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A PRELIMINARY INVESTIGATION OF LONG WAVES AT NEWLYN

By J. DARBYSHIRE

There is an arithmetical error in p. 69, line 10, of the above paper, published in the January 1958 issue.

for gives $T = 165$ min . . . . The velocity c is 87 ft/sec or 52 kt

read gives $T = 27$ min . . . . The velocity c is 59 ft/sec or 35 kt

This shows that the long waves could have been edge waves, although, as stated in the paper, the evidence is not very conclusive.

It is proposed in the near future to prepare an electronic analogue model of this bay and this may throw further light on the subject.

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AIRFLOW OVER MOUNTAINS: INDETERMINACY OF SOLUTION

By ENOK PALM

In a recent number of this *Journal* (*Q.J.*, **84**, p. 182, 1958) R. S. Scorer has written an article on two-dimensional airflow over mountains. Some of the assertions in this article deserve comment.

Intending to apply the Fourier theorem, Scorer considers, as is customary, first the flow over corrugations

$$\zeta_1 = \cos kx \qquad (1)$$

The differential equation for ζ, the vertical displacement of the particle at (x, z) (disregarding the kinematical effect of the density variation) will be of the form

$$\frac{d^2\zeta}{dz^2} + (l^2 - k^2)\,\zeta = 0 \qquad (2)$$

and if l is independent of z, this has solutions

$$\zeta = A\cos\mu z + B\sin\mu z \qquad (3)$$

where

$$\mu = (l^2 - k^2)^{\frac{1}{2}} \qquad (4)$$

When $l < k$, A and B are determined uniquely by Eq. (1) and by the requirement of finite (i.e., not infinite) value of ζ at infinity, the solutions becoming exponential in type. When $l > k$, the second requirement is fulfilled for arbitrary values of A and B, the first condition determines only A, and the solution seems to be incompletely determined.

Following Scorer the solution may in this case be written

$$\zeta = \cos kx\cos\mu z + B\cos kx\sin\mu z \qquad (5)$$

in which B is *any* function of k. Quoting from Scorer '*In the absence of a special second condition waves corresponding to no disturbance at the ground should be excluded for the same reason that lee waves on the upstream side of the mountain are excluded.*' (Italics by Scorer). The solution is then

$$\zeta = \cos kx\cos\mu z \qquad (6)$$

This assertion is, as will be shown below, incorrect.

1. What is observed in nature is that the waves are created during some initial interval and approach a (quasi) stationary form. In other words, the problem has to be treated as an initial-

value problem. This was first pointed out by Höiland. Wurtele (1953) and Palm (1953), independently, proved that for increasing values of time the solution approaches a limit. This limit is not the form given by Scorer (Eq. 6), but the solution earlier found by Lyra (1943) and Queney (1947) by using the device of introducing an artificial friction, namely

$$\zeta = \cos (kx + wz) \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

2. The indeterminacy of the solution may also be eliminated by applying the radiation condition, i.e., by the requirement that the energy flow in a frame of reference where the mountain is moving, is directed away from the mountain, as shown by Eliassen and Palm (1954). The result is identical with Eq. (7).

3. In the case of lee waves, i.e., when a discrete set of resonance waves exists, the waves on the upstream side of the mountain are excluded for instance by considering the problem as an initial-value problem. *Therefore, in the absence of a special second condition (such as required by a rigid lid) the correct solution in the actual problem is that given by Eq. (7), and not that of Eq. (6), for the same reason that lee waves on the upstream side of the mountain are excluded.*

This result may also be proved directly in the following way. Assume for the moment

$$l = ae^{-bz} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

If b is small, the solution for an arbitrarily-shaped mountain will consist of a number of lee waves plus an integral necessary to make the solution non-singular and to satisfy the boundary condition at $z = 0$. When $b \to 0$, l tends towards a constant; and the number of lee waves may be shown to tend towards a continuous infinity. The solution approaches a limit, and this limit corresponds to Eq. (7).

A similar way of reasoning was given by Corby and Sawyer (1958). They introduced into the problem a horizontal rigid lid which was displaced to infinity. They also found Eq. (7).

It may be of interest to point out that considering in this problem (l constant) waves propagating vertically, it is found that waves with a phase velocity downwards have an energy flow (group velocity) upwards and vice-versa (Eliassen and Palm 1954). From this it follows that the radiation condition mentioned above is not identical analytically with Sommerfield's radiation condition since he discussed waves with group velocity and phase velocity in the same direction. The difference will appear in a change of sign.

