will be required if the parameterization is to be fully satisfactory.

Let us attempt to examine the facts as we have them from as general a point of view as possible.

First, consider the information that we have on the magnitude of momentum transfer. Over the last decade a substantial body of data has been collected, using modern methods. Karl Brocks and his team from the University of Hamburg played no small role in amassing these data, and Figure 1 shows some of their data (Brocks and Krügermeyer, 1970). Figure 2, taken from Smith (1973), presents data collected by some Canadian groups.

These data show that the drag coefficient, defined as $C_{D_{10}} = \tau / \varrho U_{10}^2$, where τ is the stress, or rate of momentum transfer, ϱ is the air density and U_{10} is the mean wind velocity at 10-m height, has a value in the neighbourhood of 1.3×10^{-3} plus or minus

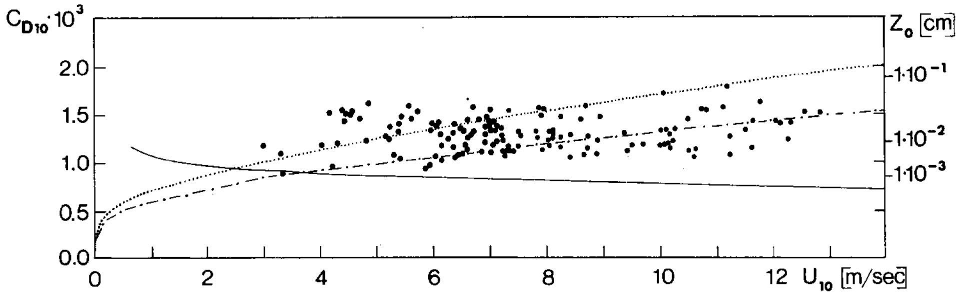

Fig. 1. Redrawn from Brocks and Krügermeyer (1970). Drag coefficient $C_{D_{10}}$ and roughness length z_0 as a function of wind speed. The data were obtained under neutral conditions. The solid line is the smooth surface result. The other two curves correspond to the Charnock formula, $z_0 = u_*^2/ag$ with $a = 28.5$ for the dotted line and $a = 81$ for dash-dot line.

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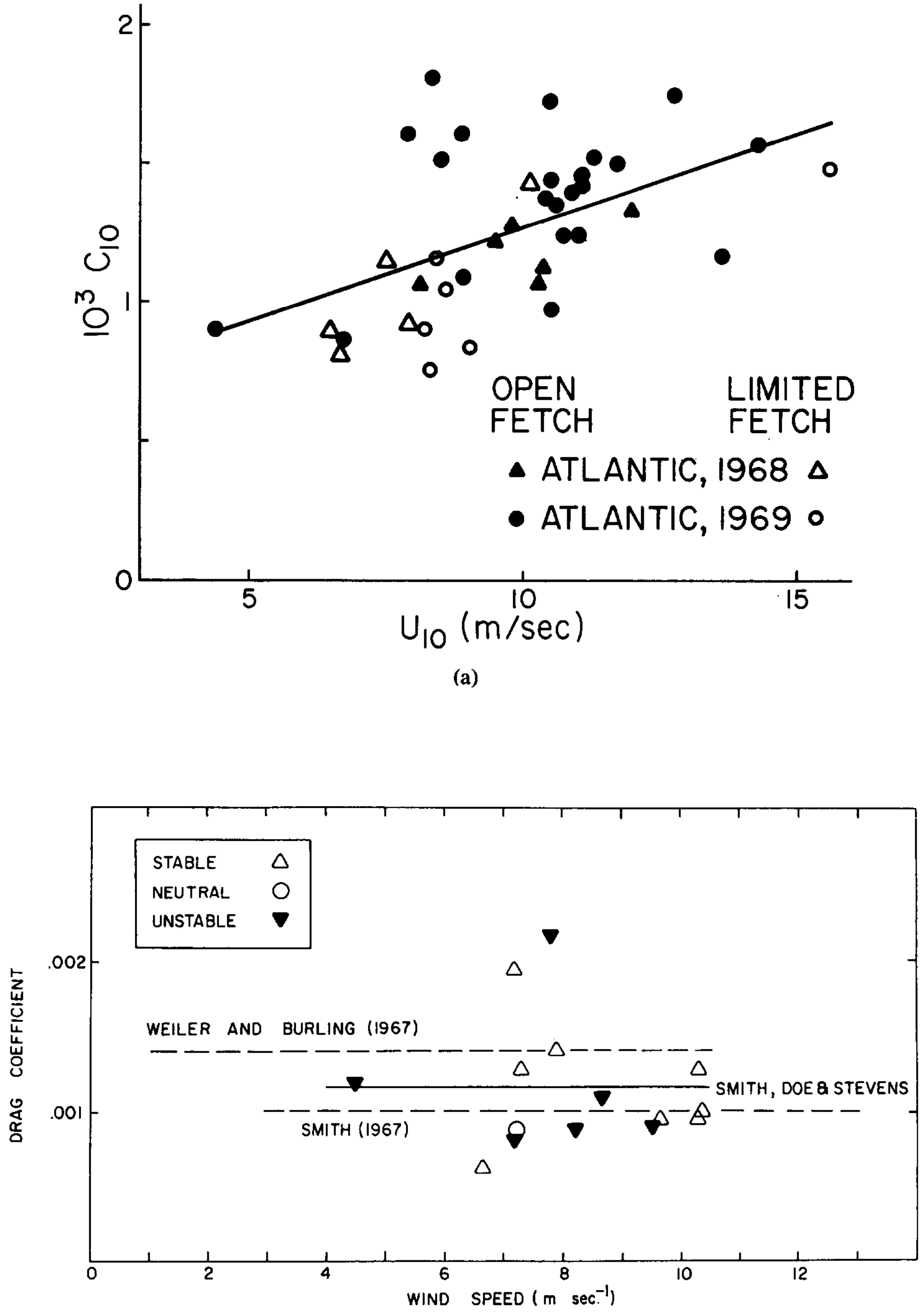

