

matter at the time of the test. The movements of the ship at the time were not such as would allow the instrument to be kept near the surface without the risk of it being over-ridden and damaged. It was therefore used at a depth of about 7 fathoms. The log-ship time gave the rate of the surface stream,

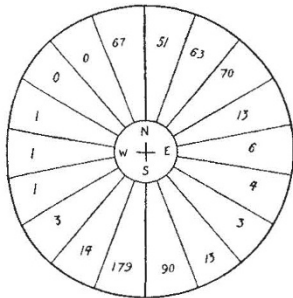


FIG. 2.—The distribution of balls in the compass box after a test of 24 hours ending 10.25 A.M., October 18, 1924.

which rate was assumed to be also that of the water at 7 fathoms depth (though only pending further opportunities of testing the instrument near the surface).

Since the rate at the depth worked is certainly less than at the surface, it follows that the conversion factor used (24 balls per mile) is too low, and that the magnitude of the residual current calculated is too high, although the direction is correct.

The above data yield the following approximate maximal value for the residual water movement or drift :

$\frac{4}{5}$ miles per 24 hours in the direction S ca. 50° E. mag.

This drift indicator can be adapted to drop balls with varying frequency, and it can be left down for a length of time depending upon the size of the hopper chosen. Improvements in shape and size are to be carried out, and it is hoped that it will be possible to add considerably to our present knowledge of the water movements in areas of interest.

J. N. CARRUTHERS.

Ministry of Agriculture and Fisheries,
Fisheries Laboratory, Lowestoft.

The Resonance Theory of Hearing.

IN reference to the recent letters of Prof. Scripture and Mr. J. Keen which have appeared in NATURE under the above heading, it seems to me that the opponents of the resonance theory rely rather too much on the assumed inherent absurdity of the theory. It is a theory which has been, and is, held by a considerable number of people whose judgment in the matter is deserving of, at all events, some consideration.

In drawing analogies from the other sense organs, Mr. Keen should bear in mind that the nature of the analysis required of them to furnish accurate information to the brain is different for each sense. To say that the retina does not analyse white light into its constituent colours is to assert what is by no means proved. It is certainly true (fortunately for our vision) that the retina does not separate these constituents of white light spacially, but that an analysis of the waves of light according to frequencies does occur is an assumption essential for the explanation of colour vision. It is generally believed that this analysis takes place in the retina and not in the brain. The analogies between tone analysis by resonance and colour vision may indeed be carried much further.

Mr. Keen's example of the senses of heat and cold seems scarcely applicable. Here temperature represents intensity, and is perceived as such. Intensity is, of course, an attribute of all sensation.

The recognition of the basilar membrane as a resonating organ is based on the regular gradation of the differentiation of its parts for length, tension, and mass, between very wide limits. So marked is this gradation, that there is no inherent absurdity in the belief that it is sufficient to account for a range of

action corresponding to the audible scale, say eleven octaves. The elaboration of this differentiation is unexplained either by the telephone or the pressure pattern theory. Indeed, the differentiation that one recognises in the cochlea is sufficient to preclude entirely the possibility of such "simple" modes of action. It is better to explain the function of an organ in terms of what is actually present than to devise theories as to how it may act in spite of what is known of its structure. GEORGE WILKINSON.

Polarised Light and Starch Content of Plants.

NEARLY two years ago (NATURE, January 13, 1923, p. 49) I was able to describe the acceleration of hydrolysis of starch under polarised light *in vitro*. It may be of interest to record that I have succeeded in confirming these results in the living plant.

Under artificially polarised light, the starch in the plastids, both in the guard-cells and in the leaf-tissue, has been shown to diminish, and the appearance in its place of a reducing substance has been demonstrated. In ordinary sunlight the starch content was increased, and in darkness the quantity remained unchanged. It is hoped to publish a full account of these experiments in the near future.

I wish to express my thanks to Prof. Neilson Jones, and to Prof. Lloyd, of McGill University, for helpful advice and criticism; also to the latter for kindly placing the facilities of his Botanical Department at my service, thus enabling me to benefit by the blue skies of Canada.

E. SIDNEY SEMMENS,
Amy Lady Tate Scholar of
Bedford College, London.

The Forces which lift Aeroplanes.

PROF. BJERKNES, in his Royal Institution lecture (NATURE, September 27, p. 472; October 4, p. 508), introduces the phenomenon of "acoustic repulsion" before going on to the accepted circulation theory of aeroplane lift. Both these are discussed in Rayleigh's "Collected Papers," Nos. 52 and 53 respectively; "Note on Acoustic Repulsion" (*Phil. Mag.*, 1878, also 1902, 1905), and "On the irregular Flight of a Tennis Ball" (*Mess. of Maths.*, 1877).

The former paper shows that the mean pressure in the interior of the cavity of a resonator is in excess of the external atmospheric pressure by the essentially positive quantity $(2\gamma t)^{-1} / (p_1/p_0 - 1)^2 dt$. The latter establishes, for the first time, the relation, Lift = Circulation x Velocity, a quantity which may be either positive or negative.

There does not seem to be any very close analogy between the two phenomena. A. R. LOW.

London, October 25.

The Technique of Contraception.

I VERY much value the discriminating and appreciative review by Sir Archdall Reid in NATURE of October 25, p. 601, of my book "Contraception (Birth Control), its Theory, History and Practice."

About the general technique of contraception, Sir Archdall is so kind as to say: "Dr. Stopes's experience . . . is so extensive that her opinions must necessarily carry weight." For this reason I must beg the courtesy of your space to correct his criticism that my views on cervical interlocking are a "belief." On the contrary, they are based on direct observation at the Clinic of more than forty cases. Further details will be given in the report on the first five thousand cases at the Clinic, which we are at present preparing.

MARIE C. STOPES.
Givons Grove, Leatherhead, Surrey,
November 6.