What is pointed out above is also valid when the model consists of two or more layers.

References

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9 June 1958.

Reply

By R. S. Scorer

Corby and Sawyer (*Q.J.*, **84**, p. 284, 1958) and Palm have failed to see the point of my letter. I have not argued that their solutions, arrived at by various devices, are wrong.

First it should be made clear that the fact that the same solution is arrived at by two or more different devices does not mean that the flow represented by it will always occur. The lid of Corby and Sawyer which is removed very slowly to infinity while the intervening space is filled with fluid behaving in the correct manner is not found in nature. Nor does the flow necessarily start up from rest in the manner discussed separately by Höiland, Wurtele, and Palm.

Nor is the nature of the flow determined by friction as is assumed in Lyra's treatment and in Queney's early papers – as indeed Queney himself has shown, because the time required for friction to have a significant effect is too long, the particles passing through the region under consideration in a much smaller time.

When a suitable boundary condition is imposed a solution is arrived at. My italicized remarks, quoted by Palm, deal with the case in which there is no such condition; and they are not proved incorrect by considering conditions such as those discussed by Palm. If the effect of friction is discounted the train of lee-type waves which can, from the mathematical viewpoint, occur on the upstream side of a hill, can be excluded on the ground that they do not correspond to anything producing them. They can be added or subtracted quite freely and still a solution of the problem is obtained. The particular solution to choose is the one which can be attributed to the mountain. By the same argument, *when there is an infinity of solutions* the particular one chosen should omit all those which are not attributable to the mountain. Of course if Corby, Sawyer and Palm exclude an infinity of solutions by imposing a condition there is no more to be said about the solution they arrive at except that it applies under the condition stipulated. In nature these particular stipulated conditions do not in fact occur.

The waves may begin in a variety of ways. For instance, there may be flow with no waves, with eddies in the lee, and the wind or stability profiles change so that waves do occur. Palm states that ' what is observed in nature is that the waves are created during some initial interval and approach a stationary form.' But this does not mean that the flow starts from rest. Far from it! What about ' evening waves ' ? Since there is an infinite number of ways in which waves can happen it is probable that any solution which is possible can be arrived at by giving the system a suitable history; i.e., by imposing suitable boundary or initial conditions. It is not correct to assume that a solution arrived at by a device, particularly if it involves letting a parameter tend to zero or infinity, is the one which nature would produce if the parameter were always at the limit. For instance inviscid flow is not the same as flow at very large Reynolds numbers.

Thus my object was not to pose a particular (and probably unrealistic) problem in order to eliminate the indeterminacy so that it could thereafter be forgotten about, but to see what could be said when the solution was mathematically indeterminate.

With regard to the flow of energy, I showed (*Q.J.*, **80**, 1954, p. 419) that the rate of working by the lower layers on the upper was exactly cancelled by the downward transport of kinetic energy for any waves, so that there is no flow of energy. But in any case there is no valid reason for supposing that energy should flow upwards rather than downwards. The work must come from flow across the isobars and this may occur at any level, so that if there were a flow of energy it could be downwards and the airstream could suck the mountain towards the direction from which it flowed!

Then, can a streamline be frozen and a correct solution arrived at ? If we take the case of flow over a corrugated ground of form $\cos kx$ under a rigid lid, in certain airstreams there are several nodal surfaces. If we apply the principle that any streamline may be frozen and a correct solution obtained we may freeze one of the nodal surfaces and conclude that the flow between two rigid boundaries is of wavelength $2\pi/k$ and of some particular amplitude! Undoubtedly a correct solution is arrived at by freezing a streamline but since the problem is indeterminate it is not the unique solution. Corby and Sawyer argue that there cannot be nodal surfaces if there is a whole spectrum of wavelengths present but this is irrelevant to the question of whether a streamline can be frozen; all it means is that in that case the procedure cannot be made to look quite so ridiculous.

Finally, with regard to the mathematics of the general solution. I was concerned to point out that in the general solution

$$\zeta = A\cos\mu z + B\sin\mu z$$

the term B could lead to something quite arbitrary. This arbitrariness is removed by the various devices of Corby, Sawyer, Palm and others. But nature does not necessarily employ such simple devices, particularly when the flow varies with time in a complicated way. What if nature does not bother to employ any device at all ? Then according to my argument $B = 0$.

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