(b)

Fig. 2. Drag coefficients measured by some Canadian groups. (a) Drag coefficients copied from Smith (1973). (b) A compilation of results copied from Smith (1970).

some 20%, for wind speeds between about 2 and about 12 or 15 m s^{-1}. There may be some increase in drag coefficient with wind speed, some dependence on the stability of the air column, some dependence upon wind duration and fetch and perhaps on other parameters, but anyone who did not wish to believe in these dependencies would be difficult to convince from the data.

There is always a substantial amount of scatter in published drag coefficient results. Much of this is undoubtedly due to the technical difficulties of making the measurements themselves. For example, when the measurement of the eddy flux is direct, not only are the results very sensitive to the orientation of the instruments, which are themselves rather 'fussy', but the quantity being measured has some inherently unpleasant characteristics. Typically, measured values of the downstream and vertical components of the wind, u and w, are very nearly Gaussian in distribution and have rather straightforward statistical characteristics. The product uw, which determines the momentum transfer, is much less well behaved. Occasional very large values are of much more importance. For example, some unpublished data obtained recently at the University of British Columbia reveal coefficients of excess, for the distribution of the quantity uw, to be in the neighbourhood of six or more, indicating that the distribution is very far from Gaussian. The theory of the statistical behaviour of variables having unusual distributions is not well established. There is no theoretical basis for determining, for example, how long a record is required to establish the value of $\overline{uw}$ within given limits in a given situation. Empirically, however, those working with these data observe that the quantity does not 'settle down' at all rapidly and indeed in most cases it is difficult to establish a value before the overall conditions change. This sort of thing may well be the source of much of the large scatter in the observed data. On the other hand, there may very well be some external parameters which have not yet been closely examined but which contribute importantly to this scatter.

Scattered though the data may be, one thing, however, is quite clear. Over this range of wind speed over the sea surface, the drag is substantially greater than that over a flat surface. (The flat surface drag coefficient is plotted as a solid line on Figure 1). It is almost incredibly smooth, considering its appearance – much smoother than mown grass for example which has a drag coefficient of about 5×10^{-3} – but it is not like glass.

Now consider some quite general comments which can be made without requiring any detailed observations. It is interesting to note that the interaction between wind and water has very little in the way of natural scale. There is no natural velocity scale in the wind field, as it is usually considered, except for the friction velocity u_*. Typical analyses of the problem assume that the depth of the air column is effectively infinite. In the neutral stability case, usual turbulent boundary-layer analysis then yields a logarithmic profile

$$U(z) \sim u_* \ln z/z_0 \tag{1}$$

where $u_*^2 = \tau/\varrho$, z is the height above the surface and z_0 is the roughness length. The wind speed increases indefinitely, if slowly, as the height increases (Ekman-layer effects usually being taken to be irrelevant). Also, there is no natural length scale in a neutrally stable air flow apart from the roughness length. In flows over solid surfaces, the roughness length is usually determined by the nature of the surface. However, here the surface roughness is itself largely determined by the wind, and the parameter z_0 must be regarded as internally determined as part of the problem, rather than externally imposed. Solution to this problem then requires that z_0 be determined as a function of u_* and such other parameters of the wave field as may be relevant.

On the other hand, in the water, there is a natural length scale L, associated with the longest waves which have been vigorously excited. The length of these longest waves depends upon wind speed, the duration of the wind and the fetch. There is a characteristic velocity C_L associated with L, given by the dispersion relationship for gravity waves. As well, this wavelength L can be used vertically in the air to establish a height at which the wind speed can be determined. We thereby obtain another scale velocity U_L. The ratio C_L/U_L is a dimensionless parameter which can differentiate one wind-driven gravity wave system from another. This ratio normally approaches unity when both the duration of the wind and the fetch become large. (The parameter is closely related to, although not quite identical to, the so-called 'wave age'.) If only gravity waves are important, we would conclude by dimensional analysis that

$$z_0 = (u_*^2/g)\, f\,(C_L/U_L) \tag{2}$$

which is a generalization of the well known Charnock (1955) relation

$$z_0 = u_*^2/ga \tag{3}$$

where a is a constant.

The observational evidence is that the functional dependence on C_L/U_L is weak. If $f(C_L/U_L)$ is essentially constant, then one expects the Charnock relation to hold. With the wind-speed variation with height taken to be logarithmic, we get a relationship between drag coefficient and wind speed, indicating a significant increase in drag coefficient with wind speed. On the whole, most observations tend to indicate that if there is an increase in drag coefficient with wind speed, it is weaker than that predicted by this relation, as is shown in Figure 1.

