CORRESPONDENCE

Atmospheric signal velocity

By R. S. Scorer

Department of Meteorology, Imperial College of Science and Technology, South Kensington, London, S. W. 7.

18 July 1950

The idea of a "signal velocity" was expounded recently by Dr. Charney.[1] He did not indicate what kind of disturbance was being propagated or how it arose, and this makes all the difference between the study being practical or purely academic. There are two kinds of gravity waves in the atmosphere: those which travel with a speed comparable with that of sound, and those whose speed is comparable with the wind. Each may be propagated independently. As examples of the former we have the atmospheric tides and the oscillations produced by the eruption of Krakatoa in 1883 and the fall of the Great Siberian Meteorite in 1908, and of the latter there are the standing waves often found in the lee of mountains. There is a need that writers discussing possible stable waves in the atmosphere should consider more carefully how they could be set up.

Any non-adiabatic process which changes the heat or mass content of a region will set up waves which travel, like the Krakatoa air wave, with about the speed of sound. It is only when their energy is enormous that they can be observed, and even then they are accompanied by no evident weather phenomena. Nevertheless their signal velocity is about the speed of sound, and it depends upon the wavelength so that any pulse is dispersed as well as attenuated and dissipated. The only indisputable examples of the other kind are waves in the lee of mountains, and then the mechanism is well understood and fairly described mathematically. The real problem is whether they are ever set up any other way and are propagated through the air across the ground. Even supposing that they can be set up, they would be dispersed because over the whole range of wavelengths the velocity may easily change by a factor of two or three, so that a solitary disturbance would be broken up into a long, probably unobservable wave train in the course of a few hours.

If this is so, one may ask, how can disturbances that look like waves travel across weather charts and retain their individuality for days? The answer lies in the fact that all pressure gradients are not propagated under gravity, like unevenness on the surface of water, but are maintained by circulations that are advected with the air stream like dimples at the surface over vortices in water. Most pressure gradients are set up either directly by non-adiabatic processes, or more often, since the processes are slow enough for a geostrophic balance to be nearly maintained, by the development of circulations which derive most of their energy from vertical overturning. Any energy that may be derived from the mean motion is retained locally and not propagated as stable wave motion. A region where overturning is taking place may be advected with the wind, but since it is a doublet with a source and a sink close together any gravity waves it might set up that could be propagated as vertical wave fronts vanish at a very short distance by cancellation of the effects of the source and sink. A "short distance" here means a horizontal distance equal, say, to ten times the depth of the overturning. The wavelike disturbances that travel across weather charts are not stable waves at all, but are another type of disturbance that is entirely advected.

May I, therefore, venture the opinion that no stable waves are ever observed that do not either travel with about the speed of sound or are stationary relative to their cause? This is not to say that the forces that would propagate a stable wave, were it set up, are not important; they are of course continuously operative —the static stability and deviating force. But to quote Dr. Charney, "a necessary attribute of a meteorological theory is that it express those factors which are consciously or unconsciously used by the forecaster, since his skill is essentially positive." Forecasters do not take cognizance in forecasting for a point of properties that cannot be advected to that point, which is to say that no meteorological influence traverses the ground faster than the wind at some level, and accords with the present thesis that observable stable waves do not travel over the earth. It is incumbent upon anyone asserting that a pattern is a stable wave to say how it is set up. Since they merely serve to transmit energy, a source is required—and not just a vague physical source, but a precise mathematical one for the mathematics of waves is precise enough.

Unstable waves always move with the wind at some level and so in forecasting for a point one needs take into account only the region from which effects might be advected there, and the spread of observations required for a forecast is thus determined. Examination of Dr. Charney's fig. 5, in which he gives an example of the initial, predicted and finally observed northward velocity at 500 mb at 45°N, reveals that as good a

[1] J. Charney, "On a physical basis for numerical prediction of large-scale motions in the atmosphere," *J. Meteor.*, 6, 371–385, 1949.

forecast as his could be obtained by advecting the initial values. Sutcliffe's technique of using the geostrophic equation in the process of calculating the horizontal divergence from the vorticity field is the same as Dr. Charney's filtering technique, but Sutcliffe finds no need to consider the signal velocity in his method which is primarily a forecasting method. Scherhag's forecasting technique depends partly on finding the correct advection level, and even Dr. Charney proposes to use large-scale patterns as steering currents for the smaller.

Dr. Charney's evaluation of the vertical signal velocity is wrong. Firstly it contradicts the hydrostatic equation on which its calculation partly depends, and secondly the possibility of ever setting up Dr. Charney's own barotropic model depends on stable wave-fronts in the actual atmosphere being almost vertical, thus supposing an almost infinite vertical signal velocity. His calculations in section 5 discuss an impossible kind of motion for he writes $\rho = \rho_0 \exp(-zH^{-1})$, and later finds that

$$\rho \propto \exp\{-\tfrac{1}{2}zH^{-1} + i(kx \pm \mu' z - \nu t)\},$$

so that whether the latter density is a perturbation or a total value (he does not say which), the density is negative at large enough z. There is, however, no cause for alarm that we might have to take into account disturbances at heights unreached by radiosondes, because no appreciable disturbances originate outside the troposphere. Furthermore, the effect of the higher layers is taken into account when note is made of the height of the uppermost isobaric surface used.

One is led to speculate as to the efficacy of a purely kinematical extrapolation of the situation as it develops. It would probably be as accurate as using Charney's barotropic model, and would be much more easily handled by a machine. It would depend on a whole series of charts, but there is no objection to that. The main criticism that it would not allow for new developments is equally applicable to the barotropic model.