In the other alternative, namely, $a > q$, we have two cases to distinguish:

(1) . . . . . $(a-q) < \frac{a^2m}{4q^2}$.

In this case λ_1 and λ_2 are both real and negative. The equilibrium at U, I is stable, the disease will become definitely established, if once started. The approach to equilibrium is aperiodic, asymptotic.

(2) . . $(a-q) > \frac{a^2m}{4q^2}$, or $\frac{m}{q} < 4\frac{q}{a}\left(1-\frac{q}{a}\right)$.

In this case λ_1 and λ_2 are complex, with negative real parts. The equilibrium at U, I is still stable, but will be approached by a periodic process of damped oscillations.

It may be remarked that a solution can still be given if the coefficients a, m, q, which have here been treated as constants, are regarded as periodic or even as general functions of the time. However, the numerical evaluation of the coefficients appearing in the solution then becomes very onerous. It will suffice, on this point, to refer to the pertinent mathematical literature, as, for example, Picard, "Traité d'Analyse," 1908, vol. 3, pp. 187, 188, 194, 197; Goursat, "Traité d'Analyse," vol. 2, 1918, pp. 482, 498.

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The Cause of Anticyclones.

IF space permits, I should like to reply to one or two points in the letter contributed by Mr. W. H. Dines to NATURE of April 14, p. 495.

(1) In the first place, when one is dealing with two different sorts of air, probably of unequal frequency of occurrence, it appears to me to be unsafe to depend very greatly on comparison with mean values derived from all cases considered *en masse*. Has not the Bjerknes theory been elaborated as the result of an attempt to deal with the problem of atmospheric circulation on the assumption that discontinuities might exist, and that therefore—as other methods would probably fail to reveal them—only close study of individual cases could hope to succeed?

Apart from this, Mr. Dines deals with departure of temperature from the mean for the *height* and date. In the paper (Q.J.R. Met. Soc., Jan. 1923) to which reference was made in my earlier letter (NATURE of March 31, p. 429) temperature is dealt with throughout in relation to given *isobaric surfaces*. This seemed particularly desirable in the case of polar air, for a mass of such air, leaving polar regions with high velocity and low barometric pressure, may eventually find itself, with much reduced velocity and with a pressure increased by some 20 to 30 millibars, forming the surface layers of an anticyclone in temperate regions. The corresponding adiabatic increase of temperature (communication of heat from warmer seas, etc., being left out of account as being more or less common to all polar air moving southward) would be 3° to 5° F., or enough to bring the temperature at a fixed *height* up to about the mean temperature for that height. If the fifty-two cases of anticyclones referred to by Mr. Dines are considered from the point of view of normal temperature for a given pressure it will be evident that about half must be of polar air up to 1 km., and I think this is about as large a proportion as I should claim for that level. Rather more than one-fifth would then be polar up to 3 km., and so on.

(2) The question in regard to humidity is very complex; but I have always taken exception to the view that humidity (either relative or absolute) would be of much value in distinguishing between polar and equatorial air apart, that is, from its value for locating the discontinuity. In particular, polar air, in its passage over warmer seas, should have its humidity at all heights affected quite as greatly as its temperature. Equatorial air, on the other hand, is being cooled in its surface layers in the course of its northward journey, and the cooling effect does not tend to be propagated upward to any comparable extent; such factors as are at work within equatorial air tend rather to rob it of its water vapour without renewing the supply.

I do not, therefore, consider that where polar air lies under equatorial air the inversion of temperature need necessarily be associated with any particular peculiarity as to relative humidity. At the same time, the conspicuous decrease of relative humidity is well known and appears to be common, at least to all inversions in anticyclones. It may, therefore, be a natural sequel to the inversion itself, and I offer an explanation which seems to me not altogether impossible. It is that the inversion of temperature once formed acts as a non-return valve to moisture (in the same way that it almost certainly does to dust and haze in the atmosphere), and that very soon the "convectional lid" accumulates a concentration of water vapour just beneath it; the layer of air just above, on the other hand, succeeds in passing on upward or allowing to drop below the greater part of both its dust and its moisture while replenishment of these from below has ceased.

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Wimbledon, S.W.19,
April 26.

THE reply of Mr. W. H. Dines, in NATURE, April 14, p. 495, to Major Goldie's letter, brings out very convincingly the peculiar fact that the temperature conditions of the troposphere, both in cyclones and anticyclones, are such as would rather obliterate than maintain them. Indeed, when we consider the problem of pressure distribution, we find that the conditions are generally exactly the reverse of those required by the ordinary accepted theory, except in latitudes within the tropics of Capricorn and Cancer. We are thus faced with a very striking theoretical difficulty; for the winds of the earth do not appear, in the main, to derive their force and direction from the temperature conditions at or near the earth's surface.

One of the most marked effects of surface temperature on the pressure distribution, other than the phenomena of the trade winds, is the fact that along the high-pressure belts of the tropics the pressure is greatest over the cold land masses during the winter and lowest over the heated land masses during the summer. Another clear effect of surface temperature is the fact that the North Pacific cyclone and the North Atlantic cyclone (the eyes of the North Polar cyclone) are more powerful during the summer than they are during the winter. However, we have to set against these considerations the striking facts that throughout the year the great low-pressure areas are over the frigid poles, which are not even exposed to the sun's rays during the winter, and that the high-pressure belts are near the tropics of Cancer and Capricorn, and cover the intensely-heated desert lands of the continents. To surface temperatures, on the other hand, must be ascribed the great seasonal changes of pressure and temperature which occur over the elevated areas of Asia.