Of course there are some other factors and perhaps they should not be ignored. Both the air and the water have molecular viscosities which tend to become important when scales become less than about a few centimeters, somewhat smaller in the water than in the air. Perhaps more important is the fact that the water has surface tension. This surface tension adds another length and velocity scale to the system – the wave length λ_m and the propagation speed C_m of the slowest waves in the system – respectively about 1.7 and 23 cm s^{-1} for clean cold water, but dependent on both temperature and surface contaminants. It is possible that the parameter C_L/C_m is an important one. Perhaps equally or even more important is the influence of the surface tension at the crests of the larger waves. The wave-generation process is limited by the fact that for a given wavelength, a wave can have only a limited amount of energy before the crest cusps. Near this limiting amount of energy, strong non-linear processes come into play which cause vigorous wave dissipation, absorbing the additional energy being put in from the wind. At fairly low wind speeds, the surface tension is able to hold the surface together and these non-linear effects involve the creation of surface tension-dominated waves which are rapidly dissipated by viscosity. At higher wind speeds, the surface tension is unable to hold the surface together and it becomes disrupted, with the formation of droplets. The wave breaks; a different non-linear effect controls the wave amplitude. The difference between breaking and non-breaking waves may be of importance in determining the drag coefficient. Certainly, in principle, there is no reason to believe that it should not. In practice the evidence is slight. What evidence we have seems to indicate that any additional effects such as those of surface tension seem to act to make the drag coefficient less dependent on wind speed than that predicted in the Charnock relation (3), as illustrated in Figure 1.

Be that as it may, let us return to the points made above. The drag over a water surface is substantially greater than that over a smooth surface. Particularly at lower wind speeds, the surface may not be fully rough in the aerodynamic sense, but it is equally not smooth in the aerodynamic sense and at least a large fraction of the momentum transfer must be through the mechanisms of aerodynamically rough flow. These mechanisms are known to involve the correlation between the surface slope and the pressure, with the pressure on the upstream slopes being greater than that on the downstream slopes. This being the case, we must conclude that momentum transfer into the water from the air is to a very large extent effected by pressure fluctuations. As I pointed out, many years ago now (Stewart, 1961), this means that when one looks at this phenomenon from the point of view of the water, one finds that the momentum must be going into the water by pressure fluctuations – and the only kind of motion which pressure fluctuations are able to set up in a homogeneous fluid are irrotational ones.

Of course, natural waters are rarely truly homogeneous so this statement may not seem to be a very strong one. Motions other than irrotational ones can in principle, and probably will in fact, be induced. However, occasions do occur, particularly in the autumn, when the water is very close to homogeneous. There is no evidence that either the wave-generation process or the air-sea momentum interchange process is appreciably different under these circumstances than in the more usual ones for which the water is stratified. (As an aside, it is rather curious that while a great many parameters have been examined for their possible influence on the air-sea momentum transfer process, the degree of stratification of the underlying water seems not to have been one. It may be that everyone has intuitively assumed that this parameter could not be important. I myself share this intuitive feeling.)

Having dismissed that question, then, we return to a requirement that the momentum from the air be transferred into the ocean in the form of irrotational motion. This leads us to seek some irrotational motion capable of carrying horizontal momentum relative to that of the deep water. That is, assuming the deep water to be stationary, the motion we seek in the upper water must carry horizontal momentum. Figure 3 represents a vertical cross-section of the water body with the x axis being that for

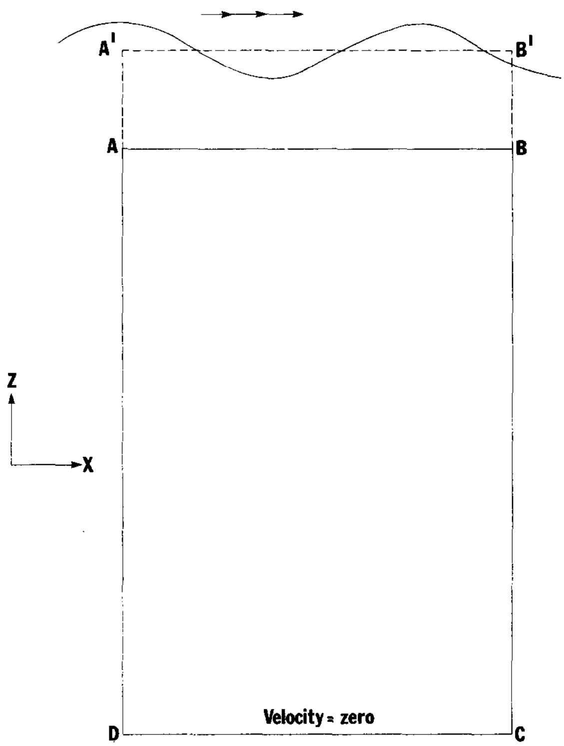

Fig. 3. Diagram illustrating the argument that an irrotational flow carrying momentum should be wave-like. The circuit A, B, C, D, must have no circulation, but the circuit A', B', C, D, may have circulation since parts of it lie above the surface and therefore outside of the irrotational flow.

which the motion we seek must have positive momentum. The diagram illustrates the following argument. The total momentum of a box of unit thickness in the y direction (into the page) is given by

$$\varrho \int_{\text{area}} u \, \mathrm{d}x \, \mathrm{d}z .$$

For this to be greater than zero, there must be at least one level for which

$$\int u \, \mathrm{d}x > 0 . \tag{4}$$

Indeed the requirement that the motion be irrotational means that there must be a range of levels for which this inequality is valid since if inequality (4) resulted from the existence of a delta function for u at some value of z, infinite values of shears would surround this value of z – incompatible with the assumption of irrotationality. We may draw a rectangular circuit,with the upper path being at some level for which inequality (4) is valid and the lower line of the path in deep water, where the water is assumed stationary. Now since the motion is irrotational, the line integral of velocity around the circuit must vanish, so we require that the line integral of velocity at the right-hand side of this circuit be up or that the line integral of the velocity on the left-hand side be down, or both. Further, we require from the nature of the problem that the magnitude of these vertical line integrals must increase as the horizontal size of our circuit increases – i.e., as the length AB increases.

The vertical velocities necessary are very difficult to believe in. It might be easier if they had the opposite signs! A vertical velocity upwards at the right and downwards at the left demands from continuity a divergence at the right and a convergence at the left and this cannot easily be reconciled with a horizontal motion to the right in the upper regions. We seem to be faced with an impass. However, the escape is easy. We know that irrotational waves carry momentum. They escape from the above argument because it is possible to draw a horizontal line, like the line $A'B'$ drawn dashed in Figure 3, which is only partly within the irrotational fluid. Other portions of the line are outside of the irrotational fluid (in this case in the air), and are not constrained by the irrotational requirements. In the case of the waves, where the line is in the water, the motion is systematically to the right. Any closed circuit entirely within the water like $ABCD$ in Figure 3, has zero circulation. But a closed circuit, like A', B', C, D, in the same diagram, has a net clockwise circulation and there is net momentum to the right in the neighbourhood of the dashed line $A'B'$. (None of this demands that the total motion in a real water column be irrotational – we are discussing merely an irrotational component of the motion. It is well known that any fluid motion can be divided into a portion derived from a scalar potential which is irrotational and a portion derived from a vector potential which is rotational. We are here discussing the former.)

All of this *forces* one to conclude that the principal mechanism by which momentum is transferred to the water is through the process of wave generation. This is not to say, of course, that most of the momentum in the water is in fact carried by the waves. There are all kinds of wave-dissipation mechanisms acting, including wave breaking and a variety of non-linear wave interactions which generate very short waves susceptible to rapid viscous dissipation. When a wave loses its energy, it must lose its momentum as well, but the momentum is not in general lost from the water. It will appear in the form of mean water movements – the drift current.

The observational results most directly related to the statement that the momentum transfer is in fact wave generation, are those of Dobson (1971). Dobson directly measured the transfer of momentum into the water by pressure forces, and within the rather large scatter of his observations found that it was not significantly less than the rate at which momentum was leaving the atmosphere. This result proves also to be consistent with those obtained in the JONSWAP Experiments (Barnett *et al.*, 1973). However as is indicated above, it is possible to convince oneself of this result without any measurements at all.

Dobson's (1971) measurements, interpretation of the JONSWAP (1973) observations and also simple calculations based on standard wave climate data (Stewart, 1961), show that a substantial proportion of this momentum is transferred into rather long waves – waves not very much shorter than the longest ones vigorously excited in the system. It is frequently claimed that the momentum transfer is mostly determined by short waves, but the evidence for this seems to be thin and the notion may itself be wrong. The principal argument for believing in such an effect is the observed fact that the drag coefficient depends rather little – indeed practically undetectably – upon such features as fetch and duration of wind. The long waves depend critically upon these features, the short waves much less so.

The strength of this argument has been weakened recently by the JONSWAP (1973) discovery that the amplitude of the short waves tends to decrease as fetch increases and is not in fact a constant. Altogether, the weight of present evidence seems to support the JONSWAP (1973) conclusion, and Dobson's (1971) observations that the input by wind to waves is mostly in the central region of the wave spectrum.

The theory of wave generation was awakened from a long somnolence by Eckart (1953) and in particular by Ursell (1956). Since then it has been the subject of a large number of theoretical and observational studies. However, I think it has to be admitted that the problem is not yet solved.

The theories which have been most extensively discussed recently can be placed in two categories: Those I will call P-Type and those I will call M-Type, after O. M. Phillips and J. W. Miles, the authors who have probably done the most work and been most quoted in this area. P-Type theory discusses wave generation in terms of pressure fluctuations generated in a turbulent atmosphere and advected over the surface by the wind. The rudiments of the idea were first presented by Eckart in 1953, but it was much refined and clarified by Phillips. It is probably easiest to grasp by noting that the original Kelvin (1887) wake theory discussed the effect of a local disturbance of pressure advancing with constant velocity over the surface. P-Type theory can be taken as discussing an ensemble of Kelvin wakes, generated by turbulent pressure fluctuations. There is no question that this is a real mechanism for wave generation. However the information we now have indicates that it cannot provide an important proportion of the transfer of momentum from the atmosphere to the water. It appears that pressure fluctuations are in fact much smaller than was originally assumed by Phillips, and recent work by Stewart and Manton (1971) has shown that, quite apart from this, the mechanism is somewhat less efficient than was originally thought. Thus one has to consider the P-Type mechanism to be real, but not very important except perhaps in the very initial stages of the generation of waves on a smooth surface.

M-Type theory is non-linear, involving the interaction of the existing wave field with the shear flow in the atmosphere above it. It bears a strong family resemblance to the classical work on the instability of boundary layers. Qualitatively it can be described as follows. Let us put ourselves in a coordinate system fixed relative to the waves. We find the water is moving rapidly to the left, as shown in Figure 4 and the wind at upper elevations is moving to the right. The air right at the surface must follow the water since there is no slip, and therefore at very low levels, the air is moving to the left. There must be thus some particular level at which the mean motion of the air is stationary. Above this level, air moves to the right and below it to the left. However, in order to conform with the wave profile, the air close to the surface must be subjected to vertical pressure gradients, which must fluctuate horizontally according to the phase of the wave. This necessarily gives rise to horizontal pressure gradients.

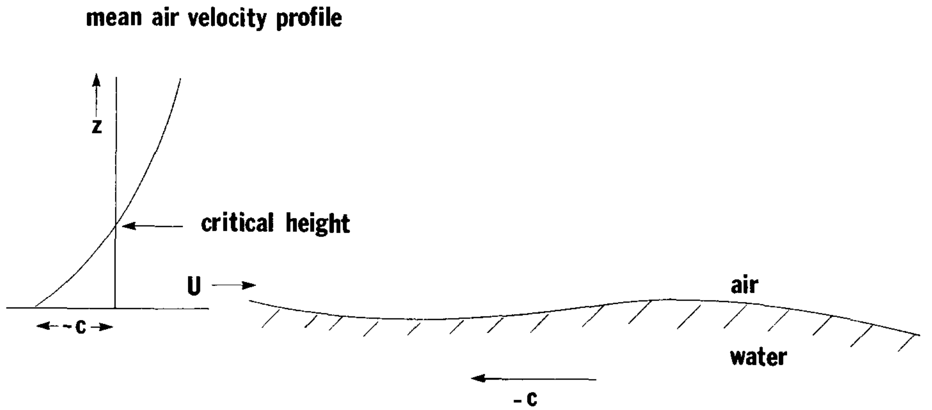

Fig. 4. The critical height. In a coordinate system in which the waves are stationary, the water moves to the left with a speed equal to the phase velocity of the wave. Since there is no slip, the air at the surface must have the same mean speed. At higher levels the wind velocity may be greater than the phase velocity of the wave so that in this coordinate system it is positive. In this case there must be some intermediate height at which the mean velocity vanishes.

M-Type theory is essentially two-dimensional. Flows of this kind have some properties which can be rather simply described in a way which improves intuitive understanding of the hypothesized two-dimensional flows, and may even have some relevance to the real three-dimensional flows. Suppose that a wave field exists, which we will simplify by assuming that it can be represented by a single sinusoid. (The relevance of discussions of this kind depends upon the importance of non-linear effects. M-Type theory usually assumes that the wave field can be broken into Fourier components and each component treated without reference to the others. If non-linear effects are not too great, this treatment will have some validity). Let us choose a coordinate system, such as the one used in Figure 4 above, in which this wave field is quasistationary (only *quasi*stationary since presumably there are some wave growth or decay mechanisms in effect, but their time constant is assumed long relative to the characteristic periods under consideration). Let us further assume that this wave field induces in the air a sinusoidal pressure fluctuation p and a sinusoidal vertical displacement ζ of the air flow, each with the same wavelength as the underlying wave. Now let us see what are the consequences of making the assumption that there is no shear stress in the system – that is, that the Bernoulli expression

$$p + \tfrac{1}{2}\varrho (U + u)^2 = \text{const} - \varrho g z - p(z) \tag{5}$$

relating velocity and pressure along a streamline is valid. For this purpose, we may ignore hydrostatic effects and acceleration due to gravity and take the right side of (4) to be constant. Thus we find that the flow in regions of high pressure must be slower than that in regions of low pressure. If the pressure and the velocity fields are in phase (or in opposite phase) for a particular streamline, the result is that they remain in phase (or opposite phase) for the neighbouring streamline. Where the phase is such that minimum pressure coincides with maximum upwards displacement, the amplitude of the streamline displacement must decrease as we go upwards, as is shown in Figure 5.

Another simple argument permits us to examine the vertical dependence of the pressure fields. The vertical acceleration of the streamline must be provided by a vertical pressure gradient. In fact

$$\frac{\partial p}{\partial z} = -\varrho (U+u)^2 \frac{\partial^2 \zeta}{\partial x^2} \tag{6}$$

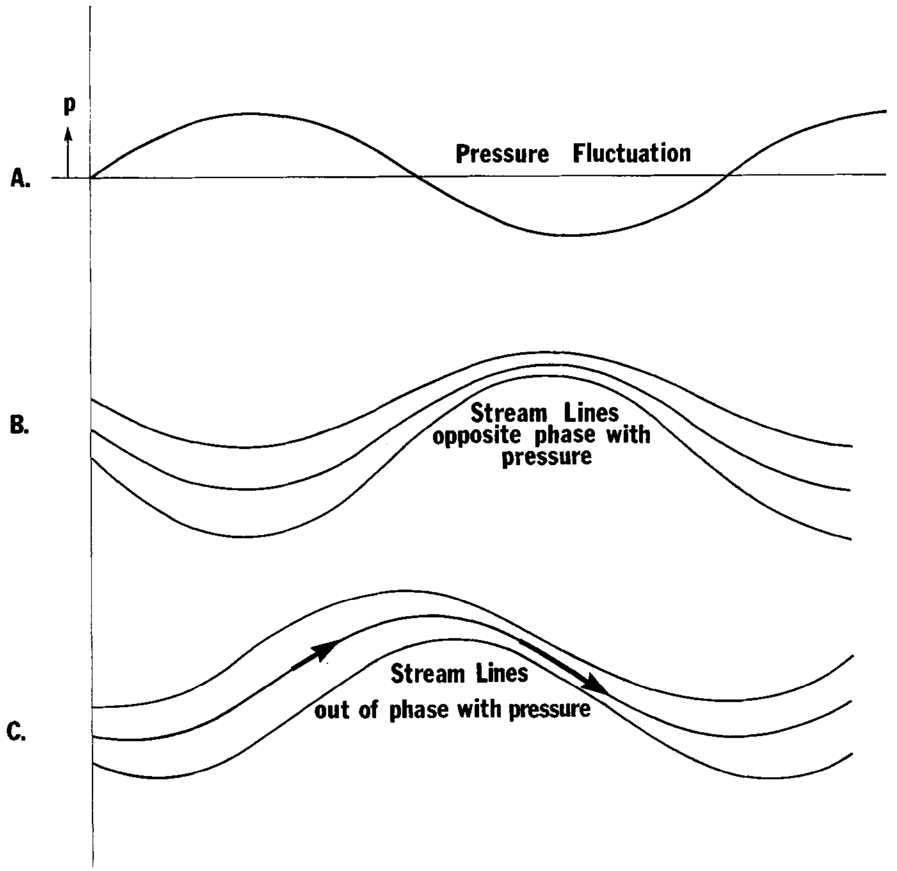

Fig. 5. Effect of pressure fluctuations upon streamline configuration, assuming Bernoulli's equation to hold. When the displacement of the streamline is in phase or 180° out of phase with the pressure fluctuation, the neighbouring streamlines have the same phase, although different amplitude. When the streamline is out of phase with the pressure, neighbouring streamlines have different phases.

and we see that $\partial p/\partial z$ is in phase with the vertical displacement. In other words, if the vertical displacement and the pressure field are in phase at any value of z, within the assumptions we have made, they remain in phase at all other values of z.

Now let us see what happens if the pressure field is different in phase from the displacement field. In this case, the Bernoulli relation requires that the minimum velocity, and therefore the widest separation between streamlines, must again occur at the point of maximum pressure – but this is no longer a point of maximum displacement. As a result, the phase of the neighbouring streamline differs from the phase of the original streamline, as shown in Figure 5(c). When there is a difference in phase between neighbouring streamlines, there are several important consequences. For example, in Figure 5(c) the upward flow is slower than the downward flow. Thus averaged over a horizontal plane covering one full wavelength, the product $\overline{uw}$ is negative. That is, a Reynolds' shear stress transporting momentum downward exists. Also, the pressure has a component in phase with the *slope*, so that if a streamline coincides approximately with the surface, there will be a pressure-slope correlation which will transmit momentum to the surface. This is a requirement for wave generation and in fact if the water motions involved in the wave are taken into account, it can be shown that this is sufficient for wave generation.

Note that if minimum pressure coincides with maximum displacement, the amplitudes of both pressure and displacement fluctuations decrease as one moves upwards. (And vice versa if the phase is reversed.) The pressure fluctuation cannot increase indefinitely. The value of $\frac{1}{2}\varrho(U+u)^2$ in Equation (5) cannot become smaller than zero, so the magnitude of the pressure fluctuation along a streamline cannot exceed $\frac{1}{2}\varrho U^2$. Thus as the value of z decreases, and the pressure fluctuation increases, either the situation must change, or there must ultimately be some kind of surface capable of supporting a pressure fluctuation.

All M-Type theory involves examining various ways in which the displacement fluctuation may get out of phase with the pressure fluctuation in the face of the situation described above, where under simple assumptions once these two get in phase they remain in phase. A wide range of effects may be invoked. The simplest, that of Kelvin-Helmholtz theory, discards the quasistationary assumption and by introducing a time rate of change, generates a phase shift which is capable under some circumstances of producing an instability. Others invoke the effect of various kinds of turbulent stresses which cause modifications in Equation (5) and generate phase differences.

The most complex are probably those studied in detail by Miles and his successors. This kind of theory is analogous to that which has been worked out in considerable detail to describe the instability in shear flows in viscous fluids. Here attention is drawn to the particular level shown in Figure 4 and discussed above in that context, where the mean flow has zero velocity relative to the fluctuating disturbance (in this case the wave). As was pointed out above, if there is a pressure fluctuation, it is only the mean flow which can be zero, and particles are accelerated backwards and forwards depending on where they are in the pressure gradient. At this so-called 'critical height', if the assumptions of no stresses between fluids on different streamlines – i.e., the assumptions of Equation (5) – remain valid, a symmetrical closed flow will occur such as is sketched in Figures 6(a) and 6(b). This closed flow must be symmetrical since the vorticity of the fluid following the closed streamline must remain the same everywhere, in particular at both ends. The result can be either of two kinds, shown in Figures 6(a) and 6(b). In Figure 6(a) the displacement has the same sign above and below the critical layer, but is sharply changed in amplitude. In Figure (6b) the closed circulation (or 'cat's eye') causes the displacement to change phase by 180° across the critical layer. In neither case is any phase difference of the kind required to develop a Reynolds' stress generated between the pressure and the displacement field. To get this phase difference, some other effect must be invoked.

This other effect is the diffusion of vorticity, either by viscosity or by turbulence mechanisms. If there is vorticity diffusion such that the fluid moving in the closed

A.

B.

C.

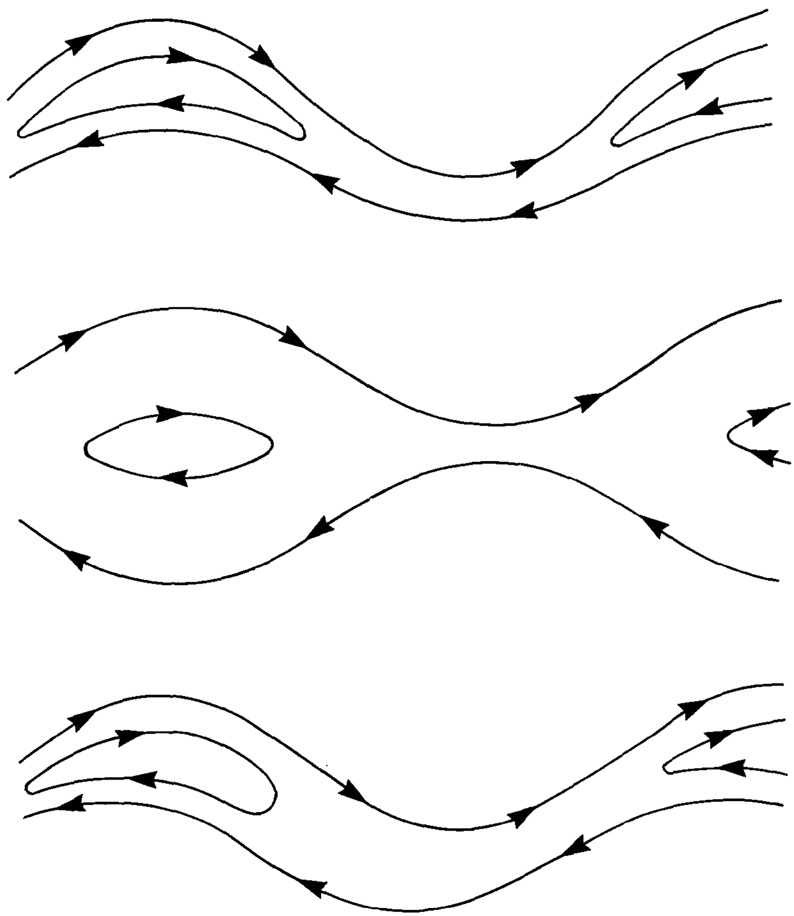

Fig. 6. Behaviour near the critical height. If no diffusion of vorticity is assumed, we get symmetrical patterns as in (a) and (b), which may or may not invert the phase of the streamline above and below the critical height. When diffusion of vorticity is assumed, the closed circulation becomes asymmetrical and a phase difference is generated between streamlines above and below the critical height (as in (c)).

circulation loses vorticity as it moves to the right on the upper part of the circulation in the diagram, and gains vorticity as it moves to the left in the lower part, then the closed circulation will become asymmetrical as shown in Figure 6(c). The asymmetrical circulation forces a change in phase between the flow above the critical layer relative to that below. In the neighbourhood of this closed circulation, the flow velocities are very low so that the pressure gradients are very weak and the pressure changes very little. Therefore the displacement fluctuation is forced out of phase with the pressure fluctuation.

In this kind of theory the loss of vorticity in the upper part of the flow and the gain in the lower part is explained because in a typical boundary layer, the profile is curved in such a way that the shear decreases as one moves away from the boundary (as in Figure 4), so that one has less mean vorticity above the critical height than below. In all of these theories, then, the effectiveness of the wave-generation mechanism depends upon the rate of change of mean vorticity as one moves through the critical layer – that is to say, upon the *curvature* of the mean velocity profile.

There is no question but that this type of theory is sound fluid-dynamically, and its validity in the case of the development of instabilities in the viscous boundary layer seems well established. It is, however, possible to question its relevance to the problem of the generation of waves by a turbulent wind. I am not aware of any example of a measured, detailed instantaneous wind profile over water. However, such measurements do exist in turbulent boundary layers over solid surfaces. Figure 7 copied from Kim *et al.*, 1971, shows an extreme example – extreme in that this was deliberately selected by the authors as a particularly disturbed situation. However, it makes the point I wish to emphasize. What exactly do we mean by the curvature of the mean velocity profile? Clearly when the instantaneous velocity profile has a character such as is shown in Figure 7, it takes averaging over a very large ensemble to produce a curve smooth enough that the curvature of the mean velocity profile would be a well-established quantity. The question then arises as to whether such a large ensemble is meaningful in the wave-generation process, because in a real wave a quite wide

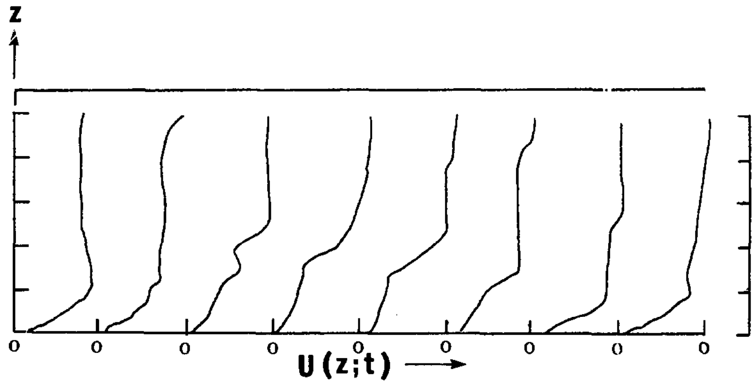

Fig. 7. Redrawn from Kim *et al.*, 1971. Instantaneous appearance of vertical velocity profile in a turbulent boundary layer.

band of wave directions exists, and because the dispersion relation causes wave forms and wave amplitudes to change significantly as the wave propagates; the meaningful averaging time for a particular wave is really quite short. In fact it is quite doubtful if it is long enough for an air particle actually to execute one complete circuit of the closed circulation. Further, the turbulence causes significant variations in the downstream wind flow, so that the height of the 'critical layer' varies a great deal from time-to-time and place-to-place. Since the curvature of the mean profile depends critically upon this height, there arises additional difficulty in relating the effectiveness of this wave-generation mechanism on the basis of observed mean profiles. What is more, in a very large fraction of the cases, the critical height is very low indeed – often significantly lower than the height of the waves.

None of these worries affects the principle that wave generation may be associated with the shift in phase between pressure and displacement which can occur around the height of the critical layer. They do point to the almost insuperable difficulties of calculating the magnitude of such changes in real situations, which must certainly be very strongly nonlinear.

There are other considerations as well. Why must we assume that the flow has an essential two-dimensional character? For wave generation we require that momentum be transferred toward the surface, and this means that downward flowing air must carry more forward momentum than upward flowing air. In M-Type theory this is accomplished, as has been outlined above, by a shift of phase of the streamline displacement as the mean height changes. But why must one assume that the downward flowing air and the upward flowing air occur in the same vertical plane? Is it not equally or more reasonable to assume that the upward flowing air is displaced *laterally* from the downward flowing air – i.e., that the flow is essentially three-dimensional. This is the opinion which has been expressed by Mollo-Christensen and his co-workers (Dorman and Mollo-Christensen, 1971). It is also consistent with the view which is rapidly gaining a foothold among those working in turbulent boundary layers over solid surfaces. These workers are placing emphasis on fairly organized structures in the boundary layer. Most of the hypothesized structures called for downward flowing air in the middle of a sort of double roll with upward flowing regions on each side – structures which have a close family resemblance to that proposed by Townsend many years ago (Townsend, 1956, p. 114).

This kind of structure seems very favourable to wave generation. Over a solid surface the picture is one of downward flowing air encountering the surface in two ways. The presence of the surface sets up a pressure fluctuation field which deflects the air laterally and eventually forces it back up again. The presence of slow moving air close to the surface slows the forward motion of the fluid so that when it turns upwards, it is moving more slowly than it was when coming downward. In the presence of a rough surface like the sea surface, the downward moving air can slide forward along a lee surface, but will be forced out laterally when it encounters a windward surface. This lends itself exactly to the requirement of a high-pressure region at the windward side and a relatively low pressure on the lee side of the wave.

A situation like this can be examined very crudely with an irrotational flow in the following way. Consider an irrotational flow moving with mean velocity U in the x direction over a corrugated surface described by:

$$\eta = A \cos kx. \tag{7}$$

Assume the existence of a downward motion spreading laterally which would be described, if flowing over a plain surface, by the velocity potential

$$\phi = B(y^2 - z^2). \tag{8}$$

It is easy to show that if $kA \ll 1$, a suitable approximation (to the irrotational flow which combines these three characteristics: forward movement relative to the corrugations, conformity with the corrugations at the surface (with slip) and a downward flow spreading laterally), can be described by the velocity potential:

$$\phi = Ux - AU\, e^{-kz} \sin kx + B(y^2 - z^2) + \\ - 2\frac{AB}{k} e^{-kz} \cos kx + A^2 B\, e^{-2kz} (\sin^2 kx - \cos^2 kx). \tag{9}$$

From Bernoulli's equation we can get an approximate expression for the pressure distribution on the surface, which turns out to be:

$$\frac{p}{\varrho} = - 2AU^2k \cos kx - 2ABU \sin kx. \tag{10}$$

The wave slope is given by

$$\frac{\partial \eta}{\partial x} = - Ak \sin kx$$

and so we see that the portion of the pressure fluctuation

$$- 2ABU \sin kx$$

associated with the downward moving flow is in phase with this slope – just the right pressure variation required to produce waves. Under an upward flowing motion, the pressure variation has the wrong sign – but we must remember that in the real situation, the upward motion will be retarded by having been influenced by the surface and so the appropriate mean velocity U would be lower. To relate the expression to wave generation, U should be replaced by $(U - C)$ – the difference between wind speed and wave speed.

The wave generating force is thus given by

$$A^2 kB(U - C). \tag{11}$$

This analysis is too crude to be worth pushing very far, but it is incumbent upon me to demonstrate that its magnitude is sufficient that it should be taken into account. This can be accomplished in the following fashion. It is not unreasonable to take $C=\frac{1}{2}U$, and $Ak=0.3$. The crudest part of the calculation centres on attempting to give a reasonable value to B.

A good deal is known about the nature of turbulence over rough boundaries – including the sea. The vertical velocity spectrum is known to have an approximately universal form which has a fairly well defined peak in the neighbourhood of $kz=1$ (Miyake *et al.*, 1970). The magnitude of the vertical velocity is about 1.2 u_* under near-neutral conditions (Wyngaard *et al.*, 1971). A drag coefficient of 1.3×10^{-3} corresponds to $u_*=3.5\times10^{-2}\,U$. From Equation (6) one finds that the vertical velocity is $2Bz$.

These numbers may be put together to yield as a reasonable assumption: $B=0.02\,Uk$.

The total wave generating force may therefore be put as

$$0.02\,A^2k^2U(U-C)\simeq 10^{-3}U^2. \tag{12}$$

This calculation should not be taken too seriously, but its result seems to indicate that the effect is sufficiently important to be worth examining.

It should be noted that put in the form of Expression (12), the resulting momentum input depends upon the wave *slope* only, and not upon wavelength or height independently. It also depends upon $(U-C)$ and becomes ineffective as C approaches U. Unlike the M-Type mechanism or the P-Type mechanism, it works in the negative sense when the waves are moving against the wind. There is substantial evidence, both from general observation and from the measurements of Dobson (1971) that when wind blows against the waves, there is a significant loss of wave energy.

Examination of these notions will require a more detailed investigation of the relationship between pressure on the water surface and flow in the air above than has yet been carried out. However, a program of analysis of this kind would seem to be compatible with the forthcoming JONSWAP II Experiment.

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